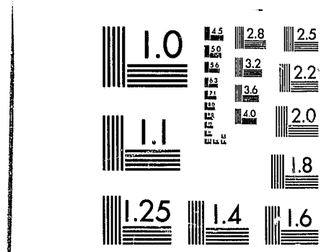

Victimology Research Agenda Development

Volume II: Workshop Proceedings

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Victimology Research Agenda Development

Volume II: Workshop Proceedings

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ABSTRACT

This document presents the edited proceedings of the Victimology Research Agenda Development Workshop held in McLean, Virginia on March 10-11, 1980. This workshop, organized around the presentation of eight commissioned papers, was designed to assist the Office of Research Programs of The National Institute of Justice in the development of a research agenda in the area of basic victimology.

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ACKNOWLEDGEMENTS

There are a number of individuals at the National Institute of Justice and at LEAA who provided valuable assistance to this project in planning the project activities, in the selection of topics and of authors and as participants in the workshop. These are Walter Burkhart, Director of the Office of Research Programs; Richard Barnes, Director of the Center for the Study of Crime Correlates and Criminal Behavior; Fred Heinzelmann, Director of the Office of Community Crime Prevention; Bernard Auchter of the Office of Community Crime Prevention; and Jan Kirby, Program Manager of the LEAA Victim/Witness Unit. We would also like to thank Emilio Viano, Director of the National Victim/Witness Resource Center, who also contributed to the project. Finally, special thanks go to Bernard Gropper, the Program Manager, for his assistance in all these areas throughout the whole effort.

INTRODUCTION

The Victimology Research Agenda Development project was undertaken in the fall of 1979 to assist the Office of Research Programs (ORP) of the National Institute of Justice (NIJ) in the development of an expanded research program in victimology. To date, most of LEAA's and NIJ's action and research efforts have been offender and crime-oriented, reflecting an emphasis on law enforcement and the criminal justice process. However, as the victim has become a more salient public issue, the LEAA and NIJ have devoted increasing resources to the victim area. By and large, the research performed has been applied in nature--directed toward improving services to victims, enhancing the witness function, and addressing the specific problems of unique victim constituencies like rape or child abuse victims. It was felt that an expanded program of research focusing on characteristics of crime victims and the victimization process and treating victims as integral parts of criminal situations would provide a valuable complement to the NIJ criminological research. Judging from the improved knowledge about the crime of rape, for example, which has emerged from information recently derived from rape victims, it seems that victimological research is particularly promising in terms of its potential contribution to the NILECJ's goals of (1) improving knowledge of the correlates of crime and the determinants of criminal behavior, (2) developing better methods for the prediction of crime, and (3) increasing the capability to prevent and control crime.

Potential topics in the area of victimology and victimization were selected based on a review of the literature and researchers were identified who have established an ongoing record of quality research in empirical victimology. Papers were commissioned in each topic area and a workshop was convened for the purpose of inviting dialogue among researchers so that new and relevant areas of victim-related research could be identified through the presentation and discussion of the invited papers.

This report is the second volume in a series of volumes devoted to the Victimology Research Agenda Development Project. This document, Volume II, contains the proceedings of a two-day workshop held in McLean, Virginia on March 10-11, 1980. Volume I contains the eight invited papers around which this workshop was organized. Volume III presents a review of issues raised in the papers and the workshop and provides research recommendations to NIJ.

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VICTIMOLOGY RESEARCH AGENDA DEVELOPMENT WORKSHOP
FIRST DAY: MORNING SESSION

REISS: Good morning. Welcome to Tyson's Corner, Virginia. Our welcoming committee consists of myself, Harry Bratt, who is the Acting Director of the National Institute of Justice, and Walter Burkhart. They and I will say a few words in introduction.

BRATT: Thank you, Al. I am pleased to be here this morning to welcome you to what I am sure will be a fruitful meeting. We are looking forward to getting the benefit of the views and recommendations of this prestigious group on victim-related research and how it will help expand our understanding crime and criminal behavior.

As you know, the Justice System Improvement Act of 1979 authorizes a restructured National Institute of Justice, a new LEAA, and a Bureau of Justice Statistics. Within that Act, it enumerates several priorities, among which are exploring the problems of crime victims and witnesses and also providing more information on the causes and correlates of crime.

While the Act places renewed emphasis on these areas, our interest in these areas has been longstanding. As all of you know the NCJISS, or National Criminal Justice Information Statistics Service, which is now the Bureau of Justice Statistics, has for several years sponsored the National Crime Survey and has done extensive analysis of the victimization data.

LEAA has also placed, particularly I would say in the last year or two, increasing focus on problems of victims, and substantial funds have been awarded for victim-witness assistance programs.

The Institute's research program has concentrated on several victim-related issues, including identifying victim characteristics and needs for service, and exploring criminal justice practices which affect citizen attitudes.

In addition, we have had a long-range, multi-year effort with Northwestern University looking at community responses to crime. We have also looked at aid to victims, such as victim compensation, restitution, rape crisis centers, and so forth.

Another interesting research effort we are undertaking now focuses on another class of victims, those inmates who fall victim to crime while they are incarcerated.

Bob Burkhart will be discussing these and other efforts in more detail. He will also outline the background and purposes of meetings such as this.

In conclusion, we hope that these discussions will identify potential research possibilities for the entire Institute, and specifically for the Center for the Study of Crime Correlates and Determinates of Criminal Behavior, which serves as a focal point for basic research in the Institute.

BURKHART: Thank you, Harry. I, too, would like to welcome all of you to this workshop on victim-related research. I think it's an extremely important area. I am extremely pleased to see so many people from different operational agencies, program people as well as people with other research divisions. We are looking forward to the discussion here today.

As you know, LEAA, ever since its inception, has supported a variety of victim-related projects. OLEAA, the Office of Law Enforcement Administration Assistance, which is the predecessor of LEAA, actually supported the pilot project of victim surveys in the District of Columbia and other cities in the late '60s. And since that time, that model has been refined and expanded to constitute the National Victimization Survey that exists today.

In addition, six to seven years ago, there was a great deal of emphasis on victim-related assistance projects. And during that period and since that time, there have been a variety of action programs that have been sponsored, supported, and encouraged to help the victim.

In terms of research, as Harry Bratt points out, we have supported a wide variety of research projects that have related to victims. Almost all of them, have been for a purpose other than looking at the victim, per se. For example, the victimization in prisons research, is being undertaken for the purpose of improving the management of prisons. We have also sponsored studies having to do with the victims of rape. There was a project some years ago that looked at police operations in terms of efforts they could conduct in the way of crisis intervention to assist victims. And there are many others. But all of those have generally been supported for the purpose of looking at some facet of the criminal justice system.

One of the difficulties is that as you begin to realize the importance of focusing on the victim, it raises a number of questions. We are quite convinced that victim-related research will help us to get a better understanding of the criminal act.

Victim-related research will better help us to understand the offender. It will also, I think, provide some guidance to action in terms of supporting programs that will ameliorate the plight of the victim, and only through research will we have a better understanding of what those difficulties are.

As one begins to ask questions about victims such as: what should be the focus of the research? what is the theoretical basis? what is the conceptual framework? one is left in a quandry. The reason why we are meeting here today is to help us determine the direction that we should go, where our foci should be, and what type of a coordinated program we can develop with the limited resources that we have available.

I want to provide special thanks to The MITRE Corporation for their assistance in developing the agenda and identifying both the individuals who will be presenting papers over the next two days and the people who will be reacting to those papers. At the same time, I do want to stress the fact that it is extremely important that we all participate. There is a wide range of interests here, and I think that is most important in getting to the point of determining what we should do with the limited resources that we have available.

Simply to reinforce the need--and one wonders why this need wasn't well recognized earlier in terms of research--I look at Marvin Wolfgang's work in homicide and how that brought to everyone's attention the crucial role of the victim in some of these critical crimes. Wes Skogan, who was a Fellow at the Institute a number of years ago is another noted scholar in this area, as are Al Biderman and Al Reiss, who are the granddaddies of the victimization surveys.

We've had a great deal of impetus for moving in the direction of a more concerted research program on victimization. Quite frankly, I even avoid the term "victimology" because I am not certain just what that is. We sat down and we started to ask ourselves, Where should the focus be? Should we be looking at the plight of victims for the purpose of improving our action programs? Should we be looking at the role of victims in terms of how they either deter or exacerbate situations that result in crime? Should we be looking at the plight of victims?

It creates many questions, and I think that the task that you have before you today is not an easy one at all. I want to thank you all for coming. We look forward to the discussion. I am sure that we will gain from the work that you have already done.

Thank you. I will turn you back now to Al Reiss, who is going to moderate the program over the next two days.

REISS: I just want to say three things. First, I hope we will be mindful of the purpose of this conference, which is to somehow help the National Institute of Justice to think about the kind of program and research it should be developing in this area. And that means that from time to time I may interrupt and try to bring us to that kind of question.

Secondly, for many of us, that puts us in a kind of bind. Even though in one sense knowledge should be public, as Merton has pointed out, there is a sense in which it becomes private property, in the race for the prize, whether it's a Nobel or the citation index. In that race people tend to give out their worst ideas and to retain their best ones for the time when the National Institute issues an RFP. And then, suddenly, these pour forth in the response to the RFP.

Now, I don't know how to encourage you to loosen up and to do your best for the National Institue in helping them, but I hope that when some of you get as old as Al Biderman and I are, you will realize that there are so many ideas out there that you are never going to be able to work on, that you might as well let them go anyway.

We are going to operate on a very short format which is to give the person who wrote the paper a few minutes, a very few minutes, to say something about that, to give the specific discussant an opportunity, but a limited one, and then try to have as much discussion on each paper as possible. We're going to begin by giving Mike Gottfredson a very few minutes to say anything he may want to say. Then we will turn to specific discussants.

GOTTFREDSON: I would like to proceed in the spirit that Al identified, and that is that I am presenting my best ideas here today, not holding anything back. It would not disturb me, however, if you will entertain the hypothesis that I might be holding back some excellent ideas that aren't identified here. I assure you that's not true.

I think the assertions that the level and kind of victimization that people have depends on their exposure to crime has never been a particularly controversial idea among criminologists. These ideas appear, or have appeared, whenever people have attended to the population at risk or the idea of a rate of criminality or victimization.

Criminologists have argued for years for the tabulation of crime data on the basis of relevant units at risk. There is a long and honorable history of that kind of argument in criminology; the refinement of the populations at risk, different counting mechanisms and so forth.

Of course, academic criminologists have not been the only ones who see relevance in the idea of exposure to risk. People who lock their cars downtown but not in the suburbs attend to that idea, as does the father who drives his daughter to the evening movie but allows her to walk to the matinee. The police have always attended to the idea of exposure to risk by increasing their activity at night, for example.

But apart from a few measurement criminologists who attended to these ideas, that is to the rate problem, most academic criminologists have either regarded the idea as trivial or simply another in the litany of problems with crime statistics that mars their utility for scientific purposes. Either way, etiological criminology need not be overly attentive to these ideas.

The advent of victimization surveys changed that to some extent, because they allowed researchers to vary their rates according to relatively specific populations in ways that official data had only grudgingly permitted in the past. That's not to say that a good deal of excellent research hadn't been done in that area with official data. It's just to say that the National Crime Panel and other large-scale victimization surveys enhance the researcher's flexibility in this regard.

Although these victimization data are far from being rid of measurement problems, they did demonstrate marked differences in victimization probabilities as subgroups vary. These differences were not specific to the type of common victimization studied, and it therefore became increasingly difficult to dismiss these findings, as purely artifacts of measurements, in conjunction with the very similar findings that had long been available in official data.

But as far as scientific criminology goes, the triviality problem, it seems to me, remains. To say that differences in probability of victimization depend on differences in the amount of exposure to crime that different populations have may be true, but is it an adequate way to go about explaining crime?

Scientific criminology asks us, "does it advance our ability to predict and explain victimization?" In considering

these issues, it seemed to me necessary to distinguish, or at least useful to distinguish, conceptually between what I have tentatively labeled absolute exposure and probabilistic exposure.

Absolute exposure consists of those characteristics of persons, objects, time or space that are logical requisites for the occurrences of a specific form of criminal victimization. Without absolute exposure, crime cannot occur. Thus the auto theft rate in the eighteenth century was zero, and the child abuse rate for childless couples is negligible, as is the male rape victimization rate. To specify these rates, which are conditioned by absolute exposure, is to state the obvious, although I think some reflection will indicate how difficult the obvious is to state in many cases.

I argue in the paper that predictions based on the concept of absolute exposure are trivial because they are logical predicates of victimization. I think I should clarify what I mean by "trivial" here. As applied to logic, for example, the word "trivial" means obviously correct or true. For example, two times two equals four. As applied to empirical results, it means common, well-known, obvious or true.

It seems to me, although I am attempting to move a step beyond absolute exposure concepts here, in the early stages of development of any scientific field it is important that, in these senses, everything be trivial.

In the paper, I distinguish probabilistic exposure from the concept of absolute exposure.

Probabilistic exposure requires absolute exposure. It refers to differences among people, objects, places and times in their opportunity for victimization, given that victimization is logically possible. Then I try to track some of the ideas and consequences of that point of view, the first being that probabilistic exposure is an important concept in scientific explanation of victimization only to the extent that it is non-random.

I go through the research that I am familiar with, that identifies--or that bears on--the question of whether probabilistic exposure is non-random. It includes the identification of correlates in the victimization data and official statistics; it includes the research on multiple victimization that attempts to apply some independence models to the victimization data. Such models have been found to be an inadequate fit to the observed data on multiple victimization. I essentially conclude that the

extant data are consistent with the idea that probabilistic exposure is non-random.

I then discuss the prediction of probabilistic exposure, which seems to me to be the central problem for the etiology of victimization, and identify the work that Mike Hindelang and Jim Garofalo and I have done in that regard, and some recent work by Larry Cohen and Mark Felson and his colleagues in Chicago. I see a good deal of compatibility between the two approaches, the lifestyle approach and Cohen and Felson's routine activities approach.

I see a couple of impediments to enhancing the predictive efficiency in data on probabilistic exposure with regard to the two important theoretical concepts, exposure to risk on the one hand and lifestyle or routine activity on the other hand. I think there are some indicator problems in the national crime survey that bear on those issues. I think there are some mechanisms that could be inserted in the survey instrument. The impediments are not incompatible with the general survey methodology that's been used. I think that the indicator problems could be accommodated that way.

And I essentially conclude my arguments by pointing to some theoretical directions that I see as being important in trying to come to grips with the research agenda for victimization. The first is the contrast between the lifestyle exposure or routine activity approach to understanding victimization and the typological approach. I think there are some research implications of adopting one or the other of those kinds of approaches to understanding victimization. I think the typological approach seeks to distinguish among victims, that is, it seeks differences between victims, whereas the lifestyle-exposure model seeks a single explanatory mechanism for victimization, one that is compatible with multi-causation. The research strategy seeks to identify what victims have in common and how they differ from people who are not victimized.

REISS: Thank you, Mike. We will give you a chance after Dick has made his remarks to come back with any further comments. Dick Sparks is the specific discussant.

SPARKS: Thank you, Al. There is a good deal of content overlap between Mike's paper and mine. I can indicate an area of difference.

If you look at the diagram that spells out the model in Mike's paper and also, of course, in Hindelang, Gottfredson, and

Garofalo, you will see that there is a connection, [illegible]re to risk, between victimization and what is called life-[illegible]. Now, my argument is that there is not merely one link there [illegible] at least six. There are six different paths there, and t[illegible] what I want to talk about in my paper. So I won't touch on [illegible] aspect of this paper, because I think there are many [illegible] things to discuss in it.

The general question of opportunity as a precondition for victimization is, I think, important and neglected.

About three years ago I thought it was important enough to write a paper on this subject, which will be forthcoming in a book edited by Feinberg and Reiss. It will be out later this year, I am told.

The important point (and I am not entirely happy with Mike's terminology) is the distinction between absolute and probabilistic opportunity. There are some things that are logical and necessary preconditions of crime. Having property around to steal is a logical and necessary precondition for theft.

There are all kinds of opportunity notions that are not so easily done away with, though. For example, social control variables, protection of property. The ordinary person has virtually no opportunity to steal from a well-designed bank vault, but a skillful bank burglar has still some opportunities left. So that is one of the ways in which opportunities vary, apart from the fact that it is associated with characteristics of lifestyle.

Analytically, you have to deal with this in different ways. Where something is an obvious precondition, like having cars around in order to steal cars, analytically the best way is to factor that out and look for residual car theft, given the distribution of cars.

If, however, your opportunity variable is something like a social control variable or patterns of protecting property or some of the lifestyle variables in this model, then of course they want to be explicitly in the model, because what you are trying to see is what effect those things have on victimization.

The findings on multiple victimization I have, of course, no general quarrel with. I am not entirely happy that the excessively skewed distribution that we observed is in no way a function of response error. We are not yet on sufficiently

solid ground to say that some of this apparently excessive multiple victimization and of course apparently excessive non-victimization is not just an artifact of what are really shockingly bad survey procedures as far as eliciting events and their details.

But in general, I think it is probably true that we do have this long-tailed multiple victimization.

The trouble is that negative binomial distributions that fit this fairly well come out of all sorts of models, as you know, and we are not very far ahead once we have said that. The heterogeneity notion seems to be plausible, but again, I am trespassing on my own paper, so I won't do that.

I'm not sure the National Crime Surveys, or indeed any victimization surveys done today, have really enhanced our ability to study this problem, but I think they certainly stimulated interest in the problem. At the moment, owing to doubts about the error structure of those surveys, I wouldn't put too much weight on them as a strategy for studying models like this one or developing models like this in the future.

I think in particular interviewing techniques designed to elicit, even in some sort of survey, information about the fine-grained context of victimization, very different from what we have to use in a large-scale survey like the NCS, is needed.

Now let's turn briefly to the model, and here I do have some complaints. The paper is called "On the Etiology of Criminal Victimization." The model is presented as being quite general, but it's plain that there is a good deal more implied.

For example, in burglary, the house doesn't move out among potential burglars. It just sits there and burglars come to it.

There may indeed be variations in the probability of burglary, and these may change over time as contiguous neighborhoods change and so forth, but that kind of thing won't fit the model.

Now, in fairness, the original theory from which this paper was drawn was titled "Towards a Theory of Personal Victimization." I think we need to realize that, as has been expressed here and this is very important: we are talking only about personal victimizations, not about victimization against households or organizations, which can only be brought within such a model by straining metaphors and analogies that are used in the model as it stands.

I think a more serious problem with this approach is that it won't even apply to all personal crimes. It probably won't apply to most assaults and it probably won't apply to most robberies or many robberies. It is best designed for stranger-to-stranger personal thefts. Assaults in particular come out of relationships between victim and offender, employee-employer, landlord-tenant, student-fellow student, and so forth--where it is not merely a matter of exposure to risk in the sense that two people are physically contiguous for some period of time, but rather that they get angry with one another and hit each other over the head or whatever they do.

Indeed, I think a way to characterize the model is that it reminds me of the kinds of models used in mathematical epidemiology to explain disease transmission. You know, you have so many infected people here and so many susceptibles there, and they mix and the disease is transmitted, and some become immune, and so forth and so on.

Now, that's fine if you have so many sorts of criminalistic people and criminogenic types, and then you have so many of what Ellenberger called victimogenic types, and these, the criminals and the victims, come together, and they do their thing on each other.

That may be an appropriate model for certain sorts of particular personal theft. I question how far we ought to approach the explanation of other kinds of crime with that kind of essentially atomistic model in mind. At a minimum, I would suggest the diagrammed model needs amendment by an arrow between associations and victimization, and probably something in there to indicate role relationships as well as role constraints on an individual's behavior.

I am certainly in agreement with the general logic that we need to embed victimization in the victim's experience, in their social and personal contexts, whether we characterize these as lifestyle or however we define them; and should the attributes of people or their behavior change, we would expect that their victimization changed, even if the association between victimization and those characteristics remained constant.

I think there are extreme difficulties, as I have said, in doing this with large-scale survey data. This is particularly true for cross-sectional surveys, like the surveys from which this model was originally derived, since there are enormous problems inferring causal order.

The question of injury to victims and self-protective measures is one that illustrates that very well. Did the association between self-protective measures and injury arise because the victim in some sense precipitated the crime? Without very different kinds of data we are never going to be able to settle questions like that.

As for the final suggestion, that there is a distinction between typological research in victimology, the kind that seeks to see what's different about different types of victims and the kind of research that says, what do all victims have in common, I think I would go with some sort of a research approach that is more typological.

As the fool in Lear said, "I'll teach you differences." I think differences are what we have to be sensitive to. In the case of different kinds of crime and victimization, we need to guard against the assumption, in doing research, that there must be some common denominator that links all kinds of victimization together.

REISS: Mike, you have a chance to respond before it's open to general discussion.

GOTTFREDSON: I think practically everything that Dick said is important and is very worthwhile to consider. I enumerated four things that I would respond to. First, I think there's general agreement that there is error in the data, the victimization survey methodology certainly, and we don't know the extent of that. We don't know all the ways it might affect our data.

The same is true, of course, of official statistics, and that indeed response error is one of the things that we have to buckle down and study directly. I think the question from the etiological point of view is always: To what extent do the errors in the data disrupt the correlates that are useful in explaining victimization? When you have marked correlates, the logical conclusion is that you need marked disruption on the basis of error to destroy those correlates.

To some extent and for some kinds of things, the other way to try to get a handle on the error structure is to look at indicators that are different kinds of measures of criminality. The extent that you have some compatibility among those indicators, you might have more confidence that the relationships are true and are not largely an artifact of error. I think for some things we have that kind of assurance about criminal victimization, but that is not to deny that we need some more measurement studies. I think that goes without saying.

With regard to the generality of the lifestyle exposure or the routine activity approach, I don't think it's a theory. I think it's a way of looking at victimization, a way to order our investigations. We did call it a model for personal victimization. I do, however, see much more generality. I think maybe in one respect it might have been too modest. I do see some generality in the model.

Although it's true that households don't move for burglarly, for example, the people who live within the households do move. They move out during the day. They move out during the night. In Cohen and Felson's terms, the household may be guarded more or less. To the extent it is more or less guarded, it may enhance or decrease its exposure to crime. And I think for commercial kinds of victimization there are analogues that can be played out, so that the model, I don't believe, is specific to personal victimization.

With respect to its applicability to non-stranger victimization, I think that obviously we need a lot of work in that area, but I think the model can be generalized to those events to some degree. What I argue in the paper is that there may be certain things that people do or say to one another in an encounter that enhances their exposure to victimization. I think this is related to the victim precipitation kinds of ideas that, once exposed to high risk people, situations, and times, there may be some things that victims do to enhance their exposure over and above that by saying certain things or reacting in certain ways. Some things can be derived from the lifestyle exposure model better than others in that regard, and I think we do have to go beyond it.

My arguments with respect to the typological versus a common explanatory mechanism are not meant to denigrate the typological approach. It's obviously important to flush out differences among different kinds of victims, but I think it should be stressed that the lifestyle exposure or routine activity models that seek a common explanation for victimization also seek differences. They seek differences between people who are victimized and those who are not. They need to concern themselves with differences, to the extent of being scientific.

REISS: All right. It is open for general discussion.

SINGER: I see the lifestyle exposure model sufficiently loose where the adaptations account for non-stranger-to-stranger as well as stranger-to-stranger violence, as opposed to a routine activities approach which speaks more to the opportunities that

Richard Sparks referred to. I see it as a very loose model in that sense.

I would hate to think we are making it overly rigid in knocking out many of the subcultural factors or non-subcultural factors besides the opportunities that you referred to.

GOTTFREDSON: I agree.

SKOGAN: One interesting problem is that of the policy implications of Gottfredson's model. If one explains increasing rates of burglary by fundamental demographic factors, such as changes in household composition, declines in the size of families, and increases in the number of single-person households (the Cohen and Felson model), what are the policy implications? Decreasing fertility rates? Or convincing people to not live by themselves, but getting roommates? While interesting for analytic purposes and very useful for forecasting, as structural and demographic factors lend themselves to very elegant forecasting models, the policy implications of this research are a bit obscure.

GOTTFREDSON: I think policy implications for this kind of model might be more direct than policy implications for other kinds of models. When you concentrate on what I refer to as absolute exposure, it may lead you to some directly manipulable kinds of variables. We have always treated demographic indicators as rough and very crude indicators of the central theoretical construct of lifestyle. Although they are not as easily manipulable, the central construct may be. There are some basic kinds of policy implications. For example, do you go out at night alone, or don't you? In the company of friends? Should you encourage people to do that kind of thing?

GAROFALO: Responding a little bit to Wesley, we do have national policies, of course, that affect things like accumulation of consumer goods, family structure, mobility, and so on and so forth.

Perhaps rather than trying to think of how we can take victimization outcome and trace it back, and say what implications do these have for broad policies, maybe it would be better if we just think about trying to get policymakers aware of the fact that crime is one particular type of output that is affected by these policies rather than try and focus the whole thing on crime.

These policies have a number of effects; crime is one of them. When the implications of policies such as those that affect family structure and so on are discussed, crime is not one of the implications that are generally brought into the discussions.

BIDERMAN: I think we can raise the question of the policy relevance of statistics. The individual criminal victimization retrospectivily is not--or at least very rarely--illuminated by any probabilistic calculus. You can explain almost any individual incident. In fact, very few people I found attribute incidents involving personal injury to accident. There was always some history that explained it--"Oh, I was told I shouldn't go there," or "I was told to keep my arm through my purse handles." You can always explain an individual case, and you pretty much explain it completely.

Now, statistics is a method of dealing with ignorance, when you want to deal with a large number of cases in some general way. We are ignorant in many ways. We're ignorant because of the heterogeneity of events makes it impossible to specify, given our theoretical models, either the detail or the level of generality that is required for a fair degree of predictive accuracy.

BLOCK: I would say that you are correct, that statistics probably haven't been a very good guide to life choices. Now whether they could be a good guide to life choices, I'm not sure.

The function of victimology may be to define who isn't a victim, or who becomes likely to be a victim, and then what can we do to prevent it, from a citizen's perspective. People are mainly not concerned about victims, but preventing their own victimization and preventing injury. It seems to me that what should be done is not public policy in the sense that we can really alter through governmental action individual behavior of victims. I don't think we can do that very much. We can inform people on a probabilistic basis what things are more or less likely to result in them becoming victims.

This is mostly common sense. When we were studying behaviors that resulted from victimization we found that when people were burglarized, they said they locked their doors at night now. Well, that's common sense. But it may be useful to remind people that there are behaviors that they can do to alter the probability of victimization in a particular setting.

Certainly you can't tell them you are in the wrong side of a negative binomial distribution. It isn't particularly a useful thing, but people are interested about being victims of crime. I think the role of government should be to inform people on a probabilistic basis and be sure that the probabilities that they are talking about are fairly accurate. That's the problem with victim surveys and police data.

SPARKS: I think this question of the usefulness of statistical data to the explanation of individual cases may be a little more complicated than Al Biderman allows. It all depends on what you mean by explanation. Of course, you can tell the story, and people may impute, what were in fact accidents, to their own carelessness or whatever. There are plenty of cases in which people appeal implicitly to generalizations that are wrong. Part of our job is to discover those generalizations that are right, and to tell people that they are right and that the other ones are not.

For example, somebody says, "I know I got my house broken into," or "I got my car stolen, because I didn't lock it." Now, if it should turn out that merely locking your car doesn't affect the chance that it will be stolen, then that person is appealing to something that's wrong. And we can tell him that. That doesn't affect the point that, especially in the present state of knowledge, we need to be much more concerned, both for explanatory and for public policy kinds of issues, with individual variations, with things that are unlikely to come out of aggregate data.

REISS: I want to extend a bit a point that Dick Block raised about the generality of the theory. There is a part of both of these theories that talk about intersecting entities, victims and offenders, and in that sense, whether or not the victim and offender are stationary, has some relevance.

I think that we need to think about three possibilities and then some variations on them. There is the case in which the victim and offender both move, and they intersect somewhere out there, in some kind of space. It may be a restricted space. We both move to work, and my offender works in the same place I do and intersects and steals my purse or my wallet, or whatever. That happens all the time with kids at school. You see the victims and offenders both move to schools, and there's a lot of crime that occurs because they intersect in the schoolroom.

Then there's the case in which only one moves, the victim or the offender, and either can be stationary. That is to say, victims move to stationary offenders and vice versa.

Then there's the very interesting case in which the harm moves, and the victims and offenders are both stationary. Some of the most serious things can happen to us because the harm moves and we're stationary. That is to say Three Mile Island is stationary out there, and I'm stationary somewhere else. We're criminalizing some of that conduct now, because it can be so catastrophic when we have stationary offenders in relation to relatively stationary victims.

Let's go back, however, to the case in which victims and offenders may be stationary. We are relatively stationary when we're locked into certain social relationships. The interesting things about child abuse and spouse abuse as compared to other kinds of assault are that you reduce your victimization by getting away--that is to say by moving out of the space in which you are. The coerced parent-child relationship and the dependency, say of a two-year-old on a parent in a household, is exactly the setting of victimization. You aren't capable of moving away from it. The more you are capable of moving away from it, the more you can escape it. This leads to other types of offenses historically, like running away from home. Indeed, the way to escape your victimization, to reduce your risk of being victimized from a coerced parent-child relationship, is to run away.

If you want a general theory, what you have to look at is a theory about where intersection increases victimization, where it's irrelevant, whether you intersect, and so on.

The trouble with the current models as I see them (the lifestyle model or the Felson model) is that they are based far too much on the movement and the intersection out there.

SPARKS: I was going to say your Three Mile Island case is not just a case of not moving. It's a case of a relationship between victims and offenders, namely a power relationship that enables these people to dump all this gunk into the air. Similarly the child-abuse case isn't just a case of running away from any household relationship, it's getting out of that particular household relationship. So the kid goes to a foster home. Your explanation is in the dynamics of the particular relationship and not just in the physical movement.

REISS: Absolutely. You have to look at what intersections mean. There's another theory, a search theory based on how two people go out in the environment and on their search behavior. There you get certain, almost stochastic processes going on from the standpoint of the victim whereas the offender is always looking

and storing, and waiting to find a victim or victim opportunities.

Because the offender group is continually moving and looking at the environment very differently from the victim, the victim becomes vulnerable to the offender's search behavior. That's another kind of intersection theory. The intersection theory that you develop is what is critical here, and the assumptions it makes about the degree to which relationships or positions are fixed as necessary conditions. I don't think you can ignore that.

An organizational position is fixed, even though I can move it. The decision to move it is the critical one. Let me put it another way. One of the critical things in victimization theory is that the extent to which victimization causes you to move is a simultaneous problem with the extent to which having been stationary is, in itself, causing you to be victimized. So we have the simultaneity problem that we have in a great many theories. I move to get away from victimization but the movement may, in fact, cause the victimization.

SCHNEIDER: The generalization of our theory depends, to some extent, on its origins. I like lifestyle and routine activities theories because they point to some interesting new directions. To some extent, however, these have arisen out of old research agendas--agendas that produced the kind of data in the current victimization surveys. These are inductive theories arising out of previous research which was not based on much theory at all.

If Wes Skogan's stories about the origins of some of the questions on the national crime survey are correct, then it seems that the dependent variables were getting a lot of attention but the independent variables were not.

These kinds of theories are restricted to the kinds of offenses and the kinds of independent variables covered in the old victimization surveys. As we move towards new research agendas, we ought to take the inductive theories and merge them with more deductive theories so that we point in some new directions.

BLOCK: Don't you think we will do that?

SCHNEIDER: Yes.

BLOCK: Now we have a foundation, and the deductive theories are coming. I think you are right when you talk about interactions. This is a causal model, and it's really an interactive process

we're looking at. It's the intersection, as you said, of victim and offenders that we should be looking at. Lifestyle and exposure and victimization interact; they don't follow from one another. That's my objection to the figure that you present.

GROPPER: I'm wondering about the state of the art in models that may not aspire to broad generality, but that might still provide significant explanatory power for some purposes, if developed in greater depth. Such as ones that attempt to characterize the major behavior patterns of specific groups like the elderly or minority youth.

Might significantly different issues play such dominant roles for specific sub-populations that we might approach models of their behaviors, at least as an interim strategy, as being in qualitatively different categories, even though we also describe them in quantitative terms. For practical and theoretical reasons, we would want to be able to resolve any inconsistencies and integrate the concepts. But it isn't clear whether this might be productively done as parallel efforts or later.

SPARKS: I think such a group as the elderly that can conveniently be demarcated by a simple variable, namely chronological age, fits very nicely. That's, perhaps, not qualitative, but certainly quantitative and supported by all of the existing data I know of.

You ask people how often do you go out each week, you get a negative association between that and age. Partial that out and the elderly victimization rates will rise a little because they're being damped out by the fact that they don't go out. Qualitatively, no, we're not there yet. I think you need to look at the types of things that people do when they go out.

You know, it seems fairly obvious that the elderly don't go to rough bars where they pick fights with people. But we don't have data on that yet. Is that what you mean by qualitative types?

GROPPER: I'm thinking more of areas where my kids may claim I'm not really asking the right questions, because they feel I approach the world in different and untranslatable terms than they or their peers do. If there is any validity to that, beyond generation-gap rhetoric, then it's not so much like viewing the world from different scale positions within the same conceptual framework as it is like having orthogonal sets of factors. Are there any approaches you feel we should be discussing which even if they don't work very well in accounting for the behaviors of the

general population, are still more than trivially meaningful and useful for explaining the characteristic behaviors of some important sub-populations?

BIDERMAN: The heterogeneity and homogeneity of the data set we use has a lot to do with the fit, the degree of empirical fit of the data that Mike Gottfredson has used in talking about this paper. I don't know the degree to which he got that kind of discussion into this paper, but there is the paper by Nelson in the current American Journal of Sociology on the negative binomial, which makes the obvious point that the more heterogeneous the mix of the probability functions, the less likely the Poisson is to fit.

Now in the National Crime Survey, the alternatives as to where an event happened that encompass at-home and near-home exhaust most of the incidents. I forget what the proportion it is of all incidents.

SKOGAN: That is true only for property theft.

BIDERMAN: I recall a memorandum from the staff complaining about the National Crime Survey analysis, complaining about that item, because those two alternatives exhausted so much of all of these location replies.

REISS: That's because you bias the locations. You don't throw commercial ones in.

BIDERMAN: Now I would like to introduce another bit of data about that. They have the street item which is heterogeneous. It includes parks, playgrounds and so forth, and they have school as a separate item.

Now at-school picks up a very tiny part, I think it's under 2 percent, of the cases of the incidents. Now the National Institute of Education survey, administered in schools, produced rates for the major crime classes including assault and larceny that are covered by the scope of the National Crime Survey.

The rates in the NIE survey for a month are about the same as the rates for a year for the pertinent NCS age classes, the same age range. The annual rates for the NCS are about equal to the monthly rates for the NIE survey.

Now that has a good deal of bearing on the degree to which an instrument is directed specifically to what goes on in a particular domain and that it breaks out and gives the respondent a chance to respond to what goes on in a domain.

If you have a work survey of victimization, you would again increase the rate of victimization away from home. You had a house survey and you're going to concentrate very much on the incidents at home. The orientation to the particular phenomenon of the measurement instrument has a great deal of influence on what we're deriving with regard to the application of this kind of general model--binomial probability models.

We can concentrate them. If we ask about victimization in the context of the annual housing survey (there is a little bit in there), then, again, we would increase, enormously, the close-to-home component relative to the away component.

I'm very upset, generally, about the character of the work reported in the degree that it does not pay attention to the fact that the data are not only reflections of what goes on out there in the world, but what goes on in the data collection situation. Data have to be interpreted in terms of the data collection mechanism, our observational modes, and the theories have to be related to the operationalization of concepts.

REISS: I'm biased against neighborhood concepts, that is, locational concepts and theories generally. We need to think much more about them, like the type Boggs set forth about how offenders pick victims in terms of neighborhood characteristics--that is, low income offenders can get higher payoffs in high-income than in low-income neighborhoods so they move to the closest, usually adjacent, high income area. Such hypotheses are worth exploring.

GOTTFREDSON: I guess I could identify three things that emerge from our discussion this morning. There are at least three things where at least some degree of concensus exists about research agendas. I may not understand everybody's point of view correctly, but there was a good deal of agreement that we need more specificity with respect to where people go and so forth, and where victimization events occur.

The kind of broad crude categories we've been dealing with don't seem to be adequate. At-home, on-the-street, those kinds of categories were useful initially, but now we need to know quite a bit more about the specific time and space dimensions.

There's quite a bit of agreement about the role of interaction between the participants in a criminal incident. That kind of drama, we really don't know very much about, on the basis of the indicators we have now. We don't know how that interaction takes place with respect to the specific moves, countermoves;

where it leads, and so forth. I think we all believe we could benefit by knowing more about those kinds of things.

In general, there is a concern about the response error, the error structures and the data we have now and I think everybody is concerned that we come to grips and do research that helps us to identify those error structures, not as a research agenda per se, but in terms of how those error structures alter, in significant ways, out substantive results.

REISS: Thank you Mike. On the last point I just want to remind us of a general problem most of us, or a good many of us, are going to face in this group. We live off the national victim surveys, and therefore we tend to think in terms of the problems of measurement, data collection and so on, in terms of the victim survey as if there weren't alternatives out there.

A great many people out there are not BJS (Bureau of Justice Standards) people. They are not interested in the victim surveys per se. They may be interested in alternative methods of data collection and analysis and so on. So to the degree that we can turn our backs to our livelihood, we may be more helpful. That's partly the hobby-horse we have been riding lately, but I think it may be more helpful to people here.

Wes Skogan is going to begin by talking about things other than his paper.

SKOGAN: I will talk briefly about things that people do to reduce their chances of victimization and to manage the risks when they are exposed to the threat of victimization.

There are a variety of ways in which you can look at the role of citizen behavior in crime that may have significant policy implications.

First of all, one can look at individual and household behavior as an antecedent to victimization. In verbal models of victimization this is how we explain why individuals and households are victimized. When we talk about crime and the elderly, or the victimization rates of women as opposed to men, the verbal models we commonly employ are dominated by discussions of exposure to risk. For a variety of reasons, most of which don't have to do with crime, women and the elderly lead more circumspect lives than their counterparts, and as a result they have a greatly reduced exposure to risk.

We also refer to individual behavior when we talk about victimization proneness, in particular, multiple victimization. As Richard Sparks indicates in his paper, there are great differences across the population in what he calls "gamma," or transition probability--that is, one's chance of being victimized more than once. In many cases proneness to victimization can be linked to role-constrained behavior, when people do things often even though they don't want to do them, things that expose them to risk of multiple victimization.

Thus one view of individual behavior is as an antecededent to victimization, as a way of explaining risks and the particular crime problems of specific subgroups in the population.

We also usually focus on individual and household behavior when we try to gauge the consequences of victimization. For example, one of the things that victims do, is change their telephone number, and another is move. This creates problems for prosecutors and it creates tremendous problems for the National Crime Survey, for neither can keep track of victims and hold them in place long enough to make the fullest use of them.

At a more subtle level, people who are victims engage in withdrawal from community life. They avoid suspicious persons, and expand the content of their conception of what a suspicious person is, leading to a decline in the quality of their lives and on overall reduction in activity at the community level.

We also know that victims, as a consequence of their experience, engage in lots of communicating behaviors. The first thing people do when they're victimized is tell everyone they know about it for as long as they can keep their friends' attention. This is one reason why if you ask whether or not people have been victimized in the last year, you find relatively low rates of victimization, but if you ask people if they know a victim, between 40 and 60 percent report knowing a victim of relatively infrequent crimes like robbery and assault. So the indirect consequences of victimization, in terms of its effect on individuals and households, can be quite dramatic.

When we raise our eyes from the individual to the neighborhood, one of the ways of thinking about behavior is the way in which it describes the structure of opportunity for crime in a community.

For example, take the three fundamental concepts that came out of CPTED research. One is surveillance behavior--the extent to which individuals keep an eye on their person and property,

kids in the neighborhood, their neighbors' houses, and generally suspicious behavior. The second is territoriality, which is intervention behavior. These are proprietary kinds of behaviors that people undertake. The third is access control, which is the CPTED term for keeping doors and windows locked. Surveillance, access control and territoriality are three very useful ways to think about the aggregate responses to crime of individuals and households in a neighborhood. We use them to explain differential rates of crime across neighborhoods and to explain trends of neighborhood decay and revitalization.

Thus we can focus on the antecedents and the consequences of behavior, and we can look at the neighborhood level, at the collective sum of those behaviors, and the consequences for the community as a whole. The next issue is that of the policy implications of all of this.

At the individual level there has been research on the antecedents of victimization which is aimed at telling people what to do if they are attacked. It seems to me to be the most dubious kind of research. The NIMH Rape Center has funded a number of projects which involved interviews with rape victims in order to look for crucial points where various kinds of victim strategies would enable people to thwart a victimization. Richard Block has been guilty of this in his analysis of police files in Chicago. While at the individual level there have been these "policy-linked" studies of individual behavior, I think most of the interesting work has been conducted at the neighborhood level.

There are really two kinds of research at the neighborhood level which have been pursued to date: there is a genre of evaluation research concerning what works, and there is a genre of research which is essentially implementation studies. That is, once we know what works, how do we get people to do it?

In the "what works" category are a variety of evaluation studies of individual and household behavior, property marking programs, block watches, CB patrols, and the like. The second collective level question is: How do you get people to do it? These are etiological studies of collective behavior which study the causes of behavior in an implementation sense, trying to figure out how to motivate people and organizations to do the kinds of things which will reduce their chances of victimization.

In my paper, I talk at some length about problems in conceptualizing behavior, and I criticize current research in that

regard. I then review five major methodological problems in studying individual and household behavior, talk a bit about the problems of modeling behavior, and finally, discuss the problem of displacement.

For reason of brevity, let me just talk about one of those four things that I discuss at length in the paper, the question of modeling behavior.

It is clear when you look at how we think about victimization at either the individual or neighborhood levels, that we think of victimization, individual behavior and assessments of risk, as reciprocally related. People, when victimized, reduce their exposure to risk as a consequence. In the victim surveys, when you look at the relationship between victimization and behavior, you find that victims report themselves as being less exposed to risk than non-victims. One way you can interpret that is as a reduction in exposure to risk as a consequence of victimization.

Over time, the relationship between victimization and exposure to risk involves causal arrows running in both directions between the two, and you can see the same thing at the community level as well. Stable communities are places characterized by negative feedback. In stable communities, when particular crimes occur, that stimulates mobilization at the household and neighborhood level. This activity then reduces the collective risks of vulnerability of the community in the next time period, bringing victimization rates back to previous levels.

We can think of good neighborhoods as places which have iterated to a stable solution in the relationship between behavior and victimization rates. Bad neighborhoods are places characterized by positive feedback; that is, responses to crime like withdrawal from community life, flight from the neighborhood, declining property values and the like, reinforce the criminogenic forces which had started the cycle. This is a positive feedback system which involves very rapid neighborhood decay.

Thus we can see at both the individual and neighborhood level processes involving reciprocal causation. While there are some elaborate techniques for studying reciprocal causation in a set of cross-sectional data, they are not very good, especially for data that are filled with error, like survey data. The study of victim behavior and its consequences will of necessity involve a panel analysis, if only because we best measure behavior in the current slice of time: What are you doing now?

What are your typical behaviors now? What is your current pattern of activity? However, we almost of necessity study victimization retrospectively: What's happened in the last six months or the last year? Victimization surveys examine past victimization events and current exposure behaviors, and thus we cannot get at the kind of reciprocal linkages that are required to study this kind of problem.

What's really required is panel data, where one interviews people, and then gauges their experiences between that interview and the next interview, making it possible to tease out both the antecedents and the consequences of victimization. Currently we're largely stuck with data that speak only to the consequences part of that reciprocal relationship.

Another modeling problem, it seems, is that we have tended to treat behaviors as invention rather than diffusion phenomena. A review of the research which examines the correlates of individual and household behavior, indicates that these tend to be viewed as independently invented; that is, they look at the characteristics of persons, their individual resources, and their assessments of neighborhood crime problems, and use these as independent variables to explain who does or does not a particular kind of crime reduction activity. It is more likely, however, that rather than individual invention, these things spread through communities by diffusion. Diffusion models of behavior really focus on quite different kinds of variables than do invention models of behavior.

Diffusion models of behavior focus on the connectedness of persons to communication networks; they emphasize the characteristics of earlier adopters as opposed to later adopters; they focus on the role of entrepreneurs in the spreading of those behaviors.

This is true if you're studying hybrid corn, and it seems to be true if you're studying programs like property marking. Thinking seriously about these things as diffusion rather than invention processes will lead us to collect different kinds of data. This also is a better perspective for policy-oriented research. Seeing crime reduction behavior as a result of a diffusion process would focus research on the entrepreneurial activity and on the unique characteristics of earlier adopters. The research would give policy agents a much more direct handle on how to encourage the kinds of programs they're trying to sell.

REISS: Thank you, Wes. Bob Woodson is going to be our first discussant on this paper.

WOODSON: I don't know if I was invited in the mistaken belief that I am a researcher, which I am not, or because I am a practitioner, which I am. I address my remarks to the theme of this conference and that is: what is the theoretical base or conceptual framework that will guide or influence action, the action of a research agenda for the Institute.

Mark Twain said, "If the only instrument that we have is a hammer, then all problems look like nails." I think this relates to the whole topic of research.

I would like to respond to the practice implications to which I am very, very sensitive. I have just completed a three-year study of several neighborhood organizations' ability to interrupt the cycle of crime, and violence of serious offender kids. My research was informative because I visited those communities and lived with some of the people and the kids and I tried to understand the phenomena of how they interrupted the cycle of crime. I now have more understanding of what they have done.

What has to happen is an activity that is different or extends beyond what Wes has just identified, and that is not limited just to approaches of target hardening and surveillance and intervention. We try to interpret neighborhood actions and then assess how we can get people involved.

Neighborhoods do a great deal more than site-hardening activities, and their approach to victimization is altering the behavior, the values, and the outlook on life of the victimizers. Thereby, the whole issue of displacement is eliminated since these are neighborhood kids who are creating the problems and therefore they are more accessible.

This is not done by the liberal approach that Wes identified in private conversations as "kid programs"--providing recreation or other busy activities for these youngsters. The neighborhood programs engage these youngsters in a process to improve their self-image as it relates to their culture and their value system.

I think researchers operate as if all people in this country live in a cultural vacuum. We don't take seriously the complications of looking at the culture and the ethnic context in which people live. This is most important.

Other areas of activity that contribute to victimization that need some serious study are violent crimes within the

neighborhood and family violence. We know that 50 percent of the number of police who are killed or injured on duty occurs as they intervene in neighborhood or family disputes.

Over the years, some neighborhood organizations have successfully intervened in those situations by relying on a limited understanding of research and statistics. By talking with the police they knew that, if calls were received two or three times from a family, the third or fourth incident usually results in violence. So they recruited people in that neighborhood who knew the combatants and when that second or third call came they intervened and talked with the people involved. This had some very measurable consequences in reducing the number of police homicides.

Researchers are not available to measure the impact of this kind of behavior. It is another area in which there would not be displacement, because of the nature of the activity.

So, I think it's important for researchers to begin to take the activities of neighborhood people seriously and to go beyond the exercise of site-hardening and start to understand that some very profound things occur in the communities, activities that lend themselves to measurements.

In order to devise solutions to these problems, we have to understand that knowledge in the criminology field has to be consistent with improved information given to policymakers. I don't think it is important to spend more money on research. This increases the political problem of research today, where judges, prosecutors, and laypersons look very critically at abstract research projects and agendas that interest researchers but have few practical implications.

I would like to end on that point and perhaps other people could challenge Wes more specifically on this paper.

SKOGAN: I think what Bob Woodson has pointed to in my discussion is the focus on individual and household activities. In my mental map of these things, there is another category--collective activities. There has been a substantial amount of research on the kinds of neighborhood arrangements and characteristics of neighborhoods that encourage individual citizens to participate in these kinds of programs. Bob Woodson is calling for something further, an assessment of the consequences of these programs for their clients and their participants.

There has been a fairly substantial amount of work on the motives and characteristics that encourage individuals to engage in organizing activities, to become involved in community efforts, much of which suggests that the relationship between community disorganization and community organization is complex.

In highly cohesive communities there may be relative low levels of collective organization. There, alternative mechanisms like family and church satisfy the functions of formal organization. In highly disorganized neighborhoods, for quite different kinds of reasons, there is not much formal group involvement. In the salvageable neighborhoods in the middle, organizational levels are high. In addition, collective activity is always high in black neighborhoods.

WOODSON: In order to consider such an area of study important or worthy of time and attention, it means focusing on existing neighborhoods' strengths instead of pathology. I think that's a very important dimension in viewing the potential for collecting useful information.

We look at the two children within a family out of six who failed and find them in the criminal justice system. We don't look at how the other four survived and were able to overcome very desperate situations. Perhaps we can build on those positive strengths to inform us how to support such efforts and disseminate the knowledge within the field.

SPARKS: I think this discussion points to an important area where there is great potential for research which is not survey research--and this comes out of many of the things in the discussion of Wes Skogan's paper as well. If you want to know how people act in day-to-day situations, you can either ask them how they typcially act, you can ask them what they did and make some inferences from that, or you can go out and watch them.

There is a danger that we are focusing on the asking strategies to the exclusion of ethnographic work. We don't even have baseline data on a lot of these things that we could collect easily. You ask people how often they go out, they will tell you one thing or another. You could also sit there and watch their house and see how often they go out.

A few years ago, some researcher wanted information on comets or asteroids, and persuaded the Signal Corps to set up television or movie cameras that just took pictures of sample areas of the sky and they ran these cameras for years and years and they wound up with around six pictures that would do.

Well, there is nothing easier if you want to know how many people are out on the streets than to stand there and count these people. But we don't have this information.

At the family level there is a lot of work done by people like Gerry Patterson and others on troublesome families. There is no reason why these techniques can't be applied to family or workplace organizations: how often, when the secretary goes out, does she actually lock her purse in the drawer, and things like that. I would like to see a whole lot more of that done.

SKOGAN: Most of the observation work that's really interesting is done in evaluation mode. The difficulty with observation is that you cannot judge the motives of actors. In an evaluation mode, one can vary crime through the manipulation of the research design and then go out and count things. Then it becomes a very powerful kind of tool.

Jack Fowler has done this with a less powerful research design in Hartford, and I think quite usefully. There has also been a great deal of work done in Portland in evaluating a commercial project by looking at changes in the use of commercial strips with time series observations scattered across randomly sampled time points in terms of who is actually in the street. The evaluation mode, I think, is a very powerful tool.

SPARKS: I think the conceptualization may not be adequate, but if you are worried about implying intention and motive, I don't think that's as much a problem as you say it is.

REISS: You don't need to opt one way or the other. I thank Dick for pushing systematic social observation, since I usually feel like a voice in the wilderness in my papers on that. As I point out in my papers, you can observe and interview in the same study. You can go out and observe people who are in a shopping center and what they're doing and then you can stop and ask them where did you come from and find out how many people came from high-crime-rate areas, how many from low-crime-rate areas, and so on.

So, you don't need to be locked into an evaluation design where you're either simply looking or you're asking. You can both look and ask. You can be out there on the street and stop every fifth person whom you're observing, as well as putting down your observations on them.

This notion that somehow we opt for one tool or another is only a figment of our imagination. It's not built into the data points.

SKOGAN: We're doing that now, in Chicago. It has lots of interesting procedural problems, largely with sampling issues.

EPSTEIN: We tried to do that in one or more studies, and what we get is, that it's a lot more expensive to do than straight survey. I think a number of the universities are reluctant to do this. Do you know ways that it can be done without great cost? Do you know places that are willing to do this?

We had a lot of resistance to doing these kinds of things. They say it's harassment.

REISS: Unethical?

EPSTEIN: Unethical.

REISS: I said recently, Sid, that I would have lost two-thirds of my vita if the human subjects committee had existed. I made my career out of violating all of the things that you now can't do.

SPARKS: I don't agree with that. It seems to me that the observation of public behavior is not something that would require anybody's permission.

REISS: I agree. I said so before the National Commission for the Protection of Human Subjects in Biomedical and Behavioral Research.

The new subject rules, as a consequence of some of us battling, exempt public behavior.

WOODSON: I am not so sure that the issue has to be limited to observing people and obtaining their permission or getting into arguments as to whether you can do that. I think within every community as in every human body there are antibodies that naturally do ward off disease. The question is what is the antibodies' nature? What is it they do to heal the body politic or the neighborhood body or the individuals therein?

It means working with the neighborhood people and helping them understand what they do and analyzing information about why they are successful. This doesn't present any ethical problem.

I would say that one of the reasons that you don't find people anxious to do this is because it requires going into the neighborhood. It requires listening to people, and perhaps

acknowledging that there is some phenomenon out there that the researchers and others don't understand. So it means losing something on 42nd Street and searching for it on 57th Street because there is more light on 57th Street.

So you find yourself saying, "I'll include the techniques for researching them on 57th Street," when my contention is the answers are not there, no matter what sophisticated techniques you might employ.

ZIEGENHAGEN: It seems to me that the direction of our conversation is really running towards the idea of social control mechanisms, and this is what we have not paid a great deal of attention to, given the structure of inquiry that has been tied to the large-scale surveys.

This doesn't mean that it can't be done. I think it can and we can come up with a very viable combination. But it really means a reordering of the categories of variables that we have and the priorities that we assign to them if we have to go back to the question of how incidents are defined and who defines them. That sounds like an awfully elementary kind of point, but I don't think we can really escape that.

Then we have to really go back to the question of what significance persons assign to the losses arising from incidents. What do you do about it? Finally, if you do anything about it specifically, what is it that you do within the normative context of that community?

That's something very different from what we have been doing before.

REISS: You know when you talk about intervention in terms of social control, there are some things that people do such as experiments. Police do experiments. They put decoys out there.

The interesting thing is, all we know is about the people who responded--that is to say, who got picked up. We don't know anything about the people who passed the situation by and never took advantage of it.

SPARKS: The honest Congressman.

REISS: That's right.

All of the information is on those who engaged in the behavior and none on the part of the people who conformed in that

situation or who were honest. So it's another dimension that I think runs through all of the research.

We're always looking at the take and not at the non-take.

SKOGAN: Even if you take the most dangerous places in New York--take these very high-crime subway stops--when you compute victimization rates based on the number of people who pass through them, you find that the individual risk rates of victimization are extraordinarily low. If you take a look at the elapsed time between crimes, what you find is that even in these very dangerous places almost all the time, nothing is going on.

REISS: Right. People aren't necessarily going to throw up their hands and say, "Well, you've got 99 percent out there nothing happens to," or "The time between incidents for any one person is a very long time." We don't try to sort that population in terms of whether they took any action to reduce victimization or didn't. That's the critical thing.

It seems to me too little research has been done on the question of looking at that larger population and asking that kind of question. We asked them, did they have a lock on their door or that kind of trivial stuff, but not what kinds of actions they take such, as "I never park in that parking lot" and so on.

For all the non-victims or the persons who had longer times between incidents, what was their behavior like that was risk-avoiding or precautionary? How do you really study precautionary behavior?

BLOCK: I would say that our discussion of this paper centers around two things. One is, what is our unit of analysis? Should we be looking at individuals or communities, or should we be looking at a combination of individuals in communities? We have neglected, because of methodology, the study of communities.

Our emphasis has been on the individual and on victim surveys of the kind we have now. That's because we were concerned with incidents.

The second thing is: Should we perhaps refocus what we're looking at, from victimization, to how people prevent crimes or how communities prevent crimes or other units of analysis?

It seems to me once again, looking at this intersection--and I think Mr. Woodson was talking about the intersection of

victims and offenders, once again--we have to look at the things that don't intersect. Why didn't they intersect? Why is it that some people have a high probability of victimization while most people don't?

BIDERMAN: Wes Skogan made the generalization that people when victimized reduce their exposure and reduce thereby their vulnerability and hence, their subsequent victimization. Of course, it's an overgeneralization.

Bob Woodson brought up the medical analogy of immunization. I wrote a paper on immunization.

This paper is on immunizing effects of victimization. It treats what the effects of victimization are on subsequent victimization, both immunizing effects but also the reverse--that is, one victimization increasing the other kinds of victimization--and it uses some analogies to medical models.

These can be either the deliberate or the non-deliberate kinds of reaction to victimization, individual ones or collective. Herd immunologies and other medical etiology concepts are applicable, and they can be positive or negative.

Now the instances in which victimization increases subsequent vulnerability to victimization, you can illustrate those with legions of classes of cases. So the kid who is once a victim of a bully at school becomes more likely to be a victim of that bully and other bullies at school. A shop that gets identified as a mark is a mark.

There are more subtle things. The guy who walks around the streets looking like a mark is a mark. His very victimization increases his subsequent vulnerability. You can go on. There is no end of such cases.

On the other hand, we have lots of cases in which victimization makes for zero chances of subsequent victimization. The most potent immunization for a statistical series is killing off the victim, so homicide is zero in the binomial, or the shop that goes broke because it really was hit hard without insurance and goes out of business. That's zero in your repeat victimization series.

Now we also neglect the analogy in medicine that the major form of trying to deal with immunization is to do something about the pathogen. In this case, it's offenders, and various classes of events affect offenders differently. In medicine there are various cases where the virulence of the agent

multiplies, depending on the number of victims until the process runs its course. We also do things to isolate the pathogens. For contagious diseases, if we can reduce the number sufficiently, it can become absolutely an extinct species, as has occurred recently apparently with regard to smallpox.

I can go on and on in this vein, but the major point with regard to models and with regard to information is not to let them get in the way of thinking about the phenomenon.

REISS: It seems to me that what you are saying is, taking Wes Skogan's proposition, we really need to look carefully at that and do some research on the conditions under which it holds and the conditions under which it doesn't hold, or if it holds at all.

BIDERMAN: As highly developed an area as medicine is, it is a very fruitful source of analogies and shows you can fill up just about all of the cells you have. It doesn't become an empirical case at a very general level of filling the models, but being able to have theory which sorts out the different kinds of things, the qualitatively very different kinds of things.

Individual immunization is a very different kind of thing and can actually work in the reverse from collective immunization.

REISS: I quite agree. The other thing I was going to say that's implied in what Wes Skogan was saying and what you're saying is that, in terms of future research, while people may take actions to reduce the risk of victimization, the question is: Do they work? One of the things that I found is that people seem to move to reduce their victimization, but if all you can move is to another high crime rate area, the action is pretty ineffective.

It may increase, in fact, your vulnerability, because you may know less about the new place than you knew about the old place and therefore it just increases the probability that you're going to be victimized again. I think you're suggesting that we need to look at the effectiveness. Or do I misread what you are saying?

SKOGAN: No, no. What I was suggesting is really fundamental. This discussion has misrepresented the bulk of what's in my paper, which is largely a methodological exegesis of research problems in this area.

The most fundamental problem is trying to tease out what are strongly reciprocal relationships in cross-sectional data. Secondly are very substantial problems in measurement. Behavior has been the least conceptualized and most poorly measured part of what we looked at. The reliance on single-item measures of behavior within single contexts has probably led us to falsely reject lots of consequences of programs and to lead to survey data which don't seem to be very revealing.

I think it's a very serious measurement problem.

SPARKS: I think we need to avoid preconceptions here. You point out victimization exposure: there needs to be a feedback, otherwise you can't adequately express it.

I don't think that's strictly true. There are, of course, some exponential models that explode; they go one way. There are also a lot of models in econometrics, which are of oscillatory processes. In this context here you may well find that changes in behavior, precaution-taking in general terms, wear off after a certain amount of time, and that is something else we need to look at.

This has to be done over time, but not with the notion that it's victimization that brings about behavior change which changes victimization and so forth.

SKOGAN: It's an iterative process.

BIDERMAN: These binomial models we use are very useful for constructive thought, but not terribly useful so far for being able to match with hypotheses, because there are so many hypotheses which fit them.

One of the reactions, as in medicine to disease, is a high tolerance for the victimization, and you find that the people have moved back into the inner city. They find, you know, first of all it's not so bad to be burglarized, and they get ways of adjusting to the repeated hits, and so they hide more of their stuff so that it doesn't get taken every time there's a burglary, but nonetheless, they are burglarized.

Now some of these events become so trivial for people in high victim-prone occupations, the people who have delivery routes in rough areas, for example, that they dismiss them. They are the kinds of things you may not get as much of in a victims' survey, but they are events that are occurring. What's happening is immunization to the consequences.

REISS: Not necessarily immunization. You may carry it, such as in herpes simplex II where it keeps occurring, but there's enormous tolerance by the system.

SKOGAN: There's plenty of evidence that neighborhoods can iterate to a stable solution in the relationship between victimization and behavior, with victimization at a relatively high level. Certainly Jerry Suttle's studies in the West Side of Chicago of the kinds of routines of behavior antagonistic communities develop so they can continue to exist side by side, by using space and time in different ways, suggests that places with very high levels of victimization can be quite stable and people can go on with their lives. It's the people who don't know the rules that get into trouble.

BIDERMAN: That's not universal, because you also get the hyper-reactions, as you do in disease, that one victimization intensifies the reaction to others. The threatening, obscene telephone call is very much of that kind. People build up very intense reactions to these series of threatening calls, obscene calls.

REISS: We now have about an equal amount of time left for the last paper this morning. It's Simon Singer's paper.

SINGER: In a complex society where there are few of the traditional similarities that guide social action, it is difficult to think of a single homogenous population. There is generally too much variation in the social attributes of a group. Rather, it seems more appropriate to define a given population in relation to another on a range of homogeneous and non-homogeneous variables.

Briefly, victim and offender populations can be conceived as falling along this continuum of homogeneity. Studies of patterns in crime indicate that for serious violence both populations are similar to another in their demographic characteristics and prior social interaction.

Thus, if the homogeneity of the two populations is perfect, then victims alternate, seemingly at random with offenders in their reported legal roles. We can see that in the earlier studies, especially Wolfgang's homicide study.

In contrast, the victim-offender relationship for incidents motivated by theft appears more a function of the availability of a suitable target than shared normative responses, as Sparks pointed out.

Along the hypothesized continuum it would be expected that both populations are negatively related. If there is similarity, it may be attributed to structural constraints that determine the potential victim's probability of interacting with an offender.

In his daily activities, a juvenile is more apt to meet others in his same age group who are likely to commit crime than an elderly person who is more restricted in movement with those in crime-committing years.

Thus, demographic characteristics are key variables in explaining this similarity between victims and offenders. They not only account for the variability of the victim, but for the probability that offender and victim will socially interact, as Hindelang, Gottfredson and Garofalo have shown in their analysis of the National Crime Panel victimization surveys.

A key question to a life style exposure approach is the extent to which a particular life style reflects a subcultural adaption whereby victims and offenders would be part of the same homogeneous population.

This is something that couldn't be tested with the NCP data because, of course, it just surveys for victimization experiences and not experiences as an offender or a criminal.

Thus, rotating in a web of violent relationships, victims and offenders may share certain understandings and misunderstandings that support the use of lethal force in perceived situations of threat.

Although the thesis of a violent subculture (Wolfgang and Ferracuti) is grounded in official recorded incidents that indicate the victim-offender interaction for serious assault, other measures are needed to determine the extent to which victims are also offenders involved in crime.

A few studies have already provided some indication of the relationship. In a London sample, Sparks surveyed both for victim and offenders. The only significant association, as we might expect, was from personal threat for assault or violence which ranged from simple to aggravated assault.

In another survey, Savitz, Lolli, and Rosen in Philadelphia contrasted survey victimization with juvenile arrest status and,

again, as expected, the association was significant for assaultive violence. But this included serious as well as not-so-serious types of assault.

Although these two studies provide some support for the subcultural hypothesis of homogeneity between victims and offenders, they are not specific to the serious assaultive conduct that subcultural theory intends to address.

As part of my dissertation, I examined follow-up data contained from a sample of the original Delinquency in a Birth Cohort, and this consists of the 10 percent sample.

Of the 567 cohort members who were interviewed at age 26, 106 reported having been shot or stabbed at some point in their lives.

For these victims, a clear pattern emerged in terms of their social and criminal backgrounds. First, they were most often non-white, high school dropouts, unemployed and single at the time of the survey; second, they were more frequently involved in official and self-reported criminal activity. Victims of serious assault had the highest probability of having a friend arrested, belonging to a gang, using a weapon, committing a serious assault, and having an official arrest.

Highly significant is the relationship between having been shot or stabbed and having committed a serious assault, as measured by offenses resulting in a victim's hospitalization, death or rape. It doesn't matter if I excluded rape from the categorization of committing a serious assault.

It should be noted that in the original cohort study, the most significant variable in predicting serious assault was race. It explained much of the variation in the seriousness of the offense to official delinquent behavior. Although with the self-report data, 46 percent of non-whites reported committing a serious assault compared to 32 percent of whites, part of the variation is explained by victimization.

The relationship to victimization is significant for both whites and non-whites; 68 percent of the cohort victims reported committing a serious assault compared to 27 percent of the non-victims.

When other significant indicators of offender status are included in a logit analysis using Goodman's procedures, the victim experience proves to be the best predictor, wiping out any relationship between assault and serious offender status.

In accounting for the observed variation in offender status, victimization, gang membership and weapon use provided the best model for explaining the observed relationship between offender status and race.

Although there is homogeneity in the hypothesized subcultural relationship between victim and offender, it can further be related to age specific periods that were surveyed in the follow-up cohort.

Theorists suggest that the learning of crime may not be as direct as social learning, and may include negative as well as positive associations.

To test this aspect of the similarity between victims and offenders, the odds of committing an adult offense by juvenile victim status were calculated. For whites and non-whites, the plotted relationship supports the hypothesis of learning by means of negative associations.

To Al Biderman's point about the victimization leading to further victimization experiences I've added crime-committing or criminal behavior. The percentage committing a serious assault as adults is significantly higher for juvenile victims: 64 percent compared to 22 percent for non-juvenile victims.

For gang members the relationship is almost perfect in that 94 percent of juvenile victims who are gang members report committing a serious assault. For non-gang members who are victims, however, the probability of committing a serious assault is higher than if not a victim and a gang member, which illustrates again the additive effect of victimization experience.

Fifty-four percent of juvenile victims who are not gang members reported committing a serious assault compared to 42 percent of non-juvenile victims who happen to be gang members.

With self-reported offenses, it is possible that whatever we're seeing here may be a function of "yea" or "nay-saying." To control for this effect, I looked at official recorded arrests. When this relationship was examined again by victim and offender status, it is quite significant: 80 percent of non-whites who were victims were offenders and 52 percent of whites who were victims were offenders. Of course the additive effect of race is illustrated, accounting for some of the observed variation in delinquency status and having an official reported arrest.

I should also add that this is specific to only the adult years and does not include the juvenile years as well.

To wind up, the relationship between victim and offender status for gang members and nonmembers was plotted and is similar to what we find by race, again, emphasizing the significance of the victim experience in having recorded adult arrests.

Although I have looked here at just a simple probability of having an adult arrest, when I control for seriousness (that is, using the dependent variable weighted for the seriousness of adult criminal career), in multiple regression models, the victim experience is quite significant. Along with gang membership, race, and the seriousness of juvenile arrests, 36 percent of variance is explained.

The results presented along with those of studies of victim-offender interaction gathered on police-recorded and victim-surveyed incidents indicate support for homogeneous victim and offender populations, but the evidence should not be taken as confirmation of subcultural theory.

More data is obviously needed to provide a more complex test of subcultural propositions.

Homogeneity between populations should be examined in a model that allows for feedback between indicators of violence as measured by victim and offender experiences.

Thus, it seems worthwhile in self-report studies of criminality to include additional information on the victim experience in order to provide the multiple indicators of violence that the testing of the theory needs.

It is not only important to distinguish the victim-offender relationship for the further development of criminological theory, but also for the purpose of anticipating, over time, patterns in criminal conduct and the public's response to crime.

If there is an increase in stranger-to-stranger violence, it can be expected to correspond to increasing fear and efforts to reduce the potential threat of attack. There may be an increase in social isolation and personal expenditures on security devices.

Motivation and opportunity for criminal conduct may also be examined by race, age and sex of victims and their offenders. An increase in interracial violence may be attributed to a rise in racial hostility or residential integration. Similarly, variation by sex in victim and offender populations may be due to females becoming less restricted in their traditional social roles.

In terms of the continuum of homogeneity, it is important to consider variables that lead to the overlap of victim and offender populations.

Despite the fact that the elderly have a lower probability of victimization by violence, it appears that those who are in blighted urban areas and age-integrated public housing, are more susceptible to personal attack. To reduce homogeneity with respect to residential proximity would consequently minimize the opportunity for the elderly's chance of victimization.

Aside from the availability of a target, there are other reasons for a structural change in the distribution of victims and offenders. The National Crime Panel data can be examined over time for the surveyed demographic characteristics of victims and the perceived race, age and sex of their offenders.

In combination with various social surveys, the distribution of victims and offenders could be related to shifts in public attitudes. The cost of further analysis is minute in relation to that of obtaining the data. In particular, the National Crime Panel data, with their large sample size, afford the opportunity to trace more precisely the multivariate distribution of victims and offenders with statistical procedures applicable to qualitative variables, such as the log linear and latent structure analysis recently suggested by Goodman.

SCHRAMM: The brevity of Simon Singer's report limits my ability to comment on the study that is reported here and on the relationship between this study and the recommendations for the future research that are indicated at the end of the paper.

The methodology that was described in the study is sketchy and, from private conversation with Simon, I understand this was extrapolated from a much larger chapter which appear in Dr. Singer's dissertation. From this paper, however, we don't know the means by which the sample of 567 individuals were obtained, or the extent to which they were representative of the total cohort population. These specific issues may not be important for our concerns in terms of future research, but I

think some of the implications of this research might well lead to recommendations regarding the use of this methodology.

Unfortunately, some aspects of the methodology were not articulated. For example, it was unclear how such terms as "serious assault" or "serious crime" were defined in the research, what instruments were used, or how the dynamics of the interview situation might have influenced the responses contained in the self-reported victimizations and self-reported criminal activities. However, we do know that the findings obtained were consistent with previous studies by individuals in this room which relied on official recorded instruments and reported criminal activity.

Regardless of these limitations this approach is very likely to be a fertile area for research in the future. For example, I am very much in agreement that it would be worthwhile to include information on victim experience in self-report studies of criminality.

Despite the issues related to the conduct of self-reported offense interviews, once you have managed to clear the human subject hurdle, you might as well take advantage of the opportunity and gather information on victimizations. I believe, however, that the study of the extent to which, (1) victims are also offenders, or (2) they share common demographic characteristics, should be undertaken with considerable caution. They are not only beset with all the pitfalls of possible inappropriate causal attribution, they are also very likely to be limited in the generalizability of their findings.

Let me elaborate a bit on the first point, that is that such studies might suffer from a tendency to establish inappropriate causal relationships between victimization and commission of serious offenses. In the current study, Dr. Singer's findings stimulated my curiosity about the circumstances of the self-reported victimizations. Were such victimizations frequently associated with the commission of a serious offense? If so, then I would expect to see a relationship between or among the variables of victimization, serious offenses and, perhaps, gang membership, because essentially they represent the same occurrence. Thus, it would be very helpful for future research to explore in detail the circumstances and antecedents of the victimization and the offenses. This approach would allow us to know whether serious victimization increases the odds of finding a history of serious offenses, or whether a history of serious offenses increases the odds of serious victimization.

Second, on the issue of generalization, Dr. Singer's research was conducted on a very small, male subpopulation located in an urban environment. I would be very interested in knowing the extent to which these kinds of findings are applicable to other kinds of subpopulations which share common demographic characteristics. For example, how would victimization which involved female victims of violent crimes relate to the extent to which these female victims have a history of serious crime. Some research has been done previously on this topic, but this would, of course, be an extension of that.

One could go on and on, and I would simply like to close by stating that this is a fertile area for research and one that should be explored in the future. I think it is very important, however, for us to be very clear about what the independent and dependent variables are when conducting this kind of research. There is an opportunity here to utilize, in a very fruitful way, either good descriptive data or information that's derived from theory to look at those relationships in more detail.

SINGER: As far as the sample goes, there are problems, and it mainly stems from non-response. We only captured 50 to 58 percent of the 10 percent random sample that was aimed at.

WOLFGANG: It wasn't non-response, it was nonlocation.

SINGER: Right. We've done extensive analyses of the characteristics of non-respondents because we have criminal histories and the demographic characteristics from the original cohort studies. So we know, for instance, that nonrespondents are underrepresented in terms of non-whites and also in the officially reported offender status. There is not much we can do with that for the purpose of this analysis. I think when you control for race, part of that non-response effect is also controlled.

As far as the methodology goes, there are other problems as well in terms of response. It's a lousy reference period that I've looked at, consisting of the respondent's entire life. One asks the subjects to recall their victimization in time periods, as adults, as juveniles, and before the age of 12.

We didn't get many shootings or stabbings as children, but we got quite a few as juveniles as well as adults. I would hazard to say that the juvenile victimizations are probably much less accurate than adult victimizations. That can possibly explain the reason why we have a relationship between an adult offender status and adult victimization which is not as strong for juvenile victimization.

The only thing I suppose that makes the data a little more credible based on the reference period is that it includes highly salient events, being shot or being stabbed. At least that's something I would remember if it happened to me when I was 13 or 14. In that sense, I don't see why it's necessary if we're dealing with statistically rare events such as victimization by aggravated assault, that we be limited to a six-month reference period or a one-year reference period, as in the case of the National Crime Panel surveys. If we are interested in capturing serious assaultive behavior, then the cost of increasing the sampling frame is astronomical in relation to just increasing the reference period, which would allow us to pick up more of the serious violence that occurs.

As far as the details about the incidents surveyed, there were none. This was just a small part of a much broader study, and no deep follow-up questions were asked as far as who did the shooting or stabbing or did it occur in a gang. I think for the serious assaultive offenses, it's well worth the effort to ask those questions. I agree with Donna Schramm that we need to know specifically whether the victimization occurred in relation to a criminal act, and that goes for the National Crime Panel surveys as well.

We might be picking up in serious assaultive victimizations, serious offenders as well as victims. If the relationship pointed to in Philadelphia holds, I think that would be the case with the victimization surveys elsewhere.

As far as independent and dependent variables, I don't view it as strict in methodological terms. I would like to think that the victim and offender experience interact with one another in a process of feedback. There is good theory to support that interaction in criminological theory. We can now look at the work of Matza, for instance, in pointing to a feedback process and a sense of desperation as the impetus to committing the crime.

My interest in adding a bit of new data to our understanding of victims is to basically get at criminal behavior, which I think is something in which we're all interested.

REISS: We are open for discussion. Just in keeping with trying to mark things for possible research, it seems your remarks do suggest it's an area in which we need some research about victims as offenders. That is, when you choose the assault case

you are dealing with a peculiar situation where you can simultaneously be victim and offender. Victims are usually losers in certain kinds of assaults. It doesn't mean the other person wasn't injured. That is to say, one can have simultaneous injury. We really haven't sorted that out very well. So that's an area which needs to be earmarked for research.

I would go further than that and say we shouldn't only look at rape victims. In rape cases we know some women resist and cause injury to the offender. We think of rape as being an assault on the woman and an injury. We don't correlatively think of the kind of injury that's inflicted on the offender and the relevance of that.

Assault is a very peculiar and interesting case of simultaneous injury. It seems to me you have earmarked that as an area of research.

BLOCK: I would like to make a few points. Maybe you can respond to them.

The first point is that in the area of self-reports of victimization and self-reports of offense, and in several other areas that we've mentioned--for example, opportunity structures--there has been a lot of research in other countries. In Germany, in Stuttgart and near Freiborg--I can't remember the city--they have done this kind of thing fairly well, and with some problems.

REISS: Done what kind of thing?

BLOCK: Studied the victimization of self-reported offenders and officially reported offenders. We can do that again and look at their problems which I know they have had. There's much of that kind of research going on, taking off from our models and trying to improve them in other countries.

A second point is that I believe there is a confusion in this paper between stranger-to-stranger violence and assaultive violence. The interaction, the similarity of victim and offender in assaultive violence--family fights or gang fights--is much greater than in stranger-to-stranger violence and robberies.

I know that over time there was, in non-stranger assaultive homicides in Chicago, no difference in age between victim and offender. In stranger-to-stranger robbery and homicides in Chicago there was at least 10, sometimes as much as 20 years age

difference. The relationship is very different, especially when you consider the violence that we're most concerned with is stranger-to-stranger violence. We can't get these two things confused.

The third problem I think is one that Al mentioned. There has been a history in the victimization surveys and a history in police data of problems measuring assault. Some of us have concluded that you just cannot measure assaultive behavior, especially the intrafamily assaultive behavior, very accurately with the methodology that we have been using, either the National Crime Survey or the police data. That has been obvious, since 1966, in retrospect.

SPARKS: Simon, is it right to say that an interpretation of the data you have is this: That for people who are not juvenile victims, gang membership makes less of a difference in the probability that they will commit an adult offense than is the case if they were a juvenile victim? Or, conversely, that for gang members being a juvenile victim makes less of a difference to the probability of committing an adult offense than it does for non-gang members?

If that's so, then it suggests that these might be independent influences on committing an adult offense.

SINGER: Yes, victimization and gang membership, they are.

SPARKS: Victimization and gang membership, how are they related?

SINGER: They are related positively.

REISS: Suppose you modeled that and said that one of the peculiar things about gang membership in some areas is that it involves conflict behavior with other gangs, and the form of that conflict behavior generally takes is that of assaults.

I think an interesting question is, what's the causal link between that and adult assaultive behavior? One is a collective violence phenomenon, the other is largely individual in the more restrictive sense. Gang membership tends to disappear with aging. That is to say, one drops out of the gang. I'm just puzzled by what kind of reasoning goes behind setting up that kind of model.

SINGER: In looking at the seriousness of adult offenses (recorded official offenses), gang membership clearly has a very strong influence in victimization. Is your question more general or specific to this data?

REISS: My question is the theory which lies behind the selection of putting things together and looking at the relationship.

SPARKS: A plausible model for those three variables then would be with an arrow from gang membership directly to adult criminality, and then to adult criminality through juvenile victimization.

SINGER: Right.

REISS: But how plausible is that?

SINGER: The social implications of being a victim haven't been theoretically developed, and that's part of the problem. Our theories of delinquency and criminality have taken gang membership as a focal point of concern, specifically subcultural theory. We really have very little theory as far as the phenomenon of being a victim as it relates to further criminality.

There are certain bits and pieces, specifically Matza's work, but there's really not much to make it that credible theoretically.

REISS: Suppose one separated the problem of theft behavior, which is also presumably gang-linked, in maybe perhaps a different way than what it is in the violence case. If one thinks of sort of the ideal type of gang conflict, it is a rumble. But in that case an assault becomes a kind of mutual victimization, mutual offending situation. The larceny or theft case is rather different. It's a matter of group offending.

One of the things that might be worth looking at is how much one continues to be a group offender as an adult as compared to a single offender, because there's one line which says that increasingly one moves away from group offending to individual offending as one gets older. So then what is the relationship? I mean, is it being mucked up here by the fact that some people continue to be group offenders as adults and other people individual offenders? It's that kind of model which might be worth pursuing.

WOLFGANG: As I recall, the evidence we have suggests that there is a reduction of group behavior, group criminality after 18. It's not gang behavior any more. There may be aggregate group behavior, but that becomes greatly reduced, and much more individual behavior occurs. Not only that, but there appears to be an

escalation in the degree of seriousness of the offense with age, up to the age of 26.

If we had more cases, we would be able to examine the degree of specificity in the kind of victimization; that is, we could examine not only, as Donna Schramm would suggest, the sequence (e.g., are you victimized first and then become a criminal or are you a criminal and then become a victim), but if you're a victim of a robbery, then when you become a criminal do you commit a robbery?

What's the probability of committing a like offense? On the basis of the data we have there is a tendency to repeat a like crime, but we do not have enough to be sure.

Sellin and I used mutual victimization in our category of victimization. We have primary, secondary, tertiary. We ran out of terms. The mutual victimization was the only phrase we could come up with relative to gang activities in which somebody was both hurt and hurt others as well.

Both Al Reiss and Wes Skogan said something about the difficulties in measuring assaults. That's true with respect to the UCR and the victim survey data, but there are ways in which you can measure that.

Sellin and I found in the original crime severity study that among the offenses that the police had listed as simple assaults, about 28 percent had ingredients that by statute or by UCR definition were indeed aggravated assaults. A high proportion of those people went to the hospital and lethal weapons were used. There is a lot of confusion in the official data.

You opened talking about space and intermix between victims and offenders, and I just couldn't help thinking about that in West Philadelphia. Right on the edge of the University campus, in order to build a new extension of a small animal laboratory for the veterinary school, it was necessary to tear down a convent that was built in 1851. It's the Convent of the Sacred Heart, a cloister. As the University committee went through that place, it was the first time that any males had stepped over the threshold of that convent since it was built. They were being led through the convent by a 94-year-old nun who had lived her entire life, had been there 73 years, really, in this one small confined life space.

When they finally tore the building down and moved the nuns to another convent in the city, they moved them in hearses so that they could neither see nor be seen by the world outside.

Now, I put it to you: If you are willing to undergo that kind of voluntary constraint in your life activities, you will never be victimized.

I wanted to underscore the comments I heard before about qualitative research and the need for what we call ethnographic observations as well as interviews, looking at situational analyses. I think this approach is the up and coming thing in our field, not only in victimology but in criminology in general, that is, more situational analyses.

The more deeply we can probe into those microactivities and behaviors, the more we will be able to make linkages with the more macro level data. Then adding these ethnographic behaviors together we can get aggregate ethnography and can even talk quantitatively about qualitative data.

In the references that have been made to counting, for example simply standing there and counting the number of people in a given amount of public space, I remember in England, that the Home Office Research Unit had done a study of the hourly population in certain quarters of London. I never saw it published.

It made references to subpopulations. I had a typewritten copy of it. It was a fascinating study, because in different sections of London the estimates were made of the number of people in every one of the 24 hours of the day, and this produced then the capacity to give an at-risk denominator to the crime numerator. Something of that sort I think could be done here.

The last thing I wanted to say is that we don't really ask questions about why people were not victimized. Perhaps that's difficult to do. What occurred to me was research, and again, this would have to be through interviews, to get as close to the scene of the crime as possible. To what extent do the victims of crime play any role in the crime drama itself, in reducing the gravity of that crime?

Probably the most obvious question is with respect to rape: Does the victim's response to keep the more serious crime from occurring have any significant role? Are there differences in

the capacity of criminal victims to reduce the gravity of victimization in terms of subcultural variations, ethnic variations, neighborhood variations?

REISS: Marvin, in connection with that latter, I think that's a case where economic models may be helpful. I thought of it when I read Ehrlich and Beckler, that we can look at these things in terms of their marginal utility rather than their simple utility. That is, we're continually doing evaluation study after evaluation study looking at simple utility rather than asking what's the marginal utility of any one of these behaviors given the fact that most people are doing a lot of things anyway.

What's the marginal utility of your doing X, Y and Z, of keeping grandma at home of adding a deadbolt lock to the house? It's probably close to zero in terms of its capacity to reduce victimization.

GROPPER: Among the issues circulated in the pre-conference materials, for potential consideration here, were several related to the possible effects of perceiving yourself as being victimized, either as a specific individual or as a member of a group. For example, such experiences or self-perceptions might produce more than fear or defensive reactions. Anger, aggression and retaliation reactions can also result.

Suppose you feel you've been wronged, or a member of your family or group has been. You may not just passively try to avoid a recurrence, you may try to get revenge. If I'm a nice guy and someone rapes my daughter, all of a sudden I may change and go get the bastard, even though up to now I've been just a nice guy. When the system fails to protect you, you may feel justified in retaliating.

Do experiences of either personal or indirect victimization tend to contribute to violence, or economic crime, outside of gang situations? Do people who try to rip you off do it partly because they feel you're also a representative of a social or racial group who did the same or worse to them?

If offenders consider their own victimization as partial justification for their offenses, how about society in general, and judges and juries? And, even if those experiences aren't legal excuses for such behaviors, do they at least help to explain them? These are some of the types of interrelated questions we may want to consider further. They are not nearly as simple as those victim-offender questions that tacitly assume

you play only one role or the other, but not both. But I suspect we may find them more powerful explanatory tools in many situations.

ZIEGENHAGEN: To add to that, we might also consider the victim as a crime control agent and, specifically, the responses of the victim. The immediate responses of the victim at the outset of the crime might be something that we want to look into much more deeply. We are wedded to the idea of intervention by the criminal justice system. The first area of intervention, of course, is the victim.

REISS: There's a little data on that. In a study being put out by the Kansas City Police Response Project, there are some very interesting data about what's the first thing that victims do.

It's absolutely fascinating the way we have false conceptions of how people can mobilize the police. Setting aside the fact that when you're victimized out there in a strange situation, you don't have a portable telephone with you to call up the cops, you have got to find some way of mobilizing them.

But the interesting thing is that even when victims have a telephone, the first thing they don't do is call the cops. The first thing they do is to talk it over with somebody. That's fascinating. It tells us a lot about most victim behavior, and it's that kind of micro model you are saying we need to look at.

WOODSON: That is consistent with two studies, one out in Michigan, the other at the University of Southern California, that asked neighborhood people to list the first eight people they would turn to in time of crisis. The first seven, of course, were people in their immediate environment. The eighth person was a professional or an outsider.

Yet most of our services and responses are directed or developed around the person of last choice.

REISS: You're so right. You can go back to Koos' Families in Trouble. Earl Koos went to live in the neighborhood, the thing you were suggesting earlier. He actually went and lived there. And where do families turn in times of trouble? The last thing that they turned to, the last person they turned to or the last agency, was the professional one.

SPARKS: This is a very general phenomenon. An important implication that it has is that people vary in terms of their social connectedness. Some of these people are going to run out before they

get to eight choices. People who are relatively isolated run out of that support system fairly quickly.

SINGER: I hope my paper filled in just a tiny link somewhere in terms of what we have been referring to as the lifestyle exposure model. I don't think that opportunity, although it's important, is enough, nor availability nor exposure is enough, to explain victimization.

It reminds me of Sutherland's warning at the beginning of his introduction to differential association, when he gives the example of two brothers, one with short legs and the other with long legs. They both stole something and the one with long legs was able to get away and become a priest, and the one with short legs got caught and was sentenced and became a committed criminal for the rest of his life.

Opportunity is a first level of abstraction in explaining criminal behavior, and I think it holds in explaining victimization as well. I think it's important now to proceed beyond that point and to get many other indicators, one of which I hope I have uncovered a little bit.

FIRST DAY: LUNCHEON PRESENTATION

REISS: Our treat today is Dr. Morton Bard, who is going to talk with us or share with us some of his past and his current experience in looking at the psychological impact of crime on the victim.

As many of you know, Mort has done a great deal to look at the impact of crimes on victims and is associated with one of the major forms of dealing with certain kinds of crime victims, particularly in families, and developed a literature on family crisis intervention.

More recently, Mort has been working on homicide. I look forward, as you do I'm sure, to having him share with us both his reflections on the past and where he is going in the future.

BARD: Thank you so much, Al and friends. I haven't seen many of you in a long time. I'm very pleased to be here, just having been myself victimized by the flu and only recently out of bed. I feel a little bit shaky, but delighted to be here.

And I also must say that I had that personal experience that most victims have of "Why me?" and "I never thought it could happen to me." I've never had the flu before. So I experienced it in just that way and I am sharing with you a victim's personal experiences.

The victim of any experience is a person who experiences the event at a number of levels. As a psychologist, my unit of analysis is the internal, experiential, cognitive, affective realm. My first go-around with victimization involved ten years at Memorial Sloan-Kettering Institute in New York.

There is a lag very often in the way systems respond to insights and knowledge building. The current issue of The Psychological Bulletin carries an extensive review article entitled "The Psychosocial Correlates of Breast Cancer," and that was my doctoral dissertation.

It's interesting to me that there is an analogy that can be drawn. I tried to do it in my recent The Crime Victims Book to draw a medical analogy with the crime situation...on the issue of malignancy that I was concerned with in the earlier research.

The last paper that I wrote when I was in that field was "The Price of Survival for Cancer Victims." That is, what price

do they pay in order to survive the ministrations of all of the professionals who deal with them? Is living enough, and what price does one pay in terms of human dignity and in terms of function?

These are questions to which I have addressed myself in the course of my career. To respond to something Al Reiss raised, I personally feel strongly that the crime victim is the object in a social sense of a kind of malignancy, a social malignancy, if you will, just as the cancer victim is the object of a physical malignancy.

Oncologists of varying kinds have studied cancer for a long, long time, with the most highly sophisticated technology. In the last analysis we know that there are a whole range of disorders which attack at a cellular level with varying degrees of malignancy, yet nobody can say why. Nobody yet knows what the mechanism is...the causative factor which turns a normal cell into a wild and disordered one.

One can speculate that the same thing is true in the social environment...that for all that we know and for all the years of study criminologically, sociologically, psychologically...for all that we have been able to understand, we are still no closer to really understanding the cause of the social cell becoming wild and disordered and attacking and destroying in the social area than we are in the physical. Now, that doesn't mean that we shouldn't consider and continue to pursue a causative explanation, not by any means.

But what I would like to speak to about, and I haven't yet heard any comments about it at this conference, except in passing and in a glancing fashion, is about the victim, the object of the malignancy? What is it that is defiled or demeaned or hurt? What is the feeling?

It's very hard to come to this through aggregate data. It is very difficult in survey research and it is extraordinarily difficult in secondary analysis of official records. The basic unit is the individual, the person whom I think we have to be able to understand better.

I think Al made reference earlier as one of the last things he said during the morning session about people not calling the police instantly, but calling a range of individuals who proffer support; for what reason? Well, there is a reason. It seems to me that if one regards the issue in the terms that I have tried

to put forward, one can understand why there is a reaching out for this kind of support in the most human sense.

What do we know about that? We know very little. We haven't subjected crime victims to that level of analysis, to the qualitative insights that could be derived therefrom. Our methods have not been equal to the task, and that is unfortunate. I believe we really ought to be moving in that direction.

Not that we shouldn't continue to look for preventive insights or to reduce risks. But I think we also should, at least to balance the equation, try to understand the experience of the recipient of aggressive, assaultive or violative acts.

The model I put forward has to do with the violation of self. It deals with the fact that the individual is a highly organized psychophysiological social entity, and that this organization is preserved by the effective operations of an executive. In various psychological theories, it goes by different names, and in non-psychological theories as well. Whether it is called ego or self or soul or heart, it is that indefinable abstraction that executively integrates people as functioning human beings.

I would like to suggest that in many violative crimes which intend to do harm to another person, the intent is really to damage and disrupt this integrative mechanism. Studying people who have been through the experience, one can understand how that operates, whether one is dealing with a cancer victim or whether one is dealing with a crime victim.

For example, what is the meaning of the event to different people at different points in time? At Memorial Sloan-Kettering we could say about a given patient that this patient has adapted this well to this adversity because, fortunately for her, the operation for her involved the removal of a breast. We could speculate about that because of our understanding of her personality organization. Had she needed a colostomy, removal of the anal sphincters and an opening in the abdomen for the purposes of elimination, her adaptation would not have been anything as positive as it was. For another patient the reverse might be true.

That is, different crimes, I would suggest, too, may have different meanings for different people, at different points in time. What do we know about those meanings? How do we get to understand them?

To return to my continuum of violation of self...the most extreme form of violation is homicide, in which the self no longer exists, it is destroyed ultimately... Nevertheless, there is a victim. The victims of homicide, probably in social and psychological terms about as extreme a kind of victimization as is possible in modern society, are the close family members of a homicide victim.

What do we know about these folks? Very, very, little. The literature is virtually silent. Once the homicide victim is off the front page, if it ever makes the front page, there is absolutely no social concern about the aftermath. In my current research we are seeking to compare the survivors of homicide victims with those of other forms of sudden death...seeking to understand their adaptation in the short-term and long-term and trying to measure the psychological and social costs.

Our two control groups happen to be deaths caused by suicide and motor vehicle accident. The reason for those three is that we are hoping for a similar socioeconomic, ethnic and age distribution in those three events. Illness deaths, for example, would have yielded an older age group.

In any event, what we are hoping to do is to understand what the adaptive requirements are following that kind of a devastating loss. I already have some sense, in the very little bit of information we have been able to glean thus far, that the adaptation of lower class, working class people to that kind of a loss seems to be more effective than it is for middle class and upper class groups.

And it provides us with a funny kind of an explanation, perhaps, the kind that was suggested during the Korean War with the prisoners of war. Apparently, the American prisoners who did best under the adverse circumstances to which they were exposed in the prisoner of war camps were those who had the most deprived early childhood experiences, people who had come out of foundling homes, had been abandoned or lived in foster homes. It was almost as if those early life deprivations prepared them.

What we are discovering with the homicide victims among our poor blacks and Hispanics in Harlem, South Bronx, is how hard life is...and that death is no stranger, not only death from things like homicide, but also because of illness, poor medical care, and the like. When a homicide occurs it is in a context of deprivation and adversity the likes of which escapes most of us in the middle classes.

I suspect we will see class determined perspectives with regard to the impact of sudden death.

In any event, the violation-of-self continuum begins for me with some of the lesser violations. Just as for the cancer patient there are some cancers that are unlikely to kill, they are low-level malignancies, so it is for crime. There are low-level malignant acts by one person against another which are likely to have little impact.

The lowest level for the purposes of our discussion would be a burglary...in actuality a violation of the extension of self. When some of the recent survey data was reported it indicated that burglary victims--I think these were the data in Toronto--seemed to have a worse time of it than robbery victims, and this was a surprising finding.

Obviously the extension of self being violated is carried on for some time afterward in its aftermath. Psychologically, I believe that this cannot be minimized as to its significance.

A robbery is a direct confrontation. An extension of self is usually removed from the person under force or duress, and then there is also a loss of autonomy in the process.

Finally, moving to a robbery with assault, there is a clear assault upon the external self, with some marking, some physical injury.

Before we get to homicide, for me at any rate, rape and sexual assault are the most dire and are the most ultimate of violative acts, simply in that it's the only crime I know of, the only act by one person against another intending to do harm in which the individual intrudes upon the interior of the body. Now, in psychological terms I think that's a very crucial issue.

As you know, most of those acts of sexual assault and rape are hardly sexual acts at all; they are assaultive or violative acts. The person intrudes through some body opening, intending to do harm. And if in the process the victim does something "wrong," it's a very, very short step to the homicide. For many females, homicides in fact had their beginnings as a rape or sexual assault.

What does the person feel under these various criminal circumstances? What are the cognitive and affective elements? How does one perceive the experience? How does one adapt to the experience?

My brief remarks to you today are in the nature of a request that you consider, in thinking about work in the field of victimology, in which sociology and criminology has had priority of interest, my request to you is that you think of at least conceptualizing in the area of personal experience. I think we are going to have crime victims no matter what. If history is any indication and if the future is as we think it will be, it will be a long time before we get causative explanations which will permit us to do away with crime victimization, if ever.

In the meantime we have casualties, casualties whose systems of service need to be informed by research. I believe that our methodology needs improvement so that we can understand, hopefully derive some insight based upon the kinds of information we can glean from those who are the victims.

Those are my brief comments. I would be very pleased, because we have a little time, to discuss any of them with you.

BENNETT: I'm Larry Bennett, Director of Office of Program Evaluation, National Institute.

Is there any relationship between a sense of violation in the case of burglary and social class or among people who differentially view property as a personal extension? Is there a different rate or intensity?

BARD: That gets right to my point, to the heart of my point. One perceives burglary only in terms of property, as the UCR does. They don't even call it a personal crime, but a property crime. And yet, one has to say that there must be a differential response dependent on the value of the objects taken.

My sense is that value of the loss is the least important element in a burglary. The violation of one's territory, of one's extension of self, the intrusion upon one's place of security, is borne equally heavily regardless of the dollar value of what is removed.

To get at the intention of the criminal, or the burglar in that connection, most police officers in this country have at one time or another--and for many, many more times than one--found that the scene of the burglary is marked by human waste, that is, urine or feces left on the scene, usually in the most noxious place.

There has rarely been any explanation of that, beyond the very superficial, that is, that the criminal has been overcome by some urgency to relieve himself and that this is somehow compulsive. I believe that that act is an act of communication. It is saying something and it expresses it in the most repulsive way, and the person who lives in such premises experiences it exactly that way. Indeed, it clarifies why burglary victims often express feeling "dirtied" even in the absence of such a direct expression.

So I would prefer to look aside from the issue of value of what is taken to the issue of the significance in psychological terms of giving up your privacy, of the need to leave. The impulse to flee the nest is atavistic. Birds and animals will abandon the nest if intruded upon by another species. I think the same kind of an impulse is to be found in people who have suffered the violation of a burglary.

GROPPER: Along the same lines, there are some violations which may not always be defined as crimes, but which we may fear and choose to consider criminal because we recognize the threat of potential linkages between them and other more serious offenses. For example, purely symbolic vandalism--painting swastikas on synagogues, burning crosses on lawns--may not be defined as crimes until people force them onto the books because they realize the intent is to intimidate and they fear potential escalation. The offenders are warning you. They want to control you. "Get out; we don't want your kind here"; or "If you don't shape up, the next time we're really going to get you instead of your house," etc.

That kind of communication implies a pretty clearly threatened behavioral sequence. First they attack you symbolically or through your property, and then you.

BARD: I think of Wes Skogan's comments this morning about the subway system in New York and the crime rate in the subway system on certain stations. Even though there is a very low occurrence of crime, there seems to be an enormous amount of fear, exaggerated way beyond the actual occurrence. As a matter of fact, logic would tell you that it is much easier to control crime in a confined space like a subway than it is above ground, where there are limitless escape opportunities.

Why has there been an increasing crescendo of concern about crime in the New York City subways? Well, I think you can trace that increase in psychological terms to the incredible increase in graffiti and in mechanical breakdowns and in litter over the

past five or ten years. Many of our colleagues have interpreted the graffiti in the most positive terms, as expressions of art, an effort of young people to assert their individuality and identity throughout the city.

Let's talk about the victims, the victims being the subway riders, most of whom have no choice but to ride in the subway. What that signals to them, the graffiti and the litter and mechanical breakdowns, occurring increasingly as the fiscal problems of a city like New York have gotten worse, is disorder. Disorder means something out of control, and something out of control means danger. People are claustrophobic about being down underground in a rapidly moving vehicle over which they have no control; the expression of that sense of disorder and of danger is, in my view, psychologically translated as a fear of crime.

There is a reciprocal relationship with the media, which exaggerates the phenomena even further. It's happening in many of our large cities, where the fiscal constraints and problems are increasing, and evidences of disorder are signaling danger in a variety of ways.

SPARKS: Yes, I think it's an important point that a great deal of problems, environmental problems in cities, get interpreted in terms of crime. I hope you're not following the line that Nathan Glazer had in his article in The Public Interest in which he said that subway riders are really being assaulted by these graffiti, because the biggest nuisance about the graffiti is that there is one character who has a vaguely Arabic script and he will scrawl over the map. What's the harm?

BARD: How about not being able to look out the window to see what station you're in?

SPARKS: You're supposed to know what station you're at. I think we need to examine very carefully how much of what is expressed as fear of crime is really a general discontent with the way things are going.

BARD: I agree. It's very clear to me that there is much that is being stated in terms of the quality of life. In a way, you cannot say you're insecure because you feel helpless. Older people are that way. Older people who are experiencing powerlessness in a variety of ways feel that it's perfectly acceptable to focus on fear of crime. As we know, the incidence of crime victimization is probably lowest in that group. But to express fear of crime is an acceptable way of saying, "I'm

afraid to go out because I'm afraid of crime," not "I'm afraid to go out because I'm afraid I don't have the strength to get back up the stairs," or because "I might slip on the ice and be injured."

BIDERMAN: Victimology started out as a sort of anti-victim discipline. It became a pro-victim discipline. We have these pro and anti tendencies in common, and in using theoretical models, in the field.

Your talk was pro-victim. Is it necessary or advisable for a humanistic orientation to the field to adopt these stances?

For example, in talking about homicide victims, a lot of homicide victims are very sweet people, but many are not. As a matter of fact, even when sweet people die of perfectly natural causes, psychodynamic theories and explanations tell us that one of the reasons for the strength of the bereavement experienced is guilt because there are various reasons that people have, or in various subterranean levels of the psyche, for being glad the person is gone.

We have a good deal of theory about crime and reactions to crime, which says the strength of the reaction in relation to the horrendousness of the deed varies as a function of the degree of jealousy people have because they are denied the opportunity for behaving in that way by the existence of the control system.

Now, these kinds of theories have a great deal of potency for ordering, making sense out of experience. To the extent that we take a facile normative position to the phenomenon, don't we preclude a good deal of sensitivity towards what's going on?

BARD: The only way I can really answer that question is to say the very same position could be applied to the cancer victim. There are some very mean people who fall victim to that disease. There are some very good people. I am speculating about a kind of social disorder that we call crime, and the occurrence of the degree of malignancy. If you view my statement as being pro-victim, I would rather say that it's a request for equal time...that we be as focused on the victim as we have been on the offender.

I think that at least as much time of the research community should be devoted to understanding the victim, the victim's problems, the social and psychological cost to the victim,

as is devoted currently to the criminal. I have said the same thing to cancer surgeons and oncologists. Their only interest in a cancer patient is as the bearer of an interesting disease.

The same thing has been true, I must tell you, historically in our field. The only interest the victim has for us is as the bearer of an interesting disease we call crime. There are many similarities between surgeons, cancer surgeons, and criminologists in that regard.

Just as for the police, the victim is of interest only insofar as he/she affects their personal combat with evil. And if there is no interest in the victim, does it matter in the outcome? The measure is the personal combat and stemming the tide of evil or capturing the wrong-doer. But what about the victim? A surgeon can do awful things to people in order to save them...so too can we do awful things to victims as we focus on the criminals.

FIRST DAY: AFTERNOON SESSION

REISS: Our first paper is Richard Block's on "Victim Offender Dynamics and Violent Crime."

BLOCK: In this paper I proposed something that we were talking about in the morning, a shift in the concentration of victimology from estimations of incidents to a few other things, differential crime prevention and looking at the opposite of victimization--that is, crime prevention or the ability not to be a victim.

Second, what happens when a crime occurs, when that prevention fails?

Third, what about the impact of the crime event on the victim and on the criminal justice system and the impact of a victim on the criminal justice system?

These concepts are developed around the figure that I passed out, which is trying to look at the intersection of offender, and what I call target, which implies it's an individual. It doesn't necessarily have to be. Their intersection of course is in a crime event, which involves an interaction of target and offender. The crime has an impact, and that impact results in notification or invoking the criminal justice system process. There is also an environment of the criminal process.

All of these environments overlap. The macro environment of the target might be the community, which overlaps with that of the offender and with the criminal processing system, but they aren't contiguous. They aren't exactly the same. The particular overlap I think varies by the crime, so that, for example, a victimless crime would be more likely to have a close coincidence of target and offender, if you can think of it that way; whereas, a robbery of a bank would be farther apart.

So what has to be done then, is to look at these environments, then look at the crime itself and how it is related to criminal processing.

I first discuss the importance of considering what sample you are looking at. I discuss the effect of sample on completion of robberies in Chicago.

If you consider victimization surveys of robberies in Chicago and make an estimate from that, and then an estimate from police records, these estimates, based upon approximately the same kinds of individuals, victims who are residents of Chicago, then you see that there is a large difference. Thirty-three percent of robberies from the victims survey had no loss versus 6 percent in the found police records. This is related, as I note in my paper, to resistance.

Resistance seems to have little effect in police data. It has a large effect in victim survey data. If you view these as two samples of reality, they are samples which would lead you to two very different sets of conclusions. Studying these interactions is very important, but the sample is crucial to knowing what that interaction is.

In a book I wrote a few years ago I failed to realize that. Looking at police records, they are not just a random sample of victims, of victimization. They are a different sample. They are different kinds of events than reported in victimization surveys. I discuss other crimes also, and conclude that what is necessary is a coordinated research strategy. This strategy would look at variations in the probability of victimization, using models of individual behavior, of community and environment; then targetting, with a concentration on the concept of crime prevention; and then a study based upon the idea that the victim surveys are, in fact, a very expensive screening device.

We haven't been doing enough to learn about crimes given that we have spent all this money on screening. I would propose a crime-specific analysis of the victimization, not with every reported crime, but with a sample of reported crime, so that we can really learn what has happened, what the impact of that victimization is and also how the victim views the criminal justice system. The early surveys, which were concerned with that, have been virtually abandoned. I think that that's a mistake.

There should be a study of the bridge process of notification or invoking the criminal justice system, first, from the point of view of the victim and then from the point of view of the police. Finally, there should be a new emphasis on the role of the victim in criminal processing systems; what effects the victim's desires and characteristics have upon prosecutor, police and court decisions.

If we remember to think of victim and offender as a duet, as Von Hentig noted, then I think we should think of the crime

as a coincidence of victim and target. Then crime prevention and lessening the impact of crime can occur both through changes in target and environment, in both target and offender.

We haven't had a great deal of success in changing offenders. Perhaps we can have more success with great caution in altering target probabilities.

SCHRAMM: Let me preface my comments with the fact that, like Robert Woodson, who is no longer here, I am both researcher and practitioner, and I play whatever role is appropriate at the time. Here I will be a practitioner, although practitioners would assume I was a researcher.

The reason I appreciated and enjoyed this particular paper is because, as a practitioner, I find the model and the utilization of the data very useful from the perspective of planning and the development of particular crime prevention strategies. In addition, I think the model has useful implications for vichmology theory.

For practitioners, I think Dr. Block's paper very nicely characterizes what anyone who works in the criminal justice system already knows, i.e., that the characteristics of victimization are determined by the points in the process where the search is undertaken. This is illustrated dramatically with the information presented in the paper. The model also recognizes the different roles that are played by various actors in the system. As cases are processed, whether they be rapes, homicides, assaults, or whatever, each successive practitioner requires different types of information. Thus, as cases are selectively processed, through each stage their characteristics are modified.

I think it's important to remember that victimization studies generally rely upon one of two very different sources of data. The first reflects the perspective of the event as it happens to victims (as they may report it, as it may be observed, or as others may report it to you); the second is dependent upon the perspective of and information available from the criminal justice system. In the latter case the victim's view of his or her own victimization is basically irrelevant. For practioners in the criminal justice system, the primary issue is the intent of the offender, not harm done to the victim, not the perspective of the victim, not whether the victim thinks they have been victimized. I think there's a very big difference in the kinds of information that the criminal justice

system uses and the information which you might want in victimization studies. I think this kind of a model begins to recognize some of those differences, both in terms of the macro and micro levels for the event and for the criminal justice system itself.

The model also beautifully illustrates the measurement differences at different decision points. If another set of data had been used from other jurisdictions, particularly in relationship to rape, you begin to sense how difficult it is to draw conclusions from any of the information that we have so far about victimization to date.

I would like to add one point in terms of the kinds of research that are proposed for the future. Dr. Block suggests that there is a need for a better understanding of the victimization event itself from the perspective of the victim. I would suggest that, in addition, the criminal event itself, can be viewed as a duet. There are two perspectives that are involved in that event, that is, both the offender's perspective and the victim's perspective. There has been some research linking the victim's perspective with the offender's perspective for the same event. It is difficult research to undertake and it requires enormous creativity, but my suggestion is that it could be a very useful research strategy. Clearly this type of research requires that offenders be available to you to study. When this occurs, I see no reason why you not only investigate differences in victim and offender perception of the event itself, but also their perceptions of the criminal processing.

We are talking about the same event that sets off a series of activities that flow from that. I think from the practical side of things, this kind of a model would be extremely useful to people in the field and people who are developing a range of programs, including crime prevention models. I don't know how many of you ever heard police officers give lectures to women in relationship to resisting rape. The police data upon which these talks are based suggest resistance rarely prevents the rape and may result in greater physical injury to the victim. Dr. Blocks data indicates the opposite interpretation, that is, that resistance is an effective prevention strategy. This latter research suggests that one might seriously question some of the crime prevention strategies that are currently proposed.

REISS: I think Donna Schramm has made an extremely important set of statements concerning things that are in this paper about the critical nature of legal processing of victims.

One of the things we know very little about is how the law itself has structured that and its full ramifications, particularly from the standpoint of the prosecution as against the defense counsel. The prosecution sometimes turns the victim into its witness and the defense counsel sometimes sees that victim as an adversary. The person in the adversary proceeding gets caught, being treated in both ways.

I think you have called attention to an area we know almost nothing about. In fact, it occurred to me as you were talking that no one has sat down and looked at the law in this sense and said, "What is it we even admit about victims?" For example, it's well known that we don't allow certain pictures to be brought before the jury, because it might so emotionally excite them that they would lose their objectivity. But the judge can see them to decide whether the jury should be allowed to see them. I don't know of any studies of that kind.

BLOCK: It seems to me that research would not be difficult to do. It's just that there has been no thrust to that form of research.

I see one of the major problems being the screening of events and the detection of events. For the prosecutor the event is there. I have also seen this form of research being done in other countries. In Holland they have a study currently under way now of the judicial process and the victim.

REISS: That's not an adversary proceeding?

BLOCK: That certainly isn't, that's right.

I appreciate your comment that I did not discuss the offender in that duet nor the offender's perspective. I think that that would be a fairly fascinating thing to do. It might be difficult since we know so few offenders.

I also believe that the different views that the victim, that the police, that the courts have of the same phenomena is a very important thing to study. That's why I said that the neglect of looking at how the victim views the criminal processing system, even though they may view it incorrectly, is very bad. I think in that sense our victim surveys have deteriorated from the beginning because of that.

REISS: There are problems of completed crimes versus attempts, but you are way overloaded with attempts at crimes in one part of your analysis. People in the criminal justice processing system

pass on their experiences as victim-witnesses, which must have a feedback property back to it.

In the restitution study where attempts to set up restitution projects have been made, one of the things that seems to hang up a great many of them, and particularly those that want to develop some kind of contractual relationship between the victims and the offenders, is that the victims want nothing to do with that damned offender. "You mean I should bring him around? Have him work in my yard? You've got to be out of your head." So our attempts to create something very nice like restitution focuses on trying to create a relationship that people don't want in the first place.

One other example of how we do that sort of thing in society, and this, I recognize, is very controversial, is when we make blacks and whites try to live in prisons the way nobody is going to live in society. What are the implications of that? What are the implications of that kind of model for what goes on in prisons, like riots?

I'm saying the same thing--logically we are trying to create a model of a relationship between a victim and an offender in a restitution program that is one based on a contractual relationship which doesn't go on in the real world.

EPSTEIN: Something occurred to me when Donna Schramm was talking about the way the criminal justice system ignores victims, and I would like to suggest that maybe one of the reasons it behaves as it does is that the law is, or at least seems to be, written so that the State is the victim.

REISS: Sure, that's the criminal law.

EPSTEIN: When the criminal justice system deals with the victim, they are not dealing with the victim that we are thinking about, they are dealing with the State and I think it's something to bring into the picture, because it may have something to do with how it works.

BLOCK: Oh, I'm sure it does. I say there's very little overlap between the environment of the target and the environment of criminal processing, and that's precisely because of the very small impact that the victim has on the criminal processing system.

SPARKS: It's perfectly true that in a formal sense, there are some crimes, at any rate, for which the State is the victim. That's

not the same as the State being the complainant. Victims come into this when they report the event and there's an arrest. They come in if only because the prosecutor and/or the police may have to clear them out of the system at some stage such as when they make a deal with the defendant to bargain his charge down.

So there is no reason why we couldn't do a study of the way in which victims pass through the system. Then look at their later attitudes to the justice system.

REISS: There is a civil process possibility for victims, which generally doesn't get very well developed in our society for the kinds of crime we are talking about, but when they are "white-collar crimes" that's not necessarily true at all.

There are all sorts of alternatives there.

I think we are really trying to keep this open for different kinds of research with practical implications. We ought not to get ourselves locked into a model as it is, but begin to ask ourselves questions such as: Why is it that the civil process can be used so little?

SPARKS: In a white-collar crime case, often there is no victim. There's no assignable victim, and so there you have administrative agencies bringing action on behalf of "the public."

REISS: Yes, and now there are cases in which people initiate action on their own behalf. In fact, the more likely the victims are to be organizations, the more likely they are to initiate action on their own behalf, which is one of the interesting things. When we change from persons as victims to organizations as victims, then the public on their own behalf may be very critical.

BARD: If you look at the history of the criminal law, you go back far enough to the Anglo-Saxon kings, you get that kind of civil restitution view in relation to the victim. Then the Norman kings came along and changed that. We have ended up now from kings to State, and the most atrocious of all is where the victim no longer exists, as in a homicide. The family has absolutely no rights in the law at all, not even to be informed of the judicial process.

If there is somebody who is apprehended and a trial going forward, the family will have to find out from newspaper or other sources the fact that something is actually happening. They have no right in law at all.

REISS: You have no right as a victim to be notified as to how it came out, whether homicide or not.

BARD: If you're a victim, and you're needed usually by the prosecution, you will learn about it in that fashion.

REISS: In a world of plea bargaining, you're unnecessary, you're irrelevant.

SPARKS: You may influence the process because the police and prosecutor may see your forensic ability as lacking, therefore you're considered a flaky witness, therefore they're going to deal with the defendent because they think if they put you on the stand you're going to blow it.

REISS: Indeed, it's a much more complicated process. That's what I'm suggesting to Dick Block concerning the criminal process only instead of civil and criminal possibilities as a general model, and the way one would fit in an adversary proceeding.

It's not a simple criminal process, and even in that adversary proceeding, it's a plea bargaining process that's very different from the trial process.

WOLFGANG: Just a comment to Mort Bard's statement about the family having no rights in a homicide case. It may be true, but in Europe, particularly in Germany, the family can bring in its own lawyers to aid in the prosecution, which we can't do in this country, of course.

There was an article in the Sunday Times about the change from the Coroner to Medical Examiner system in New Jersey. We are just now reminded of that because of a case of a family that disagreed with the medical examiner's report about the suicide of a family member, how they have brought an action against the medical examiner, challenging him in court, and bringing in their own forensic medicine expert.

BIDERMAN: In my paper I present some considerations that relate to Dr. Block's model, and again, they are in the direction that the model is extremely over-generalized for crime phenomena and many of the most important kinds of crime phenomena do not fit this nice, sequential arrangement of crime event, impact, invoke and so forth.

We have the cases in which the official system is invoked before the event. That is one of the things in victimology, particularly the domestic quarrels kind of thing, where

something is going to happen to me unless you, the police, do something. The police say: "We're impotent," and something indeed does happen.

There's also the on-going type of event, but the time dimensions which I attend to I think relate to important things.

There are two other things I'd like to say; one is that we should note that the taking-off point of Block's thesis is contradictory to the generalizations Gottfredson gave, and that Hindelang has stated, with regard to the similarities, the congruencies, of police data and survey data.

BLOCK: If you're talking about estimates of the amount of crime, the differences aren't all that spectacular.

The differences occur in the nature of the crimes. You can't assume that victim surveys are just a larger random sample of the same phenomena as police data. They just aren't.

REISS: That's right. We've known for a long time that victim surveys report many more attempts.

SKOGAN: How much of that is reporting and how much is unfounded?

REISS: That's another matter. Nobody's telling.

BLOCK: There are a lot of attempted crimes so you can say there are less serious crimes. If you look at resistance, there is a very major difference in the robbery data for whether the victim resisted or not, and for practical application. If you looked at police data, a policeman would advise someone, correctly from his data, to not resist.

On the other hand, if you looked at the victim survey, resistance seemed like the more logical thing to do. I'm not saying which is right.

REISS: Your point is very well taken, it's a very important point.

Resistance is independent of the attempt. The attempt turns out to be an attempt, maybe in part because you thwart it. That's what has to be teased out of this and pinned down definitively.

You've come a long way on that. But how much of what we pick up extra as attempts in the victim survey is a function of

all these different things that people do to thwart becoming victims?

BLOCK: There's much more that has to be done, and that's why I am trying to concentrate attention on the macro-environment of the target as well.

BIDERMAN: This paper is much improved in the conception over earlier treatments of this data set, and particularly with regard to not accepting the victim survey as simply a larger set of the same events. I still think that it's important to recognize the large component of events that are in the police data that are not in the victim survey for interpretations of the model.

Then there is a large component that would be within the boundaries of something we would like to include, but are not in either data set.

Now, the importance of that relates to the proposed strategies, and the implications of the strategies you proposed for what we do next with regard to the limited resources that are available for a victim survey, and particularly National Crime Survey-based work.

I agree totally that the amount and quality of the information developed after the screening is deplorable, and is terribly wasteful in terms of poor design and, given the amount of investment in the entire activity, makes for precious little return.

However, I think there is a more important problem with the NCS than that, and that is the degree to which it selectively captures phenomena of interest by its screening procedure, by the effectiveness with which the sampling plan is implemented in practice, the extent to which the design is indeed realized in practice. I think that is much more serious because it leads to persistent serious misrepresentation of realities in substantive uses of the data.

People may have noticed I have never used those data for substantive purposes except for illustrations that one can back up with other solid bases.

I am extraordinarily troubled by the effectiveness of screener function in the survey or that representation in the NCS of the set of things that are going to be called by a certain name, "crime," including this group of things that we are going to associate with the word for a particular "crime."

Those problems, I think, have very great priority. Before we do too much further misinterpretation of a basic data set that is picked up in this instrument (National Crime Survey), I would like to see the resources allocated to doing that basic function correctly, or as well as we can.

REISS: May I say, in relation to this discussion, that I think we have to be careful, as I said earlier, not to focus unduly on the victims? But there is a kind of interesting question here which Al Biderman's paper again raises in another form.

Of the different ways of what I call intelligence or data collection; how do they affect the information we get? We should not ignore the fact that some systems of collection under-represent certain kinds of victims.

A good case in point is the rape data. The NCS victim survey undercounts rapes relative to those known to the police. Of all the crimes known to the police, when one compares victim survey data with NCS data, the biggest gap is for rapes.

BIDERMAN: Assaults also.

REISS: The point I wanted to make has to do with mobilization systems. The interesting thing about statistical systems is how in fact that system detects the event so it can collect some information about it.

One of the interesting things about rapes reported to the police is how often the victim herself doesn't report it. It is someone other than the victim who reports it to the police. Now, what we do in the victim survey is, we go after the victim, and then we compound two types of errors. One is, we ask, "Did you call the police?" And they say no, but that doesn't mean the police weren't called, which is interesting. Somebody else called the police, a mother, a relative, etc.

I think we have to be very careful when we talk about how events are picked up, who reports them. If we try to compare the information-gathering systems, we're going to get this differential, we're going to get different kinds of information.

ZIEGENHAGEN: The approach that emphasizes the holistic model is suggestive of intervention strategies that tie the micro process to the macro process. For example, there was a practice, it may still exist, very often, for an arresting officer to take a person back into a community where the crime was committed and to consult with people in that community as to what is to be done.

Through this process victim assessment is tied into the criminal justice system, and the outcome in this particular case may never be reported as a crime. What it does suggest is that there are intervention strategies available that tie the offender and the victim, that integrate these two levels, that we would never pick up otherwise, but yet may be functioning to deal with the control issue.

REISS: School is a good case in point, an excellent case.

That's a very important one to look at. Someone said the other day that his wife's tires were slashed at school. That got fed in through the school reporting system. Since he didn't report any of those offenses to the police, they never got to the police, even though it was against an adult teacher.

ZIEGENHAGEN: We have all kinds of justice systems functioning.

REISS: Right. So if you're an employee of the school you get treated just like a kid.

BIDERMAN: When I said that the priority is to be able to screen correctly or to capture events correctly in the victimization survey, what I really should say is that we need to be able to understand what we are capturing in that survey, what it is that we have and why do we have it, and what it is that we don't and why we don't.

That is how we have to use those data in analysis.

BLOCK: I would like to emphasize the necessity for future research to be based upon a kind of holistic system. The three environments I deal with overlap, yet they all have the commonality that we have to look at. Maybe a comparative perspective would be of assistance in looking at that commonality.

I think that we have to look at the victim and the offender within their environments to understand both the probability of victimization and what occurs. I don't think we can do that using any one method. We have to adopt several methods to do it. More methods can be added if we have the money to do it.

It has to be a set of coordinated research strategies, rather than the victim survey, rather than the police survey. They have to be coordinated and integrated together.

REISS: We'll turn now to Jim Garofalo's paper.

GAROFALO: Basically, what I tried to do in the paper is to take stock of where we are in our knowledge about the fear of crime, the causes and consequences of fear of crime. I drew this model as the main exercise in the paper, a model that I believe is consistent with the evidence that we have accumulated over the years about the fear of crime.

Now, the first issue I take up in the paper is the question of the definition of fear of crime. I argue that we should probably restrict fear of crime to anxiety about potential physical injury and differentiate it from worry about property loss.

I also raise the issue of differentiating between actual and anticipated fear. Actual fear, for example, is the experience of one who is in a high crime area walking the street at night versus anticipated fear, when one is being asked about whether they would go to such an area hypothetically, by a survey interviewer.

I can give a brief description of my general model. The model has to be understood as operating within a broader socio-economic framework. The implications of that are probably too deep and detailed to draw out in the present context, and I didn't even try to do it in the paper. It is bound, then, by space and time to a given culture, a given society, a given social system, a given economic system.

With that understood, the model starts out with what I call position in social space, which includes what has been talked about earlier here as lifestyle, routine activities, but also attempts to include on the individual level the totalities of people's past experience and potentials and possibilities in their lives. This position in social space exposes a person in systematic ways to certain information about crime. Information about crime is shown in my model as coming from three primary sources: direct experience either as a victim or a witness, interpersonal communication with others about their direct and indirect experiences, and from the media.

This information about crime is processed through some mediating factors, which generally are factors that influence selective perception on the individual level. From this information and the mediation, an image about crime is formed by the individual.

The model also contains the definition of appropriate cues that people use to infer the threat of crime in their environment. People assess their risk of victimization against that image of crime.

Now, by risk assessment I don't simply mean the likelihood of being victimized. There are four components in the model: (1) prevalence, the actual amount of victimization or crime; (2) likelihood, how likely is it that someone will attempt to victimize the person, given that they are in certain areas which have certain amounts of crime; (3) vulnerability, which gets at the issue of how people evaluate the possibility of resisting or protecting themselves against an attempted victimization; and (4) the consequences, namely, the costs that people can expect to endure if they are victimized.

There are some feedback loops in the model. I don't think I'll be able to go through each one of them.

At the next step, we actually have the fear of crime being initially produced at the individual level; and again let us maintain the distinction between actual and anticipated fear. We wouldn't be too concerned about fear unless it led to some kind of responses and outcomes. I think we have to see responses to fear as being mediated by considerations of other costs and options which affect what the person can and cannot do in response to that fear of crime.

For example, the person may be very fearful in his/her neighborhood, and yet the possibility of them moving as a response to that fear may be strictly limited by economic factors, such as wanting to remain in the neighborhood to be close to one's family. So the person does weigh the costs of taking certain actions in response to fear.

There are many constraints. People don't have all options open to them. When we get to the actual behavior in responses to fear, I discuss six categories in the paper. Five are drawn from the work of Fred DuBow at Northwestern University, and I added a sixth category, information seeking, and also tried to further differentiate one of the other categories that DuBow suggested.

The categorization of behavioral responses that DuBow and his colleagues present is very useful, in the sense that it seeks to make reasonable differentiations on types of responses. I think the categories are quite flexible. They can be used in studying the fear of crime. They could also be used in studying worry about property, if you maintain the differentiation

between fear of crime and worry about property loss that I made in the paper.

The model also depicts other motivations and options. This is a very vague idea, but there are a lot of responses and behavioral adaptations that are taken, not motivated by the fear of crime, but which in fact do have an effect on the person's risk of being victimized and on the subsequent fear of crime.

For example, a person may move from a high-crime to a low-crime area. The fear of crime may not be a major motivation in that move, but the move can have some long-term effects back on the fear of crime, because it decreases the risk of victimization substantially. So although the motivation may not have been specifically from the fear of crime, it does have fear-relevant effects later on.

There are a number of feedback loops going from behavioral response to fear. I don't think I have time to go into those in any detail, either. I will just to point out that some of them may be representing positive feedback loops which aggravate the fear of crime, others may be negative feedback loops which dampen the fear of crime.

I think it's important for us to examine these types of feedback loops and to determine whether they are positive or negative and the situations under which they are positive and are negative, because they are important places for program intervention in trying to control or decrease the fear of crime.

The last part of the model reflects social outcomes, and this concerns what occurs on a broader societal level, not simply the summation of individual responses to crimes. We can think of individual responses, as I put in the paper, as being a catalyst for initiating these wider social processes, that then take on dynamics of their own--anything from avoidance of social life, causing all kinds of wide social consequences, to a greater social solidarity in reaction to crime, causing more close social interactions.

At the end of the paper I try to draw out some possibilities for research that would have policy implications. The first thing I discuss, although it's not specifically a research suggestion, is the rationality-irrationality argument. There has been a debate in the literature about whether the fear of crime is rational or irrational amongst particular groups in the population. I think that has become an impediment to discussion

about the fear of crime, rather than a good conceptual issue with which to move forward.

There is nothing in the model that prevents a group from having a higher fear of crime and a lower victimization rate than some other group, which has a lower fear of crime and higher victimization rate. It doesn't imply irrationality at all. The problem comes in explaining those disparities in the fear and risk levels, rather than labeling one as irrational or rational.

I suggest that we focus a lot of research on the last part of my model, the social outcomes. If the fear of crime and responses to fear of crime do not create very widespread social outcomes, then the policy reasons for doing further research on the fear of crime are not strongly motivated. Unless the fear of crime can be shown to have some widespread social effects, I don't think we can expect to devote a large share of research resources to that investigation.

I also suggest that we try to link the fear of crime and social outcomes that are produced thereby to some broader social concerns. I refer to some work I did earlier about the fear of crime in relation to concern about community and quality of life.

I talk about the necessity of research looking into the fear of crime in relation to other fears, and the fear of crime as opposed to the lack of fear concerning events which can have similar or even greater amounts of physical harm for the person, such as air pollution or automobile accidents. By contrasting and comparing the fear or lack of fear to those different type of events, I think it will help us get a better handle on what the actual nature and components of the fear of crime are.

I raise again, in suggesting research, the differentiation between actual and anticipated fear, because I think that this differentiation has important policy implications. The way we go about trying to intervene with anticipated fears is probably quite different than the way we have to intervene with actual fear.

I spent some time talking about the development of indicators, both indicators of the fear of crime as well as for some of the other components in the model. I discuss the issue of the feedback loops that are in the model and why they are particularly important for policy-relevant purposes as intervention points. Finally, I raised an issue at the end of the paper that

perhaps the fear of crime should not be looked at as an unmitigated evil. To some extent, a little anxiety about crime is probably a good thing. If people had no anxiety whatsoever, they wouldn't even take the minimal precautionary behaviors which are very rational and very useful in our society.

I don't think we have any chance of eliminating crime, so perhaps we should recognize that there is some functional purposes involved in the fear of crime, as well as dysfunctional.

SCHNEIDER: I think this is a very important paper. It has a lot of good ideas in it. I think it moves us forward in several extremely important ways in the study of the fear of crime. I am particularly pleased to see Jim Garofalo's paper views people as rational, thinking, calculating individuals who make fairly reasonable decisions based upon the information available at any particular point in time.

This is an important departure from a lot of the previous work, some of which he referred to, namely the studies that look at young females who have low victimization rates and very high fear. Those kind of studies lead us to the assumption that fear is exaggerated all out of proportion to the true incidence of victimization, and therefore it's irrational. Those kind of approaches make the assumption that the only piece of information we process in terms of our behavior is the probability of something happening, when in fact all the theories of choice that I'm familiar with, are based on the notion that you assess the benefits and the costs to be gained (or lost) from certain actions as well as the probability of obtaining those if you take the action.

Individuals assess the alternatives and may even make some assessment of the kind of information not available. People choose what they are going to do.

Some of those choices are passed over very quickly without much thought. I don't mean to imply that according to a rational model of behavior we click through each step all the time. Clearly we don't. Behavior can become rather habitual, even though it might have been based on rather rational choices made at some point in the past.

At any rate, I think it's very important that we start from a theory of behavior that has a rational component. It's hard to explain irrational behavior. It's very hard to study irrational behavior. From a practical point of view, I can't imagine how you would design action programs to change irrational behavior, except to make it rational or more rational,

in which case you ought to have started with a theory of rational behavior.

I think that's important. In and of itself, it will move our theory and our research in a useful direction. Also, it's important because it will relate some of our theories of victimization to other theories of behavior that are also based on theories of choice.

Some of these have been referred to before. I'm thinking in particular of people's assessment of the fear of disaster, such as earthquakes or fires. There is a good deal of research on how people assess the probability of a disaster happening to them. These studies suggest that microeconomic theory, utility theory, doesn't work in practice, and the studies point in new directions, toward other theories of choice. Some of those newer choice theories, which are fairly well developed now, could be applied to crime and our assessment of risks and losses from it. Let me emphasize here that a rational model of victimization behavior does not mean a utility maximizing model. We should not get locked into a rigid version of utility maximizing theory.

We should develop some good theories of choice that are specific to the assessment of crime. There are studies which show that when people are presented with choices involving heavy losses, or the possibility of heavy losses, they don't make utility maximizing choices.

For example, it would make sense for you to pay $1 for a game, a gambling game, where the odds are 99 out of 100 that you will win $100 every time you play. But the odds are one in 100 that you will lose $5000.

Now, from a utility point of view, you ought to play that game because you will make a lot of money. But many people won't play those kinds of games. Robyn Dawes has done some work showing that the negative choice curve, so to speak, simply doesn't operate the way a positive choice curve operates.

With crime, we are dealing with the very low probablities of very bad things happening, so the kind of choices people make are very different. They are irrational from a strict utility maximizing point of view, but they are not irrational in terms of the assessment. Simply, it isn't worth the $100 that you might win if you played the game to risk losing $5000.

I think the other thing, we are dealing with in disasters or crime, is that you don't play these games 100 times. You play them once or you play them twice, and the odds of losing something big will thwart you from playing the game in what the economists would call a rational manner.

I don't want to dwell on that too much, but I think it is really important, and it will open up some very nice and useful individual level theories for use in victimization.

There are some dangers in the use of rational models or theories of choice. One danger is that our theories will always be too broad. They will point us towards interesting variables and concepts, but they may be too broad. You say people assess the benefits and the losses and the probabilities and you never get specific enough to actually hone in on the variables that make the most difference.

Another important contribution of the paper which Jim mentioned in his presentation is that research undertaken in this framework would not rest on the assumption that the fear of crime should necessarily be reduced or that it is necessarily dysfunctional. I think that that has been the common assumption underlying most of the research.

The whole orientation would shift, it seems to me if we come out of this framework with a view towards increasing people's accuracy in their risk assessment and increasing their understanding of what are appropriate behaviors, given their assessment of their own risk. The emotional response which is the "fear" is another thing that should be appropriate given the risks and given the kinds of behaviors. It would reorient programs not necessarily towards reducing fear and its outcomes, but towards making it appropriate.

Another thing I think that's important here, this is much more of a deductive theory rather than an inductive theory, and, therefore, it suffers from what Al Reiss has mentioned several times today, it is overly general.

Here are some suggestions.

I think the last part of the model is the least well developed. The linkages between individual responses and social outcomes are not spelled out.

The way we study victimology has a great deal to do with what we think the social outcomes or the public outcomes of

people's behavior actually are. The implications of our studies in victimization are very important in defining what should the government should do in terms of crime prevention.

Why should the government do anything about crime prevention? One could argue that individuals should be responsible for their own protection--that it is not an appropriate role for goverment.

Of course, one also can argue that it's the government's responsibility.

The fact is we know that victimization is not randomly distributed, it falls much more heavily on some people than on others. We also suspect, from a very small number of studies, that private protection is not randomly distributed either. In fact, the amount of private protection which people have, particularly of their homes and their property, is related to their income, and to other social class variables. Furthermore, we have reason to believe that different kinds of private protection are effective in reducing the risk of victimization.

We also know that private protection for ourselves has consequences for other people. If I successfully avoid all crimes against me, and there is a persistent criminal out there, I increase the chance that that criminal gets someone else because he is not going to be getting me, or particularly my house.

From this point of view, what should the government do? We could say that the government should try to control the external factors around private behavior. Thus, if we do something that has positive effects for other people, the government should supplement those activities.

If we do things that have negative effects for other people, the government either should discourage those or should supplement those who can't afford to do those things, so that they can have those kinds of protection, thereby equalizing our access to protection from crime.

Now the point of this is that we won't ever get to those kinds of policy implications unless we think about protection and people's behaviors in terms of the externalities. I think we are not very inclined to do that. We are inclined to think, policies help protect the individual and we don't really look at their externalities.

I would suggest that particularly in the protection part of this model, under individual responses, that some attention be given to the externalities of individual behavior.

Another thing that is important in terms of policy outcomes (and has not been covered at all at this conference), is the question of where and how should victim programs should be operated.

Should victim programs be the responsibility of the criminal justice system, or should they be the responsibility of social health and welfare system? Does it make any difference to the delivery of these services?

Maybe it isn't an appropriate topic for basic research in victimology, but it is an important philosophical question.

How do you implement these programs? How does a federal agency implement them at the local level? How, at the local level, do you get people to do it? Again, from a federal perspective, how do you get the local people to continue to pay for and support that program after its three years of LEAA money is gone?

It might be worthwhile to spend time thinking about these kind of intragovernmental organizational problems. To go back to Jim Garofalo's paper, I think it is excellent conceptual framework for orienting our studies of fear of crime and victim behavior.

GAROFALO: I would like to pick up on the last point you mentioned about where victim services and responses to victims should be located. I would say fear of crime should be a concern of the criminal justice system as well with other social service agencies.

I think it is important to tie the fear of crime to broader considerations. I mentioned specifically the quality of life.

The fear of crime gets tied up with a lot of social issues, which are not criminal justice issues, like housing and transportation and employment. I think that needs to be taken into consideration when you raise that issue, that a lot of these services are definitely outside the realm of the criminal justice system and very well should be.

GROPPER: Looking at Garafalo's model it has more than superficial similarities to the kinds of models that the NIJ's Office of

Research and Evaluation Methods have dealt with; e.g., perceptions of risks. There are basic functional similarities of these models with regard to analyzing crime from the viewpoint of the offender reactions to deterrents and other policies.

I think it might be very informative to try to take comparable kinds of perceptions by offenders and potential victims, estimates of risk in illicit, licit, and adaptive behaviors, and map them on each other. It might be very helpful in revealing the interrelations among various levels of individual and small group processes, legal and community processes, and the adaptive reactions that are common to offenders and their victims or potential victims.

There are also other econometric and analytic models, from which victimization research might benefit by using similar techniques or analytic schemes, or by building in the same kinds of conceptual variables.

We might consider attempting to create a metamodel of the victim and offender, using parallel processes to describe their risk-related behaviors.

GAROFALO: That partially relates to what Ann Schneider was saying about the displacement of facts, that assumes certain costs are being weighed on the part of the offenders.

GROPPER: Some of the Institute's studies on deterrence attempt to model offender behaviors by such techniques. They have been concerned with such issues as how effective sanctions with given characteristics are as deterrents. Most of those models use three basic notions about sanctions--speed, certainty, and severity.

The results typically indicate that different types of potential offenders tend to behave as if they are weighing, more or less rationally, different risk factors as more important to them than others. A very severe sanction with a very small likelihood of occuring in the remote future is usually not nearly as significant in its behavioral effects as a more moderate one that is almost certain in the near term. The same types of risk considerations can be found in victim behaviors.

Successul metamodels would be useful for revealing and dealing with the common processes probably involved in each of these interrelated behaviors.

BIDERMAN: Well, my comments are mostly to the discussion, because I think most of what the discussant praised I would condemn, because I think it has set back criminology a long way.

I don't mean to say that models are overgeneralized. The application of models are overgeneralized. They are overgeneralized with regard to the phenomena to which they are applied, and they are overgeneralized with regard to the confusion of the names for arrows and boxes on the models, and with things in the real world that we call with those same names.

That is more true of the models of rationality than anything else I know, and it screws us up more than any other single kind of model I know.

There is something in addition to rational and irrational, and that is nonrational. There are many, many varieties of the nonrational. There is the nonrational because there is just no rational solution as there is none to the St. Petersburg game, or any number of other such models.

Life does not always present us with problems that you can solve rationally, and there is often no solution.

Also a lot of what we do is not rational because it is habitual. Now you can say that habit, or what nature built into us as autonomic response tendencies are somehow the rational wisdom of the Creator. They nonetheless, have nothing whatsoever to do with choice, but they explain one whole heck of a lot of behavior.

Further, any model that is individualistic in its orientation is bound to only tell us a little tiny bit about phenomena that by definition are social and not individual in orientation.

Thus, we don't go around this life choosing every thing we do. Most of what is rational behavior in this world is avoiding the necessity of making a choice every time we do something. We don't have to choose our word orders, they come flowing out.

With regard to choice in the economic model the question is, well, where the heck do preferences come from?

There is a philosophical bind, you know something is instrumental to something else, which is instrumental to something else and so on. There are many ways of looking at phenomena which get you absolutely nowhere. It gets you into an infinite regress.

We want to know in regard to crime and fear of crime, well, where do these preferences with regard to particular kinds of risks and hazards in the world come from.

The kinds of things with which the word "crime" is associated in the minds of people are of either homicide or some other very horrendous crimes, which are extremely rare for most people's lifespans, not everybody's, but most. These fears thus fall into the nonrational model because they are the high risk, extremely low probability events that we haven't figured out a way to solve in terms of a formal rational model.

Or criminal incidents are things which are so trivial relative to the myriad of hazards of life, that we have to knock ourselves out in victimization surveys to get people to remember them even a month later.

Nonetheless, the reaction to those very trivial incidents is on a scale very different than what the impact of those events is on an individual life space. The reaction is properly a reaction to those events as evidences of a more general social condition and they are societal responses. It makes no sense to look at them in the way in which they are viewed in these individual cost-benefit kinds of modes.

It makes a good deal of difference if you look at them in terms of how preference distributions arise.

SPARKS: It's certainly an important point, the fear of crime does provide a rhetoric for justifying all sorts of things.

I think one of the interesting things about Jim Garofalo's model is that it allows for--and at one point mentions--a number of sources of information or, if you want to say it, misinformation.

I think the term "information," has got to include misinformation as well. This is where the rational model comes in. Direct experience as a victim is of relatively little importance, but I think it's of some importance for us to try to find out what impact the other two informational components, the media and interpersonal communications with neighbors, friends, workmates, has if a guy on the next block gets burgled, or somebody at work gets beaten up, or you hear about it.

REISS: Everytime I see fear of crime, I have trouble. I don't know whether I should go back to what Frank Furstenburg said when he was trying to make a distinction between what I call fear, generalized fear, and then fear of being victimized. He was arguing that suburbia perceives crime in some abstract way which goes with conservative politics and all sorts of things, and other people actually fear being harmed.

Are people acting on different assumptions? Some people fear being victimized and some people just fear that the society is going to pot because there is that high crime rate?

That's one problem I have. It seems to me it enters at various points in the model.

The more serious objection is that I don't like a model that sees individuals solely as victims. Organizations also are victims, and the intersection between individuals and organizations can be critical. Sears Roebuck left the West Side of Chicago and moved downtown, and built a huge building, because it had something to do with a whole lot of fears on the part of the organization and on the part of people who work there. Sears had to begin spending more money to go over and pick up people at the elevateds and bring them so that they would work in the Sears building, and so on.

Finally, they said, "My God! This is crazy. We can go build ourselves a high-rise downtown and everybody will be happier." So they moved out of the West Side, which then had something to do with that whole community.

That is to say there's a relationship between landuse, what goes on there, what kinds of organizations are there, and so on.

On the other hand, in terms of social outcomes, I think the best thing that ever happened to public housing in the United States, from my own perspective, was when those tall high-rises got absolutely crime-ridden. I thought that was absolutely great. They built such grotesque environments, where the quality of life must have been negative because people were poor and had no choices about where they would live.

I don't know whether it's a rational model or what kind you want to call it, but from my standpoint, they had relatively little choice; many of them were just put in those environments.

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Well, they managed to destroy them fast enough with crime, so crime was very functional in getting rid of that kind of public housing.

There was an RFP about "Land-use, Crime and Fear of Crime," a very short time ago, and it wasn't a foolish RFP. I think that's maybe a tough thing as to how you'd link those things together, but it's not a nonsensical kind of issue.

I don't see your models addressing that, is what I'm saying. I wouldn't find your model very helpful.

GAROFALO: Obviously my paper does not talk about structural features in communities, locations of organizations, etc. There is a footnote in there saying that that is part of the author's thinking on the subject, but to defer discussion of it.

REISS: It's not contextual, though; why do you conceptualize it as contextual? Organizations make decisions, don't they? They make choices, and organizations assess the consequences of victimization. I don't know whether they quite fear it in the same sense, but they certainly fear losses.

GAROFALO: I was really dealing with the fear of crime by individuals. I'm not too concerned about the corporate fears of Sears and Roebuck, how Sears and Roebuck reacts to the crime rate, whether they move their building and how that affects individuals left in the neighborhood or in the neighborhood they moved into.

But if the criticism is that the model does not seek to explain the fear of an organization, then I admit that it does not do that; it was not intended to do that.

SKOGAN: If you look at organizations, you must take an entirely different tack, for crime becomes a cost item.

REISS: I'm not so sure. Sears and Roebuck stayed on the West Side way beyond all advice to stay on the West Side. They tried all sorts of alternative solutions, which were not simply solutions based on costs, but solutions based on thinking they owed something to the community in which they lived.

I'm not sure I'm prepared to buy the argument of total corporate irresponsibility. You know, that's an empirical issue, and not to be solved a priori by models.

GAROFALO: It's an empirical issue, sure, it's open for research. I don't think it's necessary to include it in this model, though. We just focused it on individuals.

REISS: Why do we always focus on individuals in these kinds of models? That's my question.

GAROFALO: I don't know what you mean by "these kinds of models."

REISS: Why must we develop models as if it were only individuals making risk assessments or who have images, when corporate groups do. In fact, Al Biderman's argument earlier was that I as an individual don't create such an image, I don't do any of these things.

It's an interactive process out there, and I just learn about crime, for the most part, and contribute very little to it.

GAROFALO: Yes, I think that's what the model refers to.

WOLFGANG: I think if we are going to construct models, probably a singular model could apply to both individuals and organizations, at least that's what I infer from what you are saying about organizations making decisions.

The paper suggests there's functionality as well as dysfunctionality to the fear of crime, and therefore it suggests there must be an optimum level of fear that we shall try to induce in society. That might be an interesting researchable question. If we cannot reduce crime, there is benefit in reducing the fear of crime.

That raises another question, a researchable one. What is the role of legislative and judicial reactions to public sentiments in the reduction of fear of crime?

I have a rather pedestrian example: does the existence or passage of the death penalty, or increasing the number of crimes for the death penalty (even though we may assume that has nothing to do with the homicide rate or capital crime) invoke a reduction of fear in the population? What about having guns, increasing the severity of sanctions, having foot patrolmen?

We do research about many of these items, indicating that they do not seem to have much effect in reducing crime, but they may have a significant effect in reducing the fear of crime.

The other comment is just one that Dick Sparks was mentioning. That is, the expansion of the information network, or the information that we have about crime and perhaps many other things, it seems to me is related to age. That is to say, what we know about crime when we're in kindergarten, first grade, or elementary school, is very little, and out social network is limited to the family and the kids in our class, or perhaps in the school and the neighborhood; it is not very large.

As we grow into adolescence and to early adulthood, we are reading more, we're now reading newspapers, we get more input about crime, so that now we're not concerned just with our neighborhood, but we hear about it in the outskirts, we hear about it in Chicago, we hear about the "kooky people in California" who are killing people, we begin to watch TV news with lots of crime information.

Passing through the prism of our minds is a lot more information about crime, and therefore, it can increase our sense that there's a lot more crime out there than we ever had when we were younger.

This is one of the reasons I suspect that every generation says: "Things are much worse than when I was a kid." Maybe they are, but it's also the perception of what is worse.

Not only are there different media that are involved in increasing our information loading about crime, but our personal acquaintanceships too, such as knowing somebody down the block, or having a relative that was victimized. They all begin to have additive effects over time, and increase the fear.

REISS: To me, in a community, the most visible evidence of victimization that would generate fear in me is the city of iron gates; that is, it is organizations that give out the largest message of "Be afraid," and put a gate across the front, take the diamonds out of the windows, board the place up. They erect all sorts of barriers, and say to the community: "Beware. This is a mighty risky place."

That's a message that seems to be about organizational victims. That is the most powerful thing to all of us. We know when we're going through the city of iron gates. I don't have to know anything about that city. I'm an outsider. I know when I cross the border and it says "Beware."

That's a big signal. So organizations, out of their victimization experiences take actions which become, in a sense, the most powerful generators.

WOLFGANG: I think that's culture-bound, Al, because it's been, as you know, a long and standard practice in European cities to have the shops bring down the gates?

REISS: That's right. That's because they did that in a different sense than we do it in this country.

But I'm saying, in this country we would never convince people that that's the reason why we do it, because we don't do it in the nice areas.

If we did it everywhere, as they did in Europe, you wouldn't know, and that might be a lot better, but here it gives you a message.

SPARKS: They don't do it in small towns in Europe either, all that much.

REISS: It has to do with the history of European cities, with riots and the breaking of windows, and the expensiveness of glass. So it wasn't iron gates so much in Europe as the iron shell that came down to protect the glass in case of riot. People weren't very civilized in the streets. It wasn't necessary that my business was victimized.

GAROFALO: What you just said, I think, is consistent with what I said previously; in this case you're talking about the behavior of the organizations providing cues for individuals. I don't think that's at all inconsistent with the model.

REISS: But the cues are a consequence of the victimization. That's what the model has to show. You've got a continual process going on here. I don't give out the cues unless I'm victimized.

Don't look at it cross-sectionally--that's the big mistake. All studies make us look at it cross-sectionally rather than historically. When does an area start showing "its" fear, and when does business after business put up their iron gates? Don't look at fear of crime cross-sectionally, look at it longitudinally. This is a cross-sectional model for the most part, no matter what it says.

BARD: Al, I live in a housing project, and not all housing projects are like Pruitt-Igo. Pruitt-Igo failed, by the way, because the St. Louis Housing authority did it entirely on Federal funding, and the good burghers of St. Louis gave no funding to those projects after the Federal Government built the structure.

That had to do with a long history of paternalism on the part of people in St. Louis; there are other cities where housing projects worked very well.

In my city, the public housing projects, largely high-rise, have 660,000 people living in them, and the crime rates in these housing projects are no greater than they are in the City of New York generally.

Now, I live in a private housing project, with a very, very low crime rate. It has to do with the nature of the people living in the project. It's very large, yet one day I became aware of the fact that on my floor, where there are eight apartments, I was the only resident that had only the lock provided by the management.

Now, it's a Medaco cylinder lock, break-proof and everything else, and it's a very low-crime area. Why, thought I, do all these people have two and three locks on their doors?

Now, there are many explanations for it. One is that the project has an increasingly aging population.

Number two, the only community newspaper is a newspaper that has nothing else to talk about but the one crime that may occur in this area of 50,000 residents. By the way, I stopped taking that newspaper years ago because I became aware that it was making me nervous, and I'm sure that that had something to do with my perception and my family's perception of life, in that I feel very safe there. This is such a complicated issue that it can't be easily spoken to.

REISS: Two things. I would never argue about an individual's experience; because of that I can't be as authoritative as you can, Dr. Bard. But, it's a question that can be researched. It's an empirical question. We need to look at whether or not those messages from organizations are powerful, and how they arise from actual victimizations. I am convinced of one thing, from my own research on organizational victimization, that the rates are much higher than they are for individuals. That is to say that no person that is victimized by robbery experiences robbery at the rate of, say an organization. In fact, for the cashier who is robbed, the chances are that she quits, and the organization has to hire another cashier to take her place, and she gets robbed, and so on. So they've got a turnover problem.

When you calculate the rates for organizations, they are just of an infinitely higher order than they are for even the

most highly victimized individuals. We can't look at these rates as if they were the same order of rates as for individuals. In high crime-rate areas that's why organizations have trouble with insurance.

SKOGAN: You have to determine what the exposure risk-rate for an organization is. It's also not the same as for a person, it's enormously more. A convenience grocery store accumulates a very large number of victimizations, but it is also enormously at risk.

REISS: I have to hire a lot of cashiers, my employees turn over, whereas, if I'm operating a 7-11 store out in a nice part of Denver, I don't have to do that.

I think we need to look very carefully at what multiple or repeat victimization means to organizations and how it generates behavior on the part of the organizations.

Organizations do behave. I don't see why we have to think of them as a different kind of model, and they intersect with individuals who interact with them, and they give messages out to individuals.

Art Stinchcomb remarked to me, when he went to China, that maybe property theft was lower in China than it is in the United States, because of all the new material that was left in the streets.

So, environments do give out cues. Look where people will leave their coats. When I was at the University of Chicago, we never left our coat outside the Commons dining hall, because we were afraid it wouldn't be there when we came out. Anybody could come off the street and take it. Kids would come off the street and pick the stuff up, whereas, you and I know all sorts of colleges in this country where you can leave your coat. Those are the environment's cues for offending and victimization. It seems to me that we resist that in our model construction. Organizations behave the same way.

GAROFALO: I think the way the fear of crime is defined precludes the possibility of it not being tied to an individual because it talks about the anxiety about suffering physical harm.

I don't think that we can categorize an organization that way.

In terms of your example of the organization suffering continual robberies and the turnover of cashiers and so on, I think you could analyze the fear of crime in cashiers who turnover according to the way the fear of crime is defined in this paper. I don't think that we can define Sears and Roebuck or any other organization's worry about property loss in terms of the fear of crime, as I tried to specify it here.

REISS: We have one more paper, Dick Sparks' paper on multiple victimization.

SPARKS: The paper starts, in a sense, with victim surveys and then moves away from those data. It starts with surveys because it wasn't until the first surveys were carried out that it became quite clear that if you look at populations over a period of time, most people are not victims.

Some people are victims on one occasion, but a certain number report being victimized on more than one occasion.

So the answer to the question--are there multiple victims?--is yes, and that's more or less how that finding became prominent.

The next step, and it's one that was repeated by most of us who did this kind of work, was to ask, are there more multiple victims than we expected purely by chance?

I say, in my paper and, as Mike Gottfredson pointed out this morning, the evidence on that is invariably yes; that the simple Poisson distribution will not adequately model the victimization distribution as we know it from surveys.

Now two things about that finding. One thing is, it's about time we stopped doing it because everybody has found it, and the other is, it would be more interesting that we could reject the Poisson assumptions, if they were ever reasonable to begin with. Why anybody would suppose that exposure to victimization would cluster in human populations the way the flying bombs clustered in the war in London is really not very sensible.

I do want to emphasize the limitations of our existing survey data, which are enormous. It may be that before we go on to do more complicated modeling, we ought to give more careful consideration than we have done, to the ways in which the shape of the observed frequency distribution is artifactual. I don't believe it all is, by any means, but I think some of it may be.

The distortion effects, particularly in certain subgroups, may be quite acute. One problem we haven't discussed is the problem of series victims; people who are victimized so many times, they just cannot remember particular incidents and who have over a period of time endured a large number of victimizations or a continuing chronic condition. These cases complicate the analysis of multiple victims. Studying them is obviously an important high priority step.

Most of the time we are working from cross-sectional data. However, recently, the owner of the only longitudinal data set (Al Reiss) will confirm that that data set from the National Crime Surveys has enormous problems of use in studying this. Yet, to get firm answers from these questions, we must look at the data over time.

Another problem that I mention concerns the time period. Any time period could be arbitrarily subdivided. But if you take a 6-month period in the NCS and try to look within it, you're in trouble.

What is a multiple victim? For example, if somebody is assaulted in the first of seven time periods and then assaulted again in the fourth, then twice in the seventh, when does he become a multiple victim?

Can you ever stop being a victim or a multiple victim? Are you a multiple victim if your house is broken into in 1956 and then again in 1980? Are you a multiple victim if you suffer victimization on more than one occasion at any time in your life?

There, the answer is plainly no. But we haven't even decided what frequency or what temporal periodicity defines multiple victimization.

I think that one thing that this kind of curve fitting exercise has done is potentially very harmful. It has been suggested that multiple victims would not be of any particular interest substantively, or for purposes of public policy, if they were no more frequent than a chance model would predict.

That really is a very serious error. As far as explaining victimization, you may be able to see it more clearly in the case of multiple victims because there's more of it there. The relationship between victimization and the attitudes of the people, the way they act and so forth, may be clearer than in the case of one-time victims.

I'm not suggesting, and it's far too early to say, that the causal processes leading to multiple victimization are different than they are for one-time victimization. We don't know that yet. So when I talk about multiple victimization, the sketchy theory that I'm going to talk about in a minute may apply quite generally.

The other thing, of course, is that from a public policy point of view, anyone whose life is intruded on two, three, four, five or a dozen times is obviously more of a problem than somebody who just suffers a one-time victimization within a particular time period, however long that is. They are an important group to study from that point alone.

I sketched two or three modifications of the basic Poisson probability process. There is the well known contagion model, which I think in general is a loser here. However, I do not deny that occasionally victimization may lead to increased proneness as when you get singled out as a mark in school and so forth, but in general, I'm skeptical about that. That there is some heterogeneity of proneness in the population seems undeniable.

There are a number of quite well known modifications of this process in the literature. They all generate more or less the same negative binomial values and yet, we're in the situation where we have all sorts of alternative models that make the same predictions, but not yet much basis for choosing among them.

We are not going to get any further until we start trying to pin down variations in proneness, allowing that given an individual's proneness, there is still some stochastic variation. We need to know what the causal determinants of that proneness are. Then we'll be able to predict within the limits of the stochastic variation, victimization in the population and changes in that over time as people's behavior or an organization's behavior actually change.

I want to distinguish the approach I have taken from that which Mike Gottfredson took this morning. I argue that while opportunity or exposure to risk is obviously one important intervening variable in hooking up things about people, organizations, places or situations with victimization experience, there are other things to be considered as well. I have attempted a short catalog of those, at least a half dozen.

These categories that I have listed are really the variations relating to why different persons, places, situations, organizations, or locales have a higher victimization rate than some others, on the average.

To begin with, we have to list Marvin Wolfgang's notion of precipitation, typically applied to personal crimes; for example, one person working on the other's emotions which precipitates the offense.

I suppose that an analog could apply in the case of organizations or groups. Nazi groups that insist on marching through Skokie, Illinois would be an example in which the precipitation by the victim is clear and obvious, and a reasonably analogous situation to provocation in homicide or assault.

A somewhat larger category, which I call facilitation, presents a somewhat different kind of account of variations in victim proneness. It may well be that people facilitate the commission of crimes by deliberately, recklessly, or negligently placing themselves in some sort of special risk; that is, by failing to take what would generally be regarded as reasonable precautions against crime, they facilitate the crime that's actually committed against them in those circumstances.

This is something that doesn't need to involve a transaction with the offender, but is the creation of a special risk. It is complicated because the standards are context-dependent and culture-dependent. We can ascertain what reasonable standards are thought to be in a particular area, and then identify deviations from those.

It may be that some increased victimization and, therefore, increased proneness, can be associated with the victim placing himself in high risk situations. This also can apply to organizational behavior though I have explained this in terms of persons primarily.

The next term, "vulnerability," is a term that I don't much like and I think that we really ought to use a term that Hindelang, Gottfredson and Garofalo use, which is "vincibility." I don't like the term "vulnerability" because it's been used in this literature in so many different ways. In the National Academy Report it was pretty well identified with what I here called proneness, namely aggregate variation in risk.

It is the case that some people, because of their attributes or social situations, are abnormally susceptible to crime

in the sense that they are less than normally capable of preventing such crimes. That's the general category that I am referring to there.

We have then to distinguish vulnerability in that sense from opportunity in the sense Mike Gottfredson discussed this morning.

The next category, attractiveness, is one that might seem too obvious to need mentioning, but I do think that it needs mentioning in a full account. Plainly, some targets are more attractive from the intending criminals' point of view than other. Persons who look affluent are going to seem like better prospects for robberies than persons who look impoverished. Expensive houses full of consumer durable goods are a better bet than houses in the slums. This is something that people and organizations, to a certain extent, tend to be struck with, though it does structure their behavior in lots of ways.

Women will take off their jewelry when they go to parties. I know there are limits to what one can do to hide attractiveness. You can't make a Lamborghini look like a dirty old Ford just by taking plates off and putting on other plates and saying, "dirty old Ford."

Finally, there is another category which may be important in some kinds of victimization. This is the concept of impunity. The word is wrong because, of course, it suggests the victim had the impunity, whereas what I am driving at, as I hope the text makes clear, is that some categories of would-be victims may confer impunity on would-be offenders.

I offer those as six different ways in which some persons may have a higher proneness to victimization than others. Each one is polar, so that we can think of the people as being high or low on vulnerability, attractiveness and the other characteristics.

Although I think they're analytically distinct, they may or may not be empirically correlated. It may be that people who are highly attractive prospects for theft are also those who are not vulnerable because they are well protected or they take measures to limit opportunities for crime and so forth.

I offer this as a sort of list then of things that we might think of when attempting to look at persons, their situations,

locales, and lifestyles, in attempting to classify and to analyze their behavior and their environments with a view to explaining variations in proneness.

Operationally, there may be difficult problems in definition. That doesn't bother me. Everything we have to deal with is difficult to define operationally.

Now comes the question of future research. I think that that body of writing that, by and large, has not been based on social surveys as a research technique has considerable advantages.

I'm not saying that social victim surveys, the NCS in particular, are not going to be important in the study of victimological issues. Plainly, they are, especially if we ever get the NCS to look more like a proper victim survey.

But they do have problems where multiple victims are concerned. I describe these as relating first to sampling and second to data collection.

Now the sampling issue is fairly straightforward. If victimization in any reasonable reference period is rare in any American society, then multiple victimization is even rarer. It's usually better if you're trying to look for a needle, to look in a boxful of needles than it is to look in a haystack. In a general population survey, even one that oversamples so-called high crime areas, it may be difficult to find people who have had frequent or repeated or chronic victimization conditions.

I suggest, therefore, that effective research on multiple victims might be conducted from persons we know who have reported offenses to the police. We know that they are at least one-time victims.

In some police systems, they keep records about these things. We might even be able to identify multiple victims or their addresses from police records, and that would give us better samples right away to work with.

It seems to me that in studying this particular problem, samples of officially recorded victimizations are a good first step.

Data collection problems relate, in my opinion, to the nature of the interviews that we are going to have to conduct in cases like this.

I agree with much that has been said here today about the way in which the screener and the incident forms and the general questionnaire used in the NCS could be improved. Nonetheless, for cases of the series or the chronic or the multiple victim, very much subtler kinds of interviewing are going to be needed. Almost certainly, these can't be accomplished in the framework of large-scale general population surveys.

This, too, argues that insofar as interviewing is a strategy, we need a different kind of thing from what we can expect in a general survey like the NCS.

I list a few things right at the end of my paper, a few of the very many kinds of questions that we would want to ask about high proneness groups and multiple victims. That's only a small sample of things that we might wish to investigate. Studying multiple victims, we may only be seeing more clearly processes that underlie victimization in general.

That, too, is a hypothesis and one that we can start on in the ways that I suggest.

ZIEGENHAGEN: As I understand it, part of our task is to identify research strategies and designs to pursue some of the questions that we have identified. I think that there are several questions that can be raised with respect to the paper, one which I think is very worthwhile in the sense that it does offer some statistical models which are very closely linked to a lot of the questions that we have raised today.

The difficulty comes about from adopting statistical models that have origins outside of the social sciences. After all, the Poisson process is based originally on the number of persons killed in the Prussian Army Corps over 20 years by being kicked by a horse.

We have to attempt some kind of conversion to a social science issue. I think we move in that direction by dealing with Poisson with contagion. We also may have to deal with Poisson with exhaustion as, quite simply, some victims who are seriously injured are just out of circulation for awhile, maybe in the hospital, and therefore they are not in the neighborhood where they are going to assaulted.

Certainly we are dealing with a relatively few number of cases, which is a problem. If we are going to deal with statistical models, then we should probably deal with the widest range

that are particularly adapted to the phenomenon that we are studying.

That brings me to the second point. I am very sympathetic with the six categories that Dick Sparks was talking about. The only question I would raise is, how can we move from the statistical models in the first part of the paper to the six categories of victimization proneness in the second part of the paper? This is really a question of elaboration, rather than anything else.

I think the third general area is really one of earmarking particular issues for further exploration. If we are to deal with particular attributes that distinguish multiple victims from the single-event victims, then we have to look at a wide number of categories in addition to the ones advanced. We also have to look at the results of other studies. There's a Texas study that was based upon data collected from emergency room admissions, and in this particular instance, the researchers were not only concerned with the victim, but they were also concerned with the victims' criminal backgrounds. They were able to make some distinctions between single-incident victims and multiple victims, simply on the basis of the person's criminal record.

Other kinds of efforts along these lines, I guess would have to include my own, dealing with attitudinal dimensions that seem to distinguish multiple victims from single-incident victims. The multiple victims in this case very clearly had very low expectations of good treatment as victims and had very low expectations of treatment by the criminal justice system as offenders. We are looking at another dimension that may very well be important in distinguishing between multiple victims and the single-incident victims.

The fourth point is really a question of orientation more than anything else, and this involves something like the specification of the problem being addressed from a policy viewpoint. Is multiple victimization of such severity that it requires special research treatment and special program treatment?

For example, are we concerned with it because through reducing multiple victimization, we can reduce an appreciable number of incidents themselves? I don't know. That certainly is something to be addressed.

The last point entails going back to the categories Dr. Sparks listed. Employing these categories, what sorts of policy

interventions would seem most appropriate if we can distinghish between multiple victims and single-incident victims?

In any case, I think there is a very good reason to include them, since we do have a lot of intervention programs in the field which, in one way or another, deal with the general phenomenon of victimization and may well have special implications for multiple victimization.

SPARKS: I want to respond to a couple of the points.

I would be willing to be eclectic about where these models come from. I think one of the strengths of William Feller's Introduction to Probability Theory is that he makes you see analogies between all sorts of things. You can dismiss Poisson assumptions as simplistic; but you at least reject something that might fit. We have a surfeit of mathematical expressions of the ways things might work; in our present state of knowledge that's apt to degenerate just into curve-fitting and not be very illuminating.

But once we have more of a handle on what goes on, then there's a wide variety of models that will enable us to tie that together, looking perhaps not at the frequency of incidents but at the time between incidents as the variable. There are lots of convenient ways that you can turn the issue around.

Is multiple victimization so severe as to need special programs? A very important question.

A lot, I think, depends on how we conceptualize it. In cases of spouse abuse, for example, or terrorization of kids in schools, it's not the number of discrete incidents in the counting sense. It's rather the continuity of the state that the people are in, which may well need something special.

For example, a person who is victimized 50 times--it wouldn't necessarily be the most appropriate thing to give them 50 times the compensation of a person victimized once. You might have to do something much more radical to change the social situation or the structure of their experience in some way, not just attend to the numbers.

I was interested in the study of emergency room admissions. I would like to get a reference on that. I know, of course, of your own work on attitudinal distinctions between single and multiple victims. The difficulty there is, we're not sure whether we're working with the causes or the consequences of multiple victimization. Conceivably it may be both, in that

persistent victimization may lead one to have very jaundiced attitudes toward the criminal justice system, for example, and indeed quite realistic negative expectations.

SKOGAN: Thinking about the issue you raised a minute ago about whether multiple victims are just like one-time victims only more so, my guess is probably, in the main, this is not the case.

If you think people who are victimized generally respond in some way by adapting or trying to adapt their behavior or changing their circumstances, then those who continue to be repeatedly victimized might be those who are trapped by role constraints or by environmental structuring in such a way that they're unable to make those kinds of adaptions. So it would be a special type of person who is, a four-or-five-time victim, as contrasted to a one-time victim, because of their inability to make meaningful responses. We might find quite a different kind of causal structure behind severe multiple victimization as contrasted to one-time victimization.

SCHNEIDER: Following up on what Wes Skogan was saying, I think we may confuse our theories if we don't separate stranger-to-stranger victimization from crimes between people who know each other. The kind of theories we need, particularly for multiple victims who know each other, might be vastly different from stranger-to-stranger victimization.

Something you might think about adding to your model is a little more emphasis on the notion that the proneness, or vulnerability is relative to the other targets that the offender might be screening or be able to screen in that situation. We might assume that an offender will screen victims for X amount of time before choosing the best available target. In that case, whichever victim was most vulnerable, relative to others is going to be victimized.

Victimization survey data is of very little value for testing these kinds of propositions. We don't have neighborhood data in those surveys to tell us what the relative amount of protection is.

Second, if offenders screen potential victims one at a time and each one is assessed according to a kind of satisficing model (e.g., is this one low enough in terms of its protection, suitable enough, etc.), then that's going to vary with the offender's perception of what's going to be available.

Again, the survey data is not going to be very good at testing those kinds of theories.

BIDERMAN: Dick Sparks said that historical perspective is useful. I'm old enough to bring in an historical perspective to this binomial question. The original reason I looked at that kind of model in our first pilot was my suspiciousness about the zeros and ones.

Given the number of ones there were and then the number twos, threes, fours, fives, and sixes--given that there were so many ones, why weren't there more zeros?

My hypothesis about why there weren't more zeros was that when the respondent had taken care of the task by giving the interviewer an incident, the motivation to give two or three was that much less. That would seem one reasonable theory.

You have to look at the characteristics of the data-developing situation in the application of these models. You forget to apply imagination and intelligence to try and understand what the model is about and what the phenomenon is about.

SPARKS: That's why I said I think we're still not clear about the role of response effects in producing this distribution. I'm not sure it's worth trying to build models that would incorporate that until we know more about it. I think it makes a difference whether we suppose that what is going on is completely missing of any mention of the set of events we're asking about, or whether it is some kind of truncation effect (for example, respondents who mention one or two events and think, "The hell with it; I'll be here all day if I say anything more.")

In our data we messed around with this a little bit. It didn't seem to make much difference, but that may well not be the case.

REISS: Before I shift to something else, I've seen only one set of data in recent years that fit a Poisson. The trouble is it fitted for about three cities and not for all the rest. That happens to be a set of data on police officers killing persons.

It turns out, of course, most police officers, first of all, don't fire their weapons. But if they fire, they don't kill. When you look at how many police officers kill more than once, more than two, three times, etc., it turns out it fits a Poisson, but it doesn't consistently do so across cities.

It's rare to find a set of data in this area that fits, but it turns out for Houston, for example, that it fits.

SPARKS: That's just the point, I think, of starting with this model and then rejecting it. You're not just rejecting some expected values, but rather a set of assumptions that give rise to those values. If it's going to be wrong somewhere, I want to build in more assumptions.

SKOGAN: By changing the scope of crime coverage, we probably could greatly enhance our number of multiple victims. I can think of two in particular where there is data revealing a lot of multiple victimization. One is obscene telephone calls. In Australia, among those who reported being victims, the average number of calls was five. The Canadians attempted to do a study of victims of boistrous and disorderly conduct, and for the 50 percent of their sample who were victimized, the average number of victimizations was eight.

So the rarity of the event, and the shape of the tail, is affected by the kinds of things we decided to bring in as victimizations.

REISS: I don't think we should dwell too much on the measurement. It's clear if we went to white collar crimes like consumer fraud, we could beef up the numbers beyond belief.

I'd like to turn to the causal model for a few minutes, Dick, and let me tell you what troubles me.

You may recall that for awhile one of the things that I was trying to show was that the relationship between offenders and victims, as Ann Schneider suggested, may have something to do with not so much whether they had a prior relationship, but whether it was repeated by the same or different offenders.

Now one notion I had was I could make repeat victimization go away by locking up the offender if it was the same offender causing the victimization, in other words, lock up a woman's husband and her repeat victimization goes away.

The other was that she could change her relationship to the offender. So she could get a divorce.

The first I couldn't test with the victim survey; the second I thought I could test with the victim survey data, that

is to say, I can look at her status change. Does she get a divorce, and then what happens to her?

What happened was that no matter what you do, series victimizations tend to go away awfully fast. It may be related to Al Biderman's point that there's a measurement problem. I thought that I could account for the disappearance fast by changes in status. Moving is another thing. I'm saying, as I look at your explanation now, I see those kinds of variables missing.

Am I right, or do they fit in your six categories?

SPARKS: Yes. For example, if you take the relationship of a woman who's married to a guy who beats her, her presence in the house provides an opportunity, but also a special vulnerability, in that she is presumably less able to defend herself. She may come to be seen by him as an attractive target for beating.

REISS: But it seems to me for a causal theory, and particularly a policy-oriented causal theory, it makes a difference how you conceptualize that. For example, I think in the case of wife-battering, the growth of all these homes out there and programs that provide opportunities for women to move out should in fact have an effect on the repeat victimization of women.

SPARKS: I'm assuming it does.

REISS: It's true, you can alter opportunity. But you see, for some things the victim can alter the opportunity and some things the victim can't. That's the point.

SPARKS: I agree with that entirely.

REISS: So you have to make a distinction in terms of repeat victimization as to who has the leverage on the opportunity. In the case of the offender getting locked up, if I'm a business now and the same guy has been coming in regularly and robbing, and one of the times the police get him and they send him up for ten years, that has some effect.

I think that what we need to really look at here is how substitutable offenders are in the repeat victimization case and for the type of crime.

GOTTFREDSON: I think Dick Sparks' model is very important and raises some conceptual issues. I think one of the things that it does,

when you think about a research agenda, is it seems to get a better handle on the interactive nature of victimization.

It seems to me one of the ways to flush out some of the concepts that Dick has identified, since they do in a sense relate to offender perception, is to get the offender's story about the interaction, too.

REISS: Get both victim and offender stories, yes. Even in the victim survey, we could improve it a bit by asking the victim whether those offenders are the same ones as the time before, not simply whether there was a prior relationship. They might be strangers, but they rip off my purse every time I get off the bus.

BIDERMAN: That label "stranger" has connotations altogether different than the kind of data that can produce that non-stranger response. Offenders very frequently are people that have continuing contact with victims and are known to the victim. That doesn't mean they are buddies or are in any positive social relationship with those victims at all. That's not the way the questions are worded.

So the interpretations are not the same as the identity we associate with the label. One of my cases was a woman who was old, worked as a domestic and had to ride the bus to home. The bus only stopped once every so many blocks, and she had to get off. Every time she got off, these kids that hang around on that corner, would take her purse, rough her up, call her names, and did all these things to her.

How often did it happen? Well, it happened almost every day. Well, why didn' she do something else? Well, she can't afford a taxi. She has to ride the bus. So she knows them, she knows their names. That's a non-stranger offense, but that's not the way the data are being interpreted.

REISS: A finding of a small pilot I did in New Haven, on simultaneously looking at victimization and offenders for a high school population, was that you always encounter the same problem in high offending that you would encounter in high victimization. Historically, we call it series victimization in the case of the victim. We haven't called it series offending in the case of the offender.

Whenever you have a high rate of offending and try to recover discrete instances, you have an even greater problem than you have in series victimization, because the offending rates

usually can be even higher. While I agree we ought to try to do those kinds of studies, after a while these kids would say, "How do I know?" They don't even remember whether it was the same place in which the incidents happened.

GOTTFREDSON: I think one other point Dick Sparks made may be worth picking up on. If you intercept the individuals at the official data level and interview victims and offenders in that context, you might learn some things.

BLOCK: Yes. Many of the series victims are victims of crimes for which the police data may be particularly bad as a data base. I was thinking that police data is rather bad for measuring assault. It may be bad for measuring the number of incidents of assault. So that a series victim eventually gets into police data, although the actual number of incidents don't get in at all, or just a few of them do.

REISS: Let me say there's a further complication we know when we go to something like burglary. It's only in person crimes, where the victim sees the offender, that we can even think about the victim as the main source of the data. When we get into the household and the impersonal kinds of crimes, then it's the offender that has to tell us about what happened.

I think it's an absolutely critical sort of target area that we learn a lot more from offenders, and we ought to target that as one of the areas we would support.

BIDERMAN: Not entirely. I think it's important that the crime survey should pick up victim attempts to identify the offenders on whatever basis the victim has for thinking he knows who, or what kind of offender, was involved. That's useful. But it's also useful to get data on what the perceptions of the victim is on who did it and why they did it, because that colors much of this reaction, whether they had personal contact or whether they had circumstantial evidence or somebody else told them or however. Even if it's just a pure speculation and theory about which they have a good deal of confidence, it is important because that colors the reactions of people to what goes on. And I think it's worth knowing about.

In a victim survey we can identify separately the basis the victim had for knowing and discriminate with regard to the presumptive accuracy of the information on that basis; discriminate that from being able to get more information about suspects.

REISS: I recall that for three to six percent of the property crimes, you get information on the offender. When you look at

that, they turn out to be mainly attempts, which was consistent with what you would think. That is to say, the offense was disrupted somehow and the victim saw the offender. It isn't as if we don't get anything. But we don't know if the attempts are different, that is to say that the attempts differ from the actual completed acts.

SPARKS: Well, that's perfectly true. But take burglary. We don't now ask a lot of the stuff that we could ask to get at something that would be reasonable and what the police would call modus operandi type of things. Okay, you come home, you find your house broken into: Did they shit on the floor? Did they go through the valuables? How did they enter?

BURKHART: I did want to comment that I think much of what came out in the discussion today is a reflection of some of the problems we have been grappling with. There is no simple answer for any of these by any means. One basic question is, why are we doing research that focuses on the victim? Is it to better understand the plight of the victim, the criminal act, the offender, or all of the above? I am afraid that the correct answer may be all of the above.

I don't know whether you are experiencing this sort of difficulty, but this is the problem we have had. Where do we begin and, especially, where do we focus our attention? I think to answer that question we must first answer the question, why are we doing it? So hopefully tomorrow you will address that particular question and in that way help us generate our research agenda.

SPARKS: I think, to reply to that, I think it's very important to emphasize what some of us tend to forget, which is that the question, "Why do we have the patterns and amounts of victimization that we have?" is not the same question as what causes or why do we have the distribution of crime that we have now. The whole thrust of the victimology movement has come about because you may find it useful to answer the first question about victimization as a way of answering the second question about crime.

Clearly this is so where the victim precipitates the crime, also where he facilitates crime. They are not, however, the same question. I think it is important to keep distinct the purposes for which we ask about victimization.

Conversely, as some people here suggested, it may be useful to get information from offenders in order to explain the distribution of victimization. Absent that, we have to make

averaging out assumptions about the motivation of offenders, and that's not as satisfactory as actually going out and finding out about it.

We ought to be clear about the purpose for which we do it. Is it to explain why some people suffer where others don't? And is that question in turn being asked as a way of saying, why do certain crimes take place? They are distinct questions. It's very important to keep them that way.

REISS: May I ask you to tuck away in the regions of your mind the thought that at some point late tomorrow morning or right after lunch we want to sort of get a sense of what people think are the critical things we ought to talk about in the last session, as we will try to pull together out of today's session and tomorrow morning some possibilities. It would be enormously helpful if each of you would list what you think are the one or two major issues on which research might be done that comes from either your own paper or someone else's.

SECOND DAY: MORNING SESSION

REISS: We have two research papers this morning. The first paper is Ann Schneider's paper, "Methodological Problems of Victim Surveys and their Implications for Research."

SCHNEIDER: There are a lot of methodological problems in surveys, not just victim surveys but any kind of surveys. Those that seem most important at any one time tend to depend on what it is you are trying to study and what kind of survey you already have to work with or what kind of data you might be trying to obtain. It is difficult to talk about methodological problems in victimization surveys; there are just so many of them.

I have tried to conceptualize what we're trying to do with victim surveys and deal with the methodological problems in a logical fashion. There are many methodological errors in victim surveys that I don't cover at all.

A fundamental problem is that victimizations are rare events, and the kinds of crimes that many people like to study tend to be the rare ones. There are sampling errors if you have small samples, or there is an enormous expense if you use very large samples like the National Crime Survey.

If you use a more productive source of victims, such as police records or program files, you have the problem of representativeness. It was shown very dramatically yesterday that you don't know whether those victims are representative of the population, and, further, we don't know much about who is screened in and who is screened out or the other kinds of biases in the data.

Another fundamental problem that Wesley Skogan mentioned, is that surveys done at one point in time are of limited use in testing propositions derived from theories, or causal propositions. You can't sort out the cause-and-effect relationships, what came first, what came second, even if we think of causation in a simple fashion. We need panel data. The National Crime Survey has panel data, but, as Al Reiss has told us, it's very hard to link it up. They don't follow different people in the same houses. There are a number of other problems then with that kind of data.

Panel studies are very expensive, and so for most of us who might want to do some of longitudinal study, a panel approach would simply be out of the question.

Another problem, if we're dealing with someone else's survey, is that it never contains quite the variables that we want. Maybe that's because all of our theories tend to be too unique and they're not enough like someone else's theories.

On the other hand, it could be that the people who put together the surveys aren't coming out of a theoretical framework. The richest potential source of data, the National Crime Surveys, contain the smallest number of interesting variables, at least in terms of our theories. On the other hand, LEAA is not terribly amenable to funding another victimization survey everytime someone wants to test a new proposition from a theory.

But even if you do have a survey that you might have designed yourself there is going to be a lot of measurement error, so much so that some people won't use it for substantive purposes.

Response error that's random, across victims and across offenses, won't bother us too much if there isn't very much of it, because at least it's randomly distributed. It won't change the direction of relationships amongst variables. Variables that are negatively related won't turn out looking like they're positively related.

On the other hand, the amount of measurement error in a variable depresses the maximum correlation that you can get between any two variables. So when we have measurement error, our studies are biased towards a conservative approach. Our tests of significance aren't really what we think they are and maximum correlation coefficients are not one or minus one, they're somewhere in between. There are ways of correcting or adjusting for that.

That's one kind or error, random error. Another kind is "directional error" or "bias" in the aggregate data. Surveys may systematically over or underestimate some things so that it's always too high or always too low. It has been a real concern in victimization data that we may have too many crimes or too few, or that the estimates of loss are too high or too low, and so on. The implication of directional error is simply that it gives us a distorted picture of the phenomenon we are trying to study.

Then we have errors that are correlated with other things that we are trying to study. These kinds of correlated errors are, in many respects, the worst because relationships, for

example, that ought to be positive might, in fact, even be negative if you get particular kinds of errors in your data.

For example, we know that assaults are not measured as accurately as burglaries; nonstranger crimes are not measured as accurately as stranger-to-stranger crimes. Even if error is random within a crime type, it means the kinds of propositions we develop for burglaries, which are measured better, are more likely to be supported than are propositions having to do with assault because it is measured less accurately. In fact, we may think we have a general theory. We try it on burglaries and it works for burglaries but it doesn't for assaults. Thus, we start out again trying to find a new theory for assaults when, in fact, the problem is that there are different amounts of error in these two types of crimes.

In doing research the choice of data collection method has to be guided by the purposes of the study and by the kinds of errors that are most important for the person to avoid in that particular study. It's difficult to propose any general survey strategy appropriate to all research, and, in fact, one shouldn't necessarily propose a survey strategy at all.

I started with the notion that we are not particularly concerned about measuring the aggregate victimization rate, but rather we are concerned about theory and basic research. A fundamental premise is that we must be able to tell the difference between people who are victims during the time period in which we are interested and people who are not victims during that time period.

This distinction is not as important when we're looking at an aggregate victimization rate. Some kind of errors, like "forgetting" about incidents, can be offset by other errors, such as external forward telescoping where you move events into the time period of interest. If these two exactly offset each other, then you have a good estimate of the victimization rate. But if you are interested in specifying which people are victims and which ones aren't, then these errors don't cancel each other out, they add up. You've got victims, real victims, who forgot the incident, and are not counted, and you've got people who are not victims in this time period but who may have reported an incident and shouldn't have been counted. So, these errors don't cancel; they add up.

Forward telescoping, or external forward telescoping, refers to pulling events in from outside the time period in which you're interested. External backward telescoping means

this person would not get counted when, in fact, they had been a victim. People seem to forget incidents if they're not very salient. Or, the respondent may be unwilling to tell an interviewer about it, or they underestimate some situation that would have qualified under the definition that was being used and didn't consider it as a victimization. Some people think that victims exaggerate a situation or lie and make up victimization incidents or overcount them.

Basically, those are the most commonly known errors in the survey data. In police data we have nonreporting of incidents; that is, people don't report all incidents to the police, and the police don't write down all the ones that are reported to them or they don't at least record them so that we can later find them.

In order to get some very rough estimate of how much nonsampling error we deal with in survey data, I went back through the reverse record checks that have been done and the other relevant studies. Those reverse record checks are not comparable to each other. They are done differently. The forward records check in Portland is not comparable to the reverse record checks. None of these are really comparable to the other and none of it was really done particularly for the purpose that I used it.

Nevertheless, it seemed to me we could use that data to get some notion of what's going on. The implications are that it is critically important in surveys that we use techniques to reduce forgetting and to reduce telescoping. In fact, the latter may be even more important than the former.

Richard Sparks used some techniques in his London survey that I think have enormous promise. Some of Richard's results on his reverse record check are much better than what we got on the procedures being used in this country. It's possible that we would be able to use longer time periods if we used better questioning techniques, and that would be an enormous help to those who need longer time periods or more incidents and can't afford continual interviewing.

For some types of basic research studies it isn't very important to know that a person has been a victim in a particular time period. It may be more important just to determine who has been a victim and who hasn't. In that case, then, the problem you have to deal with is forgetting of incidents, and that may not be a problem. If it's not important enough to

remember, not salient enough to remember, maybe you don't want to count it anyway.

One of the axioms in survey research pertaining to the recall of events is that shorter recall periods, reference periods, are better than longer ones. If we ask people about how many crimes or what kind of crimes occurred yesterday, that's more accurate than if we asked them about a week, and that's more accurate than if we ask them about a month, and so on.

Now, there is certainly no proof at this point, but it appears to me that the data we have suggest that, under certain conditions, a 12-month reference period may be better or just as good as a six-month period. That's a very controversial kind of statement, and I know there are people who think it's pure fiction.

Forgetting doesn't seem to drop off that much after you go back three months or so. Telescoping seems to occur in a short time period. One moves an event around, say, by four months. If you're only dealing with a six-month time period, the chance is that you're going to get too many incidents if they're telescoping forward. If you're dealing with a longer time period like 12 months and respondents move incidents around three or four months, it's not as likely to be included, or not when it should. It simply depends on the length of the time period.

When we're dealing with unbounded surveys and rather unsophisticated questioning techniques, the 12-month data that we have got may be just as good or may be better than the six-month unbounded data that we have. This, of course, does not mean that very short recall periods are poor. In fact, the very short ones undoubtedly would be superior to both the 6- and 12-month ones in terms of accuracy of when the incident occurred.

On the whole, the conclusions that I drew from reviewing the studies in terms of response errors of this type (that is, telling the difference between victims and nonvictims) aren't all that bad. Police records or program records need to capture quite a lot of the total incidents before their coverage would be as good as the surveys. The National Crime Surveys, in terms of this particular kind of error, probably aren't all that bad. I don't think that's the kind of error that people are most worried about in the National Crime Surveys. I think we are more worried, to some extent, about the kind of bias in terms of differential recall of people.

The amount of error that we have in terms of categorizing victims and nonvictims varies by the kind of offense. Burglaries are probably best captured and measured in a survey, and assaults and rapes are measured the worst.

I looked at forward telescoping to see whether certain kinds of victims forward telescope more than others and pull their events into a time period, because if certain people do this more than others, then you will overcount victimizations for those people. But, that doesn't seem to be the case. It appears that error is randomly distributed across victims. There are a number of studies which show that "forgetting" is not related to characteristics of victims.

On the other hand, forgetting does seem to be related to different kinds of crimes. For instance, crimes committed by persons who know one another are not recalled as much in the survey data, particularly assaultive violence. We found exactly the same thing in the forward record check. In that study the crime we couldn't find in police data tended to be the same one that you couldn't find in reverse survey checking; that is, assaultive violence involving people who knew each other were the ones we had the hardest time finding. When we did find them, they often were not labeled as assaults at all.

So, it seems that not only the survey data undercapture assaultive violence, but so do the police data. It may be a very, very undercounted type of incident.

We also looked at whether the information given the interviewer was the same kind of information about the crime as given to the police. Most kinds of information, such as whether the offender had a gun or got into a place, seemed to be generally rather accurate and quite similar to the police account.

We didn't find that the length of time between when the crime occurred and the interview took place was related to these kind of memory errors. It almost seems that if people remember the crime at all, they remember the details about it and tell the interviewer basically the same kind of story that they told to the police. This doesn't mean the story is true, it just means both of them seem to get the same kind of story.

In terms of the seriousness of the crimes, dollar losses appear to be estimated higher in the survey than in the police data. That is a rather consistent finding. The difference wasn't all that bad. A lot of the error is in auto theft, where the police--at least in Portland--didn't write down the value of

stolen cars, whereas the survey victims were more inclined to give an estimate.

Characteristics of suspects such as race and whether or not the suspect is known to the offender, weren't as accurate as other information. In fact, it just wasn't very accurate at all or the police information wasn't very accurate. I don't know which is more accurate, but they certainly are different. With regard to activities of victims and police, victims seemed to overstate their activities in comparison with police, and police seemed to overstate their activities a bit in comparison with victims, as you might expect.

In terms of correlates of these errors, in a pretty small sample, we didn't find the characteristics of victims were associated with systematic errors in recall.

The other thing that was interesting was that the time lag between when the crime took place and when the interview took place wasn't related to most of these kinds of errors. The time lag is related to errors in recall of the date of the incident. I think a lot of our assumptions about the importance of a shorter recall period are based on what we know about people's ability to remember the date. But it appears that recollection of the date itself is rather unique amongst recollection of other things about the crime and that we shouldn't base our understanding of the kind of error that we've got in all the data on the basis of what we know about errors in the recall of the date.

From reviewing these studies, it seems that even though the surveys have a lot of problems, most of them are usable. Researchers need to be more careful and cautious in understanding the kinds of errors in the data and every proposition that turns out to be true ought to be examined in terms of possible response error, and every one that turns out to be false, likewise.

GAROFALO: I am not going to try to make any technical comments.

I read the paper over several times, and I was impressed by the way she systematically went through the evidence, talked about various sources of errors, tried to assemble the evidence, and make some estimate of what effects those errors might have, especially on what types of substantive analysis we deal with in using the victimization survey data.

Particularly interesting to me was the finding from the forwards record check about assaults between people who know each other. Based on reverse records checks, we have always assumed that victim reports about crimes that are already in police files tend to underreport assaultive violence between people who know each other. We have always assumed that was because the victims didn't want to talk to the interviewers about it. Well, there is something else going on. When we start with victim survey and then look at police records, we find police records also underrepresent these crimes relative to what the victims initially report. So, there are some definitional problems going on. The questions of defining what is a victimization and what is not a victimization, is one of the key problems, and I don't think we can overlook it.

What we heard yesterday about the police data and the victim surveys sampling the universe in two completely different ways I think is true. We are talking about victims' perceptions of the events, and we're talking about the systems' response to particular events. Those are two completely different things, and I think we have to keep that in mind. There may be some event that the victim may not feel is a crime; there may be an event that the victim does feel is a crime and the police don't. There may be an event that the victim feels is not a crime and the police feel it is. It's something to which we haven't paid very much attention.

So, we are dealing with, really, two very disparate universes of discourse when we talk about official data and victim survey data. I think we have to keep in mind at all times that we, as researchers, are a third source. We are also trying to impose some definitions out there, and whether we like it or not we have to deal with that sort of triangulation. We have a system defining events, we have the victims defining events for themselves, we have ourselves out here trying to set up some third standard in asking how do these two compare with what we think should be some ideal. We are bound to run into difficulties.

I think we have to try to develop some conceptual way of dealing with that issue before we progress.

SKOGAN: You said right in the beginning one of the problems in victimization surveys in general populations is the rare-event nature of the phenomena of interest.

The Canadians, in one of their methodological tests, attempted to measure a "new" crime, being so offended by boistrous conduct that you thought you might want to call the

police. About half the respondents said "Yes," and the average number of incidents they recalled was eight. This was not a rare event and it had lots of multiple victims. It so clogged the data gathering process they had to drop it. The costs of the survey could only be borne because most respondents are quickly screened and didn't have anything to say. The respondent burden problem is only bearable when the average respondent is answering to only one or two incidents. When you come to a very frequent crime with a very long tail, it both clogs the administrative and data-collecting apparatus, and it also overwhelms the interview situation.

So they dropped it from their survey for precisely the opposite reason: it wasn't a rare event; it happened all the time. It's a feature of the survey, I think, more than a problem.

REISS: In looking at self-reporting of drug offenses, if you take an incident report on each one and with anyone who is a drug user, you would spend from here to eternity taking the information. So, it just clogs the whole apparatus.

GOTTFREDSON: I was very impressed by Dr. Schneider's paper. I think it's exactly the kind of thing that we need when we are trying to design some research agendas in the future. It very carefully goes through the kinds of problems in victimization data and juxtaposes those against important etiological questions. It tries to identify the sources of bias that make an impact on the conclusions we might have and identifies some very important potentially biasing effects that should focus the kinds of research that we're thinking about doing in the future.

SPARKS: To echo what Wes Skogan said, the rare-event notion tends to get paradigms, ideas stuck in our head. We think of attempted murder or even burglary and so forth. Of course, those are rare. But it's very much a matter of what you screen for in a survey. If you include vandalism or damaged property in the inner city, even in inner-city London, you will get a substantial portion of people who, if asked with sufficient persistence, will recall that somebody smashed something, broke the aerial on their car, or in some way damaged their property.

Now, that means that the volume of victimization that people can recall--and, by the way, care about--is very much a function of the demands of the interview and the structure of the interview. We ought to be careful, I think, in talking

about variance, not to forget that there are very frequent, albeit rather trivial, sort of considerations.

REISS: To say nothing of littering and loitering.

BLOCK: That's a point I wanted to ask you and Wes: where do you stop? There are some kinds of crimes that are not rare events.

REISS: You don't stop. It's a question of what you're interested in.

SPARKS: You rapidly give up the notion that it makes any sense to count discrete incidents, not because of the continuing status of terror, but just because it's continuing--you know, somebody disfigures a wall of your building with a spray can and you've got to live with it. There's graffiti down the street from my house; somebody wrote a message to his girlfriend. What annoys me about this is he misspelled a couple of words. But it's there; you see it every day.

The same thing would be true with the incidents that Wes mentioned, you know, obnoxious behavior in public. There, I suspect, there are definitional thresholds that are very important, too.

Now, on the question of response effects in relation to attributes of people, I believe it is the case in the general population in the United States, you have a negative association between age and education; that is, the older you are, the less educated. The black population tends to be younger so that there is a negative association between race--black--and age. The black population tends to be less well educated. So you have a three-way negative suppressing system. If you control for any one of those, the other correlation will get even more negative.

Now, there is reason to believe each one of those things is independently related to reported victimization for either simple mnemonic reasons or in cultural definition. Reporting of incidents because of this education effect, which is really at least a three-variable effect, is very small across certain kinds of offenses. You look at the pattern of aggravated versus simple assaults in the crime survey data, and you can't believe it. There are a lot of effects here that probably are related to attributes of people, and particularly ones that relate to interview performance in a very broad sense that we have not yet begun to explore.

I think we ought to be very cautious before we accept the conclusion that there are <u>not</u> systematic and very important differences of that kind in survey data.

REISS: I want to enter two cautions about interpreting what is in Ann Schneider's paper. One is related to a point that Al Biderman will be making in his paper, and the other one is also one that appeared yesterday.

When we talk about the household victim survey, we can make a great deal out of the difference between police data and survey data. But when we talk about the commercial survey, we couldn't make a lot out of the difference between police data and survey data. Indeed, one of the reasons for abandoning the commercial survey was that the commercial survey didn't do very much more than police data. That is one of the reasons.

The other reason was the more important, namely, that the commercial survey was a lousy survey and a lousy sampling frame. It has a lousy schedule and so on. The police data may be lousy also because they agree so well. So, that relates to the second point, namely, the message Al Biderman and I try to make all the time; it relates to the data collection problem. Now, all of Ann Schneider's data are based on a lot of assumptions about the current collection procedures whether you talk about reference period or whatever.

I am convinced that if you'd just let me find out for the sample what happened last week and forget about all the rest, or find out what happened just yesterday and use that as an estimate for the last six months or the last year, that I would be a hell of a lot better off in my count than I am with what we have now.

Now, isn't that strange? I would forget about telescoping. If you are undercounting terribly and miscounting, then you start talking about telescoping.

Now, if we do look at the data, we do know we get a higher count for the most recent months than we do for any other time. When we start looking at making mistakes, well, it doesn't make a lot of difference if your undercounting by a factor of 300 or 500 or a thousand. The message is that it's very hard to make much sense out of a lot of what you have said in your paper if at the same time you are correct in your assumptions that we may be having very substantial problems in counting.

The first error we have really got to pin down is the count problem. The same thing is true if I want to know something about offending in the population. Start and ask about yesterday, "Did you do anything yesterday?" And, you know, there aren't that many offenses for yesterday in a population of high school kids. Then you ask them next, "Well, during the past week, the rest of that week?" Then you begin to get a sense of what it is that you are counting, what the rates of offending look like in that population, the time between offenses, how many people are victimized more than once a week.

I think that is a central issue here, and the whole question of other kinds of errors are fundamentally linked to that kind of error.

SPARKS: As you know, I think that with proper interviewing procedures we can get away with a 12-month unbounded reference period, but that's a hypothesis. What we're talking about, really, is the so-called "reporting load effect," not merely the difficulty of recalling events earlier, but the fact that you've got more to remember when you ask about a longer period, which may be very, very severe for some of these high-frequency things. Optimum length of reference period has not been tested because we have not given similar sample recall for one month, three months, six months, 12 months, and then looked at the one month period within all of those groups.

REISS: But, Dick, don't turn it simply into a reference period problem. I am saying any other kinds of errors we talk about, while they are related to the reference period, it's the count problem that is significant. That is to say, if my total undercount is just enormous, I have a capacity to swamp any finding I make.

SKOGAN: If the undercount is enormous, then a short reference period will still yield enough events.

REISS: If we go beyond our six crimes or seven that you are ordinarily interested in, things get rather interesting in terms of how they occur and when. Much more interesting than they do for those ordinary crimes.

BIDERMAN: Mike Gottfredson said that this paper is exactly the kind of thing we need. That's correct. It's also exactly the kind of thing we don't need, because it's riddled with mistakes and misinterpretations. She has seven conclusions. Six of them are riddled with error. The seventh is one that is true but only depending upon interpretation of what a "serious offense" means.

That one is number five, "Evidence is accumulating that less serious offenses tend to be subject to more extreme patterns of memory decay." That's true in some senses but not true in others, because things that may be serious on the Wolfgang-Sellin Scale may not be terribly serious to the victim, and vice versa.

All the rest of the conclusions are very seriously in error and are extremely misleading, and sometimes they are just downright backwards. Now, some of them are based on a very important research by Schneider herself, but research which has so few cases that making conclusions about the absence of a difference when you do a Pearson correlation on 16 cases is just plain not the kind of thing to do.

Ann Schneider is absolutely backward with regard to the effects of length of period in the particular research procedures used, or survey procedures, with regard to the consequences of reference period on things like telescoping and the resulting accuracy of the identification of victims versus nonvictims.

That effect is not a function of the ability of the human nervous system to store information, nor of the ultimate capacities of the interrogatory method to yield that information, but it is a function of the specific techniques that are used in the surveys.

Now, I have used extremely long reference periods. For example, in research on detainees and prisoners of war who were captive for five or six years it was possible, I think, with considerable accuracy, to get moment-by-moment accurate reports of what happened to those people on the day they were captured.

We worked with people, interviewing people over as long as three months, interviewing them individually and interviewing them in groups. Now, that's a very different kind of thing than running through a screener with regard to heterogeneous events covering scattered possible dates and where the person's mind is, as it were, being scattered all over the place by a screening device. The longer you make that recall period and the more heterogeneous you make the kinds of things a person has to think about, the more there is to scan. The more heterogeneous and confusing that scanning pattern has to be, then, well, the worse the data.

That's obvious, not only in the data, but in the most simple experiment you want to make on yourself on what you can

remember and with what accuracy and what you will forget. That's true with strong perturbations because of particular anchoring points like certain of these surveys have had. I have had the first of the year as the beginning of the recall period and consequently you get some bunching up of incidents around Christmas. We have advocated that as a device to help people anchor.

But currently, there are extreme problems with the six-month period because it's too long. And the three-month period, we know from internal analyses of the data, seems to give you much less noise in the data than six. Using 12 months, with the procedures we have, as it was in the city surveys, just gets too difficult so that we had to reject the data.

Now, that doesn't mean that you can't adopt devices like Sparks has done which may make a 12-month or a two-year period pertinent, or, if you are after relatively homogeneous events, you can make it four or five years. However, you have to take a lot longer time and have a much superior device for getting at it than the sort of screeners now in use.

GOTTFREDSON: I think that this is exactly the kind of approach that we need; it seriously asks the questions concerning what kinds of bias with respect to what kinds of findings and takes a look at the data to try to unravel those.

In Dr. Schneider's paper there are words like "the evidence suggests," "the evidence is accumulating," and so forth. Some hypotheses are tentatively held and the data brought to bear on them are also tentative. I think that that's a reasonable thing to do.

With respect to the issue of Point 5 in Dr. Schneider's paper, evidence is accumulating that the less serious events tend to be subject to more serious patterns of memory decay. That's true, when you read the paper and discover what the operational definitions for serious offenses were. That conclusion is tied to the operational definitions as they were used in the paper.

BLOCK: It seems to be that there are two things we are talking about. One is, are we concerned with the effect of victimization or are we concerned with the amount of victimization?

I think that the reference point argument is really dependent upon which of those is our concern? If we want an accurate count, then we can't have a very long time period. If we want

to know what the effect of victimization on someone's life or life style, then maybe we need a longer time period.

It seems to me that the argument here is going back and forth between these two components of victimization. There is a difference between talking about how victimization affects someone's life and getting a count.

REISS: I think in a conference like this, we ought to avoid thinking that the victim survey is the only way to measure things. It might be more appropriate to ask the question, if we are interested in certain consequences, is the survey the best way to do it? Is the current victim survey, even no matter how much we dress it up, the best way to do it?

I think everything we know as social scientists tells us we ought to have at least two ways of doing anything in order to find out whether one way is any good.

What are the alternate ways, given certain kinds of questions? We ought to be paying some attention to that. The victim survey gets lots of attention but we ought not to devote all our time to it.

If you're trying to find out what the consequences of victimization are to people, is a retrospective victim survey the best way to do it, or a prospective survey technique in which you follow people the best way?

WOLFGANG: I would agree with you on that, especially on the matter of not relying upon the survey to answer all of our questions about victimization. We need additional research and I thought that that was one of the reasons we were here, to set up some kind of agenda for it.

I would agree with you also about the desirability of prospective surveys, of following people once we have captured a proper population.

One of the issues about error is a definitional problem that Jim Garofalo was talking about; the system response, the research response, the individual interviewee response to what happened, and what label is given to it.

The police term called "founded" or "unfounded" I think is a very important one relative to any disparities between the victim's interpretation and the police interpretation.

My own personal anecdote concerns my house. I have 12 brick pillars. On top of them is a concrete kind of cannon ball. One morning I came out and one was removed. This is an early 20th Century house and there was a lot of that stealing from Victorian houses and Edwardian houses going on. When I reported it to the police, the police put it down as vandalism. I insisted that it should be put down as burglary.

When I checked to see how much it would cost to replace that little old thing, it cost me $500. I followed it up and I wanted them to report the amount involved, too. But if I hadn't intervened, it would have been simply vandalism.

Furthermore, as time passes, differential definitions may occur. In the immediate moment, in the passion of anger, one may call an incident an assault and even call the police as simply an anger response to being hit by an acquaintance or an uncle, or by a spouse.

As time passes, we know there is a dissipation of that anger and passion and we reinterpret, redefine the situation. Now it's simply a squabble among intimates. It's not something for intervening of external agencies to be involved in.

REISS: Marvin, I'm glad you brought up the issue of the cost of your concrete ball because I wanted to comment on Anne Schneider's paper about what we should not be doing either as police departments or as survey people. And that is trying to find out how much things cost.

I did a study a number of years ago which helped get rid of the under and over $50 in UCR reporting as a result of showing it was an insoluable problem.

What do I mean by an insoluable problem? I said if the IRS can't solve it, what makes police and survey research people think that they can solve it?

It's nonsense to think that we can calculate those things in any reasonable way, that it makes sense that we talk about value of goods stolen, and we don't know how to do it.

GOTTFREDSON: I'm just trying to follow up on Marvin Wolfgang's point for a minute. I think there is quite a bit of agreement about that as being a critical issue, particularly in trying to get a better handle on assaults.

I think in terms of trying to come up with a research plan that would be suitable for that kind of study we have some major impediments. We can't get inside of people's houses and observe and the survey data don't seem to do the job for us.

We need a new strategy.

WOODSON: That was the other footnote to what Marvin was saying. In certain communities I know, particularly Dominican, you have to understand what the acceptable level of violent interchange is and how people determine it.

I know in certain communities in New York, the Dominican community, violence and retaliation for previous assault is seldom considered a crime because it's then resolved.

So if someone was to come in and ask questions about were you assaulted, you would get a different answer.

I think that whole value perspective of the researcher is a factor in an outcome.

ZIEGENHAGEN: What this really seems to suggest coming from a number of directions is that we ought to be paying a lot more attention to subjective assessments of the incident.

The first thing that comes to my mind is the use of the old feeling thermometer in a lot of the political surveys. If we are really going to talk about victim behavior, then we might as well get into this sort of assessment as well.

GROPPER: With regard to the kinds of remarks that Bob Woodson made yesterday, could we work to clarify our categories of interests, and their relations to our research tools; categories of issues that either our present type of survey mechanism or other mechanisms might illumiate?

We have tended to focus on one broad category dealing with descriptive concerns--what's happening to whom, how much of certain events occur, counting various types of victims, etc. But we also obviously need to deal with a second category of explanatory concerns about the causal linkages between them, and the roles of the particular actors (the victims, the offenders, etc.) in creating the events or influencing their outcomes.

I'm wondering about our abilities to deal with both these kinds of issues with our present mechanisms. How much could a modified victimization survey help illumiate that second category beyond what is already has?

REISS: I think we need to measure the value of goods, the value of things, the harm and the whole question of what do harms cost and not simply in dollars.

That is an area that desperately needs to have some research done on it. That's the consequences of harms and their cost.

I know my friend Dudley Duncan says, Al, you people are just out of your minds. You don't understand what that burglary meant to me. It meant not only the kinds of thing that you were talking about, but he said, you have to understand they took my father's watch. And nobody is ever going to give me back my father's watch. There is no replacement value for my father's watch.

We don't understand all of those things, the kinds of things Marvin Wolfgang was talking about yesterday, the kinds of things that Al Biderman has talked about in terms of harm consequences.

The dollar loss problem is something that we have been stuck with because of the tax people and economists and that's not necessarily the way people out there think about them.

ZIEGENHAGEN: That's right. We ought to be interested in how victims do think about them.

REISS: I agree. We ought to earmark this as really one of the significant areas of crime for research.

The victim survey is not going to solve it. It shouldn't have the burden of solving that problem. There are alternative ways of measuring it.

BIDERMAN: The victim survey is going to come closer than the police reports, which is one of the ways, one of the kinds of statements in Ann Schneider's paper I find misleading.

I think we can come down to a more definitive conclusion about it, at least some of them, because the Census Bureau people have published some egregious nonsense about, and misinterpretations of, data.

They are repeated here and elsewhere, and I think they are important to pin down. Dr. Scheneider's last conclusion is that the amount of time that elapsed between the incident and the

interview is generally not related to the amount and the direction of error in the data. I say it certainly is. I would like to know what evidence there is for that kind of proposition.

There is also the proposition that errors aren't associated, aren't strongly correlated with characteristics of the respondent. I say they are in ways that allowed extremely distorted interpretations to be made of these data. There are powerful relations and some of these have been suppressed because they do get in the way of interpretations of the data that people want to make.

I think that the question about offender identification by respondents is completely open, contrary to the statement here that we know something about it by the matches against police records. What are we talking about when we say offender identification? Do we mean racial identification, which is one of these identifications which are mentioned here? Are police records or the respondent interview, testimony, the more accurate? Is an age identification within broad categories used? Is any identification of the offender at all something which also relates to the question of the length of the period? Do you have any information at all on the event?

We had at the very beginning the assumption that victimization is a rare event. Now, victimization in specific classes seems to be. I don't think victimization, in general, as surveys indicate, is a rare event, particularly since it depends upon what chunk of life experience you want to take. Sure, it's rare in a second; it's rare in a day. But in six months or a year, is it rare?

SINGER: I just want to add something as far as the relationship between social class and distortion. In the cohort follow-up data, where we have a little more of the other demographic variables of social background, there is severe distortion in the expected direction. As Richard Block mentioned with the simple assaults, we can see that gang members and people who use weapons report less often being a victim of a simple assault than those not gang memebers. We would expect the opposite relationship. They are picking up being shot and stabbed, but they are not perceiving being beaten up or pushed around as significant in their past.

It's just not that significant in their life to be in a recall to an interviewer, especially compared to middle-class white respondents for whom assaults are quite significant events.

REISS: We're going to give Ann Schneider a chance to summarize.

Anne, if, in your summary, you could address this question about the offenders, I would appreciate it because it left me rather confused, since I have tried to cope with the age data on offenders and it's no simple matter.

SCHNEIDER: Sometimes I get accused of being overly cautious; other times, of being outrageously speculative. Today was the latter, I think. I usually find myself in agreement with Al Biderman. Today is not one of them.

But I would agree with one point, these are open questions; the evidence isn't all in. We don't have very much good evidence on a lot of these questions. If we take the bits and pieces that we do have and put them together, then it looks like the way I have described. Whether that's the way it will turn out to be with more evidence, I don't really know. If it doesn't, I will recant on this.

The correlations between victim characteristics or offense characteristics, and the error patterns, are very really dependent on which kinds of errors we're talking about and which characteristics of victims are being considered. In particular, our evidence is very scanty on the problem of forgetting, which I think most of you were talking about. The victim correlations, in terms of "forgetting" are based on some different methodologies than for some of the other kinds of variables.

On the offender identification, that's based on a very small number of cases. It's not based on 16 exactly, but it's based on a very small number of cases and it concerns race and whether the person was known or not. For those two particular characteristics we just didn't find all that much correspondence between the interview account and the Portland police department account. It is really hard to say who is right, and the fact is that subsequent information, either to the police or to the respondent, but not to both, could have distorted that to some extent.

I think these are tentative kinds of propositions about what we think the errors are. As we design a research strategy, we need to think about the kind of data in terms of the kinds of errors and biases in that data.

I don't want to encourage people to be overly optimistic. I think maybe Al Biderman is overly pessimistic. We could do a

lot better than we are now in terms of improving the survey data.

REISS: I want to make one observation of caution to all of us. You defined a source of error as "forgetting." Forgetting is a hypothesis; it's an explanatory system. If we operate on that hypothesis, we are going to go after measurement in one way; if we operated on the assumption that it's not forgetting but what we are doing, that's a totally different hypothesis.

Similarly, the notion of recall. Remember, I said yesterday that all methods are based on laws about human behavior, but we have to use methods to discover those laws. This is a very good example of it. So, I am saying we need to leave it open. As for the question of whether people are forgetting, I don't believe they forgot.

SCHNEIDER: I have gotten accustomed to using that word. It's just a catch-all word.

REISS: But it does get in our way of thinking about this problem.

SKOGAN: We have another construct that we use: telescoping, which is the opposite. The term is not explanatory, but descriptive.

REISS: This is the last of the papers and Al Biderman will open with a statement on data sources for victimology.

BIDERMAN: I just have several objectives. The first has already been accomplished, which is to show that social scientists can't necessarily predict, as Reiss evidenced in revising his estimate as to how long I would be taking for the remarks I will now make.

My other objective in limiting my remarks here is to compensate for the amount of time I have consumed all during the rest of the proceedings. I think I have touched on much of the spirit of the paper and a good deal of the content.

Finally, my recall is not sufficient to distinguish what I did include and what I didn't in previous remarks. So I will leave it to the discussion to bring out things that might have been in parts of the paper that anybody read that they would like opportunity to challenge or to clarify.

SKOGAN: There are three interesting points in this paper I would like to comment on. The first has to do with what Biderman calls the problem of stereotyping of victimizations as incidents

in time. He argues that there are important benefits to be gained from looking at events not as incidents, not as things bounded in space and time with a distinctive beginning and a distinctive conclusion, but rather as a continuing series of conditions that characterize the lives of persons.

He talks at some length about that notion of victimization as a condition of life and suggests that these may be particularly important phenomena to understand. The difference between zero victimizations and one victimization may be from the point of view of the victim, largely random. It may be a happenstance incident and not part of a pattern. It may not occur again and has relatively few policy implications for the criminal justice system. Events which are more accurately characterized as conditions, where there is repeated contact between a victim and some class of offenders, and repeated instances of harm, may offer opportunities for intervention on the part of social agencies which are greater than for the vast bulk of crimes.

These conditions may be relatively rare compared to one-time criminal events, but the payoff in terms of being actually able to treat the condition may be greater in this class of events than the more frequent one-time events.

As an example of the kinds of incidents that involve conditions of people's lives, many seem to be job-related, or related to kind of social roles people play, and to the relationships between the parties based on blood or marriage, or relations among neighbors.

Thirdly, Dr. Biderman mentions incidents which arise among those occupying a common space, like a school, on a regular basis.

It seems that the point-in-time incident stereotype is a very useful way of thinking of certain classes of crimes. States of victimization may have distinctively different conditions from what we might think of as normal crimes, and may present many more opportunities for use of policy intervention, some of which may not be by the criminal justice system. Many of these may be as well characterized as social service problems or social work problems as they are criminal justice problems.

The second very interesting set of arguments in this paper have to do with focusing victimology around physical inquiry and individual and household economic loss, using those as foci for organizing data collection efforts.

This is particularly useful from the point of view of agencies other than the criminal justice system. Injury and loss are crucial from the point of view of other parts of the social service network that can supply insurance advice, solace, or assistance to victims. For them, the concept of harms is the central focus, the things that actually happen to people, rather than in the intent of the offender or the circumstances under which it took place.

So if we think beyond the criminal justice system to other kinds of agencies that deal with victims and the consequences of victimization, the harms focus looms greater and greater in importance. Biderman recommends focusing victimological research specifically on harms and tracking back the etiology of incidents, beginning from the harms rather than beginning with the incidents.

The third major discussion has to do with alternative mechanisms for gathering data on victimization.

You may notice the title of the paper is "Sources of Data for Victimology." Much of our discussion in the last day has revolved around victim surveys and so is the bulk of the discussion in this paper. Discussion about alternative mechanisms is relatively brief.

There is some talk about police as agents for meaningful data collection for crimes involving victimization and for trying to harness other social agencies which victims turn for help or assistance as alternative gatherers of data about victims. This would be particularly strong in the case of detailed data about harms. There is much discussion about the particular advantages of victimization surveys over either police or social agency data on crime.

But in many ways, we have a mindset that locks us into victim surveys as the most effective means of gathering data on victimization. There has been no serious discussion that I know of a data-gathering organization which could effectively gather police, social agency and survey data in some coherent fashion so that the resulting data could be used to make estimates of the incidents and the consequences of crimes on a national basis.

For example, as Al Reiss pointed out, there is some evidence that police data on commercial crime may be of relatively high quality. The problem is that most police departments have disorganized record-keeping systems, so we still have no

national capacity for tapping into them on a sample basis. The fact that that good data on commercial establishments is hidden there doesn't do us any good. We need an integrated data collection mechanism which could tap police departments for the kind of data which they collect well.

Another arm of this integrated data-gathering organization could tap hospitals and social agencies for the data that they could contribute, all integrated with survey data which would focus its attention on those things which distinctively can be best measured and evaluated through surveys.

This plan for an organizational arrangement, for the integration of social inteligence, is certainly beyond the scope of this particular paper. However, it seems to me that's the direction which everyone thinks we ought to go. Perhaps it's time to think systematically about how we could build a national sampling frame of police departments for that kind of data and a sampling frame of social agencies for their kind of data.

BIDERMAN: Some of the full nature of my thinking about these topics has not been clearly represented. I would just try to elaborate on a few of these topics. One is that the paper deals primarily with the time dimension and the importance of considering extensions of that time dimension and the importance of the contributions of the victimiological orientation to the criminological field by broadening the time dimension backward with regard to the causes of events and forward with regard to the significance of the consequences of events.

There are constellations of events. There are actors with a multiplicity of involvements. The place is not always one place. The time is not an instant point in time. The concept we have of the guy who jumps out of the bushes and mugs somebody is not applicable to some portion of victimizations and we haven't the vaguest idea of how large a portion.

So one of the things on the agenda is to fill in the things that don't fit the stereotypical conception with regard to any of these dimensions.

Wes Skogan said that my focus was on the physical injury and the economic consequences. I was really saying that the victim survey has that focus. But I also said that most of our data focus on psychic harms, and, while I think the psychic consequences are very great, the durable physical consequences have yet to be dealt with adequately.

Another important theme of my paper was that the most important consequences are not individual, they are social. The most important consequences for the individuals are his attitude with regard to the social meaning of those events. That is very poorly handled in almost all of the data sources that we have.

On alternative sources to the victim survey, my discussion was very brief, but it raised the general matter that our data are dependent on organization.

Our efforts as statisticians and as social scientists affect social organizations; the agenda for the reform of data is an agenda for organizational reform. Without changing the character of organizations, we can't improve the quality of our data.

There are three classes of ways in which organization may change to yield better data.

One of those is the way in which we organize the enterprises which are essentially our own (the social scientists), those that are specifically for data development purposes.

The second is the ties, the essential ties that data development has to other organized activities of the society. The third is that much of data just depends upon how those activities organize their own record-keeping systems.

Now one of the things that has been going on, partly because of the general change of society, particularly towards statistics, is to extend the scope of things that are defined as events that are worthy of explicit social normative definition and become subject to organized social action.

Much of the victimology movement has resulted in the creation of organizational forms which are defining things that society should do something about and should attend to systematically that previously weren't in that scope.

I talk about two ideal models, one in which everything, every harm would be subject to attempts at explicit social prevention or remediation, and another, a Hobbesian model, in which none would be.

The general movement is towards the former model, and with that movement, many new sources of data develop.

The quality of those data for any of the purposes that are to be addressed is very poor because of the failure to incorporate into the principles of organization those necessary for the production of data.

REISS: Just as a footnote, I have always thought that the great strength of Marvin Wolfgang's book on homicide and the one on rape by Amir was that they looked at these patterns so that one knows at least from the police data that a lot of rapes don't take place at a single place. They are moving.

It's exactly those patterns in forcible rape and the patterns of criminal homicide that you are saying we need to know a whole lot more about. I suspect from the standpoint of future research, we're talking about getting more of that kind of research so that we can understand it.

Is that right?

BIDERMAN: Yes.

WOLFGANG: There are so many ingredients regarding the situational aspects that preceded the homicide drama that I think are important both to criminology in general and so-called "victimology."

One of these, for example, is alcohol.

REISS: Which we know so little about.

WOLFGANG: We know an enormous amount about alcohol. We know a lot about crime. We don't know much about the interaction of the two.

REISS: It's often present in the rape situation, and in the homicide situation. I found in my police observation studies, that in about four out of every 10 calls that the police make to a household, we smelled alcohol. That was our test.

It has to do with weapons, too. We found that in certain areas, for the number of people that the police did search, many weapons were found. What we never knew was what would a random sample of shakedowns show in these areas.

SPARKS: For alcohol, though, it is known. Alcohol research has been extensive.

REISS: Not at that time of night and that kind of community, under those kinds of circumstances. We don't know.

SPARKS: I would be surprised if they din't have some handle on it. I think this is one of the places in which we have been remiss in not looking at other kinds of specialized data.

REISS: I haven't seen detailed household surveys on drinking. There is an anthropologist who tried to estimate drinking on the basis of looking at peoples' garbage. The idea is that people don't tell you the truth about their drinking pattern, so you look at what they put out for the garbage man to pick up and you go along with the garbageman and you see how many bottles of what kind are dumped.

The reason I bring it up here is as an alternative way of estimating something. It's a survey of a kind. You go with garbage trucks, and of course there are all the problems that go with that kind of survey.

BLOCK: When you think about alternative mechanisms for gathering data on victims, you have to remember what your base is. If your mechanisms are social agencies or if your mechanisms are police, fine, maybe it's very important to study the victim with regard to the police system, or the victim with regard to the social agency. But you aren't studying victimization, you are studying the victim in those agencies. Alternative mechanisms to study victims have to start with the victim as a base.

I think to get some measure of what is happening to victims, or harm to victims, you have to start with the victim.

I have found that concept of harm extraordinarily useful in looking comparatively at victimization surveys and differentiating victimization in the United States and in other societies. Looking at differences in levels of harm in victimization surveys, as defective as these surveys are, there are really tremendous differences in the impact of the victimization on the individual.

SPARKS: I think Dr. Biderman's paper is very interesting and valuable. But it has running through it an assumption that might be read into the paper by others, and it's wrong, totally wrong. On page one he is talking about indicators for the normative components of this definition, such as victim, offense, and harm; "These concepts remain nonetheless normative," which is okay. The next sentence says: "Hence, there are extrascientific, evaluative components intrinsic to any data for victimology." I think that is, at best, very misleading, because

while there indeed are normative components in all of these concepts, in the sense that they involve the application of the rule, that does not deprive the concepts of a factual reference. You can test the application in particular cases against what would be a correct interpretation, and that is just as objective, just as factual and scientific as the question of what constitutes a cow.

BIDERMAN: I think Dick Sparks is confusing what is made explicit in the paper, the difference between a concept and an indicator. A value concept is extrascientific. The value component of the concept is extrascientific. It's existence in terms that somebody arrives at a particular value judgment, has particular preferences, is ascertainable. But the source of that norm is extrascientific.

The sources of evaluative judgment need not necessarily be those of the data recorder. The recorder of data may reflect the evaluative judgments by others.

Some of the normative concepts are nonobjective because they are subjective, and they are only ascertainable by either a verbal report or by some inference to a concept from some other behavior, as in observing people's choices. The value component is a subjective concept, "intent" is subjective.

We also have great difficulties in operationalizing normative indicators, indicators of normative concepts. Let's take the case of the harm. In that harm survey you have cases that come about because of automobile encounters. Now, how do you tell whether somebody is driving hostilely towards you? When is a particular maneuver of another driver unambiguously hostile, rather than a mistake? You may not even be able to ascertain it reliably from the standpoint of various psychodynamic theories, with regard to relation of hostility to driving behavior, from self-reports. You might be able to hypnotize the guy and make some inferences as to whether the normative component in that driving behavior related to hostility. We rely on indicators to which we attach particular interpretations, resting those interpretations upon the judgments of observers with regard to a normative criterion that has to be ultimately their application of a normative criterion and which, in turn, relates to their own normative set.

SPARKS: If you say that, then you are perilously close to saying that he can't be wrong. Now, if I take a particular article and I say it's a good carving knife, it is, in your sense, a value judgment. There are criteria for being a good carving knife,

and they are logically related to a judgment that a carving knife is good. If it's dull, the handle doesn't work and so forth and so on, it's terribly heavy, it's not a good carving knife.

The applicability of those criteria derives from the purpose.

You need to distinguish between what you perforce must measure in social research, particularly in survey practice where you have to rely on indicators that have varying interpretations, from the notion of what you are really trying to measure and what could be, in principle, observed by God if only he existed.

REISS: I am having trouble with what you are saying because Al Biderman and I both think very highly of a small book by William Lowrance called Of Acceptable Risk. It's a very important book because it deals with the question of what is safety in relation to risk, which enters into this whole area. Lowrance is a biochemist, as I recall. He's trying to deal with this question of acceptable risks that are thrown to scientists all the time.

What he says is we, in science, can determine risks. We can't determine acceptable risks. That is, in Al's terms, extrascientific, evaluative and so on. You can give us that and say that it is not acceptable if five people per 100,000 are killed.

It's critical in this whole area of crime, because we are setting crimes in terms of those consequences. We are saying that there are risks associated with generation of nuclear power, or even going to the dentist's office with X-rays. Everytime we get X-rayed in the dentist's office, we are being harmed, in some sense. At the same time, as Al says in his paper, there is good being done.

WOLFGANG: But we can "scientifically study" these various kinds of evaluative decisions that are made.

REISS: Right. We can study those, but we cannot say acceptable risk, or safety is at point X.

BIDERMAN: Well, I tried to aim at a level with regard to philosophical matters that could proceed fairly consensually. I am sorry I didn't succeed. I think there may be such levels. I don't think it's at the level that Dick Sparks suggests, however, because I do believe any normative concepts are not absolute.

It has been a long, long time since I studied ethics, and I remember in that course we spent weeks and weeks on these questions and much of the discussion time spent was terribly sophistical and not terribly valuable with regard to their implications for research operations.

REISS: One of the most interesting things to study in this context is all of the vast debate over when are human subjects harmed. That is to say, what are the consequences of our interventions into the lives of people to collect data and when is that harmful? What consequences does it have, and what do we make of that? That is also a harm-oriented system. What is fact and what is value in that area? We can only know by studying it, but we may be precluded from studying it, because of the value dimension.

I think we have a lot of consensus that we somehow need studies of the consequences of harm, and we want to think about what might be the kinds of studies that ought to be done.

A second area in which it seems to me we have talked about yesterday, was the need to somehow study what nonvictimized people did to avoid harm, the whole idea of precautionary behavior and so on. We know relatively little about it. The crime survey doesn't tell us about it.

The third big area that we have earmarked is one that Al brought up today. Marvin has talked about it also. It's the whole question of the nature of antecedents and consequences, not simply of harms, but of events. What are the antecedents of events, what is the nature of their situations, what is their duration, all of the characteristics that relate to events about which we know so little. I think that there is at least some consensus about that as a third area.

WOODSON: There is one area I am not so sure is covered under avoidance of harm, and that is an examination of people's capacity to change behavior patterns of victimizers and what happens as a consequence of victimization. We know that happens, but it is an area that has not been studied.

REISS: Studies might be conducted looking at offending behavior as a means of getting at victimization. That is to say, knowing a lot more about what do offenders do, how do they select victims, what is their sense of targeting, and so on, and how many people do they victimize, and so on, and the whole relational element, which is certainly a fourth and very important area.

Whether you accept that epistemological point we might agree that we know far too little and that the victim survey is just one socially organized way or means of knowing. We have to spend time looking at alternatives such as observation, which is, again, a socially organized means, and what we learn about victimization through such different means of knowing.

BIDERMAN: We could also look at specific organizations. Terribly germane to much of the discussion we had today was the way in which the victimization survey was socially organized and, specifically, the relationship of the organizational location, the organizational form, the organizational connections of the victimization survey relative to the organization of research.

Much of the lament here about the victimization survey comes from a research orientation to this data that was not built into the system, or from the criminologist's orientation. This is very much an organizational matter.

Similarly, much of what we object to about the victimization survey are characteristics of the Bureau of the Census and the way in which that particular social organization for collecting statistics has evolved.

SCHRAMM: We can go through a long list of what we want to know about victims and victimization.

I think the main issue for me as a practitioner is why do we want to know this information, what are its policy implications?

If we simply want to know this information for proposition testing and prediction, I can accept that, but I think that should be right out front. However, we might want to know some of this information because it has some potential for being translated into policy or has some significance for prevention or intervention in criminal incidents; for assistance to victims in coping and adjusting; for assistance to victims with the criminal justice system; for legislative change; or for illumination of our knowledge of criminal events. I think it would help if I had some guidance about the extent to which significance means anything in developing this research agenda. I think I need to know whether we're talking about developing an agenda for funding through the justice system or we're talking about general areas of basic research that might be done sometime, somewhere and are important.

WOODSON: That's not at all what I had in mind. What I had in mind was something a bit more positive than that: How do local people interact with young people, gang members, and others, to interrupt cycles of crime and victimization, and what impact does that have on changing the environment and reducing victimization? There are some things that are done that I think lend themselves to measurement, but haven't been attempted.

REISS: I agree. I probably misstated the second one badly when I said "need to study the nonvictim." It's the question: How do people avoid harms, how can they collectively do so, what kind of collective efforts do they make and so on?

BLOCK: I would like to see us discuss the impact of the victim on criminal processing and how the nature of the victim affects criminal processing.

SINGER: Following up my work, I would like to examine the basic question of how people come to obey the law as related to the victimization experience.

ZIEGENHAGEN: I don't know if this was covered earlier, but I think we have kind of a general category called "coping behavior": How do victims respond to crime in respect to altering the environment?

WOLFGANG: As a corollary to victimization avoidance strategies, we need to study not only avoiding being victimized, but once in a posture of victim, the development of strategies and techniques to reduce the harm, the degree of harmfulness, of that victimization right within the immediate situation.

SKOGAN: That's what I call the "dubious" area of research.

REISS: There is the whole question about the relationship between information-generating systems and what we know about victimization, which I think is probably an area of extremely high priority. It generates, in part, from and old, old paper that Al and I did years and years ago, which says the only ways of knowing are socially organized.

We cannot know except in terms of some socially organized means of knowing. I think we might all agree that we can at least compare the consequences of different socially organized means of knowing whether or not you accept the epistemological point. It is in that sense truly at the level of epistemology.

I have not heard anyone express a concern for the need to develop "cumulative knowledge" in victimology. I believe that we need to identify areas where we should build our knowledge base vertically, either through multidisciplinary approaches or through the use of successive methodologies that address the same issue. We should begin to develop some strategies for dealing with or looking at an issue, whether it be for the purposes of model building, prediction, or application. Thus, from my perspective, it is important that a research agenda emphasize the development of that cumulative knowledge.

REISS: You are saying we need to pay attention to the cumulative nature and the consequences of knowing what we want to know.

SCHRAMM: Yes.

REIS: There is one kind of level I am not sure we can get to, Donna Schramm, when we talk about why we want to know and the consequences of knowing. When we get to certain socially organized forms, such as victim compensation. I remember a paper where Al Biderman said, "What kind of society has victim compensation schemes? It's a society which treats people badly, to begin with."

That is to say, victim compensation selects out a particular class of victims in the society. So, there is a very interesting question about why this is done. It's a very tough topic to deal with in the victimology area, but maybe we can talk about it.

GROPPER: In planning for these sessions, we considered several approaches to structuring our discussions, including a "before, during, and after" scheme. The precursors are internal dynamics and consequences of victimization events.

But, especially with regard to victim-offender role relationships and the interactive processes that produce a particular result within the event, we might want to ask ourselves about the extent to which these are "knowable," whether to outside observers or to the participants themselves. In our efforts to model their motives and actions, and develop explanations for their complex interrelations, are we inherently up against subjective judgments and the "Rashomon" effect? Developing better understandings and information on what occurs during the event is a very challenging area, and one where we are still particularly weak.

SECOND DAY: LUNCHEON PRESENTATION

REISS: We are now going to take all the time that we need to hear Marvin Wolfgang share with us his thoughts about how we ought to disentangle ourselves from the mess we have gotten into in the past day and a half.

WOLFGANG: The remarks that I am going to make are similar to those that I made at the Third International Symposium on Victimology in Munster, Germany, last September with some modifications. There is some redundancy, particularly because of the remarks that were made this morning, and in Al Biderman's paper, because of his emphasis on harm.

The title, "Basic Concepts in Victimological Theory," may be a bit misleading, for I intend to concentrate on only one basic concept that can have several theoretical formulations. I shall offer several propositions that might be linked into one form of victimological theory. The posture of these propositions is meant, especially in view of our conversations this morning, to be provocative, tentative, heuristic, and I present them for review and debate, rather than as values adversarily displayed or as empirical findings.

I take no stand on the things that I'm going to mention. I may even reject some of the policy implications that are involved in these conceptual explorations. My only hope is to increase the grammar of our thoughts in victimology and to suggest some linkages between theory, research and practice.

The focus is on specification of the victim of crime, and I shall refer to this concept as individualization of victimization or, if you wish, victim individualization. The principal point is that a variety of victim attributes and characteristics relative to the harm inflicted on the victim might be taken into account, not only in scientific research, but in statutory provisions, in adjudication, and in the offender sentencing process.

Crimes are defined primarily in terms of the type of act, in legal taxonomy, rather than in terms of the degree of harm for the victim, except to some extent for crimes against the person. The culpability of the offender--that is, the degree of blameworthiness--is another factor in the definition of crime. Science, more than legislation or adjudication, has considered a variety of victim attributes in research analyses, but mainly for descriptive purposes: to show rate of assaultive crime on

elderly victims, for example, or the rate of lower SES compared to higher SES victimization, and so forth.

Some implications are sometimes made about police patrolling areas and at specific times, special squads for the elderly, and so forth. Although some statutory provisions exist which recognize the specificity of some victims, legislative recognition has been minimal, perhaps because of a vague sense of democratization of victims of similar crimes, so as not to acknowledge the hierarchy of differences, perhaps out of a fear of retrogression to an earlier stage of social evolution.

The suggestion invoked here is that more crimes might be defined, and sanctions provided, on the basis of specific attributes of the victimization process.

As you know, in recent years major changes in penal philosophy have occurred. By reason of a confluence of the ethics of equity and the amassing of empirically sophisticated research on the lack of efficacy of rehabilitation, there has arisen what we call the neo-classical revival, with a focus not on utilitarianism but on retribution. The retributive just deserts model has been resurrected to the professional satisfaction and value orientation of many ethicists and social scientists.

The emphasis throughout, including the monumental revision of the United States Federal Criminal Code, the bill at any rate, has been that the severity of the sanctions should be based on the gravity of the crime. None of the attributes of the offender--his personality needs, his mental status, his prior record, his emotional state--is as important when the moment of sentencing disposition arrives. Only the seriousness of the crime should be considered, say some.

But what, indeed, does seriousness of the crime involve? The intent of the offender, the amount of harm or property value loss inflicted upon the victim? These are the traditionally accepted criteria for seriousness. These ingredients have been included in our national study that you all know about.

I suggest that, although the absence of attributes about the offender is now considered appropriate in the sanctioning system, that the presence of victim attributes might be considered acceptable both from an empirical behavioral science and from an ethical perspective. The de-individualization of the offender may be replaced by the individualization of the victim. For the degree of harm, and hence the seriousness of the crime is partly a function of the differential response by the victim.

That there is differential victim response to similar criminal behavior is not too difficult to demonstrate. The extent to which such differentiation should be recognized in criminal statutes and in adjudication remains more of a problematic issue.

There have been more than a few criminal statutes that have recognized this victim specification, that define a class of victims in such a way as to make an actus reus especially delineated. We all know the Hammurabi Code as one of the earliest examples. The Hammurabi Code, we should remember, was not always the strict proportionality often attributed to it--that is, approximating the punishment to the crime--for the victim also made a difference.

Professor Pritchard, who is a scholar in this area, reminds us that if a noble destroyed the eye of another noble, his eye shall be destroyed. If he has broken the bone of another noble, and I am quoting, "they shall break his bone." And if he has knocked out the teeth of a noble "of his own rank, they shall knock out his teeth."

But if the victim is not a noble, the punishment is a fine, as in the case of a commoner striking the cheek of a commoner. If a noble strikes the cheek of a noble of higher rank, he receives 60 lashes with an oxtail whip. Striking a noble of equal rank resulted in a fine. But if a slave struck a noble, off came his ear. If a son struck his father, they cut off his hand.

The history of slave cultures reveals similar rubrics, in which there is victim specification. My colleague, Federal Court Judge Leon Higginbotham, has clearly shown these in his marvelous new collection of American colonial experiences in his book called In the Matter of Color. Christians and masters were viewed as special categories of victims. "If any Negro"--and I'm quoting--"If any Negro lift up his hand against any Christian, he shall receive 30 lashes, and if he absent himself or lie out from his master's service and resist lawful apprehension, he may be killed and this law shall be published every six months."

But a slave could also be a special kind of victim, such as to modify the definition of an act if committed by a master. "Be it enacted"--said the statute in colonial Virginia--"and declared by this grand assembly, if any slave resist his master and by the extremity of the correction should chance to die, that his death shall not be accompted felony, but the master (or

that other person appointed by the master to punish him) be acquit from molestation, since it cannot be presumed that propensed malice (which alone makes murder felony) should induce any man to destroy his own estate."

South Carolina and Georgia had explicit slave victim statutes. Under the 1812 Act in South Carolina a slave "could"--and I quote--"be whipped for the first time that he struck or 'offered any violence to a white or Christian, branded or whipped and mutilated (his nose could be slit) for the second offense, and killed upon a third offense."

The act specifically stated that when the slave was branded it would be "in some part of his face with a hot iron, that the mark thereof may remain." In Georgia--and this is the last of these I'll pull out for illustration--"Death could likewise be imposed if a slave grieviously wounded, maimed or bruised any white person; was convicted for the first time of striking a white person."

Thomas Jefferson proposed proportionality, you may remember, in his comments on these kinds of issues. Jefferson knew about Beccarisa's "Dei delitti edella pena," as did Ben Franklin, and Jefferson reflected some of that in a bizarre way in his first inaugural address, in which he proposed "equal and exact justice for all men." In 1779 Jefferson drafted "A Bill for Proportioning Crimes and Punishments" and recognized victim-offender differences. For example, "Whosoever shall be guilty of rape, polygamy or sodomy with man or woman, shall be punished, if a man, by castration, if a woman by cutting through the cartilage of her nose a hole of one-half-inch in diameter at the least."

There is no evidence on how Jefferson arrived at that kind of rational decision.

He also wrote: "Whosoever on purpose, and of malice aforethought, shall maim another, or shall disfigure him, by cutting out or disabling the tongue, slitting or cutting of a nose, or ear, branding, or otherwise shall be maimed or disfigured in like sort; or if that cannot be, for want of the same part, then as nearly as may be, in some other part of at least equal value and estimation, in the opinion of the jury, and moreover shall forfeit one-half of his lands and goods to the sufferer."

Although some of what Jefferson says here may sound bizarre, he does refer to equivalences, proportionality and victim differentials.

The relationship between the criminal offender and his victim is given some modest recognition in current criminal statutes. Corrupting the morals of a minor is a crime which focuses on a class of victims, that is, those under a given age.

By the way, as a research agenda I would think a thorough perusal of criminal statutory law in the United States, if not around the world, for items of this sort in which there is victim specification would be an interesting one.

To corrupt the morals of a 30-year-old person is no crime. Presumably, a minor is less compis mentis and therefore is in a posture of greater vulnerability. So is the mentally retarded, the unconscious or even the dead victim. Stealing eight pieces of gold from a dead man's chest is nonetheless robbery, despite the lack of conscious confrontation and intimidation between victim and offender.

In more than a few jurisdictions, rape is still defined in terms of females sexually assaulted by males. Newer statutes, as you know, change this sexual distinction in order to give recognition to intrasexual assaults. Still, our sexual statutory history continues to refer to rape as "penetration, however slight," although the original legislative intent may have been to focus on the extent to which penetration refers to the victim's being penetrated.

Moreover, rape is an especially vivid reminder and most fascinating crime to examine from a legal, criminlogical and victimological perspective, perhaps more than any other specific crime. There may be major age differentials that recognize the victim. Statutory rape usually means a male 16 or older and a female under 16, despite consent. Many new statutes refer to other age differntials that define the seriousness of rape by the age distance between the victim and the offender.

The presence and amount of vaginal violence has been researched in rape studies. The litigation process may use vaginal violence as evidence to demonstrate forcible sexual assault, but criminal statutes do not. Perhaps they should, as an element in the definition of the degree of seriousness of the crime of rape.

Some states which have abolished the death penalty in general to all intents and purposes retain capital punishment for the killing of a police officer of a prison guard. I'm not challenging or defending this kind of victim specification. I

simply use it to draw your attention to the fact for analytic purposes, and to note that there is victim individualization occasionally acknowledged in these statutes.

Whenever there is a legal duty in which the relationship to others is specified, there is victim individualization. An employer is responsible in many ways for his employees. Parents and guardians are responsible for their children. A railroad employee is responsible for removing a child from a railroad track--classic law school case--before a moving train, but a non-employee is not.

Crimes of omission, crimes of negligence, crime of abuse are involved in these circumstances in which there is a victim class, a legal victim. Failure to provide becomes a crime.

Moreover, the victim may even be an animal. Cruelty to animals is surely victim specification. A bank is a specific victim target, different from a store. The victims are the collective contributors to the coffers, and hence a bank robbery is considered more serious than many other types of robberies. Larceny has been distinguished as petty and grand as you know, based on the amount of money stolen. For many years, in many statutes, burglary was defined only as entering a residential household's second story at night, such was that degree of victim individualization.

Specific victim characteristics are recognized in provisions against discrimination in public education, public accommodations, employment, housing, and interstate travel. Paragraph 1512 of the draft of the Federal Criminal Code states: "A person is guilty of a Class A misdemeanor if, whether or not acting under cover of law, he intentionally injures or intimidates another because of his race, color, religion or national origin."

Again with respect to rape, there are sex, mental status and age variables that are noted in the reform of the Federal Criminal Code: "A man who has sexual intercourse with a female not his wife is guilty of rape if he uses force, administers intoxicants, or the victim is less than ten years old."

Further victim specification is most commonly focused on the sexual offenses. The convergence of several victim traits may be noted in paragraph 1646, it's called "Sexual Abuse of Wards," in that same draft. It says: "A male who has sexual intercourse with a female not his wife or any person who engages

in deviant sexual intercourse with another or causes another to engage in deviant sexual intercourse is guilty of a Class A misdemeanor if:

"(a) The other person is in official custody or detained in a hospital, prison or other institution and the actor has supervisory or disciplinary authority over the other person; or

"(b) The other person is less than 21 years old and the actor is his or her parent, guardian, or otherwise responsible for general supervision of the other person's welfare."

Finally, in this vein, it may seem a slight twist of the terms "offender" and "victim" to suggest that a convicted offender may be viewed as a particularized victim, as designated by statute. What I am referring to is a fact that Thorsten Sellin brings to our attention in his book on Slavery and the Penal System. He reminds us that if not de facto, still de jure, the United States peculiarly still has slavery. It's in the Thirteenth Amendment to the Constitution, 1865. It says: "Neither slavery nor involuntary servitude, except as a punishment for crime whereof the party shall have been duly convicted, shall exist within the United States, or any place subject to their jurisdiction."

Let us consider the variables of differential age of the victim and of differential sex and time, for these are all related. We know from the victim surveys about the elderly that they are not victimized in significantly higher rates than are younger persons. Yet we know too that there is an inchoate and poorly articulated sense of greater harm inflicted on the elderly when they are mugged, beaten, burglarized or robbed.

The New York State Legislature debated recently, but I do not recall what the outcome was, whether a special statute should be enacted to make it a special crime to victimize an elderly person.

We may ask: Should an assaultive attack on an elderly person be viewed as more serious than an attack on a 16-year-old or a 30-year-old person? Moreover, should an assault by a male on a female, because of differential physical vulnerabilities, be considered more serious than a male-male attack? These questions relate not only to victim differentials, but to victim-offender differentials, which complicate the issues considerably.

Should the rape of a 12-year-old virgin be considered more serious than the rape of an 85-year-old grandmother? Homicide may be a special case and more difficult to delineate, for the ultimate blow may not be subject to the logic of variability. Yet again, the civil law and insurance companies recognize this age variable based on life expectancy, actuarial tables and income-producing years.

The death of a productive 24-year-old male is a more serious loss than the death of an 85-year-old retired person so far as the future-oriented perspective is concerned. On the other hand, the older victim may be viewed as a major societal investment who has previously contributed his productivity and is, therefore, by reason of his congealed labor representation, a more valuable commodity than the younger, unproved person.

There are also temporal variations in victimization that could be taken into account. A person--and there is allusion to this in Al Biderman's paper--who has been assaulted in a half-minute episode has endured, all other things being equal, a relatively short time span of being a victim. Held hostage for hours or days, kidnapped for weeks, raped over a six-hour period--these are significantly different episodes that could be considered as sustained victimization requiring a statutorial variation by degree of the gravity of the crime. A victim of a quick, non-assultive purse-snatching by stealth is surely in a different victim posture from a long-enduring confrontation that ends in a similar amount of theft. Here more than the vis a vis confrontation is involved; the duration of duress is a major ingredient in the victimization.

The victim of extortion, of blackmail, of political corruption, may be a victim of long duration that has not been adequately recognized in victimological research, statutory law, or compensation procedures.

Besides age, sex and time, there are major differential responses of victimization by degrees of physical injury. There is significant empirical research that could readily provide the basis for legislative and judicial recognition of this variable. In the effort to scale the seriousness of crime, my colleagues and I have used a relatively crude but practical classification of physical harm, as you all know: death, hospitalization, treated and discharged by a physician, and minor.

We have repeated this four category system in our national survey. These relatively gross differences are, however,

clearly discernible, are regularly reported on police offense forms, and are hence useful for making more refined statements about the measurement of the gravity of crime in a given population. I won't burden you with what those particular mean scores are, but they follow a logical sequence in order and are rank ordered from death down to minor harm.

Now, tort law recognizes these differences and informs us more than the criminal law, as Dick Sparks was, I think, alluding to this morning, about physical harm differentials among victims. Employer or automobile negligence resulting in loss of an eye, limb, hearing, etcetera, is given special compensation. Insurance companies are accustomed to these variations and there is an elaborate history and literature on the subject.

But criminal law has regressed since the Hammurabi Code by compressing degrees of harm into relatively few categories, that do recognize death, simple assault and battery, and aggravated assault and battery. The amorphous, ambiguous category of aggravated assault and battery has been detailed[5] elsewhere.

We have shown, and I mentioned yesterday, that 28 percent of the officially recorded, police recorded, simple assault and battery actually contain all the definitional ingredients of aggravated assault when measured in terms of the harm to the victim.

These gross criminal code differentials fail to focus on the victim and instead reflect more the intent of the criminal offender. But a system of criminal justice and criminal statistics that focuses on the victim can provide less ambiguous, more objective, quantitatively measurable variables.

From medicine, a carefully designed injury severity scale has been developed by a Joint Committee of the American Medical Association, the Society of Automotive Engineers, and the American Association for Automotive Medicine. In a quantitative analysis of 2128 persons whose motor vehicle injuries resulted in hospitalization or caused death, the authors, Baker, O'Neill, Hadden and Long, describe what they call the Abbreviated Injury Scale. It was developed to provide a method for rating and comparing injuries incurred in automotive crashes.

One of the purposes of that Baltimore hospital study was to develop the Abbreviated Injury Scale to describe multiple injured patients and--and I'm quoting--"to take differences in severity of injury into account when comparing the morbidity and

mortality of various groups for purposes of evaluating their emergency and subsequent care."

Scale score values are assigned to varying degrees of injury severity for all parts of the body. The quantification terminology that is used to describe the coding is the consensus of "some 50 physicians, engineers, and researchers concerned with crash investigation and human injury tolerance" to develop a single injury scale to serve the needs of all the disciplines involved.

The Injury Severity Scale developed by these authors is then based on what they call the Abbreviated Injury Scale. Each injury can be classified by body area--that is, for example, head, neck, face, chest, abdominal or pelvic contents, extremities, pelvic girdle, and general--and by the severity. We assign numbers one for minor, two for moderate, three for severe but not life-threatening, four for severe, life threatening, survival probability, five for critical, survival uncertain, six for maximum, currently untreatable; and nine for an unknown.

The Injury Severity Scale is the sum of the squares of the highest Abbreviated Injury Scale Code in each of the three most severely injured areas. For example, a person with a laceration of the aorta--his chest equals five--multiple closed long-bone fractures--this is extremities, assigned a four--and retroperitoneal hemorrhage--this is the abdomen, given a three--would have a total Injury Severity Score of 50. It is simply the sum of the squares of each of those items. For the abdominal area alone, there are 29 specific organs, with a total of 62 degrees of severity of injury.

Now, these injury scores have been well tested for reliability and validity, and are thereby supported in several countries. Differential responses to similar injuries can be recorded and are recorded according to sex and age, and the probability of recovery can be determined by total score values. Like a crime severity score, the same scores for different parts of the body are considered equivalent.

Now, this is a very brief description of the injury scale. It certainly doesn't do justice to it because it has many elaborate details, a great complexity of construction, and there is a growing research literature on the topic of which I have just recently become aware.

My reason for introducing the topic certainly should be clear to you, namely, that it should now be feasible to use an Injury Severity Scale to improve criminal statistics and to provide greater specification, or individualization, to the harm done to the victims of criminal acts.

It might be asserted that the amount of money stolen from the victim's pocketbook is very much a chance factor, and that, similarly, the degree of physical injury to a victim may be partly a function of the age and thereby vulnerability of the victim, as well as chance factors. Nevertheless, it's logically consistent with a penal philosophy that claims that the severity of the penalty should be commensurate to the gravity of the crime to claim, further, that different degrees of bodily injury to the victim should be included in the meaning of gravity.

I have a word or two about the emotional trauma and economic loss that again is somewhat redundant to the things particularly in Al Biderman's paper. There are also differential psychological effects on victims as a function of age, sex, and other physical, physiological and cultural factors. Reactions to rape and other sexual assaults probably are among the strongest and the most lasting traumas. But there are many fears, phobias, and other deleterious, destructive and damaging effects on the conscious and subconscious lives of victims of other kinds of criminal acts.

Perhaps so varied in degree and complex in content are these psychological harms that no formal rules can be established to satisfy the principles of equity. So difficult would be the attempts to assess psychic damage that this kind of victim individualization might best be put in the same posture as the current movement to abolish the insanity defense.

Moreover, emotional, mental damage may have short-lived or long-term effects, or may only become evident long after the act of victimization. The opportunities for feigned responses are also more present in these kinds of effects. Events in which a functional disorder as a response to being victimized is linked to a more measurable physical variable could be more readily acceptable, such as full or partial paralysis, or the assumption that might be made about all child victims of forcible rape. Perhaps only the most glaring and obvious psychological effects, for which there would be complete inter-observer reliability, could be taken into account by statutory or case law.

Despite the difficulties of assessing psychological harm for purposes of litigation or compensation, the phenomenon exists, and the research community could do more than it has in

attempts to classify, describe and analyze these types of effects on victims of crime.

Now, I have purposefully concentrated on victims of physically assaultive behavior and prefer to leave to the economists and econometricians a more full assessment of property loss than the mere mention of such loss as part of a focus on victim specification. And there are many obvious complications that arise.

As we have often said, the theft of $1,000 from the meager savings of an elderly couple is more grave for them than the same amount stolen from a millionaire. A $1,000 theft from a bank can be said to have miniscule effect because of the diffusive victimization spread throughout all depositors.

From the viewpoint of victimological research, we know relatively little about these kinds of victim differentials. The greater the spread of economic inequality in a given society, the greater can be the corresponding degree of economic loss, holding monetary value constant, to property victims of that society.

Property, in one sense, represents congealed labor. Morton Bard described burglary yesterday. And although money or other property may have a given market value, the ratio between that value and the pool from which it is subtracted by victimizing the owner of the pool is an item for a future research agenda.

Perhpas, like psychological harm, economic victim individualization is more an academic than a substantive law phenomenon. Yet, if victim differentials are a part of the definitions of crime, then the ratios of economic disutilities cannot be entirely ignored.

Finally, the victim-offender relationship--even though I have mentioned it before, some attention might be given to this, more than we have, in assessing the degrees of harm. That is, to the descriptive research literature that has examined degrees of affinity or distance between victim and offender, we might add the dependent variable of harm inflicted.

There may be many situations for which the victim-offender relationship is irrelevant in its effect on the dependent variable. In others, the relationship may be critical. In some societies patricide has been viewed as much more serious than other kinds of criminal homicide. Is an assault by a friend worse, equal to, or less serious than an assault by a stranger?

We do know that some particular role relationships between actor and those affected by the act affect the definition of the act as a crime and the degree of perceived harm. For example, as a private citizen with no public official responsibility, I may accept $10,000 to convince a legislative body to pass a bill favorable to a given party, business or industry. I have committed no crime and no harm. But as a legislator, when I accept $10,000 to vote for a bill favorable to the donor, I have violated a position of public trust.

The act now, because of the role and relationship to the body politic, is defined as a crime. I have harmed public morality, I have victimized the political institution, the entire community involved in the social contract.

In the national survey of perceived seriousness of crime, our respondents have given high mean scores to such acts, scale scores as serious as ordinary burglaries and street robberies. The degree of harm here is a function of the relationship between actor and those acted upon. There are many other circumstances for which the victim-offender relationship may have differential effects.

One could then conclude with a kind of multiple victim variablility and try to determine, measure and weigh the relative contributions of each one of these variables that I have mentioned in victim individualization: age, sex, duration, physical injury, psychic harm, economic loss, and victim-offender relationship, etc.

My major thrust in this perspective can be very simply stated and related to current penal philosophy about just deserts and retribution. If the criminal offender is to be punished primarily on the basis of the gravity of the crime, if individualized punishment is to be muted or diminished, the seriousness of the crime can be defined not only in terms of the culpability of the offender but--and perhaps principally--by the degree of harm inflicted on specified victims.

This victim individualization not only does not violate the model of just deserts, it may indeed enhance it or enrich it by providing greater precision in the proportionality of the severity of the sanctions to the gravity of victimization. If varying the attributes of the victim and the consequences to the victim alters the degree of harm, corresponding variations in the penalty can be justifiably argued, so long as degrees of harm define seriousness and seriousness commands the sanction. Thank you very much.

SECOND DAY: AFTERNOON SESSION

REISS: The question is which of these seven, eight, nine, or 10 research topics do we want to start with? I sense that some people would like very much to talk about the area relating to precautionary behavior and the actions people take either individually or collectively to deal with victimization.

SKOGAN: I will start off with a different one. I nominate an area which could be called "consequences of crime," a subject which seems to me would involve both careful thought and investigation into the measurement of the nature and extent of harms and would also involve research on the psychological-attitudinal consequences of victimization. It could encompass the behavioral adaptations that victims and communities make to crime as well.

This topic seems to have both substantive and important methodological implications, and it would involve focusing on measures of harms which would be central to a whole variety of different topics as we have listed them here.

BIDERMAN: I think one of the important things to get at is in what ways are losses that are measured on various scales--for instance, the scale that the highway safety folks are using, or those an economist would use for losses of property--equivalent for losses from crime and from other classes or different sources. How is the reaction to these losses by victims different and what differences are there in the sense in which they are harms regarded as important enough for the law to speak strongly about?

Now, it is my hypothesis that it is indeed very different to lose $10 by having your pocket picked than to spend $10 more because you were misled by an ad for an item and consequently lost it that way.

So, it is these consequences of harm that are not measurable in the way we are wont to measure them that I think must be the subject of research.

WOLFGANG: What's the notation for this research?

BIDERMAN: The crime-distinctive significance of losses and harms.

REISS: I also thought included in that was "and the reactions people have to different ways of calculating those."

BIDERMAN: There are a number of agendas in this. One of the hidden agendas is that people react to events as social animals, and

they recognize that what the meaning of the act is that it is an offense against rules on which they are dependent for many other things. The reaction can concern the meaning for their own regular environment at the micro social level, the significance of what has been offended in their person, or it can be macro social in terms of what is going on in society that this should happen to me and that my feeling of well-being as a citizen has been offended against.

I think that that important element of harm is lost, and it frequently predominates the reaction because there is no major material loss. There are all of these attempts in which there is no loss, there is no physical danger; all that is threatened is the feeling of social security.

SKOGAN: So this falls among the psychological consequences?

BIDERMAN: It's the psychic counterpart of an attributed social consequence.

REISS: I think it's an area that research needs to be done. I tell my neighbor someone tried to break into my garage. There is the consequence for me; there is a consequence for my neighbor. And my neighbor may feel offended by the fact that this was very close: "It might have been me." And in the whole neighborhood the word passes very quickly as to who was victimized, and it has consequences for the way everybody behaves, at least for a while.

So, people react to it not as "your victimization," but it is a collective threat: "There has been someone in our neighborhood who has been hit and there, but for the grace of God, go I."

WOLFGANG: I was thinking, for example, if your fingers are injured in some kind of a robbery, you can measure the degree of injury. It wouldn't be the same for everybody. If I am a concert pianist, I wouldn't react in the same way.

REISS: There is also the other end, Marvin. The Sellin-Wolfgang Scale has been around a long time. If you apply that to the NCS victimizations, you find most victimizations are in your lowest class score. That doesn't mean if you went and asked people and said, "Now, there are two people out there--Sellin and Wolfgang--they say this gets a score one, and it's defined in the following way. How do you feel about that?" Some might say you're out of your mind.

BIDERMAN: Marvin Wolfgang's illustration puts me in mind of a good example to illustrate crime distinctiveness. There was a woman who was pushed in front of the subway train, or was allegedly pushed, and who was a pianist. Now, I think there was great difference in the significance of that event for her and, certainly, the social reactions to that event, because of the fact that she was pushed into the path of that train rather than fell.

So, if she fell, the significance for her and for everybody else is different than if she were pushed. I think it is that element or those elements we have to understand. It's not a unitary thing; there are many dimensions to it. That's what we have to understand if we want to understand criminal victimization's significance and its relations to what we're doing.

REISS: Wes Skogan said he had an RFP he was going to propose.

SKOGAN: I am not quite sure of the extent to which we are drafting it, but it would involve a conceptual elaboration of physical, economic, and psychological consequences of victimization. It would involve work on the development of indicators, and probably would be embedded within some kind of generally defined illustrative research that should be conducted, using the concepts and methods that were developed.

REISS: Al Biderman keeps pushing societal consequences and I would add it might be enhanced if people could at the same time look not simply at individuals as victims, but organizations and groups as victims.

As a matter of fact, you might learn more by looking at organizations as victims at this point than looking at individuals as victims, strange as that might sound. I mean, starting backward and looking at individuals victimized in organizations. Examining victimized schools, for example, and kids victimized in schools might tell you one heck of a lot.

Looking at the organization: How it is victimized? What are the consequences when kids are victimized in school? For the school? What are the consequences for the kids? What's the consequence if the school is vandalized and you can't have classes? We had whole schools put out of commission for months in a place like West Haven, Connecticut, because of vandalism on the part of kids.

When organizations are victimized, there are consequences for individuals; when individuals are victimized, there are

consequences for organizations. And pick one where it intersects and study these intersections. You might learn a great deal.

BLOCK: Can I put in a disclaimer for the whole thing? It seems to me that, in proposing to study this, you are studying outcomes and you are not finding out why those outcomes occurred.

I think this is really not what should be proposed at all. I think what we have to do is to look at how those outcomes came about.

REISS: You can't know about how outcomes came about without knowing outcomes, as far as I can tell.

BLOCK: If we concentrate research on outcomes we will never know any more than a social accounting of what those outcomes are.

REISS: I don't believe that at all. I would not know very much about what causes black lung disease unless I had the outcome of black lung disease. It's a delayed effect. I could go down the line with all the delayed-effect harms. You don't begin to think about the causes of harm until you have seen the consequences.

BLOCK: I would start with the failure of crime prevention, and study in detail particular victimization events--what happened in those victimization events, and then follow them throughout, rather than do an in-depth study of outcomes.

REISS: Are we prepared to leave the consequences of crime area and tackle another area?

SPARKS: I think to put the notion of the causes of crime and victimization in somewhat more concrete perspective, a good deal of what we have talked about would seem to require research that uses information or that seeks information about victims from offenders.

For example, how do offenders pick targets? What do they perceive as attractive choices, or ones that are difficult or ones that they have some opportunity?

WOLFGANG: Or were they planned, or was it impulsive?

SPARKS: Certainly. That's going to get into complications when we get to some of the personal crimes, where we need some type of assumptions about rationality among all parties, but there is a

wide range of crimes in which the selection of targets is more than problematic.

REISS: If we're going to do this, I think people should be cautioned that if you take account of the age of the offender, that it's very hard to apply an individual decision-making model to young offenders. It's almost always a collective, and indeed, it's made up of different sub-parts of a larger collective very often. Individual choice models don't seem to make sense in that context where we know very little about group decisions.

The point is getting information from offenders, because you need a model of different kinds of offender selection. Most of the models I know of are individual decision-making models, and I'm saying the research ought to force people to think about other than individual decision-making models.

GAROFALO: Since we're talking about looking at the offender, it might be an opportune place to add the question of the intersection between offender and victimization as they exist within the same person. We need to look at not only, as Dr. Singer's paper showed, where there may be some subcultural factors involved, but also where the victimization or perceived victimization becomes some kind of motivating factor for future offending.

REISS: You need to make an important decision in this research, as to what kinds of crimes you want to consider. When you enter the so-called "white-collar" crime area, offender decision-making will look very, very different than certain ordinary crimes.

And I suspect if one thought of political crimes, decision-making models would look different for those.

I'm talking about things like mail fraud, which may not be white-collar crime, depending on how you define it, but those kinds of crimes are rather different in terms of selection of victims than a personal crime.

If I do a national mailing, I just know that there are fish out there that I can defraud.

I am just adding that as a kind of constraint. People could go in very different research directions on this, depending on what kinds of crimes they selected.

Anything else about the offender selection of targets?

BLOCK: Yesterday we talked about the intersection of victim and offender, and discussed the victim and offender related to the same crime.

I think that getting the victim and offender view of the same event is an important part of this.

REISS: That's again a problem; most people think of only a victim and offender event. There are a substantial number of events where there were witnesses, and so we need to have different accounts, and in some cases even the police are present when the event takes place. Those different accounts need to be looked at.

BIDERMAN: I would like to be the perennial moralist in the group. An interesting thing about offender-victim relationships that relates to the atrociousness of the offense, is when the harm to the victim is totally disproportionate to the gain to the offender, where it is presumably a gain-oriented offense.

BARD: This is described in the press as senseless crime.

BIDERMAN: The offense that I see most often, that has very appreciable consequence in terms of Marvin Wolfgang's criteria, for example, is the parking offense, double-parking, particularly, but also illegal parking during rush hours.

If you take the average value of the time to the motorists, and how much lost time a double-parked car or an illegally parked car in the rush-hour lane mounts up to, my rough calculations come to something like a minimum of five or six hundred dollars for the cost of a car just stopped illegally for five minutes in rush-hour on L Street in Washington, D.C.

The law does not regard that proportionally because there are other elements going into it, including what's going on in the offender's mind, in terms of the proportion of gain in terms of the loss suffered.

So if we want to develop things which will intensify social control, we have to look at the way ethical-moral principles that guide social control, operate or fail to operate on what kinds of people and why.

REISS: I think that's an interesting topic; it's a very complicated one in terms of criteria. The one that has bugged me is police

officers being killed as compared to police officers killing, which you know is a very interesting kind of question.

From one standpoint it's much more rational for an offender to kill a police officer, because what is at stake may be a lifetime in prison, so it's a very rational act, if that's all that stands between you and a lifetime in prison.

Why should a police officer kill any citizen? What's his gain? It's a complicated problem.

What should clearly be researched, is the way in which the social definitions of harm and their consequences are derived, and particularly where there's a disproportion between the gain and the harm inflicted.

SKOGAN: What would an RFP look like for this requisition?

WOLFGANG: The title would be something like "Disparity between offender gain and victim loss."

REISS: The general area is how offenders select targets and their consequences. Included under this area are: kinds of selection models; group decision models; repeat victimization; the Rashomon problem; and the disproportion of harm to gain.

SCHNEIDER: I would like to bring a little more applied value to this area. There have been a few studies where offenders are questioned in particular about precautionary measures victims or non-victims might take.

If we were having a general research category dealing with offenders in the field, I think that is a useful approach to honing in on the whole question of how they view the defensibility of potential victims.

REISS: That's an interesting area. There's a small amount of work being done in asking offenders when they didn't commit crimes, when they started to commit crimes, when they stopped and why did they stop.

SCHNEIDER: Additionally, why they chose one target as opposed to another. Which ones did they screen out and for what kinds of reasons?

REISS: There's a little on that, but it's mostly for burglary cases. It's a fascinating topic and has a lot of implications for intervention programs.

Another area is something that might be called the "situated nature" of victimization. What kinds of studies do people think they would like to propose in that?

SKOGAN: How is that different from intersection of victim and offender?

REISS: Well, I think it's more than intersection, if you look at the pattern studies. It's something more than the intersection.

We can talk about precautions, how people avoid harms, as a topic that is not related to the consequences of harms, but a separate topic.

BIDERMAN: Well, I'll relate it to the consequences of harm, because I did a pilot study under a grant from the Bureau of Justice Statistics for pursuing the current consequences screening approach. I think that was an excellent approach for getting to what victims attributed their harm.

This approach asked what they did, what other people did, what nature somehow or other did, that caused what shouldn't have happened.

SCHNEIDER: Under precautionary behavior, I know there are a lot of topics we could add, but I think it would be useful and interesting, as well, to follow up on some of the ideas in Jim Garofalo's paper on risk assessment and harm assessment by victims. How do they process that kind of information, how distorted is it, and how do we come up with those kinds of estimates?

The second thing I think would be important and potentially useful is the extent of distribution of protection, protective devices and behaviors amongst the population, and the assessment of those in terms of what kinds of benefits and costs they impose on other people. This would try to get the question of what kind of behaviors or devices should public agencies be encouraging or discouraging, which ones might they be providing free?

In other words, if we can't reduce crime, then maybe we ought to help those who are least protected from it.

REISS: People have locks on their cars, and they don't lock their cars. It's interesting that we could have locks on our wheels, but we don't put them on cars.

WOLFGANG: Just a little aside because I have no theoretical conceptualization for it. When we are interviewing offenders and asking them about the selection of targets, I would like to combine that with the avoidance strategies approach and ask these offenders how they avoid being victimized, particularly very streetwise offenders.

REISS: That's not inconsequential for certain kinds of offenders. As a matter of fact, many don't avoid it. Drug sellers report high victimization. It's a very risky business all the way around. If you're working for the syndicate, it may be very high risk.

BLOCK: We might also consider at the same time social kinds of crime prevention, as well. It seem to me that victimization surveys, given that they are surveys, have consequences in that you have an in-person interviewer that can be used for other kinds of observation.

REISS: I think that's a very good idea. We go into different areas and neighborhoods and interviewers can tell us a lot more about it.

I think there is much more coercive kind of crime out there against businesses and organizations and persons. Juice rackets, and all of those things that go on in communities in the United States, where it doesn't affect us because we are not victimized by that sort of thing. But there are a lot of people that are affected, and it's a very difficult thing to measure. I think we ought to be looking very closely at that: Who is paying off to do business? Who is being subjected to the juice racket?

That kind of victimization we don't ordinarily think of because we don't know how to quite study organized crime.

That ought to be a challenge, because it's one of the serious victimization problems in this society.

Take contracting and the whole way it corrupts. The consequences are enormous in the society. Paying off your businessman is a way of life in much of New York, yet we don't know how much they feel victimized, but most businessmen tell you they feel victimized.

If you interview business people in the period that followed the definition of "payoffs" to foreign governments as illegal, they'll say they're glad to be free of it. Most of

corporate America will say they're glad to be free of it; they felt victimized by that kind of practice. I am not sure that's true, because I think there is a lot of strain and tension out there.

BIDERMAN: I think one of the most important things to do is to study what we are doing wrong. It is my strong feeling that most of what goes on in crime prevention, in community crime prevention, is not helpful, it's harmful.

There are a number of different issues, some different because there are different consequences at the individual level and at the societal and community levels.

The kinds of things I have in mind are the extent to which precaution, in fact, may provoke the offense.

If it is clear in the behavior of a kid at school that his defense against being attacked is his personal ability to repel attack and that is the terms on which he negotiates with his environment, then someone who can successfully beat him up is perfectly free to do so. If his defense is a moral norm--that you don't beat up people like me--then the potential offender has to be willing to challenge and to overcome whatever that defense is.

I see this regularly in precautionary measures. The precautionary measures are extremely vulnerable, and at the same time they sort of lead one to think about beating them, getting around them. From the standpoint of the psychic security, protective devices are continual reminders to citizens and to offenders that crime is an inherent and endemic part of the community. Do those measures provoke greater insecurity? Do they provoke greater crime?

WOODSON: If we are talking about new knowledge, an area that I have mentioned that we pay little attention to is the reduction of victimization over the long run. We must do something about the offender, something that has not been done through the criminal justice system.

Marvin Wolfgang and I have discussed this. The people who come into the system represent a very small minority. You can take them and hang them up by their thumbs. It still wouldn't significantly reduce the amount of victimization, even though, according to Marvin's study, a very small number of people commit these crimes. I would dare say the number is larger.

You have groups of people throughout the country who have in their uninformed way addressed this problem, but their knowledge goes unexamined, unidentified, unassessed.

The issue is: How can we apply the technology that exists on one end of the continuum to some of the emerging new problem-solving approaches? Some people have demonstrated that they can take hardened juvenile offenders and over a 10-year period influence their behavior so there is lasting impact on these youngsters, some of whom have outstanding homicides but who have never been caught.

It would make sense to demonstrate that these things work. It seems to me that prosecutors and judges that I talk with want this kind of information. Perhaps public policy could better support such institutions at the local level instead of directing money to institutions that are now receiving it.

It seems to me that research has to begin to connect the priorities of the policymakers and the priorities of practitioners who are desperately seeking alternative approaches and options. I don't think the legislatures that are passing laws to incarcerate kids are doing so out of malice. They don't have any other options, and the research community is not proposing alternative options.

REISS: Bob, I can see two kinds of studies that might be done that are related to what you are talking about, without being what one might call "strict evaluation" studies.

I don't know anywhere where we have, in a sense, a map of a community in the United States and what are all of the ways it is organized to cope with preventing crime or taking precautionary measures. We just don't know that for an entire community. We know about formal actions of official agencies, but we don't know what informal actions are.

The other study is related to the whole question of innovation and its diffusion. We don't have a very good idea about how you find innovative things; the less formal they are the harder they are to find.

WOLFGANG: I think what Bob Woodson is saying and what you are suggesting is a kind of Lund's Middletown study, which was focused on crime prevention.

REISS: That's right. It's really trying to understand how did the community come to those crime prevention strategies, what is the

history of them, how do they relate to one another and do they seem to have effects.

WOODSON: In my study I tried to identify successful elements, ones that are peculiar to a community, and ones that represent a common thread. What are the elements that lend themselves to measurement, and what are the elements that do not?

REISS: That can be very, very difficult and a very expensive thing to do if one does it on any scale.

I am just thinking of it in the context of how I would propose to do it, which would even include studies of land use, which I think are terribly important in terms of what they have to say about strategies of controlling crime. I think it's very well worth doing. I am not saying you don't put the money into it. I am saying it's a question of how you break it up to do it.

I think the community crime prevention area is where we want to pay a lot of attention.

SKOGAN: Is this micro-process description of what goes on in events?

REISS: Well, in some of the cases Al Biderman talks about, it takes a long time for it to go on.

I proposed earlier looking at the organized crime problem, which is situated in its own important sense here, under continuous coercion to deliver daily payoffs, monthly payoffs, weekly payoffs.

BIDERMAN: Let's just have some general attention to the crimes of duration.

REISS: These kinds of organized ones I am talking about are a subset of those crimes of duration.

WOLFGANG: I had in mind not only the present, when it occurred, the presence of alcohol, etc.; I was thinking also of the kind of things that Henry Steadman is doing now on situational aspects of violence. This is done through interviews, talking not only to ex-prisoners and a couple of other populations, but also trying to get a better exposure of those conditions that lead toward escalation from verbal aggressive behavior to physically aggressive behavior, and also how to de-escalate that spiral into violence.

REISS: I would include quite a few possible sub-studies in here. One is the relationship of states of offenders and states of victims and its effect upon the crime. That's like alcohol or drugs, and it would include psychological states of people.

The second one I would call the study of movement, change of location in terms of victims being taken to places where crimes are committed as compared against crimes occurring where there is "jumping out of the bush," to use Al's term.

How many crimes really fit this model of being very point-in-time, of short duration. The only ones I know of that really fit that are robberies.

A final study would examine the way crime areas are situated in relation to one another or the degree to which communities, such as metropolitan areas, import and export offenders and import and export victims. It's a fascinating set of relationships when you start looking.

SKOGAN: Doesn't that fit more into target selection, though?

REISS: It's partly target selection, but it has something to do with the situational ways in which crimes occur; that is, where you are situated in space. If you have to move long distances to work by way of public transportation as compared to driving to work, it can affect your chances as victims.

WOLFGANG: We know so little about the invasion of suburbs by city offenders. They move out and then move back in, and we know so little about that.

I think it could be very useful to have a more systematic study done than we tried to do in the Violence Commission. It really was done post-Violence Commission by Lynn Curtis, taking into account the location of the crime, the residence of the victim and the residence of the offender, by crime type.

REISS: We don't have a lot of time left, and Ed Ziegenhagen has some things he wants to say about victim coping.

ZIEGENHAGEN: I was just going to remark about the idea that victimization really is not an episode in the sense that we have been talking about, it isn't space and timebound. This is another way of thinking about it and should help us devise some strategies and identify particular elements of the situation.

I don't think we have paid very much attention to how victims cope with the impact of various kinds of crimes. What do they do? Do they get their big brother to take after the person, and extract remedial harm? Do they withdraw?

What really happens? I would think that this would be worthwhile knowing. It has something to do with what we refer to as social control, which is likely to be a far more effective control system than anything we can do with the criminal justice system.

WOLFGANG: Victim retaliation, which I think is what you are referring to, is a way of cleansing. That has long historical routes in the feudal society. There was certainly more justification for a family going after the family that either raped or killed one of their clan members. There was honor to it as well as a kind of social cleansing.

Now, I suspect that in American society there is more of that going on. It may vary by social class, residential density and proximity, and knowledge about one's neighborhood. That kind of coping mechanism of victim retaliation is a cleansing episode. I think, descriptively, it would be interesting to know how much of that occurs.

REISS: I think it's interesting, again, to think of victim coping in terms of organizations, too. If you've got shoplifting, one way you cope is to pass on the cost to every consumer. How many times are we victimized, and we pass on the effects by a kind of victimizing behavior?

ZIEGENHAGEN: I was just going to add to this that you can also consider the tendency of persons to move out of an area after they have been victimized, to cut their losses.

REISS: I'm convinced that's very critical in the National Crime Survey. We don't follow those people.

ZIEGENHAGEN: So those people not only disappear for survey purposes, but it also makes prosecutors very unhappy when they can't be found.

So there is a system intersect here. There might also be instances that cement the relation of those two systems. The traditional reaction to the local policeman on the beat, bringing the offender back in the community to figure out what in the world should be done with him. This is cementing social control to state control.

So we might well want to be very sensitive to this, not just think about coping behavior as individual behavior, as such, but how it is linked to control systems.

BIDERMAN: All this coping is linked to larger domains, so it's very difficult to disentangle the consequences of victimization which are parts of larger systems. When you look at data from surveys and try to get at that, or from studies, you really don't do very well.

I mention the Annual Housing Survey, which does ask people about why they moved from where they were, and why they picked the spot where they are living now. If you look at that, crime just isn't important. It's insignificant as a determinant of residential locations. But I don't think those data are quite right.

People live where they do because of where they work, primarily, or because of where schools are, so there is a whole raft of reasons. General safety is another. A victimizing event is one element in a large constellation of values, and patterns and habits and linkages and so forth, that determine where one lives.

In this survey, the victimization has to be of an extreme kind, and of some kind that is identified so peculiarly and so specifically to be the cause of residential movement, in order to be a cause.

If people are victimized as employees, if they are a cashier and they are robbed repeatedly, does it make them change jobs, and what are the consequences of that?

That is to say this is a whole kind of coping strategy. You go get a new job because you've been victimized where your old job is, or on the way to work to it, so you keep looking for a job.

We don't know very much about that. We don't ask on the NCS whether you are out of work or quit your job because you were victimized, for example.

GROPPER: I think it would be useful to clarify whether you're intending to lump both levels, community or organization and individual long-term coping behavior, in each of these areas. Such as moving, avoiding, etc.; do you mean to include these primarily as sidelight examples of escalation and de-escalation kinds of behaviors?

Did you mean to include the interactions not only of the direct victims and offenders, but also of third parties, such as the police?

Did you mean to include things that are of moderate duration, even if they are not repeated, such as bank robberies, hostage situations, or kidnapping?

WOLFGANG: Yes.

BARD: Are you talking about the coping strategies in terms of what people do subsequent to the event? Would you accept the following: that we would be interested in the short-term and long-term psychological and social adaptations following the event, so that it's not just a question of the behavioral coping strategies implied, but also the adaptive techniques employed in a cognitive and in an affective way as well, over time, so that there are two phases--the short-term and the long-term.

ZIEGENHAGEN: Yes, I think that makes a lot of sense, because we were speaking earlier about what the victim makes of that event, what sort of assessment exists. That is going to be linked to adaptive responses.

REISS: I would like to think of these in a sense in which Al Biderman has been pressing us to think of it. Since these adaptions can be societally-conditioned, we ought not to think of a person having to be victimized to adopt such coping strategies. We are interested in looking at coping strategies first, and then to what is the relationship of actual victimization experiences to those coping strategies?

For all of us, there is some time that elapses before our first victimization, whatever it may be. For some of us that occurs earlier in life than for others. Some of us may begin to develop coping strategies because our parents tell us: "Be careful of this," so we learn all sorts of coping strategies. We want to know about those, and then see how actual victimization is related to these coping strategies.

WOLFGANG: I would like to know the extent to which the social and psychological distance between an offender and a victim has a bearing, not only on selection of targets but on the capacity of a criminal to commit the crime--any kind of crime.

I think we say these things a priori intuitively: that offenders, particularly in cases of personal crime and violence,

have a stereotypical image of victims. They do not individualize their victims; therefore, that gives them the capacity to neutralize any feelings that they may have about victimizing the little old lady, or anyone else.

I do not know what bearing that would have on prevention of crime.

REISS: A good friend of mine makes that point on behavior in extreme situations. What saved him and some others in the concentration camps from being sent to the ovens was to convince the keepers that they were human beings, individuals, and that it was that strategy that saved him from the ovens.

There is a literature on this. It's a fascinating kind of literature, when everybody is doomed, and when you have genocide, which is sort of the ultimate in victimization.

WOLFGANG: It also permits a whole society to enslave other human beings as well.

I do not know to what extent it has any bearing on preventing future crime, but we all know that the more we learn about an offender, even a murderer or a multiple killer who is on Death Row, his background, his childhood and so forth, the more we have a tendency to sympathize or empathize, with him. We are less likely to hurt those people, less likely to want to kill them.

We know that prison superintendents and wardens have regularly opposed the death penalty, because they get to know the people on Death Row.

The reason I mention this is that if we capture offenders, if somehow or other, some kind of treatment program were designed to produce an individualization of these victims, or any future victims, that might prevent his doing the same thing again.

REISS: Does anyone want to suggest we move to a new topic?

SKOGAN: I would like to suggest an integrated crime information system. This is the time to think about what data is gathered by police departments that's useful, how to get it, and to think about the very difficult problem of making regularized contacts with other kinds of institutions that feed useful information, and how that integrates with other more expensive data collection efforts, like victim surveys.

REISS: I think that's very important.

To what extent can we reconstruct information from police departments? It's interesting if you go to a police department, they've got a file on offenders, but they don't have a file on victims. No one in this country, would you believe, not even the SEC, keeps a file on victims of their crimes. They don't even keep a file of organizational offenders. It's marvelous what is kept by organizations in this country.

They have got victim information but they don't think of compiling it and using it to go out, like the Fire Department might, to send someone out to see what's wrong at that place because they have had three victimizations in the last month or something.

It's interesting if you compare a fire department with a police department in the city. A fire department is interested in victims as well as offenders; in fact, historically, more so, and so it goes out and makes proactive inspections of places for their fire prevention, but it also watches victimizations. You get a couple of fires at a place, and you're going to find out why, and what can be done about it. A police department is so hung up with tracing offenders that it never thinks about creating a victim file.

SCHENEIDER: There are some that do.

REISS: One of the things we might do is get them to construct victim files? Can we do it with police records, why or why not? What does it tell us? And can we estimate repeat victimizations and other things from police files by doing that?

SKOGAN: My guess is that a system like this would involve a sample of police departments that would receive money and assistance to become better recorders, keepers, and sorters of data. This can be used on a sample basis, as opposed to a complete enumeration of the UCR, to produce the kinds of things that seem to be useful that police departments can supply.

BLOCK: Other agencies might be equally important, social agencies as well.

SKOGAN: We have models in the Census Bureau's national program for securing vital statistics. This is a useful reporting system for births, deaths, divorces and marriages, and various kinds of diseases.

REISS: We have injury systems which are worth looking at, which Al Biderman has talked about.

BIDERMAN: You know, a Friday-Saturday night emergency room is a very important crime-victim service agency. The number of crime victims they get is legion.

Now, the people that come in with something caused by a microorganism are apt to get some counseling about taking care of that complaint, so it doesn't reoccur. Victims of crime aren't serviced that way--or they are, but less so.

That is one of a number of things that fit Wes Skogan's general topic suggestion. It's a very important source of data, and it's not integrated with our other sources of data. We ought to pay some attention to it as a data source as well to how the concept of what they're organized to do, and what they are not organized to do, affects their ability to serve both as a data source and as an action agency in this regard.

REISS: It's an interesting question. It's something I tried to look into a couple of years ago. I tried to get some students to tell me how many places in New Haven had information reported to them on crimes occurring.

We set about, first, to create a list. We came up with interesting ones, like welfare checks stolen. They say the Welfare Department gets these reports and they don't believe all of them are stolen, because they claim a lot of those are false reports of crimes. They've got a terrible problem as to how they select out the bona fide welfare checks stolen from the fake ones where you want to rip off the system.

One of the things that Gary Cook argues all the time is that you really want to get these uniquely identified as cases, and then see to what extent they occurred in one or another system of information. That is to say, how many of the welfare checks stolen are reported to the police, how many are reported to the crime victims' interviewers, and so on.

It's the degree of non-overlapping that gives you some hold on what's going on out there.

BARD: Whatever the final agenda turns out to be, in relation to these subject areas, there should be a call for something beyond survey research as the basic research strategy; the agenda should encourage the elaboration and the conceptualization of

new strategies to deal with the complexities that we are suggesting exist in this field?

SKOGAN: One of the disadvantages of trying to make a list of all the places that get data about crime, as I think Al Reiss pointed out in his paper, is that virtually any organization is a repository of such information. The principal's office of a school does a lot of work; when my coat was stolen from the LEAA building one afternoon, I called the building manager, not the police. It seems to me in one sense that the list of places that are recipients of information is almost endless.

GROPPER: Over the last day or two, several conceptual models have been offered, in the form of diagrams and discussions, and in terms of comments that attempt to elaborate or generalize beyond the specific data bases from which the particular examples originated.

To what extent does the panel feel that we are at a stage, or soon will be at a stage for some of these models, where we can attempt to integrate and refine them in parallel with each other? In other words, given the state of the art, can we try to work in parallel toward the more detailed development of some of these conceptual models before additional results from the others become available? Can we start some efforts at integration or elaboration now? Or if not yet, when?

REIS: We don't lack for models in this conference.

GROPPER: I'm trying to do an update to get them together, and this conference is part of that effort, to develop a conceptual framework to guide our research. To what extent can we do it?

REISS: I think that's obviously one topic area. Implied in this is the development of models and their testing, and trying to develop a smaller number of integrated models.

A lot of the models that we have had here, if we really sat down and asked ourselves what kind of data would we need to test them and how would we collect those data, and are they available, might look quite different.

So it would be an interesting exercise to take just the models, in this conference, and look at them from the standpoint of what kinds of information are needed to test them, and where would I get it, and how?

That's a sobering experience.

SKOGAN: One important point is the victim in the criminal justice system.

SCHNEIDER: How does the system process victims?

SKOGAN: I think that's part of it. How victims use the system is another way of thinking about it.

BLOCK: Or the other way around: How does the system use the victim?

SCHNEIDER: That's what I was thinking of.

BLOCK: I think that that is more of a reemphasis of current research. I think the data to analyze that is really being collected, or almost being collected; if we could just add something about the character of the victim. The data in the PROMIS system in Washington had something about the victim, but it could have had much more about the victim.

SKOGAN: At Northwestern we have worked on data from several cities. What we found was that most prosecutors don't enter much of the information. The data entry is a pain in the neck, and they don't need to know if the victim has this or that characteristic to go about their business of bargaining a case.

BLOCK: I would agree with that. What you are saying is that it's hard to convince people in any agency to collect data about victims because victims are largely irrelevant to the criminal process.

I would like to just push that in a slightly broader direction by saying we have ignored the whole question not only about victims in relation to the criminal justice, but victims in relation to insurance companies and insurance systems, and victims in relation to compensation systems of all kinds. The whole intersection of how one generates a series of organizational relationships as a consequence of having been victimized, and what that does to the victim, is important.

We know little bits and pieces about it. We know people don't want to go and testify, because they have got to take off from work and they don't get compensated. We know that some people are dissatisfied with their insurance settlements,

That is to say, there's a lot of settlement or adjudicative systems out there and it seems to me that the National Institute

ought to be particularly interested in them, because the criminal justice system, as we said before, is only a small part of that.

We have come to the end of the conference.

I first of all want to thank all of us as participants in it, but I think we owe some special thanks here. We want to particularly recognize the important role that some people from the National Institute played, Dick Barnes, Jan Kirby, Fred Heinzelmann, and Bud Gropper.

We also want to recognize particularly the persons from MITRE who played a significant role in this--Joe Sasfy, who was responsible for organizing and coordinating the conference, Judy Dahmann who worked with him, and then above all, Addie Normandy who made our lives pleasant and significant in various ways.

MITRE Department
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