DECISION MAKING BY CRIME VICTIMS: A MULTIMETHOD APPROACH

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Since most crimes investigated by police result from notification by victims, crime victims can be viewed as the gatekeepers of the criminal justice system. This paper describes the results of a series of studies which employed multiple methodologies to investigate the decisions of property crime victims to notify the police. The four methods used in this research included (a) an archival analysis of police records, (b) interviews with crime victims, (c) simulation studies using college students, and (d) experiments in a field laboratory. Two convergent findings emerged from the data. Social influence was found to be an important determinant of both the decision to call the police and the delay in such notification. If others advised the victim to call the police, the crime was more likely to be reported, but, ironically, the greater the number of people consulted, the longer the delay in reporting the crime. The second convergent finding was that situational factors played a stronger role in affecting delay in notification than did characteristics of the victim. Situational factors included the type of crime and the time of day when it was discovered, as well as the number of others talked to and the type of advice received from them. Several divergent findings across methods raised questions about the relative adequacy of each method for the problems being investigated, and helped identify sources of similarities and differences in the conceptual and operational definitions employed.

Crime victims play a critical role in the operation of the criminal justice system. Victims' decisions may have an important impact on the police decision to arrest a suspect (Black, 1971), the prosecutor's decision to file charges (Hall, 1975), and the judge's sentencing decision (Dawson, 1969). Indeed, whether police even learn of a crime is largely determined by what the victim decides to do (Van Kirk, 1978). As Black and Reiss (1967) have shown, 85 percent of crimes investigated by the police result from notification by citizens,

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and the vast majority of these citizen reporters are victims. Hindelang and Gottfredson (1976) have aptly suggested that the victim is the "gatekeeper of the criminal justice system."

Despite the vital role victims play in mobilizing the criminal justice system, until recently there was little research on the primary victim decision—the decision to notify the police. Victims are clearly selective in their reporting behavior. The results of recent victimization surveys conducted jointly by the Law Enforcement Assistance Administration (LEAA) and the Census Bureau suggest that just one out of three crimes committed in the United States is ever reported to the police (Law Enforcement Assistance Administration, 1981). Even when victims do decide to call the police, how long they delay in reporting is the major determinant of when the police will arrive on the scene, and this delay often influences police effectiveness in investigating the crime (Van Kirk, 1978).

The research described here was designed to examine variables which affect a victim's decision to notify the police and variables which affect the amount of time that elapses before such notification occurs. We have employed a variety of methods, each with different weaknesses and strengths, to study decision making by crime victims. These methods include (a) an archival analysis of police records, (b) interviews with actual victims of crime, (c) simulation studies using college students, and (d) experiments in a field laboratory.[1] For the most part, we have focused on victims' reactions to property crimes, especially small thefts. Not only are they most amenable to the multimethod approach, but these crimes are among those most commonly committed and least likely to be reported (Law Enforcement Assistance Administration, 1981).

I. AN ARCHIVAL APPROACH TO VICTIM DECISION MAKING

In order to explore the circumstances associated with the reporting of property crimes, we conducted an archival analysis of police files. Archives represent a source of nonreactive[2] data and have proven to be an important source of information on

[1] The order in which the studies are discussed represents the most logical order for presenting them rather than the temporal order in which the studies were actually conducted.

[2] A research participant's knowledge that he or she is the subject of investigation can bias the investigator's data. This is commonly known as the reactive effect of measurement. A nonreactive measure can be obtained when a subject is studied without his or her knowledge.

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decision making in the criminal justice system (e.g., Konečni and Ebbesen, 1979). Police files contain a rich assortment of data pertaining to incidents of criminal victimization and, therefore, can provide clues about why victims delay in calling the police, and, to a lesser extent, about why the police are notified at all. Police reports include information about the victim's characteristics (e.g., age, race, sex), the identity of the person who reported the crime (victim, friend or relative of the victim, or bystander), the characteristics of the person who reported the crime (e.g., age, race, sex), when the crime was discovered, the type of property taken and its estimated value, when the crime was reported, and whether or not there was a suspect.

A sample of 989 police reports was selected for this study. These reports involved cases of larceny-theft (n = 597; 60 percent) and burglary (n = 392; 40 percent) in Mt. Lebanon, Pennsylvania for the years 1976-1978 (Ruback *et al.*, in press). In most cases the crime was reported to the police by the victim (85 percent); the remainder were reported by a friend (3 percent), a relative (8 percent), or a bystander (4 percent). Reporters ranged in age from 7 to 91 ($\bar{X}$ = 39); 59 percent were males, and almost all were white (over 99 percent).

The data were analyzed to find out first, who reported the crime (the victim or someone else), and second, what factors might explain the delay in reporting. Results showed that in reported theft cases, the reporter was more likely to be the victim (89 percent) than in reported burglary cases, when the reporter was the victim 79 percent of the time X^2 (1) = 18.89, $p<.001$. This may have been due to the fact that nonvictim others probably have a better opportunity to detect and report burglaries, because the commission of this crime usually involves a more complex series of actions than does theft (e.g., a neighbor notices a light on or a door open). Whereas the commission of a theft involves only the unlawful removal of property, burglary involves unlawful entry as well. The victim's sex also seemed to affect whether the crime was reported by the victim or someone else. When the reported crime involved a female victim, 89 percent of the cases were reported by the victim, whereas when the reported crime victim was a male, 83 percent of the cases were reported by the victim, X^2 (1) = 6.41, $p<.02$.

We next attempted to identify factors associated with delay in reporting. The delay interval was operationalized as the amount of time that elapsed between the discovery of the

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crime and when it was reported to the police. Because data were missing from 270 of the 989 cases in the file, the analyses of the delay in reporting were performed on the remaining 719 cases. This subsample did not appear to differ from the original sample in terms of who reported the crime (86 percent by the victim), the reporter's sex (59 percent were male), the reporter's race (over 99 percent were white), the reporter's age ($\bar{X}$ = 39), or the type of crime (59 percent theft, 41 percent burglary).

The median delay in reporting was 1 hour and 42 minutes; 36 percent reported within 30 minutes of discovering the crime, and 84 percent reported the incident within 24 hours. The delay interval ranged from zero (the incident was reported as soon as it was discovered) to 115 days. Since the distribution of scores had a strong positive skew, the data were transformed (as recommended by Kirk, 1968: 66) by adding a constant of one to each delay score and then taking the reciprocal. This form of transformation is often appropriate when the data consist of reaction time scores in which a number of scores are zero (14 percent had a delay score equal to zero). In order to identify factors that predict delay in reporting, a hierarchical multiple regression analysis was performed on the transformed scores. Predictor variables were entered into the regression equation on the basis of their logical temporal order. First, factors about the crime were entered. They included the type of crime (burglary or theft), the time of day it was discovered, and whether or not there was a suspect in the case. Next entered were factors about the person reporting the incident. These factors included who the reporter was (victim or nonvictim) and the sex of the reporter. The reporter's age and the value of the stolen property were not entered because of the large number of cases that did not contain this information.

Results of the multiple regression analysis revealed that three factors explained some variation in the delay in reporting: (a) type of crime, F (1,685) = 23.81, $p<.01$; (b) the time of day when the crime was discovered, F (1,684) = 9.82, $p<.01$; and (c) whether or not there was a suspect in the case, F (1,683) = 12.26, $p<.01$. Longer delays in reporting occurred when the crime was a burglary, it was discovered earlier in the day, and no suspect was named. Together, the three factors, all of which pertained to the crime, accounted for just seven percent of the variance, F (3,683) = 17.18, p<.01. When factors about the reporter were added to the regression equation (i.e.,

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victim or nonvictim, sex of reporter) the amount of additional variance accounted for was not significant.

Another regression analysis based on fewer cases with complete information was performed in which both the value of the property stolen and the age of the reporter were added as predictor variables. As before, factors about the crime were entered first (type, time, suspect, value), and then factors about the reporter were entered (who reported, sex, age). In this analysis factors about the crime accounted for only four percent of the variance in reporting delay, F (4,371) = 3.81, $p <$ .05. When factors pertaining to the reporter were entered, the amount of additional variance accounted for was not significant. This was true even when, in a subsequent analysis, characteristics of the reporter were entered first. These results suggest that characteristics of the situation (in this case the crime) were better predictors of reporting delay than were the characteristics of the reporter, at least those available from police records.

The archival data also call into question an explanation commonly offered for the reporting of property crimes—namely, that of receiving reimbursement from an insurance policy. In 65 percent of the cases in which there were complete data, the victim did not qualify for compensation from an insurance policy because the property was valued at less than $100 (the amount deductible on a typical insurance policy) or because the loss consisted of cash, which is not covered by most policies. Assuming that many victims were familiar with the rule governing their insurance deductible, it appears that their motivation to notify the police could not have been insurance compensation. This reasoning is reinforced by the finding that in many cases the value of the property was less than $25, a minimal loss for the reporters who tended to be affluent middle- to upper middle-class suburbanites.

Although police data archives contain a rich store of information about the characteristics of the crime and the person who reported it, such information accounted for only a small portion of the variance in reporting delay. In retrospect, this may not be surprising. Information contained in police files reflects the purposes, biases, and errors of those who contribute to and assemble such archives. Thus, important information relevant to victims' reporting behavior may be recorded inaccurately or may not be recorded at all. Police reports are usually obtained from victims who have incomplete knowledge and who may still feel stress from the victimization

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incident. For example, in our data archive 270 of the 989 cases did not contain complete dates and times for both the discovery and reporting of the crime. The value of the missing property and the age of the person reporting the crime were also frequently omitted. Further, police report files contain little or no information about other possible determinants of victims' decisions to report the crime and their delay in reporting, such as how many people victims consulted with before calling the police and what advice they received from them. In order to investigate the effects of social psychological variables not typically included in police files, we decided to conduct interviews with crime victims.

II. INTERVIEWS WITH CRIME VICTIMS

A major advantage of the interview is that it permits in-depth questioning of victims concerning their thoughts, feelings, and behavior upon learning of their victimization. For this reason, the interview approach is probably the most common method for studying victims' reactions to crimes. Exemplifying this approach are the national victimization surveys (e.g., Law Enforcement Assistance Administration, 1981). In the national surveys a sample of United States citizens is questioned about any criminal victimization they have experienced in the previous six months. The sample thus involves victims who notified the police and those who did not. Because of the tremendous expense involved in conducting such a survey of the general population (Garofalo, 1977), and because we were interested in learning more about time delays in reporting victimization, we interviewed only those victims who reported their victimization to the police. Specifically, we wanted to know, first, about the role played by social influence in victims' decision to call the police. Experimental research by Greenberg *et al.* (1979) has shown that theft victims are strongly influenced by the advice received from others. A second purpose of the interviews was to examine whether the social influence experiences of theft victims were different from those of burglary or robbery victims. Since theft is generally considered to be a much less serious crime than the other two, we expected that theft victims would be least certain about the advisability of notifying the police and might report more extensive advice seeking. As Festinger (1954) and others (e.g., Suls and Miller, 1977) have shown, people who are uncertain about the correctness of their attitudes and beliefs are likely to turn to others for advice to resolve their uncertainty. Third, we

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wanted to find out if there is a relationship between the number of social influence attempts and the latency in reporting of the crime. The fourth purpose was to assess the relative strength of situational versus person variables as predictors of delay in reporting.

To answer these questions, we conducted structured in-depth interviews with 58 crime victims: 21 theft victims, 24 burglary victims, and 13 robbery victims. Respondents were 24 males and 34 females, predominantly white (76 percent), between the ages of 16 and 76 ($\bar{X}$ = 40), and with an average household income between $15,000 and $20,000. All had reported their victimization to the police between 1978 and 1980 in one of four communities: McKeesport, Swissvale, and Mt. Lebanon, Pennsylvania; and Atlanta, Georgia (Westcott *et al.*, 1982). Respondents, who were paid $10, volunteered for the interviews in response to a letter from police departments in these four communities.[3] The average length of time between the victimization and the interview was 12 months.

The interview, which took about one hour, was usually conducted in the respondent's residence by one of two male graduate students. Two interview schedules were employed—one for victims of burglary and theft, and another for robbery victims. The 130-item questionnaire administered to burglary and theft victims dealt with the specifics of the crime (e.g., time and place of occurrence, value of property taken); victims' feelings, perceptions, and actions following discovery of the crime; attitudes of victims towards the local police and the criminal justice system in general; and victims' demographic characteristics. The 160-item questionnaire administered to robbery victims included a number of additional items which assessed victims' recall of the crime itself and their description of the robber(s).

Results suggest that social influence factors may have played a role in victims' decisions to notify the police. Sixty percent of the victims said that before calling the police they had spoken with at least one other person, and one quarter of the victims reported they had spoken with two or more others. Over half of those who talked with someone (56 percent) said that the person(s) offered them advice as to what to do. In

[3] A total of 525 letters were sent out which produced 72 replies (a 13 percent response rate). Interviews were conducted with 61 of these respondents, but three respondents were excluded from the final analysis: two provided insufficient information, and the third was a psychiatric patient living at home who reported being on medication the night of the incident, an incident the police doubt ever occurred.

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only one case was the victim advised not to notify the police. In essence, the victims, all of whom reported their victimization to the police, acted in a manner consistent with the advice given them. However, because the study lacked a group of nonreporters, the findings can be viewed as suggestive at best. From reporter data alone we could not learn whether nonreporters also received advice to report the crime and chose to ignore it, whether they received less or no social encouragement to call the police, or whether they were advised not to report the crime and chose to accept this advice. The present data do, however, testify to the high frequency with which crime victims are exposed to social influence attempts, a finding consistent with results of a similar study conducted in Kansas City, Missouri (Van Kirk, 1978).

The comparison of theft victims who reported their victimization with burglary and robbery victim-reporters revealed that the number of people reporters talked to differed with type of crime, $F\ (2,55) = 8.16, p < .001$. Planned pairwise comparisons indicated that, among these reporters, theft victims talked with more people ($\bar{X} = 1.86$) than did burglary victims ($\bar{X} = .71$), $F\ (1,55) = 12.18, p < .001$, and robbery victims ($\bar{X} = .54$), $F\ (1,55) = 11.5, p < .005$.

Another set of analyses examined the relationship between social influence attempts and delay in reporting (defined as the victim's estimate of the time elapsing from the discovery of the crime to the time when the police were notified). Across all three categories of crime, the median delay time was 15.25 minutes. Results revealed a rather robust positive relationship ($r = .54, p < .001$) between the number of people victims said they had talked to and the reported delay in calling the police. The more that victims consulted with others, the longer was the delay in reporting. This may explain in part why theft victims delayed longer in reporting the crime (Mdn = 59.3 minutes) than either burglary (Mdn = 8.5 minutes) or robbery victims (Mdn = 5.1 minutes), $F\ (2,55) = 4.40, p < .02$. Planned pairwise comparisons between theft and burglary, $F\ (1,55) = 6.99, p < .025$, and between theft and robbery, $F\ (1,55) = 5.73, p < .025$, were both significant. Before notifying the police, victims of theft talked with more people than either of the other groups, which led to a longer delay in reporting the crime.

In order to test this reasoning further, a hierarchical multiple regression analysis was performed on the delay scores in which the first predictor entered was the number of people

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talked to and type of crime was entered next as two dummy variables. The number of people talked to accounted for 30 percent of the variance, F (1,56) = 23.63, $p < .001$, while type of crime accounted for only an additional two percent of the variance ($F < 2$). Thus, once the number of people talked to was entered, adding type of crime to the equation did not increase the predictability of reporting delay. A more conservative test of this reasoning was performed by conducting a second regression analysis in which type of crime was entered first and the number of people talked to was entered next. Results of this analysis showed that type of crime accounted for 14 percent of the variance in the delay score, F (1,56) = 8.93, $p < .005$.[4] Despite being entered later, the

Table 1. Regression on Delay in Reporting

- Interview Study -

Variable	F	Rsq	Pearson r
Number of people talked with prior to reporting	19.83*	.297	.54
Anything stolen? (1 = yes, 2 = no)	2.21	.032	.06
Desire for getting property back as reason for reporting (1 = not at all important, 7 = very important)	2.15	.031	.17
Desire to see thief caught as reason for reporting (1 = not at all important, 7 = very important)	1.29	.018	−.37
Race of respondent (1 = black, 2 = white)	1.96	.027	−.08
Education of respondent	1.49	.020	.21
Type of crime (burglary vs. robbery & theft)	1.51	.020	−.22
Employment status (1 = employed, 2 = unemployed)	1.28	.017	−.16
Type of dwelling (1 = house, 2 = apt.)	0.94	.013	−.17
How upset were you? (1 = not at all, 2 = very)	0.91	.012	.04
	Total =	.49	

*$p<.001$
Overall F (10,38) = 3.62, $p<.005$

[4] There were three types of crime, and therefore two dummy variables were required. The F reported here refers to theft *vs.* the other two types of crime (robbery and burglary). The F for the second dummy variable (burglary *vs.* robbery) was not significant ($F < 1$), once the first dummy variable was entered. Also, the reported amount of variance accounted for by type of crime is the sum of the R squares for the two dummy variables. When type of crime is entered first the number of people talked with was actually entered third.

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number of people talked to accounted for an additional 17 percent of the variance in reporting delay, F (1,54) = 13.7, $p < .001$. These results suggest that while type of crime may influence the number of people one talks to, the number of people talked to makes an independent contribution to delay in reporting.

In order to address the fourth purpose of this study—assessing the relative strength of situation versus person variables as predictors of delay in reporting—a stepwise multiple regression analysis was conducted in which 24 predictor variables were entered into the regression equation. As shown in Table 1, the number of people spoken with by itself accounted for more of the variance of delay in reporting (30 percent) than did the additional contribution of the next nine predictors together (19 percent). Of the 10 best predictors, only three were clearly victim characteristics (race, education, and employment status). Further, these person variables accounted for only 6.4 percent of the variance. Thus, delay in reporting was generally unrelated to victims' demographic characteristics, their attitudes towards the criminal justice system and local police, the existence of a suspect, whether or not the property was insured, or how angry victims reported they were when they first discovered their victimization.

III. COMPARING THE ARCHIVAL AND INTERVIEW RESULTS

The results of the interviews with victims of crime both extend and reinforce the findings from the archival study. As in the archival study, victims' reactions appeared to be more a function of characteristics of the situation than of victim characteristics. However, the interviews did yield two findings which are inconsistent with results obtained from the archival analysis. Whereas the archival study found that delay in reporting was greater for burglaries than for thefts, the results of the interview study pointed to the opposite conclusion. The results of both studies conflict with the results of an interview study in Kansas City, Missouri (Van Kirk, 1978), which found no differences in reporting delay for burglaries and thefts that were discovered after the crime had occurred. The fact that two studies using the same methodology yielded inconsistent results suggests that the discrepancy between our interview and archival data concerning the delay in reporting burglaries

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and thefts may derive from other factors besides differences in methodology. Clearly, more research is need on this topic.

Another discrepancy between our archival and interview results for which there is no readily apparent explanation concerns the effect of the existence of a suspect on delay in reporting. Whereas the archival data showed that the absence of a suspect in the case increased the delay in reporting, no significant effect was found in the interview data. In fact, the means in the interview study differed in the opposite direction from those found in the archival study.

While the interview approach to the study of decision making by victims offers the advantage of allowing for in-depth probing of responses, the validity of the findings rests on two tenuous assumptions—namely, that victims can accurately recall details of the victimization incident and their ensuing reactions, and that they are willing to relate this information to the interviewer. With regard to the first assumption, evidence gathered by Loftus and her colleagues (Loftus, 1979) demonstrates the unreliability of eyewitness recall and shows how easily memories for events can be modified by extraneous factors. Reverse record checks further testify to the difficulty victims have in recalling the victimization incident (Yost and Dodge, 1970). The second assumption, that victims will be motivated to tell the truth, also seems suspect. What little evidence there is suggests that victims are fully capable of withholding or concealing information from the interviewer. A reverse record check (Law Enforcement Assistance Administration, 1972) revealed that crimes in which the suspected offender was a relative or acquaintance of the victim were less likely to be "recalled" than crimes in which the suspected offender was a stranger. Presumably, victims were less willing to disclose to an interviewer details of criminal incidents perpetrated by a relative or acquaintance.

Even assuming the best intentions, it may be difficult for a crime victim to reconstruct a troublesome event that occurred several months earlier. To produce an accurate retrospective report on the information gathered and the process of reaching the decision to report the crime may be positively Herculean. In an attempt to obtain more information about this decision making process, we used a simulation approach as our next investigative procedure.

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IV. A SIMULATION APPROACH TO VICTIM DECISION MAKING

According to Abelson, a simulation involves the "flexible imitation of processes or outcomes for the purpose of clarifying or explaining the underlying mechanisms involved" (1968: 275). Participants in simulation studies are asked to imagine themselves in a certain role or situation and to render a hypothetical decision. It is a procedure commonly used by psychologists to study decision making by jurors (Weiten and Diamond, 1979).

In the first of these studies, which used college students as subjects, we investigated victim decision making using techniques borrowed from cognitive psychologists. The first study was based on the theorizing of Schank and Abelson (1977), who suggest that people have ideas about stereotyped sequences of events called "scripts." These scripts enable people to predict what typically happens in a given situation. For example, a person's "eating in a restaurant script" would begin with receiving the menu and placing one's order and would proceed through eating the meal, asking the waitress for the bill, paying the bill, leaving a tip, and exiting the restaurant.

In the first study to be reported here, we attempted to discover subjects' scripts for reporting a theft to the police. Our reasoning was that these scripts would provide us with some insights and clues about their expectations and thus tell us something about their decision making in such situations. Using procedures borrowed from Bower, Black, and Turner (1979), we asked 21 males and 16 females to list in chronological order the typical events that would occur following a report to the police of a $5 or a $300 theft. We found that in the $300 theft script, subjects believed that the police were more likely to come to the victim's home, to ask the victim questions about the theft, to fill out a report, and to investigate the crime. In response to a question following the script task, subjects judged that the police were more likely to recover the stolen money when the theft was $300 than when it was $5.

A second simulation study employed an explicit information search task (Payne *et al.*, 1978). Undergraduate subjects (11 males and 10 females) were asked to imagine that they had returned home one day and discovered that their residence had been broken into. They were then shown an information board containing 12 items of information which, they were told, they might want to consider when deciding whether or not to call the police. Subjects were free to choose

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Table 2. Type of Information Requested Following Discovery of a Burglary

Type of Information Requested	Percentage Choosing	Frequency
1. What property was taken?	81%	17
2. Did the neighbors see anything?	57%	12
3. How did the burglar get in?	48%	10
4. Likelihood of getting the property back?	38%	8
5. Likelihood of catching the burglar?	33%	7
6. How much crime has there been in the area?	19%	4
7. How much time will I have to spend with the police?	19%	4
8. Will the neighbors talk to the police?	19%	4
9. What is the likelihood of the burglar retaliating?	10%	2
10. Advice of friends?	10%	2
11. Advice of relatives?	5%	1
12. How much time will I have to spend in court?	0%	0

Note: $n = 21$.

the order and the number of information items that they wanted. The information items had been drawn from our previous experimental research on victims, from the national victimization surveys conducted by the Law Enforcement Assistance Administration (1977) and the Bureau of Census, and from a pilot study in which college students had been asked to list questions that they would want answered after discovering a burglary. Subjects were asked to assume that they did not already have any of the information. As shown in Table 2, the three most frequently requested items concerned, respectively, what property was taken, whether the neighbors saw anything, and how the burglar got into the house. The least requested items concerned how much time would have to be spent in court and what friends and relatives would advise them to do.

Another study using a slightly different methodology, confirmed these results. In this study, 51 undergraduates (22 males and 29 females) were asked to imagine that they returned home to find that a burglary had occurred. Subjects were given a list of 18 questions in one of two counterbalanced

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Table 3. Mean Ranking of Order in Which Subjects Would Want Questions Answered

Mean Rank	Question
1.92	Was any property taken?
3.92	What kind of property was taken?
5.51	Did my neighbors see anything?
6.61	How much is the missing property worth?
6.71	Is the property insured?
8.94	If I call the police, how much time will it take for them to come here?
8.94	Was anyone else burglarized?
9.06	If I call the police, how likely is it that I will get my property back?
9.10	If I call the police, how likely is it that the thief will be caught?
9.61	What would the police do if I called them?
9.92	If my neighbors saw something, would they tell the police?
10.33	What is the right thing to do in this situation?
11.90	Is this the kind of thing the police should know about?
12.39	Do I have to call the police to file an insurance claim?
13.27	Do I know the thief?
13.39	If the police catch the thief, what is likely to happen to him?
14.12	If I report the crime, how much time will I have to spend in the future going to the police station and to court?
15.54	What would my friends recommend I do?

Note: $n = 51$.

orders[5] and were asked to rank the questions in terms of the order in which they would want them answered. Again, the questions ranked near the top concerned the property taken and whether the neighbors saw anything. The question that ranked last dealt with what their friends would recommend they do.

The results of these simulation studies seem inconsistent with the interview findings concerning the role of social influence in victim decision making. Whereas the victims in the interview study generally reported exposure to such influence, the simulation victims reported little interest in the advice of others. Several explanations may account for these apparently disparate results. First, the interview findings dealt with crimes of theft, robbery, and burglary; the simulation

[5] The order in which the questions were presented had virtually no effect on the mean rankings (rho = .91).

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studies concerned only the crime of burglary. Perhaps, as the interview findings suggest, the importance of social influence varies with the type of victimization, with victims of burglary being less open to social influence than theft victims. Second, the victims in the simulation studies may have accorded low priority to the opinions of others because of the low social desirability associated with asking others for advice. That is, in order to manage the impression (Goffman, 1959) of being an independent thinker and therefore not easily swayed by others, subjects in the simulation studies may have attempted to minimize the importance of social influence in their decision making. Third, simulation subjects may have found it difficult to place themselves in the psychological state of a burglary victim, since very few of them had been in this situation before.

A fourth explanation is that the *existence* of opportunities for social influence reported in the interviews does not necessarily imply that available social influence attempts are *successful*. The simulation results may reflect a lack of genuine need for advice, even when frequently offered. Finally, the content of the type of information requested in the simulation data does suggest some value of social interaction: while subjects did not request the category 'advice,' they would most probably have questioned other people to find out if the neighbors saw anything and if the neighbors would talk to the police and to help assess the likelihood of getting the property back and catching the burglar. Thus, all of these categories involve, to some extent, forms of advice.

V. A FIELD-EXPERIMENTAL APPROACH TO VICTIM DECISION MAKING

In order to avoid the pitfalls of relying exclusively on records collected by others (the archival method), or retrospective self-reports (the interview method), or on the "as if" experience of the simulation method, we chose to examine victim decision making through experiments in the controlled setting of a field laboratory. This approach attempts to account for victim decision making by placing participants in a phenomenologically "real" situation and observing their responses to a series of systematic manipulations. This method permits the controlled examination of variables that appear to be potentially influential, but which the other methods discussed may not investigate adequately. Specifically, in the controlled field experiments we could

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manipulate social influence attempts and test their effect on reporting.

Whereas the virtues of the experimental approach have been recognized in the study of decision making by bystanders (e.g., Bickman and Rosenbaum, 1977; Latané and Darley, 1970), they have gone unnoticed in the study of decision making by victims. With support of a grant from the National Institute of Mental Health's Center for Studies of Crime and Delinquency, we created "victims" of a petty theft in a field laboratory located in a middle-class retail section of Pittsburgh. The laboratory consisted of a suite of offices made to resemble a research organization conducting research on office efficiency. The organization was given the fictitious title, "Industrial Research Associates of Pittsburgh," and was furnished with office equipment and staffed by a supervisor, secretary, and clerical assistant.

With some slight variations, the procedure to be described was used in a series of six studies (Greenberg *et al.*, 1980). Participants responded to a newspaper advertisement offering eight dollars for one-and-a-quarter hours of light work. After respondents were screened for various medical problems, those who were accepted were given an appointment time. Participants (n = 768) represented a broad spectrum of the Pittsburgh community. They ranged in age from 16 to 66 ($\bar{X}$ = 31); 66 percent were female, 33 percent male; 83 percent were white, 17 percent black. Four percent had graduated from junior high school; 55 percent had a high school degree; six percent had a two-year college degree; 28 percent had a college degree; and seven percent had a graduate degree. Asked their family income, 43 percent of the participants listed it as less than $6,000, 27 percent between $6,001 and $12,000, 17 percent between $12,001 and $18,000, and 15 percent above $18,000. A broad spectrum of occupations was represented including blue-collar workers (26 percent), white-collar workers (30 percent), housewives (20 percent), students (18 percent), artists (four percent), and retirees (1.5 percent). At the time of their participation, 69 percent classified themselves as unemployed.

When they arrived, participants were directed by a sign to a room containing several desks behind glass partitions and two couches near the door. After a few minutes, the participant was joined by a confederate of the experimenter, who would later turn out to be the "thief" in the staged crime.

The secretary then entered, showed the participant and confederate to their seats, explained to them that this was a

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research organization studying "office efficiency," and then paid the participant and confederate the eight dollars promised in the newspaper advertisement. The supervisor then entered and instructed them on the first clerical task, which was to alphabetize a stack of name cards. The participant and confederate worked on this task for six minutes, and then their completed work was collected by the supervisor. After a few minutes, during which time their work was ostensibly scored, the participant and confederate were told that they had performed above the norms for their age group and therefore had earned an additional 12 dollars. They were paid the 12 dollars and asked to sign a receipt. The supervisor then instructed them on the second task, which was described as a bookkeeping task. They were told that they had to transfer columns of numbers from a card to a blank form, add the numbers in the columns, place their initials in the upper right-hand corner of the form, clip the card and form together, put them in a manila envelope, and put the envelope in a box behind them. The participant's box was directly above the confederate-thief's box, so that participants could see that the thief had easy access to their completed work. The experimenter explained that both could earn additional money depending on the number of forms they completed accurately in the eight-minute period. With this incentive of earning additional money, the participants worked diligently and quickly, much quicker than the confederate-thief who was working in the cubicle directly behind them. The thief deliberately and noticeably worked more slowly than the participant, spending a great deal of time erasing. After the participant's and confederate's work was collected and ostensibly scored, the participant was told over an intercom-speaker system that because he or she had performed below the norms for his or her age group 11 of the 12 dollars earned earlier would have to be returned. The announcement also informed the confederate-thief that he had performed above the norms for his age group and had earned an additional 11 dollars. Both were then instructed to go to the main office where the final money transactions would be performed and some final forms filled out.

In the main office the participant was instructed to give the 11 dollars to the thief (who had earned 11 dollars) and then to begin filling out some forms. At this point a clerical assistant knocked on the door to the office and asked the secretary to step into the anteroom. After a brief conversation with the

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assistant which the participant could easily overhear, the secretary returned and stated that there appeared to be a problem with some of the completed work. She asked the participant and the confederate-thief to remain in the office while she investigated the problem. Shortly afterwards, the thief left the room. The secretary, upon returning, presented evidence that the thief had taken some of the participant's work and had erased the participant's initials and substituted his own. This amounted to stealing the participant's money. At this point many participants spontaneously recalled that the thief had been working very slowly, and that they had wondered how he could have done so well on the task. Many also recalled having heard the thief erasing during the task.

The secretary stated that she could not do anything about the lost money without her supervisor's permission. She then left the room to look for him. When she returned after a few minutes without being able to locate him, she suggested that the participant should call the police if he or she wanted to do something about the theft. She then employed a series of prearranged verbal prods of increasing pressure, which were designed to induce the participant to call the police. Participants' willingness to call the police was measured on the basis of which of the prods, if any, they yielded to. Participants who took the phone after the first prod were assigned the highest reporting score, and those who resisted all the prods received the lowest reporting score. The statements participants made while they were deciding whether or not they should call the police were written down by an assistant who was in an adjacent observation room. When participants either decided to call the police and took the phone or decided not to call the police, the experiment was ended. Thus, in no case were the police actually called about the staged theft.

After participants had decided either to call or not to call the police, the supervisor returned. In detail, he explained the true nature of the study and why deception had been used. When participants said that they understood this information, they were questioned about their perceptions of the theft and the factors that influenced their decision. They then received a bonus of three dollars, and were asked to sign a consent form allowing us to retain their data. Finally, they completed an anonymous questionnaire assessing their personal reactions to the study.

The six studies to be described investigated the influence on reporting of the following: (1) the characteristics of the

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crime (magnitude of the theft), (2) the victim's characteristics (age, sex, race, family earnings, education, religion, and state of anger), (3) the thief's characteristics (race, proximity at the time of reporting), (4) characteristics of others on the scene, such as their sex, proximity to the victim, offer of emotional support, type of advice offered, type of supporting argument, and the similarity of their fate to that of the victim (i.e., bystander or covictim).

First Study: Magnitude of Theft, Race, and Proximity of Thief

In the first study three independent variables were factorially manipulated in a 2 × 2 × 2 design (Greenberg *et al.*, 1982). Subjects were 94 females and 26 males who were randomly assigned to the eight conditions (n = 15 per cell). The three variables consisted of (1) magnitude of theft ($20 versus $3), (2) race of the thief (black versus white), and (3) proximity of the thief at the time of the report opportunity (thief present versus thief absent). Proximity of the thief was manipulated by calling the participant in first to meet with the secretary (thief present) or second (thief absent).

Table 4. Mean Reporting Scores as a Function of Thief Proximity and Magnitude of Theft[a]

Proximity of thief	Magnitude of Theft	
	$20	$3
Present	$2.40_{a,b}$	$2.50_{a,b}$
Absent	2.80_{a}	2.20_{b}

[a]Means with different subscripts differ at the .05 level of significance (Duncan's Multiple Range Test).

An analysis of variance performed on the data revealed that none of the main effects were significant. The analysis yielded a significant interaction between proximity of the thief and magnitude of the theft, F (1,112) = 4.09, $p<.05$. When the thief had left the premises (thief absent condition) participants were more likely to report the theft it they lost $20 than if they had lost just $3. But if the thief was still in the building (thief present condition) the amount taken did not affect reporting. Although there was no main effect for participants' sex, there was a significant interaction between proximity of the thief and sex of the participant, F (1,112) = 6.37, $p<.02$. Males were more likely to call the police when the thief had left the premises, whereas females were more likely to call when the thief was still in the building. As we observed participants wrestle with

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the decision about calling the police, it was readily apparent that many were influenced by the prodding of the secretary.

Table 5. Mean Reporting Scores as a Function of Thief Proximity and Sex of Victim[a]

Proximity of thief	Sex of Victim	
	Male	Female
Present	2.25_c	$2.51_{a,c}$
Absent	3.00_a	1.56_b

[a]Means with different subscripts differ at the .05 level of significance (Duncan's Multiple Range Test).

The remaining five experiments focused on the effect of social influence on victim reporting. In these studies a white female confederate in her mid-twenties, posing as a participant, played the role of bystander. While the secretary was looking for her supervisor, the bystander attempted to influence participant-victims' decision to call the police.

Second Study: Victim Anger and Bystander Advice

The second study was specifically concerned with the effects of victims' emotional state and the advice of a bystander on victims' decision to report the theft to the police (Greenberg *et al.*, 1979). The interest in the victim's emotional state grew out of our observations during the first study. We informally observed that many participants became emotionally aroused when they learned of the theft, and that those who appeared most angered seemed most willing to call the police. In the second study 126 participants (90 females and 36 males) were randomly assigned to one of nine conditions in a 3 × 3 factorial design. The two independent variables were (1) victim anger (high, low, or none) and (2) bystander advice (advised action; advised no action; gave no advice). After the thief (who was played by a black male in his early twenties) had "fled" the scene, the bystander provided a model for one of three levels of anger about the theft:

High anger: "He erased your initials and put his own on your papers? I can't believe this! Boy, that really makes me angry. I'm really surprised that something like this happened." Throughout, she appeared very agitated.

Low anger: "You know, this really bothers me. It's annoying having something like this happen." Throughout, she appeared slightly agitated.

No anger: The bystander said nothing to the participant. She remained seated and responded only if the participant directed a question to her.

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The attempt to manipulate participants' anger by having the bystander model different levels of anger was based on Schachter and Singer's (1962) finding that the reactions of others can influence how one labels a state of arousal. After displaying one of these levels of anger, the bystander offered one of three forms of advice:

Advised action: "I wouldn't let him get away with this. I think you should do something about it."
Advised no action: "I wouldn't do anything. This probably isn't worth bothering about. He's already gone."
No advice: The bystander simply remained silent and offered no advice.

After delivering her lines, the bystander looked at her watch, said she had to keep an appointment, and left the room.

Prior to testing the effects of the independent variables, we analyzed the data to determine if any of the demographic variables correlated with reporting. These analyses revealed that reporting was negatively correlated with participant's age (r = -.20, p<.02.) and household earnings (r = -.22, p<.01.). Therefore both were employed as covariates in an analysis of covariance which was used to test the significance of the independent variables. This analysis produced a significant main effect of bystander advice, F (2,110) = 3.13, p<.05., but did not produce a main effect for victim anger or an interaction effect. Post hoc comparisons using Duncan's New Multiple Range Test showed that those advised to take no action ($\bar{X}$ = 2.03)[6] were significantly less likely to report than were those in the no advice condition ($\bar{X}$ = 2.72, p<.05). However, participants who were advised to take action ($\bar{X}$ = 2.57) were no more likely to report the incident than were those who were advised to take no action or who were given no advice.

A subsequent check on the manipulations showed that while the level of bystander anger was perceived to be in line with the manipulations F (2,117) = 14.69, p<.001, the manipulations did not influence participants' self-reported anger. Participants may have been sufficiently aware of their subjective feelings of anger that they did not need to rely on cues provided by the bystander to label their state. Accordingly, participants were reclassified on the basis of their level of *self-reported anger* into three groups: high, moderate, and low self-reported anger. An analysis of covariance (using age and household earnings as the covariates) performed on the reconstituted self-reported anger × bystander advice matrix produced a significant self-reported anger main effect, F

[6] All mean report scores in studies 2-6 are adjusted for the covariate.

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(2,109) = 5.37, $p<.01$, and a marginally significant advice main effect, F (2,109) = 2.70, $p<.08$. The higher the level of self-reported anger, the greater was the willingness to report. The test for linear trend was significant, F (1,109) = 25.68, $p<.001$, as was the post hoc comparison between high and low self-reported anger ($p<.01$). Since the self-reports of subjective anger were obtained *after* participants made their decision to report the incident, the casual relationship may be the reverse of what we were hypothesizing. That is, those who agreed to report may have reasoned from their behavior that they must have been quite angry.

In order to clarify the direction of influence, what was needed was a measure of anger or arousal obtained *before* the decision to report. Such a measure was obtained. Participants' behavior immediately after the experimental manipulation was unobtrusively observed by an assistant blind to the manipulations. Two judges, also blind to the conditions being run, later coded the observations into two categories: high overt arousal (e.g., nail-biting, cursing to oneself, pacing) and low overt arousal (absence of the above). The percentage of participants showing signs of high overt arousal in the high, moderate, and low self-reported anger conditions was respectively, 74 percent, 59 percent, and 50 percent. The difference between the high and low conditions was significant, X^2 (1) = 3.86, $p<.05$. Thus, unobtrusive observations of participants' level of arousal taken prior to the reporting decision, paralleled participants' level of self-reported anger. These data, therefore, lend support to the hypothesis that anger facilitated reporting rather than the reverse.

The marginally significant main effect of advice on the reconstituted matrix yielded the same pattern of differences as was found in the original analysis. As tested by a Duncan's New Multiple Range Test, those advised to take no action were significantly less likely to agree to call the police than were those given no advice ($p<.05$). Those advised to take action were no more likely to report than those in the other conditions.

The results, while demonstrating the importance of the victim's emotional state at the time of the report decision, provide rather weak evidence for bystander social influence. There are several possible explanations for why the bystander's advice did not have greater impact. First, the advice was quite diffuse—that is, she merely advised the victim to "do something" or not to "do anything." Conceivably,

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victims intended to heed the bystander's advice "to do something," but this did not involve calling the police. Perhaps if the advice were more specific, such as "*You* should call the police," the effectiveness of the bystander's advice would have been more visible. Another factor that may have weakened the bystander's influence was failure of the bystander to back up her advice with any supporting argument. Finally, the bystander's influence may have been minimal because after offering her advice, she left the scene. Possibly her absence during the secretary's prodding reduced the salience of her position with regard to calling the police. A stronger influence attempt would have had the bystander be more explicit in her advice, provide supporting arguments, and remain present while the victim decided what to do.

Third Study: Type of Bystander Advice and Supporting Argument

In a third study (Ruback *et al.*, 1979) 138 participants (104 females and 34 males) were assigned to one of 8 conditions in 2 × 4 factorial design. The two independent variables were (1) type of bystander advice (advise to report, advise not to report), and (2) type of supporting argument (theft magnitude, police effectiveness, punishment of the thief, or no supporting argument). In addition, a "baseline" condition, consisting of a no advice-no supporting argument group was included. As in the previous study, the thief was played by a black male and the bystander by a white female.

The type of bystander advice was manipulated by having the bystander say to the participant, "I think you should call the police," or "I don't think you should call the police." The advice to report the theft was supported by one of the following arguments: (a) *theft magnitude*—"Eleven dollars is 11 dollars. You can buy a lot with that"; (b) *police effectiveness*—"They can do something about this. I'm sure they could get your money back"; (c) *thief punishment*—"I'd like to see him punished. I think it would teach him a lesson." The advice *not* to report the theft to the police was supported by one of the following three parallel arguments: (a) *theft magnitude*—"It's only 11 dollars. It's really not that much money"; (b) *police effectiveness*—"They can't do anything about this. I'm pretty sure they couldn't get your money back"; (c) *thief punishment*—"I wouldn't like to see him punished. I don't think it would accomplish anything." In the no-supporting-argument conditions, the bystander did not offer any argument

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to support her advice to report or not report the theft. Finally, in the baseline condition (i.e., no advice-no supporting argument) the bystander remained silently seated during the secretary's absence. Unlike the previous study, the bystander remained in the room while the secretary issued her prods to the participant.

Since a preliminary analysis showed that the participant's age correlated negatively with the report score (r = -.20, p<.02), an analysis of covariance was used to analyze the report data with participant's age serving as the co-variate. This analysis yielded a significant main effect for type of bystander advice, $F(1, 116) = 36.86, p<.001$. Those advised to call the police had a higher adjusted mean reporting score ($\bar{X}$ = 2.54) than those advised not to report the incident ($\bar{X}$ = 1.50). There was no significant main effect for type of supporting argument, nor was there any significant interaction effect. In order to determine whether the bystander's advice increased and/or decreased participants' willingness to call the police, a separate one-way analysis of covariance was used to compare the no-advice baseline condition with the advise-to-report/no-supporting-argument condition and the advise-not-report/no-supporting-argument condition. This test proved highly significant, $F(2, 38) = 7.61, p<.002$.

Individual comparisons using the Duncan's Multiple Range Test indicate that participants in the advise-to-report/no-supporting-argument condition had a higher adjusted mean report score ($\bar{X}$ = 2.95) than those in the baseline condition ($\bar{X}$ = 1.76, p<.05). In contrast, no significant difference was found between the advise-not-to-report/no-supporting-argument condition ($\bar{X}$ = 1.27) and the baseline condition.

The failure of the bystander's supporting argument to influence reporting may be accounted for by participants' failure to attend to and thus comprehend the message. A manipulation check showing that the bystander's argument was incorrectly identified by 37 percent of the participants supports this reasoning. Conceivably, participants were still sorting out details of the theft when the supporting argument was offered. Given this preoccupation, participants may have attended only to the simplest and clearest of messages, such as the bystander's advice to report or not report. Morever, the bystander's advice may have added to participants' distraction, causing many to "tune out" during delivery of the supporting argument.

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The significant results with regard to the type of advice variable show that a bystander's unambiguous advice to call the police can be a strong facilitator of victim reporting. However, these data leave unanswered the question of *how* such advice facilitated reporting. Conceivably, the bystander's presence during the secretary's prodding may have led participants to infer that they could count on the bystander's continued support when dealing with the police. In addition, participants may have believed that the bystander could provide the police with valuable information that could strengthen their case against the thief. These possibilities were explored in a fourth study (Ruback *et al.*, 1979).

Fourth Study: Bystander Proximity, Support, and Knowledge of the Theft

In order to investigate how bystander advice facilitated reporting, 120 participants (47 males and 73 females) were assigned to conditions in a 2 × 2 × 2 × 2 experimental design. The variables were (1) proximity of the bystander (present *vs.* absent), (2) the bystander's willingness to support participants in their future dealings with the police and the criminal justice system (support *vs.* no support), (3) the bystander's direct knowledge of the theft (eyewitness *vs.* not an eyewitness), and (4) the participant's sex. Unlike the previous two studies, the bystander *always* advised participants to report the theft, and the role of thief was played by a white male in his early twenties.

After evidence of the theft was presented by the secretary, the bystander said either that she had seen the thief take some of the participant's work but had not initially labeled it as a theft (eyewitness condition) or that she had not seen anything (not an eyewitness). The bystander then advised participants to report the theft and indicated her willingness to support them in their dealings with the police and the criminal justice system by stating, "I'll back you up. And if anything happens later on, you can call me at work." She then wrote her phone number on a slip of paper and handed it to the participant. This was done in half the cases (support condition). In the remaining cases, after advising participants to report the theft, the bystander indicated her unwillingness to support participants by stating, "But don't use my name, I don't want to get involved" (no support condition). The bystander then either remained present during the secretary's prodding or left after the secretary said she could.

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Table 6. Adjusted Mean Reporting Scores as a Function of Bystander's Support and Proximity[a]

	Proximity of Bystander	
Type of Bystander Support	Present	Absent
Support	2.73_a	1.80_b
No Support	1.80_b	1.90_b

[a]Means with different subscripts differ at the .01 level of significance (Duncan's Multiple Range Test)

Since age was negatively correlated with the reporting score (r = -.30, p<.001), the data were analyzed by analysis of covariance, with age serving as the covariate. This analysis yielded a significant Bystander Support × Bystander Proximity interaction, $F(1, 103) = 4.48$, $p<.05$, and a significant sex main effect, $F(1, 103) = 8.86$, $p<.01$ (females were more likely to report than males). As shown in Table 6, the bystander's advice to call the police facilitated reporting only when the bystander offered support *and* remained present. When one or both of these conditions were absent, participants were equally disinclined to report the theft. The main effect for sex may have resulted, in part, from the fact that the bystander in all cases was a female. Thus, females may have been more willing to accept the bystander's advice because of the perceived similarity between themselves and the influencing agent. The bystander's first-hand knowledge of the theft (eyewitness *vs.* not an eyewitness) did not influence reporting.

Fifth Study: Similarity of Bystander, Reason to Report, and Bystander Surveillance

In the fifth study (Westcott *et al.*, 1980) both male and female bystanders were used to test whether similarity between the bystander and the victim with regard to sex affects how likely victims are to follow a bystander's advice. In addition, there was still a question from the third study regarding the relative effectiveness of different supporting arguments in convincing victims to call the police. Thus, two arguments for calling the police were employed in the fifth study. The first, that calling the police is "the right thing to do," was the reason most often given by participants in the first four studies who agreed to report the theft. The second reason, relating to the effectiveness of the police, was chosen because police ineffectiveness was the reason most often given by participants who did *not* report their victimization in the first four studies. Our concern in this study was to see if these

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supporting arguments could be used to facilitate reporting rather than to inhibit reporting. Therefore, in all instances the bystander always advocated that the theft be reported to the police.

The final independent variable included in this study was the bystander's intention to remain with the participant during the prodding by the secretary. That is, would participants feel more pressured to follow the bystander's advice if the bystander *chose* to remain in order to see what the participant would do, than if the bystander were instructed to remain by the secretary?

In order to answer these questions, 132 participants (72 females and 60 males) were assigned to conditions in a 2 × 2 × 2 × 3 factorial design. The variables were (1) sex of the participant, (2) sex of the bystander, (3) type of supporting argument used by the bystander (principle *vs*. police effectiveness) and (4) type of bystander surveillance (intentional, unintentional, no surveillance). As in the previous study, the thief was played by a white male in his early twenties.

After the secretary had left to look for her supervisor, the bystander (who was played either by a white male or female in his or her twenties) advised participants to call the police and offered one of the following two supporting arguments:

> *Principle*—"You know, it's not the amount of money, it's the principle of the thing. He took something that didn't belong to him. What he did was wrong, and he shouldn't be allowed to get away with it. It's the right thing for you to call the police."
>
> *Police Effectiveness*—"You know, the police will probably catch him. The secretary has that guy's address and phone number. And the police in Squirrel Hill have a pretty good reputation with this sort of thing. It's the best thing for you to call the police."

When the secretary returned from her unsuccessful search for her supervisor she proceeded to manipulate the bystander surveillance variable. In one-third of the instances she asked the bystander to leave. The bystander replied "No, I'm curious to see what he'll (she'll) do" (intentional surveillance). In another third of the instances the bystander asked if he (she) could leave. The secretary responded "No, would you mind staying a little while longer" (unintentional surveillance). In the remaining third of the instances the bystander asked if he (she) could leave and the secretary responded, "Yes, you're free to go" (no surveillance).

An analysis of covariance (with age as the covariate) yielded a single, marginally significant Sex of Participant × Type of Supporting Argument interaction, $F(1,105) = 3.54$,

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$p<.07$. The argument based on police effectiveness was more persuasive in inducing females to call the police, whereas the argument based on the moral principle that it is the "right thing to do" was more effective in inducing males to report the theft. None of the other main effects or interaction effects was significant.

Although this study found that similarity of sex between the participant-victim and the bystander did not influence the bystander's persuasiveness, it may be that dimensions of similarity other than sex were being weighed by victims. One such dimension is similarity of *fate* between participants and the person attempting to influence them. If fate similarity is important in enhancing the persuasiveness of another person on the scene, then it would be expected that greater influence would be exerted by a covictim than by a bystander. Further, the more similar the covictim's losses are to the participant-victim's, the greater would be the expected influence of that covictim. These ideas were tested in a sixth experiment (Westcott *et al.*, 1980).

Sixth Study: Fate Similarity Between Victim and Others Present

There were 132 participants in the sixth experiment (72 females and 60 males) who were randomly assigned to conditions in a 2 × 2 × 3 × 2 factorial design. The four independent variables were (1) co-victim's decision to report (report *vs.* not report), (2) bystander's advice (report *vs.* not report), (3) magnitude of co-victim's loss relative to the participant's (greater, equal, less), and (4) sex of participant. As in the previous two experiments, the role of thief was played by a white male. The roles of bystander and co-victim were played by two white females.

Three confederates were present during each session (covictim, bystander, and thief). Participants received an influence attempt from *both* the co-victim and the bystander. In half the cases they agreed on whether or not the theft should be reported, and in half the cases they disagreed about what to do. No supporting arguments were used, and the order in which they spoke was counterbalanced to control for order effects. Whereas the participant's loss as a result of the theft was always $11, the co-victim lost either $20 (greater), $11 (equal), or $3 (less). An important procedural change was made in order to increase the generalizability of the paradigm. Instead of prodding participants to call the police, they were

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merely asked once by the supervisor if they wished to call the police. The response latency was then recorded by an observer. This latency measure was used to construct a continuous measure of reporting which was derived in the following way. First, a positive sign was assigned if the participants reported and a negative sign if they failed to report. The report score was derived by multiplying this positive or negative sign by 201 (the longest latency in seconds plus one) minus the latency score. That is: Report Score = ± 1 (201 − latency score). Thus, a participant who agreed to call the police in 10 seconds would have a report score of 191, [+1 (201 − 10) = ± 191]. If the participant took 50 seconds to decide *not* to call the police, the report measure would be −161. That is, −1 (201 − 50) = −161.

For all conditions combined, the median response latency was 25.5 seconds. An analysis of covariance was used to analyze the data with participant's age once again serving as the covariate. This analysis yielded a significant co-victim decision main effect, F (1, 107) = 4.27, $p<.05$. Participants were more willing to call the police when the co-victim decided to call than when the co-victim decided not to call. None of the other main or interaction effects were significant.

Fate similarity appears to be the important dimension of similarity which facilitated social influence in this study. Participants' assessment of fate similarity between themselves and the influencing agent apparently operated at a very gross level (i.e., co-victim or bystander). The degree of similarity between the participant's and the co-victim's losses apparently had no impact on the co-victim's influence on the participant's decision to report the theft.

Comparison of Field-Experimental Results with Results Using Other Methodologies

The results of these experimental studies show quite clearly that under appropriate conditions victims of a small theft can be influenced to call or not call the police by a bystander whom they have known for less than an hour. A bystander's advice is particularly likely to be followed when there is no co-victim present to dispute such advice, when the advice is specific (e.g., call the police), when the bystander remains at the victim's side while the victim is making his or her decision, and when the bystander offers the victim future support.

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These findings contradict the results from the simulation studies which showed that simulated victims accorded little or no weight to the advice of others when deciding whether or not to call the police. We would place more faith in the experimental results since they derive from the *actual* decisions of a diverse population of participants, than in the simulation results which are based on the *hypothetical* judgments of college students.

Table 7. Correlations Between Characteristics of Participants and the Decision to Report or Not Report the Theft

Characteristic	Pearson r	n
Sex	.03	768
Race	−.05	768
Age	−.21**	759
Family Income	−.14**	751
Education	.02	750
Employment status	−.03	512
Need for social approval	−.18*	245
Authoritarianism	.00	365
State of anger	.23**	521

*$p < .01$
**$p < .001$

The importance of the social environment as a factor in victims' decision to report a theft does not necessarily imply that personal characteristics of victims have no impact on this decision. As shown in Table 7, across all six field-experiments a number of person variables were significantly correlated with the decision to report or not report the theft. Participants' age, family income, and need for social approval (as measured by the Crowne-Marlowe Scale) were negatively correlated with the report decision. That is, the theft was most likely to be reported when the victim was young, had a low income, and had a low need for social approval (this last finding is directly opposite to what one would expect). Finally, the more angry participants rated themselves, the greater their willingness to report the theft. The significant correlations concerning participants' age and anger are inconsistent with findings from the archival and interview studies. However, as we will show in Section VI, both inconsistencies are more apparent than real.

It would appear that whether or not the police are called is a joint function of both situational and personal variables. Can the same be said with regard to delay in reporting? Results from the archival and interview studies indicated that

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Table 8. Regression on Delay in Reporting
- Experimental Studies -

Variables	F	Rsq
Type of advice (advised to report vs. advised not to report & no advice)	8.59**	.033
Family income	5.47*	.021
Race of participant	.35	.001
Type of advice (advised not to report vs. no advice)	.15	.0006
Age	.08	.0003
Sex	.07	.0003
Education	.06	.0002
		Total = .06

*$p < .025$
**$p < .005$
Overall $F(7,241) = 2.10, p < .05$

characteristics of the situation are better predictors of delay in reporting than are characteristics of the reporter. In order to explore this proposition further, we performed a step-wise multiple regression analysis on experimental participants who reported the theft. Since reporting in these experiments was measured in terms of which prod participants yielded to, we used the report score as a proxy for measuring delay. That is, those who delayed the longest had the lowest report score. Only the data for the reporters in studies 1-5 were used in this analysis since the procedure in study 6 did not involve the use of prods by the secretary.

As shown in Table 8, the variable that entered the equation first and accounted for the most variance was type of advice, $F(1,248) = 8.59, p<.005$.[7] The only other variable to account for a significant portion of the variance was family income, $F(1,247) = 5.47, p<.025$. A situational variable, type of advice, accounted for more variance than the cumulative additional variance accounted for by the five person variables (household income, race, age, sex, and education). These data, therefore, are consistent with the earlier reported finding from the archival and interview methods. However, this conclusion is

[7] There were three types of advice, again requiring two dummy variables. The F reported here refers to advised to report *vs.* the other two types of advice (advised not to report and no advice given). Once this variable entered the equation, the F for the second dummy variable (advised not to report vs. no advice) was not significant ($F < 1$). The second dummy variable actually entered the equation fourth. Again, the amount of variance accounted for by type of advice is the sum of the R squares for the two dummy variables.

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tempered by the finding that the seven variables entered in the regression equation accounted for only six percent of the variance in delay, and thus unmeasured individual variables may explain at least some of the variance still unaccounted for.

We would be remiss if we did not acknowledge some of the major limitations of our experimental paradigm for studying decision making by crime victims. Because of ethical concerns, its use is restricted to only the most minor of crimes. Alternate methodologies are needed to study more serious victimization incidents. Another question concerns the generalizability of these findings to victims of real crimes. While participants at the time were unaware that they were victims of a staged theft, they were aware of the fact that they were participating in a research project. This knowledge may have affected their responses. In addition, the theft itself may not have had as strong an impact on participants as would a theft of something that was in their possession before they arrived for the experiment.

VI. DISCUSSION

Our choice of a multimethod strategy to investigate decision making by victims is based on the belief that no one methodology is best and that using a combination of methods, each with differing sources of bias, is the most fruitful research strategy (Campbell and Stanley, 1966; Webb *et al.*, 1981). When convergent results are obtained from divergent methodologies, it is more plausible to attribute these results to features of the phenomenon under investigation than to a consistent error across methodologies or different errors within each methodology all yielding the same results.

The research reported here produced several convergent findings. First, it can be stated with some degree of confidence that social influence is an important determinant of both the decision to call the police and the timing in such notification. The potential role of social influence was shown in the interviews with crime victims who reported that prior to notifying the police they frequently consulted with and received advice from others. The causal impact of this advice was supported by the results of six field-laboratory experiments. The experimental studies revealed that the advice from a bystander, who less than an hour earlier was a total stranger, can significantly influence the decisions of victims of a small theft, especially if three conditions are met: (1) the advice is specific (i.e., "call the police," rather than "do

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something"), (2) the bystander remains present while the reporting decision is made, and (3) the bystander offers to be of help if needed in the future.

While results from the simulation studies failed to confirm the importance of social influence, a review of these studies revealed several potential reasons for this. They include (a) the type of victimization which subjects were asked to simulate (i.e., burglary victims), (b) the difficulty that college students might have had placing themselves in the role of a victim, (c) efforts by subjects to give a favorable self-presentation, and (d) the difference between being *offered* advice and the *need* for such advice. It may be that simulated burglary victims simply did not feel a genuine need for advice, although the type of information requested by them involves interaction with others.

The second convergent implication of this research is that factors about the situation tend to be more important than factors about the individual in affecting the delay in notifying the police or, at least that such factors are more easily identified. This was shown in the results from the experimental, interview, and archival studies. Situational factors which were found to play a role in delay in reporting crimes included social influence factors such as the number of others talked to and the advice offered to the victim, and factors about the crime such as the type of crime and the time of day when it was discovered.

The research also produced several divergent findings. Diametrically opposite results were obtained from the interview approach and the archival approach regarding the effect of age on reporting. More often, the divergence took the form of a significant difference obtained with one methodology which disappeared when a different methodology was employed. For example, whereas anger appeared to influence reporting in the experimental research and the existence of a suspect appeared to affect reporting in the archival research, neither variable was related to reporting in the interviews. Are such divergent findings atypical of psychological research on the criminal justice system? On the contrary, inconsistent findings seem to be the rule in multimethod research in the criminal justice system rather than the exception. Different methods have produced divergent findings with regard to the bail setting decision (Ebbesen and Konečni, 1975), the effect of a defendant's attractiveness on jurors' verdicts (Stewart, 1980;

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Efran, 1974), and the sentencing decision (Konečni and Ebbesen, 1979).

Although initially puzzling, the ultimate effect of divergent findings has been salutary in that they have led to a more careful examination of the various methodologies and the conceptual and operational definitions of the independent and dependent variables. Such closer examination has helped identify sources of unreliability (Vidmar, 1979; Weiten and Diamond, 1979). One conclusion suggested by divergent findings is that all methods should not be accorded equal weight in resolving discrepancies. Clearly, certain methods are better at answering particular questions than others (Greenberg *et al.*, 1982; Konečni and Ebbesen, 1979). For example, Konečni and Ebbesen (1979) employed multiple methods to study the bail setting and sentencing decisions. A careful assessment of their divergent findings concerning both decisions led them to conclude that the observation method is more reliable than the simulation approach when studying the bail setting decision, but is less reliable than archival data in predicting the sentencing decision. Their conclusion was based on their finding that what "goes on in court" affects the bail decision but not the sentencing decision. The latter decision was best predicted by factors not observable at the hearing (e.g., probation officer's recommendation) but which could be found in court records.

Our own divergent findings have prompted a similar re-examination. Several inconsistencies can be resolved when a distinction is made between the decision to notify the police and the delay in such notification. The conceptual dependent variable—reporting the incident to the police—was operationally defined in two different ways. In the experimental and simulation studies reporting was defined in terms of *whether or not* the police were called, whereas in the interview and archival studies reporting was defined in terms of *delay* in reporting. Thus, the fact that the degree of victims' anger correlated with reporting in the experimental studies but not in the interview study, suggests that how angry victims are may affect whether or not the police are called, but not the delay in such notification. The distinction between the two measures or reporting is also helpful in explaining inconsistent results concerning victims' age and reporting. Whereas the experimental results showed that younger victims were more likely to report their victimization, the archival data showed that they took longer in making this decision. The two findings,

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therefore, are not incompatible; they simply involve two different decisions.

The re-examination led to the resolution of yet another apparent inconsistency. In the experimental studies social influence generally facilitated reporting whereas in the interview study it was associated with longer delays in reporting. In this comparison, the experimental and interview approaches differed with regard to how the independent *and* dependent variables were operationally defined. In the experimental studies social influence usually took the form of a bystander advising the victim to call the police. The dependent variable was the decision to call or not call the police. Thus, in the experimental studies we expected, and indeed found that social influence facilitated reporting. However, in the interview study social influence was operationally defined as the number of people that victims spoke with before calling the police, and reporting was measured in terms of the delay in reporting. Using these definitions of the independent and dependent variables it is easy to see why interaction with others was associated with greater delay in victims' reporting. These analyses imply that conflicting results obtained from multiple methods might be reconciled by a closer examination of how the independent and dependent variables are operationally defined. Our analyses suggest that the decision whether or not to call the police is not the same as how quickly the call will be made.

Of course there are other ways to resolve divergent findings. As Konečni and Ebbesen (1979) have done, one can simply decide that one method yields more valid findings than another. With regard to the present data, we placed more confidence in the experimental findings than in the simulation findings concerning the ability of social influence factors to affect victim decision making. We reached this conclusion in part because the experience of the experimental participants was more phenomenologically real than the "as if" experience of participants in the simulation studies, and in part because of the clearer causal inference possible in designs involving manipulated independent variables.

While we are able to offer explanations for most of our divergent findings, there are two which we cannot explain. The first concerns differences in delay of reporting burglaries and thefts between the interview and archival studies. In the interview study, theft victims delayed longer in reporting; but in the archival study, burglary victims took longer to report

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their victimization. The second concerned the existence of a suspect. Whereas the archival analysis found shorter delays in reporting when there was a suspect, an insignificant tendency in the opposite direction was found in the interview study. An explanation for these divergent results must await further research.

Our results using the multimethod approach to study decision making by victims leads us to conclude that no single methodology has a monopoly on virtue. As we have shown, each method is better at addressing certain problems than other methods. For example, evidence of strong causal impact of bystander advice in a controlled experiment would not provide evidence that advice is frequently sought or available in the aftermath of a crime. In order to assess the availability of advice following a "real" victimization, we had to rely on data from the interview methodology. This suggests the use of a strategy in which multiple methods are employed in tandem. Findings produced by one method may lead to questions which can better be answered by a different methodology. Upon reflection, this is exactly the strategy which we have employed in the present research.

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