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Social Psychology and the Law: The Choice of Research Problems, Settings, and Methodology

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The purpose of this chapter is to examine three related issues:

1. The relative values of laboratory simulations and *in situ* research on legal decision-making.
2. The relative values of various data-collection methods, given that the *in situ* research strategy has been adopted.
3. The implications of 1 and 2 for the choice of research problems in social-psychological approaches to legal issues.

These three topics will, admittedly, be examined from the standpoint of a basic premise about the interface of psychology and the law that has motivated this entire volume; namely, that the task of using psychological methods to gain a comprehensive understanding of the operation of the legal system should take precedence—especially in the early stages of the development of legal psychology—over the testing and application of the currently popular psychological theories in legal or quasi-legal contexts.

LABORATORY SIMULATIONS VERSUS *IN SITU* RESEARCH

A close examination of the literature in legal psychology shows that a large proportion of all research studies falls into the categories of jury decision-making (Efran, 1974; Kalven and Zeisel, 1966; Kerr et al., 1976; Landy and Aronson, 1969; Mitchell and Byrne, 1973; Ne-

meth and Sosis, 1973; Sigall and Ostrove, 1975; Vidmar, 1972a), eyewitness identification (Buckhout, 1974; Buckhout et al., 1974; Doob and Kirshenbaum, 1973; Egan, Pittner, and Goldstein, 1977; Levine and Tapp, 1973; Loftus, 1975; Loftus, Altman, and Geballe, 1975), and procedural justice (Doob, 1976; Farmer et al., 1976; Lawson, 1970; Thibaut and Walker, 1975; Walker et al., 1974). Of these, jury decision-making research has been by far the most voluminous and visible (Davis, Bray, and Holt, 1977; Tapp, 1976). Perhaps more than 90% of the research studies in all of these areas, especially in jury decision-making, has been conducted in the laboratory (Bermant et al., 1974; Davis, Bray, and Holt, 1977; Konečni, Mulcahy, and Ebbesen, 1980; Tapp, 1976). In view of this state of affairs, the relative utility of laboratory simulations and naturalistic research for the success of efforts to reach a sound understanding of the criminal justice system and process should be closely examined.

Many of the reasons that laboratory studies can cause serious problems with regard to the generalizability of findings are well known and need be only briefly mentioned here. For example, the researchers' implicit claim that college students can successfully mimic the responses of the participants in the real-world legal system has been frequently criticized (Miller et al., 1977), as has the fact that laboratory subjects' behavior and decisions have no real consequences (Ebbesen and Konečni, 1980; Wilson and Donnerstein, 1977), unlike decisions made in the real world. Another frequent criticism has been that the materials presented to the subjects in the laboratory vastly oversimplify the kind and amount of information to which the participants in the real-world criminal justice system are exposed, and that the stimuli and stimulus dimensions are typically presented in decomposed rather than more wholistic form, which is typical of the real world (Ebbesen and Konečni, 1980; Gerbasi, Zuckerman, and Reis, 1977). Another frequent and obvious criticism, directed particularly at jury-simulation studies, has to do with the fact that laboratory subjects' decisions are often made in the absence of key procedural features that characterize decision-making in the criminal justice system, such as, for example, the absence of the discussion and deliberation stage in which actual juries engage, the absence of a foreman, etc. (Izzett and Leginski, 1974; Myers and Kaplan, 1976; Vidmar, 1972b). A related criticism has to do with the nature of the dependent measures that are often used in laboratory tasks (Ebbesen and Konečni, 1976; Konečni, Mulcahy, and Ebbesen, 1980). For example, laboratory juries frequently judge the extent of guilt of the defendant on a scale, whereas their real-world counterparts have to make a dichotomous decision. Similarly, laboratory jurors are frequently asked to determine the fictitious defendant's sentence, even though in many states the judge, and not the jury, does this (see

Table 5 in Bray, 1976). Moreover, laboratory jurors are forced to set prison terms for types of crimes that in the real world almost never result in a prison sentence (Ebbesen and Konečni, 1976).

If one considers the possible motivations of the researchers who do laboratory experiments in which some or all of the features criticized above are present, one is inevitably led to the conclusion that either they are not truly interested in understanding how the criminal justice system operates or they believe that a correct understanding of the functioning of the system and the behavior of the participants in it can be obtained regardless of the subjects, the consequences of the decisions, the materials and information presented to the subjects, the decision alternatives at the subjects' disposal, etc. A considerable amount of evidence suggests that the latter view is naive and untenable because subjects' decisions are generally quite *task-specific*. Since this appears to be true not just in experiments concerned with legal issues and decisions, but in many different kinds of experimental tasks, the discussion that follows is concerned with the problem of task specificity in decision-making research in general (Ebbesen and Konečni, 1980).

It is instructive to consider first the conclusions that are currently being drawn from the laboratory simulations of various types of decision-making. Humans emerge as intellectual cripples, biased by cognitive processes that interfere with rational decision-making (Dawes, 1976; Slovic, Fischhoff, and Lichtenstein, 1976). They are oversensitive to variables not included in normative models (Kahneman and Tversky, 1972) and undersensitive to variables that are (Kahneman and Tversky, 1973). They become more variable when given more information (Einhorn, 1971; Hayes, 1964) and increase their confidence in the accuracy of their judgments when they should not (Kahneman and Tversky, 1973; Slovic and Lichtenstein, 1971).

If one eliminates the derogatory tone of these conclusions, a simple statement remains suggesting that decision-makers often seem to be responsive to task characteristics that are incidental to, and not specified by, prior theoretical conceptions (Olson, 1976) and, more importantly, researchers do not know when such oversensitivities will occur. In some tasks certain variables have smaller effects than expected; in other tasks the effects are larger than expected. Put differently, there are no theories that can tell us when people will be Bayesian, when they will average, when they will add, when they will be sufficiently sensitive to characteristics of data samples, etc. It is quite unclear which features of tasks control when and which of these many different processes will have causal effects on decisions.

Given the state of affairs described above, it seems to make more sense to assume that subjects *create* decision rules and processes specific to each particular decision task than to try to develop ever

more elaborate theoretical models that take into account the broad range of task-specific behaviors. If the former view is adopted, one would not be surprised to find that the substantively irrelevant aspects of a task or of a measurement procedure would have an effect on the results and that the factors, such as the features of the material presented to the subject, the consequences (or lack of them) of the decisions, the order in which the information is presented, whether or not the stimuli are presented in decomposed form, whether or not a subject knows that the material comes from a fictitious legal case, the number of times a decision is made, the response scales used, the presence versus absence of a deliberation stage, the amount of time available for making a decision, and so on, might well substantially affect subjects' decisions.

If subjects indeed create decision strategies to fit various elements of a task, how can one place any confidence in the information allegedly obtained about the legal system from the ubiquitous laboratory simulations? For example, it is not unreasonable to expect that a student jury given two or three bits of information about a case on a piece of paper may well react to these bits in quite a different way than a real jury does to information about similar issues in a trial. Trivial features, such as the number of words required to describe, say, the defendant's credibility (or levels thereof) and family history, respectively, may determine which of these will be given greater weight by the student jurors. In a real trial, information is presented over much longer periods of time, by different participants, and impressions presumably jell gradually. The presentation of some types of information (e.g., family history) may take far less time in a trial than that of others (e.g., the defendant's credibility), but the latter bit of information might be presented less explicitly than the former. Note also that presenting information in decomposed form to subjects automatically eliminates a major decision-making task that real-world jurors have to face—that of *extracting* information from the ritualistic and often incomprehensible goings-on that a typical trial involves, and of deciding how to evaluate the various bits of information.

Similarly, finding oneself in a $2 \times 3 \times 4$ within-subjects simulated-jury experiment and making the guilty/not guilty decision 24 times in a row within 10 minutes on a 100-mm scale are clearly somewhat different from being on a jury once in a lifetime, watching a 7-day trial, and deliberating for 2 days behind closed doors with eleven complete strangers. A picture of the "defendant" appended to the sheet that gives other information about the "case" (which is how the variable of the defendant's "attractiveness" is typically manipulated in psychology/law experiments) may place too much or too

little (Who knows?) emphasis on the defendant's appearance (in comparison to a real trial), but it would seem more than plausible that the subjects exposed to the information in this way would respond to it *differently* than would real jurors to a live defendant. Note that we are not arguing that the real-trial procedures are better, more rational, or more conducive to the advancement of justice than are the laboratory procedures. It is simply that the laboratory experiments are presumably attempting to simulate the real-world legal process and decisions, and not the other way around.

A few of the above criticisms lose some of their force if one believes that the real world is "additive," i.e., that factors occurring in it have only main effects and do not interact with each other. However, a more plausible view of the world is that it is highly "interactive" (Cronbach, 1975). The high frequency of findings of interactions in psychological, especially social-psychological, experiments supports this view. Sometimes the interactions are between the major factors under investigation, but as often as not they fall in the category of "context effects"—an umbrella term that subsumes the interactions between the major factors under investigation and certain aspects of the research setting, the experimental task, the particular confederate used, the time of day, and a myriad of others. Thus, it would seem that the inference that because a particular factor has a particular main effect in a laboratory experiment it would have a similar effect on the real-world decision in an entirely different setting, with different participants, is probably quite suspect. Moreover, quite apart from the "interactive-world" idea, the above criticisms are nevertheless valid from the standpoint of the "percent-of-variance-accounted" argument discussed in Chapter 1 and later in this chapter.

One could also argue that some simulations are better than others and that many of the problems mentioned can be avoided by conducting "good" simulations. However, to the extent that a simulation is trying to discover something about the operation of the real-world legal system—a goal that we heartily endorse—how can one know whether a simulation is "bad" and which of several simulations is the "best," without actually collecting the data in naturalistic settings, that is, in the real-world legal system? If one accepts the view that on logical grounds only a real-world study can validate the results of the simulation, it only makes sense to begin the research program by doing real-world studies (especially in a young and largely unmapped discipline), and that in situations where there are limited funds, time, and manpower—frequently encountered in the social sciences—the choice as to which type of study to do is obvious.

What should one do in situations where real-world research cannot be carried out? For example, many aspects of the legal system are

confidential. It is impossible for researchers to be present during jury deliberations, and it is extremely difficult to obtain access to files containing information that leads to certain decisions (e.g., the prosecutor's files). Many would probably think that simulation research in these cases is fully justified even if all of our criticisms are correct. A more cautious point of view, and one that we favor, is that erroneous information obtained by scientific methods (and therefore having an aura of truth) is more harmful than no information at all, especially when issues as sensitive as legal ones are being dealt with and when people's futures are quite literally at stake.

An important argument against the point of view espoused here should be considered next. Much of the evidence against the use of laboratory simulations comes from real-world studies based largely on *correlational* data. Therefore, it could be argued that the discrepancies between laboratory and real-world studies are due to the inability to tease apart real from spurious causal relationships in the real-world data (Phelps and Shanteau, 1978). However, it could be reasonably maintained that *all* decision models, whether based on data from simulations or on observations of real-world events are, in fact, only *paramorphic representations* (Hoffman, 1960) of the *actual* decision processes of the subject (whether the subject is a judge or a sophomore in an experimental situation). Models merely *simulate*, i.e., are correlated with, the input–output relationships that are observed (Payne, Braunstein, and Carroll, 1978).

In addition, experiments do not eliminate the possibility that causal relationships other than those proposed as explanations may be producing the results. The fact that randomization generally breaks the correlation between one variable and all prior variables has no implications for the correlations between that one variable and all *following* variables. A manipulation might create many mediating variables and processes each of which, individually or in combination with others, might play a causal role in a final decision (Costner, 1971). Because these mediating processes might be correlated with each other, one winds up in a similar position to the researcher dealing with real-world correlational data. The best one can hope is that the models one develops will describe and predict patterns in the data.

Finally, it should be pointed out that various statistical techniques for estimating causality from correlational data—such as path analysis and other types of causal analysis—are being continuously refined and made increasingly sophisticated (Blalock, 1971; Heise, 1975; Mayer and Arney, 1974).

We realize that some of our remarks may lead to the accusation that we are preaching scientific nihilism. After all, if laboratory tasks

create specific decision processes rather than tap some basic ones, then why not assume that real-world tasks also create just as task-specific decision strategies? We agree with the latter point, but disagree that nihilism is the consequence. It seems to us that in the area of legal decision-making, as well as in many other types of decision-making, the really important truths are to be found in the real world rather than in laboratory simulations, no matter how high the *face* validity of the latter might be. We would prefer to base our conjectures about how people make various types of decisions on observations of actual people making actual decisions. Moreover, even if real-world judges' decision strategies do change when certain features of their real-world legal task change—for example, because of administrative or legal modifications—such changes merely reflect the reality of decision-making in actual courts. Quite another matter are changes in decision strategies that are brought about by scientists' often arbitrary decisions to change this or that feature of the laboratory task. Such changes typically have no substantive, let alone practical, importance or relevance, and their effects on subjects' decision strategies are therefore of minimal interest.

In addition, we are not arguing that laboratory simulations should be abandoned altogether. There are conditions in which they might serve as useful tools in teasing apart further questions about the real-world process. However, rather than assume that the simulations are good, one ought to collect sufficient evidence to test whether the constructed tasks have captured the necessary detail of the real world to be *real* simulations.

Because of the various considerations described above, we have, in our own work, always collected data from actual cases and from the real-world participants in the criminal justice system. In many projects, we have also conducted simulations, and it was in part the discrepancies between simulation and *in situ* studies (which will be discussed in detail in our chapters on bail setting and sentencing) that led to our skepticism about the utility of laboratory simulations for studying the legal system and to our decision, in editing this book, to give precedence to researchers who have obtained data in real-world legal settings.

THE CHOICE OF METHODOLOGY IN NATURALISTIC SETTINGS

The decision to collect data from the participants and/or in settings within the criminal justice system by no means resolves all problems regarding generalizability and external validity. For example, the de-

cision to go to judges' chambers and conduct interviews about the factors that affect their sentencing decisions may lead to conclusions about the causes of sentencing that are quite incorrect. The judges may deliberately try to mislead the interviewer for self-presentation or political reasons, or may be quite unaware of the factors that they are actually taking into account in sentencing. Similarly, sitting at the probation/sentencing hearing and meticulously coding the goings-on, as well as the sentence that is imposed, may lead to similarly erroneous conclusions, because the information that best predicts judges' behavior may be contained elsewhere (such as in the probation report) and never surface in the hearing itself. Thus, neither the decision to deal with the actual participants in the legal system nor the decision to collect data in the actual settings guarantees to any extent that the findings will lead to the discovery of the real causes of a participant's behavior and therefore have external validity.

In attempting to cope with these problems in our own research, we have been guided by several considerations. One of the steps we took in almost all of our projects in both the criminal and the civil areas of legal decision-making (e.g., on bail setting, sentencing, police decision-making, prosecutorial decisions, personal-jury and child-support decisions), many of which are not reported in this book, was to use more than one research method, thus generally following the Campbellian tradition (Campbell and Stanley, 1966; Webb et al., 1966). When using a multimethod approach, one's confidence in a conclusion to which all of the various methods lead is, of course, much greater than if only one method had been used. Unfortunately, in our work, the results from the multiple methods have seldom led to a single, common conclusion. When this happens in other areas of research, there is a deadlock that cannot typically be resolved by applying a priori and logical criteria. However, we believe that when one studies an intact, functioning social network—such as the criminal justice system—there are certain logical and practical criteria that would lead one to trust the conclusions reached by one method over those reached by another on a priori grounds, with the important proviso that the researcher is interested in how the system actually operates, rather than in the phenomenology of the participants.

Let us suppose that a particular legal decision has been examined in a variety of ways including:

1. An elaborate interview with the participant making the decision.
2. A lengthy questionnaire that the participant fills out.
3. The observation and coding of the public hearing (using the customary time-sampling observational techniques that code the behavior, appearance, and other characteristics of all the

participants present; the order in which, and to whom, they speak; the issues that are brought up in the hearing, and by whom; etc.) at which (and, ostensibly, as a function of which) the relevant decision has been made.

4. An archival analysis, i.e., coding of at least two kinds of written materials (to the extent that they exist): the transcript of the hearing and the file containing a variety of documents pertaining to the case that is available to the key decision-maker.

In this situation, if the various methods were to lead one to different conclusions, we would be inclined to trust the archival analysis of the documents available to the decision-maker more than any other method, provided that this analysis had most of the following characteristics:

1. The coding categories used are similar to those used by the participants in the real-world system, rather than derived from the currently popular social-psychological theories. This typically means that the coding categories will be concrete and low-level, as opposed to abstract and high-level. An example would be the coding of a category "prior record" in terms of the actual number of prior felony convictions, rather than coding "consistency of prior criminal behavior" (a more abstract concept derived from attribution theory) on a 5-point scale. Such a procedure makes the coding more reliable and also facilitates the communication of the research findings to the participants in the legal system—if one's goal includes producing change in the system. Moreover, having done the initial coding in terms of very concrete, low-level categories, one can always subsequently collapse across these categories (or levels within a category) to achieve a more abstract classification.

2. Coding is as exhaustive as possible, covering as much information in the written materials in the file as possible, so that initially a very large number of predictors (i.e., coding categories) is isolated. This step, of course, minimizes the likelihood that an important predictor will be omitted from the analysis.

3. The statistical analyses examine the effects on the criterion decision of various combinations of predictors, with a relatively large number of predictors in each "predictive set," so that both main effects and interactions can be captured.

4. Prior to archival analyses, a sufficient amount of background research had been done by the investigators concerning the actual, routine, day-to-day operation of the system so as to leave

no doubt that the file being coded is, in fact, at the disposal of the decision-maker prior to the time when the decision is being made. Note that whereas it is important to demonstrate that the decision-makers *could* have seen a particular bit of information in the file, it is not necessary to demonstrate that they have actually done so, especially for every case. It may well be that the bits of information that are being coded are correlated with other bits of information at either the same or a higher level of abstractness and that the decision-makers are actually attending to these other bits of information as they examine the file. This, however, in no way precludes that the *coded* categories are treated as "true predictors." (In fact, one could argue that even if the decision-makers do not see a file, a predictor isolated from the file that accounts for a very large percent of the variance in the decisions could be considered a "true cause.")

Given that such precautions have been taken, we would trust the conclusions based on the archival methodology more than those based on other methods for a number of related reasons. First, the use of both the interview and the questionnaire for studying decision-making in the legal system is highly suspect because the participants in the system (especially judges, assistant district attorneys, police officers, parole officers, and probation officers) may well subjectively believe—in line with their deeply ingrained view that "every case is complex and unique"—that they are responding to various multifaceted and complex aspects of the case and combining these many bits of information in a complex, configural manner (amply aided by their judicial training, experience, skill, and wisdom), when in fact they may be responding to very few bits of information, combined in a simple manner. Thus, whereas the content of the decision-maker's minds as they are pondering the decisions—at least as revealed by interviews and questionnaires—may be highly complex, very few mundane and simple predictors may account for a large percent of the variance in the output decisions. In fact, because of what may be called "judicial evaluation apprehension," the interview and the questionnaire may be poor methods *even* when one is interested in the phenomenology of these decision-makers (rather than in factors that actually predict their decisions). To take a hypothetical and extreme example, racist judges may well *want* to systematically give harsher sentences to defendants of a certain race, and also be *under the impression* that race is one of the major factors in their sentencing decisions; yet they would be very unlikely to admit this in an interview or a questionnaire. The final twist would come when one found out, upon examining 300 cases in which a racist judge had passed

sentence, that other factors, say, prior record and severity of the crime, accounted for 96% of the variance, whereas race accounted for less than 1% (even if the direction were "predicted" by the judge). In other words, the questionnaire and interview responses may not reflect what judges think or intend; moreover, even if they do, neither the judges' private responses (intentions and thoughts) nor the public ones (questionnaire and interview responses) may affect what they actually do!

Second, coding archival materials has an advantage over observing and coding hearings in that:

1. More predictors are typically available in written materials.
2. The nature of the two research situations is such that greater coding reliability can be obtained in the archival case (because of time and other pressures in observational research).
3. Archival research is less obtrusive (although this does not necessarily always have to be the case).
4. When one examines the system as a whole, it is clear that written materials accompany a defendant through the system; therefore, the predictor of a particular decision that is discovered in the written materials is also more likely to be the predictor of many subsequent decisions by other participants in the system, by virture of the same piece of paper (such as the "rap sheet," the prior record of the defendant) being a part of the case at almost every node in the system.

Third, perhaps the greatest value of the archival approach is that when archival materials are a routine part of the procedure, they are typically very detailed and contain a large number of bits of information. For example, a probation report contains many types of information about the defendant's history that will never emerge in any kind of hearing or trial, *in addition to* most of the information that is available from hearings (e.g., the defendant's sex, age, and other demographic characteristics).

Finally, a potentially considerable benefit of archival materials is that certain predictors that can be isolated from the files *temporally* precede the location of other predictors in the causal chain. Occasionally, this means that the best predictor of a particular legal decision may be a factor available in the files (and thus one that can be discovered by both researchers and participants in the system) even *before* the offense—about which the various decisions in question are subsequently made—is even committed! For example, in a study

of the processing of mentally disordered sex offenders (MDSOs) in California (Konečni, Mulcahy, and Ebbesen, 1980), we found that the convicted offender's prior sex-related criminal record almost inevitably led the court-appointed psychiatrists to diagnose and classify the offender as "sexually deviant" and an MDSO, respectively, which, in turn, resulted almost automatically in the judge's verdict that the defendant be sent to a mental hospital (rather than be remanded to the trial court for sentencing). In other words, the offender's prior sex-related criminal record is an excellent predictor of both the final and the intermediate decisions, and this information is known even before the offense under consideration has been committed. The psychiatric diagnosis and classification, and the judge's verdict, may be correlated with many other predictors, but the simplicity and temporal primacy of the prior sex-related criminal record forces other predictors into the role of epiphenomena. For example, differences in the content of probationary reports and psychiatrists' letters can be considered as merely serving to justify an already formed conclusion based on the prior sex-related criminal record, in order to give the appearance of complexity to the processing of MDSOs and to smooth out the rough edges of the causal sequence.

Although this particular example may be somewhat extreme, the fact remains that the archival method has several apparent advantages over other methods, not the least of which is that the meticulous coding of the defendant's file may yield factors capable of predicting the outcome of a case very early in the process.

THE CHOICE OF RESEARCH PROBLEMS

The choice of research problems clearly depends on many considerations, among the most important of which are theoretical and methodological concerns. The message we tried to convey in the previous chapter on theoretical issues and in the present one on methodological issues can be summarized as follows:

1. In a young and largely unmapped discipline, such as legal psychology, it makes sense first to pay attention to understanding, at least in general terms, the operation of the system that is the bread and butter of the discipline, namely, the legal system, rather than worry in these initial stages about issues and problems imported from other disciplines.

2. *In situ* research, especially of the archival variety, seems to be superior to other approaches when the primary goal is to un-

derstand the operation of an intact, functioning social system that one cannot experiment with at will.

3. Given limited resources and time, the potential pressures from the impatient public and legislatures, and a need for well-documented, data-based change, the particular decisions made by participants in the criminal justice system that should be especially attractive for research seem to be those that account for the *greatest percent of variance in the processing of cases through the system as a whole.*

In fact, one could argue that the first two points reduce to the third and that percent of variance accounted for is the key factor in our analysis of the sort of research problems one might choose. After all, the decision to try to understand the functioning legal system (as opposed to, for example, the development of norms and moral judgment in preschool children) clearly has a bearing on the amount of variance in the output of the legal system that one is going to understand. Similarly, certain methods and approaches may be better than others in *correctly* identifying the predictors of certain decisions and thus contributing to the percent of variance explained.

The percent-of-variance argument (also discussed in Chapter 1) and its relationship to the choice of research problems can perhaps best be illustrated by reference to some data concerning real-world legal decisions. Figure 2.1 presents the data for the processing of criminal cases in San Diego County, California, for the years 1976 and 1977. Several things should be noted about this figure. First, the general pattern of the data for San Diego County is quite similar to the federal data presented in Figure 1.1 and the California state data in Figure 1.2. Second, the figure shows that for a full 63% of all felony arrests, a felony complaint is not filed. Third, of the 19% convicted of a felony, 78% were convicted because of a guilty plea, whereas only 6% had a jury trial. Finally, of all the felony arrests in San Diego County in 1976–1977, only 1.2% had a jury trial.

Many other statistics could be extracted from Figure 2.1 that challenge both the popular view of the operation of the criminal justice system (myths perpetuated both by the entertainment industry, through the Perry Mason types, and by the news media, through their selective coverage that focuses on sensationalist or unusual cases) and the wisdom of the overall research effort in legal psychology, where a very high percentage of studies is devoted to decisions (such as jury verdicts) that occur extremely rarely and thus account for a very small proportion of the total variance in the processing of criminal cases.

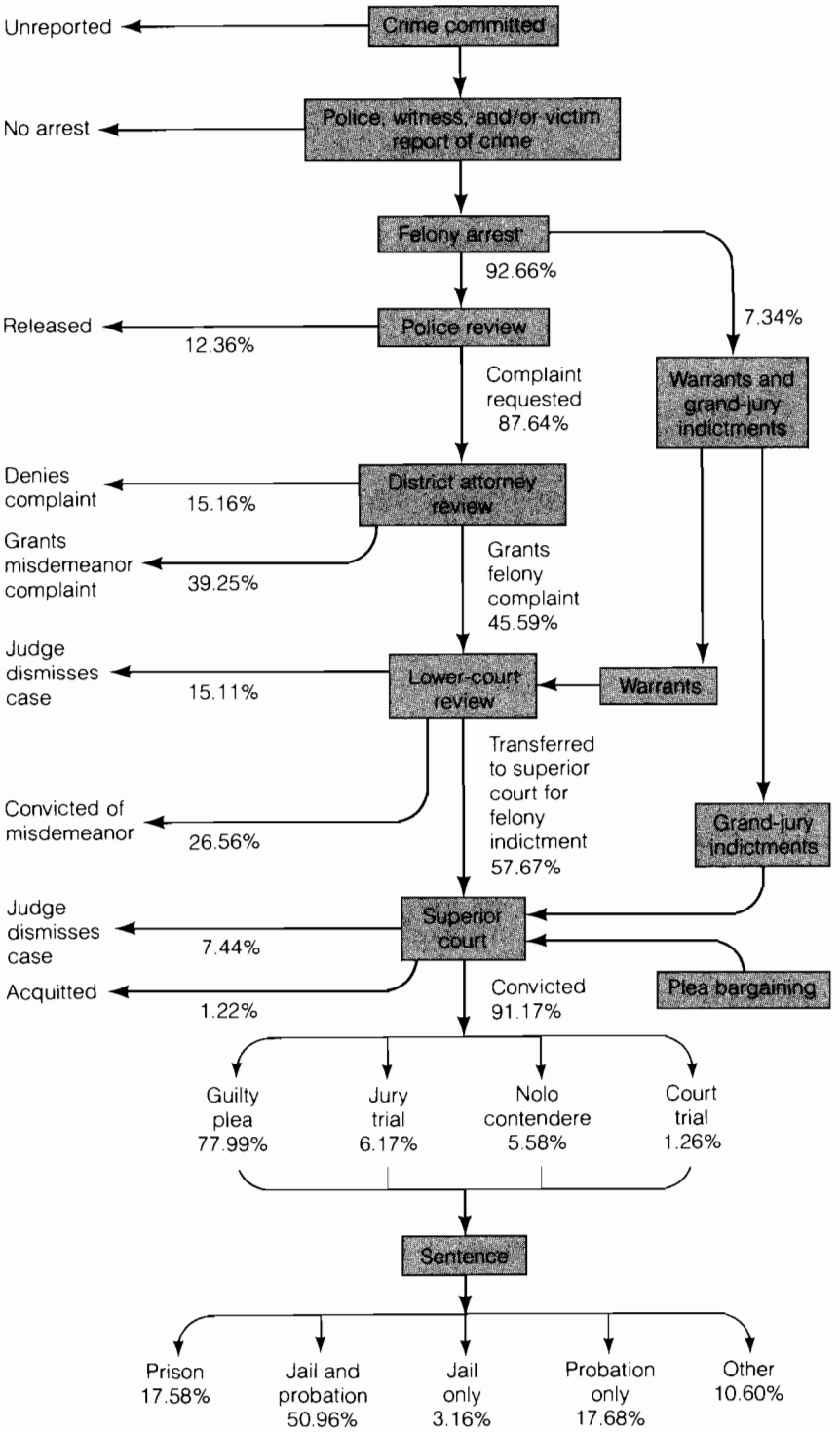
Unreported
Crime committed
No arrest
Police, witness, and/or victim report of crime
Felony arrest
92.66%
7.34%
Released
12.36%
Police review
Complaint requested 87.64%
Warrants and grand-jury indictments
Denies complaint
15.16%
District attorney review
Grants misdemeanor complaint
39.25%
Grants felony complaint 45.59%
Judge dismisses case
15.11%
Lower-court review
Warrants
Transferred to superior court for felony indictment 57.67%
Convicted of misdemeanor
26.56%
Grand-jury indictments
Judge dismisses case
7.44%
Superior court
Acquitted
1.22%
Convicted 91.17%
Plea bargaining
Guilty plea 77.99%
Jury trial 6.17%
Nolo contendere 5.58%
Court trial 1.26%
Sentence
Prison 17.58%
Jail and probation 50.96%
Jail only 3.16%
Probation only 17.68%
Other 10.60%

Figure 2.1
Case-processing data for 1976 and 1977 in San Diego County, California. Approximately 24,000 adult felony arrests were made during this two-year interval. The percentages show how these cases were disposed of at each decision node.

In a sense, Figure 2.1 essentially maps out both the system and the problems to be studied in that it identifies the key decision points. These could be viewed as the major "bifurcation points" regarding whether or not a case remains in the system or drops out. A simple rule of thumb would be that the closer a node is to a 50/50 split (that is, 50% of cases being passed on to the next node in the felony-processing system, and 50% exiting the system either altogether or with charges reduced to a misdemeanor). the more important that decision is and the more intensely it should be studied—if one's goal is to understand the operation of the system.

On the basis of these considerations, it would seem to us that there is a real need to spread research efforts more evenly throughout the system and away from certain heavily "overpopulated" nodes (such as jury research); this is simultaneously one of the major purposes of this book.

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