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REDUCING DISPARITY IN JUDICIAL
SENTENCING: A SOCIAL-
PSYCHOLOGICAL APPROACH

SIEGFRIED LUDWIG SPORER

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PSYCHOLOGICAL APPROACH

University of New Hampshire

PH.D.

1980

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REDUCING DISPARITY IN JUDICIAL SENTENCING:
A SOCIAL-PSYCHOLOGICAL APPROACH

BY

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A DISSERTATION

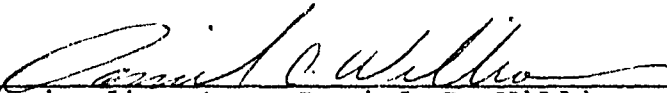
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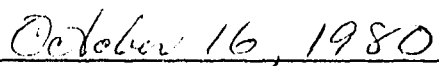

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To the memory of my father, Ludwig Sporer, who instilled my social conscience, and to those who suffered unduly in the hands of the criminal justice system.

JUSTITIAE CAUSA

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ABSTRACT

REDUCING DISPARITY IN JUDICIAL SENTENCING:

A SOCIAL-PSYCHOLOGICAL APPROACH

by

SIEGFRIED LUDWIG SPORER

University of New Hampshire, 1980

Researchers from diverse disciplines--e.g. sociologists, criminologists, political scientists, legal observers, and most recently also psychologists--have studied judicial sentencing. More than half a century of research, employing multiple methodologies, strongly demonstrates that the unguided use (or abuse) of discretion has frequently led to vast disparity in judicial sentencing; i.e. large variations among sentences given for highly similar offenses and/or offenders. Among the various reform proposals reviewed, sentencing councils have been suggested as a constructive solution to reduce sentencing disparity without abandoning judicial discretion or displacing disparity to other agents in the criminal justice system.

The present research attempts to demonstrate the disparity reducing effect of discussion in sentencing councils in a controlled laboratory setting. From a social psychological perspective, sentencing a criminal offender

can be conceptualized as judging an ambiguous stimulus object. Convergence toward the mean of the sentencing decisions (reduction of variability) was predicted as a function of group discussion in line with theories of norm formation processes (e.g. Moscovici, 1974; Sherif, 1935, 1936; Sherif & Sherif, 1969). Discussion of goals of judicial sentencing was expected to further reduce variability. Also of interest was whether or not writing down the sentencing decision prior to discussion would make judges less susceptible to group influence processes. In line with the polarization hypothesis (Moscovici & Zavalloni, 1969; Myers & Lamm, 1976; Lamm & Myers, 1978), penalty shifts (leniency or severity) were also predicted as a function of group discussion.

Extending the Solomon-four-group design, this study crossed three levels of the council factor (no council, council, and extended council) with two pretest conditions (no pretest and pretest). College students (N = 277) simulated mock judges who were presented a description of a case of armed robbery. As predicted, variability within councils was considerably less as a function of discussion in three-member councils and this effect was stronger for mock judges who had not written down their sentences than for those who had committed themselves that way. There were no differences in within council variability between the council and the extended council condition but somewhat less overall variability was observed in the extended council

condition as compared to the no-council condition. There was no evidence for leniency and/or severity shifts.

The reduction of variability findings provide strong support for the postulation of norm formation processes as a function of group discussion. The observed commitment effect, and its absence in the no-pretest conditions, are also compatible with this interpretation. Contrasting traditional inductive approaches to validity with more recent deductive approaches, the applicability of these theoretical principles to real world sentencing councils is argued. The implementation of sentencing councils on a trial basis, in which council members do not write down their sentences before discussion, is recommended. Sentencing councils would be expected to reduce sentencing disparity to some extent but would have to be supplemented by other structural and procedural innovations that could also be investigated within the theory-testing interventionist approach promulgated here.

CHAPTER I

SENTENCING DISPARITY: THE KEY PROBLEM IN JUDICIAL SENTENCING

The Need for Studying Judicial Sentencing

Historically, back to the earliest stages of psychology as an independent discipline, there has been a mutual interest of legal scholars and psychologists in each others' disciplines. Legal scholars have looked to psychology for answers they regarded to be of a psychological nature, and psychologists have been eager to analyze some of the legal issues of interest to them. Periods of enthusiasm have given way to periods of disillusionment, and vice versa. Throughout these periods of interaction, the scope of the type of issues being addressed has always been restricted to a relatively narrow set of topics (see, for example Arntzen, 1980; Gross, 1908; Munsterberg, 1907; Tapp, 1976; Undeutsch, 1967).

Only most recently has there been an upsurge in research at the interface of psychology and the law that has exploded the topical boundaries of the past. Continually more issues are being tackled by psychological investigators, especially within the criminal justice system (for a plethora of examples see Bermant, Nemeth, & Vidmar,

1976; Friedman & Macaulay, 1977; Marshall, 1980; Saks & Hastie, 1978; Sales, 1977; Shaver, Gilbert, & Williams, 1975; Tapp & Levine, 1977; Thibaut & Walker, 1975).

Collaborative efforts of the two disciplines are becoming institutionalized through the foundation of journals and interdisciplinary graduate programs, conferences, publications, and research funding (Tapp, 1976).

Despite this massive extension in scope, research efforts are not equally distributed over the many issues to which psychologists could contribute. For example, there is an abundance of research on one of the key institutions of the American legal system, the jury, but relatively little effort is spent on the role of other key figures in the courtroom, such as the prosecutor and the trial judge (Saks & Hastie, 1978; Shaver et al., 1975). The trial judge holds a central position in trial court (Frankel, 1972; Saks & Hastie, 1978), and it is his/her decision-making process of arriving at a criminal sentence that is the focus of this investigation.

The sentencing behavior of the judge is the most significant of his/her activities, and the reasons for studying this decision-making process have been succinctly summarized by Sutton (1978a):

There can be little doubt that, however justifiable, the imposition of criminal sentence is one of the most substantial intrusions the State can effect upon individual liberty. Judges are given nearly unparalleled discretion over the lives of millions who come before them each year. Bounded only by the broadest of statutory

constraints, the sentencing judge is generally empowered to exact penalties ranging from little more than verbal reprimand to life imprisonment or even death. Although not all offenders face such a range of sentences, sentences actually imposed for each offense category tend to cover the statutorily allowed range. That discretion of such formidable consequence should be authorized--indeed, that it should be exercised so variably--should provide not only a justification but a compulsion to examine the manner of its use.
(p. 1)

Judicial Sentencing, Discretion, and Sentencing Disparity

The decisional power of the judge is inherent in his/her exercise of discretion that allows him/her to arrive at decisions in a free and independent manner, only governed by his/her conscience and the rules and spirit of the law (Shaver et al., 1975). This vast opportunity at discretion has led to an often deplored disparity among judicial sentences, i.e. large variations among judges in deciding upon highly similar offenses and/or similar offenders have been frequently observed and amply documented by legal scholars, political scientists, sociologists, and psychologists (for reviews, see Austin & Utne, 1977; Austin & Williams, 1977; Bullock, 1961; Chiricos & Waldo, 1975; Cole, 1973; Dershowitz, 1976; Hagan, 1974; Hogarth, 1971; Hood & Sparks, 1970; O'Donnell, Churgin, & Curtis, 1977; President's Commission on Law Enforcement and Administration of Justice, Task Force Report: The Courts, 1967; Sporer, 1978; Sutton, 1978a; Zimmerman, 1976).

In light of such massive evidence--some more and some less conclusive--it is again and again surprising to find investigators who feel a need to establish the existence of disparity de novo after half-a-century of research on this issue. Of course, many of the studies, particularly some of the earlier ones, are fraught with methodological problems and subject to rival explanations (critically, see Austin & Williams, 1977; Chiricos & Waldo, 1975; Diamond & Zeisel, 1975; Hood & Sparks, 1970; Sutton, 1978a; Zeisel, 1969). But the very fact that sentencing disparity has been studied with the whole gamut of the methodological armamentarium, ranging from collection of anecdotal evidence, case histories, in-depth interviews, questionnaire surveys, and large scale archival studies to controlled experimental simulations, increases our confidence in the existence of this phenomenon on the basis of the mutual corroboration achieved through triangulation and multiple operationism (Diamond & Zeisel, 1975; generally, see Campbell & Stanley, 1966; Cook & Campbell, 1979; Webb, Campbell, Schwartz, & Seechrest, 1966; Zeisel, 1969).

This countdown on sentencing disparity should not be mistaken as a countdown on judicial discretion, or a call for its abolition. To be sure, the issue is not one of whether or not to allow discretion at all. Discretion is at the heart of our criminal justice system (Davis, 1969; Shaver et al., 1975; critically, American Friends Service Committee, 1971). It is considered essential to guarantee

flexibility in a penal system that attempts to make punishment fit not only the crime but also the criminal. The concern is with abuse of discretion. This abuse of discretion runs counter to the constitutionally guaranteed right to equality of treatment before the law (cf. Rubin, 1966; Zimmerman, 1976). The judge's function is then to navigate between the Scylla of inequality and the Charybdis of rigidity and flexibility.

The evidence accumulated above indicates that the pendulum of judicial decision-making has swung too far to the side of unrestrained discretion and resulting inequality, and that it is time to strike a better balance again.

Understanding Sentencing Disparity:

Its Roots in the Statutory Sentencing Framework

At the very heart, sentencing disparity is a function of unguided judicial discretion in the absence of "substantive control or guidance" (Kadish, 1962). The possibility of abuse of discretion is inherent in the judge's power to choose among a great variety of sentencing options virtually without legal guidance or control. As O'Donnell et al. (1977) have observed:

The lack of "substantive control or guidance" for sentencing judges is evident in Title 18 of the United States Code, which contains most of the federal criminal statutes and penalties. Bereft of sentencing standards for judges, this chaotic patchwork of penalties authorized by individual statutes, enacted at different times and having no

relationship to each other, has established a bizarre range of penalties for an enormous variety of criminal activities.(p. 1, notes omitted)

This chaotic state of affairs observed at the federal level is reflected, and probably exacerbated, at the state level (Frankel, 1972). Consider for a moment the vast array of sentencing alternatives, subject of regional modifications, a sentencing judge can choose, including combinations thereof (cf. President's Commission on Law Enforcement and the Administration of Justice, 1967):

He/she can decide

- whether or not to impose a fine;
- the magnitude of the fine;
- whether to incarcerate the offender or put him/her on probation
- the maximum length of the prison term;
- the minimum length of the prison term (parole eligibility):
- the length of the probation sentence;
- whether or not to sentence the offender under the Youth Corrections Act, or similar provisions for subpopulations of offenders;
- whether or not to commit the offender for observation and study, or psychiatric examination etc.;
- whether or not to suspend a sentence.

This wide range of sentencing alternatives provided by statutory sentencing frameworks of the individual states allows for maximum flexibility of the judge to fit the

punishment to the individual offender and the circumstances of the crime. This individualized treatment model of sentencing had its historical origins in the rise of the rehabilitative ideal during the post-Civil War period, and progressed through the adoption of indeterminate sentencing laws by many states in this country (for a review of the historical development, see Dershowitz, 1976; Rothman, 1977). Until most recently, almost all the states had some form or another of a mixed sentencing model which, in essence, is a compromise between legislatively fixed, judicially fixed and administratively fixed sentencing models (Dershowitz, 1976).

For example, a sentencing judge who is to decide a felony case (e.g. armed robbery) for which the legislature has provided a sentencing range from seven-and-one-half to fifteen years in that particular state may sentence the defendant to eight years minimum to twelve years maximum of imprisonment, leaving the ultimate decision of release of the defendant to the parole board. The parole board, in turn, may schedule the first parole meeting after about five-and-one-half years, i.e. the minimum sentence minus the amount of "good time" credit which may be forfeited as a function of misbehavior in prison (see American Bar Association Commission on Correctional Facilities and Services, 1974). Thus, a substantial amount of discretion is vested in the decisional power of both sentencing judge and the parole board, and consequently the possibility of

disparate outcomes ensues at both institutional decision points. Although the present study focuses on the discretionary power of the sentencing judge, it will be wise to keep in mind that the judge's decisions are being made within the court as a system (e.g. Saks & Hastie, 1978). Therefore, they are both dependent upon, and determinants of the actions of other criminal justice agents, especially the prosecutor and correction officials (e.g. parole boards).

Unguided discretion of judges and/or parole boards had its heyday under indeterminate sentencing laws, e.g. in California, and the subsequent disillusionment with the individualized treatment model and rehabilitation outcomes (e.g. American Friends Service Committee, 1971; Fogel, 1979; Martinson, 1974; Mitford, 1973; Orland, 1979) has led more and more states to shift toward the opposite direction of various forms of determinate sentencing (see Alschuler, 1978; Bagley, 1979; Dershowitz, 1976; Fogel, 1979; von Hirsch, 1976; Orland, 1979; Interim Report of the Sentencing Study Committee to Florida Supreme Court, n. d.; Lagoy, Hussey, & Kramer, 1978; Twentieth Century Fund Task Force on Criminal Sentencing, 1976). Legislative changes such as these and their actual and potential impact will have to be taken into account in the interpretation of an empirical analysis of sentencing behavior.

Types of Sentencing Disparity

Some of the confusion about some of the differences in findings of previous research on sentencing disparity have arisen from the failure to take regional and temporal variations into account (Hindelang, 1969; Sutton, 1978a). Many of the studies on judicial sentencing are rather limited in scope, often focusing on a limited number of offenses within specific jurisdictions within a limited time period, making comparisons among them rather difficult (Sutton, 1978a). Therefore, it is suggested that an analysis of the study of sentencing variations should pay attention both to the level of analysis (regional scope), and the time dimension of the study.

Levels of analysis and time may be conceived of as two orthogonal dimensions within each of which several further distinctions can be made. Within the time dimension, we may conveniently distinguish between relatively short-term and relatively long-term variations in sentencing. For example, a single judge may give different sentences to highly similar offenses and/or highly similar offenders as a function of (short-term) situational variables such as the previous case sentenced (Pepitone & DiNubile, 1976). On the other hand, his/her sentencing behavior may change as a function of the number of years on the bench (Hogarth, 1971), or in the course of long-term historical-political changes (e.g. Rothman, 1977).

With regard to the levels-of-analysis dimension, several levels may be distinguished (see also Hogarth, 1971, for a somewhat different distinction). At the highest most encompassing levels, i.e. at the cross-cultural and cross-national levels, one would most obviously expect quite different sentencing practices in different societies (e.g., compare the penal codes of different countries; see also Hood & Sparks, 1970). At the next lower level, sentencing variation across states within the United States (or Provinces in Canada) has been repeatedly noted for the Federal Court System (e.g. Administrative Office of the United States Courts, 1972; Frankel, 1967; Frankel, 1972; Hogarth, 1971; President's Commission of Law Enforcement and the Administration of Justice, 1967; Sutton, 1978c; Tiffany, Avichai, & Peters, 1975). For the State Court System, geographical variations across states is obviously necessitated by the differences in sentencing that are legislatively fixed for each individual state (Seymour, 1977).

One notch below this level, one may observe variations across districts within a single state, i.e. among courts under the same statutory authority (Hogarth, 1971; Jaffary, 1963, cit. in Hogarth, 1971; Zimmerman, 1976). At the lowest level of analysis, focusing on the individual as the unit of analysis, variations across individual judges within a single court (or within a state, or even across cultures) have frequently been studied (e.g. Gaudet, 1933, 1938,

1949; Gaudet, Harris, & St. John, 1934; Green, 1961; Hogarth, 1971; Nagel, 1962; Schubert, 1977; Smith & Blumberg, 1967; Winick, Gerver, & Blumberg, 1961).

Many of the latter approaches essentially employ an individual differences approach which is geared to discovering the "personal equation", i.e. personality traits, attitudes, etc. that may be useful in predicting sentencing decisions. Hood & Sparks (1970) and Hogarth (1971) review some of these studies and criticize the circularity of reasoning characteristic of some of them.

In studies of this kind, differences in sentencing behaviour which cannot be explained by known differences in the kind of cases dealt with, are usually attributed to the "policies," "attitudes," or even the "personalities" of the judge or magistrate concerned. Thus, punitive judicial attitudes are inferred from apparently punitive sentencing behaviour. Judges are said to be "tough-minded," "tender-minded," "conservative," "liberal," "rigid," or "prejudiced," because their sentences appear to be so. To infer judicial attitudes indirectly from judicial conduct and not from specially designed questionnaires can lead to circularity in reasoning. It is arguable that one should not, by observing sentencing behaviour, impute an attitude and then employ that attitude to explain the behaviour, but this is precisely what most of these studies appear to do. (Hogarth, 1971, p. 10)

Other studies within this general category have been criticized by Sutton (1978a) for committing the "ecological fallacy." Sutton's criticism also stresses the importance of the levels-of-analysis distinction introduced here.

Towards a Definition of Sentencing Disparity

The above distinctions have important implications for a definition of sentencing disparity. A rational discussion of sentencing disparity will benefit from a consensus on the type of sentencing disparity (i.e. what level of analysis is being discussed). Although the terms disparity, variability, and variation will be used here coterminously--except when the latter refers to a statistical term--it should be noted that not every variation in sentencing necessarily deserves the label "disparity." For example, variations across cultures or nations are usually not denoted as disparities although comparative analyses at this level also contribute to our understanding of sentencing practices. But more importantly, the term sentencing disparity has negative emotional overtones, and for many experts it usually connotes inequality before the law, unfairness and injustice. Zimmerman (1976) has summarized this issue succinctly:

The word "disparity" has been much used and abused. Sentencing disparity is best characterized as unwarranted or unwanted variability in sentencing; it assumes that there should be relative consistency in sentencing after legitimate differences among cases have been accounted for. The problem, of course, centers around the definition of legitimate differences... (p. 18)

"Legitimate" differences are generally accepted when they arise as a function of "legal factors" (Sporer, 1978, 1979, 1980) such as the type of offense or the number of

prior convictions of the offender (although the latter is occasionally debated; cf. Frankel, 1972). However, even some of these legal factors may lead to disparate results in sentencing outcomes in the absence of guidelines to determine the weight they should be given in the sentencing decision (Frankel, 1972; Twentieth Century Fund Task Force on Criminal Sentencing, 1976). There is also a middle-ground of factors subject to legal dispute, e.g. a guilty plea vs. insistence on the right to trial, or the number of prior arrests (not convictions), which make the distinction between legal and extra-legal factors at times difficult.

To arrive at stringent estimates of the magnitude of sentencing disparity, as many of these factors as possible will have to be controlled. Archival studies that have attempted to control for many of these variables through "matching" (the so-called "comparable case" method, Diamond & Zeisel, 1975) approximate this ideal but can always be criticized because it will never be possible to know all the possibly relevant variables and to control for them (Austin & Williams, 1977; Diamond & Zeisel, 1975; Hood & Sparks, 1970; Partridge & Eldridge, 1974). The only method that allows us to estimate the true magnitude of disparity is the "identical case" method (Diamond & Zeisel, 1975), i.e. a "true experiment" (Campbell & Stanley, 1966; Cook & Campbell, 1979), in which identical cases are randomly assigned to different judges whose sentences can then be

directly compared.

The present investigation has used the latter approach, and disparity will be defined accordingly as the variation among individual judges sentencing an identical case (identical offender convicted of an identical crime). Within the levels-of-analysis and time dimension framework suggested above, this definition addresses the issue of disparity among individual judges within a single court and also more generally among judges within the same statutory authority at a particular point in time.

CHAPTER II

REDUCING SENTENCING DISPARITY: PROPOSALS FOR SENTENCING REFORM

Legal journals, scholarly and popular books, and governmental and administrative reports frequently make suggestions for changing the judicial sentencing structure. They are so numerous that it would be impossible to review them all here. Most of the arguments advanced are highly repetitious, and very seldom supported by empirical evidence. However, most of them seem to have one thing in common: They do acknowledge that sentencing disparity does exist--to some greater or lesser extent--and that it has had negative consequences for the criminal justice system and the public's perception thereof. Therefore, many of these proposals constitute attempts to reduce sentencing disparity in one form or another, or to impose some external check on sentencing decisions. The most common proposals are briefly outlined here, along with some of the drawbacks and common criticisms thereof.

or

Determinate Sentencing

During the last few years, about half a dozen states have replaced the former indeterminate sentencing laws with various forms of determinate sentencing provisions (e.g. Maine, California, Arizona, Illinois and Indiana), and about another half a dozen other states are considering similar legislation (cf. Bagley, 1979; Gettinger, 1977; Orland, 1979; Serrill, 1977). These legislative reforms are a response to the general disillusionment with the rehabilitative model inherent in indeterminate sentencing, offering instead a justice model ("justice-as-fairness," Fogel, 1979) and "fair and certain punishment" (Twentieth Century Fund Task Force on Criminal Sentencing, 1976).

Determinate sentencing exists in various forms, such as mandatory or minimum mandatory, flat-time, and presumptive sentencing, each curbing discretion of the sentencing judge to varying degrees. However, it would be illusory to believe that this would end the disparity problem. More likely, discretion will simply be displaced from judges and parole boards to legislators, prosecutors and prison discipline committees (Alschuler, 1978; Interim Report of the Sentencing Study Committee to the Florida Supreme Court, n.d.; Orland, 1979). Thus, the "sledgehammer" approach of determinate sentencing may easily backfire by raising the bargaining power of the prosecuting attorney even further, an already serious problem and a thorn in the public's eye (Alschuler, 1978; President's Commission on Law Enforcement

and Administration of Justice, 1967; Saks & Hastie, 1978; for a somewhat more optimistic perspective, see von Hirsch, 1976; generally, on plea-bargaining, see Law and Society Review, 1979, whole issue). From a somewhat different perspective, determinate sentencing laws also indicate a shift of responsibility away from the judiciary to legislative organs of society. Well-intended as some of these proposals may be, that attempt to reduce the incarcerative tendencies of the present system (e.g. von Hirsch, 1976; Twentieth Century Fund Task Force in Criminal Sentencing, 1976) they may easily fail when they are forced to face up to public pressure toward crime control through even longer prison sentences (Zimring, 1976, cit. in Interim Report of the Sentencing Study Committee to the Florida Supreme Court, n.d.). Not only would this overcrowd the prisons even more, but it would also throw us back to a rigid and inhumane sentencing structure in which the punishment only fits the crime but not the criminal.

Appellate Review of Sentencing

Much of the criticism of the disparity problem cited above are directed at the fact that

at present the United States is the only nation in the free world where one judge can determine conclusively, decisively, and finally the minimum period of time a defendant must remain in prison, without being subject to any review of his determination. (Chief Judge Kaufman, 1964, cit. in O'Donnell et al., 1977, p. 1)

It seems obvious that appellate review of sentencing or some other sentencing review board has been frequently suggested to ameliorate this problem (e.g. among many others: Frankel, 1972; Gaylin, 1974; Morris & Hawkins, 1977; O'Donnell et al., 1977; D'Esposito, 1969; President's Commission on Law Enforcement and Administration of Justice, 1967). However, unsettled issues remain: Who should do the reviewing--the already overburdened appellate courts or a sentencing review panel (as for example, in Georgia or New Hampshire)? Should the reviewing court be only allowed to reduce the sentence (creating the possibility of frivolous appeals), or also be allowed to increase it (critically, see Frankel, 1972; Gaylin, 1974; cf. also D'Esposito, 1969, n. 92, for constitutional objections regarding double jeopardy)? There are also some additional problems related to the fact that Appellate Court judges are generally trained to decide on matters of law, not sentencing policy, and that they are too removed from the trial and the defendant who is not given the possibility of allocution (cf. O'Donnell et al., 1977). Last but not least, appellate review may be regarded as an insufficient solution to the disparity problem because it can only remedy the problem after the fact--a time-consuming process--but never prevent it.

On the other hand, the possibility of review may refrain judges from excessive sentencing decisions, especially when they are required to put their reasons for

their decision into writing a sentencing opinion. Appellate reviews may also be helpful in the establishment of sentencing guidelines which could aid judges to structure judicial discretion.

Sentencing Guidelines

Another response to the lack of control in judicial sentencing power is the call for sentencing guidelines and explicit sentencing criteria. O'Donnell et al. (1977) and the Sentencing Study Committee to the Florida Supreme Court (Interim Report, n.d.) have recently elaborated some detailed proposals that make use of the positive aspects of many of the earlier suggestions and at the same time try to minimize some of the weaknesses of them. One of the positive features of these proposals is that they preserve discretion and the possibility of flexible individualization while at the same time subjecting these decision-making processes to legislative guidelines. They also require of these decision-making processes that they be made explicit and therefore subject to public inspection and judicial review.

For example, the Proposed Federal Sentencing Statute described by O'Donnell et al. requires an explicit brief statement of reasons for the sentence imposed as part of the record on a routine basis, to be disclosed to the defendant at the time of sentencing. This proposal also requires explicit consideration of the goals of sentencing, i.e.

deterrence, incapacitation, rehabilitation, and denunciation. The development of the guidelines with respect to recommended normal sentence is delegated to an independently established United States Commission on Sentencing and Corrections.

These proposals open some new avenues, the consequences of which can hardly yet be anticipated. They offer some distinct advantages, especially when coupled with some of the other approaches that deserve attention in the future. One of the key problems underlying this approach, as well as the presumptive sentencing approach to which it is similar, does not seem adequately faced: For one thing, this approach presupposes an adequate detailed taxonomy of criminal behavior which we simply do not possess, and secondly it presupposes a knowledge of adequate criteria for the prediction of dangerousness of offenders which are similarly nonexistent (on the prediction of dangerousness, see Levine, 1977; Monahan, 1976; Monahan & Hook, 1978; Shah, 1978). The latter problem, of course, is a dilemma faced by any approach to sentencing and can therefore not be held against a singular reform effort.

Other Approaches

Numerous other approaches have been suggested to remedy the sentencing disparity problem at one time or another. Suggestions include a more careful selection of judges, a better training and education of judges, continuing

education of judges, e.g. through sentencing institutes on various sentencing issues; improvements in presentence investigations and presentence reports; continual feedback to judges; information storage and retrieval systems, among many others. Some of these suggestions have more far-reaching implications than others, and some of them may be very useful in ameliorating sentencing disparity. It would be impossible to discuss them here in any detail. However, there is one other institution that has been frequently suggested: sentencing councils. I shall first describe some of the operating characteristics of sentencing councils and then examine some of the claims that have been made for their effectiveness.

CHAPTER III

SENTENCING COUNCILS

Sentencing Councils vs. Sentencing Tribunals

Before I give a description of sentencing councils, it will be necessary to distinguish them from sentencing tribunals (Frankel, 1972). In sentencing tribunals, the responsibility of sentencing is taken away from a single individual and laid in the hands of a sentencing tribunal (usually three members) who are to arrive jointly at a sentencing decision. Tribunals may not only consist of judges but also contain other "experts"--such as sociologists, psychologists, or educators (Frankel, 1972; Glueck, 1936)--whose special knowledge is considered beneficial to an optimal disposition of a defendant. However, recent research into the prediction of dangerousness and the vicissitudes of rehabilitation (cf. Chapters I and II) makes it unlikely that any professional would be agreed upon to be especially equipped for the sentencing task. Therefore, this somewhat idealistic notion is not likely to find many supporters.

A second notion underlying the sentencing tribunal is more fundamental: In sentencing tribunals, the decisional power is taken away from the individual judge, and delegated to the tribunal as a collective. In sharp contradistinction, in sentencing councils the sentencing decision remains solely in the hands of the individual sentencing judge; the council's function is purely advisory. Thus, the judge's constitutionally guaranteed upon independence of judgment is not impaired. This makes the sentencing council a more palatable candidate for reform within the present constitutional framework.

Description of Sentencing Councils

Motivated by the awareness of the injustices created by sentencing disparity that were revealed at sentencing institutes (Hosner, 1970), federal court judges of the Eastern District of Michigan (Detroit) started to convene at a regular basis to share their sentencing problems. Shortly thereafter, these meetings were formally instituted as sentencing councils consisting of three judges. Council membership was rotated so that judges partook in different councils at different times. Soon other districts followed suit, and sentencing councils of various sizes (temporarily from two to nine judges) were created by the Eastern District of New York in 1962, the Northern District of Illinois in 1963, and recently, the District of Oregon (Diamond & Zeisel, 1975); Frankel, 1972).

Parsons (1964) described the procedure typically followed by the Eastern District of Michigan, and emulated by other districts:

Under the practice of our district, these meetings are held for an hour in the morning, before the commencement of the day's routine, when the judges may give the matters their undivided attention. The judges meet in panels of three, each judge having the presentence investigation report from the probation department and having prepared a study sheet, not only for the offenders he must sentence, but also for those who are the primary responsibility of the other two judges. Customarily, the one judge will call his first case, merely stating the name of the offender and giving a brief statement of the offense. He will then state to his brother judges the factors in his judgment, believed to be controlling as to disposition, and will recommend a disposition to be made. Each of the other two judges will then give, in turn, the factors believed by him to be controlling, together with his recommended sentence. The sentences will normally vary, although I have observed with a great deal of interest that the sentences of judges working together in this manner tend, as times goes on, to approach a common ground. It is in the discussion following the recommendation as to sentencing that the Council performs its most useful function.... The weights assigned the various factors thought to be controlling as to disposition of the case are sometimes modified by the sentencing judge in the light of the experience of his brother judges with their own previous sentences. (p. 431-432)

This description of three-member councils characterizes the prototype of sentencing council procedures, although there are some regional variations. Participants in sentencing councils have reported on their successes with great enthusiasm (e.g. Doyle, 1961; Hosner, 1970; Levin, 1969; Smith, 1963; Zavatt, 1967), and various authors and agencies have recommended them for widespread use (e.g. American Bar Association Project on Minimum Standards for

Criminal Justice, 1971; Frankel, 1972; National Advisory Commission on Criminal Justice Standards and Goals: Corrections, 1973; President's Commission on Law Enforcement and Administration of Justice, 1967; critical: Interim Report to the Sentencing Study Committee to the Florida Supreme Court, n.d.).

A Preliminary Evaluation of Sentencing Councils

The goal of sentencing councils is not necessarily total uniformity in sentencing but rather the mutual exchange of ideas on sentencing philosophies which should lead to the development of sentencing standards. They should also provide a safeguard against excessive sentences on either side of the scale. Sentencing councils have also been claimed to have reduced sentencing disparity, at least among judges within a given jurisdiction (i.e. at the lowest level of analysis in terms of our analysis proposed above; Hosner, 1970; Levin, 1969; Smith, 1963; Zavatt, 1967). Additionally, they supposedly have resulted in less severe sentences, especially in terms of an increased tendency to utilize nonincarcerative sentencing alternatives (e.g. probation; Levin, 1969; Zavatt, 1967).

The latter tendency would also indicate a desirable feature in the view of other recent reform committees (e.g. von Hirsch, 1976; Twentieth Century Fund Task Force on Criminal Sentencing, 1976). Hosner (1970) has pointed out that the council proceedings had additional positive effects

on the work of the probation office and specifically the preparation of presentence reports when probation officers were present in council hearings and could in this way learn how their information was utilized by the judges for their sentencing decisions. More generally, he praised the improvement of the relationship between judges and corrections as a function of the interchanges between judges and probation officers.

It should be noted however, that all the above claims are based on "insider's" evaluations. We know from the growing body of literature on research on program evaluation about the limitations of inside evaluations (see Anderson & Ball, 1978). The richness of informal observations does not outweigh the advantages of detached, objective assessment that modern evaluation research requires. Many of the observations provided by these authors are rather informal, such as illustrative descriptions of cases in which sentencing councils were especially beneficial. Some of the reports do present data obtained from court records and/or abstracted from records kept on the council meetings (Levin, 1969; Zavatt, 1967) but the data are only descriptive statistics indicating the number of cases dealt with by the sentencing councils, or the number and kind of changes from the initial recommendations of the presiding judges to their final dispositions. The data are provided over several years of the councils' operation.

These data are useful summary statistics but pose severe methodological restrictions on their interpretation. The claims are not stated as falsifiable hypotheses, and are not subjected to rigorous statistical tests. Although seasonal trends could possibly be inferred on their basis, interpretations are severely limited by the plausible rival hypotheses threatening pre-experimental or time-series designs without comparable control groups (Campbell & Stanley, 1966; Cook & Campbell, 1979).

For example, the increase in proportion of probation sentences for the first five years of the council's operation at the Eastern District of Michigan (Levin, 1967) could be a function of the sentencing council but could also be simply a reflection of a general maturational trend towards less incarcerative sentences during this time period. Only a control series design (Campbell & Stanley, 1966; Cook & Campbell, 1979) would allow us to rule out maturation as a plausible rival hypothesis given that the relative frequency of offense categories had also been controlled for.

Apart from the difficulties in evaluating some of the claimed advantages of sentencing councils more conclusively, sentencing councils have the additional advantage that they offer an "optimizing strategy" (Zimmerman, 1976) to the disparity problem. This approach keeps discretion as a vital principal of judicial sentencing intact while at the

same time introducing a procedural check that makes "equality before the law" more likely.

CHAPTER IV

SENTENCING COUNCILS:

A SOCIAL-PSYCHOLOGICAL ANALYSIS

Sentencing:

Decision-making in an Ambiguous Stimulus Situation

"There is no decision in the criminal process that is so complicated and so difficult to make as that of the sentencing judge." (The Challenge of Crime in a Free Society--A Report of the President's Commission on Law Enforcement and the Administration of Justice, 1967, p.141). With this quote, John Hogarth opened his classic study on the sentencing behavior of magistrates in Ontario, Canada: Sentencing as a Human Process (1971). There could be no better description in a few words. Sentencing is a complex human decision-making process. In other words, sentencing is a psychological decision-making process, determined by all the personal and environmental variables that have been found to influence any other human decision-making process.

Decision-making processes in the criminal justice system are characterized by the principle of discretion (Shaver et al., 1975). The principle of discretion is a unifying principle that marks an ideal entrance point for

the social-psychological study of legal decision-making processes. As Shaver et al. (1975) put it:

Although "bounded by rules," the exercise of discretion is an individual action, affected by the actor's attributions, attitudes, values, social status and role, and numerous other factors of interest to social psychologists. (p. 472)

But once we recognize the importance of discretion for a social-psychological analysis where do we go from there? Which theory or set of theories is most pertinent to an adequate understanding of sentencing behavior? From psychophysical scaling to equity theory, from theories of attitude and opinion change to attribution and information integration theory, any of these theories may or may not be relevant (see, for example Austin & Utne, 1977; Pepitone, 1975, 1976).

When we return to our focus on sentencing as a complex decision-making process, there is one aspect of the sentencing decision that is frequently overlooked: Although the task of sentencing is relatively well-defined, i.e. there is a specified set of decision-alternatives from which the judge may choose--see Chapter I--there is no correct answer to the problem of finding a sentence best suited to the offender for his/her offense at hand. Given the paucity of information available to the judge about the offender at the time of sentencing, including the general problem of our relative inability to predict violence (e.g. Levine, 1977; Monahan, 1976; Monahan & Hood, 1978; Shah, 1978), sentencing can be best described as an analogue to the

judgment of an ambiguous stimulus (the defendant and his/her actions and life circumstances). The task of the judge becomes one of integrating all the relevant information about the offender and his/her offense, and to scale it on one of several dimensions, e.g. probation or incarceration, magnitude of fine, or length of prison sentence.

It has been a long-known fact within the Gestalt tradition of social psychology that "the more unstructured the external stimulus situation, the greater the contribution of internal factors--including internalized social values and standards." (Sherif & Sherif, 1969, p. 62). Translated into the context of judicial sentencing, this proposition would indicate that in the absence of precise sentencing laws (determinate sentences) and in the absence of sentencing guidelines, coupled with the scanty knowledge about offender and offense, the personality and values of the judge, e.g. his/her attitudes and particularly his/her sentencing philosophy, would determine the sentencing decision. Empirical evidence, both from field studies and laboratory simulations corroborate this proposition (Hogarth, 1971; McFatter, 1978).

If the assumption of the offender/offense as relatively ambiguous stimulus is correct, it should come as no surprise that sentencing decisions are as variable as they have been documented to be. In analogy to the classic findings with the autokinetic effect paradigm (Sherif, 1935, 1936), great variability in judgment--e.g. the "magnitude estimation" of

number of years of imprisonment--would be expected when neither reference points (other people's judgments) nor guidelines (norms) are present.

This perspective has several advantages. It makes sentencing variability an understandable human process, and renders discussions about the rationality or irrationality in sentencing superfluous (cf. Austin & Utne, 1977; Hood & Sparks, 1970). But more importantly, it opens a wide and well-established vista of theory and research in social psychology that can guide us to find ways to reduce sentencing disparity. Of course, the specific parameters will have to be fleshed out in more detail before we can arrive at useful predictions.

Sentencing Councils and the Social Psychology of Groups

The decisional procedure followed by sentencing councils: Tentative judgment alone, discussion, judgment alone by the sentencing judge, is identical to one of the oldest research paradigms in experimental psychology (e.g. the experiments by Munsterberg, 1914; Allport, 1924; Jenness, 1933; or last but not least Sherif's classic experiments on the production of social norms, 1935, 1936). Not only is there a striking match in paradigm but there are other similarities as well that make the results of many of the studies on conformity arising from the Sherif tradition highly relevant for sentencing councils. For example, both approaches are usually concerned with the judgment of an

ambiguous stimulus--here, the offender and his/her actions--and the very factors that have been shown to operate in the laboratory to create the convergence toward the mean--Sherif's famous "funnel-shaped" curve--are likely to be at work in sentencing councils as well (for reviews, see Hollander, 1971; Lorge, Fox, Davitz & Brenner, 1958; Moscovici, 1974; Shaw, 1976; Sherif & Sherif, 1969).

Thus, one interesting finding of the above studies is that the newly acquired norm seems to persevere even in the absence of the reference group (Hood & Sherif, 1962). In our case, the judge, when he/she is by him/herself in making up the sentence after the discussion would be expected to continue to be influenced by the other council members' judgments. Findings from sentencing councils in operation in several federal districts (Diamond & Zeisel, 1975; Levin, 1969; Zavatt, 1967) as well as from a simulation of sentencing councils conducted by Zimmerman (1976) confirm this proposition. After the discussion, when judges were by themselves and had sole responsibility to make the final decision, a significant portion of them changed their decision from pre-discussion to final disposition. Overall changes were reported, in percentages of cases processed by councils: 31.9% in the Eastern District of Michigan between 1960 and 1965; 19.8% in the Eastern District of New York, for the years 1962 through 1964, and 42% for the year 1973; 33% in the Northern District of Illinois during 1973; and 43.1% in the simulation study).

Based on the above findings both from the laboratory and the field, it can be expected that relatively more judges who participate in sentencing councils will change their initial sentence than a control group of judges who engage in an alternative activity such as writing a sentencing opinion.

Reduction of Variability

If the analogy of the defendant as an ambiguous stimulus is correct, we would expect that judges' sentencing behavior would be less variable after discussion of the defendant's case in the sentencing council. The observations and archival analyses by Levin (1969) and Zavatt (1967) have led to claims of this effect but the evidence these authors present is not conclusive. For example, Chief Judge Levin's report provides an enthusiastic description of the workings and accomplishments of the sentencing council in the Eastern District of Michigan (Detroit) on its first five years of existence but the only evidence regarding reduction in disparity he provides is a commentary on the uniform philosophy that supposedly developed within this group.

We are getting closer in the development of a uniform philosophy. The meetings consume less time now than they did five years ago. The range of the varying recommendations has become increasingly narrow. We have particularly experienced a substantial decrease in the frequency with which the Council is confronted by disagreement about the type, rather than the quantum, of the sentence. For example, during the Council's second year, there were sixty-five cases in which one panel member suggested custody and a

least one other member suggested probation. During the Council's fifth year, there were only about twenty-five cases in which the judges disagreed, even initially, on whether custody or probation was the proper sentence. (Levin, 1969, p. 144)

No further data are given to corroborate these conclusions. Therefore, the reader is forced to accept his contentions on faith. Similar criticisms apply to Chief Judge Zavatt's descriptive report on the functioning of the sentencing council in the Eastern District of New York (Brooklyn).

A more detailed and methodologically much more rigorous analysis of the councils at the Eastern District of New York and the Northern District of Illinois has been conducted by Diamond D Zeisel (1975).

These authors have constructed a sophisticated index of disparity in their investigation of the Chicago and New York district courts. For both courts they found a reduction in sentence variability of approximately ten percent.

Two laboratory simulation studies on sentencing councils, using law students as mock judges, found even greater reduction in sentence variability as a function of group discussion (Sporer, 1980b; Zimmerman, 1976). In Zimmerman's experiment, this effect was even more pronounced for the group decision condition in which the judges, like sentencing tribunals, were required to reach a consensus within their group. Diamond & Zeisel (1975) have constructed a sophisticated index of disparity in their investigation of the Chicago and New York district courts.

For both courts they found a reduction in sentence variability of approximately ten percent. Zimmerman (1976), using law students as mock judges, found even greater reduction in variability on a sentence severity scale after discussion. The effect was even more pronounced for the group decision condition in which the judges were required to reach a consensus within their group. However, the absolute amounts of disparity reduction of these studies cannot directly be compared because they have been obtained with different mathematical estimation procedures.

It should be noted here that Zimmerman's (1976) study is very close both in approach and methodology to the one proposed here. His comprehensive investigation has shed light on many important details of functioning in sentencing councils. However, it also differs in many respects from the study proposed here. Most importantly, Zimmerman's experiment addressed the issue of reduction in variability only at the within-council level, and thus has limited its scope unnecessarily. The present study has in addition addressed itself to the equally important issue: whether or not it is possible to reduce overall sentencing variability across a large number of judges by the use of three-member councils. Previous investigators and legal commentators have failed to see this possibility, or have denied it altogether without any evidence (e.g. Interim Report of the Sentencing Study Committee to the Florida Supreme Court, n.d.).

A recent laboratory simulation by Sporer (1980b) has shown that it is possible to reduce overall sentencing variability across judges as a function of discussion in sentencing councils. Using a repeated-measurement design, both three- and two-member councils showed significantly less variability on the prison sentence and parole eligibility measures employed than prior to discussion.

However, all of these previous studies employed repeated-measurement designs, and thus do not allow us to assess to what extent reduction of variability would (or would not) occur if the council participants had not given their sentences prior to discussion (i.e. without a "pretest"). The present study remedies these deficiencies by employing an extension of the Solomon-four-group design (Campbell & Stanley, 1966). This design allows one to assess the effect of sentencing councils with or without a pretest, and in addition the effect of the pretest itself and the pretest X council interaction.

In terms of the sentencing council procedure, the pretest corresponds to the preliminary sentences judges record on their sentencing study sheet (cf. Levin, 1969). It would be important to know whether sentencing variability would be similarly reduced irrespective of whether or not the judge had recorded a sentence prior to discussion. The act of writing down a tentative sentence could introduce a commitment to this particular choice of sentence which would make it more resistant to change through group input. The

Solomon-four-group design employed here is ideally suited to explore these various possibilities within a single study.

In sum, it is hypothesized that post-council sentences will show less variability than pre-council sentences, and that post-council sentences will be less variable than sentences meted out by a control group that did not convene in sentencing councils. Both hypotheses are proposed to hold for variability across judges within councils (within-council variability) as well as for variability across judges across a whole array of councils (across-council variability).

Uniformity in Sentencing Philosophy

It has been variously asserted by proponents of sentencing councils that the major function of sentencing councils should be and has been to create a more uniform sentencing policy (e.g. Levin, 1969; Parsons, 1964; Zimmerman, 1976). However, data obtained over several years of operation of existing sentencing councils are at best equivocal on this issue (cf. Diamond & Zeisel, 1975; Levin, 1969; Zavatt, 1967). Methodologically, this claim requires a longitudinal design, but at this point there is no conclusive evidence supporting this contention. Therefore Zimmerman was not surprised that he could not obtain increases in uniformity in pre-council sentences over the short time span of his simulation consisting of nine cases.

However, there is an alternative way to conceptualize this issue. "Uniform sentencing policy" is not well defined and consequently can mean a variety of things to different authors. One aspect that has been repeatedly noted as an important determinant of sentencing behavior is the judge's sentencing philosophy (e.g. Hogarth, 1971; McFatter, 1978; O'Donnell et al., 1977). For example, Hogarth (1971) has shown that Canadian sentencing magistrates differ greatly in their view on the classic doctrines of criminal sanctions, i.e., reformation (rehabilitation), general deterrence, individual deterrence, punishment and incapacitation.

Now, provided that such large individual differences exist, and that these views are amenable to change, it should be possible that an exchange of ideas on judges' penal philosophies should reduce some of these differences, and hopefully, in turn, reduce sentencing variability. This is the notion underlying the implementation of sentencing institutes and judicial education. But again, conclusive evaluative data on their impact on sentencing are hard to come by (cf. Youngdahl, 1969). Therefore, the present study attempted to determine the effect of discussion of sentencing philosophy directly by testing the effectiveness of an "extended sentencing council" that would be induced to discuss the goals of sentencing in addition to the discussion of the sentences proposed and the specific sentencing reasons. It was hypothesized that sentences meted out after discussion of sentencing goals in the

extended council would be less variable than in the "normal council" or in the "no-council" control group.

Penalty Shifts (Leniency / Severity Shifts)

Informal observations and archival analyses by Frankel (1972), Levin (1969), and Zavatt (1967) tentatively suggest that post-council sentences tend to be more lenient than pre-council sentences, especially with regard to a more frequent choice of probation over prison sentences. However, these trends are subject to large temporal variations, and their interpretation is highly problematic (see Chapter III). Other investigators who have adopted a social-psychological perspective have attempted to re-conceptualize penalty shifts (leniency shifts and severity shifts) as particular instances of the polarization phenomenon that has frequently been observed as a function of group discussion. The group polarization hypothesis (originally coined by Moscovici & and Zavalloni, 1969, and further promulgated by Moscovici and Doise, 1974, Myers and Lamm, 1976, and Lamm & Myers, 1978) is an extension of the former "risky" shift and choice shift literature (e.g. Brown, 1965; Cartwright, 1971; Dion, Baron, & Miller, 1970; Pruitt, 1971a and b). Zimmerman (1976) also applied the choice shift literature to laboratory simulations of judicial sentencing, leading him to predict a general leniency shift tendency as a function of group discussion. He obtained a leniency shift from prison to probation

sentences but no reduction in length of sentence. Sporer (1980b) obtained significant leniency shifts for both three- and two-member councils on a parole eligibility measure but not for the prison sentence measure.

A series of other investigators on jury decision making some of which incorporated some form of punishment or sentencing scales as dependent variables also observed a general tendency toward leniency (Davis, Kerr, Stasser, Meek, & Holt, 1976; Foss & Foss, 1973, cit. in Lamm & Myers, 1978; Gleason & Harris, 1976; Laughlin & Izzett, 1973; Rumsey, 1976; Rumsey, Allgeier, & Castore, 1978; Rumsey & Castore, 1980; Rumsey & Rumsey, 1977; Wahrman, 1977). Others found evidence for group polarization (i.e. both leniency and severity shifts; Bray & Noble, 1978; Kaplan & Miller, 1978; Laughlin & Izzett, 1973; Myers & Kaplan, 1976; Rumsey & Castore, 1980; Vidmar, 1972). Lastly, Heimbach (1970, cit. in Lamm & Myers, 1978) observed a severity shift, and Izzett & Leginski (1974) obtained a shift toward the middle contrary to the group polarization hypothesis. To the extent that these experiments on juries are relevant to studies on judicial sentencing--juries are more concerned with finding of facts and verdicts of guilt or innocence than with sentencing decisions (cf. Sporer 1978, 1979; Weiten & Diamond, 1979; Vidmar, 1979)--the present study could shed some light on the differences in paradigm in which one would or would not expect leniency shifts, severity shifts, or both. Taken

together, both field and laboratory studies indicate a weak general tendency toward a leniency shift as a function of group discussion, and could therefore also be expected in this experiment. The leniency shift would also be of practical importance, considering the pleas for reduction of incarcerative sentences by contemporary reformers (see Chapter I).

CHAPTER V

METHOD

Participants

Participants were 277 (136 male and 141 female) psychology students in partial fulfillment of the requirements of an introductory psychology course. They were assigned to the experimental conditions. The experiment was carried out at fifteen different sessions, each with about 15 to 30 students.

Materials and Procedure

Participants were presented with a fictitious case of armed robbery for which the defendant had pleaded guilty (see Appendix: Case Summary). The defendant was described to have been convicted of armed robbery once before. The case description was modeled after case descriptions normally found in presentence reports prepared by the probation officer or some other court official (cf. Poulos, 1976).

The experimenter (the same for all sessions) introduced the experiment as a study in "Legal Psychology," dealing with judicial sentencing. Participants received one booklet

containing: (1) A cover page with a short introductory paragraph and questions to fill in the participant's sex, age, and year in school; (2)(a) a pretest assessing the participant's sentencing philosophy (Sentencing Philosophy Scale, see Appendix: Goals of Sentencing; cf. Hogarth, 1971) in the "pretest" condition, or (b) an alternative assessment scale in the "no pretest" condition (an acquiescence-free version of the California F Scale (Byrne, 1974); (3) the case description; (4) in the "pretest" condition only: a scale for the prison sentence and parole eligibility decisions (see Chapter VIII on the validity of these scales), rating scales for the confidence in the prison sentence and parole eligibility decisions, as well as rating scales for the offense and the offender; (5) in the "pretest" condition only: an Information Checklist; (6)(a) a Sentencing Opinion Sheet in the "no-council" condition, (b) a Sentencing Council Instruction to discuss the sentences and the reasons for the sentences with the other council members ("council" condition), (c) a Sentencing Council Instruction to discuss the goals of sentencing, the sentences, and the reasons for the sentences given; (7) prison sentence and parole eligibility, confidence rating and offense and offender scales as in (4) but with slightly differing instructions, according to the experimental condition; (8) an Information Checklist; (9) the Sentencing Philosophy Scale; (10) space for subjective comments.

In summary, half of the participants were "pretested" with regard to their sentencing philosophy, judgment of the case and information use prior to the council discussion or opinion writing, the other half were administered the Authoritarianism Scale (F Scale). Orthogonally, one third of the participants wrote a sentencing opinion, one third were randomly assigned to three-member sentencing councils in which they discussed sentences, and one third were assigned to three-member "extended" sentencing councils in which they discussed their sentencing philosophies, sentences, and reasons for the sentences. Assignment to conditions and to groups within conditions was determined randomly.

After about 20 minutes of opinion writing/discussion participants were (again) asked to judge the defendant. Participants in the council conditions were instructed to judge the defendant by themselves, "carrying the ultimate responsibility and enjoying the constitutionally guaranteed independence of judgment."

Participants were then explained the nature and goals of the study and thanked for their participation.

Dependent Variables

The major dependent variables were the prison sentence and parole eligibility measures, scaled from 0 to 20 years, with 1/2 year intervals. (Although the term parole eligibility, with reference to a judge's sentencing

decision, is technically inappropriate, it does convey the decision on the time after which the defendant is likely to be "back on the street" to legally relatively unsophisticated simulation participants in an unambiguous manner; cf. Chapter VII, on dependent variable validity.) Sentencing Philosophy ("Goals of Sentencing") was assessed with 5-point scales used by Hogarth (1971). The intervals were labeled "very important, quite important, of some importance, of little importance, and of no importance." Additionally, participants' perceptions of the offender and the offense were assessed with 11-point rating scales, as well as their confidence in their prison sentence and parole eligibility decisions.

The Information Checklist consisted of a numbered list of facts all contained in the case participants had read previously. Participants rated the facts they found most important for their decision separately for prison sentence and parole eligibility on a scale from one to five (not at all important to extremely important).

CHAPTER VI

RESULTS

Overview of Design and Analyses

The over-all design of the study was an extension of the Solomon-four-group design (Campbell & Stanley, 1966) to a six-group design, with the two levels of the pretest factor completely crossed with the three levels of the council factor. Although there is no single overall way to analyze this design (Campbell & Stanley, 1966), it provides several strong tests for the hypotheses advanced here. The tests will be described separately for each set of hypotheses.

Variability Within Councils

Between-groups Analyses

To assess the effectiveness of sentencing councils in reducing within-council variability, a 3X2 (council X pretest) multivariate analysis of variance (MANOVA) was performed with the posttest standard deviations of prison sentence and parole eligibility measures as dependent variables. Table 1 shows the mean standard deviations for both dependent variables. The MANOVA yielded significant

Table 1
 Mean standard deviations of the
 prison sentence and parole eligibility decisions for
 posttest scores for three-member councils ($N = 92$)

Council Conditions		Pretest	No Pretest	Marginal Means
	Dependent Variable			
No Council	prison sentence	3.76	2.56	3.12
	parole eligibility	2.32	1.83	2.06
		($\underline{n}=13$)	($\underline{n}=15$)	
Council	prison sentence	2.03	1.42	1.73
	parole eligibility	1.32	1.36	1.34
		($\underline{n}=17$)	($\underline{n}=16$)	
Extended Council	prison sentence	2.07	1.25	1.68
	parole eligibility	1.38	1.01	1.20
		($\underline{n}=16$)	($\underline{n}=15$)	
Marginal means				
	prison sentence	2.53	1.73	
	parole eligibility	1.62	1.40	

Note. All entries are given in terms of years.

main effects for both the council (multivariate $F(4,170) = 4.39, p < .002$) and pretest variable (multivariate $F(2,85) = 3.53, p < .034$). The council main effect was highly significant for both prison sentence and parole eligibility measures (univariate $F(2,86) = 8.48, p < .001$ and $F(2,86) = 5.31, p < .007$, respectively). As predicted, this effect was due to the much lesser variability observed for the council conditions as opposed to the no-council conditions, as revealed by an a priori special comparison between the no-council condition and the combined council and extended council conditions (multivariate $F(2,85) = 8.63, p < .001$; univariate tests for prison: $F(1,86) = 15.98, p < .001$; and for parole: $F(1,86) = 10.10, p < .002$). The differences between the council and the extended council condition were not significant (all $F's < 1$).

The main effect observed for the pretest was significant for the prison variable ($F(1,86) = 7.01, p < .01$), but not for parole eligibility ($F(1,86) = 1.28, n.s.$). Pretested groups showed, on the average, significantly larger variability in prison sentences than non-pretested groups. The difference for parole eligibility was in the same direction. The council X pretest interaction was not significant (all $F's < 1$). The latter interaction normally ought to be interpreted with caution if one adjusts the probability level for the fact that two dependent variables were employed in the absence of a multivariate test. But the pattern of results was exactly parallel for both

dependent variables rendering interpretation of this interaction unproblematic.

Repeated-measures Analyses

Approximately one-half of the participants judged the defendant both before and after council discussion (writing a sentencing opinion in the no-council condition). For this half of the extended Solomon-four-group design, it is possible to assess the direct effects of the council intervention and of the pretest, as well as their possible interaction. For this purpose two univariate analyses of variance (ANOVA's) with group standard deviations of the prison sentence and parole eligibility decisions were conducted, resulting in a 3X2 (council X time) design, with repeated measures (before/after discussion (opinion writing)) on the second factor. Table 2 shows the mean standard deviations for both variables.

The pattern of results is highly similar for both variables. While there were no main effects for the council variable (prison: $F(2,43) = 1.76$, n.s.; parole: $F(2,43) = 1.75$), both "time" and the time X council interaction were highly significant for both prison and parole variables. Prison sentences varied considerably less after the council discussion (time main effect: $F(1,43) = 18.60$, $p < .001$; time X council interaction: $F(2,43) = 6.21$, $p < .004$), and the same held true for the parole eligibility measure (time main effect: $F(1,43) = 7.50$, $p < .009$, time X council

Table 2
 Mean standard deviations of the
 prison sentence and parole eligibility decisions before and after
 council discussion / opinion-writing (N = 46)

Council Conditions		Before	After	Marginal Means
	Dependent Variable			
No Council (<u>n</u> =13)	prison sentence	3.62	3.77	3.69
	parole eligibility	2.20	2.32	2.26
Council (<u>n</u> =17)	prison sentence	3.13	2.03	2.58
	parole eligibility	1.76	1.32	1.54
Extended Council (<u>n</u> =16)	prison sentence	3.28	2.07	2.67
	parole eligibility	2.21	1.38	1.80
Marginal means				
	prison sentence	3.32	2.53	
	parole eligibility	2.04	1.62	

Note. All entries are given in terms of years.

interaction: $\underline{F}(2,43) = 3.64, p < .035$). The latter interaction becomes only marginally significant ($p < .07$) if one adjusts the probability level for the fact that two dependent variables were employed in the absence of a multivariate test. Therefore, it normally would have to be interpreted with caution. However, the pattern of results was exactly parallel for both dependent variables, rendering interpretation of this interaction unproblematic.

Further analyses of this interaction revealed highly significant simple main effects as predicted. Variability was less after discussion than before in the normal council condition (prison: $\underline{F}(1,16) = 25.6; p < .001$; parole: $\underline{F}(1,16) = 3.12; p < .096$) and for the extended council condition (prison: $\underline{F}(1,15) = 11.84, p < .004$; parole: $\underline{F}(1,15) = 9.06, p < .009$). No such differences were observed in the no-council condition (both \underline{F} 's < 1). For the between groups analyses, planned comparisons revealed significant differences in post-council variability between the normal council and the no-council condition (multivariate $\underline{F}(2,42) = 3.94, p < .027$; prison: $\underline{F}(1,43) = 7.25, p < .01$; parole: $\underline{F}(1,43) = 5.53, p < .023$), and between the extended council and the no-council condition (multivariate $\underline{F}(2,42) = 4.82, p < .013$; prison: $\underline{F}(1,43) = 8.96, p < .005$; parole: $\underline{F}(1,43) = 6.62, p < .014$). The predicted differences between extended council and normal council did not obtain (all \underline{F} 's < 1).

Variability Across Councils

Whereas the previous analyses focussed on the reduction of variability within councils (i.e. a lower level of analysis), it was also of interest whether the council intervention had any impact with regard to the over-all across-council variability (a higher level of analysis). Again, data are reported separately for the between-groups and the repeated measures parts of the design.

Between-groups Analyses.

Originally, it was planned to conduct a series of homogeneity of variance tests between the no-council and the two council conditions. However, the high correlations observed between the cell means and the respective cell standard deviations for both prison sentences ($r=.990$) and parole eligibility ($r=.866$) do not warrant these analyses with the given data because the differences in variances may be due to their relative positions on the scales rather than reflect true differences. Table 3 shows the cell means and standard deviations for the posttest scores of prison sentence and parole eligibility.

Whereas the pattern of the standard deviations of the pretested participants was consistent with the reduction of variability hypothesis, the results for the non-pretested participants was out-of-line with these predictions. In fact, prison sentences and parole eligibility measures in the no-pretest council condition were much more variable

Table 3
 Cell means and standard deviations of the
 prison sentence and parole eligibility decisions
 for posttest scores ($N = 277$)

Council Conditions		Pretest		No Pretest	
		<u>M</u>	<u>S</u>	<u>M</u>	<u>S</u>
	Dependent Variable				
No	prison sentence	5.20	3.67	4.46	3.07
Council	parole eligibility	2.95	2.43	2.52	2.14
		(<u>n</u> =40)		(<u>n</u> =45)	
Council	prison sentence	4.90	3.36	6.60	5.30
	parole eligibility	2.89	1.90	3.62	2.87
		(<u>n</u> =51)		(<u>n</u> =48)	
Extended	prison sentence	4.60	3.02	4.51	3.23
Council	parole eligibility	2.77	2.06	2.49	1.82
		(<u>n</u> =48)		(<u>n</u> =45)	

Note. All entries are given in terms of years.

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Table 4
 Cell coefficients of variation ($\underline{s}/\underline{M}$) of the
 prison sentence and parole eligibility measures
 for posttest scores ($\underline{N} = 277$)

Council			
Conditions		Pretest	No Pretest
	Dependent Variable		
No	prison sentence	.706	.688
Council	parole eligibility	.823	.849
		($\underline{n}=40$)	($\underline{n}=45$)
Council	prison sentence	.686	.803
	parole eligibility	.657	.793
		($\underline{n}=51$)	($\underline{n}=48$)
Extended	prison sentence	.657	.716
Council	parole eligibility	.744	.731
		($\underline{n}=48$)	($\underline{n}=45$)

Note. The coefficient of variation is defined as the ratio of the standard deviation and the mean ($\underline{s}/\underline{M}$).

Table 5
 Cell means and standard deviations of the
 prison sentence and parole eligibility decisions before and after
 council discussion / opinion-writing ($\underline{N} = 139$)

Council Conditions	Dependent Variable	Before		After	
		<u>M</u>	<u>s</u>	<u>M</u>	<u>s</u>
No Council (<u>n</u> =40)	prison sentence	4.99	3.67	5.20	3.67
	parole eligibility	2.68	2.24	2.95	2.43
Council (<u>n</u> =51)	prison sentence	4.61	3.61	4.90	3.36
	parole eligibility	2.60	1.81	2.89	1.90
Extended Council (<u>n</u> =48)	prison sentence	4.50	3.62	4.60	3.02
	parole eligibility	2.80	2.57	2.77	2.06

Note. All entries are given in terms of years.

council condition: $F(1,46) = 5.93, p < .018$; council condition: $F(1,49) < 1, n.s.$; no-council condition: $F(1,38) = 2.02, n.s.$). However, these analyses are to be interpreted with caution because (a) the probability levels have to be halved to control for experimentwise error, and (b) the means and standard deviations are also moderately correlated although not as highly so as in the between-groups analyses (prison sentence: $r = .356$; parole eligibility: $r = .420$). The same pattern of results is obtained with coefficients of variation (see Table 6).

Taken together, these findings tentatively indicate that across-council variability was significantly reduced as a function of the extended council intervention but not as a function of the normal council or opinion-writing (no-council) intervention.

Leniency/Severity Shifts

Also of interest is the possibility of leniency or severity shifts as a function of group discussion. Generally, shifts in mean sentences are used as an indicator of leniency or severity shifts. However, the overall distribution as well as almost all of the within-cell frequency distributions for the prison sentence and the parole eligibility scores were found to be positively skewed (i.e. the mean was almost always larger than the median). Therefore, it is important to look at the means and the medians conjointly. Table 7 displays the cell means and

Table 6
 Cell coefficients of variation ($\frac{s}{M}$) of the
 prison sentence and parole eligibility decisions before and after
 council discussion / opinion-writing ($N = 139$)

Council			Before	After
Conditions				
		Dependent Variable		
No	prison sentence		.735	.706
Council	parole eligibility		.836	.824
			($\underline{n}=40$)	($\underline{n}=45$)
Council	prison sentence		.783	.686
	parole eligibility		.696	.658
			($\underline{n}=51$)	($\underline{n}=48$)
Extended	prison sentence		.804	.657
Council	parole eligibility		.918	.744
			($\underline{n}=48$)	($\underline{n}=45$)

Note. The coefficient of variation is defined as the ratio of the standard deviation and the mean ($\frac{s}{M}$).

Table 7
 Cell means and medians of the
 prison sentence and parole eligibility decisions
 for posttest scores ($N = 277$)

Council Conditions	Dependent Variable	Pretest		No Pretest	
		<u>M</u>	<u>Md</u>	<u>M</u>	<u>Md</u>
No	prison sentence	5.20	4.86	4.46	4.10
Council	parole eligibility	2.95	2.25	2.52	2.19
		(<u>n</u> =40)		(<u>n</u> =45)	
Council	prison sentence	4.90	4.67	6.60	5.00
	parole eligibility	2.89	2.71	3.62	2.92
		(<u>n</u> =51)		(<u>n</u> =48)	
Extended	prison sentence	4.60	4.25	4.51	3.80
Council	parole eligibility	2.77	2.75	2.49	2.17
		(<u>n</u> =48)		(<u>n</u> =45)	

Note. All entries are given in terms of years.

medians of the prison and parole eligibility measures for posttest scores, and Table 8 displays the means and medians for the before and after council discussion / opinion-writing conditions, respectively.

The data in the two tables reveal no reliable systematic pattern that could be considered indicative of a leniency or a severity shift as a function either of council discussion or a pretest effect. The only striking findings are the relatively high means for prison sentence and parole eligibility decisions obtained in the no-pretest council condition (see Table 7). However, closer inspection of the data in this cell reveals that this finding is due primarily to one council, in which all three members gave the highest possible sentences (20 years). In all the other conditions, only one other possible 20-year sentence was given. This cell of the design also contained three members who gave sentences of 15 years whereas a 15 year sentence was given by only one other participant in the experiment. The data for the parole eligibility measure parallel this finding: Of the fourteen harshest parole decisions (i.e. parole eligibility after eight or ten years) eight were observed in this cell of the design.

Thus, the general leniency and severity shifts obtained by other investigators were not observed in this study. Therefore, analyses of the other dependent variables that were originally planned to assist in the interpretation of potential leniency or severity shifts are unnecessary.

Table 8
 Cell means and medians of the
 prison sentence and parole eligibility decisions before and after
 council discussion / opinion-writing (N = 139)

Council		Before		After	
Conditions		<u>M</u>	<u>Md</u>	<u>M</u>	<u>Md</u>
	Dependent Variable				
No	prison sentence	4.99	4.81	5.20	4.86
Council (<u>n</u> =40)	parole eligibility	2.68	2.05	2.95	2.25
Council (<u>n</u> =51)	prison sentence	4.61	4.77	4.90	4.67
	parole eligibility	2.60	2.40	2.89	2.71
Extended Council (<u>n</u> =48)	prison sentence	4.50	3.25	4.60	4.25
	parole eligibility	2.80	2.00	2.77	2.75

Note. All entries are given in terms of years.

Participants' ratings of confidence in their prison sentence and parole eligibility decisions are reported below.

Changes in Prison Sentence and Parole Eligibility Decisions as a Function of Council Discussion / Opinion-writing

In the repeated-measures part of the design it is possible to assess changes in the prison sentence and parole eligibility measures as a function of the council discussion / opinion-writing intervention. Table 9 shows the frequencies with which change (no change / increase / decrease) occurred in the various experimental conditions.

Overall, 46.8% of the participants changed their prison sentence, and 52.5% changed their parole eligibility decision between the first and second assessment. Changes occurred more frequently in both of the council conditions than in the opinion-writing condition, although changes in the latter condition were also unexpectedly frequent (chi-square tests for the no-council vs. the combined council conditions were significant for both the prison sentence: $\chi^2(1) = 5.43, p < .02$, and the parole eligibility measure: $\chi^2(1) = 4.27, p < .05$). Generally, among the participants who changed, increases in both prison sentence and parole eligibility measures tended to be more frequent than decreases across all conditions (cf. Table 9). These differences were significant for the parole eligibility measure ($\chi^2(1) = 6.04, p < .02$), and marginally significant for the prison sentences ($\chi^2(1) = 3.46, p < .07$).

Table 9
 Frequency of changes in the prison sentence and
 parole eligibility decisions as a function of the
 council discussion / opinion-writing intervention ($N = 139$)

Council Conditions		No Change	Increase	Decrease
	Dependent Variable			
No Council ($n=40$)	prison sentence	28 70.0%	7 17.5%	5 12.5%
	parole eligibility	25 62.5%	10 25.0%	5 12.5%
Council ($n=51$)	prison sentence	18 35.3%	21 41.2%	12 23.5%
	parole eligibility	20 39.2%	20 39.2%	11 21.6%
Extended Council ($n=48$)	prison sentence	28 58.3%	12 25.0%	8 16.7%
	parole eligibility	21 43.8%	17 35.4%	10 20.8%
Total	prison sentence	74 53.2%	40 28.8%	25 18.0%
	parole eligibility	66 47.5%	47 33.8%	26 18.7%

Note. Entries denote the frequencies with which judges did not change/
 increased/decreased the number of years of imprisonment, and the number
 of years after which the defendant was to be eligible for parole.

Changes in Prison Sentence and Parole Eligibility Decisions
as a Function of Confidence in the First Decision

Changes in prison sentence and parole eligibility decisions were also systematically related to the degree of confidence participants expressed in their first prison sentence and parole eligibility decision. The best indicator of this relationship is the biserial correlation (Ferguson, 1976) between participants' confidence ratings and the occurrence of change in their decisional behavior. Table 10 shows the within-cell biserial correlation coefficients for the no-council, council, and extended council conditions.

The negative correlations indicate that participants who were less confident in their original decision were generally more likely to change their decision after the experimental interventions. This relationship occurred clearly in both the no-council and the council condition (all coefficients were highly significant) but it was not evident in the extended council condition.

Changes in Decision Confidence

Besides the relationship between confidence ratings and likelihood of decision change it was also of interest whether individuals' confidence in their decision would change as a function of the council discussion / opinion-writing interventions. Two 2X3X2 ANOVA's, with two between-subjects factors (subject sex X council) and one

Table 10
 Within-cell biserial correlation coefficients
 between confidence in the original decision and
 occurrence of decisional change ($N = 139$)

Council Conditions		r	p less than
	Confidence in Decision regarding		
No	prison sentence	-.404	.005
Council ($n=40$)	parole eligibility	-.416	.005
Council	prison sentence	-.319	.025
	parole eligibility	-.562	.001
($n=51$)			
Extended	prison sentence	-.044	n.s.
Council ($n=48$)	parole eligibility	-.142	n.s.

Note. All ratings are on a scale from 0 (not at all confident) to 10 (extremely confident). Number of participants are in parentheses.

within-subjects factor (time: before/after council discussion / opinion-writing) were conducted on the confidence ratings for the prison sentence and parole eligibility decisions. Table 11 shows the mean ratings of confidence in prison sentence and parole eligibility decisions before and after council discussion / opinion-writing.

Both ANOVA's yielded highly significant main effects for the time factor but no significant effects for the expected time X council interaction. Confidence ratings were much higher after the experimental interventions than before (confidence in prison decision: before: $\underline{M} = 5.12$, after: $\underline{M} = 6.43$, $\underline{F}(1,133) = 70.11$, $p < .001$; confidence in the parole decision: before: $\underline{M} = 5.53$, after: $\underline{M} = 6.60$, $\underline{F}(1,133) = 40.62$, $p < .001$).

Ratings of confidence in the prison sentence decision showed a subject sex X time interaction that reached significance ($\underline{F}(1,133) = 3.90$, $p < .05$), indicating a stronger increase in confidence for females than for males. The sex of subject main effect was marginally significant ($\underline{F}(1,133) = 3.42$, $p < .067$).

Ratings of confidence in the parole eligibility decision, produced a significant main effect for subject sex with males as a group ($\underline{M} = 6.45$) displaying more confidence than females ($\underline{M} = 5.68$, $\underline{F}(1,133) = 4.15$, $p < .044$). The subject sex X council X time interaction was also significant ($\underline{F}(2,133) = 3.99$, $p < .021$), apparently due to the

Table 11
 Mean ratings of confidence in
 prison sentence and parole eligibility decisions before and after
 council discussion / opinion-writing (N = 139)

Council Conditions	Confidence	Before		After	
		Male	Female	Male	Female
No	prison sentence	5.33	4.05	6.39	5.95
Council	parole eligibility	6.78	3.77	7.11	5.77
		(18)	(22)	(18)	(22)
Council	prison sentence	5.84	4.54	6.80	6.08
	parole eligibility	5.28	5.35	7.12	6.42
		(25)	(26)	(25)	(26)
Extended	prison sentence	5.74	5.10	6.74	6.52
Council	parole eligibility	6.11	6.05	6.78	6.81
		(27)	(21)	(27)	(21)
Marginal means					
	prison decision	5.67	4.55	6.82	6.06
	parole eligibility	5.99	5.06	6.91	6.31

Note. All ratings are on a scale from 0 (not at all confident) to 10 (extremely confident). Number of participants are in parentheses.

large initial differences between males and females in the no-council condition (see Table 11). However, the subject sex effects, and their interactions, need to be interpreted with caution. Because there were no multivariate tests conducted with the repeated measures design, the probability levels for the two dependent variables have to be halved, rendering all these effects at best marginally significant.

Predicting Prison Sentence and Parole Eligibility
Decisions from General Dispositions

The overall design of this study can be split up into two groups of participants. At the beginning of the experiment, one group (the so-called "pretest" group) was administered the Sentencing Philosophy Scale, the other group (the so-called "no pretest group") was given the Authoritarianism Scale (F Scale). Forward stepwise multiple regression analyses were performed for both groups to investigate how well the scores from the individual measures of the Sentencing Philosophy Scale and the total F test predicted individual participants' posttest scores on the prison sentence and parole eligibility measures.

Of the five Sentencing Philosophy Scale measures, only the first two accounted for more than one percent of the variance of the prison sentence decisions (belief in the importance of special deterrence: 1.2%, $r = .109$; belief in the importance of punishment: 1.1%, $r = -.090$; together: 2.3%). Prediction of the parole eligibility

decisions was somewhat better, with the belief in the importance of incapacitation accounting for 2.2% ($r = .150$), and the belief in the importance of special deterrence ($r = .144$) and of punishment ($r = .114$) accounting for additional 1.8 and 1.1% of the variance, respectively (together: 5.2%).

The total score of the Authoritarianism Scale accounted for 2.5% of the prison sentence decisions ($r = .157$), and 4.3% of the parole eligibility decisions ($r = .207$). Generally, among the participants who changed, increases in both prison sentence and parole eligibility measures tended to be more frequent than decreases across all conditions (cf. Table 9). These differences were significant for the parole eligibility measure ($F(1) = 6.04, p < .02$), and marginally significant for the prison sentences ($F(1) = 3.46, p < .07$).

CHAPTER VII

DISCUSSION

Reduction of Variability

The findings of this laboratory simulation strongly and reliably indicate that group discussion is an effective means for reducing within-council variability in sentencing behavior. The nature of the Solomon-four-group design, and the extension of that design employed in this study, allows for several independent replications of this effect within a single study (Campbell & Stanley, 1966). As predicted, the sentencing behavior of the mock judges was substantially less variable after discussion in both types of council, irrespective of whether or not judges had written their sentences prior to discussion.

This finding replicates Zimmerman's (1976) findings with different case materials and extends them. The present study demonstrated, like Zimmerman's study, that post-discussion sentences were less variable not only when judges had put down their initial sentencing suggestions on paper but also when they had entered discussion without yet having written them down. In fact, data clearly indicated that judges' post-discussion sentencing behavior was

substantially less variable when they had not committed themselves through writing. Although there are no data from this experiment that explain why this commitment effect occurred, several areas of research in social psychology have generated data consistent with it.

Both the literatures on attitude change (e.g. Kiesler, 1971; Kiesler, Collins, & Miller, 1969) and on conformity (e.g. Deutsch & Gerard, 1955) would lead one to expect that commitment to a particular position (in this case writing down a prison sentence) would render one more resistant to influence attempts (e.g. in the group discussion) and therefore would reduce the likelihood of change in position at the second assessment. In the current study the commitment toward consistency was not terribly strong, as indicated by the relatively high frequencies of change observed as a function of discussion in both council conditions, and even as a function of opinion-writing in the no-council condition. A commitment effect also can be expected on the basis of a study by Pepitone & DiNubile (1976) of defendants. That study demonstrated that the public recording of ratings and punishments created an anchoring of these judgments.

If one focusses on the no-pretest conditions, the observed effect also lends support to the interpretation that the defendant was perceived as an ambiguous stimulus object. One may also speculate that the stronger convergence toward the mean (although not directly

observable in the no-pretest condition) reflects a norm formation process typical of the judgmental processes observed in the Sherif tradition (Sherif, 1935, 1936; Sherif & Sherif, 1969). It is easier for judges to arrive at a common norm (i.e. an appropriate sentence) when they are not yet committed through their pre-council decision.

The tentative nature of these speculations about the underlying causal processes, in the absence of direct data within this study, must be emphasized. Moreover, in places the data are inconsistent with these interpretations. For example, these speculations do not explain why there was only a main effect for the pretest but no significant council X pretest interaction. One would expect such an interaction on the basis of the theorizing advanced here. Of course, the difference in within-council variability in the no-council conditions is necessarily due to chance as the triplets were constituted post-hoc through randomization.

It should also be noted that there were no significant differences in reduction of within-council variability between the normal sentencing council in which judges discussed prison sentences and parole eligibility and the extended sentencing council in which judges also discussed the goals of judicial sentencing in general terms. In other words, the discussion of sentencing goals did not lead to a reduction of within-council variability over and above the reduction achieved through the discussion of sentences and

the reasons for the sentences. It is likely that mock judges also discussed the sentencing "goals" when they were asked to discuss the "reasons" for their prison sentence and parole eligibility decisions. There is evidence that the extended sentencing council was effective in reducing overall variability across judges (see below).

Generally, however, the hypothesis regarding reduction of variability across judges did not fare as well as the one regarding reduction of variability within councils. These data do not indicate that variability in decision-making across all judges was reduced as a function of the council intervention. The only exception was the significant before-after reduction of variability observed for both dependent measures as a function of discussion in the extended sentencing councils in which judges discussed general sentencing goals in addition to prison sentences and eligibility for parole. Thus, Sporer's (1980b) findings regarding reduction of variability across judges was replicated in the extended council condition but not in the normal council condition.

A variety of reasons can be offered to account for this difference in findings. Whereas in the present study mock judges judged a defendant convicted of armed robbery, the previous study involved the judgment of a case of rape which generally is considered a much more serious crime (cf. Coombs, 1967; Sporer, 1978, 1979, 1980a). The response scales in the rape study ranged from zero to fifty years (as

opposed to zero to twenty years in the present investigation) allowing for much greater initial overall variability which was reduced as a function of the discussion intervention. Of course, the fact that the first study involved law students whereas this study was conducted with college students could also account for the difference in findings.

In sum, the present study indicates that sentencing councils may be a very effective means to reduce within-council variability, especially when judges do not write down their sentences beforehand. However, the evidence that sentencing councils may also reduce variability across a large number of judges is at this point equivocal. Future research should focus on the question of whether or not overall reduction of variability might be restricted to some controversial cases in which the original distribution of sentences is generally widely spread as well as on which discussion formats are most likely to reduce overall variability.

Penalty Shifts and Patterns of Change.

The penalty shifts (leniency and/or severity shifts) observed by previous investigators as a function of group discussion both in legal settings and in the laboratory (cf. the review of the literature in Chapter IV) were not obtained in the present study. Neither the polarization hypothesis, nor the leniency hypothesis a laboratory artifact

created by selective was supported by these data. In fact, the overall higher frequency to increase, rather than decrease, the penalties observed in both council conditions as well as in the opinion-writing condition (cf. Table 9) might be more indicative of a severity shift that is not restricted to group discussion.

Thus, the leniency shift observed by other investigators appears to remain a relatively unstable phenomenon that is likely to be case-specific, or, worse, an artifact due to selective reporting and publication (cf. Greenwald, 1975). It is also possible that the jury simulation paradigm, which has produced the majority of the reported leniency shifts is genuinely different from the sentencing council simulation paradigm employed here. For example, role-playing a juror may activate different values than role-playing a judge. These values of jurors may be more easily enhanced by group discussion (Lamm & Myers, 1978). Concretely, jury simulations that require both the finding of a verdict and the giving of a sentence may be dominated by the "innocent until proven guilty" Kalven & Zeisel, 1966) and the "beyond a reasonable doubt" norms emphasized by the American judicial system and particularly strongly espoused by American college students (Silzer & Clark, 1978; cf. also Cvetkovich & Baumgardner, 1973; Rumsey, 1980) while judicial sentencing simulations may be more likely to elicit quite different considerations (e.g. "safety in the streets," or, conversely,

"rehabilitation--not warehousing," see Sporer, 1980a). These differential values may then become intensified during group discussion, and thus lead to either leniency, severity, or polarization shifts (cf. also Cvetkovich & Baumgardner, 1973). Future investigators would be wise to distinguish more specifically between these paradigms (cf. Sporer, 1978, 1979, 1980a and b; Vidmar, 1979; Weiten & Diamond, 1979). They will also have to specify more precisely the conditions (e.g. case characteristics, mode of discussion and decision-making etc.) under which penalty shifts (leniency and/or severity) are expected to occur.

These suggestions are not meant to imply that leniency and/or severity shifts remain a possibility we will have to reckon with. Instead, it appears that the sentencing paradigm does not allow for a satisfactory definition of a subjective neutral point against which polarization (leniency and/or severity shifts) can be referenced (cf. Myers & Lamm, 1976; Rumsey, 1980). Therefore, the polarization hypothesis cannot be tested effectively within this framework. Generally, the results seem more indicative of an "averaging effect" (Baron & Roper, 1977), i.e. convergence toward the mean typical of judgments of ambiguous stimuli in the Sherif tradition (see Hofstatter, 1971; Moscovici, 1974; Jenness, 1932; Sherif, 1935, 1936; Sherif & Sherif, 1969). This averaging effect is restricted to the council conditions of the experiment in which most of the changes occurred, and therefore appears to have been a

true group (or at least an information exchange) phenomenon.

It should be noted, however, that there was also a sizable number of changes (30% percent in the prison sentence and 37.5% in the parole eligibility decisions) in the no-council condition in which judges wrote a sentencing opinion but did not discuss the case with fellow judges. Theories of group processes cannot account for these changes. Of course, these changes simply may be attributable to chance. But this probably holds only for a few cases. For the majority of these changes we should search for a better explanation.

Recent theorizing and research on "self-generated attitude change" (Tesser, 1978; Tesser & Conlee, 1975) may shed some light on some of these processes. Adopting a more dynamic view of attitudes, these authors postulate that

(a) for various stimulus domains persons have naive theories or schemas which make some attributes of the stimuli salient and provide rules for inferences regarding other attributes; (b) thought, under the direction of a schema, produces changes in beliefs, and these changes are often in the direction of greater schematic and evaluative consistency; (c) attitudes are a function of one's beliefs. Since thought tends to make beliefs more evaluatively consistent and attitudes are a function of beliefs, thought will tend to polarize attitudes. (Tesser, 1978, p. 290)

As these writers use the terms attitude, evaluation, and affect interchangeably (Tesser, 1978, n.3), sentencing judgments may be encompassed by their general theory. As applied to the present study, judges would be supposed to invoke certain schemata about the defendant as a function of

reading about the defendant e.g. "he was armed--he must be a dangerous criminal", or, conversely, "he pleaded guilty--he must be somewhat honest?"). Consequently, as a function of further thought (writing the sentencing opinion), these schemata, according to the theory, generate further cognitions consistent with these schemata (e.g. "he could have fired the gun and killed the store-owner"), or reinterpret inconsistent cognitions (e.g. "even though he is a repeat offender, we don't know the circumstances; he deserves another chance if he shows signs of rehabilitation"). Accumulation of these cognitions would lead to a "polarization" of the evaluative response in the direction of the initial response. It should be noted that these processes may also take place in the group discussions, capturing some of the variance of changes observed there.

As plausible as this post hoc explanation appears on inspection of the judges' sentencing-opinions, it cannot be adequately tested with the present data. The major difficulty parallels the one encountered with the group polarization hypothesis noted above: There is no definable subjective neutral point against which polarization could be referenced. Future research could investigate this possibility through the construction of cases that differ in the kinds of schemas they tend to invoke (as determined through pretesting).

It must be emphasized that the postulation of these processes is highly speculative, and more suggestive of avenues for future research than a serious explanation for the opinion changes in the present data. It should also be remembered that the sentencing opinions were generally rather balanced, weighing both pros and cons, and in the majority of cases did not lead to changes.

Confidence in Sentencing and Parole Decisions

Discussed above were several theoretical processes which could account for the decision changes observed. It is also possible to relate these changes to the confidence judges exhibited in their original prison sentence and parole eligibility decisions. Levels of confidence were originally very widely distributed across the whole scale, and were generally good predictors for the likelihood of change in the opinion-writing and normal council condition but curiously not for the extended council condition. There is no explanation for the absence of a significant relationship between decision confidence and likelihood of change in the latter condition. In the other two conditions, judges who indicated lower levels of confidence in their original decisions were generally more likely to change their decisions than judges who displayed more confidence in their decisions.

Also of interest is the impact of the experimental manipulations upon judges' decision confidence. Clearly, discussion in the sentencing councils as well as sentencing opinion writing boosted the judges' confidence both in their prison sentence and parole eligibility decisions. The impact tended to be somewhat stronger with females than with males, largely due to the fact that males tended to be more confident in their decisions initially. This pattern of results makes sense in light of the fact that participants were asked to role-play judges who (at least at the time of this writing) are more likely to be males than females.

Predicting Decision Outcomes from General Dispositions

The small amount of variance of the prison sentence and parole eligibility decisions accounted for by the Sentencing Philosophy Scale and the Authoritarianism Scale (F Scale) is disappointing. It reflects just another instance of an issue both personality psychologists and social psychologists have struggled with for decades: The inability to predict specific behaviors from more general dispositions. The disillusionment with the trait approach in the psychology of personality (e.g. Mischel, 1968), and the perennial revivals of the attitude-behavior consistency controversies (e.g. LaPiere, 1932; Wicker, 1969) attest to this point.

A comparison of the behaviors elicited by the two attitude questionnaires (Sentencing Philosophy Scale and F Scale) and the measures of sentencing decisions can help us understand the weak relationships observed. For example, the F Scale measures general personality dispositions / attitudes presumed to be fairly consistent over time and situations while the judgment of a criminal offender is much more under the control of a particular stimulus situation (i.e. the particular case characteristics as well as the situational context). Of course, this somewhat contradicts the analogy of the defendant as an ambiguous stimulus object which would lead one to expect that internal factors would exert a more potent influence on sentencing judgments (e.g. Sherif & Sherif, 1969). Present data, however, suggest that the F Scale is not systematically related to the judgments in this particular stimulus domain, at least for the population sampled, and therefore that it does not adequately assess these underlying factors.

The lack of a relationship between the mock judges' scores on the Sentencing Philosophy Scale and the sentencing decisions is, at least on the surface level, more disappointing. It is likely that it is due to the lack of discriminatory power of the items on this questionnaire. Most participant mock judges endorsed almost all of the five sentencing goals (punishment, general deterrence, special deterrence, incapacitation, and rehabilitation) as more or less important. Thus, they did not display the level of

variability that is a prerequisite for a good predictor. The lack of predictive utility of the Sentencing Philosophy Scale runs counter to Hogarth's (1971) study of Canadian sentencing magistrates. He found judges to vary greatly with regard to the sentencing goals they endorsed, which in turn captured a major portion of the variance of their sentencing decisions. Recent laboratory investigations by McFatter (1979) and Austin (1979) also demonstrated the importance of selective sentencing goals for the determination of levels of punishment. Therefore, the lack of a relationship between the Sentencing Philosophy Scale and the actual sentencing decisions found in this study could be attributed to the lack of differentiation of sentencing goals in the population of college students sampled. It is possible that the lack of experience of mock judges has prevented them from developing the relevant schemata (cf. Stotland & Canon, 1972; Tesser, 1978) that typically would be expected to be well-represented in real judges.

CHAPTER VIII

VALIDITY AND IMPLICATIONS

Inductive Approaches to Validity:

Population and Ecological Validity

The present experiment was designed to contribute to the solution of a social problem observed in courts: The issue of sentencing disparity. Before drawing any conclusions on the basis of this research regarding applicability to real world problems it is vital that we come to grips with a fundamental issue facing all laboratory research on legal issues: The relationship between laboratory research and the actual behavior of legal actors in legal settings.

Traditionally, this question is couched in the language of generalizability (or external validity): How much can we generalize from this laboratory simulation to the real world of the courtroom? More specifically: How much can we generalize from the sample under study (college students) to the target population of judges (population validity), and how much can we generalize from the context of the laboratory setting to the actual legal setting (ecological validity; cf. Bracht & Glass, 1968, regarding this

distinction). Consequently, according to this view, the external validity of experimental laboratory simulations is measured on the criteria of how representative their participants are of the target population, and how closely they simulate the actual legal context.

The present study does not fare well if evaluated against these criteria. College student mock judges are several steps removed from the highly specific, narrowly circumscribed target population of sentencing judges. Sentencing judges are a subgroup of the population that differs in age, educational level, legal socialization (Levine & Tapp, 1977; cf. also Erlanger & Klegon, 1978) and in many other ways from college students. These differences in legal expertise are likely to precipitate some differences in decision-making processes. For example, Konecni and Ebbesen (1979) found judges, probation officers, and college students to differ with respect to some of the factors that seemed to have influenced their decisions in a simulation experiment on judicial sentencing. Judges may also be influenced by their past decisions in relatively similar--or different--cases during their experience on the bench (cf. also Pepitone & DiNubile, 1976).

Relatedly, Sporer (1980a) found that law students who could be seen as somewhat intermediate between these two populations on these dimensions, differed from college students with regard to their general punitiveness (they were less so). Moreover, law students and college students

were differentially affected by some of the case characteristics under investigation. Generally, selection X treatment interactions, as observed in that study, pose serious threats to external validity of these types of studies (Campbell & Stanley, 1966; Cook & Campbell, 1979).

However, despite these general attitudinal differences and the differential impact of some of the case characteristics, in that same study, there were striking similarities to the present study in the reduction of variability in sentencing behavior which occurred as a function of group discussion. Similarly, Zimmerman's (1976) simulation of sentencing councils with law students found reliable reduction of variability as a function of group discussion. Procedurally, his study differed significantly from both Sporer's (1980b) and the present study.

An important consideration in evaluating the use of nonexpert decision makers as participants in simulations of sentencing studies is that the logical alternative, i.e. to use real judges, may be fraught with even more interpretational difficulties. For example, judges would be likely to be well aware of the disparity problem, and therefore, would be likely to refrain from any extreme judgments or any form of socially or legally undesirable bias under the scrutinous observation of the scientist representing the eye of the public (cf. Diamond & Zeisel, 1975). Consequently, their behavior in a simulation experiment may not at all reflect the nature of their

behavior in the courtroom. We know from studies on bail decision-making that judges are likely to respond in a socially desirable fashion to questionnaires (Ebbesen & Konecni, 1975; cf. also Konecni & Ebbesen, 1979). To conduct the present study with real judges, without creating awareness of the experimental hypotheses, and the concomitant response biases, would no doubt be extremely difficult.

The ecological validity of the present study is also rather limited. Besides the obvious dissimilarities between the set-up of this study and an actual courtroom, all laboratory simulations of judicial sentencing have to face their most serious short-coming: The so-called "paper-defendant" problem (Partridge & Eldridge, 1974). The sentence is given to a hypothetical defendant who exists only on paper, i.e. (1) without the face-to-face contact characteristic of the sentencing hearing, and (2) without any real consequences for a real person--e.g. the vicissitudes of long-term incarceration (cf. also Austin & Utne, 1977; Austin & Williams, 1977). Although it seemed that participants in this experiment took their task as judges quite seriously, one should not generalize directly from the results of this experiment to the sentencing behavior of judges.

Deductive Approaches to Validity and
Criteria of Research

It was never the aim of this study to provide for direct generalization to the courtroom. But we should not approach generalizability this way. Implicit in these conceptions of external validity (population and ecological validity as discussed) is an inductivist view of science (Gadene, 1976; however, Cook & Campbell deny this) which is no longer tenable from a philosophy of science point of view (Hempel, 1966; Popper, 1959). Alternative ways to conceptualize these issues, based on a deductive, theory-testing approach, are available (e.g. Gadene, 1976; Irle, 1975; Kruglanski & Kroy, 1976). Such deductive approaches have recently been adopted to the social psychology of legal issues (cf. Opp, 1970). Lind and Walker (1979) make this point most succinctly:

We also argue below that empirical studies designed to test specific theories of legal behavior are not evaluated properly by the simulation-oriented criterion of methodological closeness to some actual setting and population. Rather, such studies should be judged with reference to the theoretical principles they seek to test. A study designed for theory-testing purposes is good to the extent that it provides an unambiguous test of some essential implication of the theory. Practical application of the results of this sort of study is not accomplished directly, but comes rather from the application of the theory itself to legal questions. (p. 6)

Although their statement also applies to the present study, an important difference should be noted. They are in a much better position because they can rest their argument on a well-grounded theory of legal behavior, viz. their

theory of procedure (Thibaut & Walker, 1978). Yet there is no single well-developed legal or psychological theory of discretion or group decision-making that could be used as an encompassing framework for the present study.

Rather, we have to choose from a whole gamut of theories some of which may or may not apply. Therefore, additional criteria of research become necessary. Besides the traditional criteria of scientific research put forth by philosophers of science, e.g. logical consistency, falsifiability etc. (cf. Groeben & Westmeyer, 1975), the criteria of technical and emancipatory relevance (Habermas, 1965; Holzkamp, 1972; Sporer, 1980c) can help us decide among alternative theories. The present approach has adopted an interventionist paradigm that chooses among theories not only on the basis of their value in generating understanding the problem of sentencing disparity but also their potential in pointing out ways to reduce it. For example, general theories of norm formation (e.g. Moscovici, 1974; Sherif, 1935, 1936; Sherif & Sherif, 1969) that would lead one to expect an averaging effect (Baron & Roper, 1977) as a function of group discussion, or theories of group polarization (see Lamm & Myers, 1978) that would predict penalty shifts, were preferred to theories that would not imply the possibilities of structural interventions / innovations.

Construct and Dependent Variable Validity

However, this theory-oriented approach does not rid us of our responsibility to discuss validity issues in more detail within this deductive framework. In particular, construct validity (Cook & Campbell, 1979; cf. Kruglanski & Kroy's, 1979, independent variable, dependent variable, and effect validity) and dependent variable validity (Kruglanski & Kroy, 1976) need to be further elaborated. With regard to construct validity of the postulated causal relationship between group discussion and reduction of variability, there is now accumulated evidence that the normalization processes (averaging effect) expected from theory (e.g. Moscovici, 1974) are likely to operate in sentencing councils as well. Zimmerman (1976), Sporer (1980b) and this dissertation all support this position through constructive replications with different case materials, different experimental procedures (and different participant populations). Moreover, converging evidence from anecdotal reports and archival analyses of courts employing sentencing councils (see Chapters III and IV) satisfy the requirement of multiple operationism (Campbell & Stanley, 1966; Cook & Campbell, 1979). Such evidence, despite the short comings, increases our confidence that these processes may well operate in the field as well as in the laboratory.

Further evidence for construct validity is provided by the finding indicating that the reduction of variability effect was stronger for unpretested than for pretested councils, by indicating differential effects for differential manipulations (cf. Cook & Campbell's idea of "divergence between manipulations," 1979, p. 61). The pretest effect also provides support for the operation of norm formation processes which would lead one to expect lesser variability when judgments are started with the group than when starting with individuals (Sherif, 1935).

The issue of dependent variable validity (Kruglanski & Kroy, 1976) needs also to be discussed. In many ways this issue resembles the "paper defendant" problem discussed above within the framework adopted of ecological validity. However, the deductive framework here sheds new light on this issue. It also highlights the differences between these approaches. For example, based on the criterion of "closeness to the actual legal setting," as demanded by ecological validity, one would attempt to provide sentencing alternatives to mock judges that portray the sentencing alternatives available in the actual legal setting as faithfully as possible. This may work with legally sophisticated simulation participants, e.g. advanced law students or real judges, but it also could distort the response pattern of legally less sophisticated participants (e.g. college students). For example, the present study employed the term "parole eligibility after X Number of

years" which was to indicate to legally unsophisticated mock judges the number of years after which the defendant could, at the earliest time, be released from prison. For legally sophisticated participants, this sentencing option would generate much more complex computations, such as for example "minimum sentence minus good time credit" etc. (cf. American Bar Association Commission on Correctional Facilities and Services, 1974) Yet for an adequate deductive test of the research propositions under question, (i.e. the reduction of variability or the penalty shift hypotheses) it is not important that participants can adequately utilize the whole gamut of sentencing alternatives. Rather, it is important that the changes in the dependent variables adequately reflect the changes postulated by the theory. We are not interested in the absolute magnitude of the sentences etc. proposed but in their pattern of changes as a function of the experimental interventions.

One major aspect of dependent variable validity, the "paper defendant" problem, cannot easily be overcome by laboratory simulations. However, we have to consider the alternatives. For obvious ethical and legal reasons, experimentation involving the fate of actual defendants is ruled out. This leaves us with post facto archival analyses which raise an entirely new set of methodological problems and problems of interpretation (cf. Webb et al., 1966).

In summary, the theory-testing interventionist approach promulgated here provides a viable strategy to study legal issues and to contribute to their solution by testing the assumed effects of structural interventions. Its value, however, does not lie in its direct applicability to the actual legal setting but rather in the testing of theories which provide unambiguous insights into the (causal) relationships between the variables under study. These theories, in turn, can be fruitfully applied to the real world setting. Of course, this approach should be supplemented, whenever and wherever possible, by more naturalistic "proximal" simulation experiments and in situ evaluation studies (Lind, 1978). Only through the use of multiple methodologies, in connection with theory, will we be able to arrive at an adequate understanding of and significant contributions to complex social problems such as sentencing disparity.

Implications for Policy

Despite the restrictions implied by the foregoing discussion, it should be possible to derive some tentative implications for the policy of judicial sentencing. The massive evidence on the existence of sentencing disparity (see Chapter I) has made the call for solutions more urgent than ever. The faith of the public, and particularly of the subpopulation of criminal offenders, in the criminal justice system depends to a large extent on the perceived fairness

(and effectiveness) of that system.

The present study has accumulated empirical evidence that sentencing disparity may indeed be reduced as a function of group discussion among council members. The evidence is particularly strong with regard to the reduction of variability within councils. There is mixed evidence--partial but weak support from this study, and stronger support from a preliminary study (Sporer, 1980b)--that council procedures may also reduce variability across large numbers of judges. Based on this evidence, the implementation of sentencing councils is recommended, at least for a restricted number of cases (e.g. felony cases) on a trial basis. Only when sentencing councils are instituted on a large scale basis, will we be able to evaluate their impact more systematically.

Of particular interest is a procedural change suggested for the mode of operation of sentencing councils derived from theories of norm formation processes and theories of attitude change, and supported by data from the present study. It is recommended that council members do not (as has been common practice in the few existing sentencing councils on which information on this point is available) record their recommended sentence on the so-called "Sentencing Study Sheet" (see Levin, 1969) but rather enter the discussion without having committed themselves in this form through writing. Theories of norm formation and of attitude change and commitment, corroborated by evidence

from the present study, indicate that writing down a sentence prior to discussion is likely to reduce council members' susceptibility to the moderating effects of the group. It is suggested that this procedural change be adopted in a systematically determined portion of the cases, and subjected to rigorous scientific evaluation in situ.

The council discussion experience is also likely to enhance the confidence of judges in their decisions. However, this should not be interpreted to mean that council members' reliance on other members' judgment would jeopardize their cherished independence of judgment. The council's function is to be seen as purely advisory. But the impact of the council discussion can be expected to be pervasive despite the stress on the independence of judgment, as indicated by data from this and from the previous study mentioned above.

However, the enthusiasm about the disparity-reducing quality of sentencing councils should not be overextended. Until it is better established that sentencing councils substantively reduce disparity across judges and jurisdictions, (and even if this were demonstrated more conclusively) we must search for additional means to balance the principles of equality before the law and flexibility in decision-making. Sentencing councils provide ways that demonstrate that these goals are not necessarily contradictory but can be reconciled at a higher level of integration. Other promising alternatives might be the

appellate review of sentencing (see Chapter II), and sentencing guidelines.

Appellate review of sentencing would seem a necessary counterbalance to one potential problem of sentencing councils--extreme judgments as a function of group polarization. Sentencing guidelines could be fruitful to provide judges (and councils) the frame of reference within which to proceed. The theories advanced in this thesis could also be applied to a better understanding of the impact of guidelines upon sentencing decisions.

Consideration of these reform proposals should not take place in isolation but rather such consideration needs to be related to the operation of the criminal justice system as a system. For example, the feasibility of adoption of sentencing councils, as well as their potential impact upon the backlog of cases etc., must be carefully evaluated. It is argued that short-sighted economic arguments are likely to be detrimental to the functioning of the criminal justice system, and to society as a whole. Other reform proposals of imminent appeal, such as "flat-time sentencing" are likely to displace the disparity problem to other agents in the system, e.g. the prosecutor.

Conclusion

The empirical approach taken in this thesis can aid us in understanding and evaluating the actual and potential impact of structural legal interventions from a social

psychological perspective. It draws heavily on the existing body of legal and psychological theory in pinpointing the processes and evaluating and predicting the outcomes of these interventions. But the promises of this approach need not be restricted to passive investigation of the status quo. It encompasses creative components that may provide the leverage point for constructive change.

Other possibly more important issues are beyond the scope of the present approach. They can only be resolved at a meta-level by philosophers or society at large. For example, the problem of sentencing disparity may be deeply rooted in the foundations of criminal sanctions in diverse penal philosophies. Until society at large comes to grip with some of these philosophical issues through democratic means and delineates specific behavioral prescriptions with regard to type and severity of criminal sanctions from general principles of sentencing, individual judges (and other societal agents) are likely to follow their own subjective interpretation of these abstract principles. Disparity ensues. Structural and procedural checks are needed to bridle it.

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APPENDIX

CASE SUMMARY

In the late afternoon hours of October 10, 1975, the defendant, twenty-eight year old George F. Watson, entered a small grocery store pretending to buy groceries. When the defendant was alone in the store with the owner he walked up to him and pretended to pay for a six-pack of beer. When Paul Gregory, 63, the owner, opened the cash register the defendant pulled a loaded gun and pointed it at Gregory. He stepped around the counter toward the register. Gregory was nervously shaking and was slow to move out of the way. The defendant pushed him aside and hurriedly emptied out the cash from the register. He left with a total of \$345 taken from the store. Another customer who was just about to enter the store observed part of the incident through the shop window and immediately reported it to the police. The police were able to apprehend the defendant at his apartment soon thereafter. He was charged with armed robbery and pleaded guilty after he had been identified by the store owner and the second customer. However, the money was never recovered. The probation officer studied the defendant's police record which showed that he had been convicted of armed robbery once before. He therefore recommended that George F. Watson should be given a prison sentence.

GOALS OF SENTENCING

1. Rehabilitation. The attempt to change the offender through treatment or corrective measures, so that when given the chance he will refrain from committing crime.
2. General deterrence. The attempt to impose a penalty on the offender before the court sufficiently severe that potential offenders among the general public will refrain from committing crime through the fear of punishment.
3. Individual deterrence. The attempt to impose a penalty on the offender before the court sufficiently severe that he will refrain from committing further crime through fear of punishment.
4. Punishment. The attempt to impose a just punishment on the offender, in the sense of being in proportion to the severity of the crime and his culpability, whether or not such penalty is likely to prevent further crime in him, or others.
5. Incapacitation. The attempt to protect society for a period of time by removing the offender from the community into prison.