An Evaluation of the Civil Appeals Management Plan: An Experiment in Judicial Administration

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AN EVALUATION OF THE CIVIL APPEALS MANAGEMENT PLAN:

AN EXPERIMENT IN JUDICIAL ADMINISTRATION

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EXECUTIVE SUMMARY

The Civil Appeals Management Plan (CAMP), now operating in the United States Court of Appeals for the Second Circuit, is an innovative set of reforms in the appellate process. The plan has two unique features. The first is the use of scheduling orders that the court issues in all civil appeals to notify counsel about the deadlines for critical events (e.g., filing of briefs or expected week for oral argument) in the course of an appeal. The appeal may be dismissed for failure to comply with the order. The second feature is the use of preargument conferences in selected civil appeals. These conferences are authorized under the Federal Rules of Appellate Procedure, but this is the first time the conference procedure has been implemented systematically.

CAMP began in 1974 with initial financial support from the Federal Judicial Center. As a condition for its support, the Center also mandated an evaluation of CAMP. The evaluation was conducted as a controlled experiment in which appeals deemed eligible for CAMP procedures were randomly assigned, over a period of one year, to an experimental group or a control group. Cases in the experimental group received the full application of CAMP; cases in the control group received none of the CAMP procedures. This research approach provided the best assurance that the two groups were alike in all respects save one: CAMP procedures applied only to the experimental group. Measures of CAMP goals were taken in each group. A statistically significant difference in these measures between experimental and control groups would justify a conclusion that CAMP is effective.

The goals of the plan are: (1) to eliminate appeals which otherwise would impose a burden on the judges; (2) to improve the quality of appeals should they be decided by the court; and (3) to expedite the appellate process. The plan is supervised by a senior staff attorney who conducts the preargument conferences.

There is no judge participation in CAMP activities. If effective, CAMP will stabilize or reduce the judicial burden imposed by mounting appellate litigation without truncating court processes.

Does CAMP reduce the burden on the judges? Since judge burden was not measured directly, a reduction in that burden must be inferred from the way the cases terminate and the extent to which they require judge attention. Three overlapping measures were used to test this goal.

1. If CAMP reduces judge burden, there should have been substantially fewer adjudicated appeals (nonprocedural dispositions by three-judge panels) in the experimental group than in the control group. No statistically significant difference in the proportion of adjudicated appeals was observed between the two groups.

2. If CAMP reduces judge burden, the experimental group should have had substantially fewer appeals involving some judicial investment (at minimum, judicial investment meant a decision by three judges on a contested substantive motion) than the control group. No statistically significant difference in the proportion of cases involving some judicial investment was observed between the groups.

3. Finally, if CAMP reduces judge burden, there should have been considerably fewer briefed and argued appeals in the experimental group than in the control group. Here the plan seems to offer the greatest promise of relief to hard-pressed appellate judges, since briefed and argued appeals, on the average, require the largest share of judges' attention. No statistically significant difference in the proportion of cases that were briefed and argued was observed between the groups.

The evidence in each of these measures pointed favorably to the plan, but the magnitude of the difference between the two groups was not sufficient to warrant a conclusion that CAMP--not chance factors--caused the difference. These results do not automatically lead to the conclusion that CAMP is without merit. For some of these performance measures (perhaps all three), a Scotch verdict on the plan is appropriate. That is, on the basis of a year of activity, CAMP has not yet been proved effective, but it has not been proved ineffective. Further controlled experimentation, under different circumstances, appears warranted in order to exhaust the promise offered by the plan and its architects.

CAMP was also designed to enhance the quality of appeals that survived to argument and decision. Judges were surveyed to assess differences CAMP made in overall quality and to identify particular components of quality in appellate litigation. The judges in both experimental and control cases were questioned; in general, the judges had no knowledge about experimental and control designations, which might affect their responses.

If CAMP improves the quality of appeals, the experimental cases should have received a "better" score than the control cases. On all three general questions about quality and on one component-of-quality question (out of nine asked), the experimental group scored better in quality than the control group. The difference between scores was sufficient to warrant a conclusion that CAMP caused the improvement. The extent of the improvement CAMP caused, however, is estimated to be small.

CAMP was expected to expedite the appellate process, primarily through the use of scheduling orders. If CAMP is effective in this respect, the cases in the experimental group should have taken significantly less time between the notice of appeal (signaling the start of the appeal) and the termination of the appeal. The experimental cases took significantly less time in the appellate process. Further analysis indicated this expeditiousness resulted from reductions in time for settled or withdrawn appeals. If appeals proceed through the stage of briefing and argument, CAMP does not achieve any significant economies.

Attorneys in experimental and control cases were surveyed to corroborate and enhance the analysis. The survey revealed that, in general, the plan caused no statistically significant difference in improvement, except for infrequent clarification of issues on appeal. The attorney survey also suggested that about half the attorneys in the experimental and control groups met with their adversaries to discuss informal resolution of appeals. To a lesser extent, attorneys also conferred with their adversaries to limit or narrow issues. This evidence seems to contradict an underlying premise of the plan, namely, that attorneys do not usually engage in discussions with their adversaries.

Attorneys who chose to comment about the program favored the CAMP idea; and, to a lesser extent, they praised its administration. It is important to note that a substantial portion of the attorneys familiar with the plan felt it contributed to the settlement or withdrawal of their appeals. This speculation must be balanced against the case information and the judges' survey to determine whether CAMP goals have been achieved.

Convincing evidence that the plan reduces the burden on the judges was not found in this evaluation. But much of the evidence that the plan had no effect on judge burden is inconclusive. The evaluation supports the view that CAMP improves the quality of appellate litigation and expedites the process, but the magnitude of these effects appears modest.

The results of this first experiment in appellate procedure suggest suspending judgment on CAMP. It seems clear that the plan does not yet live up to early expectations, but the evidence does not prove the CAMP idea ineffective. CAMP may still have substantial effects on appellate litigation. Further experimentation under different conditions may still yield meaningful results. Rigorous evaluation is essential to any further work in this area. Without such research, effective reform of the appellate process will remain an elusive goal.

ACKNOWLEDGMENTS

This project began in February, 1974, when Joseph Ebersole--then Director of the Federal Judicial Center's Division of Innovation and Systems Development--placed the evaluation of the Civil Appeals Management Plan in my hands. From that moment, I have been engaged in a research effort that, I venture to say, has few equals in the field of judicial administration.

In fairness, the credit for this first controlled experiment in the federal courts must be distributed among many individuals who helped me from the start. My deepest gratitude goes to my friends and former colleagues at the Center, William B. Eldridge and Anthony Partridge. Both men offered their wise, creative advice at key points. I am fortunate to have shared with them the satisfactions and frustrations of developing this experiment. Their critical comments, and Joe Ebersole's, also steered me away from a number of pitfalls in an earlier draft. This report bears witness to their invaluable assistance, for I doubt the experiment would have succeeded without them.

I would like to express my thanks to former colleagues at the Center who were instrumental in bringing the CAMP project to its conclusion: Anne Ayers, Mark Bacon, Elizabeth Baker, Alan J. Chaset, Rodney Dobbins, David Durbin, Emily Fenn, Steven Flanders, Rose Hedgeman, Alfred Kleindienst, Charles Nihan, Charles Phillips, Vivian Rodgers, Alan Sager, and John Shapard.

The judges of the United States Court of Appeals for the Second Circuit made a substantial contribution to this evaluation. They carried the burden of countless questionnaires and correspondence with only an occasional complaint. Rarely have public agencies attempted such a searching examination of their own procedures.

Let me also acknowledge the cooperation of Robert D. Lipscher, Circuit Executive; Nathaniel Fensterstock, Staff Counsel; and A. Daniel Fusaro, Clerk. Others in the Second Circuit provided essential support: Liesa Bing, Robert Bingham, John Brascia, Donna Crisalli, S. Brooks Durbin, Stuart Greenbaum, Judy Hartmann, Edward Quinn, and Ida Smyer. My thanks to them all.

My colleagues at Northwestern University--Donald Campbell, James Caporaso, Kenneth Janda, and Jerome Sacks--helped me overcome major and minor roadblocks. I am indebted to them for their assistance. Douglas C. Straus assisted me in the analysis and provided critical commentaries on many facets of this study.

Whatever remains in error is now my responsibility.

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CHAPTER ONE

INTRODUCTION

Today, it is common to point to the dramatic growth in the business of federal courts over the last fifteen years.[1] While growth in court business is nothing new,[2] the increase in the business of the federal courts of appeals during the last two decades is unprecedented in the history of this institution.[3]

Demands for the services of the federal appellate courts seem to have outstripped the available supplies. There are strong suggestions that simply meeting the growth in demand with increased resources is unsatisfactory for the court of appeals. An increase in the number of active judgeships for a circuit is not without its own costs.

More panels of judges could lead to more intra-circuit conflict on similar legal issues. To minimize

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1. See, e.g., The Courts, The Public and the Law Explosion (Jones ed. 1964); Carrington, Crowded Dockets and the Courts of Appeals: The Threat to the Function of Review and the National Law, 82 Harv. L. Rev. 542 (1969); Meador, Appellate Courts: Staff and Process in the Crisis of Volume (1975); and Carrington, Meador, & Rosenberg, Justice on Appeal (1976).

2. Frankfurter & Landis, The Business of the Supreme Court (1928).

3. Commission on Revision of the Federal Court Appellate System, Structure and Internal Procedures: Recommendations for Change 1 (1975); and Hearings on S. 2991 Before the Subcomm. on Improvements in Judicial Machinery of the Senate Judiciary Comm., 93d Cong., 2d Sess. 92 (1974) (testimony of Judge Robert A. Ainsworth, Jr.).

that possibility, the judges of a circuit would have to harmonize their conflicting views by spending more of their time reviewing the draft opinions of other panels of judges or by increased use of the en banc mechanism, a clumsy tool at best when judges are scattered across a large geographical area.

Shortcuts in the appellate process to conserve judge time have been the alternative to increasing the size of the judicial plant. Some courts of appeals have truncated the appellate process by selectively limiting or denying oral argument or curtailing opinion writing.[4] This approach has not been without its critics,[5] among them federal judges and a congressional commission. They advocate other reforms, rather than altering the appellate process in ways that may have untoward consequences. While some courts of appeals have taken the lead in altering the appellate process, others have just as strongly maintained their constancy to the traditional view by hearing oral argument in all cases. Other courts have tolerated an increase in their number of judgeships beyond what was once thought the optimum.[6]

Faced with the tradeoff of increasing the pool of judges or of truncating the appellate process, the United States Court of Appeals for the Second Circuit--through its chief judge, Irving R. Kaufman--decided to experiment with an idea that might avoid the Scylla of taking appellate shortcuts and the Charybdis of increasing the number of judges beyond its present level (and optimum size, in the view of many of the Second Circuit judges) of nine active appellate judges.

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4. Haworth, Screening and Summary Procedures in the United States Courts of Appeals, 1973 Wash. U.L.Q. 257.

5. Supra Commission, note 3, at 48.

6. The "optimum" number suggested by some observers is 9. Id. at 57. If every circuit judgeship requested by the Judicial Conference were approved, 5 out of the 11 circuits would exceed this "optimum."

In Judge Kaufman's experience, many of the civil cases appealed to the Second Circuit seemed amenable to resolution short of a decision on the merits by a panel of appellate judges. No court of appeals had ever made efficacious and systematic use of prehearing conferences to encourage informal dispute resolution. Rule 33 of the Federal Rules of Appellate Procedure seemed to provide sufficient authority for settlement discussion at the appellate level.[7] Would intervention by the court early in the appellate process effectively induce the parties to resolve their differences, and thus reduce the proportion of cases presented to the court for decision?[8]

In late 1973, Judge Kaufman wrote to the Chief Justice about his experiences with the Rule 33 procedure:

> To determine whether restrained and dignified encouragement by the court would facilitate settlement, I decided to experiment personally with the procedure [of preargument conferences authorized by Rule 33]. ... Five cases were selected at random from among a group of cases which seemed

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7. Rule 33, Prehearing Conference:

> "The court may direct the attorneys for the parties to appear before the court or a judge thereof for a prehearing conference to consider the simplification of the issues and such other matters as may aid in the disposition of the proceeding by the court. The court or judge shall make an order which recites the action taken at the conference and the agreements made by the parties as to any of the matters considered and which limits the issues to those not disposed of by admissions or agreements of counsel, and such order when entered controls the subsequent course of the proceeding, unless modified to prevent manifest injustice."

8. The theoretical justification for the use of the prehearing conference to resolve disputes without judicial decisions will be found in Mack, Settlement Procedures in the U.S. Court of Appeals: A Proposal, 1 The Justice System Journal 17 (1975) (issue 2).

> to lend themselves to private dispute resolution and which were in the early stages of the appellate process. I met with the attorneys and, although my role was limited to that of catalyst, all five cases were terminated by settlements that were entirely satisfactory to the parties.[9]

Judge Kaufman's remarkable experience developed into a larger program designed to manage civil appeals. The program--as approved by the Circuit Council--would have the following components: one part would make each civil case subject to a scheduling order notifying the parties when certain stages in their appeal would take place (e.g., filing of the record, filing of briefs, date for oral argument); the other part would systematically utilize Rule 33 by holding preargument conferences in selected appeals in order to explore settlement possibilities, to otherwise improve the quality of the appeal if it was to be argued, and to facilitate supervision of the appeal.

Judge Kaufman sought the Federal Judicial Center's financial support to experiment with this departure from appellate practice. The Center's funds would be used to hire a senior attorney and staff, whose major job would be to conduct the preargument conferences for settling or otherwise improving appeals. Judge Kaufman noted there was wide disagreement about the possible effectiveness of a staff attorney in the settlement process. Some critics felt only a judge would have sufficient prestige to resolve such disputes; others were convinced that a staff attorney could do the job. "Testing under controlled conditions," said Judge Kaufman, "may be the only way to settle questions of this nature."[10]

The board of the Federal Judicial Center, at its December, 1973 meeting, approved the Second Circuit's request for support of a one-year experiment to utilize

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9. Letter from Irving R. Kaufman to Warren E. Burger (Nov. 30, 1973).

10. _Id_.

the preargument conference procedure under Rule 33. The board allocated $50,000 for the project[11] and the Center staff was charged with evaluating it. "The hope was expressed," according to the board minutes, "that the evaluation would be able to distinguish between cases which might have been settled or otherwise disposed of without intervention by the court." [12] The evaluation of the Civil Appeals Management Plan (CAMP) was begun with this mandate.

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11. The board approved an additional sum of not more than $40,484 in Jan., 1975 in order to continue the program during the evaluation. Since Center support was terminated, staff salaries have been budgeted through the Administrative Office of the United States Courts as part of the regular federal court appropriation.

12. Federal Judicial Center, Minutes of Board Meeting 2-3 (Dec. 15-16, 1973).

CHAPTER TWO

THE CAMP EVALUATION DESIGN:

FROM THEORY TO REALITY

The use of preadjudication procedures to terminate or improve litigation is neither new nor startling. Judges have long praised the virtues of pretrial procedures; however, there is a notable lack of objective evidence regarding the effectiveness of such procedures.[1] This abundance of subjective judgment and absence of objective proof makes it even more important that any evaluation of preadjudicatory procedures be as unambiguous as possible, to determine whether and to what extent CAMP is effective.

Following the board's approval of $50,000 for CAMP, the Center staff began to construct an evaluation that would be consistent with the board's mandate. A number of competing evaluation plans were considered and rejected. For example, an evaluation that relied on changes in the rate of appeals argued each year was fraught with problems. There was some doubt that information about such appeals would be consistent over the time period in question. Year-to-year fluctuations were substantial in past time periods, suggesting that any change in argument rates after the implementation of CAMP would have to be astronomical to provide proof that the plan was effective.

Perhaps the greatest limitation on evaluations comparing performance over time was the possibility of committing the <u>post hoc ergo propter hoc</u> fallacy. If a decline in the argument rate occurred following the implementation of CAMP, it would be inappropriate to jump immediately to the conclusion that CAMP caused the decline. The decline in argument rate can be caused by

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1. Rosenberg, The Pre-Trial Conference and Effective Justice (1964).

a host of other plausible relationships that would have to be ruled out before accepting the conclusion that CAMP caused the change. For example, changes in economic conditions or in legal issues from one time period to the next might account for the change in argument rate. Since the number of competing explanations for the change in argument rate is limited only by one's imagination, there would always be ambiguity in the research conclusions since, as a practical matter, all competing hypotheses cannot be ruled out.

Judge Kaufman's letter to the Chief Justice[2] suggested a different approach. CAMP was to be an experiment, wrote Kaufman, and testing the use of the senior attorney "under controlled conditions" seemed the only way to resolve doubts about his effectiveness. A Center colleague suggested that the Court of Appeals could be used as a laboratory in which to conduct a classic, controlled experiment in court procedure.

After reviewing many approaches, the Center staff proposed the evaluation be conducted experimentally, i.e., that cases meriting CAMP procedures be assigned by a truly random process to a number of groups. The division of all cases meriting CAMP attention into groups by a random process provides the greatest possible assurance that the groups are equivalent. This is so because a random process of division is blind to the characteristics of the cases. It is akin to flipping a coin, but much more exact. The administration of CAMP procedures, either separately or collectively, to one group (the "experimental" or "treatment" group), and the withholding of CAMP procedures from another group (the "control" group) provides the clearest proof that observed differences between the experimental group and the control group are caused by CAMP procedures. For example, if none of the experimental cases reached a panel of judges for decision, but all of the control cases went to the panel, it could be said with some assurance that CAMP caused the reduction in panel considerations. Similarly, if half the cases in each group reached three-judge panels, it could be said with some assurance that

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2. Letter from Irving R. Kaufman to Warren E. Burger (Nov. 30, 1973).

the CAMP procedures had no effect. In a controlled experiment, such cause and effect statements are warranted because the experimental and control groups of cases are equivalent in all relevant respects except one: only the experimental cases are subject to the CAMP procedures.

The random assignment of cases to experimental and control groups also permits a precise calculation of the probability that differences between the groups are due to chance. The researcher's willingness to tolerate such differences is determined by the degree of risk he takes when he draws inferences from the data. This risk is dependent on the number of cases and the magnitude of sampling or chance fluctuation in each group. Probability estimates are essential when the researcher is faced with results falling somewhere between the two extreme examples offered in the preceding paragraph. Thus, the researcher's first task was to determine whether to accept the hypothesis that CAMP is effective. But the job did not end with a conclusion that CAMP is effective. The next step was to estimate the magnitude of CAMP effects.

How many groups of cases should be created for such an experiment? Obviously, one for each "treatment," and a control group to provide for a basis for comparison. In the CAMP experiment, it was necessary to specify exactly what a "treatment" was. The proposal submitted by Judge Kaufman, and the CAMP Rules[3] adopted by the Circuit Council, suggested a variety of "treatments" or procedures worthy of experimentation.

The program was based on the use of two separate procedures: first, the use of a scheduling order to notify attorneys of deadlines in the processing of their appeals, with the threat of dismissal in the event of default; and, second, the use of Rule 33 preargument conferences to discuss settlement, withdrawal, or other matters that might improve the appeal if it should be decided by a panel of judges. Of course, CAMP emphasized the conference procedure, but it is at least arguable that the scheduling procedure would discourage some appeals. Hence, it seemed only reasonable to study the effects of each procedure separately and in combination.

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3. See Appendix I.

Judge Kaufman introduced another variation by noting there was a division of opinion on the effectiveness of having a senior staff attorney conduct the Rule 33 conferences. "Testing under controlled conditions," wrote Judge Kaufman, "may be the only way to settle questions of this nature."[4] This suggestion implies that in order to test the effectiveness of the senior staff attorney, the evaluation should include separate judge participation in the Rule 33 conferences. This would provide the needed proof of whether the senior attorney was more, less, or as effective as an appellate judge in reducing the proportion of cases that are fully briefed and argued, or in improving the quality of those cases that are briefed and argued.

A complete and exhaustive evaluation of CAMP would require a complex experiment (or series of experiments) in which eligible groups of cases would be given CAMP procedures separately and in combination, in order to assess the effectiveness of each procedure and of the combined procedures. Additional groups would be subject to Rule 33 conferences administered by a judge, to settle the issue of staff versus judge effectiveness.

Although this complex research design would have produced an exhaustive test of CAMP, the Second Circuit offered compelling reasons for a somewhat less comprehensive evaluation. The Second Circuit was concerned that the senior staff attorney hired by the court to run CAMP would be involved in only a portion of eligible cases. In short, the program would be difficult to justify if the senior attorney's energies were not fully consumed. Of course, it was argued that in order to determine whether the senior attorney was effective, it would be necessary to establish appropriate comparisons to gauge that effect.

Thus, judge participation and the separation of CAMP into scheduling orders and preargument conferences, along with an appropriate control group, offered the most thorough approach. The price of an underutilized

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4. Supra note 2.

senior attorney would be worth gaining comprehensiveness in the study. After all, the purpose of the evaluation was to precisely determine whether CAMP works, which in turn meant testing the effect of the scheduling order procedure, the Rule 33 conference procedure conducted by a staff attorney or by a judge, and the combined procedures.

CAMP began operation in April, 1974, while the staffs of the Center and the Second Circuit developed and weighed alternative evaluations. From May until September, the evaluation remained in limbo. A number of compromises were suggested by Center staff but the kernel of the experimental approach--the random assignment of eligible cases to experimental and control groups--was retained.

In September, the Second Circuit consented to a scaled-down version of the classic, controlled experiment. This experiment would have two main components: (1) a single experimental group, in which eligible cases would merit both scheduling orders and Rule 33 conferences under the auspices of the senior staff attorney, now known as the staff counsel; and (2) a control group of eligible cases, in which both scheduling orders and preargument conferences would be withheld. Judge participation in the preargument conferences was sacrificed from the evaluation. Opinion was still divided about the effectiveness of staff in relation to judge. An attempt to estimate the separate effects of each CAMP procedure was also eliminated from the evaluation.

In order to minimize the threat of underutilizing the staff counsel, eligible cases would be randomly assigned so that substantially more cases would be designated experimental than control. The chief disadvantage of this approach was that it would take longer than originally contemplated to establish a control group large enough to test the program's effectiveness.

Further negotiations were necessary to translate the agreement reached with the Second Circuit into reality. Procedures would have to be established to identify and randomly assign the eligible cases and to minimize the burden on the staff counsel's small staff.

On October 21, 1974, some six months after the start of CAMP, the experimental phase of the CAMP evaluation began. For the next twelve months, cases passing through the CAMP office would be monitored, and 302 would be randomly assigned to experimental or control categories. Evidence for the evaluation would be obtained from a variety of sources, to test the propositions:

1. that CAMP would reduce the proportion of appeals that otherwise would impose a burden on the judges

2. that CAMP would improve the quality of appeals that would be briefed and argued

3. that CAMP would improve the efficiency of civil appeals by reducing elapsed time in the appellate process.

Other propositions would be examined, but these three were the mainstays of the evaluation.

These propositions would be tested through the use of "hard" evidence about what happened to the cases, and through the use of judge and attorney questionnaires. Case-related information concerning the timing and occurrence of critical events (e.g., filing of the notice of appeal, oral argument, substantive motions activity) was obtained from the docket sheets and files in the clerk's office.[5]

It was far more difficult to infer from the docket sheets or files whether a case had settled; settlement was one of the anticipated effects of the plan. Cases terminate short of adjudication for a variety of reasons: settlement, withdrawal without settlement, abandonment, etc. The docket sheets do not always distinguish these different terminations. Of course, the important point to note is that from the court's perspective,

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5. It should be mentioned, in passing, that these well-ordered and easily accessible sources made the data collection task far simpler than it might otherwise have been.

an increase in settlements or withdrawals entails a reduction in work for the judges who would otherwise have to decide those appeals. Therefore, whether a case is settled or abandoned or withdrawn, from the court's view, the burden on the judges will be lessened. One indicator of this burden that can be quite accurately measured from the docket sheets is the proportion of cases that are briefed and argued. Other measures, however, may also serve as useful indicators of judge burden.

In sum, the data derived from the docket sheets can be used to determine whether CAMP reduces the burden on the judges. It is not possible to determine from the data whether a reduction in judge burden is caused by CAMP's effects on settlement, since the settlement of an appeal cannot always be determined from the docket sheets.

Information about the issuance of scheduling orders and the holding of preargument conferences was obtained from docket sheets and cross checked in the files of the staff counsel. This verification was suggested by the staff counsel because of his concern that some immeasurable degree of error might be introduced by relying solely on the docket sheets for information about CAMP. As a rule, CAMP activities were double-checked in both the clerk's and CAMP offices. If a preargument conference was logged in the CAMP office, but not on the docket sheet, the conference was recorded as having occurred. If the docket sheet indicated that a conference had been held, but no verification could be established in the CAMP office after an exhaustive check of the daily conference schedule, the conference log, and the memorandum file, the event would not be recorded.[6] Every case was checked to determine whether or not control cases received CAMP procedures.

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6. As it happened, no control case received a preargument conference or a scheduling order.

The case-related data collected in New York also included the names of attorneys who were responsible for each of the appeals. After the cases were terminated, those attorneys were asked to complete a confidential questionnaire about their experiences with the appeals and their reactions to the plan, if any.[7] A review of these questionnaires will reveal that attorneys in the experimental cases were asked questions related specifically to CAMP procedures. These questions were omitted from the attorney survey in the control cases, for the obvious reason that no CAMP procedures were applied.

Follow-up letters and phone calls were used to encourage attorneys to respond to the survey. Although all the data are not yet in, a substantial portion of the attorney data base is included in this report. The response rate exceeded most expectations: almost 88 percent of all surveyed attorneys responded (559 completed questionnaires were returned; 637 were mailed).

It was expected that some of the eligible cases--in the experimental and control groups--would be fully briefed and argued. A procedure was devised to alert the Center staff to the composition of the panel designated to decide the appeal. The judges were then asked to evaluate the cases they were to hear. The purpose of the survey was to determine whether the cases receiving CAMP procedures were better in quality than those in which CAMP procedures were withheld. These evaluations were solicited through a mailed questionnaire which was to be returned upon completion to the Center.[8] Note that the questionnaire is the same for

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7. See Appendix II. Attorneys in the experimental group received Questionnaire Form A; attorneys in the control group received Questionnaire Form C.

8. The judge questionnaire is in Appendix III.

all cases, experimental as well as control: any questionnaire variation related to the presence or absence of CAMP procedures might have biased the judge responses.[9]

The response rate to these questionnaires also exceeded most expectations. Of 398 questionnaires mailed, 370 or 93 percent were returned completed. These figures are based on the available data; although there are still some questionnaires to be included in the analysis, the available responses represent a substantial part of the judge observations in this experiment.

It perhaps bears repeating that this evaluation's success in testing whether or not CAMP is effective rests on the random assignment of eligible cases to experimental and control categories. The procedure used here offers a breakthrough for evaluations in which units to be randomly assigned (in this evaluation, eligible civil cases) trickle into the court on a daily basis.

In most experiments, the units to be assigned are enumerated in advance and then randomly assigned to groups, but in this experiment, it was not known from one day to the next how many cases would have to be assigned or how and when to randomly divide them after they entered the appellate process. These were the choices:

1. One out of every four cases deemed eligible for CAMP by the staff counsel would be withheld from CAMP to establish the control group. This idea was rejected because it might give the program administrator considerable discretion to alter the equivalence

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9. An early version of this questionnaire contained--in the view of one judge--an inference in one of the questions that CAMP procedures were not applied. This question was modified to remove the inference, and an additional question was added (question 11) to determine whether the responding judge did or did not know that CAMP procedures had been applied.

of the controls to the experimentals. For example, perhaps some cases are very good candidates for settlement or withdrawal and others are not. Indeed, it was known before the start of CAMP that some appeals are settled or withdrawn. If the person responsible for the random assignment selected as control cases those that were unlikely candidates for settlement, and designated as experimental cases those that were likely to settle or withdraw anyway, then no doubt at the end of the experiment, there would be proportionally more control cases that were fully briefed and argued. The unwarranted conclusion would then be reached that CAMP caused a reduction in cases that otherwise would be decided by the court, when in truth this effect would be a result of the assignment procedure.

2. Another possibility was to use the last digit of each case's docket number to determine the random assignment. But the cases would have to be screened to determine eligibility for the experiment.[10] Thus it was still possible--although unlikely--that the program personnel could alter the random assignment by providing different eligibility requirements for experimental cases than for control cases. This approach, too, was rejected because there was an increased risk that the assignment procedure might produce an unwarranted conclusion.

3. Yet another technique for achieving the random assignment was to accumulate a batch of eligible cases at fixed intervals (for instance, every week), and then have someone from the evaluation staff oversee the random assignment. This alternative was rejected for two reasons. It would have introduced delay in the processing of appeals, which staff counsel viewed as unwise; and it would have tended to create distrust between CAMP personnel and Center employees, who would have been charged with overseeing the random assignment.

4. With all known conventional techniques eliminated for one reason or another, the Center staff developed

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10. Eligibility standards are discussed later in this chapter.

a technique that assured truly random assignment but without supervision and its attendant costs. All civil appeals entering the Second Circuit were reviewed after the appropriate CAMP Forms C and D were filed and, in nearly all circumstances, the docketing fee paid.[11] Once these threshold requirements were met, the case materials were then examined by staff counsel. If, in his judgment, a case merited both a scheduling order and a preargument conference, it entered the pool of eligible cases for random assignment.

Some may wonder why there was not a more specific eligibility criterion, such as a money judgment for plaintiff in the district court. Staff counsel argued that there were many factors to consider in deciding to apply CAMP procedures, especially the preargument conference. Some cases met a few requirements, others met more. Yet there was no calculable, uniform, and objective standard that, when applied to all cases, would separate the eligible from the noneligible cases. Indeed, CAMP was designed to permit this flexibility.[12] A handbook on appeals in the Second Circuit describes the process of selection:

> The staff counsel will make the determination as to whether or not the case is appropriate for a preargument conference on the basis of his study of Forms C and D, and a copy of the docket sheet from the District Court. Such a

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11. Form C provides information about the nature of the case, its disposition in the district court, and, to some extent, the issues to be raised on appeal. Form D provides information on the ordering of the transcript. These forms must be filed and the docket fee paid within 10 days of the filing of a notice of appeal in the district court, with dismissal by the clerk in the event of default (CAMP Rules 3 and 7(a)).

12. CAMP Rule 5(a).

> conference will normally be held in a private action seeking a monetary judgment, and in other actions which, in the judgment of staff counsel, seem susceptible to settlement or simplification of issues.[13]

Rather than impose arguable, objective standards as part of the evaluation, the decision as to eligibility was left to staff counsel. Under most conditions in the evaluation, the extent to which he would err in his judgment by including too many or too few cases did not matter, since more of the experimental than the control cases were expected to terminate short of panel consideration. Of course, if the pool of cases deemed eligible by staff counsel contained a substantial number that did not merit CAMP procedures, the program's effect would tend to be masked. It was reasonable to expect that staff counsel's identification of eligible cases would be based on the strong likelihood that CAMP would lead to settlement, withdrawal, or improvement in quality of those cases.

The eligiblity issue was not ignored, however. It was expected that staff counsel would learn from his experience at the eligibility stage and, over time, sharpen his decisions. The evaluation tested this "learning curve" hypothesis in order to minimize possible concern over the eligibility decision.

Following staff counsel's decision that a case merited both a scheduling order and a preargument conference, a staff member from the circuit executive's office would enter the docket number with the date in a log book. The Research Division of the Center maintained a duplicate log book in Washington, but with one important difference. Each line in this log book had been designated as a control or an experimental unit. When the staff member in New York completed his log entry, he would call the Center to transmit

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13. Appeals to the Second Circuit 15-16 (1975) (prepared by the Committee on Federal Courts of the Association of the Bar of the City of New York).

that information to the duplicate log. Only after the docket number and date were entered in Washington was the designation of experimental or control released to New York. This technique provided the greatest possible assurance that the random assignment had been made objectively.

In all, 302 cases were entered in the log from October, 1974 through October, 1975. Of these 302 cases, 225 were designated as experimental cases, in which CAMP procedures were applied; and 77 were designated as control cases, in which CAMP procedures would be withheld. Why were 302 cases entered, divided into uneven groups of 225 and 77?

One reason for the disproportionate designation of experimental and control cases was to keep staff counsel fully engaged in CAMP activities. For every three cases designated experimental, one case was designated control.

Another reason for the 302 cases is that in social research, very large samples can produce numerous statistically significant relationships of dubious substantive value. Although larger samples than the one selected here offer greater precision in estimating program effects, such precision might be of little value if the estimated effects of the program fell below a minimum level of acceptability.

Moreover, to reduce imprecision by half, a four-fold increase in sample size would be necessary. Given the Center's limited commitment of one year, an evaluation substantially beyond one year did not seem appropriate. One must therefore ask, what minimum difference (i.e., improvement) between the control and experimental cases is valuable? (Differences of lesser magnitude would be regarded as trivial.)

In this experiment, differences of less than about 10 percent between experimental and control groups would make justification of CAMP especially difficult in terms of practical importance. This was accepted as the minimum observable difference for concluding that CAMP was effective in reducing the burden on the judges.

If the observed difference between the two groups fell below the minimum, there would be two possible conclusions. One would be to conclude that CAMP had no effect whatsoever. The other would be to suspend judgment about CAMP effectiveness, i.e., to render a Scotch verdict of "effectiveness not proved." To state this issue another way, observed differences of 2 or 3 percent between experimentals and controls seemed too small to support a conclusion regarding the effectiveness of CAMP.

It would have been possible to substantially increase the number of cases in the eligible pool by continuing the experiment for three or four years. One might have then reached the conclusion that CAMP was effective when there were observed differences of about 2 or 3 percent between groups. Justifying the substantiality of effects, however, might have been especially difficult in practical terms, such as costs to the litigants and to the government. Few can take issue, however, with this experiment, which was designed to conclude that CAMP was effective if observed differences were, at minimum, in the 10 percent range.

Following the random assignment, cases designated as part of the experimental pool proceeded through the CAMP program and were subject to the scheduling order and preargument conference procedures. The control cases followed a different course. The case file and all forms were removed from the CAMP office. The docket sheet was "flagged" with the following information to prevent accidental "contamination" with CAMP procedures:

> This case is not to be processed under CAMP rules. Staff counsel must not be contacted concerning the proceedings in this case.

The question arose whether attorneys in the control cases should be notified that those cases were not to be subject to CAMP procedures. The proponents of notification took the position that CAMP had been in operation for nearly six months. During this period, some unknown number of attorneys could have altered their expectations about Second Circuit procedures to the extent that they might violate the Federal Rules of Appellate Procedure in anticipation of a CAMP scheduling order or a preargument conference.

The opponents argued that the notice would affect attorney behavior by encouraging greater attention to the Federal Rules of Appellate Procedure and the local rules, thus altering the control cases, which should ideally reflect only the absence of CAMP. In weighing the possibility of introducing positive bias (in experimental research, this is known as the Hawthorne effect) in relation to the possibility of jeopardizing the appeal because of Federal Rules of Appellate Procedure violations, the importance of the notice outweighed the bias it might introduce.[14]

This notice excluded control cases from the scheduling order requirement for all civil appeals.[15] Since the CAMP Rules left convening the preargument conference entirely to the staff counsel's discretion, it was unnecessary to mention withholding the conference in the notice.[16]

The 302 cases were randomly assigned in such a way that they could be divided into three groups, generally based on the chronological order in which they entered the Second Circuit. The first and second groups of 100 cases could each be analyzed separately, comparing 75 experimental with 25 control cases; and the last 102 cases to enter the experiment could also be analyzed separately, comparing 75 experimental with 27 control cases. Thus, each subgroup in the experiment could be analyzed separately to determine changes in the effect of CAMP as the program matured through the year of evaluation, and the results could be analyzed in total by combining the subgroups to test the program's effectiveness over the entire evaluation period.

Although 302 cases were included in this experiment, a number were excluded for various reasons. These reasons should be articulated to explain why this description of CAMP, while necessarily incomplete, is still reasonably accurate.

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14. The notice is in Appendix IV.

15. CAMP Rule 4(a).

16. CAMP Rule 5(a).

Recall that 302 cases meriting both a preargument conference and a scheduling order were used in the experiment. Approximately 400 cases were excluded because, in staff counsel's judgment, they merited either a scheduling order or a preargument conference, but not both. Of these 400 cases, nearly all were deemed eligible for scheduling orders. Occasionally, a case which was first designated as meriting only a scheduling order was later given a preargument conference. These cases, although infrequent, were nevertheless excluded from the experiment, since it was felt that the scheduling-order-first, preargument-conference-later cases (or vice versa) would be different--in ways that could not be estimated--from cases that were initially viewed by staff counsel as meriting both procedures.

In addition, some cases that merited both procedures were excluded because the issues were of such moment or the matters were so urgent that designation to the control group might--if the program really worked--pose a threat to the justice of the appeal. When a case of this magnitude arose, it was excluded from the experiment entirely. Fortunately, this occurred so infrequently (not more than five times during the year), that these exclusions from the experiment will not bias the judgment to be reached regarding CAMP effects on the nonexceptional cases.

These reasons justified excluding certain kinds of cases from the experiment; some justification should be offered for including the cases meriting both CAMP procedures. It was of paramount importance to determine whether the CAMP idea was effective at all, even under the most favorable circumstances--that is, when the two available procedures were applied in combination. Although one could argue that the scheduling order alone, or the preargument conference and nothing more, could be an effective device to reduce the burden on the judges or to improve the quality of appeals, it was desirable to apply the maximum effort to each experimental case (or withhold it for the controls), and verify the unproved proposition: CAMP is an effective way to reduce the proportion of cases that otherwise will run the gamut of the appellate process, and to improve the cases that do go the distance.

Some attention should be given to the soundness of the experiment and its successful execution. One threat to this experiment was the possibility of contamination. If the cases designated as controls were to inadvertently receive CAMP procedures (especially the preargument conference), comparisons between the experimental and control cases would be suspect. Fortunately, this form of contamination did not occur.

Another form of contamination was more difficult to assess. If attorneys became familiar with CAMP procedures because they practiced frequently in the Second Circuit, there might arguably have been some lingering CAMP effect when those attorneys were later involved in control cases. Although there are some frequent litigators in the Second Circuit, the average attorney is involved in only one case in a given year.[17] A review of the attorneys who participated in cases in the experimental and control groups, and were surveyed, suggests that some attorneys were "repeaters," but they rarely appeared in an experimental case first, then a control case.

Still, it was possible for attorneys in the control group to have gained some experience with CAMP procedures prior to the October, 1974 starting date for the experiment. The claim that CAMP affected attorney behavior during the experiment, however, requires further proof. First, the average attorney appears before the Court of Appeals once in a given year. This alone casts doubt on the claims of contamination. Second, if no case had been resolved short of briefing and argument prior to the start of the plan in April, 1974, the attorney contamination argument might be on firmer logical footing. But since a substantial proportion

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17. Extrapolation from a Federal Judicial Center tabulation, Attorney Population--Second Circuit for fiscal 1973; and Attorney Attitudes Toward Limitation of Oral Argument and Written Opinion in Three U.S. Courts of Appeals 4 (1974) (prepared by the Bureau of Social Science Research, Inc. for The Commission on Revision of the Federal Court Appellate System, under Federal Judicial Center contract no. 1040928-4-05-2501-11776).

of cases terminated short of argument even before the plan began,[18] it is far more difficult to leap to the conclusion that CAMP contaminated attorney behavior in the control cases. On the basis of the evidence, it would seem far more plausible that the pre-CAMP experience of attorneys simply continued after the plan went into operation. The evaluation will determine whether CAMP improves this given level of dispute resolution.

With true random assignment of the cases assured and threats to the validity of the experiment by contamination minimized, it is legitimate to examine the evidence to determine the program's effectiveness in the disposition of appeals.

Before analyzing the evidence from the controlled experiment, it seems worthwhile to briefly describe the operation of the plan as seen by Center observers during the course of the evaluation.

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18. For the three-year period from fiscal 1972 to fiscal 1974, 43% of Second Circuit civil appeals from the district courts terminated short of oral argument. Ann. Rep. of the Dir. of the Administrative Office of the U.S. Courts (1972, 1973, 1974) (table B-1 (excludes consolidations)).

CHAPTER THREE

CAMP IN PRACTICE

When the evaluation began in October, 1974--some six months after the implementation of CAMP--a number of Second Circuit functions were controlled by the plan. Nathaniel Fensterstock, who serves as staff counsel, and his assistants completed each of the scheduling orders required for all civil cases under CAMP rules. Mr. Fensterstock conducted the preargument conferences, which were arranged by his staff following his review of the CAMP forms.

Calendaring activities also fell within the CAMP ambit; staff counsel proposed the weekly calendar and all its attendant burdens of brief and appendix distribution. Calendaring seemed an ancillary but worthwhile function for the CAMP office because of Mr. Fensterstock's substantive knowledge of appeals, which he gained through selecting cases for preargument conferences. This information could be used to group cases raising similar issues, and to balance across panels the mix of issues and their degree of difficulty. This calendaring activity was returned to the clerk's office a few months after the start of the evaluation, in part because of the demand for regular CAMP activities.

Civil appeals in the Second Circuit are reviewed by the docket clerks following the filing of the notice of appeal to determine compliance with CAMP rules concerning the filing of CAMP forms C and D and the payment (or waiver) of the docketing fee. Failure to meet these requirements results in dismissal of the appeal by the clerk. Once an appeal meets these requirements, the docket clerks draft a scheduling order and send it to the CAMP office for completion. Staff counsel determines the dates for filing the record and the briefs, and the earliest week for oral argument. These dates are embodied in the scheduling order.

If, in the judgment of staff counsel, an appeal should be given a preargument conference, the staff will make the necessary arrangements and appointments. The decision to hold the conference is usually made early in the life of the appeal, on the ground that the parties are more willing to consider a compromise when their investment in the appeal is still small. During the year of evaluation, nearly all the conferences were scheduled in the CAMP office in the United States Courthouse.

A number of observed preargument conferences generally proceeded in the following manner. Attorneys attending a preargument conference would enter their names in the daily log, and, at the appointed time, they would be invited into staff counsel's office. Mr. Fensterstock would begin the conference with an introduction explaining the procedures, since many attorneys were new to the program.

Mr. Fensterstock would state that all matters discussed at the conference would remain confidential and that nothing that transpired would be communicated to the court, except for a monthly report that would briefly state the matters at issue and the likelihood of settlement, withdrawal, or other action.[1] Usually, the appellant would state his theory of error in the district court; the appellee would respond; and staff counsel would pose questions to both parties as they presented their opposing views.

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1. This report caused some attorneys concern: they felt the court would be biased by the failure to settle or withdraw appeals in conformity with staff counsel's suggestions. The Association of the Bar of the City of New York noted this concern in its generally favorable evaluation of CAMP. Comm. on Fed. Courts, The Ass'n of the Bar of the City of New York, The Pre-Argument Conference Experiment of the Second Circuit Court of Appeals: A Report on a Sampling of Attorneys' Assessments of the Pre-Argument Conference Procedure (June 24, 1975).

Following the release of the association's report, procedures in the CAMP office were altered to satisfy the concerns raised by the attorney assessments.

It is impossible to generalize about successful techniques for settlement discussion from observing these conferences. Without some uniformity in attorneys, in requested relief, or in techniques to reach settlement, it seems best to describe some of the approaches staff counsel used during the conference. Some overall impressions are possible. Frequently, Mr. Fensterstock would ask if there was a possibility of settling the appeal and, if so, how far apart the parties might be. Occasionally, he would place the parties in different rooms and discuss the possibilities with each party. If some movement toward compromise was made, he would then bring the parties together to hammer out a solution.

Sometimes, staff counsel would approach a complex set of issues one at a time. On other occasions, he would treat a complex set of issues interdependently, trying to resolve them as a whole rather than piecemeal. Occasionally, a stubborn client stood in the way of a settlement. In some cases, the stumbling block was a district court opinion with potentially troublesome consequences for the appellant. Mr. Fensterstock would volunteer to discuss the appeal and possible compromise with the client; and, on appropriate occasions, he would discuss the possibility of having a judgment in the trial court vacated, simultaneously exploring the disadvantages of a circuit-wide decision if the appeal were affirmed with an opinion.

Staff counsel would also inquire of the appellant whether the Court of Appeals had jurisdiction. If indeed some prerequisite was absent, this would give the appellant a chance to withdraw, or would encourage the appellee to move to dismiss in the event that the appeal was pressed.

Free, frank discussion seems essential to the conference procedure. In most of the conferences observed by Center staff, Mr. Fensterstock would offer his views on the merits; those views ranged from uncertainty regarding the outcome, to incredulity that the parties would press such appeals. In sum, if the appeal was viewed by staff counsel as without merit, or of so little merit as not to warrant the time of the judges to decide the appeal, staff counsel would--with rhetoric and logic--urge the appellant to withdraw or encourage the parties to accept a compromise solution.

Staff counsel would also draft and redraft scheduling orders as a consequence of his conference activities. For example, if counsel expressed the possibility of settlement, Mr. Fensterstock would hold the operating scheduling order in abeyance, and arrange for the key parties to report to him within a reasonable period about settlement. He would also redraft scheduling orders for advanced briefing schedules, or extend time for briefing and argument if he felt the additional time was warranted.

In general, staff counsel made his office available for follow-up conferences, conference calls, and discussions with clients, if such efforts would enhance the possibility of terminating the appeal without briefs and argument.

Staff counsel's duties went beyond the conference and scheduling procedures. He would also make recommendations to the clerk on procedural motions, such as motions for filing of exhibits and motions for permission to file oversized briefs. All matters related to the deadline for filing materials and arguing the appeal, which prior to CAMP would have been handled by motions, would now be resolved by an altered scheduling order executed by staff counsel.

With this capsule description of CAMP activities in mind, it is now time to examine the evidence concerning the effectiveness of the plan in operation.

CHAPTER FOUR

MEASURING CAMP EFFECTS: EVIDENCE FROM THE CASES

The CAMP experiment began on October 21, 1974, when the first case deemed eligible by staff counsel for both a scheduling order and a preargument conference was randomly assigned to the experimental group.[1] Over the next twelve months, a total of 302 cases were randomly assigned to experimental or control groups, to determine the plan's effectiveness.

The experiment's total number of cases was chosen to assure the accuracy of the research findings. There was some evidence suggesting the plan would increase settlements and withdrawals, in cases meriting CAMP procedures, by as little as 15 or as much as 25 percent.[2] The 302 cases in the experiment were adequate to test

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1. The random assignment of cases to experimental and control groups is much like coin flipping, but considerably more exact. It guarantees that each case has the same chance of being designated to the experimental or to the control group. That is, the random assignment process is "blind" to any characteristics of the cases. The classic work in controlled experimental research based on random assignment is R.A. Fisher, The Design of Experiments (8th ed. 1966).

2. Kaufman, The Pre-Argument Conference: An Appellate Procedural Reform, 74 Colum. L. Rev. 1094,1100 n.17 (1974); Kaufman, State of the Judicial Business in the Second Circuit 10-11 (1975) (unpublished address to the Judicial Conference of the Second Circuit, Sept., 1975).

this minimum suggested effect, as well as effects of lesser magnitude if they had occurred.

It took exactly one year to reach the goal of 302 cases.[3] During this period, for every three cases identified by staff counsel as meriting both CAMP procedures, there were four cases that, in his view, merited either one or the other but not both. This evaluation focused on appeals meriting both CAMP procedures, since this is the maximum "treatment" the plan can provide. By examining the effectiveness of CAMP under the most favorable conditions, the most convincing possible test was given the plan.

Perhaps it bears repeating that the random assignment of cases to experimental and control categories provided the greatest assurance that the two groups were equivalent in all respects save one: CAMP procedures applied to the experimental cases only. Hence, beyond a certain point determined by the laws of chance, observed differences between the experimental and control groups warrant a conclusion that CAMP is effective. In short, when the difference between the two groups of cases is sufficiently large, it can be said with some confidence that CAMP procedures were responsible for a particular effect, such as a reduction in briefed and argued appeals or an increase in the quality of appeals.

Precisely how are such conclusions reached? The first step is the formulation of a hypothesis, i.e., a statement that a certain situation might be true. An alternative hypothesis, which would necessarily be true if the first hypothesis is rejected as false, is also formulated. The next step is to examine the empirical evidence on the assumption that the initial

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3. Initially, the goal was set at 300 cases, but it was decided to randomly assign a few more, to give some leeway for consolidations and other unforeseen events. There were few consolidations, however, leaving 302 cases in the experiment. These were divided into 225 experimentals and 77 controls.

hypothesis is true. If the evidence would be highly unlikely under the assumption, the initial hypothesis is rejected, and its alternative is accepted.

One hypothesis was that CAMP has no effect on the proportion of briefed and argued cases.[4] (The alternative hypothesis was that CAMP has an effect on the proportion of brief and argued cases.) If the empirical evidence is consistent with the initial hypothesis, it stands. If the evidence is inconsistent with this hypothesis, it is rejected in favor of its alternative. For example, if the evidence is that 50 percent of the experimental cases and 50 percent of the control cases were briefed and argued, the initial hypothesis (that CAMP has no effect) probably should be retained. If the evidence is that 40 percent of the experimental cases and 75 percent of the control cases were briefed and argued, the initial hypothesis probably should be rejected in favor of its alternative. It is also possible that the evidence might not squarely support either the initial hypothesis or its alternative. In that case, a judgment about program effects would be suspended.

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4. The "no difference" or "no effect" starting point is a common feature of scientific research.

> "This seems like an extremely devious way of proceeding, but we must remember that we shall not be in a position to establish directly that there is a difference [between groups]. To avoid the fallacy of affirming the consequent, we must proceed by the elimination of false hypotheses. In this case there are logically only two possibilities, there either is or is not a difference. If the latter possibility can be eliminated, we can then conclude that some difference in fact exists."

Blalock, Social Statistics 95 (1960).

This basic approach to evaluating the evidence from an experiment can be altered to reflect the precision of the hypothesis. For example, one might expect that CAMP procedures would be effective in a particular way or direction, such as by reducing the proportion of briefed and argued appeals or by increasing the quality of briefed and argued cases. The statistical tests employed permitted evaluation of the likelihood of observing differences of varying magnitudes between the experimental and control groups.[5]

In general, the greater the difference between groups, the less likely that the initial "no effect" hypothesis remains valid. But at what point is the inital view rejected? There is no clear and convincing answer to this question. By convention, most social scientists claim that, given the initial assumption, if the likelihood of observing a difference between groups is less than 5 times in 100, the assumption should be rejected. There is nothing sacred or absolute in the standard of less than 5 times in 100, but there are strong reasons for having adopted this convention in the CAMP experiment.

When a decision to reject or to accept the initial hypothesis is made, the researcher must face the possibility of making either of two errors: rejecting the initial (no effect) hypothesis when it is in fact true; or accepting the initial hypothesis when it is in fact false. The 5-in-100 standard minimizes the first error; and, in general, the sizes of the experimental and control groups minimize the second. For social programs, the first error seems to be more threatening than the second. Keeping the potential for the first error small protects against drawing the false inference that CAMP is effective when in fact it is not.

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5. For a discussion of the statistical tests employed here, *see id.* at 176-79; and Hays, Statistics for the Social Sciences 389-428 (2d ed. 1973).

Of course, it is possible to err by concluding that CAMP has no effect when in fact it does. For experiments in court administration, however, this second error may be less critical "since the more important policy problem would seem to be how to avoid the disappointment, frustrated effort and wasted resources caused by making [the first error], that is, adopting an ineffective treatment as a social program."[6]

Thus, in the CAMP experiment, the observed differences between experimental and control groups were treated as significant in the statistical sense only if the difference could have occurred by chance fewer than 5 times in 100. This standard for statistical significance is really a procedure for ruling out the possibility that chance factors might have caused differences between the experimental and control cases.

This issue can be explained in another way. The observations made in controlled experiments are subject to a certain degree of error. This is so because repetition of an experiment will not always produce exactly the same results. Although chances are that repeated experiments will produce similar results, the laws of probability permit an estimate of the range of possible values likely to occur, without having to repeat experiments. Limits can be calculated with the assurance that, nine times out of ten--or two times out of three, or any other degree of assurance one cares to impose--the true value will fall within a specified range, called a confidence interval.

In the CAMP experiment, interest centered on the differences between the experimental cases and the control cases. Confidence intervals were calculated for these differences. Of course, if the confidence interval included zero, there was the distinct possibility that the program has no effect: a rejection of

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6. Social Experimentation: A Method for Planning and Evaluating Social Intervention 77 (Reicken & Boruch eds. 1974).

the initial ("no effect") hypothesis would not be warranted. But failure to reject the initial hypothesis does not automatically mean it is correct. Under some conditions, it may be appropriate to suspend judgment rather than risk the erroneous conclusion that CAMP has no effect whatsoever.

It must be stated once again that conclusions about statistical significance say absolutely nothing about practical or substantive value. But once one reaches a conclusion that the findings are statistically significant, the next required step is to determine the magnitude of the plan's effect. One way to measure the magnitude of the causal relationship between CAMP procedures and briefed and argued appeals, or between CAMP procedures and quality of briefed and argued appeals, is to estimate how much improvement can be made in predicting whether cases will be briefed and argued (or will be improved) when CAMP procedures have been applied, compared to similar cases in which CAMP procedures have been withheld. The merit in this approach is that improvement in prediction falls on a scale between 0 and 100 percent. For example, if no experimental case were briefed and argued and every control case were briefed and argued, the improvement in prediction of briefed and argued cases would be 100 percent, since knowledge about which cases did or did not receive CAMP provides a perfect prediction of plenary review. If the same proportion of cases were briefed and argued in both experimental and control groups, the ability to improve the prediction of which cases will be briefed and argued would be zero, since knowledge about the cases receiving CAMP procedures will not affect the prediction.[7]

With these three concepts in mind--statistical significance, confidence intervals, and improvement in prediction--it is now time to turn to the data to assay the effects of the plan.

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7. This index of predictive association is discussed in Blalock, supra note 4, at 232-34, and in Hays, supra note 5, at 745-49.

CAMP was designed in part to conserve sparse judicial resources.[8] It was not feasible to directly test the plan's effectiveness by measuring the investment of effort by the judges and their staffs in the experimental and control groups. Inferences must be drawn from other evidence to conclude that judicial resources have or have not been conserved. When this experiment was designed in 1974, a number of assumptions were made, based on previous research and available evidence, from which inferences about judge burden could reasonably be drawn.[9]

The view was that if CAMP was effective, it would substantially reduce the proportion of cases that otherwise would be adjudicated by three-judge panels. A case was considered adjudicated when a judgment by three judges terminated the appeal on a nonprocedural matter. For example, an appeal was deemed adjudicated if it was decided on the merits after briefs and oral argument, or if it was dismissed on a motion for lack of jurisdiction. An appeal was not considered adjudicated if it was dismissed by an order of three judges for failure to prosecute.

Distinctions between settled and withdrawn appeals were of no consequence, since it was assumed that neither settlements nor withdrawals entail judge effort. Experience has shown that this assumption and the inferences drawn from it are sometimes inappropriate. Settlements or withdrawals may occur after substantial judge effort has been expended. This analysis began, however, by examining the data according to the early view that settled and withdrawn appeals entail no judge effort.

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8. Letter from Irving R. Kaufman to Warren E. Burger (Nov. 30, 1973).

9. The model for this experiment--and the source of these assumptions--is Rosenberg, The Pre-Trial Conference and Effective Justice (1964).

The initial hypothesis was that CAMP has no effect on the proportion of appeals adjudicated by a panel of three judges.

As shown in table 1, 54 percent of the cases in the experimental group were adjudicated. In the control group, in which CAMP procedures were withheld, 62 percent of the cases were adjudicated. The difference of 8 percent between the experimental and control groups is not statistically significant, that is, the difference could likely occur by chance more frequently than 5 times in 100.[10]

TABLE 1

PERCENTAGE OF ADJUDICATED APPEALS

Experimental Cases (N=225)	Control Cases (N=77)
54%	62%

p* = .11

* The 'p' value represents the probability of observing a difference of the magnitude found in the table, given the initial assumption. An observed difference between the two groups of cases is treated as significant only if there are fewer than 5 chances in 100 that the difference could have occurred by chance. If the 'p' value is greater than .05, the results are not considered statistically significant. If the 'p' value is less than .05, the results are deemed statistically significant.

To state the proposition another way, about nine times out of ten, the true difference between experi-

10. A difference of 11% or more would be needed to reject the initial hypothesis.

mental and control groups will fall within a range of -19 to +4 percent. Since the confidence interval includes zero and positive values, there is a chance that CAMP has no effect or may even increase adjudications. Given the wide range of negative values captured by the confidence interval, there is also a possibility that the program is indeed effective in reducing adjudications. Therefore, although there are proportionally fewer adjudicated appeals in the experimental group, the evidence warrants neither a rejection nor an acceptance of the initial hypothesis that CAMP has no effect in reducing the proportion of adjudicated decisions. The best that can be offered is a Scotch verdict.

The presentation of this evidence is based on the view that appeals terminated by settlement or withdrawal entail no investment of judicial effort and judicial effort is invested only in adjudicated appeals. Evidence suggests that this view is unwarranted. Two appeals were settled or withdrawn well after oral argument. Clearly, there was some investment of judicial resources in those appeals: the briefs were read by judges and clerks, bench memoranda were prepared, and oral argument was heard. Since the cases terminated some time after they were argued, it is reasonable to presume that conference memoranda were prepared and a conference was held. It would seem unwarranted to equate these two cases with cases that were settled or withdrawn (although that is indeed how they were terminated), when they in fact did entail some effort of a three-judge panel.

Judge Kaufman has mentioned that CAMP is valuable in fostering early settlements or withdrawals, since the greater the involvement in the appellate process before settlement or withdrawal, the greater the investment on the part of the litigants and the greater the probability that judicial resources will be tapped, even though the appeal may ultimately be resolved by the parties.[11] This suggests that the way an appeal

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11. Supra, note 2, Colum. L. Rev. at 1095,1096.

terminates affects the amount of burden on the court, depending upon the procedural stage at which it terminates.

A more difficult judgment is required concerning another appeal, which was withdrawn in open court on the day of oral argument. Presumably, the judges had read the briefs and had called upon their clerks to prepare bench memoranda. While the withdrawal did save judicial resources, since, at minimum, the judges were spared oral argument, one cannot gainsay the investment made by the judges in this appeal.

If the cases in the experiment are examined according to whether or not some judge effort was invested (without attempting to determine the magnitude of the effort), another perspective on CAMP is revealed. Table 2 provides this perspective. Note than an appeal was counted as consuming judge effort if it involved (at minimum) an opposed substantive motion requiring the decision of three judges.

TABLE 2

PERCENTAGE OF APPEALS INVOLVING SOME JUDGE EFFORT

Experimental Cases (N=225)	Control Cases (N=77)
57%	65%

p = .11

Fifty-seven percent of the experimental cases, compared to 65 percent of the control cases, involved some judge effort.[12] As in the previous table, the

12. A difference of 11% or more would be needed to reject the initial hypothesis.

difference of 8 percent between experimental and control cases is not statistically significant. On the basis of the evidence in table 2, about nine times out of ten, the true difference between experimental and control groups will fall within a range of -19 to +3 percent. Since the confidence interval includes zero and positive values, there is the possibility that CAMP has no effect or may even be counter-productive.

The evidence does not warrant a judgment that CAMP reduces the burden on the judges. But the substantial range of the confidence interval suggests it would be inappropriate to accept the view that CAMP has no effect whatsoever. In short, suspended judgment may be called for here, as well.

Of course, the investment of judge effort varies among appeals and among judges. It may be worthwhile, however, to separate the cases in this experiment according to a general principle concerning the relative investment required for some appeals compared to others. To the extent that fully briefed and argued appeals are relatively more burdensome than other cases, it seems incumbent to focus attention on the briefed and argued appeals to isolate CAMP effects.

The data in table 3 offer yet another perspective on the effectiveness of the plan; in this area, CAMP held the most promise for the court, for it would seem that the greatest amount of judge effort would ordinarily be spent in appeals perfected through the stage of briefing and oral argument.

TABLE 3

PERCENTAGE OF APPEALS TERMINATED AFTER BRIEFING AND ORAL ARGUMENT

Experimental Cases* (N=225)	Control Cases (N=77)
54%	57%

$p = .32$

*includes two cases that were settled after oral argument.

Fifty-four percent of the experimental cases, compared to 57 percent of the control cases, were briefed and argued in the Second Circuit. The difference between experimentals and controls is not significant.[13] Even if the two cases in the experimental group that were settled after oral argument were removed, by the standards employed in the evaluation, it would still not be possible to conclude that CAMP reduces the proportion of appeals "which otherwise would run the entire gamut from record transcription and briefing to argument and opinion."[14]

About nine times out of ten, the true difference between groups will fall between -14 and +8 percent. Once again, the confidence interval includes zero and positive values and, hence, there is a possibility that CAMP is ineffective or counter-productive. But in this situation, the range of values "capturing" the true effect of CAMP does not touch the range of expected improvement of 15 to 25 percent. By the measure of briefed and argued appeals, CAMP does not yet seem to live up to its promise. Given the modest 3 percent difference between experimental and control groups and the anticipated range of improvement expected of the program, the most appropriate conclusion would seem to be that CAMP has little or no effect on reducing the proportion of briefed and argued appeals.

The case data provide an opportunity to analyze the eligibility decisions of staff counsel to determine whether there were marked shifts in the pool of cases over the year of the evaluation. If staff counsel had substantially broadened his criteria for the inclusion of cases capable of settlement or withdrawal, we would expect an increased proportion of adjudicated cases (or a declining proportion of settled or withdrawn cases) across the year. Recall that the experiment can be

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13. Here, too, a minimum difference of about 11% would be needed before the initial hypothesis could be rejected.

14. Supra, note 1, at 1094.

divided into three separate experiments, each covering approximately a four-month segment of the evaluation year.

The data in table 4 show that the percentage of appeals adjudicated in each of the time periods was almost exactly the same. In the first time period, 56 percent of the appeals were adjudicated by a panel of three judges; in the second period, 57 percent; and in the third period, 55 percent. This evidence is consistent with the initial view that the eligibility criteria for the admission of appeals into the experimental pool remained fairly constant during the year.

TABLE 4

ADJUDICATED APPEALS BY TIME PERIODS

	Percentage of Appeals Adjudicated*
Period one October, 1974 - February, 1975	(N=100) 56%
Period two February, 1975 - May, 1975	(N=100) 57%
Period three June, 1975 - October, 1975	(N=102) 55%
Total for all periods	(N=302) 56%

*Terminated by a decision of three judges on a nonprocedural matter.

It is also possible to examine the data within time periods to determine (1) whether the plan increased in effectiveness across the year, and (2) whether CAMP was effective in any one time period, with the expectation that it would probably be most effective in the last period, when the plan had fully matured through experience.

Table 5 separates the percentage of adjudicated appeals into experimental and control groups.

TABLE 5

ADJUDICATED APPEALS BY GROUP AND TIME PERIODS

	Experimental Cases	Control Cases
Period one October, 1974 - February, 1975	(N=75) 53%	(N=25) 64%
	p = .18	
Period two February, 1975 - May, 1975	(N=75) 55%	(N=25) 64%
	p = .23	
Period three June, 1975 - October, 1975	(N=75) 53%	(N=27) 59%
	p = .30	
Total for all time periods	(N=225) 54%	(N=77) 62%
	p = .11	

According to data in table 5, there were fewer adjudicated appeals in the experimental group than in the control group within each time period. The difference between groups in the first period was 11 percent; in the second period, 9 percent; and in the third period, 6 percent. It is clear from this evidence that there was no trend toward increasing effectiveness of the plan across time periods. And, within any one time period, there was no significant difference, between experimental and control groups, in the proportion of adjudicated appeals.[15]

This evidence does not support the propositions that CAMP effectiveness improved over time or that CAMP was effective in one period rather than another. But the proportion of adjudicated appeals may be an inadequate indicator of judge burden, and the use of that indicator may have affected the results.

The data in tables 6 and 7 use the alternative measures suggested earlier: the proportion of appeals involving some judge effort and the proportion of appeals decided after briefing and oral argument. As in the earlier analysis, the initial hypotheses were that CAMP effectiveness does not improve over time and that CAMP is not effective within any time period. Are the data inconsistent with these hypotheses?

According to the data in table 6, there were proportionally fewer appeals requiring some judge effort in the experimental group than in the control group within each of the time periods. In the first time period, the difference between groups was 8 percent. In the second period, the difference was 12 percent. In the third and last period, the difference declined to 4 percent. Hence, the evidence shows no significant increase in effectiveness across the time periods, as measured by the percentage of appeals involving some judge effort. The data also reveal no significant difference in favor of CAMP within any one period, as measured by the percentage of appeals involving some judge effort.[16]

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15. Within any time period, a minimum difference of about 18% would be needed to reject the initial hypothesis.

16. Supra, note 15.

TABLE 6

APPEALS REQUIRING SOME JUDGE EFFORT, ARRANGED BY GROUP AND TIME PERIODS

	Experimental Cases	Control Cases
Period one October, 1974 - February, 1975	(N=75) 60%	(N=25) 68%
	p = .24	
Period two February, 1975 - May, 1975	(N=75) 56%	(N=25) 68%
	p = .15	
Period three June, 1975 - October, 1975	(N=75) 55%	(N=27) 59%
	p = .36	
Total for all time periods	(N=225) 57%	(N=77) 65%
	p = .11	

Table 7 shows the cases by the proportion of appeals that were briefed and argued.

The data in table 7 show proportionally fewer briefed and argued appeals in the experimental group than in the control group, for periods one and two. In the first period, the difference between groups was 8 percent; in the second period, the difference declined slightly, to 5 percent. In the last time period, there were proportionally more briefed and argued appeals in the experimental group than in the control group. Once again, the data are consistent with the initial views that CAMP effectiveness in reducing the proportion of argued and briefed cases did not improve significantly over time, and that within any one period CAMP did not reduce the proportion of cases that otherwise were briefed and argued.[17]

TABLE 7

BRIEFED AND ARGUED APPEALS BY GROUP AND TIME PERIODS

	Experimental Cases*	Control Cases
Period one October, 1974 - February, 1975	(N=75) 52%	(N=25) 60%
	p = .25	
Period two February, 1975 - May, 1975	(N=75) 55%	(N=25) 60%
	p = .33	
Period three June, 1975 - October, 1975	(N=75) 55%	(N=27) 52%
	p = .60	
Total for all time periods	(N=225) 54%	(N=77) 57%
	p = .32	

*Includes two cases that were settled after oral argument.

In the data, there is a suggestion that CAMP may reduce the proportion of appeals that a panel of judges dismissed, prior to briefing and argument on the merits, on contested substantive motions. These are motions for substantive relief within the Second Circuit's Rule 27. The frequency of such dispositions is very

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17. Supra, note 15.

small, requiring a different test for statistical significance.[18]

In the experimental group, 3 of the 225 appeals (or about 1 percent) were dismissed on contested substantive motions by a panel of three judges before briefs were filed or oral argument was heard. Five out of 77 appeals (or about 6 percent) in the control group were dismissed in this manner. The initial hypothesis was that the experimental and control groups were not significantly different. The probability of observing three or fewer terminations of this type out of the 225 cases in the experimental group, when the expected proportion (determined by combining 3 in 225 and 5 in 77) is almost 3 percent, is greater than 5 times in 100. Thus, by the standard applied for all the tests, the difference between groups is not significant. This sustains the hypothesis that CAMP does not significantly affect the proportion of dispositions on contested substantive motions.

It perhaps bears noting that, with one exception, the experimental cases showed consistent improvement over the control cases in all the related measures employed to this point, but in no circumstance were the observed differences sufficiently great to rule out chance as their cause.

One hallmark of CAMP is the use of scheduling orders to control and monitor the progress of appeals. This is a dramatic departure from tradition for the appellate process. Prior to the use of such orders under CAMP, attorneys would be left on their own to follow the Federal Rules of Appellate Procedure and the local rules of the Second Circuit. Attorneys retained maximum flexibility in the timely processing of appeals, but this flexibility permitted some appeals to languish on the docket. The scheduling order--coupled with the

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18. The test to be applied is based on the Poisson approximation to the binomial distribution. It is discussed in Hays, supra note 5, at 206-08.

threat of dismissal in the event of default[19]--may draw more attention to the requirements of the appellate process.

The use of scheduling orders under CAMP should reduce the time for appeals to be processed. How should this time be measured? Quite simply, the common-sense approach would be to determine the number of days from the start to the end of the process, from the filing of the notice of appeal in the district court to the termination of the appeal in the Second Circuit. The cases in the CAMP evaluation were analyzed by comparing the median time[20] between these two events--the beginning and the end of the appeal--in a number of settings.

As in the earlier analyses, the initial view was that the cases in the experimental and control groups are equivalent with respect to the elapsed time from notice to termination. Analysis of the evidence determined whether this view should be rejected.

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19. CAMP Rule 7(b).

20. Suppose all the cases were ranked according to the number of days between filing the notice and termination. The median would be the value that divides the rank list in half, i.e., there would be as many cases above the median value on the list as below that value. The median is the appropriate statistic to use because it is less sensitive to extremely high or low values.

Table 8 presents the median time from notice to termination for experimental and control cases.[21]

TABLE 8

MEDIAN TIME FROM NOTICE OF APPEAL TO TERMINATION: ALL APPEALS

Experimental Cases	Control Cases
(N=195)	(N=65)
154 days	215 days

$p = .02$

The median time for cases receiving CAMP procedures was 154 days; the median time for cases in which CAMP procedures were withheld was 215 days. This difference is sufficient to warrant a rejection of the initial view, since the probability of such observations occurring by chance is far less than 5 times in 100. CAMP reduces, by a statistically significant amount, the time for processing civil appeals.

How much of a processing-time reduction does CAMP cause? If all the experimental cases fell below the

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21. Note that the cases in the experimental and control groups total 260, or about 86 percent of the 302 cases analyzed earlier in this chapter. The discrepancy reflects the time required to transform the data into machine-readable form for analysis on the Center's computer. The earlier analysis--using all 302 cases in the evaluation--was done by hand. The analysis measuring time between events required the use of the Center's computer, which had about 86 percent of the data on file. This should give a fairly accurate view of CAMP, although it will be subject to change as the rest of the data are included in subsequent analyses.

overall median and all control cases fell above the overall median, one could make a perfect prediction of where a case would fall relative to the median, when CAMP procedures were used. But if both the experimental and the control cases were equally divided around the overall median, there would be no improvement in the prediction of where a case would fall relative to the median, even when CAMP procedures were used. In the present example, this prediction was improved by 13 percent for cases that received CAMP procedures.

It seems appropriate to ask whether this processing-time reduction affects both appeals that run the gamut of the appellate process and appeals resolved by settlement or withdrawal, without court attention. Table 9 provides information on the median time from notice to termination of appeals in which there was no court attention.

TABLE 9

MEDIAN TIME FROM NOTICE OF APPEAL TO TERMINATION: APPEALS SETTLED OR WITHDRAWN WITHOUT COURT ATTENTION

Experimental Cases	Control Cases
(N=88)	(N=22)
77 days	120 days

$p = .01$

As shown in the table, settled or withdrawn cases in the experimental group took 77 days from notice to termination, while equivalent cases in the control group took 120 days. The conclusion here, too, is that CAMP produced a statistically significant reduction in the time required to terminate settled or withdrawn appeals. Knowing that CAMP procedures have been applied improves a prediction of where the cases will fall in relation to the overall median by about 18 percent.

These data are subject to three alternative interpretations. The experimental cases may have been settled or withdrawn earlier in the process than were the control cases; or, information about settlement or withdrawal of the experimental cases reached the court sooner than that about the control cases, because of the CAMP sanctions in the event of default of a scheduling order; or, both earlier resolutions and improved, expedited reporting of those resolutions occurred in the experimental cases, but not in the controls. The data do not aid choosing among these alternatives. All that can be said with assurance is that CAMP was responsible for reducing the lives of these appeals. If earlier settlements do, indeed, result from CAMP, the litigants might (arguably) benefit.

Table 10 provides time information on the briefed and argued appeals. Is CAMP effective in expediting these cases?

TABLE 10

MEDIAN TIME FROM NOTICE OF APPEAL
TO TERMINATION: BRIEFED AND ARGUED APPEALS

Experimental Cases	Control Cases
(N=104)	(N=37)
223 days	246 days

p = .30

The median time to disposition was 223 days for the cases receiving CAMP procedures and 246 days for the cases in which CAMP procedures were withheld. There is no statistically significant difference in median times between the groups. Thus, the initial view remains in force: CAMP has no effect on appeals that run the gamut of the appellate process. To put the matter another way, if an appeal is to proceed through argument and decision by a panel of judges, CAMP procedures cannot be counted on to quicken the pace.

This evidence on the briefed and argued cases, when viewed in relation to the evidence on settled or withdrawn cases, strongly suggests that the time CAMP saved in civil appeals processing can be accounted for by the time reductions achieved in the settled or withdrawn cases.

Conclusion

This chapter has analyzed CAMP's effectiveness in cases that were assigned by a truly random process to experimental or control groups. The experimental cases received scheduling orders and preargument conferences as provided under the CAMP rules; both procedures were withheld from the control cases.

One CAMP goal was to reduce the burden on the judges by eliminating appeals that otherwise would require judge attention; the evidence in support of that goal appears wanting. No statistically significant improvement was detected, using a variety of measures. This study measured three categories to infer a reduction of judge burden: appeals adjudicated, appeals involving at least minimal judge effort, and appeals fully briefed and argued. Research results using the first two categories were sufficiently ambiguous to warrant a suspended judgment on CAMP effectiveness: it is not yet warranted to conclude that CAMP is effective, but it is not possible to say that CAMP is ineffective. In the third category (appeals briefed and argued), the evidence strongly suggests that CAMP does little or nothing to remove these most burdensome cases from the court's docket. By this measure, the data do not support earlier expectations.

The cases were divided into separate chronological periods and analyzed both across and within these periods. There was no statistically significant improvement either across time periods or within any time period, suggesting no increased effectiveness as a result of "on the job" experience.

CAMP did cause a reduction in elapsed time from filing the notice of appeal through termination, but this seems to be a result of significant reductions in elapsed time for settled or withdrawn appeals. CAMP did not prove effective in expediting the appellate process for cases which ran the entire appellate gamut, from notice through argument and decision.

The evaluation of CAMP ought not to be based solely on the evidence from the cases. CAMP was also intended to improve the quality of appeals. The next chapter will examine this issue as seen by the judges of the Second Circuit.

CHAPTER FIVE

MEASURING CAMP EFFECTS:

EVIDENCE FROM THE JUDGES

CAMP's potential value extends beyond reducing the flow of cases through the appellate process or expediting appeals to oral argument. CAMP may also be an effective device to improve the quality of appeals reaching the court for decision. Theoretically, this improvement can be achieved through the preargument conference, when counsel can examine the issues to be raised on appeal, and can benefit from the candid views of staff counsel. These free and open exchanges can highlight weaknesses or omissions that otherwise might have been overlooked. The forum provided by CAMP also permits counsel to agree on designating the record, filing a joint appendix, or removing some procedural snag encumbering the appeal.

In some respects, this theory of CAMP can only be validated by the participants themselves.[1] But it is reasonable to assume that if CAMP improves the quality of appeals, the judges should be able to discern this improvement, and the researchers should be able to measure it.

Of course, some would argue that CAMP cannot change a poor advocate into a great one, and any search for improvement would be a foolish exercise. The plan's goal is not to remake counsel, however, but to bring about some modest yet measurable difference in the presentation of appeals.

Measuring the quality of appeals is no small task, and guidance is wanting. A serious question raised at the outset was whether judges would be consistent in

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1. See ch. six for attorneys' views concerning CAMP.

their responses to CAMP. Thoroughly inconsistent responses concerning the same appeal would limit or foreclose analysis. It was also plausible that one judge would operate according to one set of standards, and a different judge to another set. But even if the standards applied by the judges varied, it was hoped that the group of all judges would find CAMP noticeably improved cases. Of course, some judges, by experience or inclination, may be more sensitive to questions of quality than are others. This suggests that some judges would discern significant improvement, while others would not.

The device for assessing the quality issue was a questionnaire administered to all judges in all briefed and argued cases in the experimental and control groups.[2] All the judges on a panel were asked to complete the questionnaire, to determine the consistency of responses among judges hearing the same appeal.

The system used to manage this phase of the experiment should not go unmentioned, since it may account in part for the judges' extraordinary response rate. Copies of the day calendar (the weekly panel designations and cases to be argued each day) were regularly sent to the Federal Judicial Center. Only the appeals in the experimental and control groups (not every appeal on the calendar) were evaluated. Once a case was identified as belonging to the evaluation pool of cases, letters were drafted to each judge on the panel, indicating the need for an evaluation of one or more appeals set for argument that day. These letters were timed to reach the court shortly before the day of argument. All letters were sent to the United States Courthouse, to reduce mishaps such as misplaced or forgotten evaluations--especially for the judges whose chambers were located outside New York City.

Each letter was accompanied by one or more questionnaires for each case to be argued that day. Every

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2. The questionnaire is in Appendix III.

questionnaire contained a docket number and an evaluation control number which, in coded form, identified both the case as experimental or control and the name of the judge completing the form. The questionnaires were logged both when they were mailed and when they were returned. If a questionnaire was not returned within a reasonable period, the judge was alerted to the missing form.

These elaborate management efforts were designed to achieve a high response rate--this kind of exercise especially held the potential for a diminishing rate of response across time. The actual response rate of 93 percent surpassed all expectations. The judges are commended for bearing this burden, which helped to rigorously examine their court's procedures.

The questionnaire was designed to determine whether statistically significant differences in quality could be discerned between experimental and control cases. But what constitutes quality in an appeal and how would you recognize it if you saw it? An appeal would seem of superior quality if all the issues necessary and sufficient to decide the appeal were clearly and concisely presented, both in briefs and oral argument. This suggested a series of questions about the presence or absence of particular quality components. The presence of clarity was good; its absence, bad. The absence of redundant arguments marked a good appeal; their existence marked a poor one. The lack of undisputed or extraneous issues pointed to a strong presentation; presence of such issues suggested a weak one. The omission of essential issues was a sign of a poor appeal; inclusion of all essential issues was a sign of a strong appeal. Hence, the presence or absence of these components in briefs and oral argument would provide a reasonable basis for concluding that one group of cases was or was not superior in quality to the other.

It was quite possible to err in observing the presence or absence of these attributes of quality. As a check against such errors, two questions were included to evaluate the preparation by appellant's and appellee's counsel. Another question was added to provide an overall evaluation of the appeal, on the ground that it might be easier to evaluate an appeal than to identify components of quality in it.

The questionnaire also provided an opportunity to check the judges' views of staff counsel's screening decision and his relative effectiveness. One question asked whether the appeal could have been improve further; another inquired whether it could have settled.

In sum, this judge survey was designed to systematically obtain judge impressions of CAMP in briefed and argued appeals. The answers would determine whether, by omission or commission, in particular or in general, the experimental cases were significantly better prepared and argued than the control cases.

At the beginning of this phase of the evaluation, the assumption was made that the judges would not know whether the cases being evaluated were experimentals or controls. If some judges knew of the random assignment prior to completing the survey, their responses might be biased. The questionnaire form was revised, shortly after the first sets of questionnaires were mailed, to remove this possibility of bias. From the 382 judge responses, a handful, in which prior knowledge of the random assignment may have affected judgments of quality, were isolated. These 16 responses were removed from the analysis.

It is important to remember that the analysis in this chapter is based on judge observations of quality, not on cases. On the basis of these judge observations, inferences are drawn that the cases were or were not improved because of CAMP.

The information analyzed here is based on fewer than all judge observations in the evaluation. The difference between the total pool of judge responses and the smaller set available for analysis here results from the time lag in preparing the questionnaires for use on the Federal Judicial Center computer. The information pool consists of 382 returned judge questionnaires commenting on 134 argued appeals, of a possible 495 for the 165 briefed and argued cases in the study. This means that, at minimum, estimates of CAMP's effect on quality of appeals will be based on about 77 percent of the possible data.

Three hundred ninety-eight questionnaires were mailed to the judges who sat in the 134 appeals. Of the 382 questionnaires returned, 370 were completed, giving a response rate of nearly 93 percent. Whatever weaknesses can be found in this part of the evaluation, bias resulting from failure to complete and return the questionnaire is not one.

A preliminary issue must be addressed before comparing the data on quality of appeals in the experimental and control groups. If the respondents in this survey always agreed with each other when asked to rate the same appeal, the propositions that the survey questions are clear and that the criteria for judgment are similar for each panel of judges, would tend to be supported. On the other hand, constant disagreement among the judges asked to rate the same appeal would tend to cast doubt on the precision of the questions or on the criteria the judges employed in determining their answers.

What is the extent of agreement and disagreement in this survey? Table 11 presents a summary of agreement and disagreement on appeals rated by at least two judges. In this table, and in all others in this chapter, sixteen responses were excluded because the judges indicated they had prior knowledge of the random assignment. (Seven appeals were rated by only one judge. These were excluded from table 11.)

"Total agreement" was defined as "identical judge responses to the same question in the same appeal." "Some disagreement" was defined as "different judge responses to the same question in the same appeal." "Extreme disagreement" (for questions offering three possible answers) meant "different judge responses that encompassed the range of answers to the same question in the same appeal." Of course, any disagreement in a two-choice question could be interpreted as extreme disagreement. But by these definitions, "extreme disagreement" was intended to exclude "some disagreement."

The frequency of agreement and disagreement was determined for the questions in the judge survey (in Appendix III); it corresponds to the categories in the left margin of table 11. Except on questions 1, 2, 10,

TABLE 11

AGREEMENT AND DISAGREEMENT ON APPEALS RATED BY TWO OR THREE JUDGES

	Survey Questions												
	1*	2*	3	4	5	6	7	8	9	10	12*	13	14
Three judges rated													
Total agreement	33	42	75	65	60	64	66	74	73	41	32	54	58
Some disagreement	58	51	26	33	40	35	33	24	25	56	60	35	20
Extreme disagreement	7	3	--	--	--	--	--	--	--	--	3	--	--
Two judges rated													
Total agreement	13	12	17	15	17	18	16	17	16	15	15	15	15
Some disagreement	8	8	5	7	5	4	5	4	6	7	6	7	4
Extreme disagreement	0	0	--	--	--	--	--	--	--	--	1	--	--
Total cases rated by two or more judges	119	116	123	120	122	121	120	119	120	119	117	111	97

Note: The questionnaire is in Appendix III.

*Three possible choices were offered in these questions. The judges were to select one of the three. In all other questions, only two choices were offered.

and 12, there was far more agreement than disagreement among judges rating the same appeal. The questions calling for judgments on overall quality (questions 1, 2, and 12) provoked more frequent disagreement than most of the questions calling for identification of particular components of quality (questions 3-9). The differences in frequency of agreement (or disagreement) between quality and component questions may be a function of the greater number of choices offered in the quality questions, compared to that in the component questions. They may also reflect fundamental distinctions between qualitative decisions and component recognition.

The greatest threat to this survey's reliability would have been substantial extreme disagreement on all questions. This would have strongly suggested that the judges are so inconsistent that interpretation of the responses becomes equivalent to divination. On the questions of overall quality (in which the respondents were given three choices), there is relatively little extreme disagreement; that is, the responses rarely encompassed the entire range of answers to the same question on the same appeal. In only one two-choice question (question 10), was there more frequent disagreement than agreement. This question was eventually removed from the analysis because of this disagreement, which may plausibly be attributed to imprecision in the question itself.

With some assurance that interpretation of the data is possible, what do the responses reveal about the quality of CAMP cases compared to that of the controls? The following analysis first examines the particular components of quality, followed by consideration of the qualitative judgments.

One element of a superior appeal is the clarity with which the issues are presented to the court, both in briefs and in oral argument. If judge observations of clarity are found in significantly greater proportion for the experimental group of cases, compared to the control group, one can infer that (1) the first group is superior in quality to the second, and (2) the improvement in quality is caused by CAMP.

Table 12 summarizes the judge responses to the question: Were the issues raised in the appeal clearly brought out in the briefs?

TABLE 12

PERCENTAGE OF JUDGE RESPONSES AFFIRMING CLARITY IN BRIEFS

Experimental Group	Control Group
(N=262)	(N=90)
85%	84%

$p = .40$

The observed presence of clarity in the briefs is almost exactly the same in the experimental group as it is in the control group. Eighty-five percent of the judge responses in the experimental group and 84 percent of the judge responses in the control group affirmed that the issues were clearly brought out in the briefs. The difference between groups is not statistically significant, since a difference of the magnitude observed here could occur by chance more frequently than 5 times in 100.

The judges were also asked: Were the issues raised in the appeal clearly brought out in the argument? Table 13 presents the affirmative answers to this question. If the percentages in the experimental group are significantly greater than those in the control group, the improvement in clarity can be attributed to CAMP.

The percentages of affirmative responses are the same in answer to this question as they were in answer to the preceding one. Eighty-five percent of the judge responses in the experimental group and 84 percent of the judge responses in the control group affirmed that the issues were clearly brought out in the argument. Since clarity of argument seems to be present to almost exactly the same degree whether CAMP applies or not, one cannot conclude that CAMP procedures make arguments on appeal clear, at least according to the judges.[3]

TABLE 13

PERCENTAGE OF JUDGE RESPONSES AFFIRMING CLARITY IN ARGUMENT

Experimental Group	Control Group
(N=258)	(N=91)
85%	84%

$p = .34$

Some readers may wonder why these separate questions elicited exactly the same proportional responses. Certainly, when one finds a clearly presented brief, one will tend to find a clearly presented oral argument. The two observations are not perfectly correlated, however. Judges sometimes observed clarity

3. Of course, it could be argued that CAMP improves the relative degree of clarity. While most appeals meet minimum standards for clarity, CAMP may enhance that clarity. The relative degree of clarity is not addressed in this questionnaire, which attempts to identify the presence or absence of clarity in briefs and arguments.

in arguments but not in the briefs, and vice versa.[4]

The presence of clarity was taken as an indicator of quality in analyzing the responses to the preceding questions. Certain components, by their absence, can also be used as indicia of quality. One such indicator is the absence of undisputed or extraneous issues. Is CAMP helpful in eliminating such issues from appeals that would otherwise raise them? The judges were asked: Were undisputed or extraneous issues briefed? If CAMP is effective in this area, there should have beem significantly more negative responses in the experimental group than in the control group. Table 14 summarizes the judge responses to this question.

TABLE 14

PERCENTAGE OF JUDGE RESPONSES INDICATING UNDISPUTED OR EXTRANEOUS ISSUES WERE NOT BRIEFED

Experimental Group	Control Group
(N=262)	(N=92)
82%	75%

$p = .06$

Eighty-two percent of the judge observations in the experimental group and 75 percent of the judge

4. The correlation coefficient, which measures the association between the clarity-of-briefs and the clarity-of-argument responses, is fairly high ($r=.72$). When the answers match perfectly, the correlation coefficient is one. The complete absence of association produces a correlation coefficient of zero.

observations in the control group noted the absence of undisputed or extraneous issues in the briefs. But since the likelihood of observing a difference of this magnitude or greater is more than 5 in 100, it is unwarranted to conclude that CAMP reduced the briefing of undisputed or extraneous issues.

A similar question was posed to the judges concerning oral argument: Were undisputed or extraneous issues argued? Table 15 offers a summary of these responses. The greater the proportion of negative responses, the better the appeals.

TABLE 15

PERCENTAGE OF JUDGE RESPONSES INDICATING UNDISPUTED OR EXTRANEOUS ISSUES WERE NOT ARGUED

Experimental Group	Control Group
(N=259)	(N=92)
85%	78%

$p = .07$

Eighty-five percent of the judge responses in the experimental group indicated the absence of undisputed and extraneous issues in oral argument, while slightly fewer responses in the control group (78 percent) noted the absence of such issues. The experimental group scored better than the control group, but the difference was not sufficient to meet the threshold of statistical significance. Perhaps because these last two questions are nearly identical in focus, phrasing and location in the questionnaire, it is not surprising to see similarities in the pattern of answers.[5]

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5. The correlation coefficient for these two questions is also high (r=.80).

The absence of redundant issues is also an indicator of quality. In theory, CAMP should help focus attention on the central issues, and perhaps dispose of unnecessary, including redundant, issues.

The judges were asked: Were any briefed issues redundant? The extent to which appeals lacked redundancies can be found in table 16. The greater the proportion of negative responses, the better the appeals.

TABLE 16

PERCENTAGE OF JUDGE RESPONSES INDICATING REDUNDANT ISSUES WERE NOT BRIEFED

Experimental Group	Control Group
(N=260)	(N=92)
85%	74%

$p = .01$

Eighty-five percent of the judge observations in the experimental group and 74 percent of the judge observations in the control group noted the absence of redundant issues in the briefs. The experimental group scored significantly better, in the statistical sense, than the control group. Differences of this magnitude or greater could happen by chance fewer than 5 in 100 times. Hence, CAMP can be credited with the relatively greater absence of redundant issues in the briefs, as observed by the judges.

Having met the standard for statistical significance in the matter of redundant issues in the briefs, we must now estimate the strength of this CAMP effect on quality. It is important to return to a concept introduced in chapters two and four of this report. To what extent does the use of CAMP procedures in an appeal sharpen a prediction that the briefs in an appeal lack redundant issues?

The statistical measure used to answer this question is sometimes called the index of predictive association.[6] In this context, it measures the extent to which CAMP can aid prediction of improvement in quality. If the value of this measure were 100 percent, the use of CAMP would be a perfect predictor of the quality indicator. If the measure were zero, CAMP use would not enhance a prediction of the quality indicator. For example, suppose that in all the control observations, the judges noted the presence of redundant issues in the briefs, and in all the experimental observations, they noted the absence of redundant issues in the briefs. This would mean that knowing whether CAMP procedures were applied to an appeal would provide a certainty of the presence or absence of redundant issues in the briefs. In this example, the index of predictive association is 100 percent. Suppose now that in half the control observations and in half the experimental observations, the judges noted the presence of redundant issues in the briefs. This would mean that knowing whether CAMP procedures were applied to an appeal would provide no better assurance of the presence or absence of redundant issues. In this example, the index of predictive association is 0 percent. In short, this index provides a yardstick to measure improvement. With it, one can measure how much improvement (from 0 to 100 percent) is attributable to CAMP.

Based on the evidence in table 16, knowledge that CAMP procedures were applied to an appeal will improve by about 1 percent the likelihood that that appeal will not contain redundant issues.

How can such a trivial improvement be statistically significant? Remember that significance in the statistical sense has absolutely nothing to do with practical or research significance. In this context, statistical significance merely assures that CAMP has an effect greater than zero. As a general rule, the

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6. This measure is discussed in Hays, Statistics For the Social Sciences 413-17 (2d ed. 1973).

greater the number of units to be analyzed in the experiment, the smaller the effect required to demonstrate statistical significance. With more than 350 judge observations analyzed in this experiment, minute effects can be identified and labeled statistically significant. Estimating the strength of an association takes on paramount importance and searching for statistical significance becomes less important as the number of observations increases.[7]

The absence of redundant issues in oral argument was also used as an indicator of quality. The judges were asked the following question: Were any argued issues redundant? The responses will be found in table 17. The greater the proportion of negative responses, the better the appeals.

The experimental group scored higher on this indicator (90 percent) than did the control group (84 percent), but the difference in scores is not statistically significant.

This question and its mate (concerning redundant issues in briefs) are also strongly correlated with each other. The absence of redundant issues in briefs

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7. "All too often the experimenter... 'kids himself' into thinking that he has discovered some relationship observable to the 'naked eye,' which will be applicable in some real-world situation. Plainly, this is not necessarily true. The [index of predictive association]...suggests just how much the relationship found implies about real predictions, and how much one attribute actually does tell us about the other. Such indices are a most important corrective to the experimenter's tendency to confuse statistical significance with the importance of results for actual prediction. Virtually any statistical relation will show up as highly significant given a sufficient sample size, but it takes a relationship of considerable strength to enhance our ability to predict in real, uncontrolled situations." (Id. at 749.)

implies a great probability that the argument will not contain redundant issues.[8]

TABLE 17

PERCENTAGE OF JUDGE RESPONSES INDICATING REDUNDANT ISSUES WERE NOT ARGUED

Experimental Group	Control Group
(N=257)	(N=92)
90%	84%

$p = .06$

It is intriguing that of the three components of quality analyzed so far--clarity, extraneousness, and redundancy--oral argument scored as well or better in quality than briefs, in both the control and experimental groups. This may suggest that judges apply different standards when identifying the same indicators of quality for briefs and for oral argument. If the standards do not vary between briefs and oral argument, however, the data suggest an oral presentation may be better, in some respects, than a written one.[9]

Another question in the evaluation focused on the presence or absence of essential issues in the briefs. The theory behind CAMP was that the preargument conference would reduce the issues to the essentials and focus on them. Would CAMP significantly reduce the

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8. The correlation coefficient for these two questions is .76.

9. This suggests a certain efficiency in relying on oral argument, which the Second Circuit--to its credit--has managed to retain in virtually all of its cases.

omission of essential issues? The judges were asked: Were any essential issues omitted from the briefs? The greater the proportion of negative responses, the better the quality of the appeals, as viewed by the judges. Table 18 summarizes the answers to this question.

TABLE 18

PERCENTAGE OF JUDGE RESPONSES INDICATING NO OMISSION OF ESSENTIAL ISSUES FROM BRIEFS

Experimental Group	Control Group
(N=260)	(N=91)
83%	89%

$p = .94$

The results here are not significant and are counter-intuitive. Eighty-three percent of the judge observations in the experimental group noted no omission of essential issues, while 89 percent of the observations in the control group noted no omission.[10]

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10. The companion question attempted to identify the absence of essential issues at argument: "Did the court have to direct counsel to critical issues during argument?" The question possessed troubling ambiguities which make it difficult, if not impossible, to analyze. For example, does "the court" mean the judge observer, a colleague on the panel, or all three judges? Does "direct" mean posing a question to elucidate the issue or does it mean framing the issue for counsel to then address? With such uncertainty in the question, analysis seems foolish. This confusion is corroborated by the data in table 11. Note that judges observing the same

The analysis in this chapter has focused on the ability to identify certain features of appellate advocacy. These features, by their presence or absence, could function as indicators of quality in appellate litigation. Except in one question concerning redundancies in briefs, there was no statistically significant difference between the experimental and the control groups. In the one circumstance where statistically significant results were observed, the degree of association between CAMP and the indicator was slight.

These slender results may mean the identification of quality indicators is not an easy task. Even if expected differences cannot be found in these indicators, one can nevertheless measure differences in quality independently of underlying components that may give rise to quality appeals. In addition to being asked to note the presence or absence of indicators, the judges were asked qualitative questions about three facets of each appeal. Two of these centered on counsels' efforts; the other required an overall assessment of quality for each appeal.

The first of these questions was: Was the preparation of appellant's counsel (1) better than average; (2) average; or (3) worse than average, for cases of approximately the same complexity?

The choices were coded: "better than average" was given a value of "1," "average" was given a value of "2," and "worse than average" was given a value of "3." If CAMP improves counsel's preparation, there should have been a significantly lower average score for observations in the experimental group than in the control group. Table 19 summarizes the results.

Analysis of the results demonstrates that the average experimental group score was significantly

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cases disagreed more on this question (10) than on any other similar question calling for the identification of quality components.

better than the control group score. Thus, the improvement in the preparation by counsel is attributable to CAMP.

TABLE 19

PREPARATION OF APPELLANT'S COUNSEL: AVERAGE SCORE

Experimental Group	Control Group
(N=259)	(N=92)
1.85	2.09

$p = .001$

How much does CAMP aid prediction of the quality of counsel's preparation? To put the matter another way, how much variation in quality is explained by the presence (or absence) of CAMP procedures? For example, if all the judges rated the experimental cases above average and the control cases below average, the fact that CAMP procedures were applied in an appeal would provide certainty about the quality of the cases as viewed by the judges. If all the judges rated experimental and control cases exactly the same, however, use of CAMP procedures in an appeal would provide no assistance in determining its quality as seen by the judges. The estimated improvement in predicting CAMP's effect on the quality of counsels' preparation is about three percent.

The judges were also asked to evaluate appellee's counsel: Was the preparation of appellee's counsel (1) better than average; (2) average; or (3) worse than average, for cases of approximately the same degree of complexity?

The scoring scheme used for the preceding evaluation question was also employed here. Table 20 sets out the results.

TABLE 20

PREPARATION OF APPELLEE'S COUNSEL: AVERAGE SCORE

Experimental Group	Control Group
(N=255)	(N=91)
1.75	1.96

$p = .001$

The difference between scores is statistically significant. CAMP procedures improve the preparation by appellee's counsel.[11] The estimated strength of this relationship between CAMP and counsel preparation is about three percent: there is improvement, but it is on a fairly low order of magnitude, as best it can be measured.

The last question called for an evaluation of the appeal as a whole: Overall, how would you rate the quality of this appeal with respect to the presentation of issues (both written and oral) to the court: (1) above average; (2) average; or (3) below average? The scoring sceme was the same as that for the two preceding questions: the better appeal was given the lower score. The results are summarized in table 21.

The difference between average scores is sufficient to warrant the conclusion that the relationship between CAMP procedures and quality is statistically significant. Again, CAMP improves overall quality by about three percent.

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11. The correlation coefficient for appellant's and appellee's counsel evaluations is .60. This means that when appellant's counsel is either well or ill prepared, there is no guarantee (but some assurance) that his adversary will follow the same path.

TABLE 21

OVERALL JUDGMENT OF QUALITY: AVERAGE SCORE

Experimental Group	Control Group
(N=257)	(N=90)
1.87	2.12

$p = .001$

Conclusions drawn from all three of the evaluation questions--in contrast to most of the indicator questions--supported CAMP effectiveness. Although nearly all the results for indicator questions were unable to meet the minimum threshold requirement for statistical significance, the data favored CAMP in most cases.

Are general evaluative questions better than the search for quality indicators as a means of measuring quality in appeals? If so, perhaps the results across all questions--both general and specific--are consistent. The specific indicator questions produced positive but weak results in favor of CAMP--too weak to reach statistical significance. The general evaluative questions passed the significance threshold, but further examination of that data indicates that whatever improvement CAMP brings about is slight. One can only speculate that had the presence or absence of indicators revealed greater differences between groups, the predictive power of CAMP for the general questions would have increased.

Sixteen judges from the Second Circuit participated in this evaluation. The set of responses from each was analyzed separately to determine whether any judges consistently found the experimental group to be significantly better in quality than the control group. Of course, with fewer observations from any one judge, the differences between groups would have to have been much greater to reach statistical significance. Of the thirteen judges who evaluated at least ten cases, none rated the experimental group consistently better in quality than the controls.

Certainly, statistical significance was achieved in favor of CAMP on some questions, but occasionally the controls were viewed as better than the experimentals. If the plan has a substantial effect on the quality of appeals, it seems reasonable to have expected statistically significant differences between groups as viewed by at least some of the individual judges. The absence of such significant differences for any of the judges (those who evaluated at least ten cases) suggests that the plan's effect on quality is so slight that it was not consistently discernible to a single judge. The aggregated judge observations produced some statistically significant differences, but the improvement caused by CAMP seems slight. This observation is consistent with the analysis of individual judge observations.

The judge evaluation served yet another purpose. CAMP was based on the view that perfected appeals--those that are briefed and argued--are amenable to private dispute resolution if efforts are made early in the life of the appeal to encourage settlement or withdrawal. If CAMP works effectively to eliminate cases that otherwise would be argued and decided, there should have been more expectations of settlement or withdrawal in the control group than in the experimental group. As another check on the case evidence, the judges were asked: Would you have expected a preargument conference before the filing of briefs to result in a settlement or withdrawal of this appeal? The answers are summarized in table 22.

The data point in the anticipated direction, with more expectations for settlement in the controls (15 percent) than in the experimentals (13 percent), but the difference is not statistically significant. This is consistent with the findings in chapter four, in which analysis of the case information suggested CAMP causes no statistically significant reduction in briefed and argued cases.

The judges were also asked: Would you have expected a preargument conference to improve the quality of

TABLE 22

PERCENTAGE OF JUDGE RESPONSES AFFIRMING EXPECTATION OF SETTLEMENT OR WITHDRAWAL

Experimental Group	Control Group
(N=236)	(N=81)
13%	15%

$p = .35$

this appeal beyond that which was presented to you in briefs and oral argument? This question also attempted to cross-check CAMP effectiveness in the improvement of appeals. If CAMP significantly improves the quality of appeals, there should have been proportionally more expectations of improvement for controls than for experimentals. Table 23 summarizes these results.

TABLE 23

PERCENTAGE OF JUDGE RESPONSES AFFIRMING EXPECTATION OF FURTHER IMPROVEMENT

Experimental Group	Control Group
(N=254)	(N=86)
15%	14%

$p = .55$

The levels of expectation were nearly the same whether CAMP was applied or withheld, although the results are slightly counter-intuitive (the judges' expectations were greater for experimentals than controls). About 15 percent in each group of observations noted an expectation of further improvement.

Once again, the judges were unable to discern any substantial benefits (at least any that have been ascribed to CAMP) from the program.

Conclusion

This chapter has analyzed CAMP's effectiveness as viewed by judges who sat on appeals to which CAMP procedures were applied or withheld according to a truly random process. The primary question considered here was whether CAMP has an appreciable effect on the quality of appeals. Quality was measured by judge observations of the presence or absence of specific indicators or components of quality in appellate litigation. From the degree of presence or absence of these indicators, one can make relative judgments about the quality of the experimental cases (to which CAMP procedures were applied) and that of the control cases (from which CAMP procedures were withheld).

Of the eight specific indicator questions, only one warranted the conclusion that the experimental group was superior in quality to the control group. The judges were also asked three questions about overall quality. These obervations supported the view that CAMP causes a statistically significant improvement in the quality of counsel preparation and in the overall quality of the appeal. Further analysis suggests that the plan's effect on quality--either as observed in any of the specific indicator questions or in the three general evaluative questions--is of a fairly low order of magnitude.

This evidence is also consistent with the analysis of observations by each judge who participated in the evaluation. On some questions, judges observed significant differences in favor of CAMP (although the ratio sometimes favored the control cases). But no judge consistently observed the experimental cases to be substantially better in quality across more than a few indicators or general questions of quality. In sum, the evidence across all judges does not warrant the conclusion that CAMP substantially improves the quality of appeals in the Second Circuit.

CHAPTER SIX

MEASURING CAMP EFFECTS:

EVIDENCE FROM THE ATTORNEYS

CAMP's effectiveness can also be examined from the perspective of attorneys who were responsible for appeals in the experimental and control groups. Their evidence adds balance to the impressions drawn from the case information and the judge observations.

Attorneys were surveyed by mail, after cases were terminated in the Court of Appeals.[1] Attorney names were gleaned from the preargument conference log in the CAMP office and from the docket sheets. On occasion, law firms were contacted to determine the identity of the lead attorney.

The problems in a survey of this sort are substantial. Many of the administrative burdens of the judge survey were duplicated here. In addition, attorneys in the control cases could not be asked questions identical to those asked of attorneys in the experimental cases, because the attorneys in the control cases lacked the experience of CAMP in their own appeals. Both questionnaires have elements in common, but occasionally the questions vary slightly. These variations introduce an unknown degree of error that may ultimately bias the analysis.[2]

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1. An appeal is terminated when a certified order of dismissal or a judgment is entered on the docket sheet.

2. The two versions of the questionnaires are in Appendix II. CAMP Questionnaire Form A was sent to attorneys in the experimental group. CAMP Questionnaire Form C was sent to attorneys in the control group.

Follow-up letters and phone calls were used to encourage attorneys to complete and return their confidential responses. The attorney data were derived from responses concerning 262 cases in the evaluation (out of 302 cases altogether) which were ready for analysis on the Center's computer.

Six hundred thirty-seven attorneys were surveyed in these 262 cases; 559 questionnaires were completed and returned. This response rate of about 88 percent raises the possibility of bias in the analysis, due to a lack of responses. (By most rules of thumb in survey research, however, this response rate is quite acceptable.) An additional 80 to 90 questionnaires can be expected to be added to the file of cases not yet on the computer. The conclusions ventured in this chapter are subject to modification when the remainder of the data are included in subsequent analyses.

The distribution of the 559 attorney responses is shown in table 24.

TABLE 24

PERCENTAGE DISTRIBUTION OF ATTORNEY RESPONSES

Experimental Group	Control Group	Total
(N=415)	(N=144)	(N=559)
74%	26%	100%

The attorney responses are distributed in a ratio of about three to one between experimental and control groups. This is identical to the distribution of cases from which the names of the attorneys were drawn. Failure to obtain at least one response for each appeal occurred just once in the 262 cases available for analysis. There are generally about two responses for every appeal in the file.

The first task in this section is to examine the general background of the attorneys in the experimental and control groups to provide some assurance that the responding attorneys are equivalent as a group. Then comparisons are drawn across the two groups with the expectation--if CAMP is effective--that the experimental group will be significantly different from the control group. Finally, the unique features of the experimental group responses are explored, since these share the experience of CAMP.

Note that the unit of analysis here is an attorney response grouped according to the application or denial of CAMP procedures. These 559 responses do not represent 559 different attorneys. There are some "repeaters" in both groups. It was felt that attorneys appearing frequently should be included in this analysis to the extent that they appeared in different cases and could comment on their experiences in those cases. If an attorney completed six questionnaires, his six sets of answers were analyzed. The extent of this repetition affects some of the analysis only if his subsequent responses repeat his impressions of earlier appeals. Perhaps the most reasonable approach is to assume that if there is repetitious information, it is distributed randomly, i.e., it is not concentrated in one group rather than another.

Two general background questions are summarized in table 25.

TABLE 25

TWO GENERAL BACKGROUND VARIABLES

	Experimental Group (N=415)	Control Group (N=144)
Average proportion of legal work in federal appellate practice	12%	13%
Average years of law practice in the Second Circuit	12 yrs.	12 yrs.

The respondents, both in experimental and control groups, spend, on the average, about 12 percent of their time in federal appellate practice. And, on the average, these attorneys have practiced law in the Second Circuit for about 12 years. As expected, the respondents in the two groups are sufficiently alike to minimize the possibility of bias on questions in which significant differences supporting CAMP effectiveness were anticipated.

One CAMP goal was to improve the quality of appeals through modification of issues at or following the preargument conference. Modification could mean abandonment and/or addition and/or clarification of one or more issues. Attorneys in each group were asked an identical threshold question: Was there any modification of the fundamental issues in this appeal from the time the notice of appeal was filed? If CAMP is effective in modifying issues, there should have been significantly more affirmative answers to this question in the experimental group than in the control. Table 26 summarizes the results.

TABLE 26

PERCENTAGE OF ATTORNEY RESPONSES AFFIRMING MODIFICATION OF ISSUES

Experimental Group	Control Group
(N=400)	(N=136)
8%	5%

$p = .13$

Eight percent of the attorney responses in the experimental group revealed some modification in issues, compared to 5 percent of the attorney responses in the control group. This difference is not statistically significant, and therefore it cannot be concluded that CAMP causes modification of issues.

Attorneys were asked about the nature of the modification. Were issues abandoned? Added? Clarified?

Table 27 summarizes the responses to these elaborations on the modification issue.

TABLE 27

ABANDONMENT, ADDITION, OR CLARIFICATION OF ISSUES

Percentage of Responses Noting	Experimental Group (N=30)	Control Group (N=7)
Abandonment of issues	43%	43%
	p = .50	
Addition of issues	30%	14%
	p = .20	
Clarification of issues	63%	14%
	p = .04	

Note that the control group contains seven attorney responses out of a pool of 144, and the experimental group contains 30 attorney responses out of a pool of 415. Statistical significance should not obscure practical significance in these findings. According to the respondents in each group, issues were abandoned in the same proportion for experimental and control cases. Thus, no significant difference regarding abandonment of issues is attributable to CAMP.

Thirty percent of the respondents in the experimental group, compared to 14 percent of the respondents in the control group, noted the addition of one or more issues. Given the small number of observations, this difference was not statistically significant. On the clarity question, 63 percent of the respondents in the experimental group and 14 percent of the respondents in the control group noted clarification of issues. This difference is sufficient to warrant the conclusion that when issues are modified in appellate litigation, CAMP is effective in clarifying the issues.

Estimates of the strength of this association between CAMP and clarification of issues as seen by the attorneys requires a return to the notion of predictive association.[3] How much help does CAMP afford in predicting whether the issues will be clarified from the attorneys' perspective? For this data, a prediction about clarification can be improved by about 30 percent when use of CAMP procedures is revealed. This improvement must be balanced against the relatively infrequent observations of modification and clarification in this experiment. If modification were to occur more frequently, the benefit from CAMP--from the attorney's perspective--might be substantial.

A fundamental assumption of CAMP is that attorneys at the appellate level do not usually discuss the possibility of settlement with their adversaries. The appellate process is a "lonely process," according to this assumption. CAMP is designed to bring the adversaries together and, it is hoped, resolve their differences. Is this assumption well taken? The evidence from the cases certainly casts doubt on it, but the attorneys offered direct evidence on this issue. The questionnaire for the control group of respondents posed these questions: Did you confer with your adversary during the course of this appeal: (1) to explore settlement possibilities? (2) to limit or otherwise narrow issues? (3) for some other purpose?

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3. See Hays, Statistics For the Social Sciences 745-49 (2d ed. 1973).

This question had to be modified slightly for the respondents in the experimental group, to exclude from consideration affirmative answers resulting from the preargument conference. The experimental group's question was: Aside from the preargument conference procedure, did you confer with your adversary: (1) to explore settlement possibilities? (2) to limit or otherwise narrow issues? (3) for some other purpose?

The ability to draw inferences from significant differences between groups was, at this point, subject to criticism because the respondents were replying to nonidentical questions. The data are still useful in some respects. They are summarized in table 28.

TABLE 28

EXTENT OF ADVERSARY CONTACT

Reason for Conference	Experimental Group	Control Group
To explore settlement	57%	44%
(Total attorney responses)	(385)	(128)
To limit or narrow issues	24%	26%
(Total attorney responses)	(298)	(110)
For other purpose	28%	44%
(Total attorney responses)	(191)	(72)

Without examining the data for significant differences, it seems that roughly half the respondents in both groups talked with their adversaries about settlement possibilities. The greater frequency of adversary discussion in the experimental group may arguably be a result of the CAMP experience, but the data (drawn from nonidentical questions across groups) can only be suggestive, not probative, on this point. About one quarter of the respondents in both groups conferred to limit or narrow issues, and roughly a third met for some other purpose.

Far from being a lonely process, appellate practice in the Second Circuit is marked by substantial adversary contact for a variety of purposes. Perhaps this suggests why the results drawn from the case information and judge observations required a Scotch verdict on CAMP effectiveness. The fundamental premise of the program does not seem to be supported by the evidence.

Attorneys in the experimental group were polled on the extent of their contact with CAMP proceedings, primarily the preargument conference. All the cases in the experimental group were designated as meriting both a preargument conference and a scheduling order. Of the pool of attorney respondents associated with the experimental cases, nearly all (386 of 407, or 95 percent) noted a preargument conference had been scheduled. Nearly all these respondents (374 of 394, or 95 percent) noted a preargument had been held. And, of all experimental group respondents, most had been present at the conference (358 of 415 respondents, or 86 percent). In sum, a substantial portion of the respondents in the experimental group had some direct experience with CAMP upon which to based their responses.

One of the questions posed to attorneys in the experimental group attempted to gauge the necessity of the conference procedure in the settlement or withdrawal of appeals. The attorneys were asked: If a preargument conference was held and, subsequently, the appeal was settled (or withdrawn), did the conference cause the settlement (or withdrawal)?

It must be recognized that the question did not serve its purpose.[4] Since the attorneys did not know what would have happened in the absence of a preargument conference, the answers to the question are fundamentally speculative. The evidence from the cases is perhaps more persuasive than the beliefs of the attorneys. Nevertheless, some information can be derived from the answers. Of the 180 valid responses to the question, about two-thirds of the respondents answered affirmatively, while one-third answered negatively. These results suggest a substantial portion of the respondents in the experimental group believe the preargument conference was a causative factor in the settlement or withdrawal of their appeals.[5]

The results from the case information suggest CAMP has no significant effect on the burden of the judges. If the attorney responses are taken at face value

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4. The question should have been framed: In your opinion, would this appeal have been settled (or been withdrawn) without the intervention of the preargument conference? But even this question would not be free from criticism, for it requires the respondent to speculate, since the appeal in question received a conference. Thus, an attorney could believe that the conference was necessary (and answer "no" to the question), but it would still be legitimate to conclude, on the basis of direct evidence, that the conference was not in fact necessary to settlement or withdrawal. This direct evidence would be found in the analysis of the cases.

5. This result echoes the views of attorneys polled by the Association of the Bar of the City of New York: See Comm. on Fed. Courts, The Ass'n of the Bar of the City of New York, "The Pre-Argument Conference Experiment of the Second Circuit Court of Appeals: A Report on a Sampling of Attorneys' Assessments of the Pre-Argument Conference Procedure" (June 24, 1975).

without qualification, aren't these two impressions incompatible? First, increasing the number of settlements does not necessarily imply a reduction in judicial burden. Experience has shown that some settlements are reached after a tangible investment in judicial resources has been made. Second, these attorney responses may also reflect a shift to settlement or withdrawal from appeals that otherwise would be dismissed on motion or "so ordered" by the clerk. And, third, the attorney responses reflect intuitive judgments, which are clearly not probative of the necessity for the conference procedure. In sum, the impressions from the case information and those from the attorney responses are not incompatible.

Earlier in this chapter, doubt was cast on one of the fundamental premises of the CAMP program by comparing the frequency of informal adversary discussion between experimental and control groups. About half the respondents in each group indicated they conferred with their adversaries to discuss settlement. The attorneys in the experimental group were also asked to respond to the following question: Would you have explored with your adversary the possibility of settlement or withdrawal of this appeal, in the absence of the CAMP program? Fifty-two percent of the 385 respondents answered in the affirmative. These results support two possible interpretations. One is that at least half the attorneys regard such discussions as part of their routine. A second interpretation is that all attorneys consider such discussions half of the time, not as part of their routine, but as a response to the circumstances of particular cases.

Informal discussion between adversaries is not confined to the issue of settlement. About 25 percent of the respondents in each group indicated they conferred to limit or otherwise narrow issues. The attorneys in the experimental group were also asked to respond to the following question: Would you have explored with your adversary the possibility of limiting issues in this appeal, in the absence of the CAMP program? Thirty-seven percent of the 358 respondents answered in the affirmative. This evidence tends to confirm the impression that between one-fourth and one-third of the attorneys in this experiment meet with their adversaries as a matter of course to discuss the limitation of issues in their appeals.

Many of the respondents offered their own reactions to CAMP and its administration. Most of the comments were favorable to the plan, although there were exceptions. There were many commendations of its administration, but here, too, there were exceptions. It would be impossible to attempt a summary of these views without introducing contextual distortion, no matter how judicious the editing. Comments indicative of all the responses are in Appendix V, but not in the same frequency with which they will be found in the entire survey. Most respondents who offered their views frequently voiced opinions endorsing the plan and, to a lesser degree, its administration.

Conclusion

This chapter has analyzed the results of a survey of attorneys who were responsible for cases in the experimental and control groups of the CAMP evaluation. The results are in many respects consistent with the findings from the case information and the judge survey. The attorney survey, however, casts doubt on a fundamental premise of CAMP: that informal discussions with adversaries are rare or nonexistent at the appellate level.

A substantial portion of attorneys in the experimental group affirmed the CAMP conference as a causative factor in the settlement or withdrawal of their appeals. This speculation ought not to be viewed as probative of CAMP effectiveness. The more convincing, objective evidence is probably in the analysis of the cases.

One can take issue with the appropriateness of an attorney survey to demonstrate the value of CAMP to the Second Circuit. But one cannot deny the impressions of attorneys that CAMP is a cause of the disposition of their appeals. This attitude is also reflected in the additional comments of the respondents. In general, they are favorable to the plan and, to a lesser extent, the manner in which it is administered, but there are others who expressed contrary views.

CHAPTER SEVEN

CONCLUSIONS AND RECOMMENDATIONS

This evaluation has examined CAMP from a number of complementary perspectives. Each of these views is premised on the unique feature of this evaluation: the random assignment of appeals to experimental and control groups. This method provides the clearest proof of CAMP's effectiveness, compared to all other competing research approaches.

Based on the collected evidence concerning the 302 cases in the experiment, it would be unwarranted to conclude that CAMP reduces the burden on the judges. The reduction in burden was measured by three different standards: the proportion of adjudicated appeals, the proportion of appeals requiring some (minimal) judicial effort, and the proportion of appeals that were fully briefed and argued.

The plan was also designed to improve the quality of appeals that were fully briefed and argued. Quality was measured by comparing judge observations of quality components in experimental and control groups. The evidence here warrants a conclusion that CAMP improves overall performance, but the magnitude of improvement is slight. The judge responses also corroborated the evidence, drawn from the cases, that there was no discernible difference between experimental and control cases in the likelihood of settlement.

The analysis of the attorney responses indicates that issues on appeal are infrequently modified and that the modifications that do occur are not brought about by CAMP. When issues are modified, however, CAMP enhances clarification. Approximately half the attorneys in this survey also indicated they met with their adversaries to discuss settlement, and about a quarter of them revealed they met to limit or otherwise narrow issues. This was true for attorneys in both the experimental and control cases. These observations suggest that the premise "But for CAMP, attorneys would not confer" is without empirical support.

A substantial proportion of the attorney respondents in the experimental group felt CAMP was a causative factor in the settlement or withdrawal of their appeals. This is consistent with the impression drawn from a separate survey of the bar, and it does not refute the evidence on CAMP effectiveness, which was based on analyses of the cases in the experiment and the judge observations of quality.

Many, if not most, of the tests used to evaluate CAMP performance generally point in favor of the plan. The experimental group frequently scored better than the control group, occasionally rising to statistically significant levels. Although such uniformity in the direction of the evidence may be just a product of chance, it nevertheless suggests that CAMP has some effect on reduction in judge burden and on quality. But these effects are of a fairly small magnitude. The effect of the plan falls below preliminary suggestions;[1] indeed, if there is an effect, it is smaller than the more conservative estimates upon which the experiment was designed.

Is CAMP a failure? An easy answer is not possible. The evidence from this experiment certainly suggests that the plan does not yet live up to expectations. Frankly, it is difficult to find positive evidence of substantive value for the plan during the period of the evaluation. This does not warrant an immediate rejection of the CAMP idea, however. Further analysis may suggest conditions that could facilitate substantial effectiveness.

First, the initial enthusiasm for the CAMP idea was a product of judge participation in the preargument conference. Yet judge participation was not evaluated in this experiment. One can conclude only that staff-controlled conferences did not seem to significantly

1. Kaufman, The Pre-Argument Conference: An Appellate Procedural Reform, 74 Colum. L. Rev. 1094,1100 n.17 (1974).

reduce the burden on the judges. This experiment suggests that judge participation may be needed to achieve the desired reduction in overall judge burden.

Second, judges and administrators ought to examine the extent to which adversaries in appellate litigation communicate with each other, before the CAMP idea is adopted or rejected. One premise of CAMP is that the appellate process is "lonely." The evidence from the attorney survey suggests this is not so. If there are jurisdictions where the premise holds, and further, where encouragement of adversary communication will facilitate informal resolution or improvement of litigation, a CAMP program may be beneficial.

The lack of support for one of the important premises of the plan overlaps another concern that some observers may offer to limit further application of the CAMP idea. The Second Circuit, it is said, is *sui generis*. It derives nearly all its business from New York City, and most of that business comes from the Southern District of New York, the biggest of all the federal trial courts. The nature of appellate litigation is shaped by New York's commercial activities, which no other court's jurisdiction can equal. If the CAMP idea cannot work in New York, where conditions seem most favorable to the program--given the concentration of attorneys and the potential for conciliation in commercial claims--it cannot work anywhere. But this argument presumes that the concentration of attorneys and litigation is advantageous to the program. It is at least arguable that this concentration is the reason for the substantial amount of communication between adversaries. In other circumstances--where greater physical distance separates an attorney from the courthouse and from his adversary--CAMP procedures may be useful.

Third, this evaluation is incomplete in some respects. Although it covers many of the central issues, others remain to be analyzed. One of these other issues was considered in the research design and suggested by a number of attorneys responding to the survey, but lack of satisfactory evidence prevented its empirical verification. Essentially, the issue in question is as follows. A plausible side-effect from the plan is that it would encourage attorneys to pursue appeals that otherwise would not be pursued. The availability of a court-suggested compromise might return to a losing plaintiff a part of his investment in the litigation. CAMP might also encourage a losing defendant to take an appeal to diminish a trial court judgment through a settlement suggested by the appellate court. In short, there is something to be gained by appealing, at the cost of filing the notice of appeal and paying the docket fee. If CAMP were to induce appeals, the plan would be self-fulfilling. It would encourage the filing of appeals that CAMP would then resolve, but the plan would not in fact accomplish very much for the court.

The evidence needed to test this untoward by-product would be far less precise than the evidence drawn from this controlled experiment. At best, evidence would be suggestive, not probative, of the possibility of induced appeals.

The first step would be to measure and analyze the rate of appeal in civil cases for a period of years preceding the adoption of the plan.[2] If this rate of appeal were fairly constant for the years prior to CAMP, but increased sharply for the years after the plan went into operation, it might be suggested that induced demand had been fostered.[3] Unfortunately, the information needed to test this proposition is not readily available. Further experimentation with the CAMP idea should incorporate the induced demand

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2. This measure would compare the number of civil appeals filed in a given year to the number of appealable civil cases decided by the district courts in the same year. <u>See</u> Goldman, <u>Federal District Courts and the Appellate Crisis</u>, 57 Judicature 211 (1973).

3. Of course, this approach has all the weaknesses mentioned in the alternatives to the CAMP experiment (<u>supra</u>, chapter two). But since this problem does not lend itself to experimentation, ambiguous evidence may be better than no evidence at all.

issue into the research design, and efforts should be made to obtain the necessary evidence to confirm or disconfirm the proposition.

Fourth, Circuit Executive Robert D. Lipscher and his staff have collected additional information about the 302 cases in the CAMP experiment, in an effort to determine the circumstances under which the CAMP idea might fruitfully be continued. The analysis that follows is based on these data.

According to the theory justifying CAMP efforts at settlement,[4] appeals involving money judgments should be the appeals most amenable to informal resolution. When money is not the central issue in a dispute (as in "public interest" litigation), the chances for compromise seem much more remote. The matters in dispute were not central to this evaluation, because it was presumed that cases involving money judgments would be selected for the preargument conference. This presumption was based on descriptions of the plan in operation.[5]

The 302 appeals were sorted into two mutually exclusive categories: 77 of the 302 appeals (25 percent) belonged to the first category, in which a money judgment was awarded by the district court; and the remaining 225 appeals (75 percent) belonged to the other, in which no money judgment was awarded. It was not possible to determine from the additional information whether money was at issue but not awarded. Certainly, there were appeals in which money damages were sought but not awarded. These appeals were included in the "no money judgment awarded" category.

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4. Mack, Settlement Procedures in the U.S. Courts of Appeals: A Proposal, 1 The Justice System Journal 20-23 (1975) (issue 2).

5. Kaufman, supra note 1 at 1099-100; and Appeals to the Second Circuit 15-16 (1975) (prepared by the Committee on Federal Courts of the Association of the Bar of the City of New York).

If CAMP is especially effective in the informal resolution of disputes involving the award of money, there should have been significantly fewer briefed and argued appeals in the experimental group than in the control group, of all money judgment appeals. Table 29 presents these data.

TABLE 29

APPEALS IN WHICH MONEY JUDGMENTS WERE AWARDED IN THE DISTRICT COURT

	Experimental Group* (N=62)	Control Group (N=15)
Percentage of appeals that terminated after briefing and oral argument	48%	53%

$p = .40$

*includes one case that was settled after oral argument.

As shown in table 29, of all appeals in which money was awarded by the district court, 48 percent of the experimental group, compared to 53 percent of the control group, were perfected through the stage of briefs and oral argument. The evidence points in the anticipated direction, with 5 percent fewer experimental cases being briefed and argued. This difference is not statistically significant, however. A difference of this magnitude or greater could occur by chance about 40 percent of the time.[6] Because of the small

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6. Delving into the data this way can be risky, akin to a fishing expedition. A certain proportion of these tests yield statistically significant differences

number of appeals analyzed here, the confidence interval in which the true difference between groups would be "captured" is considerable. About nine out of ten times, the true difference will fall in the interval from -27 percent to +20 percent. Given this wide range of values, it seems better to suspend judgment than to conclude that CAMP is without any effect whatsoever.

It should be noted that a surprisingly small proportion of appeals in the experiment (about 25 percent) involved money judgments. If the staff counsel routinely selected money judgment cases for inclusion in the experiment, as descriptions of the plan imply, it seems that an expansion of CAMP activities to additional money judgment cases does not hold much promise.

What differences were observed across groups when money judgments were not awarded? Table 30 summarizes the evidence.

Again, slightly fewer experimental cases than control cases were perfected through briefing and oral argument when money judgments were not at issue. This difference is not statistically significant, and, therefore, it is unwarranted to conclude that the program effectively reduces the burden on the judges.

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differences merely as a matter of chance. A more stringent test than the 5-times-in-100 standard seems required when one is making repeated comparisons. See Ryan, Multiple Comparisons in Psychological Research, 56 Psychological Bulletin 26 (1959). By Ryan's standard, the higher stringency is determined by dividing the type one error, discussed on pages 31 and 32 (in this study, it is .05), by the number of comparisons to be made (using the Lipscher data, this is five) for a new significance level of .05 divided by five, or .01. Thus, the 'p' value would have to fall below .01 to conclude that CAMP, and not some chance fluctuation, was the cause of the difference between groups. See also Cook & Campbell, The Design and Conduct of Quasi-Experiments and True Experiments in Field Settings, in Handbook of Industrial and Organizational Psychology 232-33 (Dunnette ed. 1976).

TABLE 30

APPEALS IN WHICH NO MONEY JUDGMENTS WERE AWARDED IN THE DISTRICT COURT

	Experimental Group* (N=163)	Control Group (N=62)
Percentage of appeals that terminated after briefing and oral argument	56%	58%

p = .40

*includes one case that was settled after oral argument.

Another suggestion has emerged from these data: it concerns the stage in the course of the trial court litigation at which the appeal is taken. Arguably, the benefit of CAMP intervention varies with the willingness of the parties to compromise. Such compromises might be more readily accepted after the adversaries have been put to the ordeal of a trial and must confront, on appeal, a decision of judge and/or jury. At that point, the trial has, in effect, placed all the cards on the table. The estimation of risk in pursuing an appeal would seem more realistic and calculable following a final decision after trial. In an appeal from a pretrial judgment, however, the district court's decision might suggest that the issues were so clear as to warrant summary disposition. The likelihood of altering such a decision on appeal would therefore seem small. Given a pretrial judgment, it seems there would be little likelihood that the winning party would compromise, because of his higher expectation of affirmance on appeal. If the appeal were taken from an order of the district court, the merits of the dispute might have yet to be addressed. Hence, there would seem to be greater uncertainty about how the matter will be resolved. Why should the parties hammer out a settlement when there is a substantial chance of vindication by judge or jury?

This speculation suggests that appeals from trial judgments would be most amenable to CAMP procedures, while appeals from orders and pretrial judgments would be less likely candidates. At what stage in trial litigation were appeals taken for the 302 cases in the CAMP experiment? In 69 of the 302 (or 23 percent), appeals were taken from orders. Appeals arose from pretrial judgments in 116 out of the 302 cases (or 38 percent of the total), and in the remaining 117 cases (or 39 percent), appeals were taken from trial judgments.

Table 31 shows the effects of the plan on appeals arising from district court orders. If CAMP is effective, there should have been a substantially smaller proportion of briefed and argued appeals in the experimental group than in the control.

TABLE 31

APPEALS FROM ORDERS

	Experimental Group* (N=46)	Control Group (N=23)
Percentage of appeals that terminated after briefing and oral argument	46%	48%

p = .48

*includes one case that was settled after oral argument.

As shown in the table, there were fewer briefed and argued appeals in the experimental group, but the difference of only 2 percent is not sufficient to rule out chance as the explanation. The result seems consistent with previous speculation that CAMP effects would not be substantial here.

Table 32 examines the effectiveness of the plan for appeals arising from pretrial judgments.

TABLE 31

APPEALS ARISING FROM PRETRIAL JUDGMENTS

	Experimental Group (N=85)	Control Group (N=31)
Percentage of appeals that terminated after briefing and oral argument	59%	55%

$p = .66$

The results reported in the table are counter-intuitive. Four percent more of the experimental pretrial judgment appeals than the equivalent control cases survived through briefing and oral argument. Previous speculation would suggest that CAMP should have minimal effects here, too. These results are inconsistent with that speculation.

What is the evidence for appeals taken from trial judgments, the stage at which appeals may be most amenable to CAMP intervention? Table 33 presents the evidence.

According to the data in the table, there were 17 percent fewer briefed and argued appeals in the experimental group than in the controls. The difference between groups is greatest here. Because of the smaller number of cases in this part of the analysis (117 out of 302), this 17 percent difference is not statistically significant. Again, it would be unwarranted to conclude the plan is effective in reducing judge burden, but it would also be unwise to conclude that CAMP has no effect whatsoever.

TABLE 33

APPEALS FROM TRIAL JUDGMENTS

	Experimental Group* (N=94)	Control Group (N=23)
Percentage of appeals terminated after briefing and oral argument	53%	70%
	$p = .07$	

*includes one case that was settled after oral argument.

The results across each stage--appeals from orders, appeals from pretrial judgments, and appeals from trial judgments--are nearly consistent with previous speculation. Although clearly not probative, the data suggest CAMP's promise may be fulfilled by a concentrated effort on appeals taken after trial judgment. If it is assumed that staff counsel selected every appeal in which there was a chance of informal resolution, it is not unreasonable to infer that most of the appeals from trial judgments that were amenable to compromise were "captured" by the experiment. In one year of experience, roughly 40 percent of the cases fell into the "appeals from trial judgment" category. If CAMP settlement activities were to be concentrated only on these (arguably) more promising appeals, staff counsel's remaining time could be used effectively in other areas. This evidence also suggests that jurisdictions with substantially more appeals from trial judgments than the Second Circuit might find it useful to experiment with CAMP-type procedures on a full-time basis.

Conclusion

No one can deny that appellate procedural reforms should be carefully and critically examined. Generation of the best possible evidence to illuminate the critical issues may move even the most ardent supporters and the most vociferous detractors to recognize and accept the success (or, perhaps, the failure) of such reforms. This enlightened attitude will guarantee better decisions about how, when, and where to administer justice on appeal.

The Second Circuit's willingness to innovate with creative proposals for troublesome appellate problems must be commended and encouraged. That CAMP does not yet live up to its promise is valuable knowledge, for the problem CAMP addressed still remains, and can be approached anew with as much--if not more--enthusiasm and support as before.

This evaluation may suggest a replication of CAMP in a different setting, a fundamental modification of the plan, or, perhaps, an entirely new approach. Whatever steps might now be taken should be based on rigorously constructed evaluation. Without it, effective reform of the appellate process will remain an elusive goal.

APPENDIX I
CAMP RULES

UNITED STATES COURT OF APPEALS FOR THE SECOND CIRCUIT

Civil Appeals Management Plan

The United States Court of Appeals for the Second Circuit has adopted the following plan to expedite the processing of civil appeals, said plan to have the force and effect of a local rule adopted pursuant to Rule 47 of the Federal Rules of Appellate Procedure.

1. Notice of Appeal, Transmission of Copy and Entry by Court of Appeals.

Upon the filing of a notice of appeal in a civil case, the clerk of the district court shall forthwith transmit a copy of the notice of appeal to the Clerk of the Court of Appeals, who shall promptly enter the appeal upon the appropriate records of the Court of Appeals.

2. Appointment of Counsel for Indigent, Advice by District Court Judge.

If the appeal is in an action in which the appellant may be entitled to the discretionary appointment of counsel under 18 U.S.C. §3006(A)(g) but has not had such counsel in the district court and there has been any indication that he may be indigent, the judge who heard the case shall advise the Clerk of the Court of Appeals whether in his judgment such appointment would be in the interests of justice.

3. Docketing the Appeal; Filing Pre-argument Statement; Ordering Transcript.

Within ten days after filing the notice of appeal, the appellant shall cause the appeal to be docketed by taking the following actions:

a) filing with the Clerk of the Court of Appeals and serving on other parties a pre-argument statement (in the form

These Rules were amended on October 23, 1975 in order to place within the ambit of the plan review of administrative agency orders, applications for enforcement, and appeals from the Tax Court. These changes were effective as of January 1, 1976. They did not affect the plan or its administration during the evaluation.

attached hereto as Form C with such changes as the Chief Judge of this Court may from time to time direct) detailing information needed for the prompt disposition of an appeal;

b) ordering from the court reporter on a form to be provided by the Clerk of the Court of Appeals (Form D), a transcript of the proceedings pursuant to FRAP 10(b). If desirable the transcript production schedule and the portions of the proceedings to be transcribed shall be subject to determination at the pre-argument conference, if one should be held, unless the appellant directs the court reporter to begin transcribing the proceedings immediately;

c) certifying that satisfactory arrangements have been or will be made with the court reporter for payment of the cost of the transcript;

d) paying the docket fee fixed by the Judicial Conference of the United States pursuant to 28 U.S.C. 1913 (except when the appellant is authorized to prosecute the appeal without payment of fees).

4. Scheduling Order; Contents.

a) In all civil appeals the staff counsel of the Court of Appeals shall issue a scheduling order as soon as practicable after the pre-argument statement has been filed unless a pre-argument conference has been directed in which event the scheduling order may be deferred until the time of the conference in which case the scheduling order may be entered as part of the pre-argument conference order.

b) The scheduling order shall set forth the dates on or before which the record on appeal, the brief and appendix of the appellant, and the brief of the appellee shall be filed and also shall designate the week during which argument of the appeal shall be ready to be heard.

5. Pre-argument Conference; Pre-argument Conference Order.

a) In cases where he may deem this desirable, the staff counsel may direct the attorneys to attend a pre-argument *conference to be held as soon as practicable before him or a judge designated by the Chief Judge to consider the possibility of settlement, the simplification of the issues, and any other matters which the staff counsel determines may aid in the handling or the disposition of the proceeding.*

b) At the conclusion of the conference the staff counsel shall enter a pre-argument conference order which shall control the subsequent course of the proceeding.

6. District Court Extension of Time; Notification by Clerk.

In the event the district court grants an extension of time for transmitting the record pursuant to FRAP 11(d), the clerk of the district court shall promptly notify the Clerk of the Court of Appeals to that effect.

7. Non-Compliance Sanctions.

a) If the appellant has not taken each of the actions set forth in paragraph 3 of this Plan within the time therein specified, the appeal may be dismissed by the Clerk without further notice.

b) With respect to docketed appeals in which a scheduling order has been entered, the Clerk shall dismiss the appeal upon default of the appellant regarding any provision of the schedule calling for action on his part, unless extended by the Court. An appellee who fails to file his brief within the time limited by a scheduling order or, if the time has been extended as provided by paragraphs 6 or 8, within the time as so extended, will be subjected to such sanctions as the Court may deem appropriate, including those provided in FRAP 31(c) or FRAP 39(a) or Rule 38 of the Local Rules of this Court supplementing FRAP or the imposition of a fine.

c) In the event of default in any action required by a pre-argument conference order not the subject of the scheduling order, the Clerk shall issue a notice to the appellant that the appeal will be dismissed unless, within ten days thereafter, the appellant shall file an affidavit showing good cause for the default and indicating when the required action will *be taken. The staff counsel shall thereupon prepare a recommendation on the basis of which the Chief Judge or any other judge of this Court designated by him shall take appropriate action.*

8. Motions.

Motions for leave to file oversized briefs, to postpone the date on which briefs are required to be filed, or to alter the date on which argument is to be heard, shall be accompanied by an affidavit or other statement and shall be made not later than two weeks before the brief is due or the argument is scheduled unless exceptional circumstances exist. Motions not conforming to this requirement will be denied. Motions to alter the date of arguments placed on the calendar are not viewed with favor and will be granted only under extraordinary circumstances.

9. Submission on Briefs; Assignment to Panel.

When the parties agree to submit the appeal on briefs, they shall promptly notify the Clerk, who will cause the appeal to be assigned to the first panel available after the time fixed for the filing of all briefs.

10. Effective Date.

The foregoing Civil Appeals Management Program shall be applicable to all civil appeals to the Court of Appeals from the district courts in the Second Circuit, in which the Notice of Appeal is filed on or after April 15, 1974.

By Order of the Court:

/s/ IRVING R. KAUFMAN
Chief Judge

April 9, 1974

APPENDIX II
ATTORNEY QUESTIONNAIRES

CAMP QUESTIONNAIRE FORM A
(in experimental cases)

1. What proportion of your legal work is spent in federal appellate practice? ____%

2. How many years have you practiced law in the Second Circuit? ____years

PLEASE ANSWER THE REMAINING QUESTIONS IN THIS SURVEY BASED UPON YOUR EXPERIENCE IN:

______________________ v. ______________________

Docket Number: ______________

Please check

3. Were you counsel for the appellant? ☐

   appellee? ☐

   other? (Please specify:____________)

4. What was your participation in this appeal?

   -- preparation of briefs YES ☐ NO ☐

   -- oral argument YES ☐ NO ☐

   -- other participation (Please specify:________________)

5. Was a preargument conference scheduled in this appeal? (If you answered NO, skip to Q.8.) YES ☐ NO ☐

6. Was the conference held? YES ☐ NO ☐
   (If you answered NO, skip to Q.8)

7. Were you present at the preargument conference? YES ☐ NO ☐

8. Was the appeal:

☐ settled?
☐ withdrawn without settlement?
☐ decided by the court?
☐ terminated in some other way? (Please explain:

_______________________________________)

If a preargument conference was held and, subsequently, the appeal was settled (or withdrawn), did the conference cause the settlement (or withdrawal)? YES ☐ NO ☐

If you answered NO, to what do you attribute the settlement (or the withdrawal) of the appeal?

____

____

____

____

9. Was there a modification of the fundamental issues in this appeal from the time the Notice of Appeal was filed?
(If you answered NO, skip to Q.12.) YES ☐ NO ☐

10. If your answer to Q.9 was YES, then please specify whether there was:

Please check

a. Abandonment of one or more issues ☐

b. Addition of one or more issues ☐

c. Clarification of existing issues ☐

11. Did the modification result from the preargument conference? YES ☐ NO ☐

If you answered NO to Q.11, then to what do you attribute the modification?

____

12. Did you and your adversary agree to a stipulation of facts in this appeal? YES ☐ NO ☐

    a. If you answered YES, and a preargument conference was held, did the stipulation result from the preargument conference?

    YES ☐ NO ☐

13. If this appeal was the subject of a preargument conference, please identify any other results of the conference not already covered in this questionnaire.

    ____

    ____

14. If a preargument conference was held in this appeal, please specify in order of importance, the major results, if any, of this conference: [e.g., a) parties agreed to a stipulation of facts, b) reduction in joint appendix]

    a.

    b.

    c.

    d.

15. Aside from the preargument conference procedure, did you confer with your adversary?

    a. to explore settlement possibilities? YES ☐ NO ☐

    b. to limit or otherwise narrow issues? YES ☐ NO ☐

    c. for some other purpose? (Please explain below)

    ____

    ____

    ____

16. Would you have explored with your adversary the possibility of settlement or withdrawal of this appeal in the absence of the CAMP program? YES ☐ NO ☐

17. Would you have explored with your adversary the possibility of limiting issues in this appeal in the absence of the CAMP program? YES ☐ NO ☐

18. Would you have explored any other matters with your adversary in this appeal in the absence of the CAMP program? YES ☐ NO ☐

    If you answered YES, please explain:

    ____

    ____

19. Were there any drawbacks to the use of the preargument conference?

    ____

    ____

    ____

20. Was any cost incurred by your client after the Notice of Appeal was filed for the preparation of the district court transcript?

    YES ☐ NO ☐

    If you answered YES, what was the cost to your client? $_____

If you have any comments about the Civil Appeals Management Plan or about this questionnaire, please enter them below. Thank you very much for the time and effort you have given to this survey questionnaire.

CAMP QUESTIONNAIRE FORM C
(in control cases)

1. What proportion of your legal work is spent in federal appellate practice? ____%

2. How many years have you practiced law in the Second Circuit? ____years

PLEASE ANSWER THE REMAINING QUESTIONS IN
THIS SURVEY BASED UPON YOUR EXPERIENCE IN:

______________________ v. ______________________

Docket Number: ______________

Please check

3. Were you counsel for the appellant? ☐

appellee? ☐

other? (Please specify:______________)

4. What was your participation in this appeal?

-- preparation of briefs YES ☐ NO ☐

-- oral argument YES ☐ NO ☐

-- other participation (Please specify:____________________)

5. Did you confer with your adversary during the course of this appeal?

a. to explore settlement possibilities? YES ☐ NO ☐

b. to limit or otherwise narrow issues YES ☐ NO ☐

c. for some other purpose? YES ☐ NO ☐
(Please explain below)

____

____

6. Was the appeal:

   ☐ settled?
   ☐ withdrawn without settlement?
   ☐ decided by the court?
   ☐ terminated in some other way? (Please explain:

   ____

   ______________________________________)

7. Was there any modification of the fundamental issues in this appeal from the time the Notice of Appeal was filed? (If you answered NO, skip to Q.9.) YES ☐ NO ☐

8. If your answer to Q.7 was YES, then please specify whether there was:

   Please check

   a. Abandonment of one of more issues ☐

   b. Addition of one or more issues ☐

   c. Clarification of existing issues ☐

9. Did you and your adversary agree to a stipulation of facts in this appeal? YES ☐ NO ☐

10. Please identify any other matters bearing on this appeal resulting from an agreement with your adversary.

    ____

    ____

11. If you and your adversary agreed to any matters in this appeal (see Q.6-Q.10), then please specify in order of importance, the major results, if any, of your agreement: [e.g., a) parties agreed to a stipulation of facts, b) reduction in joint appendix]

    a.

    b.

    c.

    d.

12. Was any cost incurred by your client after the Notice of Appeal was filed for the preparation of the district court transcript? YES ☐ NO ☐

    If you answered YES, what was the cost to your client? $_____

If you have any comments about the Civil Appeals Management Plan or about this questionnaire, please enter them below. Thank you very much for the time and effort you have given to this survey questionnaire.

APPENDIX III
JUDGE QUESTIONNAIRE

CAMP QUESTIONNAIRE FORM B

PLEASE ANSWER THE FOLLOWING QUESTIONS BASED UPON YOUR EXPERIENCE IN:

________________________ v. ________________________

Docket Number: ____________

Date Argued: ____________

1. Was the preparation of appellant's counsel

   (CHECK ONE) better than average ☐
   average ☐
   worse than average ☐

   for cases of approximately the same degree of complexity?

2. Was the preparation of appellee's counsel

   (CHECK ONE) better than average ☐
   average ☐
   worse than average ☐

   for cases of approximately the same degree of complexity?

3. Were the issues raised in the appeal clearly brought out in the briefs? YES ☐ NO ☐

4. Were the issues raised in the appeal clearly brought out in the argument? YES ☐ NO ☐

5. Were undisputed or extraneous issues briefed? YES ☐ NO ☐

6. Were undisputed or extraneous issues argued? YES ☐ NO ☐

7. Were any briefed issues redundant? YES ☐ NO ☐

8. Were any argued issues redundant? YES ☐ NO ☐

9. Were any essential issues omitted from the briefs? YES ☐ NO ☐

10. Did the court have to direct counsel to critical issues during argument? YES ☐ NO ☐

SOME APPEALS HAVE RECEIVED CAMP PROCEDURES, OTHERS HAVE NOT. IN ANSWERING THE REMAINING QUESTIONS, PLEASE DO NOT CHECK THE RECORD TO DETERMINE WHETHER CAMP PROCEDURES HAVE BEEN APPLIED IN THIS APPEAL.

11. Do you know whether CAMP procedures (scheduling orders and/or preargument conferences) have or have not been applied in this appeal?

DO KNOW ☐ DON'T KNOW ☐

12. Overall, how would you rate the quality of this appeal with respect to the presentation of issues (both written and oral) to the court?

above average ☐

average ☐

below average ☐

13. Would you have expected a preargument conference to improve the quality of this appeal beyond that which was presented to you in briefs and oral argument?

YES ☐ NO ☐

If you answered YES, please indicate the way(s) the quality of this appeal could have been improved. [E.g., the number of issues presented for decision could have been reduced.]

____

____

14. Would you have expected a preargument conference before the filing of briefs to result in a settlement or a withdrawal of this appeal? YES ☐ NO ☐

If you have any comments about this appeal or about this questionnaire, please enter them below. Thank you for completing this questionnaire.

APPENDIX IV
NOTICE OF EXCLUSION

UNITED STATES COURT OF APPEALS

FOR THE

SECOND CIRCUIT

-------------------------------x

N O T I C E
Docket No.

-------------------------------x

The Court of Appeals has undertaken a study of the effectiveness of its experimental program of pre-argument conferences and scheduling orders. As part of the evaluation, this case, and a limited number of others, will not be subject to scheduling orders pursuant to the local rule of this Court entitled CIVIL APPEALS MANAGEMENT PLAN.

All the proceedings in this appeal must comply with the Federal Rules of Appellate Procedure and the rules of the United States Court of Appeals for the Second Circuit concerning the transmission of the record on appeal and the service and filing of briefs and appendix.

A. DANIEL FUSARO
Clerk

Dated:

CAMP 6

APPENDIX V
SELECTED COMMENTS OF SURVEYED ATTORNEYS

1. Since I was substituted as attorney for certain of the appellants, and there had been settlement negotiations prior to my entry into the picture and the parties were tremendously apart, and the issues relatively clear, I saw no point in any attempted settlement negotiations, discussions re narrowing the issues, etc. Indeed, I thought it was impossible that even the conference would produce a settlement. The conference, plus a later conference with the Staff Counsel did produce a settlement. I must admit I was amazed and surprised, and frankly, I credit to the tremendous impressiveness of Nathaniel Fensterstock, Esq., Staff Counsel, who tremendously impressed my clients, with the results that they (defendants) settled by payment of a figure much higher than had been originally contemplated, and indeed slightly higher than the one that I had recommended.

I think the program is highly useful, and should definitely be continued. The only drawback I see is from the attorney's point of view: Thus, I probably spent more hours in settlement of this matter (settlement papers were quite complex) than I would have spent to brief and argue the matter; but not unexpectedly, the clients felt that I should charge a smaller fee. However, this is the lawyer's problem, not the Court's.

2. While I think preargument conference might have significant value on appeals from lengthy and/or complex trials, I don't believe it has much use on appeals from motions or trials in which only a few narrow and clearcut issues are presented. Conceptually, I find difficulty in the approach to settlement of a matter in a preargument conference prior to an appeal. In those circumstances, lawyers usually believe that success on a legal issue raised on appeal is possible and this reduces the impulses to settle. Perhaps in cases where a money judgment was the result of the trial, economic circumstances might lead to serious settlment consideration at a preargument conference. Certainly in a case where the appellant thought a monetary judgment was excessive and thus appealed,

the appellee might also be influenced by the possibility of a reduction in the judgment or a reversal and thus give serious consideration to settling at a reduced amount.

3. While the preargument conferences scheduled pursuant to the Civil Appeals Management Plan are undoubtedly useful where money is the central issue involved in an appeal, I feel that such conferences are probably not of any particular value in the types of cases generally handled by legal services attorneys such as myself.

These cases by and large are against governmental bodies, but even where the opposition is a private party, the central issue of such cases for the most part pertain to points of law with money as a secondary issue if it is an issue at all. As such, it is doubtful that any type of a settlement could be effected especially since the government (federal, state or city) is also generally wont to settle....

4. The fine purposes underlying the Plan do not, in my opinion, undergo a "proper" test in an appeal from an SEC injunction action.

5. May I again compliment the Judges of the Second Circuit for appointing Mr. Fensterstock to the position as pre-argument staff attorney. Mr. Fensterstock has convinced me to withdraw a number of appeals, he has pinpointed the issues in several others, and has expedited the business of the Court to a remarkable degree.

6. ...To think CAMP will encourage many settlements is to assume (i) that counsel don't try to settle after trial in any event and (ii) that settlement "advice" by the type of individual running the CAMP conference will cause clients who have spent thousands of dollars to go through a trial to give up rather than risk the paltry dollars required to press the appeal. These assumptions are unrealistic. I cannot believe that the cost of CAMP, even if small, can be justified by any results. I have yet to meet a lawyer who has been forced to participate in a conference who thought it was anything but a waste of time designed to create jobs for administrators, secretaries and law clerks.

7. After several years' experience, I have concluded the pre-argument conference is a failure as presently operated. The laudable purpose is to reduce the appellate work load, but the present system is self-defeating.

In the above captioned case, the appeal was from a well-reasoned and carefully written opinion by a former Circuit Judge. There was no departure from well-established law. The administrator reviewing the appeal arbitrarily urged a settlement for $200,000. The recommendation was rejected and the decision was affirmed on the opinion of the court below.

By trying to settle cases and thereby improve his personal performance record, the administrator may gain a few compromises, but in the broader view he simply invites appeals in cases of questionable merit, because a prospective appellant is now lured on with the added possibility of a court recommended settlement.

In cases where no settlement is recommended, the administrator listens to a few minutes of oral argument* and then makes an immediate decision that the appeal does or does not have merit. An attorney who has already recommended an appeal after deep and careful consideration will not easily be dissuaded by this type of summary proceeding.

In final analysis, the administrator thrives on fear, rather than reason. Although he claims to make no recommendation to the court, we find this hard to believe. If not for the appellate judges, then for whose eyes is the report he makes? Thus, an appellant usually continues in the belief that he is being pressured, and that three careful judges will find merit which is overlooked in a brief administrative review.

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*The Court of Appeals grants more time than this, even after reviewing carefully written briefs.

8. I have on three occasions been before CAMP. Each appeal was in my major area of practice, admiralty. Each time counsel for the Second Circuit said "The judges do not like these kinds of cases." -- Is this proper? Is the purpose of CAMP the "ends of justice" or another concession to the tyranny of the computer. I believe that the Second Circuit is too "calendar minded." All litigants deserve equal hearing.

9. I have found the Plan helpful in terms of working out an appendix (particularly a deferred appendix) which minimizes the record problems, and in establishing an appropriate briefing schedule.

The practice of rehearsing the arguments on the merits is either a complete waste of time or, if reported to the Panel which is to hear the case, an inappropriate means of preparing for an argument.

The usual attempt to settle a case on appeal is also unfortunate. The parties have usually been subjected to settlement discussions in the District Court and, I believe, by the time a case reaches the Court of Appeals, there is no point in belaboring settlement.

10. The preargument conferences provided just another forum for settlement discussions. These discussions would have been held without the intervention of the Court of Appeals.

Our primary concern is twofold: one is that we do not believe that it expedites the appellate procedure and, secondly, it does increase the cost to our clients for the time spent in the preparation for and participation in the conferences.

11. There should be a more explicit description of the matters which are the subject of the preargument conference in the notice. Particularly, the need to be prepared to argue the merits should be pointed out. The failure to do so often results in lack of preparation by those attorneys who have not previously participated in a preargument conference.

12. I have had two experiences with 2d Cir preargument conferences. My overall impression is negative although, in theory, I view the idea favorably. In both cases, counsel for both sides had discussed the issues on many occasions and we knew exactly why we were at the Court of Appeals. Nonetheless, the Counsel for the Court assumed a judicial posture and made both sides literally argue the case. In discussion with opposing counsel afterwards, we agreed that we resented this time-wasting imposition, by someone without any power to affect the outcome, simply because it appeared that he wanted something substantive to deal with. In fact, I view the primary usefullness [sic] of the conference is to iron out technical housekeeping matters and can think of few cases at the appellate level where settlement can be effectuated.

13. In this particular situation, the magistrate wrongly predicted the result of the appeal in an attempt to disuade appellant from prosecuting the appeal. Such incorrect predictions may tend to weaken the magistrate's position (image?) as a spokesman for the Court itself, and thus his persuasiveness.

14. We were not able to effect a settlement until after briefs had been filed and the case ready for argument, notwithstanding the valiant efforts of special counsel who presided over the conference. Conferences can be more fruitful if the emphasis were based on a discussion of the merits of the appeal prior to the time that briefs are actually written. I would have preferred a greater opportunity to argue the merits with my adversary under the supervision of special counsel or a judge who would not be involved in the appeal but who could indicate his agreement or disagreement with the merits, that is an objective view.

15. ...I do feel that there are methods by which litigants could be offered inducements to abandon or settle appeals lacking in merit.

These inducements, in my mind, should follow the traditional philosophy of the English courts. The imposition of costs upon an unsuccessful appealing party is now in a relatively small and meaningless amount. One, even one with a legal position relatively lacking in merit, has painless access (once printing and record preparation costs are expended) to the ear of the appellate bench. If sufficiently determined, he may insist upon full access to that ear notwithstanding the operations of the CAMP Program. If, however, an appellant were required to secure his adversary's appellate costs--including counsel fees--a frivolous appellant would be given a much greater incentive to mull over whether or not to prosecute an appeal.

The Federal Rules of Civil Procedure contain devices, such as the Notice to Admit, which compel a litigant to abandon frivolous positions or run the risk of paying his adversary's cost of proving matters in contravention thereof. Why cannot the appellate bench impose costs realistic in amount and sufficient to deter unfounded claims, upon appellate litigation in the same fashion?

Further parallels with this concept may be found in the salutary practice of arbitration. There, recognizing that much litigation is essentially commercial in nature, the parties invoke commercial people and pay them for their services. It is recognized that the determination of disputed issues should be a cost borne by such party as may be ultimately found lacking in merit. In some cases, alternatively, the arbitrators will decided that the costs of the proceedings themselves should be borne equally by both parties--in recognition that there was a thorny matter to be determined and that neither party showed particular lack of good faith. It seems to me that an appellate bench which passes upon the merits of arguments made to it is in a position not only to determine the controversy but also to make an evaluation of the substantiality of the issues which it was forced to decide. Making an unsuccessful litigant bear the cost of the consideration of insubstantial arguments seems to accord not only with concepts of natural justice but also commercial reality as well....

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