

The Impact of Pretrial Conferences: An Interim Report on the Ontario Pretrial Conference Experiment

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THE IMPACT OF PRETRIAL CONFERENCES: AN INTERIM REPORT ON THE ONTARIO PRETRIAL CONFERENCE EXPERIMENT

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A. INTRODUCTION

It is generally accepted that the object of civil procedure is, or should be, to obtain not only a just determination of all disputes, but to do so speedily and at reasonable expense. However, today in Canada (and elsewhere) justice in the higher courts is expensive, and, if litigation proceeds all the way to trial, the process is far from speedy. The extent of delay varies from court to court, but in the large urban centres of Canada a timespan of three years from the commencement of proceedings to trial is not uncommon. While delay in litigation has many causes, once the case is placed on a trial list, ready for and awaiting trial, delay is largely a function of the court's ability (or inability) to reach the case for trial. In broad terms, this "court related" delay is a result of too many cases requiring trial in relation to the available judge time.

In an attempt to combat court congestion, delay, and the high cost of litigation, Canadian courts in the seventies have turned, as did their United States counterparts in the sixties and earlier, to the use of pretrial conferences.¹ In Ontario, the Supreme Court has employed pretrial conferences in civil jury cases in Toronto since 1975. The members of the Bar strongly supported the introduction of the procedure, and the court, attracted by the

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¹ Though not unknown, pretrial conferences were little used in Canada until the 1970's. Court rules in a number of Canadian jurisdictions now provide for pretrial conferences: see, Alberta Rules of Court, Rule 219; British Columbia Supreme Court Rules, Rule 35; Nova Scotia Procedure Rules 1971, Rule 26; Federal Court of Canada (General Orders and Rules), Rule 491. The Ontario Rules of Practice presently make no provision for the holding of pretrial conferences. For a general discussion of pretrial conferences in Canada and in the United States, see Ontario Law Reform Commission, *Report on the Administration of Ontario Courts*, (Toronto: Ministry of the Attorney General, 1973) Part 3 at 107 *et seq.*

potential of the procedure to increase the court's productivity, reduce the cost of litigation and speed up the resolution of disputes, contemplated its extension to the much larger Toronto non-jury list. At this point, however, the court responded to the suggestion that the proposed extension of the procedure should be done in such a way as to permit close monitoring of its effect, rather than simply extending the procedure and hoping for positive results. This decision on the part of the court was the genesis of the experiment reported here.² Essentially, it was agreed that the procedure would be introduced for only half of the cases on the non-jury list and that the court would co-operate fully with the authors in permitting the collection of the data necessary to evaluate the impact of pretrial conferences.

This article is a summary of the research design and the preliminary results of this experimental project.³ The objective of the project is to determine, through a strictly controlled experimental study, the effects of the introduction of pretrial conferences on the subsequent disposition of civil cases set down for non-jury trial in the Supreme Court of Ontario at Toronto. It has been generally assumed that the introduction of the pretrial conference procedure has the effect of increasing the rate and speed of settlement and of reducing the length of trial in those cases that go to trial. This project is addressing itself, *inter alia*, to the following questions: whether or not these effects *do result* from the holding of pretrial conferences; whether such effects are common to all or only some types of litigation; whether such benefits are of a magnitude sufficient to produce a *net* benefit to the court, taking into account the judicial manpower required to conduct the pretrial conferences.

In the United States the use of pretrial conferences became prevalent with the introduction of the procedure into the Federal Rules of Civil Procedure in 1938. It has since been adopted in most state jurisdictions and it is now widely used throughout the country, although the manner and intensity of its use varies from court to court. The utility and effectiveness of pretrial conferences is a matter on which there is considerable disagreement. Based upon personal experience many (perhaps the majority) of judges and lawyers in the United States feel that trials are shortened and settlement rates increased by the use of pretrial conferences. However, the only major empirical study of the operation of pretrial conferences in the United States, conducted in the early sixties in the New Jersey courts by Professor Maurice Rosenberg,

² Earlier the Ontario Law Reform Commission, *id.* at 121, had suggested that consideration be given to conducting a controlled experiment in Ontario in the use of pretrial conferences.

³ This Pretrial Conference Experiment Project is being directed by the authors, for the Supreme Court of Ontario, under the auspices of the Canadian Institute for the Administration of Justice and with the co-operation of the Institute for Behavioural Research of York University. Financial support for the project is being received from the Law Foundation of Ontario and the Ministry of the Attorney General of Ontario through the budget of the Civil Procedure Revision Committee. This co-operation and assistance is gratefully acknowledged.

cast severe doubt upon these conclusions.⁴ Using a "partially"⁵ controlled experiment involving only personal injury cases, Professor Rosenberg concluded that a pretrial conference procedure did not enhance the efficiency of the judicial process. More specifically, he found that pretrial conferences did not reduce the average length of trial, nor did they increase the rate at which cases settled prior to trial. Indeed, Rosenberg concluded that the use of pretrial conferences actually *reduced* the court's efficiency, since additional judge time was expended in conducting pretrial conferences without any improvement in the disposition rate.⁶

The general applicability of Professor Rosenberg's conclusions have been challenged in the United States⁷ and pretrial conferences continue to be widely employed in that country. Many lawyers and judges still believe that pretrial conferences eliminate the need for, or shorten, trials. The result is that considerable confusion continues to exist as to the efficacy of pretrial in the United States.⁸ Little is really known of the actual impact of pretrial conferences in the Canadian context. In Ontario, members of the judiciary and the Bar generally, strongly support the procedure but this project is the first opportunity to systematically test the efficacy of pretrial conferences in any jurisdiction in Canada. Furthermore, inasmuch as this project does not suffer from the problems (faced by Rosenberg) of non-random selection to the experimental procedures⁹ and inasmuch as this project is not exclusively confined to the analysis of personal injury litigation, this study may be seen as a significant advance on the pioneering work in New Jersey.

The pretrial conference is a conference between the counsel in a case and a judge, typically held several weeks before the trial date and after the other pretrial proceedings are completed.¹⁰ In Canada to date two basic

⁴ Rosenberg, *The Pretrial Conference and Effective Justice* (New York: Columbia University Press, 1964).

⁵ Rosenberg was unable to structure his experiment so as to stream all the control cases to trial without a pretrial conference. Lawyers in the control group were allowed to request a pretrial conference if they wanted one. Hence, instead of having two groups of cases (test and control) three groups resulted (mandatory pretried, optional pretried and not pretried).

⁶ However, Rosenberg's study did conclude (*supra*, note 4 at 28 *et seq*) pretrial conferences led to an improvement in the quality of trials in that, in pretried cases, counsel were found to be better prepared, a clearer presentation of opposing theories of counsel was more common, gaps and repetition in the evidence was reduced and tactical surprise was curbed. We have not attempted to replicate this aspect of Rosenberg's study; see *infra*, note 45.

⁷ See, e.g., Becker, "Efficient Use of Judicial Resources," 43 *Federal Rules Decisions* 421 (1967); Chantry, *et al*, "Pretrial Utility or Futility," 32 *Ins. Counsel J.* 602 (1965); Todd, "Pretrial Revisited," 50 *Judicature* 153 (1967).

⁸ See, for example, the recent Federal Judicial Center's *District Court Studies Project Interim Report* (Washington: The Center, June 1976) 18-21, reporting data indicating, for the majority of courts observed, an *inverse* relationship between judicial involvement in settlement (through pretrial conferences) and terminations per judge.

⁹ See *supra*, note 5.

¹⁰ The timing of the conference may vary from court to court and from case to case. Also in some courts, or cases, the clients may be expected to attend or to be available for consultation.

forms of pretrial conference have emerged. The first has as its principal goal the readying of the case for an orderly trial: the *trial oriented conference*. The second has as its major goal pretrial settlement of the case: the *settlement oriented conference*.

In either form, the conference consists of a discussion between the judge and counsel concerning the case. In the *trial oriented conferences* the major emphasis is placed upon clarification and reduction of the issues in the case, the limitation of the number of expert witnesses, the obtaining of admissions of fact, and agreements to dispense with formal proof of documents. The aim of such conferences is to reduce trial time, and to improve the overall quality of the trial by increasing the preparedness of counsel, by facilitating the avoidance of surprise, and by generally aiding the clear presentation of the case. Typically, these *trial oriented conferences* will be conducted by the judge assigned to try the case. The possibility of settlement may or may not be discussed, but it is not the focus of the conference though it may be a by-product thereof.

At the *settlement oriented conferences*, the presiding judge seeks, through discussion with counsel, to assist them at arriving at an out-of-court settlement. The role of the judge here is essentially that of conciliator or third party mediator, who points out the strengths and weaknesses of each side's case and who gives his opinion as to the likely outcome of the trial, in terms of liability and damages. If it becomes clear that settlement is not possible, some time may be spent on limiting and clarifying the issues to be tried.

The pretrial conferences conducted in this experiment were intended to be primarily *settlement-oriented* rather than *trial-oriented*. They were not to be "head-knocking" sessions at which settlement was forced upon the counsel by the presiding judge, but rather exercises in third party mediation by the judge. An important ground rule of the conferences, made clear to counsel at the outset, was that under no circumstances would the judge presiding at the pretrial be the trial judge in the action. All discussions would be treated as confidential and privileged and would not be disclosed to any fellow judge. It was felt, by the Court, that this ground rule would discourage "head-knocking" and any likelihood or appearance of unfairness, and at the same time encourage full disclosure and frankness on the part of participating counsel.

The Rules of Practice governing proceedings in the Supreme Court of Ontario (of which the High Court is simply the trial division) presently make no provision for the holding of pretrial conferences. Hence, the conferences conducted as part of this experiment were neither required nor specifically sanctioned by the Rules. As explained below, the attendance of counsel was secured by a letter of invitation from the judge who was to preside at the conference. Although attendance was not mandatory in any legal sense, in only a very few cases did counsel fail to attend a scheduled conference.

B. RESEARCH DESIGN

It is worth indicating why controlled experimental evaluation is particularly necessary in determining the impact of pretrial conferences. Today, in almost any jurisdiction the settlement rate of cases is extremely high, vary-

ing, depending on the jurisdiction, between 75-85% of all actions commenced. When a pretrial conference procedure is introduced, many cases that are pretried will subsequently settle. But whether these settlements are a result of the pretrial conference or represent settlements which would have occurred in any event, cannot be determined in the absence of a control group,¹¹ i.e., an identical group of contemporaneous cases which have not been subjected to the pretrial conference experience. Moreover, since settlement rates are very high, with or without pretrial conferences, very careful precise measurement is necessary in order to isolate the impact of pretrial conferences.¹²

This project necessitated a number of novel approaches to the construction of a research design because it was a controlled experiment carried out "in the field." That is, while the research can be considered a true experimental design, it is not being conducted in the artificially controlled environment of a laboratory, but in the real world. While laboratory experiments have the rigour of complete (or almost complete) control, this strength is the basis of the major weakness of laboratory experiments, for the very artificiality of the setting makes it difficult to generalize findings to the "real world." There are a variety of problems associated with achieving a controlled experiment in a real world environment. First, research design is difficult because of the need to monitor the complex on-going process being studied, without affecting it in the course of the study, i.e., without turning the "real" world into an "artificial" world. A second problem is establishing the requisite control while at the same time obtaining and retaining the full co-operation of all participants in the milieu. In the context of this experiment obtaining the co-operation of the court was essential. This itself involved a number of aspects: the willingness of the court to initially acknowledge the need for experimentation; the need to refrain, during the course of the experiment, from carrying out other procedural or administrative reforms that seem plausible short-term solutions to institutional problems; and the willingness of the court to co-operate in the daily gathering of information. That this experiment is being conducted, and that the degree of control obtained in the study has

¹¹ Another (but inferior) way to attempt to measure the impact of pretrial conferences is to compare settlement rates, etc., before and after the introduction of the procedure. One of the co-authors, Garry Watson, in an earlier project attempted to use this technique to measure the impact of the introduction of pretrial conferences on the Toronto jury list. The attempt was unsuccessful and the results inconclusive for two reasons. First, since the technique involved the comparison of statistics from different time periods there was no control of extraneous factors or variables, e.g., procedural, administrative or behavioural changes other than the pretrial conference. Secondly, the data regularly collected by the court was insufficiently detailed to allow any form of precise measurement. For a brief but useful discussion (citing examples) of research techniques for evaluating court procedures and changes in the administration of justice, see Rosenberg, *supra*, note 4 at 16 *et seq.*

¹² As the recent Federal Judicial Centre Study, *supra*, note 8 at 20, states, "evaluating settlement procedures presents the same difficulty as evaluating remedies for the common cold. All cold remedies appear to work as indicated by the fact that colds always go away. Similarly, all settlement procedures succeed as indicated by the fact that most cases settle no matter what procedures are used."

been possible, is a direct result of the co-operation afforded by the Supreme Court of Ontario¹³ and the practising Bar in Toronto.

While the research design of this experiment uses complex statistical techniques and methods, the overall approach is nevertheless straightforward: to examine the differences in the manner and timing of disposition for cases on the civil non-jury list in the Supreme Court of Ontario as a function of whether or not they are assigned to pretrial conferences, and to indicate the probability that such differences are due to pretrial intervention rather than to chance or other factors. The ability to isolate this latter source of variation is based upon the controlled assignment of any case to either the test condition (pretrial conference) or a control condition (no pretrial conference) by a strictly random selection.

Every case on the civil non-jury list (this list does not include divorce cases)¹⁴ was classified as either personal injury or other.¹⁵ This stratification of the sample was done to ensure sufficient representation of personal injury cases in the study, since it was hypothesized that this type of case was particularly amenable to settlement through pretrial conference.¹⁶ Randomly, each case in the list of personal injury and other cases was assigned to either the test (pretried) or control (not pretried) group. Each test case was assigned, on a random basis, a paired control case. Thus, there were four groups of

¹³ Particular mention should be made of the contribution and support of Mr. Justice Richard Holland, Mr. Justice Peter Cory, and Mr. Justice Willard Estey, Chief Justice of the High Court (now a member of the Supreme Court of Canada).

¹⁴ A separate list is operated for divorce cases. Contemporaneously with this project, pretrial conferences were introduced in the Supreme Court of Ontario in Toronto in divorce cases, but the impact of pretrial conferences in these cases is outside the ambit of this study. For an account of the operation of the procedure in such cases, see Lief, *Pre-Trial of Family Law in the Supreme Court of Ontario — Simplify and Expedite*, 10 Law Society of Upper Canada Gazette 300 (1976).

¹⁵ Inter-coder reliability (i.e., the agreement between three independent coders) for this classification was perfect, $r = +1.00$. These two basic subject matter classifications have been further broken down as follows: personal injury (motor vehicle and other); non personal injury (real estate, contract, and other). Analysis of any difference amongst these further category breakdowns must await the completion of the experiment when larger sample sizes in each category are available.

¹⁶ This hypothesis was generated in discussions with members of the Court. The reasoning was that a major issue in personal injury cases is almost invariably the amount at which damages will be assessed. Since all of the cases are, *ex hypothesi*, on the non-jury list, that assessment (if the case goes to trial) would be made by a High Court judge, and the pretrial conference provides a forum in which a High Court judge can give his tentative opinion as to the likely assessment of damages. Given these factors it was felt that the pretrial conference might be particularly effective in achieving pretrial settlements in personal injury cases. (This hypothesis is not in fact borne out by these preliminary results, see *infra* at note 40.) In view of the differences between our preliminary results and those obtained by Rosenberg as to the impact of pretrial conferences, it is worth noting that our study dealt exclusively with non-jury cases, whereas his dealt principally with cases bound for trial by a jury. See Rosenberg, *supra*, note 4 at 21.

cases: test - personal injury; test - other; control - personal injury; control - other. Personal injury cases were only paired with personal injury cases, etc. Sampling was conducted in this manner in roughly two week intervals throughout the year, taking the set of cases at the head of the list in each time period as a separate cohort. That is, each block of cases sampled and assigned at the same time was called a cohort. This temporal separation of the sample will allow us to isolate the effects of changes over time, which include certain things as "learning" by counsel about the operation of the conferences, or changes in other court procedures introduced during the course of the experiment.¹⁷

The pairing between cases in test and control groups is a special feature in the research design of this study. The effect of pairing is to create a nest of two case mini-experiments. Usually attrition¹⁸ is a problem in field experiments because test and control group cases may suffer from differential attrition rates. In our experiment the technique of pairing allows us to attrite both of these paired cases if one case attrites, making the remainder of the cases less likely to be biased. The best way to understand pairing is to see it as a randomly selected and assigned two case ($N = 2$) self-contained experiment. (In fact, the experiment can be seen as a nest of Chinese boxes, pairs within cohorts, and temporally distinct cohorts within the total sample).

The administration of the experiment, and the monitoring of the cases and their progress through the list can now be summarized briefly. At the commencement of the experiment in April 1976 all the cases then on the Toronto non-jury list (excluding approximately 130 cases at the top of the list)¹⁹ were identified and coded with the date upon which they were set down for trial (i.e., added to the bottom of the trial list) and classified according

¹⁷ One such change introduced during the course of the experiment was a "trial blitz," involving a concentration of judicial manpower in Toronto to try non-jury cases during the fall of 1976 (September - December, 1976). As opposed to the normal situation, in which the number of judges hearing cases in Toronto non-jury range between one and five, during the blitz a minimum of five judges sat continuously to hear cases on the Toronto non-jury list. In addition, during the blitz period different case scheduling techniques were in effect to expedite the flow of cases to the increased panel of judges. This preliminary report does not, in fact, include more than a handful of cases which were subject to the blitz experience. Our final report will, however, deal with a large number of such cases. However, the sampling techniques referred to above, i.e., temporal separation and pairing, will enable us to provide independent estimates of the effect of the blitz and the pretrial conference on the disposition of cases.

¹⁸ By attrition we mean the slippage of cases from the experiment before the relevant measurements can be made, e.g., test cases may attrite through settlement prior to being called to a pretrial conference, or may fail to attend at the pretrial conference. (The actual reasons for attrition in this experiment are discussed in more detail, *infra*, note 26.) Cases also disappeared from the non-jury list (e.g., through settlement) prior to being sampled as part of the experiment. These cases never became part of the experiment.

¹⁹ These cases were excluded simply because we wanted to avoid taking into the experiment cases which might be called for trial before they could be pretried.

to subject matter.²⁰ From then on, a block of cases (a cohort) was taken from the top of the list every few weeks.²¹

Ideally the time at which any cohort was drawn (and thus the ensuing pretrial conferences held) in relation to the likely trial date of the cases in the cohort, should have been constant over the course of the experiment in order to hold constant the time before trial at which pretrial conferences were held. Initially we attempted to do this. However, the rate at which cases are reached for trial on the Toronto non-jury list is inevitably a function of judge availability (which varies from week to week and month to month) and of the length of trials in those cases heard by the court. The resulting variability in the rate at which cases may be called for trial made it impossible to hold constant the period between pretrial conference and trial dates. All that we can say is that within cohorts the period between pretrial conference and likely trial date was more or less constant: across cohorts it varied considerably.²²

Within each cohort the personal injury cases were randomly sampled for test and control group assignment as were the non personal injury cases. The resulting sampling status for each case was then coded. Counsel in those cases assigned to pretrial conferences were notified of dates and times of the conference in letters sent out over the signature of the judge who would preside at the pretrial conference.²³ These letters were sent out about two weeks before the pretrial conferences. The date of the conference for the test case was coded for both that particular test case and for its control pair.²⁴ For both groups of cases, information as to the timing of settlement, trial, adjournments, and appeal were subsequently coded, as was information as to the terms of settlement or judgment. In addition, information was collected

²⁰ A similar procedure was carried out for cases that were *subsequently* added to the Toronto non-jury list. Initially we had anticipated that the number of cases on the list as of April 1976 would provide us with the desired sample size. This turned out not to be the case, and an additional (approximately) 200 cases, added to the list after April 1976 but appearing at the head of the list before May 1977, were taken into the experiment. Hence, our final analysis will be based not upon (a portion of) the cases on the list as of April 1976, but on the universe of cases surviving to the top of the list during the period April 1976 – May 1977.

²¹ The number of cases in each cohort was basically twice the number of available pretrial conference slots, so as to provide both test and control cases. In fact a slightly greater number of cases were included in each cohort so as to ensure replacements of pairs which attrited because the selected test cases could not, for one reason or another, attend a scheduled pretrial conference.

²² In the analysis of the final data we will attempt to statistically estimate the effects of variation in the timing of the pretrial conferences. However, this will present difficulties, because it is impossible to determine what the trial dates would have been for cases which settled after the pretrial conference.

²³ This method of notification was decided upon by the Court. Ontario presently has no rule authorizing the holding of pretrial conferences or requiring the attendance of counsel at a pretrial conference. The Court felt that a personal request from the presiding judge to counsel to attend the pretrial conference would maximize the likelihood that counsel would attend the conference.

²⁴ This information allows for testing of the hypothesis that pretrial conferences produce *more speedy* settlements even if they do not affect the probability of settlement.

from the pretrial conference judge about the nature of the conference in each case.²⁵ Further information was collected and coded relating to the damages in dispute or other relief claimed, for payments into court, and for the identity of counsel involved in each case.

The analysis presented here is based on the first 307 cases sampled as part of the experiment between April and August 1976. Not all of these cases were used in the analysis, however, for the following reasons. First, some cases were struck from the list or adjourned *sine die* prior to or at trial and hence had not reached final disposition at the time of this analysis. Second, in some cases trials had been concluded, but reserve judgments had not been handed down at the time of the analysis. Third, a large number of cases were attrited because of problems of scheduling and holding pretrial conferences, particularly in the initial weeks of the experiment.²⁶ Cases attrited for any of these reasons were removed from the analysis with their pairs. This procedure maintains the sampling integrity of the analysis of the 161 cases discussed below, and we have checked and ascertained that the results of this preliminary report are not biased by the exclusion of the eliminated cases.²⁷

²⁵ The judges gave information as to counsel present, quality of preparation, matters stressed (i.e., settlement or preparation of the trial through clarification of issues, etc.), judge time spent in preparing for and holding the conference and an estimate of the likelihood of out-of-court settlement or of trial time, as well as a summary evaluation of the usefulness of that particular pretrial conference.

The collection of the diverse sets of information, and the supervision of the pretrial conference scheduling, was carried out from a project office in the court house by Ms. Anne Burke and her associate Ms. Sue Carson. This information was subsequently converted to machine readable files, and computer-analysed, at the Institute for Behavioural Research, York University, with the assistance of Ms. Mirka Ondracek.

²⁶ The major reason for attrition was that the early cohorts contained many more (test) cases than there were available pretrial conference slots. Hence, some test cases were never summoned to a pretrial conference and were attrited (with their control pair). A second reason for attrition was that one or both of the paired cases were called to trial before the holding of the pretrial conference. A third reason was that a number of test cases, when summoned to the pretrial conference, indicated they had already settled and were consequently attrited along with their pairs. A very small number of cases were attrited due to the failure or refusal of counsel to appear at the pretrial conference.

²⁷ The checks conducted indicate: (a) amongst the cases not analysed because of reserved judgments, there were a greater number of control as opposed to test cases that had gone to trial rather than settled; (b) amongst the cases struck or adjourned *sine die*, more of these were test than control cases, but the settlement rate for the control cases paired with struck or adjourned test cases was not greater than that reported for the control group in our analysis, and the exclusion of these cases does not bias the conclusions from our analysis; (c) an examination of the 100 cases attrited revealed no distributional differences in amounts in dispute or manner of disposition inconsistent with the control cases analysed. In short, except for attrition, these cases were not atypical.

Further, it should be noted that in this preliminary analysis the cut off point of 307 cases represented the set of cohorts for which disposition was recorded in all cases in

In summary, the composition of the sample used for analysis and the distribution of the attrition is as follows:

161 cases analysed (81 test; 80 control) ²⁸
100 cases attrited (50 pairs)
30 cases struck off the list or adjourned <i>sine die</i> (15 pairs)
16 cases under reserved judgment (8 pairs)
<u>307</u> TOTAL

The present status of the project is that the sampling of cases and the conduct of pretrials is now complete; the last conference having been held in May 1977. In total, some 940 cases have been taken into the experiment. As already indicated, the analysis presented here is based upon 161 usable cases from the first 307 cases sampled. The balance of the cases (approximately 630) remain to be disposed of by settlement or trial and to be fully coded.²⁹ It is hoped that the final report from the project will be available in the spring of 1978.

C. ANALYSIS

The analysis that follows examines the extent to which pretrial conferences affect the settlement rate of cases,³⁰ the time spent in trial, and the speed, as opposed to the rate, of settlement. Further, in light of these effects, we assess the impact of the use of pretrial conferences on the court's efficiency. Finally, we explore whether there are differences in the substantive outcome of cases resulting from the use of pretrial conferences. Also, assessed, wherever possible, is the extent to which pretrial conferences produce differential effects as a result of differences in the characteristics of cases, i.e., the type of litigation and the amount in dispute or because of differences in the characteristics of the pretrial conference procedures employed, i.e., the identity of the presiding judge and the level of preparation of participating counsel.

1. *Impact of Pretrial Conferences on Settlement Rates*

a. For All Cases

The effectiveness of pretrial conferences as a means of increasing the rate of settlement can be estimated initially by an examination of the data in Table 1. The Table shows that 86.4% of the cases pretried were disposed

²⁸ It so happened that one cohort (sampling block) contained an uneven number of cases. Through random assignment, this case ended up in the test group.

²⁹ As of November 1, 1977 approximately 160 cases still remain to be disposed of by trial or settlement. We anticipate that the bulk of these cases will be disposed of by the end of December 1977.

³⁰ It is important to note with regard to the analysis presented below, that when we speak of settlement rates we are talking about the settlement rate of those cases taken into the experiment and used in the analysis (i.e., not attrited). These settlement rates do not represent the *overall settlement rates* of cases on the Toronto non-jury list. This latter figure will be higher because if a case was settled prior to the sampling point, it never became part of the experiment.

of by settlement, in contrast to a settlement rate of 68.8% for those cases not pretried. Expressed differently, the evidence indicates that pretrial conferences increased the rate of disposition by settlement by slightly more than 25%. This difference in settlement rates can be inferred to be the result of pretrial conferences because, given experimental control, the only alternative explanation would be chance. The statistical significance of the differences observed (i.e., $p. \leq .005$) is such that these results could be obtained by chance in only five out of 1,000 random assignments of this number of cases to experimental or control groups.³¹

TABLE 1

Disposition by Settlement or Judgment for Cases With and Without Pretrial Conference Experience

		DISPOSITION:		Total:
		Judgment	Settlement	
EXPERIMENTAL SELECTION:	CONTROL (No Pretrial)	25 cases (31.2%)	55 cases (68.8%)	80 cases (100%)
	TEST (Pretrial Conference)	11 cases (13.6%)	70 cases (86.4%)	81 cases (100%)
Total:		36 cases	125 cases	161 cases
$X^2 = 7.19$				
$p. \leq .005$				

At the 95% confidence level, a confidence interval of $\pm 10.2\%$ can be placed around the 68.8% settlement rate for the control group. At the same confidence level the interval is $\pm 7.51\%$ around the 86.6% settlement rate in the test group.

While the data in Table 1 indicate a marked difference in the proportion of cases disposed of by settlement rather than by judgment, this Table does not distinguish between cases settling prior to trial and cases settling in trial. Some cases settle only after the trial has commenced. Table 1 groups as "settled," cases settled before trial and cases settled during trial. Because this

³¹ We refer to statistical significance as a means of indicating the confidence with which we can decide that differences between pretried and non-pretried cases are due to exposure to pretrial conferences, rather than to chance. (Because of the controlled nature of the experiment we can reject, *a priori*, alternative substantive hypotheses as an explanation of differences). "Statistical significance" is simply the phrase used to describe the probability that differences observed between (or within) random samples are a result of chance. By social science convention, when the probability that the observed difference could have resulted from chance is fewer than 5 times in 100 ($p \leq .05$), the hypothesis that the differences *do* result from chance is rejected in favour of substantive explanations for the differences. When this probability is greater than 5 times in 100, the convention is to retain chance as a reasonable competing explanation for the observed differences.

latter group clearly involves the use of judge time in court, the impact of pretrial conferences on the productivity of the court as suggested by Table 1 will vary depending upon the balance between in trial as opposed to pretrial settlements. Table 2, therefore, specifies the effect of pretrial conferences on settlement before trial, during trial, or disposition by judgment.

TABLE 2

Disposition of Cases by Settlement Without Trial, Settlement During Trial, and Judgment After Trial

DISPOSITION:

		Judgment after trial	Settlement during trial	Settlement without trial	
EXPERIMENTAL SELECTION:	CONTROL (No Pretrial)	25 cases (31.2%)	15 cases (18.8%)	40 cases (50%)	80 (100%)
	TEST (Pretrial Conference)	11 cases (13.6%)	11 cases (13.6%)	59 cases (72.8%)	81 (100%)
Total:		36 cases	26 cases	99 cases	161

$X^2 = 9.7$
 $p. \leq .007$

The more detailed information in Table 2 confirms the suggestion in Table 1 that greater efficiency in the disposition of cases results from the use of pretrial conferences. It is apparent from the data in Table 2 that the rate of settlement *without trial* is substantially greater in the test group (72.8%) than in the control group (50%): the rate of disposition only after full trial proceedings and judgment is substantially lower in the test group (13.6%) than in the control group (18.8%). The statistical significance of the differences in these rates of disposition is marked ($p. \leq .007$), indicating that such differences could be expected by chance, only 7 times in 1,000.

Therefore, the general conclusions indicated, are that pretrial conferences increase the number of settlements prior to trial by 48% and reduce the number of trials required to obtain a disposition (either by settlement or judgment) by 45%.

On the basis of these preliminary results, the impact of pretrial conferences is substantial. As well, this is surprising in light of the results from Rosenberg's study.

One important difference between the situation studied by Rosenberg and the one reported on here is that the New Jersey court had for some time previous to the Rosenberg experiment employed pretrial conferences, whereas in the Toronto non-jury list no such procedure had been used prior to the

start of our experiment.³² That is, Rosenberg studied an established procedure, whereas we are studying a new procedure. Thus it is possible that the novelty of the pretrial conference, rather than its substance, played a role in producing the difference between test and control cases in our experiment and the extent of these differences may diminish over time as the Bar becomes more familiar with the procedure.³³ Further, the differences between test and control groups may be due simply to the effect of increased court interest in a case as perceived by counsel rather than the particular vehicle through which this interest was expressed (i.e., the pretrial conference).

It should be noted that the findings here may be influenced by a "Hawthorne Effect."³⁴ That is, the difference between test and control cases evident in Tables 1 and 2 may be due to attempts by the participants to modify their behaviour so as to fit in with their perception of the kinds of behaviour expected to result from the experiment. In other words, it is possible the participants were particularly interested in achieving the settlement of cases through pretrial because they knew an experiment was being conducted. It

³² Other presently apparent differences between the two studies include the following: (2) nearly all of Rosenberg's cases were headed for jury trial, whereas ours are all non-jury cases; (3) Rosenberg's cases were all personal injury cases, while ours are mixed (but with an identified sample of personal injury cases); (4) the vast majority of Rosenberg's cases involved small claims, by comparison our cases involve relatively large claims (see note 42, *infra*); (5) whereas in Rosenberg's study a very large number of judges (some 49 different judges) conducted pretrials, for the cases reported on here in our study only 4 judges conducted the pretrials; Rosenberg's judges were (presumably) a cross-section of the bench, whereas ours were highly motivated, favoured pretrial and were selected (by the court) for this reason (see *infra*, note 35); (6) in our study a ground rule was that the pretrial judge was disqualified from trying the case, whereas this was not so in New Jersey; (7) in our study it was clearly understood by the pretrial judges (and probably by counsel) that the purpose of the conference was to try and achieve settlement; in New Jersey it appears that the pretrial judges may have pursued more diverse goals. In addition, there are significant methodological differences between the two studies; in New Jersey the "control" cases were allowed to opt for a pretrial conference (see note 5, *supra*), thereby reducing the degree of experimental control, whereas this was not permitted in our study; we have employed the devices of "pairing" and "attrition" which Rosenberg did not.

³³ We may be able to explore the plausibility of this "newness" effect as influencing the difference in settlement rates between test and control groups, by making use of the multiple or replicate sampling procedure employed in this study, whereby samples were drawn from segments of the list at different times during the progress of the study. We have examined the breakdown for Table 1 in the four independent samples drawn between April and August 1976. To date, there is no alternation of the difference in settlement rates as we move from the early to the later sample "cohorts" (i.e., the difference in settlement rates between test and control groups was consistent throughout the five month period). However, even if differences due to the novelty of the pretrial conference procedure were in effect they would be unlikely to wear off so early. Such a decrease in the differences between test and control groups may yet become apparent in the data for cases entering the experiment after the period he reported. However, of course, this effect may be present but not demonstrable at all during the course of the experiment.

³⁴ So called after a famous experiment at the Hawthorne plant of the Western Electric Company in the 1920's, in which it was found that the observed effects were caused by the fact of participation in an experiment and not by the variables being studied experimentally. See F. J. Roethlisberger, and W. J. Dickson, *Management and the Worker* (Cambridge: Harvard Univ. Press, 1939).

may be that this interest produced the substantial positive results we have reported, which might not have resulted from pretrial conferences in a non-experimental setting.³⁵

There are two additional factors which may also effect settlement rates and which by chance may not be randomly distributed between the test and control groups. These are "payments into court"³⁶ and informal conferences conducted between the trial judge and counsel on the day of trial.³⁷ For this report, we have been unable to code and analyse information on these two variables and, therefore, we need to caution that when analysed, these factors may modify the preliminary conclusions drawn here on the impact of pretrial conferences.³⁸

Some of the cases reported on here were subject, in addition to their differential exposure to pretrial, to another judicial intervention, a "trial blitz" procedure used by the court to increase judicial manpower and control

³⁵ In our view, the likelihood of their being a Hawthorne effect of the kind described in the text as a result of the behaviour of participating *counsel* is quite low. However, it seems to us that such an effect might well have resulted from the behaviour of the four participating judges, since they were all strongly committed to making the pretrial conference program work effectively, and they were chosen, in part, to conduct the pretrials for just such reasons.

Alternatively, it may be the case that these particular judges behaved in the experiment as they would in a non-experimental situation and that the relevant *caveat* is whether their commitment to the desirability of achieving settlement through pretrial conferences is representative of the commitment of the Court as a whole.

³⁶ Payment into court is a procedure by which a defendant may deposit monies with the court by way of an offer to settle the plaintiff's claim. This payment is not revealed to the trial judge until after he has given his decision on the merits of the case. (See the Ontario Rules of Practice, Rules 306 *et seq.*) The important impact of this rule is that if a plaintiff refuses the monies paid into court and subsequently at trial he fails to recover more than the amount paid into court, he will normally be ordered to pay the defendant's costs (including lawyers' fees) from the date of the payment into court. This will, of course, include the very significant trial costs. When a realistic payment into court is made it obviously (because of these cost consequences) puts a great pressure on the plaintiff to settle. However, it should be noted that among the cases here analysed the proportion of cases in which a payment into court was made was not high.

³⁷ In the High Court of Ontario it is not uncommon for the trial judge to have conferences with counsel involved in the case either immediately prior to the commencement of the trial and/or during the course of the trial. At such conferences the possibility or desirability of settling the case may be discussed, in a variety of ways. Some judges may vigorously pursue settlement at such conferences, particularly where they take place prior to the commencement of the trial. Even judges who do not feel this is an appropriate role for the trial judge may, during the course of the trial, call counsel into their chambers and indicate to them their tentative assessment of the evidence to date, e.g., that to date the plaintiff's case is going very badly because his witnesses have not been believable.

³⁸ We anticipate no difficulty in eventually analysing the data with regard to payments into court. However, with regard to informal conferences between judge and counsel, a lack of accurate information may prohibit any reliable analysis of this factor.

All judges trying cases which were a part of the experiment (including both test and control cases) were provided with a form which required them to note whether, in respect of every case, they had one or more conferences with counsel concerning the case. At present we are uncertain as to reliability of the reporting in respect of this information.

over the list in order to reduce the existing backlog.³⁹ However, both test and control cases were equally subject to the "blitz" and there seems no reason why it would have had a different effect on one group rather than the other. Some of the cohorts not yet coded or analysed were not subject to the "blitz" experience and in the final report we hope to be able to analyse the difference between cases that were, and were not, subject to this blitz.

b. Differences in the Impact of Pretrial Conferences on Settlement Rate due to Subject Matter

Given the apparent impact of pretrial conferences on the settlement rate of all cases in the experiment, we now turn to a more detailed analysis of its impact on particular types of cases, i.e., on personal injury cases as compared with non personal injury cases.

Table 3 shows the differences in settlement rates for personal injury cases and non personal injury cases within the test and control groups. Each box indicates the percentage of cases of that kind which settled, e.g., of the personal injury cases that were in the control group, 83% settled, etc.

TABLE 3

Impact of Pretrial Conferences and Type of Litigation (Personal Injury and Non Personal Injury) on the Rate of Settlement

		EXPERIMENTAL SELECTION	
		CONTROL	TEST
TYPE OF LITIGATION	Personal Injury 47 Cases	83%	96%
	Other 114 Cases	63%	82%

Significance Tests for Differences

Source of Variation	F Ratio	Significance of F
(a) Experimental Selection	7.531	.007
(b) Type of Litigation	5.431	.020
(c) 2-Way Interaction	.187	.999

N.B. 1. The above significance tests indicate that: (a) there are only 7 chances in 1000 that the difference in the probability of settlement due to the presence or absence of pre-trial conference exposure could have occurred by chance; (b) there are only 2 chances in 100 that the difference in the probability of settlement associated with the different types of litigation could be expected by chance; (c) the evidence indicates there is no differential effect of pre-trial conferences in personal injury cases as opposed to non personal injury cases.

2. The settlements referred to in this table include both pre-trial and in-trial settlements.

³⁹ See *supra*, note 17.

Several observations can be made with respect to the data in Table 3. First, it is apparent that even without the pretrial conference there is a higher settlement rate among personal injury cases (83%) than among non personal injury cases (63%). Second, the settlement rate in both classes of cases is increased by a pretrial conference: from 83% to 96% in personal injury cases, while that of non personal injury cases is increased from 63% to 82%. Third, notwithstanding the very high rate of settlement resulting when personal injury cases are pretried (96%), there is no evidence of any differential affect of pretrial conferences in personal injury cases as opposed to non personal injury cases, that is, pretrial conferences are neither more, nor less, effective with respect to personal injury or non personal injury cases.⁴⁰

As part of the experiment, these two basic subject matter classifications have been further broken down as follows: personal injury (motor vehicle and other); non personal injury (real estate, contract, and other). Because of the small number of cases being analysed as the basis of this interim report, no attempt has been made to measure the impact of pretrial conferences on these further subject matter breakdowns. However, in the final report, when a much larger volume of cases is available to be analysed, we hope to be able to measure the differences of settlement rates associated with these further sub-classifications.

c. Differences in the Impact of Pretrial Conferences on Settlement Rates due to Amount in Dispute

Although no differential impact of pretrial conference is apparent as between personal injury and non personal injury cases, is there such an impact as a function of other aspects of the cases set down for trial, i.e., as between cases involving different amounts in dispute?

Table 4 shows the difference in settlement rates for cases involving different amounts in dispute within the test and control groups. The first, and most surprising factor indicated by this Table is again unrelated to the impact of the pretrial conference. This is, the dramatically lower settlement rate, with or without pretrial intervention, for cases involving relatively small amounts in dispute, as compared to cases involving more substantial monetary claims.⁴¹

⁴⁰ This third point follows from the significance test for the two-way interaction reported in Table 3. The finding that pretrial conferences are no more, nor less, effective in personal injury as opposed to non-personal injury cases or (see note 42, *infra*) in cases of small or large amounts in dispute, have important implications with regard to how a court should use available pretrial judge time (at least where that is a limited resource). For example, the non-differential impact would suggest giving pretrial priority to those classes of cases which are likely to produce the longest trials (e.g., cases where large amounts are in dispute).

⁴¹ One possible explanation for the phenomenon is that the \$0 - \$19,900 category of cases, includes cases in which no monetary relief was claimed, e.g., specific performance cases, and the possibility that these cases are more difficult to settle. However, to date there are too few cases involving equitable relief to determine whether or not they are more difficult to settle. See also the following note.

TABLE 4

Impact of Pretrial Conferences and the Amounts in Dispute on the Rate of Settlement of Cases

AMOUNT IN DISPUTE:	EXPERIMENTAL SELECTION:	
	CONTROL	TEST
\$ 0 - 19,900 (50 cases)	54%	71%
\$20,000 - 50,000 (47 cases)	74%	96%
\$50,000 + (64 cases)	77%	91%

Significance Test for Differences

Source of Variation	F Ratio	Significance of F
(a) Experimental Selection	7.249	.008
(b) Size of Claims	5.235	.006
(c) 2-Way Interaction	.149	.999

N.B. 1. The above significance tests indicate that: (a) there are only 8 chances in 1,000 that the difference in the probability of settlement due to the presence or absence of pretrial conference exposure could have occurred by chance; (b) there are only 6 chances in 1,000 that the difference in the probability of settlement associated with the difference size of claims could be expected by chance; (c) the evidence indicates there is no differential effect of pretrial conferences in respect of cases involving different amounts in dispute.

2. The settlements referred to in this table include both pretrial and in-trial settlements.

A second observation is that the pretrial conference has a positive effect upon the settlement rate of cases in all three categories and the relative magnitude of the effect of pretrial conferences is more or less similar for cases involving small, medium and large amounts in dispute, i.e., pretrial conferences raise the absolute proportion of such cases settled rather than going to judgment by about 20%. The third conclusion, which follows from the second, is that the pretrial conference appears to have no differential impact on the settlement rate of cases involving different amounts in dispute.⁴²

⁴² The inferences drawn in the text from Table 4 may be to some extent an artifact of the categorization of the amounts in dispute and finer breakdowns of any of these categories might well reveal differential effects. For example, a distinction between cases involving less than \$10,000 and cases involving larger amounts in dispute might reveal that for these smallest cases pretrial conferences have no effect. We are grateful to Professor Maurice Rosenberg for suggesting this point.

We should here elaborate another difference between Rosenberg's and our studies. The vast majority of cases in Rosenberg's study involved relatively small amounts, e.g., in 68% of his cases the recovery was less than \$3,000, and in only 14% of his cases was the recovery in excess of \$6,000. By contrast, our cases (because the lower, County Courts have jurisdiction over claims up to \$7,500) will all, or nearly all, involve claims in excess of \$7,500.

2. *Impact of Pretrial Conferences on Trial Time*

The number of cases actually going to trial in this preliminary sample is too small to allow a precise analysis of the impact on time spent in trial. However, we can give some indication of the likely conclusions by comparing the test and control groups, i.e., by computing the trial time for the 81 test cases and for the 80 control cases. Table 5 summarizes the distribution of time in trial for the test and control groups and gives the total time involved in trials for each of these two groups. The figures indicate an absolute reduction in total trial time in favour of the test (pretried) cases of 140.25 hours. The differences in trial times for cases on the two lists are not statistically significant, but approach significance ($p. \leq .09$). The reduction in total trial time is, of course, largely attributable to the substantially higher pretrial settlement rate of the test cases, i.e., many more control group cases (40) went to trial than did test group cases (22). Thus, this calculation of differences in total trial time, cannot be viewed as a benefit additional to that of an increased settlement rate for pretried cases: it is simply another way of measuring the overall impact of the pretrial conference procedure.

TABLE 5
TIME IN TRIAL

Hours	TEST GROUP		CONTROL GROUP	
	# of cases	% of cases	# of cases	% of cases
0	59	72.8%	39*	48.7%
.25 - 1	5	6.1%	10	12.5%
1.25 - 2	2	2.5%	3	3.7%
2.25 - 4	5	6.1%	6	7.4%
4.25 - 7	2	2.5%	6	7.4%
7.25 - 10	3	2.5%	6	7.4%
10.25 - 20	2	3.6%	7	8.4%
20. - 80	2	2.5%	3	3.6%
missing	1	1.2%		
	81	100%	80	100%
TOTAL TIME IN TRIAL		158 hours		298¼ hours

* 0 trial time indicates cases settled before trial. This figure (39) is one less than the figure reported in Table 2 for the number of control cases settling without trial. The difference is accounted for by an infant settlement case (coded as a pretrial settlement) which took more than a quarter of an hour of trial time to obtain judicial approval. All infant settlements must be approved by the court.

Any reduction in average in-trial time would represent an additional benefit to that of an increased pretrial settlement rate.⁴³ Calculating the aver-

⁴³ A reduction in average in-trial time might be due to the effects of pretrial conferences in making for speedier in-trial settlements and/or in reducing the trial time to judgment. Given the small number of cases going to trial to date we have made no attempt in this Table to separate cases settled in trial from those going to judgment.

age in-trial time for all cases which went to trial, we find that the average in-trial time for test cases is $158/22 = 7.18$ hours as compared to $298.25/40 = 7.46$ for control cases. Consequently the differences in average trial time for test and control cases is not (for these data) substantial. Whether or not such differences become more substantial in the final analysis of all cases studied in this project, remains an important question.

3. Overall Impact on Court Efficiency

While the use of pretrial conferences may have other benefits (e.g., a reduction of the expense and delay experienced by individual litigants) an important question is the ability of the procedure to increase the efficiency of the operations of the court. In this context, the crucial question is whether pretrial conferences reduce the amount of judicial time spent in disposing of a given number of cases? Basically, this can be calculated by offsetting against any savings in trial time the judicial time expended in conducting pretrial conferences.

The aggregate trial time consumed by the test and control cases has already been set forth in Table 5. Table 6 shows the actual, direct, judicial time expended in conducting the pretrial conferences in the test cases analysed to date. (This information was derived from the questionnaire filled out by each judge presiding at the pretrial conference.) The Table shows the distribution of time spent by judges in preparing for pretrial conferences, and in presiding over the conference sessions and, in each case, the aggregate times involved.

TABLE 6

Time in Pretrial Conference

<u>Time</u>	<u>Time for Judge</u>	<u>Time in</u>
	<u>Preparation</u>	<u>Conference</u>
	<u># of Cases</u>	<u># of Cases</u>
0	4	0
0 - 5 Minutes	15	4
6 - 10	26	0
10 - 15	19	6
16 - 20	3	4
21 - 30	5	28
31 - 40	0	19
41 - 50	1	8
51 - 80	0	3
Missing (i.e. no information provided by judge)	8	9
	<u>81</u>	<u>81</u>
Total time involved	14.58 hours	38.03 hours
Adjusting for Missing data by adding average time	$8 \times .20 = 1.60$	$9 \times .53 = 4.77$
	<u>16.18 hours</u>	<u>42.80 hours</u>

How the total judicial time expended in conducting pretrial conferences should be calculated presents some difficulties. Obviously conducting pretrial

conferences (on a regular basis) consumes more judge time than can be measured simply by aggregating the time spent preparing for, and actually conducting, each pretrial conference held: additional judge "down-time" results from gaps in the pretrial conference schedule, and while assigned pretrial conferences, a judge is kept away (possibly for the whole day) from trial duty. On the other hand, the calculation of *trial time* used in this experiment includes only the actual in-court trial time expended on each case: it does not include the time spent by the judge in preparing for trial or in writing reasons for judgment, or any "down-time" in which the court recessed for chamber's conferences between counsel and the judge or adjourned for other purposes (e.g., for discussion between counsel). On balance (at least until we have more information on these matters) it seems reasonable to assume that there is an equivalent amount of "down-time"⁴⁴ expended in conducting both trials and pretrial conferences, and aggregate only the actual time spent by the judges in presiding over pretrial conferences.

Taking this approach, as Table 7 indicates, there is a difference of 97.45 hours (in favour of the pretried cases) in the aggregate judge time spent in disposing of the test and control cases. Based on these figures, universal pretrial conferences would result in a 33% reduction in judge time required to dispose of cases on this civil non-jury list.

TABLE 7

Impact of Pretrial Conferences On Judge Time
Spent in Disposing of Cases

	<u>Total in Trial Time</u>		<u>Total in P.T.C. Time</u>		<u>Total</u>
Control (80 Cases)	298.25 hrs.	+	-	=	298.25 hours
Test (81 Cases)	158 hrs.	+	42.80 hrs.	=	<u>200.80 hours</u>
			Difference		97.45 hours

A further way of expressing this overall effect of the pretrial conference is to express the above conclusion in terms of "average judge time spent per disposition." For test cases this would be $200.8/81 = 2.48$ hours, and for control cases would be $298.5/80 = 3.73$ hours.

How would the saving in judge time, resulting from the use of pretrial conferences, translate into increased court productivity? That is, how would the reduction in judge time, using pretrial conferences, affect the absolute number of cases disposed of in a given number of judge hours? The increase

⁴⁴ If anything the "down-time" in trial is probably greater (and it can be hypothesized that pretrial conferences may decrease trial "down-time" for pretried cases going to trial). Moreover, the scheduling "holes" experienced in conducting pretrial conferences to date were to a large extent necessitated by the experiment itself: in non-experimental conditions more flexible and efficient scheduling may be possible.

can be estimated in the following way. Using the figures in Table 7, if all 161 cases had proceeded without a pretrial conference they would have consumed approximately 600 judge hours. Had all been pretried only 400 judge hours approximately would have been consumed. In the 200 hours thus saved the court could dispose of a further 80 cases (employing pretrial conferences), thereby increasing the number of cases disposed of (241 rather than 161) by 50%.

4. *Time to Settlement*

We have already seen that pretrial conferences increase the likelihood that a case will be disposed of by settlement rather than by judgment after trial. Do pretrial conferences also produce a further benefit to litigants in the form of a more rapid settlement?

Calculating the time which elapsed from the date which cases were set down for trial to the date on which settlements were reported (for all settled cases, including both pretrial and in-trial settlements), our preliminary evidence is that the average time to settlement for pretried cases was 353 days, as opposed to 404 days for cases that were not pretried. This difference is not significant ($p. \leq .09$) by conventional standards and the average reduction in time is not very marked. Moreover, the reliability of the data on which these average times are based is open to question. For test cases, settlement dates were reported by counsel, as requested at the pretrial conference, directly to our pretrial administration office and this contact allowed us to ascertain the actual date of settlement. Information as to the settlement of control cases was only received through the Trial List Office and on a less systematic basis. Since some of these cases only reported settlement when called for trial, their exact date of settlement was only roughly estimated, or the date of the report had to be treated as the date of settlement. Thus there is a possible bias in favour of over-estimating time to settlement in the control cases.

5. *Factors Influencing the Efficacy of the Pretrial Conference*

Having examined the impact of pretrial conferences on the timing and manner of disposition of cases on the civil non-jury list, let us now touch briefly on the internal dynamics of the pretrial conference which may explain the ways in which the conferences effect the case. We have not systematically observed pretrial conferences in progress, but have obtained information from the presiding judges (and will be obtaining further information from participating members of the Bar) as to the content, emphasis and utility of these procedures. For the moment, however, all we are in a position to evaluate are preliminary data on the differences in the results of pretrial conferences, depending upon the judge presiding and the level of preparation of participating counsel.

a. *Presiding Judge*

Four judges presided over the pretrial conferences in the 81 test cases reported here. One might suppose that these and other judges would vary in their ability to stimulate settlement through pretrial conferences. The only indirect evidence we have of the likelihood of such variation is the rate of

settlement for the cases pretried by each judge. As it happens, there *was* a variation in the settlement rate of the groups of cases pretried by each judge. One hundred percent of the cases pretried by one judge were disposed of by settlement; another judge pretried a group of cases of which 90% settled; a third judge was involved in pretrials in which 83% of the cases settled; and a fourth pretried cases of which only 72% settled. However, this last judge requested that only non personal injury cases be assigned to him and, as indicated already in this report, those cases have a significantly lower conditional probability of settlement. Moreover, this self-selection of cases by the fourth judge resulted in another judge receiving an increased proportion of personal injury cases for pretrial. In any event, the number of cases that each judge dealt with was never more than 24 and proportional settlement rates for such small numbers must be treated with caution. Moreover, to properly isolate the impact of the presiding judge would require control over other factors affecting rates of settlement, e.g., the amount in dispute and the type of litigation.

Therefore, whether the identity or ability of the presiding judge is a factor influencing the efficacy of the pretrial conference is a question that cannot be resolved at this time. We hope that the larger sample to be analysed in the final report will permit an analysis of this question in greater depth.

b. Preparation of Counsel

A second characteristic of the pretrial conferences that may be expected to affect the impact of the procedure on settlement rates is the preparation of participating counsel. We obtained from the presiding judges their assessment of the quality of preparation of counsel. The first point to emphasize is the generally high quality of preparation. In 75% of the pretrial conferences, the judges were satisfied that both counsel for plaintiff and defendant were well prepared. In only 11% of the conferences were there any assessments that both counsel were not well prepared. This said, it is also apparent that the settlement rates following conferences in which all counsel were well prepared are almost identical to those for the conferences at which no counsel were well prepared, or to those for conferences in which only the counsel for the plaintiff was well prepared. The only deviation from this pattern is a significantly lower settlement rate for those cases at which counsel for the plaintiff was not well prepared and counsel for the defendant was well prepared.

However, considering the very small number of cases in which any counsel was unprepared, whether or not differences in the preparation of counsel affect the outcomes of pretrial conferences must await analysis of the complete set of data from this experiment.

6. *Impact of Pretrial Conferences on the Outcome of Cases*

The final issue we can discuss on the basis of the data available so far, relates to the question of whether pretrial conferences affect the outcome of the litigation. This question has two aspects. First, do pretrial conferences affect "who wins", i.e., do settlements or judgments tend to go more often in

favour of defendants or plaintiffs in pretried cases as opposed to non-pretried cases? Second, do pretrial conferences affect the amount of recovery, i.e., do plaintiffs recover more (or less) from settlements or judgments in pretried cases as opposed to non-pretried cases?⁴⁵

In connection with the first question of "who wins" in settlements or judgments as a result of pretrial conferences our data indicate an unexpected result. Whereas Rosenberg had reported no difference in the recovery rate for plaintiffs in pretried and non-pretried cases,⁴⁶ the analysis of our preliminary data indicates that plaintiffs recover more frequently after pretrial conferences than they did without pretrial conferences. That is, in 95% of our pretried cases the plaintiff recovered something by means of either settlement or judgment, whereas in non-pretried cases plaintiffs recovered something only 80% of the time.⁴⁷ The data as to "who wins" in settlements are particularly interesting. In pretried cases that were disposed of by settlement the plaintiff always won something. But in those non-pretried cases disposed of by settlement, the plaintiff won something only 85% of the time.⁴⁸

We will not attempt in this interim report to speculate as to how and why the pretrial conference should produce this differential impact in terms of who wins. That is not to say that there are no tempting, "obvious" explanations, e.g., the difference between "who wins" in settlement resulting after pretrial conference (plaintiffs "win" in 100% of cases) in contrast to settlement without a pretrial conference (in which plaintiffs "win" in only 85% of the cases) suggest that at the pretrial conference judges tend to recommend that the defendant pay something to plaintiffs with very tenuous cases in order to settle the case. Assuming a similar pattern emerges from the final data the impact of the pretrial conference on "who wins" will present a major issue requiring further analysis in the final report.

Turning to the question of whether pretrial conferences affect the amount recovered, the preliminary results show that the average recovery (expressed as a proportion of the original claims) received in settlement or judgment is 31% for the cases that were exposed to pretrial conferences and 24% for

⁴⁵ Another question relevant to the impact of pretrial conferences is whether or not pretrial conferences improve the "quality" of trials. This is a question that Rosenberg investigated in his New Jersey experiment (and he reported that there was such a positive effect: see Rosenberg, *supra*, note 4 at page 29 *et seq.* In our view the difficulty of establishing criteria by which the quality of the trial should be assessed and the further difficulty of getting reliable data indicating the perceptions of judges and counsel as to the extent to which these criteria have or have not been met in any case, led us to the decision not to pursue this issue. Moreover, improving the quality of trials has not been considered a major objective of pretrial conferences by members of either the bench or bar in Ontario, for whom the issues of cost, delay and improved court efficiency are paramount.

⁴⁶ See Rosenberg, *supra*, note 4 at 60.

⁴⁷ $X^2 = 8.229$; $.01 > p < .001$ indicating that this could have occurred by chance between 1 time in 100 and 1 time in 1,000. That is, the statistical significance is high.

⁴⁸ A similar breakdown as to "who wins" in cases terminated by judgment (with or without pretrial) is not reported here because, owing to the small number of cases going to judgment, the data could not approach statistical significance.

cases not so exposed.⁴⁹ This difference is not significant ($p. \leq .25$) by normal scientific criteria, but if a similar difference is present in our final data the statistical significance will likely increase as a result of the larger number of cases involved.⁵⁰

Hence, it is worth noting that there is an indication that pretrial conferences may favour (either through settlement or judgment) plaintiffs. If such a substantive bias is established, particularly if it is sufficiently marked in degree, it may raise questions as to the viability and justifiability of the pretrial conference procedure.

D. CONCLUSION

The preliminary results discussed in this interim report suggest that pretrial conferences in Ontario have dramatically more impact than would have been predicted from interpolations based on past research in other jurisdictions.

Despite the caveats expressed in this report and the marginal statistical significance of the results dealing with time in trial, it is reasonable to expect that the analysis of data relating to the balance of the cases in the experiment, based on a larger sample, will confirm the trends indicated in these preliminary results. Such a prediction is reasonable given that the data analysed here are a true random sample, albeit small. In some instances (e.g., the settlement rate data) the significance levels are already very high. Where the significance levels are not high (e.g., the data on time in trial) they approach significance and are reasonably high given the small number of cases that actually go to trial. It seems probable that the statistical significance of all these results will increase when the analysis is based upon a larger sample.

We have in this article reported the preliminary results of *quantitative* research into the impact of pretrial conferences. This analysis indicates what happens when pretrial conferences are employed, but it tells us nothing about the mechanisms by which pretrial conferences produce these impacts. Obviously how and why pretrial conferences have these impacts are interesting questions, the answers to which may have important implications in terms of both the likely long-term effect of pretrial conferences and their desirability in terms of social policy.

⁴⁹ Rosenberg, *supra* note 4 at 61 *et seq.*, reported that plaintiffs recovered appreciably more in pretried cases than in non-pretried cases. He reported they recovered 20-30% more measured by the average or medium recovery, not by comparison (as in our figures) of the amount recovered as a percentage of the amount claimed.

⁵⁰ It is to be noted that the data on amounts recovered mentioned in the text are for all dispositions, whether by settlement or judgment. Differences in percentage recovery may vary as between cases terminated by judgment or settlement: we have been unable to breakdown our present data in this way. It may be particularly interesting to see whether a significant proportion of pretried cases settle for nominal monetary awards, i.e., whether the difference between "wins" at settlements (100% for plaintiffs in pretried cases, as opposed to 85% recovery for the plaintiffs in non-pretried cases) is to be explained in part or in total by the fact that while plaintiffs win at settlement after pretrial, often all that is involved is a nominal recovery.

In our final report we hope to analyse not only the quantitative impact of pretrial conferences based on the more extensive set of cases to be analysed, but also to explore some of the qualitative aspects of the implementation of a pretrial conference procedure.

ADDENDUM

Since this article was submitted for publication the Ontario Rules Committee has passed a rule, effective March 1, 1978, providing for the holding of pretrial conferences. The rule provides as follows:

Pre-Trial Conference

244(1) When an action, cause or matter has been set down for trial or hearing, the Court, upon the application of a party or upon its own motion, may, in its discretion, direct the solicitors for the parties or any party not represented by solicitor, to appear before it, in the case of the solicitors, with or without the parties, for a conference to consider:

- (a) the simplification of the issues;
- (b) the possibility of obtaining admissions which might facilitate the trial or hearing;
- (c) the quantum of damages;
- (d) estimating the duration of the trial;
- (e) fixing a date for the trial or hearing;
- (f) the advisability of directing a reference;
- or
- (g) any other matters that may aid in the disposition of the action, cause or matter or the attainment of justice.

(2) Following the conference, the Court may make an order reciting the results of the conference and giving such directions as the Court considers necessary or advisable. If such an order is made, it shall thenceforth control the course of action, cause or matter, provided that the judge at the trial or hearing may modify the order as he deems just.

(3) The judge who conducts a pre-trial conference in any action, cause or matter shall be deemed not to be seized of such action, cause or matter and shall not thereafter try or hear such action, cause or matter.

(4) All documents which may be of assistance in achieving the purposes of the pre-trial conference, such as medical reports and reports of experts, shall be made available to the judge presiding at the pre-trial conference.

(5) The costs of the pre-trial conference are to be in the discretion of the judge presiding at the pre-trial conference.

(6) Nothing in this rule shall prevent a judge before whom a case has been called for trial from holding such a conference either before or during the trial without disqualifying himself from trying the action.

It is worth noting that this new rule makes no express reference to the obtaining of a settlement as one of the purposes of the pretrial conference.

