bers of the patent bar. He was a former U.S. Commissioner of Patents, who received, in 1960, the distinguished Service Award of the U.S. Atomic Energy Commission. Mr. Ooms served with distinction, for many years, as a member of the Law School Visiting Committee.

WILLIAM S. HEFFERAN, JR., JD'15 spent about a decade in the private practice of law following his graduation. In 1928 he joined the General American Transportation Corporation, from which he retired thirty years later as Vice President and General Counsel.

Not an alumnus, but friend of the Law School and member of its Visiting Committee, TAPPAN GREGORY was perhaps best known for his service to the organized bar. During a noteworthy career, he was President of the Chicago Bar Association, President of the Illinois State Bar Association and Chairman of the House of Delegates and President of the American Bar Association. Mr. Gregory was a member of the U.S. delegation at the U.N. Conference on International Organization in 1945, and represented the ABA at the Nuremberg Trials. He is known to a generation of lawyers as Editor-in-Chief of the American Bar Association Journal.

Book Reviews


Review by WILLIAM L. PROSSER
Professor of Law, University of California

(The review which follows appeared in the Journal of Legal Education, Volume 13, Number 3, pages 431-433 (1961), and is reprinted here by the kind permission of the editors of the Journal.)

This is a most remarkable book. Requested to review it, against a rather impossible time limit, I started out with the rapid perusal, half reading, half skimming, which usually suffices a give a fast scanner a fair idea of the contents, and to permit some adequate, if admittedly imperfect, estimate of the merits. After about a dozen pages, my current eye was arrested, caught, and held by a passage, and steed and rider were halted short with an abrupt check rein. Another ten pages, and I had dismounted to a walk, with time out for savoring of sentences, and the importunate publishers with their un-
reasonable deadline were consigned to perdition. I ended by reading the book through twice at a snail's pace. I have come to the conclusion that it is a masterpiece, and a most extraordinary one, which no one but the author could have written.

Professor Karl Llewellyn is sufficiently well known to all law school men, and, indeed, to a large part of the American bar, if only as the Chief Reporter of the Uniform Commercial Code. He first emerged from the Yale Law School in 1920, after making life quite difficult for its faculty, including in particular William Howard Taft. Since then, for forty years, he has played with gusto and skill the part of the gadfly and the wasp, the man behind the jackass with the goad, prodding always with new ideas, with critical appraisal, with challenge, denunciation, and sometimes even praise, and with the urge to change and action. His voluminous writings betray a scintillating and fascinating personality, clad in a coat of many colors, which those who know him best have learned to approach with the caution and respect of one about to open a bottle of rare old Armagnac, with crumbling cork encrusted with the dust of the ancient home of D'Artagnan. His literary effusions have displayed, in amazing variety, elements of the genius, the poet, the visionary, the scholar, the skeptic, the cynic, the madman, the shrewd and sophisticated man-of-the-world, the plain damn fool (albeit with a lovely sense of humor), and the hard-headed practical lawyer with a keen sense of the limitations of the possible and his eye on the ball. When all of these assorted mules have been pulling in different directions, the effect sometimes has been one of an agreeable but bewildering schizophrenia. When all of them have pulled together, the effect sometimes has been overwhelming. They all have pulled together in this book; and it is, I think, by far the best thing that he has ever done. It is clearly the product of many long years of work, study, and thought, tried and tested in the fire of law school courses at Columbia and Chicago. Stimulated as I am by the Armagnac, and drunk with deep potions of his effervescent style, I do not find it easy to set impressions down on paper. But I can try.

The book is an elaborate and exhaustive study of the process of judicial decision at the appellate level, in so far, and only in so far, as it is or can be made apparent from the opinions written. It is, in other words, a penetrating study of the technique of writing opinions. It refers, usually in some enlightening detail, to some six hundred cases, which range over all fields of the law from marriage to election contests, and are chosen not for their substance, but for their method. Of these, I found that I had at least some nodding acquaintance with perhaps a hundred; and I was moved, from time to time, to pull others off the shelf to verify the author's observations. They were verified. Two or three times I found myself in disagreement with him as to whether a result might be a good one or a bad, but never as to the route by which the court had reached it.

There have been studies of cases before—Wambaugh comes at once to mind, as long ago as 1894—but never a book like this. Not only is it the most complete picture ever given of legal decisions as such en masse, and of the mental operations of those exalted but very human beings who become judges, and what makes them tick; it is also a very clear and, to me, utterly fascinating portrayal of the whole process by which our law has developed and grown, and of the slow-creeping change that, together with adherence to the past, has been the life of our legal system. This book is the whole story of stare decisis. That is, in a nutshell, what it is about. It is the supreme justification, the ultimate paean of praise, of what the title calls and the author has, at page 399, glorified in a very good song, as the Common Law Tradition.

The book begins by rejecting flat out the gastronomical approach to decided cases—the notion that the actual decision is made for reasons or bias not stated, and the opinion is only a reasoned justification, a "mere rationalization," "a term which for fifteen years or more meant (i) false, (ii) tricky, and (iii) a cover for the nastiest imaginable hidden motives, with carte blanche to anybody to fill in without evidence." Not so, says Llewellyn; not so at all. Judges, in the main, are honest men and moderately able men, almost always willing and commonly capable of overcoming innate repugnance and deciding against their own preference; and they do try hard, and honestly, to follow "the law" that has been handed down before. And so clear is this that to any one who really reads and understands opinions and knows a bit about what the particular court has been doing, the result of eight out of ten cases that come up on appeal is predictable, or "reckonable" in advance; and so reliable is this that the best law firms do, in fact, habitually so predict and reckon, with a constant high percentage of accuracy year after year. There follows a chapter dealing with the "steadying factors" that make for this predictability: law-conditioned judges, known legal doctrine and techniques, the written opinion, the tradition of the single right answer, issues that are limited and sharpened and phrased, adversary argument by counsel, group decision, and others sufficiently familiar to the bar.

The book then launches into a consideration of the "leeeways" of precedent, with many illustrations of the various verbal devices by which the court refuses to budge from the "rule," or inches along within it, or shapes it into a new form, or confines it within limits, or erodes it away, or, finally, jettisons it entirely and makes a fresh start. Here Llewellyn distinguishes three periods in American judicial history. There is the early one of what he calls the "grand style," characterized by a high degree of flexibility and liberal use of the techniques of change. This is followed by the era of the "formal style," with closer adherence to the letter of
Delegates to the national Conference on Classification in Law Libraries, which met at the Law School in November.

what was decided before, although by no means without more subtle methods of deviation. This is succeeded (since, say, 1920 to 1940) by the modern period, with both approaches still at war, but, on the whole, a marked return toward the "grand" method.

There is a chapter on "situation-sense" and reason, stressing the importance of the facts, and suggesting how to look for those that will tip the scale. There is another on the theory of "rules," and what they mean and are good for, followed by a return to "sense and reason" again. There is another on argument, and the art of making the prediction come true. There are nearly fifty pages of "conclusions for courts," with much sound advice to judges upon how to do it and how not, with scorn and contumely poured upon various "illegitimate" techniques, of which the simplest and most convenient is utterly ignoring the case that blocks the path like a fallen boulder. There is a great deal more, of which space and the reader's patience forbid the enumeration. No summary can begin to cover all that is in the book, or give any picture of the way in which all of it is illustrated and exemplified throughout by particular decisions, with much quotation of specific language. Nor can it indicate the quite delightful way in which it is all set forth, with a naturally exuberant and flamboyant style curbed, subdued, and channeled to say it all with neatness, point, and force.

It is, in short, a unique and most valuable book. Obviously, it is one to be taken into account in our law schools. Somewhere in the course of his legal education, any student ought to be exposed to it. I must confess that at the moment, I am a bit puzzled as to just where or how. It is not, I think, anything to be inflicted upon an entering freshman. The neophyte who is still struggling to understand that a demurrer is not the comparative of an obsolete adjective formerly applied to young ladies would simply be drowned. Somewhere later on, when he has learned to deal with cases in batches of a dozen at a time and search out trends and patterns, and to try his wings at advocacy and other skills, he ought to be handed this book, and allowed to sit quietly in a corner for several days. Even then, I have some lingering suspicion that the boys at the bottom of the class, at least in the law schools I have frequented, would make precious little of it, although the top man would benefit enormously. Since professors can sometimes be useful, the desirable thing would be to discuss large chunks of the book somewhere in class.

As to one thing, I am entirely clear. It should be required reading for any judge.


Reviewed by the Honorable Irving R. Kaufman
Judge of the U.S. District Court
Southern District of New York

(The review which follows is reprinted from 74 Harvard Law Review 807-15 (1961) and appears through the kind permission of the author and the Review. Copyright 1961 by The Harvard Law Review Association.)

Since Delay in the Court was published, at least a score of legal periodicals have seen fit to carry reviews. Such nationwide response may be attributable solely to the present topical interest of the subject matter, but that seems unlikely. Anyone who has sought to follow the literature on judicial administration with assiduousness knows, somewhat to his sorrow, the number of books and articles which are published on that topic each year. He is also aware that by far the greater part of such works receive little or no comment, save that made under their breath by persons attempting to read them and find filing space for them. It would seem, therefore, that the authors of Delay in the Court must have done something different, or, more likely, differently, in order to have stirred up such a flurry of response.

To put the matter in its most general terms, the authors of this book have attempted to combine "the skills of the lawyer with the techniques—especially the 'quantifying' techniques—of the social scientist," giving their study "a firmer foundation in statistical data than any previous study of the problem." It appears that it is not the book's conclusions which are the spurs to interest, nor the breadth of its scope, but its approach. That being so, a few general remarks on that approach seem in order.

Ordinarily, methodology is better disclosed by viewing it in operation than by reading the methodologist's dis-
cussion of it. Thus the following fragment from the text, apparently inserted for little other reason than to advance the narrative, appears to me symbolic of the entire technique of the book:

[W]ith behavior as subtle and complex as that underlying the settlement process, a priori speculation is always risky. So once again our chief concern is to see if the inquiry can be advanced by resort to empirical data. (p. 112) (Footnote omitted).

In my view, that little fragment discloses both the strengths and weaknesses in a statistical or "quantifying" approach to the complex problem of judicial administration.

The strengths of such a technique are obvious. By carefully collecting, collating, and interpreting hard and cold figures, the authors are able to fill in the fuzzy phrases that typify and stultify the subject of delay. Moreover, these same techniques make it possible to create standards for comparing judicial administrations, and especially make it possible to evaluate the "cures" in a more or less objective fashion. Of the strengths of quantification I shall have more to say presently.

But the weaknesses of the qualitative approach are so much more subtle that they deserve immediate attention. First, statistics are by themselves unfit to value judgments; they can only describe and circumscribe "amount." Knowing how much of something exists tells us nothing about whether that something is bad or good. For instance, as any chemist is aware, discovering that each liter of X contains one gram of Y is practically significant only when one knows the usual use of X and the ordinary effect of Y. If Y is poisonous to man, and X is a city's drinking water, then ethical considerations enter and the information becomes significant outside the closed statistical or quantitative system. But, that is only because a prior, unarticulated, considered judgment has been made that the poisoning of human beings is bad. That unarticulated ethical premise has nothing to do with quantity. The statistic is significant only in more complex contexts, e.g., in a world where there will always have to be some Y, and where Y kills human beings only if it is in a certain concentration. Such a quantitative analysis is most significant where Y is a good or an evil depending on its quantity, as for instance in a case where a certain amount of Y is necessary for human life while a slightly greater amount is poisonous.

To apply the above analysis to the subject at hand, this book can measure the quantity of delay, and the extent to which certain variables affect that quantity. But quantification is of no use unless certain qualitative assumptions are made. Some of these are quite simple to make, e.g., that delay is "bad"; that full utilization of court time is "good." But some assumptions depend on far more subtle discriminations that are not at all quantitative. For instance, abolishing the jury would in one sense be "good" because it would reduce delay, which is postulated as "bad." But, as the authors are aware, the administration of justice depends on things other than speed. The danger is that the quantitative "good" and "bad," which often mean little more than "faster" or "slower," will be accepted as ethical value judgments. The authors rarely fall into this trap but, as I shall attempt to indicate later in this review, occasionally they do.

The second subtle weakness in the use of quantification and statistical analysis as a method is that it necessitates reliance on the most dangerous of abstractions: averages, means, and so forth. These are abstractions because they take from actuality a quality that can be reduced to number and combine it with another similar quality to produce a result which inheres in neither actuality. It is somewhat like what Arthur Koestler called a Communist's definition of an individual: a crowd of one million divided by one million. These abstractions are dangerous because they are presented in the form of a mathematical quantity, and our modern, scientific conditioning predisposes us to put our faith in numbers as somehow "accurate" and "true."

One clear example of how averaging can mislead was the most sophisticated statisticians appears early in the book. In contrasting the periods of waiting faced by those litigants who are able to get a trial preference and by those who will have to await trial in regular order, the authors state that the proper measure of delay for the system as a whole is the average of these two waiting periods. Then they go on to state that this average delay "is the delay that interests a litigant who really does not know whether he will get any preference or not." (p. 45)
I submit that an “average litigant” might be interested in the “average delay,” but that a real litigant would have to wait out either the preference or the no-preference period. In fact, if there is one period of waiting that a real litigant would be certain not to have, it is the average between the two waiting periods.

With these general considerations which define my approach stated, the book under consideration can be discussed somewhat more specifically.

**Delay in the Court** is divided into five major sections. The first of these is devoted to measuring, as closely and accurately as possible, the actual extent of delay in the Supreme Court of New York County. The last is devoted to various problems tangentially related to the delay problem. The middle three sections are devoted, one each, to the only three possible “cures” for delay:

- The time required for the disposition of cases can be shortened; the number of cases requiring official disposition can be reduced by affecting the settlement ratio; or the amount of available judge time can be increased, either by directly adding judges or by increasing somehow the efficiency with which the current judge power is now used. (p. 5)

Each of these five parts is worthy of comment. But the nature of the book is such that merely sketching the authors’ conclusions would not only not do justice to the painstaking development of those conclusions; it would be positively misleading. Certainly, the authors come to conclusions, often very precise ones, but their statistical sophistication is such that the total context of any one of their conclusions modifies it significantly. Under such circumstances, one must either reproduce the book *in toto*, or concentrate on very small segments of the total work. For obvious reasons, I have chosen the latter approach.

Chapter 13 is devoted to pretrial proceedings. (pp. 141–54) Except for two brief paragraphs, (pp. 99, 141–42) the pretrial conference is treated as if it were primarily a settlement device. There is nothing unreasonable about such a treatment. Pretrial is a settlement device, for some of the judges all of the time and for all of the judges some of the time. As a matter of fact, it is one of the best settlement devices existing. Held early in a case’s career, it helps the attorneys to clarify the facts, preparing them for intelligent talk about settlement. Held on the very eve of trial, when the attorneys know as much about the case as they ever will (at least until the witnesses begin to testify), when the emotional drain of actual litigation is imminent, when the clients finally must face getting nothing or giving more than they like to think about, it comes at precisely the right moment for “private ordering.” The addition of a neutral, impartial observer in the person of the judge would often seem to be enough to tip the balance toward a settlement. And indeed, the figures bear out what logic suggests. “In the New York Court . . . not less than 37 percent of all pre-trials end in settlement then and there.” (pp. 142–43)

However, the authors are wise enough not to accept their own figure uncritically. “The problem . . . is whether these cases settled at pretrial would have been settled anyway in the natural course of events.” (p. 143) As they point out, since most judicial time is used up on cases which go to trial and judgment, the settlement at pretrial of cases which would be settled before assignment, whether pretrial or not, does not reduce delay. In fact, it uses up judge time which could otherwise have been spent on actual trials. (p. 148)

However, this particular evaluative problem has often been noted elsewhere, and is well known to students of pretrial techniques. It is indeed the rare judge who is as naive as the one hypothesized by the authors (p. 143) who believes that a 37 per cent settlement figure in pretrial means a net gain in time of 37 per cent. We all know better than that. But the authors do not stop with the observation that the pretrial often wastes judge time on cases that would never get to trial in any event. They do not even stop with determining whether there is in fact any increase in the number of cases settled because of pretrial conferences. They go on to give a quantitative answer to the crucial question, “whether this additional number of cases reaching trial [if pretrial were abandoned] exceeds the number which the pretrial judge could try if he were transferred to the trial part.” (p. 148) Using figures from the New York Court, the authors conclude that to justify the pretrial judge’s activity his pre-trying must in fact have reduced the percentage of cases reaching assignment from 37 to 30 . . . . [If it were not to reach that level, the court ought to stop pre-trials because it could dispose of more cases by having the pre-trial judge try cases instead. (p. 149).

In this conclusion is typified, I believe, one of the strength-weakness dyads alluded to above. On the one hand, the reader is not just left with the bland fact that pretrial-conference settlements are not all net gain. Instead he is given a fairly precise figure, which can be used in different years and, with modification, in different courts, for the precise amount of gain necessary to justify pretrial as a means of reducing delay.

On the other hand, pretrial is not only a means for reducing delay by settlement. The authors apparently became so impressed by the neatness of numbers that they were led into a highly questionable generalization: that pretrial should be abolished in any jurisdiction where the net-gain figure is not met. In so concluding, they overlooked the fact that pretrial, though it may not lead to the settlement of a suit, may nevertheless lead to admissions, clarifications, and stipulations which will shorten the actual trial time necessary, thus acting indirectly to reduce delay. This oversight was no doubt caused by the authors’ allowing their approach to compartmental-
ize their thinking into a deceptively neat category entitled "the contribution of pre-trial to changing the settlement ratio." (p. 141)

More important than that relatively minor oversight, however, is the fact that the authors' quantification misled them in this instance into giving no consideration to qualitative factors. By dogged accretions of data and painstaking juxtapositions of figures, the authors could no doubt incorporate into their pretrial-justification figure the factor of more efficient and speedy trials for non-settled cases. But no amount of quantification will disclose whether the time spent in pretrial is justified by the fact that the trial eventually held is a better trial in ways having nothing to do with time. If the attorneys are not surprised at a trial, it will be a better trial. If they fully understand their own cases, and the ramifications of the evidence to be adduced, it will be a better trial. If needless duplication of evidence and the introduction of needless technical evidence are eliminated, again, the trial will be better. It may also be faster. But whether it is or not, it will still be more likely to achieve substantial justice because time was taken to pretry the case. Quantification is valuable, perhaps indispensable, for dealing with quantities—like time. It is extraordinarily dangerous if it obscures qualities—like fairness.

Part 4 of Delay in the Court is entitled "More Judge Time." It deals with the third major manner in which a delaying backlog may be removed—by providing more adjudicators. I found myself, ex officio, reading this portion of the book with the highest degree of attention. I must confess that when a judge reads a brightly lit study of the work habits of judges, his original inclination is to write what is in effect an Apologia Pro Vita Sua. But upon calmer reflection I have concluded that (a) the authors of this book have performed a service both to judges and to the cause of efficient judicial administration; (b) this service involves not only pointing out that judges are mortal, but that their work habits are, though variable, quite good; and (c) that once again the value of statistics is illustrated by rendering inapplicable any radical generalizations like "perfect" or "shocking."

Exclusive of holidays, there are 196 trial days each year in the New York court. The average number of trial days for any single judge of that court, however, is 170. (p. 174) The net loss in trial days is therefore 13.4%. Moreover, the average number of hours which each judge spends on the bench each day is 4.1. (p. 181)

Once the above-quoted figures are derived, however, the problem has just begun. The question of what has become of the "lost" days and hours remains. Table 70 (p. 175) gives some indication. Officially, 9.1% of the lost days are assignable to "no ready cases," and 8.0% to "in chambers." Another 125% was lost to illness, with 13.8% being used up by religious holidays. These categories account for 43.4% of the lost days. Furthermore, as the authors note, "absence" here denotes an absence of trial activity. It does not necessarily mean that the judge was wholly absent from the court. In any of the above categories, except illness, religious holidays, and miscellaneous personal reasons, the judge may have been in chambers." (p. 174) It is for this reason, among others, that the authors state:

we have been unable to isolate satisfactorily how much of the loss is due to inefficiency in the trial scheduling process, producing gaps over which the judge himself has no control, and how much of the loss is due to a disinclination on the part of the Court to work as hard as it might. (p. 179)

Nevertheless, the authors do discover a "lost weekend" effect, which appears sinister at first blush. I am able to think of a few explanations for this effect which the authors could not (e.g., the difficulty in finding one-day cases to try, the manner in which official judicial conclaves are held in distant cities on Saturdays), but in the main I must agree to some extent with what the authors imply, that judges are indeed human.

To an extent this humanity also explains the 4.1 hour court day. But, that explanation is operative, I believe, to a much smaller degree than those unfamiliar with the work of judging would assume. No one has yet determined how long a trial day should be. In New Jersey, which the authors postulate at one point as possessing "the top performance that can be expected of a court system" (p. 176) the average judicial day on the bench is 4.5 hours. My own quantifying techniques tell me that this adds up to a difference of 24 minutes per day. This difference would certainly not be de minimis over the course of a year, but it is not earth-shaking either. There are always things to be done in chambers, even on "sitting" days and at home even after the court day. Charges must be written. Opinions on holdover cases must be researched and written (yes, even book reviews prepared). In many ways, a judge's four-hour day is very much like a professor's twelve-class-hour week: There is more to doing it than meets the public eye.

My major point in regard to this portion of Delay in the Court is not, however, merely that the authors have attacked a difficult and delicate subject with a great deal of insight. Once again, I am drawn to a "discourse on method" and its inherent dangers. In this instance, however, the pitfall of oversimplification was completely avoided by the authors. The danger remains, however, that far less sophisticated persons will be drawn into error. For one of the weaknesses of statistical analysis is that it may become scripture for various "devils" to quote. A statistic out of context is a deadly weapon. It has a seductive air of exactitude about it which does not fool experts in quantification (like the authors of Delay in the Court) but does mislead those less practiced in the art. Great care must be taken not only to avoid espousing quantification as the only method of analysis, but also to avoid oversimplifying the use of statistical analysis to render it less worthy of even the jobs it can do best.
My only other comment on this portion of the book deals with a cure for delay which I believe was given too little attention. Naturally, only so much can go into one book. But I believe that the authors would have been well advised to explore the use of masters in the alleviation of court congestion. The book does treat of the various plans and experiments utilizing substitute judges, (pp. 214–20) but no consideration is given to the possibility of having semi-judicial officers take over the burden of work which is not strictly adjudicatory. For instance, it has long seemed to me worthwhile to explore the possibility of employing standing masters to supervise the discovery or interlocutory phase of litigation. The less judge time that must be devoted to the interlocutory phases, the more that is free for trials. Moreover, the utilization of non-judges for discovery and related procedures would provide less of “an aura of . . . second-class justice,” (p. 219) of which complaint is quite properly made when substitute judges are used for actual trials. In England, no trial judges take part to any significant extent in the management of the interlocutory phases of cases; it is all done by masters. It is further worth noting that as far back as 1904, when that year’s New York Commission on Law’s Delays pointed out that twenty-three English judges conducted twice as many trials as did their New York counterparts, they did not mention that this estimable efficiency was achieved with the aid of the highly trained English masters. True, the English masters have a very long history which gives them the automatic prestige of venerability. But if some litigants will accept substitute judges for actual trial, they are even more likely to accept standing masters or pretrial examiners to assist in the management of the interlocutory phase. Moreover, pretrial hearings under the supervision of pretrial masters or examiners might be as effective as judge-supervised proceedings. If that were to prove true, then we would have all the advantages of pretrial, with no use of judicial time. While it is foolish to complain when an author does not do what he has not set out to do, I for one would have appreciated a comparative study of the English and American practices.

I have dealt with only a small portion of the book, and that part has been treated mainly from the viewpoint of method. One nevertheless ought not take away the impression that Delay in the Court is slim on interesting conclusions. Not only the authors’ methods, but the results of those methods are of signal importance in any rational study of judicial statistics and judicial administration. Of especial importance are the chapters on the actual cost in time of retaining the jury system (ch. 6) and the effect of the concentration of the trial bar on trial delay. (ch. 17) In these fields it appears that our intuitions have led us somewhat astray, as they are often wont to do. The trouble with so many of the recent works on judicial administration is that they are often framed in terms of generality. I have yet to find anyone who dared dispute that “excessive delay is bad” (especially in the light of the way the adjective assumes the conclusion, not to mention the subtle question-begging of the noun). But it is absurd to think that such statements will ever get us anywhere unless someone appears to tell us, with reasonable exactitude, just how much delay there is. For that job, the quantifiers are in demand. If there is one thing the authors have done, it is to show us through their book how a group of sophisticated men can use figures to reach valuable results. Occasionally they slip, and forget that people are not merely measurable quantities, but that is extraordinarily rare. For the most part they succeed splendidly in giving some content to matters which were “pretty well understood” by those familiar with the field. And occasionally they succeed in showing that such “understanding” was illusory. Already the book is being extensively utilized by other scholars. As time goes on its figures will no doubt be refined and its techniques applied to other courts and other judicial problems. In time we will have more facts with which to work. It will not be a judicial millenium, but it will be a distinct gain for justice. This is a pioneering study and the work of the authors is worthy of high praise and emulation. Indeed, the authors serve to remove the mental block which has permeated the thinking on court congestion, that removal of backlogs is an impossible task in this litigious nation. They give us heart and hope.