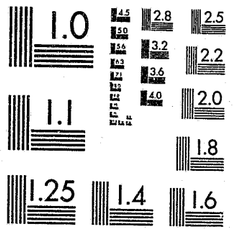

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CASE PROCESSING TIME: MEASUREMENT AND EXPLANATION

by

Mary Lee Luskin
and
Robert C. Luskin

Indiana University
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INTRODUCTION

Where criminal trial courts are concerned, "performance" is a matter of the disposition of cases. Thus many of the dimensions of court performance have to do with case outcomes. Others have to do with the procedures by which outcomes are reached. But another important dimension is time. How long does the court take to process a case? This is the variable this report is about. The report consists of this introduction and three discrete but related papers.

If it were not for the skeptical view recently taken by Church (1982), it would be hardly necessary to argue the importance of case processing time. Indeed, for the moment we shall simply assert that it is important, that it has consequences for prosecutors, defendants and their attorneys, judges, court administrators, politicians, and the public at large--in short, for everyone who works in, has to deal with, or is affected by the courts. Later (in the opening paragraph of Chapter 3 especially), we sketch some of the stakes for court participants and the rest of us and (in Chapter 4) trace the effects of processing times on caseload or backlog in particular.

If, therefore, we want to evaluate court performance, we must define and measure case processing times. Previous definitions have varied, but we think it most important to examine the entire period from arrival to disposition by dismissal, plea, or verdict. As the phrasing has already implied, we also think it important to study the processing times of individual cases, as opposed to the mean (or median) processing times of

populations or groups of cases. Given this definition, measurement is straightforward, except as it touches on sampling. In contrast to most other research on case processing time, we sample "forward," taking a sample of cases initiated, as opposed to cases disposed, during the period of study.

Of course, the problem is not merely to evaluate court performance, but to explain and improve it, and if we want to do that, we must consider the process by which case processing times are determined. What factors affect processing times, by how much, and under what conditions? These are the central questions of this research. The answers are important both in themselves, for understanding, and for their practical implications. Only by knowing what factors are influential, how much influence they have, and for what sorts of cases can we judge proposals aimed at reducing processing times, or the processing time side-effects of other changes. Only then can we forecast what the processing time consequences will be and decide whether they are worth the cost.

Recent work has taken us some distance toward explanation (Gillespie, 1976; Rhodes, 1976; Church et al., 1978a; Hausner and Seidel, 1979; Neubauer et al., 1982; Neubauer and Ryan, 1982), but our understanding remains incomplete. We still do not have a theory-informed model of processing time as a function of all the case- and (especially) court-level variables that stand to affect it. Here we build and advance on previous work by developing a more general, inclusive, and theory-derived model and estimating special cases of it on individual-level data collected over two- to three-year periods in three courts.

To give a brief preview, we begin with the proposition that everything that directly affects processing time comes under one or more of five general headings: the burden of work in relation to resources, the

complexity of the case, events in the life of the case, participant incentives, and structural and administrative facilitation. This premise guides the selection of explanatory variables. A variable directly affects processing time and belongs in its equation only if it falls into one or more of these categories. For the most part, we take the effects (roughly, slopes; see Chapter 2, n. 13) to be linear and additive (i.e., constant). But theoretical considerations dictate nonlinear or nonadditive effects for some variables, and we write the equation accordingly.

We estimate the effects on data from three courts, each of which saw major structural and administrative changes during the period of study. Admittedly, we do not have observations on all the variables we might like to include. (This is almost in the nature of things.) Some variables simply do not vary in these three courts during the period of study. Others may have varied but went unmeasured or inadequately measured. The ratio of jail population to jail capacity is an example. But despite these and other, lesser limitations, the data allow us to estimate the effects of an unusually inclusive array of explanatory variables. Most important, they allow us to estimate the effects of actual changes, as opposed to mere differences, in structural and administrative arrangements.

In Chapter 2, "Case Processing Time: Issues of Explanation and Reform," we develop the model of case processing time and estimate its parameters on data from one state criminal court. Chapter 3, "Case processing Time in Three Cities," is a replicative and comparative effort in which we extend the model to accomodate additional court-level variations, estimate it for two more cities, and compare the results. We try both to generalize where the results justify generalization and to speculate about the boundary conditions responsible for differences.

Finally, in Chapter 5, "Accounting for Caseload: A Simple Model," we shift from causes to consequences, proposing and estimating a distributed lag model expressing a court's pending caseload as a multiplicative function of the number of new filings and the mean processing time for each of a number of preceeding months.

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CASE PROCESSING TIME IN CRIMINAL COURTS: ISSUES OF EXPLANATION AND REFORM*

From the time of Shakespeare and before, there have been complaints about "the law's delay," and it is a common perception that cases generally take too long to wend their way through the American courts (National Center for State Courts, 1978). Normatively, this is a matter of "delay" versus "haste," but the factual question is simply of time. At the individual level, how long does a given court take to dispose of a given case? The answer is the variable increasingly and most descriptively known and case processing time. The cases involved can be either civil or criminal, and if criminal, either misdemeanor or felony, but here we shall restrict our attention to felony cases.

Definition aside, the most fundamental question is of explanation. Why does a given case take as long or as short as it does? If we know the answer, we can easily aggregate to find why cases in general take as long or as short as they do. Beyond that, we can draw lessons for attempts at "reform." Given the more or less consensual view that case processing

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times are generally too long,[1] there have been numerous efforts to reduce them, but we cannot tell what is likely to succeed, unless we know what has an effect, under what conditions, and by how much.

Although a number of studies have recently tackled this question, our understanding of the factors that determine case processing time remains tenuous and incomplete.[2] Many of the relevant studies are of the effects of single causes and do not adequately control for other variables. Church et al. (1978) and Flanders (1977), the major sources of what Church (1982) proclaims the "new conventional wisdom," are essentially bivariate (as Church acknowledges). The same is true of Grossman et al. (1981), Boyum, (1979), and Nimmer (1978). In addition, many of these studies rely on a "backward" sampling of cases, measuring processing times on samples of cases disposed as opposed to initiated over a given interval. Cases disposed at a given time may have begun at widely varying times and may not really be comparable in the sense of being processed under common conditions.

More recently, a couple of studies have taken us further by estimating multivariate models on adequately sized samples of cases initiated (Hausner and Seidel, 1979; Neubauer and Ryan, 1982). But even here there remain serious shortcomings. Neubauer and Ryan (1982) provide no general ground for the inclusion or exclusion of specific variables and in fact surrender the final selection to stepwise regression (on the atheoretical nature of which, see Lewis-Beck, 1978). Hausner and Seidel (1979) do a better and more explicitly reasoned job of variable selection, but still neglect some obvious candidates, including pretrial motions and the defendant's failing to appear for scheduled events. And neither study really addresses the effects of court structures and arrangements--an unavoidable omission where, as in Hausner and Seidel, the data simply do not provide a window on

structural variation, but an important one nonetheless.

Consequently, these efforts still leave us far short of the goal of a theoretically-informed and fully-specified model of case processing time. The object of this paper is to advance toward that goal. We analyze a large sample of cases initiated over a two-year period in Detroit's Recorder's Court. These data have their limitations, but are exceptionally rich in comparison with most, and especially so in providing glimpses of structural variation. Drawing on these data, we shall specify and estimate a more comprehensive model of case processing time. We shall propose explicit criteria for the inclusion and exclusion of variables and explicitly confront the question of functional form, making the model nonlinear and nonadditive where appropriate.

Setting and Data

Recorder's Court is the municipal Criminal Court of Detroit, with both preliminary and trial jurisdiction. At the time of this study, it had an elective bench of 20 judges, augmented by a variable number of visitors, and received approximately 11,000 new felony cases each year. Our data consist of a random sample of the cases that entered the court from April, 1976 through March, 1978. The sample was stratified by month of origin and totaled 2,079 cases. As a baseline, the sample-wide mean processing time was 76 days, with a standard deviation of 94. The monthly mean varied from 52 days for cases entering in July, 1977 to 147 days for cases entering in May, 1976.[3] The advantage of studying this particular court during this particular period is that Recorder's Court was at this time the site of an LEAA-sponsored delay reduction project that altered the calendaring system, temporarily increased judicial manpower, and made other changes in the operation of the court (see Neubauer et al., 1981, for details). These

changes present an unusually good opportunity to examine structural as well as case-specific influences on processing time.

Influences on Processing Time

Everything that affects processing time, it seems to us, must be an aspect of one or more of five generic causes:

I. Caseload. The conventional premise is that over any fixed period of time, court personnel have a limited number of manhours at their disposal and that any given case requires a certain minimum number of manhours to be processed. If so, the more cases there are to compete for available manhours, the longer it should take, on average, for any one of them to receive its necessary minimum. Granted, courts may process cases more or less efficiently, disposing of a greater or lesser number in a given amount of time, even with constant resources (see Gillespie, 1976). But ceteris paribus--that is, controlling among other things for the factors that govern efficiency, which come under III and V below--one might well suppose that increasing caseloads lengthen processing times.[4] On the other hand, a court's most basic task is simply to dispose of cases, and from this perspective caseload is a criterion of court performance. Concommitantly, it is a source of pressure, with judges and prosecutors preferring to keep it within reasonable bounds for fear of embarrassment or electoral displeasure (Eisenstein and Jacob, 1977; Heumann, 1979; Nardulli, 1978; Nardulli, 1979). Hence judges and prosecutors may process cases more, not less, expeditously as caseload rises. Either way, case processing time should depend on caseload. The only question is one of sign.

II. Case Complexity. Some cases are almost inherently more time-absorbing than others. In state criminal courts such as this, there may

actually be little variation under this heading, as Neubauer and Ryan (1982) suggest, but there is probably some. For example, cases with larger numbers of defendants can be expected to take longer owing to the necessity of coordination.

III. Incentives. Court participants--defendants and their counsel, prosecutors, judges, and others--are semi-independent decision-makers with particular institutionally-defined options and associated reinforcement contingencies. Like the rest of us, they respond to economic (Blumberg, 1967; Nardulli, 1978; Eisenstein and Jacob, 1977; Feeley, 1979), social (Eisenstein and Jacob, 1977; Mather, 1979; Flemming, 1983; and Nardulli, 1978), intellectual (Heumann, 1978), and professional (Heumann and Loftin, 1979; Loftin et al., 1983; and Eisenstein and Jacob, 1977) incentives to take certain actions and avoid others. Some of the incentives relevant to processing time are case-specific. Other things being equal, a jailed defendant has a greater interest in speed than one who is free on bail. Others derive from the court's structural and administrative arrangements. For example, an individual as opposed to central docket or calendaring system may make judges more eager to keep the docket uncluttered.

IV. Case Events. As the case unfolds, participants make their decisions, taking some actions and not others, and what they choose to do often shortens of lengthens the processing time, either by design or as a side-effect. Defendants fail to appear, prosecutors choose to bargain, judges grant continuances, and so on.

V. Structural Facilitation. A court's administrative arrangements may be such as to facilitate or retard the movement of cases, quite apart from their effects on the motivations of court participants. Arrangements of this sort include scheduling procedures and the assignment of cases to courtrooms.

Admittedly, the variables that fall under these headings are not entirely the same from court to court. Different courts are differently organized and function in different environments, and a variable that matters in one court may not matter or even vary in another. Inevitably, the model for any one court is a special case, and involves only a proper subset of the variables, of the general model that applies anywhere, anytime. In Detroit during this period, the variables that figure to have affected case processing time include the following:

Disposition Type: Trial and Early Dismissal (IV). One major case event is the way the case concludes--by dismissal at or before the preliminary hearing,[5] by subsequent dismissal, by guilty plea, or by trial. Plainly, dismissal at or before the preliminary hearing abbreviates processing time. At the other end of the spectrum, a trial is intrinsically more time-consuming and requires lengthier preparation than any other mode of disposition--this despite cross-court comparisons of trial rates that seem to suggest that trials add little or no time (Church et al., 1978; Flanders, 1977; Gillepsie, 1976; 1977); case-level analyses of more fully specified models show the expected effect (Neubauer and Ryan, 1982; Hausner and Seidel, 1979).[6] Cases that end by plea or dismissal are in-between.[7] All this may be so obvious as to seem uninteresting, but variables cannot be omitted for lack of excitement, and the model would be misspecified without disposition type. Besides, interactions with other variables (see below) will add spice.

Pretrial motions (IV). Motions for discovery, to supress evidence, or for evidentiary hearings--all take time in both hearing and preparation (Neubauer and Ryan, 1982). It is the fact rather than the number of motions that seems to matter.

Psychiatric Evaluation or Treatment (IV). Another place time is lost is where the defendant must be evaluated for competency to stand trail. More time is lost if he is found incompetent and must be treated. But the increase in processing time is not tautologically equal to the number of days of evaluation or treatment. The court may attempt to make up for lost time once the defendant returns. Or there may be auxilliary delays surrounding the actual evaluation and treatment.[8]

Failure to Appear (IV). Defendants may fail to appear for scheduled hearings. The longer the defendant remains AWOL, the longer it takes to complete his case, though as with psychiatric evaluation or treatment, the number of days the case awaits the defendant need not translate to an equal number of days of processing time.

Repetition of Preliminary Examination (IV). A preliminary examination that was originally waived may belatedly be asked for and held. In other cases, a preliminary examination that was in some way insufficient may be reheld. Either way, it is back to square one, and typically, a longer time to completion.

Mistrial (IV). Repetitions of later events have similar effects. When a trial ends in a mistrial and the case is sent back for a new trial, there is an obvious cost in processing time.

Continuances (IV). Continuances other than those resulting from failure to appear, motions, or psychiatric evaluation or treatment also add to processing time, and in the same obvious way. The more numerous the continuances, the longer the processing time.[9]

Number of Defendants (IV). Multiple defendants exacerbate problems of scheduling and coordination and can complicate and thus prolong the bargaining process.

Attorney Type (III). A number of authors have suggested that retained attorneys handle cases more slowly than public defenders or court-appointed counsel (Blumberg, 1967; Nimmer, 1978; and Nardulli, 1978, for example). The difference is that retained attorneys collect their fees directly from clients, who generally need time to scrape the money together. Since clients may be less eager to pay once the case has been resolved, the attorney may prefer to keep the case open until he has received all or most of his fee.

Pre-Trial Incarcertation (III). A jailed defendant has more incentive to reach a conclusion, despite the strategic advantages of delay, than a defendant who is at liberty. (See Nimmer, 1978, and Nardulli, 1978, on the perceived advantages of postponing disposition, and Bernstein, et al., 1977; Rhodes, 1976; Rossett and Cressy, 1976; and Goldfarb, 1975, on the effects of pretrial incarceration.) Less directly, jail status also affects the incentives felt by judges, prosecutors, and defense attorneys. To the extent that the state imposes shorter time limits for the disposition of cases involving jailed defendants (Nardulli, 1978; Thomas, 1976), that judges believe that presumptively innocent defendants should be detained as short a time as possible, or that jail space is a scarce commodity (Flemming, 1983), the court can be expected to give priority to jailed defendants.

Seriousness (III). In more serious cases, the defendant risks a greater penalty if convicted, and should therefore be more desparate to obtain an acquittal or favorable plea bargain, and thus more determined to draw out the case in the hope that it will eventually deteriorate. At the same time, the seriousness of the charge is one of the bases on which judges and prosecutors allocate time and other resources (Mather, 1979; Heumann, 1978; Forst and Brosi, 1977). In more serious cases, judges tend

to allow a fuller exercise of the adversary process, which of course consumes more time. As a rough and ready measure of seriousness, we use the maximum term of incarceration (in months) on the original charge.[10] Where there is more than one count, we use the highest of the maxima involved, and we arbitrarily quantify "life" as 480 months.

Prior Record (III). Like the seriousness of the charge, the defendant's prior record affects the degree of jeopardy in which he finds himself and thus his incentives to impede the progress of his case. The greater the number of previous convictions the heavier the sentence, if the present case ends in conviction, is likely to be. Hausner and Seidel's (1979) results lend support.

Regular or Visiting Judge (III). The visiting judges who served on Recorder's court during this period did not really have dockets of their own, even under the "individual docket" (see below). Hence they escaped the regular judges' incentives, under the individual docket, to keep their caseloads within bounds. This implies an interaction with docket and caseload, to which we shall come presently.

Docket Type (III). During the period of study, Recorder's Court changed from a central docket or master calendar for the entire court to an individual docket for each judge.[11] Rather than being responsible, as under the central docket, for an illdefined share of the work, each judge now had to answer for a specific set of cases. To reinforce the effect, the delay reduction project staff published bi-monthly reports listing and ranking judges' caseloads. To avoid appearing in a bad light, judges had to move cases rapidly enough to keep their dockets down. Thus accountability brought incentive. Even Church et al. (1978), who are skeptical of docket type's effect in criminal courts, report "competition among the

judges in virtually every individual calendar court visited" (p.73).

Anecdotal evidence suggests that many court participants saw it the same way. As one administrator put it, the central docket's supporters were motivated by the feeling that "the pressure is getting to me and I don't want to work this hard." Certainly, it was difficult to get judges to accept cases under the central docket. One employee of the state court administrator's office described the court scheduling officer as having to go "around the court on his knees." Judges who worked hard became resentful, and worked less hard. In the words of the same administrator, they became

> unhappy about having less productive judges foist cases on them, encroaching on their leisure, trying to harness their productivity for the good of the order against their will, so they ensured that there would be no excess productivity on their part.

One of the harder-working judges said much the same thing:

> Some of the . . . more ambitious judges finally adopted the attitude of "What's the use? It doesn't pay to carry more than your fair share of the load because the others just don't care."

In contrast, the individual docket made it difficult for slackers and sluggards to escape attention. In addition, the docket change touched the judge's incentives to offer the defendant incentives to plead. Under the central docket, the judge supervising pretrial negotiations would not have to try the case himself if negotiations failed, and there was reason to be cautious. As one judge put it,

> Under a central docket a judge is not active [in plea bargaining] in that he's not going to be stuck with the case. There's no reason why he should be, you see. One of the most difficult things that a judge does is not in the trial stage, but in the pretrial of cases. The risk that a judge takes on his reputation--his political reputation--and everything else is in the pretrial stage. When I decide to take a reduced plea and give a sentence bargain, that's when I lay myself on the line for criticism.

Of course, critics of the individual docket argue that the central docket is more rational and efficient (For a review of arguments on both sides, see Solomon, 1973.) In essence, this is to assert that the docket variable is not really of Type (III) but of Type (V), and that its impact is not negative but positive. We believe the effect is negative, but the data will tell. A number of previous studies suggest that the effect is neither positive nor negative but nil (see Church et al., 1978, and the critical review in Nimmer, 1978). But it is debatable how far these studies are to be trusted, since only one--Nimmer's own (reported in Nimmer, 1978)--is able to control for even a handful of other relevant variables, and it is based on a sample of cases involving only three offenses, which may or may not be typical. Again, the data will tell.

Case Track (III, V). Another innovation was a "case-track" designed to bring all cases to disposition within 90 days of arraignment. The case-track required that each of the major events in the life of a case--preliminary examination, plea negotiations, pretrial motions, and trial--take place by a specified deadline. In part, this was an instance of administrative facilitation. Project personnel prepared forms showing scheduling dates that would meet a ninety-day track for trial cases and held workshops for judges and their clerks on case scheduling and the use of the newly developed forms. But the case-track also added incentives. First, it provided more exigent standards for judicial performance. Although judges could not be forced to adhere to the deadlines, a considerable effort was made to persuade and/or shame them into compliance. Second, it altered the defendant's strategic situation. The plea cut-off date, after which no further concessions could be offered, undermined the defendant's incentives to foot-drag.

The Decentralization of Plea Bargaining (I, III, V). The delay reduction project also aimed to decrease processing time by transferring plea bargaining from the central prosecutor's office to individual "docket prosecutors," one for each of five groups of neighboring (same-floor) courtrooms. Proximity, it was thought, would breed familiarity, which in turn would breed efficiency. In addition, there may again have been an effect on incentives. Under the central system, plea-bargaining was the province of specialists, who did nothing else. With decentralization, the docket prosecutors were made responsible for monitoring and coordinating the (by that time, individual) dockets within their domain. Consequently, the docket prosecutors had reason to adjust their plea-bargaining so as to keep "their" courtrooms' processing times relatively short and their dockets relatively small. Finally, the decentralization of plea-bargaining was also accompanied by an increase in the overall number of prosecutors, and may have decreased processing times by that route as well.

The Crash Program (I, III, V). A final element of the delay reduction project was what was known as the "crash program." This did several things simultaneously. It expanded the bench by bringing in visiting judges, provided for monitoring the hours each courtroom was in operation, instituted meetings among prosecutors, judges, project staff, and representatives of the police department and sheriff's office, and reopened negotiations on selected older cases to bring them to a speedy conclusion. The addition of the visiting judges decreased the per-judge caseload; the monitoring of courtrooms rewarded those who worked long hours and sanctioned those who shirked; the reopened plea negotiations upped the defendant's incentives to plead; and the efforts at increased coordination may have been a facilitating factor.

Caseload, Individual and Court-Wide (I). Under the central docket the

relevant caseload is that of the entire court normed against judicial manpower; under the individual docket it is the caseload of the judge handling the case. Operationally, both caseloads are as of the beginning of the month. Under the individual docket, at least, we expect caseload to have a negative effect, since with the advent of individual accountability, caseload became a highly visible criterion of performance. Judges who improved their position in the monthly rankings pointed proudly to their achievement--even judges who claimed not to pay attention to caseload. Judges who lost ground conspicuously avoided the subject.

Most of these variables are veterans of one study or another, yet we want to emphasize that this is no mere census of putative causes. Not every variable in the literature is here.[12] Nor is the selection random or ad hoc. Constraints of measurement aside, the inclusion or exclusion of specific variables is a function solely of their ability to serve as the minor premise of an implicit syllogism. The simple rule is, if it comes under one or more of the five generic causes above, then it belongs on the list of causes; if not, it doesn't. While recognizing that this is theory of only a simple, unmathematical sort, we claim three advantages for this self-conscious approach to variable selection. First, it at least points us in the direction of comprehensiveness. Within the limits of observation, we have tried to include everything that has to do with caseload, case complexity, incentives, case events, or structural facilitation. At the same time, the list is selective. The major premise tells us not only what to include but what to ignore. And, third, the simple deductions involved provide clear reasons for us to believe that these variables, and not others, have an effect.

What Kinds of Effects?

Of course, it is not quite enough to specify a list of explanatory variables. We must also consider the ways they affect case processing time. This is a question of the shapes of their effects (roughly, slopes).[13] In the absence of reason to the contrary, we shall presume linear and additive--which is to say, constant--effects.

Nonetheless, several effects would on the face of it seem to be nonconstant. The effect of prior record is probably nonlinear (a function of its own value). An additional conviction should make a little difference to the likely sentence, and thus to the defendant's incentive to procrastinate, when it is merely the latest of a long string. The greatest difference should be between 0 prior convictions and 1. The difference between 1 and 2 should be smaller, the difference between 2 and 3 smaller still, and so on. To capture this pattern, we write processing time as a linear function of the logarithm of the number of prior convictions, rather than of the number itself.[14]

The remaining nonconstancies are nonadditivities (effects that are functions of the values of other explanatory variables). Since a judge has more control over trials than other dispositions, the individual docket should have tended to shorten trial cases more than others. Since the case-track introduced new deadlines for both motions and trials, we should expect it to have achieved greater reductions in case processing time for cases involving motions or trials. Because the definition of caseload depends on docket type, it is the per-judge caseload of the entire court that should matter under the central docket, but the individual judge's caseload that should matter under the individual docket. Moreover, the individual caseload should matter only for regular judges, because the visiting judges did not really have dockets of their own. And, finally,

because a case is not officially part of the caseload until after the preliminary examination, neither caseload variable should have an effect for cases dismissed at or before the preliminary examination. We accommodate these interactions by including the appropriate multiplicative terms.

With these and only these departures from linearity and additivity, and by abbreviating the variable names, we may write the equation for case processing time as[15]

$$\begin{aligned} CPT = {} & B_0 + B_1\,TRIAL + B_2\,DPE + B_3\,MOTION + B_4\,PSYCH + B_5 FTA \\ & + B_6\,RPE + B_7\,MISTR + B_8\,CONTIN + B_9\,\#DEF + B_{10}\,RETAIN \\ & + B_{11}\,BAIL + B_{12}\,SERIOUS + B_{13}\,PRIOR + B_{14}\,DOCK \\ & + B_{15}\,TRACK + B_{16}\,LOCPLEA + B_{17}\,PROGRAM + B_{18}\,DOCK*TRIAL \\ & + B_{19}\,TRACK*TRIAL + B_{20}\,TRACK*MOTION \\ & + B_{21}\,AVLOAD*(1-DOCK)*(1-DPE) \\ & + B_{22}\,JLOAD*DOCK*(1-DPE)*REGJ + u. \end{aligned}$$

Table 1 provides a key to the abbreviations and a reminder of

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Table 1 About Here

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operational definitions. The u is an unmeasured disturbance, and the B's are the unknown parameters on which the effects depend.[16]

Estimation and Results

For the most part, we make the conventional assumptions about u, but homoskedasticity is an exception. The precipitous decline in processing times over the period of study suggests the possibility of a similar decline in their variance, and, more to the point, in the variance of the disturbance. Dividing the observations by month of origin and subjecting

Table 1

Variables in the Model

Variable	Abbreviation	Operationalized as	Min.	Max.	Mean	S.D.
Case processing time	CPT	Days between arrival (arraignment on warrant and disposition)	1	774	76.28	94.13
Number of defendants	#DEF	Number of defendants	1	8	1.21	0.56
Dismissal at or before the Preliminary Examination	DPE	1 When case is dismissed at or before preliminary examination 0 Otherwise	0	1	0.19	0.39
Trial	TRIAL	1 When case is disposed by trial 0 Otherwise	0	1	0.11	0.31
Pretrial motions	MOTION	1 When formal pretrial motions are filed 0 No pretrial motions	0	1	0.15	0.36
Psychiatric evaluation	PSYCH	Days lost to psychiatric evaluation or treatment	0	98	0.87	7.85
Defendant's failure to appear	FTA	Days lost due to defendant's failure to appear	0	605	5.82	37.09
Number of continuances	CONTIN	Number of continuances	0	8	0.14	0.54
Repetition of Preliminary examination stage	RPE	1 When case sent back for preliminary exam 0 Otherwise	0	2	0.02	0.16
Mistrial	MSTR	1 When mistrial declared 0 Otherwise	0	1	0.002	0.05

Variable	Abbreviation	Operationalized as	Min.	Max.	Mean	S.D.
Type of defense attorney	RETAIN	1 When attorney is retained counsel 0 Otherwise	0	1	0.18	0.39
Pretrial incarceration	BAIL	1 When defendant is free prior to the disposition of the case 0 Jail	0	1	0.66	0.47
Seriousness of charge	SERIOUS	Statutory maximum of count with highest maximum in months	0	480	150.47	151.97
Defendant's prior record	PRIOR	Number of prior convictions	0	62	2.02	3.87
Regular vs. visiting judge	REGJ	1 When case was heard by regular judge 0 When case was heard by visiting judge	0	1	0.77	0.42
Docket type	DOCK	1 When case was initiated under individual docket 0 Under central docket	0	1	0.73	0.44
Case-track	TRACK	1 When case was initiated after case-track in place 0 Before case-track	0	1	0.53	0.50
Decentralization of plea	LOCPLEA	1 When case was initiated after plea bargaining decentralized 0 Before decentralized plea bargaining	0	1	0.18	0.39
Crash program and aftermath	PROGRAM	1 When case was initiated in crash or post-crash periods 0 Otherwise	0	1	0.24	0.43

Variable	Abbre-viation	Operationalized as	Min.	Max.	Mean	S.D.
Judge's caseload	JLOAD	Number of defendants on disposition judge's individual docket in month in which case was initiated	0	399	75.215	93.186
Average caseload	AVLOAD	Total number of defendants before court at beginning of month in which case was initiated, divided by the number of judges available at that time	103.10	423.42	224.95	39.07

the hypothesis of equal variances to the usual likelihood ratio test (see Mood et al., 1974, pp. 439-40) produces a ^{2} of 2006.8, with 23 degrees of freedom. Faced with this evidence of heteroskedasticity, we have turned to generalized as opposed to ordinary least squares (GLS as opposed to OLS) for our estimates. In effect, each observation is weighted by the reciprocal of the sample standard deviation of the (OLS) residuals for its month. The results are displayed in Table 2.[17]

Table 2 About Here

On the whole, the results are as expected. Most of the parameter estimates have the signs explicitly or implicitly predicted above, and most attain conventional levels of "significance." At .47, the R^2 is rewarding by micro-data standards,[18] and higher than any previously obtained for processing time.[19]

For the most part, the effects are simply the corresponding parameters, but the nonlinear or nonadditive ones are more complicated functions of both parameters and variables. Thus Table 2 supplies estimates of many but not all of the effects; the rest must be estimated as the appropriate functions of the estimated parameters and explanatory variables. These results are reported in Table 3. Let us consider each variable in turn.

Table 3 About Here

Disposition Type. The "how" of disposition has a major effect on the "how long" of it. A dismissal at the preliminary examination cuts processing time by about 31 days. Under the individual docket, there is, in addition, a small but nontrivial decrement that varies with the size of the

Table 2

Parameter Estimates

R^2_{GLS} = .4703 N = 1233

Variable	Coefficient	(S.E.)	Variable	Coefficient	(S.E.)
#DEF	6.77*	(2.32)	PRIOR	0.86*	(0.45)
TRIAL	68.99*	(14.57)	DOCK	-1.88	(14.70)
DPE	-31.04*	(4.43)	TRACK	-9.13	(4.76)
MOTION	53.70*	(7.99)	LOCPLEA	-13.68*	(3.18)
PSYCH	1.39*	(0.18)	PROGRAM	-18.58*	(8.99)
FTA	1.05*	(0.04)	DOCK*TRIAL	-15.14	(17.94)
CONTIN	17.46*	(3.07)	TRACK*TRIAL	-0.06	(11.89)
RPE	12.87	(10.41)	TRACK*MOTION	-33.87*	(9.04)
MISTR	23.90	(26.10)	AVLOAD*(1-DOCK)*(1-DPE)	-0.03	(0.04)
RETAIN	-1.18	(3.27)	JLOAD*DOCK*REGJ*(1-DPE)	-0.08*	(0.02)
BAIL	9.83*	(2.98)	CONSTANT	58.45*	(12.88)
SERIOUS	0.06*	(0.01)			

*Significant at the .05 level (by one-or two-tailed test as appropriate).

Effects on Case Processing Time

Variable	Effect (S.E.)
Number of Defendants	6.77 (2.32)
Dismissal at Preliminary Examination	
Central Docket	-31.04-.03 AVLOAD[a] (4.43)
Individual Docket with Regular Judge	-31.04-.08 JLOAD[b] (4.43)
Individual Docket with Visitor	-31.04
Trial	
Central Docket	68.99 (14.57)
Individual Docket	53.85 (11.04)
Individual Docket and Case-track	53.79 (4.73)
Pretrial Motions	
Before Case-track	53.70 (7.99)
Under Case-track	19.83 (4.30)
Psychiatric Evaluation	1.39 (0.18)
Failure to Appear	1.05 (0.04)
Number of Continuances	17.46 (3.07)
Repetition of Preliminary Examination	12.87 (10.41)
Mistrial	23.90 (26.10)
Retained Attorney	-1.18 (3.27)
Pretrial Release	9.83 (2.98)
Seriousness of Charge	0.06 (0.01)
Regular Judge	
Post-Preliminary Exam Cases and Individual Docket	-0.08 JLOAD
Otherwise	0.00

Table 3

Effects on Case Processing Time

Variable	Effect (S.E.)
Log of Prior Record	0.86 (0.45)
Docket Type	
Dismissal at Preliminary Examination	-1.88 (14.70)
Other Non-trial Disposition with Regular Judge	-1.88-.08 JLOAD+.03AVLOAD[c] (14.70)
Other Non-trial Disposition with Visitor	-1.88+.03 AVLOAD[d] (14.70)
Trial Disposition with Regular Judge	-17.02-.08 JLOAD+.03AVLOAD[e] (23.18)
Trial Disposition with Visitor	-17.02+.03 AVLOAD[f] (23.18)
Case-track	
Non-trial Disposition without Motions	-9.13 (4.76)
Non-trial Disposition with Motions	-43.00 (9.50)
Trial Disposition without Motions	-9.19 (11.56)
Trial Disposition with Visitor	-43.06 (12.44)
Decentralized Plea Bargaining	-13.68 (3.18)
Program (Crash and Post-crash periods)	-18.58 (8.99)
Court's Average Caseload for Central Docket	-0.03 (0.04)
Judge's Caseload for Individual Docket	-0.08 (0.02)

[a] At a near-mean value for AVLOAD (225), the effect is -37.75 with a standard error of 10.55.

[b] At JLOAD (100), the effect is -39.04 with a standard error of 6.03.

[c] At JLOAD (100) and AVLOAD (225), the effect is -3.13 with a standard error of 21.49.

[d] At AVLOAD (225) the effect is 4.87 with a standard error of 21.50.

[e] At JLOAD (100) and AVLOAD (225) the effect is -18.27 with a standard error of 28.75

[f] At AVLOAD (225) the effect is -10.27 with a standard error of 28.72.

individual judge's caseload (assuming he is not merely a visitor). The effect of going to trial is even larger, depending on structural arrangements. Under the central docket and sans case-track, a case's going to trial increased its processing time by 69 days. As expected, the introduction of the individual docket seems to have reduced this effect, although the difference falls well short of statistical significance. Low variance in DOCK*TRIAL (which is 1 only for trials, which are rare to begin with, and only under the individual docket) seems to be to blame. Contrary to expectation, the effect seems not to have changed with the addition of the case-track. This suggests that the case-track's plea cut-off date provision had little effect (see the further discussion below).

Pretrial Motions. Before the case-track, pretrial motions added an average of 54 days to the processing time of a case. Afterward, they added only 20 days. Motions still made a difference but a much smaller one. This is another and a clearer instance of the conditioning effects of administrative arrangements.

Psychiatric Evaluation or Treatment. Every day of psychiatric evaluation or treatment appears to prolong the case by 1.4 days, an estimate that is significantly different from 1.0. Probably the reason for this slippage is that cases in which the defendant needs psychiatric attention are particularly problematic. Plea negotiations may take longer because it is more difficult for the defense attorney to determine what his client wants or is willing to do and for the defense attorney, presecutor, and judge to settle on an appropriate outcome.

Failure to Appear. In contrast, the defendant's failure to appear neither entails the loss of any additional days beyond the time he is gone, nor brings any compensatory acceleration. The estimated coefficient is almost exactly 1.

Repetition of the Preliminary Examination. The point-estimate of the effect of having to re-hold the preliminary examination is roughly 13 days--modest but not tiny--but the standard error is large, so that we cannot put much stock in it. Most likely, the standard error is large because this variable has so little variance, as repetitions of the preliminary examination are infrequent.

Mistrial. Mistrials are rare but costly in processing time, adding an average of 24 days. Here, too, however, the standard error is large, and probably for the same reason.

Continuances. Each continuance increases processing time by 17 days. This is roughly consistent with Hausner and Seidel (1979), although differences in variables, models, and courts make precise comparisons impossible. Hausner and Seidel suggest that continuances are a stand-in for a number of actions by court participants for which they have no measures. Here we have measured and entered several of these separately, in motions, psychiatric examinations, and failures to appear, yet continuances still have a major effect--even larger than in Hausner and Seidel. Consequently, the court management literature's emphasis on the importance of a strong continuance policy (as in, for example, Sipes et al., 1980) would seem to be well placed.

Number of Defendants. On the average, a case seems to take almost seven days longer for every additional defendant. If the number is large, the cost in time involved is substantial. A case with 5 defendants can be expected to take 34 days longer than a case with only one.

Attorney Type. Contrary to expectation, cases with privately retained attorneys do not take distinguishably longer than those with court-appointed counsel.

Pretrial Incarceration. As predicted, the cases of jailed defendants move more rapidly. The estimated difference is roughly 10 days. In part, this results from the difference in defendant incentives discussed above. But in part it also results from differences in incentives for other court participants. The cost of housing the overflow population from the overcrowded Wayne County jail was the immediate impetus for the delay reduction project (Flemming, 1983, and Neubauer, et al., 1981), and "what kept our feet to the fire," in the words of a delay reduction project manager. Our interviews reveal that the "jail problem" loomed large in participants' minds.

Seriousness. For every additional month of maximum penalty, there is an average increase of .06 days of processing time. This means, for example, that the difference between a charge of possessing cocaine, which under Michigan law carries maximum sentence of 24 months, and one of arson of a dwelling, which carries maximum sentence of 240 months, is roughly 14 days.

Prior Record. Though significant, the effect of prior record is somewhat smaller than anticipated. The first prior conviction adds roughly 4 days of processing time, the next adds roughly 2 1/2 days, the thirds adds under 1/2 day, and so on.

Docket Type. As expected, the effect of docket type depends on disposition type. For cases dismissed at the preliminary examination, the change from the central to the individual docket had essentially no effect. That is hardly surprising since the change altered calendaring only after the preliminary examination. For cases ending in plea or dismissal, however, the individual docket did bring a modest reduction in processing time, although the size of the reduction varied with the judge's caseload. The heavier the caseload, the more docket type mattered. Thus, for example, the effect of being under the individual rather than the central docket was to reduce the expected processing time by 12 days for a judge with 125 cases, but by only 6 days for a judge with only 50 cases. For cases going to trial, the effect becomes larger, though still not huge, reaching 21 days for a judge with 50 cases and 27 days for a judge with 125 cases.

If taken at face value the estimates suggest that the effect of changing from the central to the individual docket was to reduce the processing times of cases by some two or three weeks. These figures, however, fall considerably short of statistical significance. The failure to attain significance even for trial cases where the effect should be and apparently is at its largest, is attributable to the large standard error for the docket X trial interaction, which again is a matter of low variance. All we can say with assurance is that the effect varies with the judge's caseload. Still, we believe the effect is there. Except for its statistical insignificance, the effect looks right--negative, and larger in absolute value for trial cases for judges with larger caseloads. And the anecdotal evidence cited above lends support.

Case-Track. Like the individual docket, the case-track cut processing times dramatically for certain kinds of cases. Cases with motions were 43 days shorter under the case-track. Even for non-trial cases without motions, the case-track made a difference of 9 days.

Location of Plea Bargaining. The decentralization of plea bargaining seems to have been a success, reducing the expected processing time by 14 days. Regrettably, we cannot say whether this was the product of administrative facilitation, change in prosecutorial incentives, or the addition of prosecutors.

Crash Program. The intense monitoring, reopening of negotiations, decrease in sentence lengths, and hiring of judges that constituted the crash program decreased case processing time by about 19 days. Again, we cannot definitely separate the motivational, administrative, and manpower components of this effect. Among veterans of the crash program, however, the increased hours and heightened motivation loomed largest in retrospect.

Caseload. One of our more interesting findings is that under the central docket, the per-judge caseload has no significant effect, whereas under the individual docket, the judge's own caseload has a negative effect. When, for instance, a judge's own caseload increases by 50 cases, he compensates by speeding up his average case processing time by roughly 4 days. Under the individual docket, judges seem to have become more "docket conscious," monitoring their dockets and attempting to keep them under control.[20] Of course, the other side of the coin is that judges whose caseloads diminished tended to relax their efforts, moving cases more slowly. The story is told that one judge who had a three-day trial removed from his docket promptly took a three-day vacation.

Discussion

To sum up: the majority of the model's variables do seem to affect processing time and, together, to explain it quite well. Only attorney type and average caseload plainly fail to have their anticipated effects. Three more effects are arguable: docket type, repetitions of the preliminary examination and mistrials show up with substantively nontrivial but statistically insignificant effects.

The variables that clearly have an effect include representatives of all five overarching factors. Dismissal at the preliminary examination, going to trial, motions, continuances, psychiatric evaluation or treatment,

and failure to appear reflect the actions of participants. The number of defendants is an aspect of case complexity. Whether the defendant is in jail, the seriousness of the charge, case-track, the decentralization of plea bargaining, and aspects of the crash program have to do with incentives. The individual judge's caseload and, indirectly, the addition of judges under the crash program reflect caseload. And the case-track, the decentralization of plea bargaining, and, again, aspects of the crash program come under structural facilitation. Precise comparisons are impossible, but, as between categories, the caseload variables seem relatively ineffectual. The impact of average caseload is nil, and that of individual caseload nontrivial but small. On the other side of the scale, it is impossible not to be struck by the role of incentives. Like the rest of us, court participants respond to rewards and sanctions.

As anticipated, certain of the effects are significantly interactive, their magnitudes hinging on the values of other variables. The effect of a dismissal at the preliminary examination is a bit greater under the individual docket, where it is an increasing function of the judge's caseload, provided that the case is being handled by a regular judge. The effect of pretrial motions is much slenderer under the case-track. Symmetrically, the effect of the case-track is greater for cases with motions. For post-preliminary examination cases, the effect of docket type is an increasing function of the judge's caseload under the individual docket. And although the difference is not statistically significant, the effect of docket type seems a good bit greater for trial cases as well.

In our view, these results should put paid to the discouraging notion that the causes of processing times (and other court outcomes) can be traced no further back than to some nebulous "local legal culture," defined primarily as a matter of participants' norms and expectations (Church,

1981; 1982; Sherwood and Clarke, 1981; and Church et al., 1978).21 These variables' success clearly shows that more specific and precise explanation is possible. Since we take no explicit account of them, it also suggests that norms and expectations are neither so influential nor so exogenous as the champions of local legal culture explanations would have us believe. Norms and expectations may have some effect on processing times, but the dominant flow is almost certainly the other way around. Court participants develop a sense of how long given cases can be expected to take by implicitly averaging the processing times of similar cases. Moreover, the effect norms and expectations do have is too proximate to be of much interest. To the extent they actually affect processing time, the interesting question is how they are determined, and that leads us back to specific structural and incentive-related variables of the sort we employ.

The structural variables are of special interest, as they bear on the effectiveness of the reforms that produced the structural variation in Detroit and the potential of similar efforts elsewhere. For certain kinds of cases, at least, these all had substantial effects--the decentralization of plea-bargaining and crash program, unconditionally, and the individual docket and case-track, for cases going to trial and with pretrial motions, respectively. Of course, the other side of these interactions is that the effects of trial disposition and pretrial motions depend on docket type and case-track in turn. Post-innovations, a case's going to trial or involving pretrial motions still delayed it, but not nearly so much. The effect of caseload is similarly contingent, if to a lesser degree. Under the central docket, caseload has no discriminable effect, but under the individual docket it does, with increasing caseloads resulting in shorter processing times. Evidently, forcing the judges to be answerable for their own case-

load makes the system responsive to the length of the queue. Obviously, these results have policy implications. The structural innovations in Detroit are typical of the ways in which courts have attempted to reduce processing times. Courts have commonly tried to achieve tighter and earlier control over the progress of cases (the case-track), to hone the incentive structure by increasing individual accountability (the individual docket and decentralized plea-bargaining), to improve coordination (the crash program), and to increase the participants' familiarity with cases (decentralized plea bargaining again). The success of these reforms in Detroit should be encouraging to those making or proposing similar changes elsewhere.

To be sure, the encouragement depends on the similarity. Superficially similar innovations may be sufficiently different to deprive them of impact. Case-tracks may have varying deadlines for varying events with varying sanctions. Individual dockets may be more or less individual. Judges in some individual docket systems are rotated to other assignments and thereby relieved of their dockets periodically. There may also be more or less publicity given to docket size. Similarly, decentralized plea-bargaining may be more or less decentralized. Both the lattitude of local discretion and the ratio of cases to prosecutorial units may vary. And so on, and so on. The details of structural changes will matter. Other results also have policy implications. Constrained only by manipulability, every effect suggests a way of reducing (or increasing) processing times. And it is not just the structural variables that are manipulable, at least in the aggregate. For example, the cases of defendants with lengthier prior records seem to take longer, which means that police or prosecutorial policies--not charging or diverting more first-timers--that result in a caseload with a higher proportion of repeat offenders will increase the

average processing time. Similarly, consider the effect of a case's going to trial. Plainly, sentencing structures or plea-bargaining postures that lead to a higher proportion of trials should greatly decrease the pace at which cases are processed. Thus although plea-bargaining is now under some attack (in the public, if not in the courts) and may be undesirable in other respects, there would be a considerable cost in processing time if more cases were brought to trial. Another example is motions. Any rule or behavior that discourages pretrial motions should result in shorter processing times. Or, again, there is failure to appear. It may be possible, by being more efficient about reminding defendants of upcoming appearances or about apprehending those who do not appear, to shorten the period of defendant-less limbo, and thus the mean processing time.

These observations are not intended to be normative. We are merely examining some of the possibilities. We do not mean to recommend any of the changes that would reduce processing times or to reject any of the changes that would increase them. In either case, there may be tradeoffs that would outweigh the gain or loss in processing time. But we do wish to underline the fact that the effects of the nonstructural variables, too, suggest ways in which court policies and behaviors may have effects--either intentional or inadvertant--on case processing time.

At the same time, the operative word here is "suggest." Our results derive from only one court in only one period. Parameters and effects in other courts or in this court at other times may look quite different. Time and further study will tell. Where differences emerge, the task will be to educe the boundary conditions (implicit interactions) responsible. That, however, we may leave for future research. This model and these results lie near the beginning, not the end, of investigation.

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Notes

[1]See Church (1982) and to a lesser extent, Grossman, et al., (1981) for dissenting views.

[2]For a now slightly dated review of the literature, stressing the need for a more theoretical and systematic approach, see Luskin (1978).

[3]Operationally, we consider processing time as beginning with the defendant's initial appearance in court and ending with the disposition by dismissal, plea, or verdict. Since Recorder's Court has preliminary as well as trial jurisdiction, a defendant's initial appearance is at an arraignment on the arrest warrant, which occurs within twenty-four hours of the arrest.

[4]On a priori grounds, we consider this possible, if not probable, despite its not seeming to have occurred in a number of studies based on aggregate data (Church, et al., 1978; Nimmer, 1978; Gillespie, 1976). See the argument below.

[5]In some court systems, this would be a dismissal in a lower court, but as noted above, Recorder's Court has jurisdiction over preliminary matters in felony cases.

[6]It should also be noted that Church et al. (1978) and Flanders (1977) are bivariate, while Gillespie (1976) has productivity, not processing time as its dependent variable.

[7]We do not distinguish post-preliminary-examination dismissals from pleas, despite the former's oft-observed correlation with processing time. Dismissals at this stage are not a cause but either a spurious correlate or

a consequence of longer processing times. When the evidence is weak yet there is public or police demand for action, the prosecution may have reason to prolong the case until the heat is off, then seek dismissal so as not to waste resources in a hopeless cause. At the same time, the longer the case drags on, the weaker it tends to become and the more likely, therefore, to be dismissed. Indeed, the defense often behaves on precisely this premise, with a dismissal often the result of a successful use of "strategic delay."

[8]It may be thought that this and "failure to appear" (below) should not be included as explanatory variables but instead subtracted from processing time prior to analysis (as in Neubauer and Ryan, 1982). After all, the argument runs, they represent time not under the court's control, and for which the court should not be held responsible. The trouble with this approach is, first, that, whether under the court's control or not, the time involved is still part of processing time, and, second, that the effects on processing time may not be unity.

[9]See Hausner and Seidel (1979) for empirical support. Their equation differs from ours, however, in not separating out motions, failures to appear, and psychiatric evaluations.

[10]Alternatives would include a Sellin-Wolfgang (1964) or Sellin-Wolfgang-like measure based on the amount and kind of harm involved together with the nature of the relationship between defendant and the victim (as in, e.g., Forst and Brossi, 1977; Bernstein et al., 1977; and Jacoby et al., 1982) or a measure based on the severity of the penalties actually imposed (as in McDavid and Stipak, 1981). But the statutory maximum penalty is preferable for our purposes. First, it has the practical advantage of being readily obtainable from the court records. Second,

it is, in effect, the legal ordering of seriousness, and as such presumably carries some normative weight, for court personnel especially (McCleary et al., 1981). Third, it probably reflects the central tendency of societal perceptions of seriousness, as Rossi et al. (1974) would seem to suggest. And, fourth, and perhaps most important, it is a direct measure of the defendant's maximum risk, which is one of the aspects of seriousness that presumably matters most to processing time.

[11]Recorder's Court has shifted back and forth between central and individual calendars. In late 1975, it changed the docket from individual to central; in late 1976 (during our study) it changed it back. See Neubauer, et al, 1981 for an account of these changes and Eisenstein and Jacob, 1977, on prior oscillations.

[12]Among other variables, we exclude the defendant's age and sex, the type of crime, and the judge's sentencing severity. None affects processing time with the same immediacy as the variables we have included. They may affect it, but if so, only, we contend, through the variables listed (perhaps most plausibly through disposition type).

[13]Nonmathematically, the effect that an explanatory variable x has on a dependent variable is the amount of change induced in y, on the average and other things being equal, for each unit of change in x. For example, the average caseload's effect on case processing time is the number of additional days a case can be expected to take for each additional case-per-judge when ~~it~~ enters the court.

[14]Since the log of 0 is undefined, we add .01 to the count of prior convictions, taking the log of (PRIOR + .01).

[15]The current caseload is affected, with some lag, by the average case processing time. But the effect is lagged, and only in the mean; the processing times of contemporaneous cases have no effect, and that of any

single case in the past practically none. Consequently, we need not specify and estimate a caseload equation alongside the case processing time equation.

[16]The reader may note that the data form an "unaggregated" time-series, with cases implicitly indexed by month of initiation. By averaging within months, we could reduce them to a mere time-series, but we should be the poorer for it, having thrown away our case-level information and increased our standard errors without compensatory gain.

[17]Since collinearity appears to be something of a bugbear, in this field, it may be worth mentioning that it is not a problem here. Only extreme collinearity does much damage, and the only damage it can do is to produce large standard errors, which in turn mean wide confidence intervals and the possibility of substantively but not statistically significant estimates. Even then, the estimates stay asymptotically on target, i.e. "consistent." Here, as we acknowledge in the text a few of the substantively large parameter estimates (for mistrials, repetitions of preliminary examinations, and the docket x trial interaction) are statistically insignificant. But the cause is lack of variance, not collinearity.

[18]The R^2 is computed as the squared bivariate correlation between actual and GLS-predicted processing time. (See Luskin, 1984, for a discussion of R^2's for non-OLS estimators.)

[19]In an analysis of earlier Recorder's Court data, Eisenstein and Jacob (1977, pp. 234-37) achieve an R^2 of only .11. On data from Dayton, Las Vegas, and Providence, Neubauer and Ryan (1982) obtain R^2's of .11, subset of the Las Vegas data, but the standard adjustment for degrees of freedom brings this down beneath .3 (our calculation). In Washington, D.C., Hausner and Seidel obtain an R^2 of .32.

[20]For a parallel finding in the case of U.S. District Courts, whose dockets are individual, see Gillespie (1976).

[21]To be fair, Church (1982) admits the fuzziness of local legal culture explanations and calls for clarification. He also suggests--and we agree--that if local legal culture is to be more than a catch-all or residual category it must consist of norms and expectations. Where we disagree is over the role and importance of local legal culture, so defined.

[22]This is Church et al.'s reading which is premised on the exclusion of two master calendar courts with particularly lengthy median processing times. If they remain in the analysis the master calendar courts average 25 percent longer median disposition times than the individual calendar courts (see Church et al., 1978, pp. 36-39).

CASE PROCESSING TIMES IN THREE COURTS

That law trial was a slow business--appeared like
they weren't ever going to get started at it.

Mark Twain, Huckleberry Finn

Among the most important dimensions of court performance is what is increasingly known as case processing time--simply the length of time it takes a court to process a case, from arrival to disposition. At the level of the individual case, processing time clearly affects the hardship on the defendant and his family and the cost and inconvenience to witnesses. It may also have a curvilinear effect on the quality of "justice" dispensed, with either haste or delay beyond some reasonable limit making "just" outcomes more difficult to obtain. Certainly, it affects the probability of conviction, since the prosecution's case tends to decay over time. In the aggregate, therefore, the distribution of processing times has consequences for the level of public safety and the deterrent value of legal sanctions. More directly, it also affects the preemption of jail space and other resources by pending cases.

As the foregoing implies, case processing times vary--from court to court, over time and circumstance within a given court, and from case to case. The main question is, how? What factors affect case processing times, under what conditions, and to what degree? The question of "reform" is corollary. The common perception is that processing times in American courts are generally too long, and there have been numerous efforts to reduce them. But to judge what has actually worked, and what is likely to

work, and to what degree, we must first define the full set of variables that affect processing times and precisely how they do so and then estimate their effects.

To this end, we have previously advanced and estimated a single-equation model to account for the processing times of individual felony cases in the state criminal court of Detroit (Luskin and Luskin, 1984). The model is encouragingly predictive and the results enlightening, but they still pertain to only one court. In this paper, we extend the analysis to similar data on the state criminal courts of Dayton and Providence. The object is both replicative and comparative--to see how far the results from Detroit can be generalized, how the effects on processing time differ between courts, and what contextual factors can account for those differences.

The Variable and the Data

Definitions of case processing time have varied considerably (as Church et al., 1978b; Luskin, 1978; and Cook et al., 1981; who review them, point out). For our part, we think it important to consider the processing times of individual cases, not just of cases in the aggregate. The interval with which we are concerned is simply the number of days from receipt to disposition. In Detroit, where the court has preliminary as well as trial jurisdiction, we count a case as received when it is first arraigned ("on the warrant"); in Providence and Dayton, where the courts we study have trial jurisdiction only, we count it as received only when the indictment is filed. In all three, we consider it disposed of when the question of guilt or innocence has been decided.. This end-point excludes the time, if any, consumed by sentencing, appeals, new trials that result from appeals, proceedings due to violations of probation, and so forth. On the

other hand, it maximizes comparability across cases because all cases must eventuate in a dismissal, plea, or verdict, while only some will require further action.

The data consist of samples of criminal cases initiated over more or less contemporaneous two- to three-year periods in the late 1970's. The reason the data were originally collected, and what makes these courts particularly worth studying during these periods, is that they were the sites of LEAA-sponsored programs to reduce processing times, that wrought significant changes in court structures and practices. These changes provided an unusually good opportunity to study the influences of court-structural as well as case-specific variables.[1]

The Model

Explanatory Variables

As we have previously argued (Luskin and Luskin, 1984, pp. 4-6), the variables that affect case processing time directly and thus belong on the righthand side of its equation should all be aspects of one or more of five overarching factors:

> I. Caseload. The conventional premise is that over any fixed period of time, court personnel have a limited number of manhours at their disposal and that any given case requires a certain minimum number of manhours to be processed. If so, the more cases there are to compete for available manhours, the longer it should take, on average, for any one of them to receive its necessary minimum. Granted, courts may process cases more or less efficiently, disposing of a greater or lesser number in a given amount of time, even with constant resources (see Gillespie, 1976). But ceteris paribus--that is, controlling among other things for the factors that govern efficiency, which come under III and V below--one might well suppose that increasing caseloads lengthen processing times. On the other hand, a court's most basic task is simply to dispose of cases, and from this perspective caseload is a criterion of court performance. Concommitantly, it is a source of pressure, with judges and prosecutors preferring to keep it within reasonable bounds for fear of embarrassment or electoral displeasure (Eisenstein and

Jacob, 1977; Heumann, 1979; Nardulli, 1978; Nardulli, 1979). Hence judges and prosecutors may process cases more, not less, expeditiously as caseload rises. Either way, case processing time should depend on caseload. The only question is one of sign.

II. Case Complexity. Some cases are almost inherently more time-absorbing than others. In state criminal courts such as this, there may actually be little variation under this heading, as Neubauer and Ryan (1982) suggest, but there is probably some. For example, cases with larger numbers of defendants can be expected to take longer owing to the necessity of coordination.

III. Incentives. Court participants--defendants and their counsel, prosecutors, judges, and others--are semi-independent decision-makers with particular institutionally-defined options and associated reinforcement contingencies. Like the rest of us, they respond to economic (Blumberg, 1967; Nardulli, 1978; Eisenstein and Jacob, 1977; Feeley, 1979), social (Eisenstein and Jacob, 1977; Mather, 1979; Flemming, 1979; and Nardulli, 1978), intellectual (Heumann, 1978), and professional (Heumann and Loftin, 1979; Loftin et al., 1983; and Eisenstein and Jacob, 1977) incentives to take certain actions and avoid others. Some of the incentives relevant to processing time are case-specific. Other things being equal, a jailed defendant has a greater interest in speed than one who is free on bail. Others derive from the court's structural and administrative arrangements. For example, an individual as opposed to central docket or calendaring system may make judges more eager to keep the docket uncluttered.

IV. Case Events. As the case unfolds, participants make their decisions, taking some actions and not others, and what they choose to do often shortens of lengthens the processing time, either by design or as a side-effect. Defendants fail to appear, prosecutors choose to bargain, judges grant continuances, and so on.

V. Structural Facilitation. A court's administrative arrangements may be such as to facilitate or retard the movement of cases, quite apart from their effects on the motivations of court participants. Arrangements of this sort include scheduling procedures and the assignment of cases to courtrooms.

Admittedly, the variables under these headings are not entirely the same from court to court. Because different courts are differently organized and function in different environments, a variable that matters in one court may not even be definable in another. In a given court at a given time, some of the variables that are influential in other courts or

at other times (or from court to court) are not variables but constants. These three courts are no exception. In each, the variables that affect processing time are only a proper subset of those that affect it in one court or another, at one time or another. This is particularly true of the structural variables, on which we are inevitably able to observe variation in only a few. Nor are the operative variables precisely the same in all three. Indeed, most of the structural variations we observe are confined to only one court.

Correspondingly, the case processing time equation cannot be precisely the same for all courts, nor specifically for these three. Not that this is a troublesome disjuncture. Each of our three court-specific equations is a special case of the single, more general equation containing all the explanatory variables appearing in any one of the three. (Similarly, this more general equation is itself a special case of the single, <u>most</u> general equation, containing all the explanatory variables that belong in the equation anywhere, at any time.) In each court-specific equation certain of the variables of the more general equation happen to equal 0 or some other constant, and so drop out of the picture entirely or are absorbed by the intercept term. Any two court-specific equations can thus be rendered comparable by substituting the appropriate constants for the variables excluded by either but not both.

It is plainly best--most compact, not to mention most theoretically satisfying--to expound the model in this more general, any-one-of-the-three-courts version. Hence we begin by listing the variables we take to be aspects of at least one of the five generic factors in at least one but not necessarily all three of the courts. Roman numerals in parentheses indicate the factor(s) each variable represents.

Number of Defendants (II). More numerous defendants can be expected to exarcerbate the problems of scheduling and coordination and thus to lead to longer processing times.

Disposition Type (IV). Because they are more complex, trials both require lengthier preparation and are themselves more time-consuming than other dispositions (see Nimmer, 1978; Rhodes, 1978; and Wice, 1978). On the other hand, cases dismissed at the preliminary examination (held within the court in Detroit) are obviously less so.

Pretrial Motions (IV). Like trials, pretrial motions require preparation, and both the preparation and the actual hearing take time. Because the hearing of plural motions can be consolidated, it is the fact of at least one motion having been filed, not the number,that should chiefly matter. Indeed, we get distinctly lower R^2's (but otherwise almost identical results) when we substitute the number of motions for the simple yes-no variable.[2]

Psychiatric Evaluation or Treatment (IV). In some cases, there must be a determination of competency to stand trial, a more general psychiatric evaluation pursuant to an insanity defense, or, where the defendant is found incompetent, a period of treatment prior to trial. Days lost in this fashion obviously add to the processing time, although the translation is not necessarily one-to-one, since associated inefficiencies may make the effect greater than 1, while efforts to make up for lost time may make it less.

Failure to Appear (IV). When the defendant does not appear for a scheduled event, the court must wait until he does (voluntarily or otherwise) before proceeding. Again, the variable is the number of days lost in this fashion, and again the coefficient may be but is not necessarily 1.

Continuances (IV). Every continuance granted requires a rescheduling

to a later date agreeable to a number of participants and thus prolongs the case. To avoid double-counting, we exclude continuances associated with motions, psychiatric evaluations, and failures to appear, itemized separately in the variables above.

Repetition of Preliminary Examination (IV). Cases do not always move unidirectionally toward completion. In Detroit, where the preliminary examination is held within the court, post-preliminary examination cases occasionally return to that stage. In some cases, the actual examination is first waived, then belatedly requested; in others, it is held but later ruled deficient. Either way, the regression costs processing time.

Mistrial (IV). When a trial ends in a mistrial, even more work and time will be necessary to reach a conclusion. This variable enters the equation only for Detroit, since the Providence and Dayton samples contain no mistrials.

Attorney Type (III). Several authors have argued that privately retained attorneys tend to keep cases simmering long enough to make sure of collecting their fees (Blumberg, 1967; Nimmer, 1978; and Nardulli, 1978, for example). Apart from that, retained attorneys are presumably more independent than public defenders or court-appointed counselors and less inclined to forgo the strategic advantages of delay for the sake of obliging other participants with a greater interest in speed. Under either hypotheses, cases in which the defense attorney is privately retained can be expected to take longer.

Pretrial Incarceration (III). The strategic advantages of delay will be more persuasive to defendants who do not have to trade them off against lengthier pre-trial incarceration (see Nimmer, 1978, and Nardulli, 1978; also Bernstein, et al., 1977, Rhodes, 1976, Rosset and Cressey, 1976). In

addition, courts generally give priority to the cases of jailed defendants (Neubauer and Ryan, 1982; Nardulli, 1978; Thomas, 1976; Fleming, 1979).

Seriousness (III). Defendants charged with more serious crimes risk greater penalties if convicted and should be that much more interested in lowering the odds of conviction by delay. Besides, court personnel may be willing to devote more time to more serious cases (Mather, 1979; Heumann, 1978; Forst and Brosi, 1977).

Prior Record (III). By the same logic, defendants with a lengthier prior record (a greater number of past felony convictions), who likewise risk a heavier sentence if convicted, can be expected to make greater efforts to slow their cases.

Regular or Visiting Judge (III). Even under an individual docket (see below), judges who are merely visiting do not have their "own" standing dockets and thus face few if any of the regulars' incentives to hold their caseloads down. It follows that the effect of the judge's caseload (again see below) should be contingent on the judge's being a regular.

Docket Type (III or V). The docket or calendar of cases pending may be either "central" or "master" (a single docket for the entire court) or "individual" (one for each judge). Some have argued that the central docket permits a more efficient allocation of resources and thus minimizes processing times. Others have argued that it is instead the individual docket that by making judges more specifically accountable results in shorter cases.[3] We ourselves are inclined to the latter view. But in either event, there should be an effect. The only question is one of direction, and the answer depends on whether the variable is predominantly of Type III or Type V. In these data, docket type varies only in Detroit, which changed from a central to an individual docket. In Providence the docket was central, in Dayton individual.

Case-Track (III). A "case-track" is simply a schedule: a series of deadlines for each of the major events in the life of a case. All three courts introduced a case-track during the period of study, although the details varied. In Providence, the goal was to complete cases within 180 days, while Detroit and Dayton aimed for only half as long. Also in Detroit and Dayton but not in Providence, the track incorporated a plea cut-off date, after which no further concessions could be offered in return for a guilty plea. The Detroit court made the greatest effort to persuade its judges, by both carrot and stick, to keep their cases on track. In all three, however, the sanctions were informal. A judge whose cases were behind schedule would typically receive a talking-to from the chief judge.

Because Dayton introduced the case-track at the same time as it changed to a more centralized system of plea bargaining (see below), it is impossible to disentangle the effects of these variables there. Still, the consensual perception of court participants was that the centralization of plea bargaining had little effect, and that what effect it had was to decrease the efficiency of bargaining and thus to increase processing time. This is consistent with our earlier results in Detroit (Luskin and Luskin, 1984, and below), where we find that the decentralization of plea bargaining decreased processing times. Consequently, our estimate of the effect of the case-track in Dayton should be conservative, depressed as probably it is by the countervailing effect of the centralization of plea bargaining.

Declaration of Goal (III). This is rather like the case-track, only less so. During the period of observation, the Judicial Planning Commission of Rhode Island proclaimed a 180-day goal for the completion of criminal cases. There were no intermediate deadlines and no real sanctions. Even so, the establishment of an explicit norm may in itself provide some

mild incentive to keep processing times somewhere near it.

Court Control of Scheduling (V). The literature has repeatedly emphasized that a court can only control processing times and docket size by exercising active and effective control over scheduling (among others, Church et al., 1978; Sipes et al., 1980; Flanders, 1977). The case-tracks and, in a feebler way, the declaration of goal in Providence were efforts in this direction. But as a minimum condition a court cannot exercise control without the authority to do so. And although the Detroit and Dayton courts had scheduling authority throughout, the Providence court did not. Initially, there, control over scheduling was officially shared with and de facto belonged to the prosecutor's office (Neubauer et al., 1981). Later, it was transferred to the state court administrator's office, and then finally to the local court administrator. Since these two changes brought control over scheduling sequentially closer to where it figured to do the most good, we should expect both to have reduced processing times.

Statistics (III). This is a variable in Providence, where the court did not keep track of caseloads or processing times prior to September, 1976. The argument here is that feedback can be motivating. There is little urgency about keeping processing times as short as possible when there is little way of knowing of how well or poorly one is doing.

Administrative Judge (V). Supreme administrative power--to superintend the scheduling of cases, approve continuances, and so forth--may or may not be vested in a single "administrative judge" (who may or may not also be the chief judge). This may arguably have some effect on efficiency, and therefore on processing times. We can only assess the effect in Providence, which gained an administrative judge partway through the period of study. Detroit also acquired an administrative judge during the study,

but at the same time as the Crash program, from which the effect is therefore inseparable. Dayton had an administrative judge throughout.

Centralization of Plea Bargaining (III, V). The negotiation of pleas may be conducted either centrally, in the prosecutor's office, or locally, by prosecutors assigned to specific sets of courtrooms. In Detroit, the court made the switch from central to local, on the theory that the local prosecutors--"docket prosecutors," as they were called--would be more familiar with their cases and able to negotiate pleas more expeditiously. Moreover, the more definite and narrower assignment of responsibility increased the incentive (within other contraints, of course) to keep processing times short. On the other hand, the decentralization was accompanied by an increase in the number of prosecutors, and that too may have had an effect.

In Dayton, the court made the opposite change: from decentralized to centralized plea bargaining. Anecdotal evidence suggests that the effect on case processing time was small (Neubauer et al., 1981). It also suggests that what effect there was was negative, because the parties were less familiar with the cases and less prepared to negotiate at scheduled conferences. Unfortunately this is as much as we can say about the effect of centralization in Dayton. Since it was simultaneous with the case-track, there can be no separate estimation of its effect.

The Crash Program (I, III, V). This was an emergency program unique to Detroit, although one or two elements were also part of the Push program in Providence. One object was to clear the docket of the most antediluvian cases. The program thus reopened plea negotiations in older cases and augmented the bench with visiting judges to hear them (and some current cases as well). In addition, the program provided for a special judicial administrator with extraordinary powers, set up a system to monitor the

hours each courtroom was in session, and instituted daily meetings of the delay reduction project staff, judges, prosecuters, and representatives of the police department and sheriff's office. Unfortunately, these innovations were all introduced more or less at once, making it impossible to distinguish their individual effects. We can only estimate the impact of the package as a whole.

The Push Program (I). This too was a temporary, intensive program to purge the docket of its oldest cases. As such, it resembled aspects of the Crash program in Detroit. As in Detroit, there was a reopening of plea negotiations in selected older cases and a concentration of effort on them. But in Providence that was all there was to it. Most importantly, perhaps, there was no infusion of visiting judges. Since the allocation of effort between older and younger cases was thus zero-sum, we should expect the processing of cases initiated during the program to have been lengthened rather than shortened.

Caseload, Individual and Average (I). As the discussion under Docket Type implies, this is not really one variable but two: the caseload of the judge responsible for the case and the average caseload of all the court's judges. The first is what matters under the individual docket, the second what matters under the central docket. Of course, the general equation we are talking about contains both, each switched on or off by docket type (and other variables; see below).

These, to repeat, are the variables we take to affect processing time "directly." Other variables may also affect it but only through them. For example, the defendant's age, sex, and race may affect processing time through their effects on pretrial release and disposition type. Such indirect effects can be modeled with additional equations, but given recur-

siveness,[4] we may safely ignore them. More worrisome, but unavoidable, is the omission of some variables that probably do affect processing time directly. There is, for example, the strength of the evidence against the defendant, which may have some motivational effect. Another possibility is the number of lay witnesses, an indication of complexity. We would include these variables if we could, but the data are not there.

The measurement of the variables included is mostly straightforward. The dichotomies are scored (0,1), the rest in their natural metrics--days, numbers of defendants, prior felony convictions, etc. The only exception is seriousness, which we operationlize as the statutory maximum term of imprisonment (in months) for the original charge. (For justification, see Luskin and Luskin, 1984.) In the event of multiple charges, we choose the one that carries the highest maximum, and where the maximum is "life" we arbitrarily assign a value of 480 months. We score the court-level variables as of the date the individual case enters the processing stream. The scorings are summarized, along with the resulting minima, means, maxima, and standard deviations, in Table 1.

Table 1 about here

A Question of Form

In the absence of compelling reason to the contrary, we take the effects on processing time to be linear and additive, which is to say constant.[5] Nevertheless, a few seem likely to vary. For example, the effect of going to trial should be smaller under the individual docket, because judges, who are then motivated to keep processing time to a minimum, have more control over trials than over most other elements of cases.

Table 1

Variables in the Model

Variable	Abbreviation	Operationalized as	Min*	Max*	Mean*	S.D.*
Case processing time	CPT	Days between arrival (arraignment on warrant or indictment and disposition	1	774	76.28	94.13
			1	1362	263.88	287.534
			1	910	96.41	110.51
Number of defendants	#DEF	Number of defendants	1	8	1.21	0.56
			1	3	1.19	0.49
			1	4	1.14	0.44
Dismissal at or before the Preliminary Examination	DPE	1 When case is dismissed at or before preliminary examination	0	1	0.19	0.39
		0 Otherwise	X	X	X	X
			X	X	X	X
Trial	TRIAL	1 When case is disposed by trial	0	1	0.11	0.31
		0 Otherwise	0	1	0.05	0.21
			0	1	0.05	0.22
Pretrial motions	MOTION	1 When formal pretrial motions are filed	0	1	0.15	0.36
		0 No pretrial motions	0	1	0.41	0.49
			0	1	0.39	0.49
Psychiatric evaluation or treatment	PSYCH	Days lost to psychiatric evaluation or treatment	0	98	0.87	7.85
			0	516	1.27	19.14
			0	413	2.66	25.21
Defendant's failure to appear	FTA	Days lost due to defendant's failure to appear	0	605	5.82	37.09
			0	1001	25.72	95.30
			0	206	1.11	11.39
Number of continuances	CONTIN	Number of continuances	0	8	0.14	0.54
			0	9	0.69	1.14
			0	6	0.21	0.55

Variable	Abbreviation	Operationalized as	Min*	Max*	Mean*	S.D.*
Repetition of preliminary examination stage	RPE	1 When case sent back for preliminary exam 0 Otherwise	0 X X	2 X X	0.02 X X	0.16 X X
Mistrial	MISTR	1 When mistrial declared 0 Otherwise	0 X X	1 X X	0.002 X X	0.05 X X
Type of defense attorney	RETAIN	1 when attorney is retained private counsel 0 Otherwise	0 0 0	1 1 1	0.18 0.52 0.48	0.39 0.50 0.50
Pretial incarceration	BAIL	1 When defendant is free prior to the disposition of the case 0 Jail	0 0 0	1 1 1	0.66 0.27 0.71	0.47 0.45 0.45
Seriousness of charge	SERIOUS	Statutory maximum of count with highest maximum in months	0 0 1	480 480 480	150.47 149.38 120.75	151.97 135.78 103.00
Defendant's prior record	PRIOR	Number of prior convictions	0 0 0	62 7 30	2.02 1.24 0.83	3.87 1.57 2.37
Regular vs. visiting	REGJ	1 When case was heard by regular judge 0 When case was heard by visiting judge	0 X 0	1 X 1	0.77 X 0.90	0.42 X 0.30
Docket type	DOCK	1 When case was initiated under individual docket 0 Under central docket	0 X X	1 X X	0.73 X X	0.44 X X
Case-track	TRACK	1 When case was initiated after case-track in place 0 Before case-track	0 0 0	1 1 1	0.53 0.27 0.48	0.50 0.44 0.50

Variable	Abbreviation	Operationalized as	Min*	Max*	Mean*	S.D.*
Adoption of 180 day goal	GOAL	1. When case was initiated before 180 day goal	X	X	X	X
		0 Before 180 day goal	0	1	0.48	0.50
			X	X	X	X
Control of scheduling: state court administrator	STSCHED	1 When case was initiated under state court administrator control of scheduling	X	X	X	X
		0 Otherwise	0	1	0.47	0.50
			X	X	X	X
Court performance statistics	STATS	1 When case was initiated after court performance statistics kept	X	X	X	X
		0 Before court performance statistics kept	0	1	0.79	0.41
			X	X	X	X
Administrative judge	ADMINJ	1 When case was initiated after administrative judge appointed	X	X	X	X
		0 Before administrative judge appointed	0	1	0.43	0.50
			X	X	X	X
Decentralization of plea	LOCPLEA	1 When case was initiated after plea bargaining decentralized	0	1	0.18	0.39
		0 Before decentralized plea bargaining	X	X	X	X
			X	X	X	X
Crash program	CRASH	1 when case was initiated under the crash program	0	1	0.24	0.43
		0 Otherwise	X	X	X	X
			X	X	X	X
Push program	PUSH	1 When case initiated under the push program	X	X	X	X
		0 Otherwise	0	1	0.04	0.20
			X	X	X	X
Judge's caseload	JLOAD	Number of defendants on disposition judge's individual docket in month in which case was initiated	0	399	75.215	93.186
			X	X	X	X
			19	73	45.60	9.92

Variable	Abbreviation	Operationalized as	Min*	Max*	Mean*	S.D.*
Average caseload of the court	CLOAD	Total number of defendants before court at beginning of month in which case was initiated, divided by the number of judges available	103.10 468.00 X	423.42 3191.00 X	224.95 672.87 X	39.07 343.72 X

*In each cell, the first figure reading down is for Detroit, the second for Providence, and the third for Dayton.

It should also be smaller under the case-track, which establishes a deadline for the holding of trials. Similarly, the case-track should reduce the effect of motions, since it also establishes a deadline for them. Again, the individual judge's caseload is defined only under the individual docket, and can have an effect only then, while the average caseload of the court should be relevant only under the central docket. Moreover, the individual caseload is defined only for non-visiting judges and can have an effect only for them. And the effects of both individual and average caseload should be conditional on the case's having passed the preliminary examination and the court's providing caseload statistics. Pre-preliminary examination, a case is not yet officially part of the caseload, and unless judges know the caseload, they cannot respond to it. The equation for processing time can accommodate these nonadditivities by including the appropriate products: TRACK*TRIAL, DOCK*TRIAL, TRACK*MOTIONS, DOCK*JLOAD*REGJ*STATS*(1-DPE), and (1-DOCK)*AVLOAD*STATS*(1-DPE).

We also anticipate one nonlinearity. The impact of prior record should diminish with increasing numbers of convictions. The biggest difference should be between having a prior record and not having one. Having 2 previous convictions versus 1 should matter less, 3 versus 2 still less, and so on. The difference between, say, 6 and 7 convictions should be trivial. The simplest and most convenient way of constraining the effect to follow this pattern is to make case processing time a linear function of the logarithm of prior record.[6]

Together, these propositions lead to the following model:

$$\begin{aligned} CPT = {} & B_0 + B_1\#DEF + B_2TRIAL + B_3DPE + B_4MOTION + B_5PSYCH + B_6FTA \\ & + B_7CONTIN + B_8MISTR + B_9RPE + B_{10}RETAIN + B_{11}BAIL \\ & + B_{12}SERIOUS + B_{13}PRIOR + B_{14}DOCK + B_{15}TRACK + B_{16}GOAL \end{aligned}$$

$$\begin{aligned} & + B_{17}STSCHED + B_{18}LOCSHED + B_{19}STATS + B_{20}ADMINJ \\ & + B_{21}LOCPLEA + B_{22}CRASH + B_2PUSH \\ & + B_{24}DOCK*TRIAL + B_{25}TRACK*TRIAL + B_{26}TRACK*MOTION \\ & + B_{27}AVLOAD*(1-DOCK)*STATS*(1-DPE) \\ & + B_{28}JLOAD*DOCK*REGJ*STATS*(1-DPE) + u, \end{aligned}$$

where the variable abbreviations and definitions are as in Table 1, u is an unmeasured disturbance, and the B's are the unknown parameters on which the effects depend.

In Detroit, GOAL=STSCHED=PUSH=0 and LOCSHED=STATS=1 for all cases, with the result that these variables and their coefficients disappear, the first three without a trace, the other two into the intercept, which in Detroit becomes $A_{De}=B_0+B_{18}+B_{19}$. (Note that STATS=1 simply vanishes from the AVLOAD and JLOAD terms.) We also delete ADMINJ, not distinguishing its effect from that of CRASH, of which it is an element. In Providence, DPE=MISTR=RPE=DOCK=LOCPLEA=CRASH=0 and REGJ=1, and all these variables drop out of the equation entirely (even REGJ, which=1, since it appears only in the product term for JLOAD). And in Dayton, DPE=MISTR=RPE=GOAL=STSCHED=CRASH=PUSH=0 and DOCK=LOCSCHED=STATS=ADMINJ=REGJ=1. Again REGJ and the variables that equal 0 drop out entirely, while the only vestige of the rest is in the intercept, which becomes $A_{Da} = B_0+B_{18}+B_{19}+B_{20}+B_{25}$. In addition we delete LOCPLEA, not distinguishing its effect from the presumably larger one of TRACK, with which it co-occurs.

Estimation

We need not pretend that the variance of u is constant over time (or across courts). All three courts saw innovations that drastically reduced both processing times and the variance therein, which raises the suspicion

that the variance of the disturbance declined accordingly. In fact, a homogeneity of variance test (Mood et al., 1974, pp. 439-40, e.g.) shows significantly different variances by time-period (^{2} = 2006.8 with 23 df in Detroit, 48.2 with 1 df in Dayton, and 272.0 with 3 df in Providence).[7] Hence we estimate the equation by a form of generalized least squares (GLS) that weights each observation by the reciprocal of the standard deviation of the estimated disturbance in that time-period. Under the usual assumptions, the estimates are consistent and asymptotically efficient.

Results

The GLS parameter estimates and R^2 are displayed in Table 2.[8] In the main, the results are consistent with expectation. The R^2's are .47 for Detroit, .28 for Dayton, and .38 for Providence. These represent a very satisfactory level of explanation on individual-level data. At least in Detroit the fares substantially better than previous efforts to account for processing time. especially high in comparison with previous R^2's for processing time. The majority of the parameter estimates are significant and of the right sign.

Table 2 about here

Note that the parameters imply but are not always the same as the effects. The linear, additive effects are simply the corresponding parameters, but the nonlinear or nonadditive ones are more elaborate functions of the parameters and explanatory variables. Hence Table 3 presents estimates of the effects. Let us consider the variables in turn.

Table 3 about here

Table 2

Estimates of Coefficients

Variable	Coefficient* (Standard Error)		
	Detroit	Providence	Dayton
#DEF	6.77* (2.32)	-9.08 (11.70)	-0.12 (7.59)
TRIAL	68.99* (14.57)	171.78* (42.66)	38.35 (24.97)
DPE	-31.04* (4.43)		
MOTION	53.70* (7.99)	124.77* (18.66)	23.29* (10.06)
PSYCH	1.39* (0.18)	0.29 (0.86)	0.82* (0.14)
FTA	1.05* (0.04)	0.77* (0.07)	1.17* (0.23)
CONTIN	17.46* (3.07)	12.85* (4.53)	29.28* (6.10)
RPE	12.87 (10.41)		
MISTR	23.87 (26.10)		
RETAIN	-1.18 (3.27)	13.25 (11.58)	11.25* (6.43)

Table 2

Estimated of Coefficients

Variable	Coefficient* (Standard Error) Detroit	Providence	Dayton
BAIL	9.83* (2.98)	37.41* (13.55)	13.50* (7.37)
SERIOUS	0.06* (0.01)	-0.02 (0.04)	-0.02 (0.03)
PRIOR	0.86* (0.45)	-7.19* (2.10)	-2.03 (1.35)
DOCK	-1.88 (14.70)		
TRACK	-9.13* (4.76)	-10.78 (22.91)	-27.17* (8.38)
GOAL		-128.09* (37.68)	
STSCHED		-15.98 (31.92)	
LOSCHED		-45.38 (35.61)	
STATS		-182.37* (37.85)	
ADMINJ		31.46 (35.28)	

Table 2

Estimates of Coefficients

Variable	Coefficient* (Standard Error)		
	Detroit	Providence	Dayton
LOCPLEA	-13.68*		
	(3.18)		
CRASH	-18.58*		
	(8.99)		
PUSH		99.50*	
		(37.35)	
DOCK*TRIAL	-15.14		
	(17.94)		
TRACK*TRIAL	-0.06	-183.83*	
	(11.89)	(55.12)	
TRACK*MOTION	-33.87*	-100.69*	
	(9.04)	(23.28)	
AVLOAD	-0.03	0.06*	
	(0.04)	(0.02)	
JLOAD	-0.08		0.06
	(0.02)		(0.27)
CONSTANT	58.45*	324.49*	56.33*
	(12.88)	(29.74)	(18.33)
N	1233.00	801.00	417.00
R^2	.4716	.3	.2

*Starred estimates are significant at the .05 level for a one- or two-trial test, as appropriate.

Table 3

Estimated Effects on Case Processing Time

Variable	Detroit (S.E.)	Providence (S.E.)	Dayton (S.E.)
Number of Defendants	6.77 (2.32)	-9.08 (11.70)	-0.12 (7.59)
Dismissal at Preliminary Examination			
Central Docket	-31.04-.03 AVLOAD[a] (4.43)		
Individual Docket with Regular Judge	-31.04+.08 JLOAD[b] (4.43)		
Individual Docket with Visitor	-31.04 (4.43)		
Trial			
Central Docket	68.99 (14.57)	171.78 (42.66)	
Individual Docket	53.85 (11.04)		38.35 (24.97)
Individual Docket and Case-track	53.79 (18.68)		28.01 (18.33)
Central Docket and Case-track		-12.05 (35.56)	
Pretrial Motions			
Before Case-track	53.70 (7.99)	124.77 (18.66)	23.29 (10.06)
After Case-track	19.83 (4.30)	24.08 (14.43)	15.56 (8.94)

Table 3

Estimated Effects on Case Processing Time

Variable	Detroit (S.E.)	Providence (S.E.)	Dayton (S.E.)
Psychiatric Evaluation	1.39 (0.18)	0.29 (0.86)	0.82 (0.14)
Failure to Appear	1.05 (0.04)	0.77 (0.07)	1.17 (0.23)
Number of Continuances	17.46 (3.04)	12.85 (4.53)	29.28 (6.10)
Repetition of Preliminary Examination	12.87 (10.41)		
Mistrial	23.87 (26.10)		
Retained Attorney	-1.18 (3.27)	13.25 (11.58)	11.25 (6.43)
Pretrial Release	9.83 (2.98)	37.41 (13.55)	13.50 (7.37)
Seriousness of Charge	0.06 (0.01)	-0.02 (0.04)	-0.02 (0.03)
Log of Prior Record	0.86 (0.45)	-7.19 (2.10)	-2.03 (1.35)

Table 3

Estimated Effects on Case Processing Time

Variable	Detroit (S.E.)	Providence (S.E.)	Dayton (S.E.)
Docket Type			
Dismissal at Preliminary Examination	-1.88 (14.57)		
Non-trial Disposition with Regular Judge	-1.88+.03 AVLOAD -.08 JLOAD[c] (14.57)		
Non-trial Disposition with Visitor	-1.88+.03 AVLOAD[d] (14.57)		
Trial Disposition with Regular Judge	-17.02+.03 AVLOAD -.08 JLOAD[e] (22.97)		
Trial Disposition with Visitor	-17.02+.03 AVLOAD[f] (22.97)		
Case-track			
Non-trial Disposition without Motions	-9.13 (4.72)	-10.78 (22.91)	-27.17 (8.38)
Non-trial Disposition with Motions	-43.00 (9.41)	-111.47 (23.97)	-34.90 (10.96)
Trial Disposition without Motions	-9.19 (11.46)	-194.61 (59.85)	-37.51 (31.58)
Trial Disposition with Motions	-43.06 (12.44)	-295.30 (60.26)	-45.24 (29.59)
180 Day Goal		-128.09 (37.68)	
Control of Scheduling: State Court Administrator		-15.98 (31.92)	

Table 3

Estimated Effects of Case Processing Time

Variance	Detroit (S.E.)	Providence (S.E.)	Dayton (S.E.)
Control of Scheduling: Local Court		-45.38 (35.61)	
Court Statistics		-182.37 (37.85)	
Decentralized Plea Bargaining	-13.68 (3.18)		
Crash Program	-18.58 (8.99)		
Push Program		99.50 37.35	
Court's Average Caseload for Central Docket	-0.03 (0.04)	0.06 (0.02)	
Court's Average Caseload for Individual Docket	-0.08		0.06 (0.27)

[a] At a near-mean value for AVLOAD (225), the effect is -37.75 with a standard error of 10.55.
[b] At JLOAD (100), the effect is -39.04 with a standard error of 6.03.
[c] At JLOAD (100) and AVLOAD (225), the effect is -3.13 with a standard error of 21.49.
[d] At AVLOAD (225) the effect is 4.87 with a standard error of 21.50.
[e] At JLOAD (100) and AVLOAD (225) the effect is -18.27 with a standard error of 28.75.
[f] At AVLOAD (225) the effect is -10.27 with a standard error of 28.72.

Table 3

Estimated Effects of Case Processing Time

Variance	Detroit (S.E.)	Providence (S.E.)	Dayton (S.E.)
Control of Scheduling: Local Court			
Court Statistics		-45.38 (35.61)	
Decentralized Plea Bargaining		-182.37 (37.85)	
Crash Program	-13.68 (3.18)		
Push Program	-18.58 (8.99)		
Court's Average Caseload for Central Docket	-0.03 (0.04)	99.50 37.35	
Court's Average Caseload for Individual Docket	-0.08	0.06 (0.02)	0.06 (0.27)

[a] At a near-mean value for AVLOAD (225), the effect is -37.75 with a standard error of 10.55.
[b] At JLOAD (100), the effect is -39.04 with a standard error of 6.03.
[c] At JLOAD (100) and AVLOAD (225), the effect is -3.13 with a standard error of 21.49.
[d] At AVLOAD (225) the effect is 4.87 with a standard error of 21.50.
[e] At JLOAD (100) and AVLOAD (225) the effect is -18.27 with a standard error of 28.75.
[f] At AVLOAD (225) the effect is -10.27 with a standard error of 28.72.

CONTINUED

1 OF 2

Table 3

Estimated Effects on Case Processing Time

Variance	Detroit (S.E.)	Providence (S.E.)	Dayton (S.E.)
Control of Scheduling, Local Court		-45.38 (36.32)	
Court Statistics		-182.37 (38.60)	
Decentralized Plea Bargaining	-13.68 (3.15)		
Crash Program	-18.58 (8.99)		
Push Program		63.26 (46.21)	
Court's Average Caseload for Central Docket	-0.03 (0.04)	0.06 (0.02)	
Court's Average Caseload for Individual Docket	-0.08 (0.02)		0.05 (0.26)

[a] For AVLOAD = 200 (the mean monthly caseload in Detroit), the effect is -37.04 days with a standard error of 9.63.

[b] For JLOAD = 123 (the mean monthly judge's caseload for courts under an individual docket), the effect is -21.20 days with a standard error of 5.75.

[c] For AVLOAD = JLOAD = 200, the effect is -11.86 days with a standard error of 16.82. We set AVLOAD = JLOAD under the assumption that a court making a change will divide the caseload equally among the judges.

[d] For AVLOAD = 200, the effect is 4.12 days with a standard error of 20.51.

[e] For AVLOAD = JLOAD = 200, the effect is -27.02 days with a standard error of 26.64.

[f] For AVLOAD = 200, the effect is -11.02 days with a standard error of 27.79.

Number of Defendants. In Detroit, there is a significant effect, with each additional defendant adding roughly seven days of processing time. In Dayton and Providence, the estimate is negative but insignificant. The actual effect, we remain inclined to believe, is either zero or small but positive.

Disposition Type. Trial cases take much longer than others--up to 69 days longer in Detroit, 172 days longer in Providence, and 38 days longer in Dayton. As expected, the effect is generally smaller under the individual docket and case-track. The individual docket reduces the effect by some 15 days, although the difference is insignificant, because low variance in DOCK * TRIAL produces a large standard error. The case track makes no significant difference in Dayton and Detroit, but reduces the impact in Providence essentially to zero (the estimate is insignificantly negative).

Dismissal at or before the preliminary examination shortens processing time in Detroit considerably. How much depends on the docket system and, under the individual docket, on the judge's caseload. In general, a dismissal at this stage abbreviated case duration by a bit over a month.

Pretrial Motions. Pretrial motions substantially lengthen processing times in all three courts. Absent a case-track, they make a difference of 54 days in Detroit, 125 days in Providence and 23 days in Dayton. The case-track generally cut this effect, all the way back to 20 days in Detroit and 24 days in Providence. In Dayton, on the other hand, the case track did not reduce the (already comparatively short) time added by motions significantly.

Failure to Appear. For every day the defendant is truant, his case takes almost exactly one additional day to process in Detroit, roughly three-fourths of a day in Providence and just over a day in Dayton. Only

in Providence is the departure from 1.0 significant, however. There, the court seems to make an effort to make up for lost time once the defendant finally appears.

Psychiatric Evaluation and Treatment. This effect varies considerably from court to court. In Detroit, it is significantly greater than 1, in Dayton significantly less. Thus while in Detroit the picture is one of inefficiency, in Dayton it is one of compensatory acceleration. This is even more true in Providence, where the estimate is only 0.30, the only wildly implausible point-estimate we obtain. It is hard to believe that a court could make up over two-thirds of the days lost to psychiatric evaluation and treatment. It is worth pointing out, however, that the standard error is .87, so that the estimate is insignificantly different from 1.

Continuances. In all three cities continuances slow processing time by some 2 to 4 weeks per continuance. The cost varies a bit--from 13 days in Providence, to 17 days in Detroit, to 30 days in Dayton. Except for the difference between Detroit and Providence, all these estimates and the differences between them are significant. Thus the frequent prescriptive emphasis on the importance of a "firm continuance policy" as means of reducing processing times (as in, for example Sipes et al., 1980) seems well-placed.

Repetition of the Preliminary Examination. This variable operates only in Detroit, where its effect is positive (as expected) and moderately large (13 days)--but quite wide of statistical significance, owing to a large standard error. Again, this is the unfortunate but unavoidable result of low variance--few preliminary examinations are repeated.

Mistrial. The case of mistrials, which are similarly confined to Detroit, is similar. An even larger estimate, also positive as expected, is rendered insignificant by an equally large standard error. And again

low variance is the culprit; mistrials are exceedingly rare.

Attorney Type. Having a retained attorney makes no difference in Detroit, makes a modest but statistically insignificant difference in Providence (13 days), and makes a similar but significant difference in Dayton (11 days).

Pretrial Release. In all three courts, cases proceed faster when the defendant is awaiting the outcome in jail. Apparently, non-jailed defendants are more willing to avail themselves of the tactical advantages of delay. Or the courts do give priority to jailed defendants. Or both. In Detroit and Dayton, the defendant's being in jail makes a difference of about a week-and-a-half. In Providence, it makes a difference of five weeks.

Seriousness. The seriousness of the charge makes an appreciable difference in Detroit, where, for example, the difference between a felonious assault, carrying a maximum penalty of 48 months, and an arson, carrying a maximum penalty of 240 months, is about twelve days of processing time. In Providence and Dayton, however, the seriousness of the charge does not seem to matter.

Prior Record. Only in Detroit does prior record have its anticipated effect. There the coefficient estimate of .86 means that the case of a defendant without prior convictions should run 4 days shorter than the case of a defendant with a single prior conviction, which should run 2 1/2 days shorter than the case of a defendant with two prior convictions, and so on in declining fashion. (Convictions beyond two make little difference.) In Dayton, however, prior record has no significant effect, while in Providence it has a significantly negative one, with lenthier prior records producing shorter, not longer, processing times. This is not what we

expected, and we shall have more to say about it below.

Docket Type. The impact of the individual docket (assessable only in Detroit) varies with the case and caseload. It depends on the case's having passed the preliminary examination, whether the judge is a regular, and the size of his caseload. It may be substantially larger for trial cases, although a large standard error leaves some doubt (see the discussion under Disposition Type.) At the low end, there is no discernible effect for cases disposed at or before the preliminary examination. For other non-trial dispositions handled by a regular judge, the effect is a bit larger, and a negative function of the judge's caseload.

Case-Track. The case-track decreased processing times in all three cities, although the magnitude of the decrement depended on disposition and motions and varied from court to court. In Detroit and Providence, the effect was significantly larger for cases with motions; in Providence, it was also significantly--and massively--larger for trial dispositions. But the case-track curtailed even non-trial cases without motions in Detroit and Dayton. For the most susceptible cases, the case-track reduced the expected processing time by 43 days in Detroit, by 295 days in Providence, and by 45 days in Dayton.

Declaration of Goal. The adoption of the 180-day goal--a sort of embryonic case-track--had a surprisingly large effect in Providence, decreasing case processing times by 128 days.

Statistics. The publication of the caseload, age distribution of cases, and other statistics was more influential still, accounting for a difference of 182 days (less a much smaller amount that vaired with the average caseload).

State and Local Scheduling. On the other hand, the transfer of control over scheduling first from the prosecutor's office to the state court

administrator and then from there to the local court administrator had a smaller and less certain effect. The first move seems to have reduced processing times by 16 days and the second by an additional 45. Though smaller, these numbers are still not small. On the other hand, they are not significant either.

Administrative Judge. The effect of concentrating scheduling authority and the like in an "administrive justice" is similar--fairly large (31 days) but insignificant.

Location of Plea Bargaining. The decentralization of plea bargaining in Detroit had its intended effect, abbreviating processing times by 14 days. Since a more or less opposite change in Dayton coincides with the advent of the case-track, we can only guess that its effect, consistent with this, was to increase case processing time and thus to dilute the effect of the case-track.

The Crash and Push Programs. The docket-clearing miscellany known as the Crash program appears to have reduced processing time by 19 days in Detroit. In Providence, meanwhile, the partly similar Push program raised processing times by 100 days. The difference in sign was expected, since the Push programs concentrated on older cases at the expense of current ones, whereas the Crash program worked on both.

Caseload, Average and Individual. The average caseload has no effect in Detroit but a positive one in Providence, where more cases mean lengthier processing. The judge's caseload, in contrast, has no effect in Dayton but a negative one in Detroit, where (under the individual docket) more cases mean accelerated processing. These effects are modest. If, in Detroit, the judge's caseload increases by 50, he speeds his processing of the average case by 4 days to catch up. If, in Providence, the average

caseload increases by 50 days, the diminished resources available for each case add an extra 3 days to the processing time.

Discussion

We have presented the results by variable; let us summarize them by court. In Detroit, the dominant effects are those of trial, dismissal at or before the preliminary examination, motions, continuances, mistrial, and the case-track (for cases with motions). To a somewhat lesser degree, processing times were also a function of the number of defendants, pretrial release, seriousness, prior record, the location of plea bargaining, the reforms of the crash program, docket type, and (under the individual docket) the judge's caseload. Days of defendant absence seem to have been translated on a one-to-one basis into days of processing time, while days lost to psychiatric evalutaion or treatment seem to have been magnified by surrounding inefficiencies. The remaining variables had no major impact. The model and results are the same as in our previous analysis of the data on Detroit alone (Luskin and Luskin, 1984).

The new information is from Providence and Dayton, where we now discover that the picture is similar yet different. In Providence, the largest effects belong to trial and motions (before the case-track), bail, the declaration of goal, the publication of court statistics, the case-track (for cases with motions or trial or both). Indeed, these effects are enormous--much larger than any in Detroit (or Dayton). Continuances, prior record, attorney type, and the Push program have smaller but still sizable effects, though prior record's is not in the expected direction. The effects of scheduling control are in the same ballpark, except that large standard errors prevent them from attaining significance. Both "days lost" variables show up with coefficients under 1, but only for the coefficient

for failure to appear is the difference significant.

In Dayton, too, there are some impressive effects, if nothing nearly so mammoth. The largest belong to trial and motions (before the case-track), continuances, and the case-track itself (especially for cases involving a trial or motions). The defendant's having a retained attorney and being at liberty have more modest effects. Days lost to psychiatric testing and treatment translate to significantly fewer days of processing time, suggesting a compensatory effect once the case reenters the normal flow, while days lost to a missing defendant add roughly the same number of days to processing time.

Clearly, then, there are differences between courts. Not every variable is at work in every court, and the effects of variables operative in more than one vary in magnitude and occasionally even in sign. In some degree, the differences in magnitude are simply a matter of scale. The effects tend to be largest in Providence because the processing times varied most widely there. The standard deviation was by far the highest there (see Table 1 again), as was the coefficient of variation (the standard deviation divided by the mean). An increase or decrease of, say, 200 days would be gargantuan in Detroit or Dayton but only moderately large in Providence. Against this standard, the several-fold differences between the Providence and the Detroit and Dayton effects of trial and motions (before the case-track), pre-trial release, and the case-track (especially for trials or cases with motions) are much smaller than the raw estimates suggest.

Other differences are more difficult to explain. Assuming that the chance element in sampling has not played us tricks, the differences in estimated effects are presumably a function of as yet unidentified structural or environmental differences between courts. Identifying them is the

hard part, and we do not claim to have discovered them all. We do not know, for example, why the court loses extra time on psychiatric cases in Detroit but makes up for lost time in Dayton, or why it recovers some of the time lost to absent defendants in Providence but not in Dayton or Detroit. Nor do we know why the number of defendants seems to matter only in Detroit. Some residual puzzles of this sort are more rule than exception in comparisons of causal weights across populations and may resignedly be left as a stimulus to future theorizing and research.

For the present, some explanations of some differences suggest themselves. Consider, for example, the effect of attorney type. In Dayton, the defendant's having a privately retained attorney seems to prolong his case, as expected. In Providence, too, the effect is positive and roughly as large, if less certain, owing to a larger standard error. In Detroit, however, the effect is apparently nil. The explanation may lie in subtle differences in the meaning of the variable. Although the retained attorney category is the same in all three courts, the alternatives are not. In Dayton and Providence, most indigent defendants--nearly all in Providence and some 60 percent in Dayton--are represented by the public defender's office, whereas in Detroit some 75 percent are assigned to court-appointed private counsel. Even the remaining 25 percent go to attorneys from the Legal Aid and Defender's Society, which though it serves much the same function as a public defender's office, stands formally apart from and is consequently more independent of the rest of the court bureaucracy. Thus the differences between privately retained and other attorneys is smaller in Detroit, which may be why it makes less of a difference there.

Perhaps the most surprising departures from the results in Detroit are the estimated effects of seriousness and prior record. Our working assump-

tion was that a more serious charge or a lengthier prior record means higher stakes and thus encourages defendants to stall and judges to proceed more deliberately, both out of fairness and for fear of appeal. Consistent with that, we found positive effects for both variables in Detroit. But in Dayton and Providence, lengthier prior records seem to bring shorter, not longer processing times, while the seriousness of the charge seems to make little difference. One reason may lie in the measurement of seriousness. The correlation between the maximum penalty for the charge and the actual seriousness of the offense may be lower in Providence and Dayton than in Detroit. In Detroit but not in Dayton or Providence, prosecutors rigorously screen out "serious" cases that are not really serious.

Another explanation may lie in prosecutorial priorities, which the hypothesis of positive effect neglects. Almost always, there are prosecutorial incentives to concentrate on cases that will make for "good" convictions--cases, among other things, in which the charge is serious and the defendant has a lengthy record (Forst and Brosi, 1977; Mather, 1979; Heumann, 1978). But the incentives may be stronger in some courts or at some times than others. We suspect that large caseloads and the slow processing times that generally go with them tend to heighten the motivation to dispose of at least the "worst" cases before they can deteriorate and end in an embarrassing acquittal or dismissal or before the time elapsed can itself attract media attention. Thus, in Providence, where caseloads were staggering and the processing of cases tortoise-paced, the prosecutor, acutely aware of these problems, made a conscious decision to give first attention to defendants with serious charges against them or long prior records. As he put it,

> What I tried to do was to establish a system of priorities . . . . Top priorities were people who were unable to obtain bail . .

> . . Capital offenses were priorities . . . . We have statistics indicating that most crimes were committed by a limited number of people, so that, if you could get off the street somebody who was a repeat offender, . . . you would probably be preventing future crimes as well.

But if the effects of prior record and seriousness are contingent on prosecutorial priorities, they are also contingent on the extent to which prosecutors influence scheduling. In Providence, it is therefore worth noting, the prosecutor had much more control over scheduling than in Dayton or Detroit. Even after formal control over scheduling was shifted elsewhere, the prosecutor retained an informal say. In Dayton, where caseloads were small and the prosecutor not especially influential in scheduling, this explanation admittedly leaves the results something of a mystery, but it at least helps account for Providence.

We also think we can account for the difference between the Detroit and Dayton effects of the judge's caseload, if there is one. The mildly positive estimate for Dayton is significantly different neither from zero nor from the estimate for Detroit. Hence the Dayton effect may not be real, if real, may not really be different from the Detroit effect, and if both real and really different, may not really be positive. Indeed the difference would scarcely be worth mentioning if it were not what one would expect on the basis of two differences in context. One has to do with administrative encouragement. In Detroit, but not in Dayton, there was a strenuous effort to raise judges' "docket consciousness." The individual docket by itself may not be enough to reduce processing times. The effect may depend on the extent to which court administrators, the press, or the public make caseload a salient criterion of performance.

The other difference has to do with the caseload itself. We have argued that under the individual docket the judge has reason to monitor his

caseload and keep it within tolerable bounds. But where caseloads are sufficiently small, there is less potential for embarrassment and may therefore be less incentive to pay attention. If so, the effect of caseload should be less negative--or perhaps even positive, since without an effort at containment, longer queues may bring longer processing times. Much the same may apply for very large caseloads--large enough that efforts at appreciable reduction seem forlorn and a few dozen cases more or less matter little anyway. In short, the effect may be nonlinear, with the domain of caseoad divisible into regions of "innocence," "practicality," and "hopelessness," in which the effect is first small and possibly positive, then larger and negative, then smaller and possibly positive again. Caseloads in Dayton, which are quite small, may fall in the lower region, while those in Detroit, which are larger (and more typical) may fall in the middle one.

Some of the variation in the impact of the case-track is also explicable. Although the case-track trimmed processing times everywhere, the sizes of the reductions and the kinds of cases for which they were largest varied from court to court. In general, the reductions were largest in Providence. The deadlines there were geared to achieve a 180 day processing time, as opposed to only a 90 day processing time in Dayton and Detroit, but the pre-track distribution of processing times in Providence was such that a 180 day processing time typically represented a greater reduction there than a 90 day processing time did in Dayton or Detroit. Other differences are probably a function of the ways in which the tracks' deadlines meshed or broke with prior court practices. In Dayton, where the major change was an earlier to start for plea negotiations, the track reduced processing times even for non-trial cases without motions; in Detroit, where the new deadlines were at the later stages, the effect was

mainly confined to cases with motions.

Of course, there are also similarities between courts. Trials and motions add considerably to processing times in all three (though only before the case-track in Providence). So do continuances. And so does the defendant's being free on bail. Although it is perilous to generalize from an N of 3, these variables would appear to be influential most places. Similarly, all three case-tracks , despite varying centents, had an imposing effect, at least for certain kinds of cases, which augurs well for case-tracks more generally.

Indeed, one of the most striking features of these results is the effectiveness of the structural variables. Docket type, unfortunately, remains a doubtful case. Processing times of trial cases seem substantially shorter under the individual docket, but the standard error is too large to maintain this with much assurance. But the case-track is not the only clear success story. The decentralization of plea bargaining helped in Detroit, and the keeping of court statistics and the declaration of goal cut processing times sharply in Providence. Less certainly (a large standard error again), the localization of scheduling authority may also have brought speedier dispositions.

These results offer hope and some practical lessons to courts seeking to reduce processing times. The experience of these three courts shows that processing times can be reduced, and the parameter estimates suggest two broad approaches. Processing times can be reduced either by shifting the distributions of case-level variables within the court's partial control or by reshuffling the court's sturctural arrangements. The first approach has some drawbacks. Granting fewer continuances, discouraging trials or encouraging pleas, releasing fewer defendants on bond, or dismis-

sing more cases at the preliminary examination will curtail processing times, but there may be obvious objections, on other grounds, to doing any of these things. On the whole, a better hope of avoiding unwanted side-effects lies in the structural path. The imposition of sanctioned deadlines for case events seems the surest way of bringing processing times down. The history in Providence suggests that even the establishment of a nonmandatory goal of so many days per case may help. Those presumably few courts not already providing caseload and other statistics would do well to begin. And the decentralization of plea-bargaining seems to make for greater efficiency and hence shorter processing times. Of course, the details of structural changes will matter, as the variation among our three courts in the impact of case-tracking suggests. Most of the structural effects are mainly a matter of shaping incentives, and although changes elsewhere need not replicate the ones we have studied to be successful, they can generally be expected to reduce processing times to the extent they increase the interest of key participants--judges, prosecutors, defense attorneys, and defendants--in doing so.

All these conclusions are tentative, to be sure. We have only looked at three courts. Other analyses, of other courts, will add to our understanding of the process that determines processing times, of what is constant and what is variable about it, and what accounts for that variation. We claim only to have made a start.

Notes

[1]The data we use here are largely but not entirely the same as those used by Neubauer and Ryan (1982). The differences are that we have filled in the values of some previously missing observations and added some previously omitted court-level variables, while Neubauer and Ryan exclude some non-missing cases from the analysis in Dayton and Providence. In addition, we do not analyze the data from Las Vegas (because theirs do not include prior record), while Neubauer and Ryan do not analyze the data from Detroit.

[2]Clearly, it cannot be true that no motions after the first have any additional effect. Hence a logarithmic transformation may seem appropriate, as in the case of prior convictions below. This would imply an effect that drops rapidly toward zero as the number increases. But the fit we obtain with the variable in logarithmic form is still somewhat inferior to the fit we obtain with the motions/no motions dichotomy. Although motions beyond the first undoubtedly add procesing time, they apparently add so little that even a linear-in-the-logarithm specification overstates their effects.

[3]On the advantages and disadvantages of alternative docket systems, see Solomon (1973).

[4]None of the explanatory variables figures to be affected by processing time. Caseload, which might be affected at time t+1 by the mean

processing time at time t, is lagged by a month (entered as of time t-1) and not much affected by the processing time of any individual case anyway.

[5]Roughly, an effect is the amount of change that can be expected to occur in the dependent variable (here, processing time) in response to each unit of change in one of the explanatory variables, other things being equal. In a linear, additive equation, a variable's effect is its coefficient.

[6]Since the logarithm of 0 is undefined, we add .01 to the number of prior convictions.

[7]In Detroit, cases were divided by month; in Providence and Dayton, where the monthly subtotals were too small to permit reliable estimation, the division was necessarily coarser. In Dayton, cases were divided into two groups, the first ten and remaining fourteen months. In Providence, the division was into four groups, covering the first three, the next sixteen, the next nine, and the final eight months.

[8]The R^2's are computed as the squared bivariate correlation between the actual and the GLS-predicted processing times.

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ACCOUNTING FOR CASELOAD: A SIMPLE MODEL

Among the most salient of the variables commonly used to characterize and explain the operations of courts is the number of cases pending, or caseload. Descriptively, it is a measure of the burden of work the court has to shoulder (in which role, it is often normed against the resources available--principally, the number of judges hearing cases--or weighted by complexity, as defined by types of cases). Other uses are evaluative. Caseload is a frequent criterion of court performance. When courts seek to demonstrate their accomplishments (as, for example, to legislatures), they commonly point to graphs showing decreases in the "backlog" of cases, which is to say, caseload. If, on the other hand, the backlog is on the rise, it may be cited as evidence of the inadequacy of present resources relative to demand, and hence of the need for more resources.

Still other uses are explanatory. Caseload has long been presumed to have a major impact on various other aspects of court performance and behavior, including the attractiveness of plea bargains offered, the proportion of cases ending in pleas (and on the other side of the same coin the proportion of cases going to trial), and the length of time it takes the court to process a case. The actual evidence is mixed,[1] and much of it methodologically flawed. Many of the relevant studies are entirely or essentially bivariate, many of the samples are hopelessly small, the dependent variable is not always the number of cases actually pending, and so on. But enough of the results are positive, and positive enough, to suggest that caseload does have some, at least, of the effects traditionally

ascribed to it--if perhaps not all of them, or not under all conditions, or not to the degree once generally thought.

Explaining Caseload

That is as much as we need say about caseload's effects on other variables[2]--enough merely to establish that it is a variable worth looking at, and its own antecendents worth looking into. Here, our concern is with the explanation and prediction of caseload, not the other variables it may help to explain and predict in turn.

In a rather distant way, caseload is of course a function of the legal system and the social, economic, and technological environment in which it operates, since they affect both the rates at which people engage in various behaviors and the behaviors that get defined as criminal or actionable. In short, these factors affect the potential inflow of cases, and thus, other things being equal, the number of cases on the docket. Previous attempts to account for caseload have drawn upon variables of this general sort (Goldman et al., 1976, and Casper and Posner, 1976). But here we shall focus on more proximate causes. We take the number of new cases arriving as given ignoring the prior variables that may influence caseload through it.

The Model

At this level of proximity, there are exactly two variables on which caseload (abbreviated hereafter as C) depends. One, as we have already indicated, is the number of cases arriving (call it A). The other is the rate at which the court has managed to dispose of the cases arriving (call it P, for processing time). The effect of each should be spread over the recent past. The greater the number of recent arrivals, and the less rapidly they are handled, the larger the accumulation of cases pending

should be. Moreover, these effects should be nonadditive.[3] The smaller the number of cases arriving, the less it should matter how fast they are processed, and the faster they are processed, the less it should matter how numerous they are. At the extremes, if no new cases arrive, the rate at which they are (or would be) processed is irrelevant, and, if all new cases are processed instantaneously, the number arriving is irrelevant.

This is more than merely plausible. The relationship is almost an accounting one, giving the "Accounting" of the title, which is used there in the extended and more usual sense of explanation, something of its literal meaning as well. Let us denote the caseload at the end of some given time-period t--for consistency with the analysis below, let us say a given month, although it could as easily be a day, a week, or a quarter--by C_t. We may suppose that we have data on series of T time-points in all, so the t=1,2,...,T. Similarly, we may denote the number cases arriving during any previous month, say the i^{th} one before, by A_{t-i}, where i=0,1,...,t. Finally, let us suppose--unrealistically, of course--that every case arriving during the $(t-i)^{th}$ time-period takes the same time to be processed. Let us denote that time as P_{t-i}.

Now consider what would happen to the caseload C_t if the number of cases arriving during the i^{th} prior month A_{t-i} were increased by some number--call it ΔA. If $P_{t-i} < i$ (where i is the time difference between t and t-i), all the cases arriving in month t-i will have passed out of the system by month t regardless, so that the addition of a ΔA or any other number of cases makes no difference to C_t. On the other hand, if $P_{t-i} \geq i$, the cases introduced in month t-i will still be on the docket at the beginning of month t. In that event, ΔA additional cases arriving in month t-i will increase C_t by ΔA cases. In short, there is either a zero or a

one-to-one increase in C_t, depending on the processing time P_{t-i}.

Next consider the effect of a change in P_{t-i}, say, by ΔP months. If $P_{t-i} < i \leq P_{t-i} + \Delta P$, so that cases introduced in month t-i would have passed from the docket by the beginning of month t before the increase but would still be on it after, the increase would augment C_t by A_{t-i}, the number of cases involved. But if $P_{t-i} \geq i$, so that the cases introduced in month t-i would still be on the docket at the beginning of month t either with or without the increase in P_{t-i}, or if $i \leq P_{t-i} + \Delta P$ (which implies $i < P_{t-i}$), so that cases introduced in month t-i would be over and done with either with or without the increase, the effect of the increase on C_t is obviously 0. In short, the effect of variations in prior processing times is either zero or the number of cases involved (A_{t-i}), depending on what the processing time is before it is varied (P_{t-i}) and on the size of the variation (ΔP).

These relationships are instructive, but not really useful. Even if the assumption of uniform P_{t-1} were accurate, they would only be useful postdictively, as a means of assessing the hypothetical impact of variations in past arrivals or processing times. Since we cannot know how long cases presently arriving will take, we cannot generate predictions as to what would happen to the caseload if they were more or less numerous or took longer or shorter to complete. Another problem is that for a given lag i the effects of variations in P_{t-i} and A_{t-i} on C_t will be different for each t, which makes for unparsimonius and unwieldy explanation. Finally, cases introduced in a given month do not all have the same processing time. To allow processing times to vary would make the accounting unwieldier still. But not to allow it makes the accounting inaccurate.

Perhaps the best solution, if we are interested in prediction and more concise explanation, is to substitute the average processing time in the

$(t-i)^{th}$ period (call it $\overline{P}_{t-i}$) for P_{t-i} and to develop a model that essentially averages the effects of both $\overline{P}_{t-i}$ and A_{t-i} over a number of time-periods, i.e., over a number of values of t. How much, on the average, can C_t be expected to increase when A_{t-i} increases by X cases, or when $\overline{P}_{t-i}$ increases by Y days? A model that can tell us that can enable us to predict and understand variations in caseload in the near future.

This argument suggests a "distributed lag" model. Clearly, the effects of both A and $\overline{P}$ must occur either "contemporaneously (given that C_t is defined as the end of the month) or with some lag. Clearly, too, the effects must be spread or distributed over a number of lagged observations. The number of arrivals will matter for several previous months, and so will speed with which they are processed.

Together, these considerations lead us to a distributed lag model in which the variables A and P are combined multiplicatively. Specifically, the model is

$$(1)\quad C_t = \alpha + \beta_0 A_t P_t + \beta_1 A_{t-1} P_{t-1} + \ldots + \beta_M A_{t-M} P_{t-M} + u_t ,$$

where α and the β's are unknown parameters to be estimated, M is the lag before which A and $\overline{P}$ have no effect, and u is an unmeasured disturbance summarizing the causative factors of which the model takes no explicit account.[4] (We assume that they are uncorrelated with the $A_{t-i}P_{t-i}$, so that their having been omitted is not an obstacle to estimation.)[5]

Under this simple model, the effect of the number of arraignments in the $(t-i)^{th}$ month is

$$(2)\quad \beta_i P_{t-i}$$

and the effect of the mean processing time in the same month is

$$(3)\quad \beta_i A_{t-i},$$

where, in both cases, i-0, . . . , M. Provided that $\beta_i > 0$, each quantity is both positive and an increasing function of the value of the other variable in the same month.[6]

The Data

The data here come from Detroit Recorder's Court--the municipal criminal court of Detroit--and span the two-year period from April, 1976 through March, 1978. Among other things, the data include the number of initial arraignments ("on the arrest warrant") each month and the number of cases (of defendants, actually) on the docket as of the end of the month, both taken directly from pretabulated court records. For our purposes, however, the data from the records are not sufficient. The caseload, as Recorder's Court reckons it, includes only those cases which have made it past the preliminary examination. This means that the "arrivals" variable is most appropriately the number not of the initial arraignments "on the arrest warrant" but of the post-preliminary examination arraignments "on the information." But one deficiency of the pretabulated records is that they afford no count of the latter. Another is that they do not include anything in the way of case processing time.

Nonetheless, A and $\overline{P}$ can be estimated. The data also comprise a (random) sample, stratified by month, of all of the cases begun within the two-year period, and two of the case-wise variables recorded are the length of time to completion and whether or not the case is disposed of before or at the preliminary examination. Since the sample size is adequately large--about 85 cases each month--we may readily compute estimates of the monthly mean processing time and of the number of arraignments each month,

the former directly, and the latter as the number of arraignments on the arrest warrant times the sample proportion surviving the preliminary examination.

We end up with a monthly time-series of 24 months. The variables' minima, means, and maxima over this period (and over the somewhat shorter period on which we actually estimate the model--see below) are given by Table 1. In passing, we may note that this period saw the introduction of a number of structural innovations that were specifically designed to, and did, reduce processing times, with the result that both processing times and caseloads were as a rule substantially lower toward the end of the period than toward the beginning. Thus, since the sample on which we estimate the model consists of the last 17 observations, the means and minima of C and $\overline{P}$ are lower there than in the sample as a whole.

Table 1 About Here

Estimation

The estimation of equations such as (1) is typically hindered by the presence of extreme collinearity among the lagged values of the explanatory variable: in this model, the product term $Z_{t-i} \equiv A_{t-i}P_{t-i}$, which, for purposes of estimation, is most conveniently treated as a single variable at each i. The variables Z_t, Z_{t-1}, . . . Z_{t-M} are in general so many and so similar that it is impossible to distinguish their individual effects very well. But by adopting some simplifying assumptions about the β's, we can reduce the number of regressors and the collinearity among them. Here, we assume that the 's can be sufficiently approximated by a polynomial in the lags (in i) of some pre-specified degree. This assumption, which

TABLE 1

Descriptive Statistics for the Variables in the Model*

Variable	Minimum	Maximum	Mean	Standard Deviation
CASES	2723	6563	4014	1333
	2723	6580	4472	1368
#ARR	543	987	782.2	112.6
	543	1133	823.0	137.0
AVCPT	43.84	80.4	59.75	10.92
	43.84	157.8	76.02	30.92

*For each variable, the values in the first row are those computed over the 17 months on which we estimate the model. The values in the second row are for the full 24 month sample period.

results in what is known as a "polynomial" or "Almon" lag scheme (after Almon, 1965; see also, e.g., Johnston, 1972 or Pindyck and Rubinfeld, 1981), has the advantage of being relatively flexible, in that it permits various patterns of effects over varying lags.

Admittedly, the results may to some extent depend on several additional and mostly nontheoretical specifications: the length of the maximum effective lag M, the degree--call it F--of the polynomial that approximates the β's, and the further restrictions, if any, that are placed on the latter. Of necessity, the choice of M and F is generally made on empirical grounds. First, M is generally set so as to maximize the proportion of variance explained, but without excessive cost in degrees of freedom, collinearity, or plausibility of results. Here, the proportion of variance explained is maximized at M=6, but is only trivially (.004) lower at M=7, where the collinearity is substantially lower and the precision of the estimates correspondingly greater. Thus we set M=7. Once given M, the choice of F is a matter of whether the necessarily higher proportion of variance explained with successively higher F's is statistically worthwhile, a criterion that leads here to F=2. We may note that F=2 compels the pattern of effects over i to be either flat or more or less U- or inverted-U-shaped, with at most one turning point.

That leaves the question of whether to constrain the β's, and, if so, how. A common practice is to set β_{-1} an/or β_{M+1} equal to 0--which in the present model would be to assert that the values of the explanatory variables in the future (at times t+1 and after) and/or their values more than M months in the past (at times t-(M+1) and before) have no effect on caseload. These have a certain intuitive appeal, but are also capable of exerting a heavy--critics say excessive--influence on the estimates of the other, nonzero effects. With F=2, the pattern of effects is forced to be

symmetrical, with $\beta_i = \beta_{M-i}$ and the peak (or nadir) precisely in the middle. Thus, in the interest of letting the data speak more nearly for themselves, we leave the model unfettered. The one constraint we do adopt is less confining, and is in fact suggested by the data themselves. Without constraints, the estimate of β_0 is anomalously but insignificantly negative, the likeliest inference from which is that β_0 is some small positive number. Thus strictly as a means of tidying up the results, we do impose the one restriction that $\beta_{0=0}$. Since the unconstrained estimate is insignificantly different from 0 anyway, the effect on the rest of the estimates is slight. Indeed, no reasonable specification--of M, of F (for which F=2 is the only reasonable choice), or of the values of the β's (not even $\beta_{-1} = \beta_{M+1} = 0$)--produces results too greatly different.

The final choice to be made is of estimator. This is not an open-and-shut matter either. As always with time-series data, one cannot but suspect the disturbance of being autoregressive and should usually make statistical allowances if it is. Here, however, it is not clear whether disturbance is autoregressive, or if so in what way. The relatively small number of observations--the lagging of Z up to i=7 reduces the effective N from 24 to 17--makes such determinations difficult. The evidence of autogregression is weak and murky. The Durbin-Watson test for the first-order variety is inconclusive (DW=1.34). And although the correlations between (the ordinary least squares-generated estimates of) u_t and u_{t-i} are not tiny (averaging a bit below .3) and seem to display the damped sinusoidal pattern characteristic of second- or higher-order autoregression, they are neither individually nor collectively significant. Similarly, some of the partial autocorrleations are not really small, but none is significant either.[7]

Even if we were to conclude on this tenuous evidence that the disturbance is autoregressive, it would still be unclear what, if anything, to do about it. The usual remedy is to use generalized instead of ordinary least squares to estimate the equation (GLS instead of OLS). But the advantages of GLS are only asymptotic, emerging only as the sample size becomes infinitely large. Whether they would emerge here, given the limited number of observations and the apparent mildness of whatever autoregression there may be, is uncertain (see Rao and Griliches, 1969). Furthermore, the partial autocorrelation function rapidly runs out of degreees of freedom, making the specification of the order of the autoregression involved a more than usually risky business. If it is not accurately specificed, the move from OLS to GLS may do more statistical harm than good. The results, in this instance--we have in fact seen them for both estimators--do not differ too dramatically.[8] But, given, as we have noted, that the data do not exactly cry out for a correction for autoregression, we are likely, we think, to do best by opting for OLS.

Results

The estimates we thus obtain are displayed in Table 2, along with their estimated standard errors [9] and the R^2.

Table 2 About Here

The first thing to observe is the size of the R^2. At .975, it could hardly be larger. The model as estimated explains the variation in caseload almost perfectly. True, the smallness of the sample does make explanation easier, but even the "adjusted R^2" (where the adjustment is in es-

TABLE 2

Estimated Effects on Caseload

(AVGCPT) x (#ARR)		β	S.E.
Lag	0	0	0
	1	.0045	.0008
	2	.0078	.0012
	3	.0100	.0013
	4	.0110	.0010
	5	.0108	.0005
	6	.0095	.0009
	7	.0071	.0022
Constant		161.6	174.7

$\sum_{i=0}^{7} \beta_i = .0607$

$R^2 = .975$

$R^2_{adj} = .972$

sence for the smallness of the sample in relation to the number of independent parameters)[10] is fully .972. This is high even for time-series data, though given the near-accounting relationships behind the model not entirely surprising.

Consider next the estimates of the coefficients.[11] In accordance with (2) and (3), these reveal the impact of any given change in the value of either of the explanatory variables, for any given value of the other, in each and any of the months preceding. If, for example, the number of arraignments is s constant 1000, the current caseload can be expected to increase by 45 cases for each day's increase in the mean processing time of the cases begun in the month before, by 78 cases for each day's increase in the mean processing time of the cases begun two months before, and so on. By the same token, the coefficients also reveal the effect of a change in the value of either of the explanatory variables (again for a fixed value of the other) in a given month on the caseload in the same or any subsequent month. Thus, if the number of arraignments is again a constant 1000, and the mean processing time in a given month were to increase by one day, the caseload could be expected to increase by 45 cases at the end of the next month, by 78 cases the end of the month after that, and so on. Notice by the way, that the coefficients trace out an essentially inverted-U-shaped pattern as they vary over the length of the lag involved. The effect of each of the explanatory variables, given a constant value of the other, is smallest in both the most immediate and most distant past, reaching its peak roughly mid-way between.

In a sense each variable's effects can be summarized by caseload's "long-run response" to it. Adapting the usual definition to this multiplicative model, this is simply the amount of change that can be expected to occur in response to a <u>constant</u> until change, at every effectual lag, in

the value of the one explanatory variable, for a given, constant value of the other. Thus the long-run response to the number of arraignments is

$$(4) \quad \left(\sum_{i=0}^{7} \beta_i\right) \bar{P},$$

while that to the meanc ase processing time is

$$(5) \quad \left(\sum_{i=0}^{7} \beta_i\right) A,$$

where $\bar{P}$ and A (note the absence of a subscript) are constant values of the "other" variable. For a given value of the other variable, the long-run responses can be estimated by simply substituting estimated for actual 's in (4) and (5). This gives, as the long-run response to arraignments, (.0607) $\bar{P}$, and, as the long-run response to mean processing time, (.0607) A.

Let us consider some plausible numbers. Suppose, for example, that the number of arraignments increased by 100 in every one of the preceding months. This is only a 12 percent increase over the average monthly level in this court during this period, and is thus by no means so big as to be at all unlikely. If the average case processing time was, say, 44 days (which was the shortest we observed in the period we studied), the long-run response would be (100) (.0607) (44) = 267. In other words, we ought to expect an increase of 267 cases in the caseload as a result. This, from a practical point of view, is in the nature of a lower bound. Mean processing times much shorter than 44 days are possible but not likely. If, on the other hand, the court averaged as much as 158 days per case (the highest monthly average we observed), the additional 100 cases per month in

the preceding seven months would results in an additional 959 cases to the caseload.

Or, again, consider the long-run response to mean processing time. Suppose the mean processing time increased by 10 days per case in each of the seven months before. This is again a shift of roughly 12 (actually, 13) percent of the mean over the entire period, and not unlikely to occur. If the number of arraignments each months were a constant (543) the lowest number in this period), the result would be another 330 cases on the current docket. If, at the other end of the range of likely responses, the number of new cases each month were at its observed maximum of 1133, the result would be instead another 688 cases.

Still other estimates can be formed for other combinations of changes in the one variable and values of the other. But the main points to be made are, first, that the model enables one to form such estimates by simply plugging in the appropriate values of A and $\overline{P}$, and, second, that even very modest changes in A and $\overline{P}$ have a very substantial impact on caseload.

Summary and Conclusions

To sum up, then: we have developed and estimated a simple, theoretically appealing, and empirically successful model of caseload as a multiplicative, lagged fuction of the number of cases arriving and the speed with which they are processed. The effect of each is (a) substantial, though spread over a number of lags; (b) dependent, at a given lag, on the value of the other at the same lag; and (c) at its peak in the middle temporal distance.

The major practical use of this model is to generate predicitons of caseloads. Given estimates of the parameters, one need only plug in the

actual or anticipated (or feared or hoped-for) mean processing times and number of new cases in each of a series of seven consecutive months in order to project the caseload at the end of the eighth.[12] In similar fashion, the increment or decrements that would occur in response to changes in the number of arraignments or mean processing time can also be estimated. This is true both of transient chagnes occurring in only a single month and of long-term changes occurring over any subset of the preceding months. For any such change(s), it is possible to trace the time-path of the resulting changes in caseload, and thus to see when it peaks and what it is at the peak. Such estimates should enable a court to anticipate its workload more accurately. And to control it, to the extent that it can control the number of incoming cases or (more likely) the time it takes to dispose of them.

Of course, we have estimated the model for one possibly atypical court only. In other courts, the number of lags over which both arraignments and processing times have their effects can be expected to differ. Similarly, the magnitudes of the effects will doubtless vary from court to court and even, perhaps, from period to period within this court. One would not want to use these data to make predicitons for other courts, or even for this court too far in the future. But judging from the R^2, the model seems to approximate an averaging out of the underlying near-accounting relationships very nicely, which suggests that it should be predictively useful wherever appropriate data are available.

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Notes

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[1]See, on caseload's relation to plea bargaining and the relative frequency of trials, Feeley (1979), Heumann(1975, 1978, and 1979), Nardulli (1979), Rhodes, (1978), and Hausner and Seidel (1979); on its relation to the decision to prosecute, Rhodes (1976); on its relation to bail-setting policy, Fleming (1979) and Feeley (1979); on its relation to sentencing, Feeley (1979) and Nardulli (1979); on its relation to court "productivity," Gillespie (1976); and on its relation to case processing time, Gillespie (1977), Church et al. (1978), Martin and Prescott (1981), and Luskin and Luskin (1983).

[2]Except for n. 5 below.

[3]Roughly speaking, the <u>effect</u> that one variable has on another is the amount of change that can be expected to occur in the one as a result of each unit of change in the other, other things being equal--i.e., again roughly, the partial derivative of the one with respect to the other (see Luskin, 1983). Accoridngly, the effect of arraignments on caseload is the number of additional cases we can expect there to be on the docket as a result of each additional arraignment, and the effect of case processing time is the number of additional cases we can expect there to be as a result of each additional day the court takes, on the average, to process its cases.

[4]This model bears a strong resemblance to the queuing theory equation known as "Little's formula." Specifically, Little's formula is $L = \lambda W$, where L in this context denotes the expected caseload, λ the expected number of arraignments, and W the expected processing time. (See Hillier and Lieberman, 1980, e.g.) Plainly, L corresponds to C, λ to A, and W to $\overline{P}$. But there are differences, too. L, λ, and W are in the nature of expected values, whereas C, A, and $\overline{P}$ are not, and partly because of that (1) is stochastic (i.e., includes a disturbance), whereas Little's formula is not. Further, Little's formula is derived--indeed, L, λ, and W are defined--on the assumption of a "steady" or equilibrium state. In (1), in contrast, the time-invariant quantities L, λ, and W are replaced the time subscripted C, A, and $\overline{P}$, with the effects of the second two on the first allowed to vary with the time elapsed, and apportioned over a set of lags.

[5]We are of course aware that mean processing time may depend on the caseload as well as vice versa (see the discussion in Luskin and Luskin, 1983). Given the C_t indicates the caseload at the end of the t^{th} month, P_t

may be a function, among a good many other things, of C_{t-1}. Nevertheless, we consciously ignore this additional equation, and thus avoid the complications that the combination of multiple equations with lagged endogenous variables and autoregressive distrubances, not to mention nonadditivities in the endogenous variables, would bring. We feel justified and tolerably safe in doing so because the dependence of processing times on caseloads seems to be slight (Luskin and Luskin, 1983), with the result that the system consisting of both equations is practically recursive.

[6]Admittedly, these effects and the equation that implies them do not quite capture all the subtleties of the near-accounting relationships described above. But to judge from the R^2 below, the approximation of an averaged-out version of them must be close.

[7]The .3 figure and the tests of significance are based on the autocorrelations betwee u_t and u_{t-1} through u_{t-4} only, in keeping with the rule-of-thumb of considering only the first N/4 elements of the series (Hibbs, 1974). For larger i, the pattern is much the same, however. For more on these sorts of diagnostics, see Hibbs again.

[8]The GLS R^2 (defined as the squared correlation between the predicted and actual values of caseload) differs by only .007, and the GLS and OLS estimates of the coefficients show broadly similar profiles. The biggest differences are that the GLS estimates rise and then fall a bit more sharply with increasing lags and that the GLS estimate for $_7$ is substantially smaller (in fact, insignificant at the .05 level). The long-run response is a trifle larger, at .0639.

[9]Estimated as in Johnston (1972).

[10]For this model, F+1 less the number of additional constraints on the β's: here, since wer constrain $\beta_0=0$, (F+1) - 1 = F = 2.

[11]Despite its size, the estimate of the "constant term" (about which we shall not bother to comment apart from this note) is a small fraction of its standard error, and thus statistically indistinguishable from zero. This, too, is an attractive result, since, under the purely hypothetical scenario in which the court either received no additional cases or processed all the cases ti received instantaneously in each of the previous months, we should expect the caseload, which in that event would simply be α, to be 0. Not that it would tell very much against the model if $\alpha \neq 0$. That would merely mean that the actual regression hyper-surface bent toward the origin as it approached it, and hence away from the regression hyperplane of our linear model--i.e., that the model did not apply so far outside the range of values we actually observe.

END