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COURT CONGESTION AND TRIAL RATES

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1 Introduction

The relationship between court congestion and guilty plea rates has long interested social scientists studying courts. The purpose of the present study is to expand the theory surrounding this relationship and to apply methods of analysis suitable to the issues posed. More specifically, we elaborate the theory from the single hypothesis usually posed into a broad range of possible interactions, which include likely reciprocal causation. We explore these complex theoretical relationships by applying several research methodologies not commonly used in criminology: the time series-cross section research design, the Granger-Sims test for causal direction, and the strategy of multiple replication. The latter involves studying the same issues in eleven states, and using alternate specifications and variable definitions.

To the best of our knowledge, there have been seventeen quantitative studies of the relationship between court congestion and trial rates, generally exploring the hypothesis that the two are negatively associated. The studies vary greatly in research design, variable specification, and results. Broadly summarized - more detail will be given later - ten studies found little or no relationship (Bureau of Justice Statistics 1984; Church et al. 1978a; Einstein and Jacob 1977; Feeley 1982; Gillespie 1977; Heumann 1977; Klein 1976; Miller et al. 1978; Nardulli 1979; Rhodes 1976); six studies found a negative relationship (Clark 1981; Flango et al. 1983; Grau and Sheskin 1982; Jones 1979;

Meeker and Pontell 1985; and Ross _et al_. 1981), and one found a positive relationship (Rubenstein and White 1979). Most of the authors who found a relationship, however, present additional, contrary evidence or suggested that the relationship may be spurious. Overall then, the weight of authority strongly supports the contention that court congestion and trials are not related; but this is far from sufficient to establish this a social fact, especially because major theoretical and methodological complexities are involved.

2. Theory and Specification.

In an important attack on economic and criminology research, Leamer (1982) claimed that researchers often use theory to justify weak research designs, and research results often depend on these theoretical assumpitons. Studies of the relationship between court congestion and trails, we argue, start with very incomplete theory and, thus, poor specification. They simplify complex relationships into abbreviated hypotheses, probably because the research methods used cannot handle more complex hypotheses. The following paragraphs discuss one aspect of this complexity, the various causal directions between congestion and trials. The next section discusses the variations in conceptualization and measurement of these two variables.

One can categorize the existing theories concerning the relationship between court congestion and trials into three perspectives: 1) court overload perspective, which focuses on the assumption that trials may overtax court resources, 2) court management perspective, which views trial scheduling as a means to reduce congestion, and 3) the economic perspective, which stresses the motives of defendants.

2.1 Court overload perspective.

Most research in the topic is concerned with the possibility that more trials may overtax court resources. This perspective has produced two similar hypotheses that are not entirely consistent. The first, and most obvious, is the that more trials lead to more court congestion: because trial dispositions consume more court, prosecution and defense resources then dispositions by guilty pleas or dismissal, more trials cause more congestion in the criminal justice system. The dominate force behind this theory is the frequent rationale for plea bargaining given by lawyers: if pleas were not encouraged, the courts cannot handle the additional trial workload (e.g. see Alschuler 1968: 54-55; Meubauer 1974; Blumberg 1974; and the summary in Nardulli 1979). Several researchers have asked participants why plea bargaining and guilty pleas are so common in their courts, and the answer is usually that if there were fewer guilty pleas there would be more trials, which would cause considerable delay (see Miller et al. 1978: 24-25; Klein 1976: 59-83; Heumann 1978: 25-26; Jones 1979: 63-65; see also summaries of participants' views in Haney and Lowy 1978: 638-639 and Buckle and Buckle 1977: 25-25). An exception is Farr (1984: 303-310) who found that lawyers in the Portland, Oregon, criminal court give at most moderate consideration to conserving staff resources when engaging in plea negotiations, much less consideration than strength of evidence. But the Portland court has little delay problem to begin with (Church _et al_ 1978: 10-15) and, thus, the lawyers may not perceive congestion is a problem.

This theory leads to a prediction that time series studies will show that congestion increases after the number of trials increases. In cross section research, the theory leads to the prediction that courts with more trials have more congestion.

The theory, however, leads to the contradictory hypothesis that more congestion is related to fewer trials: courts and lawyers react to congestion by encouraging less time-consuming dispositions methods. Unlike the initial hypothesis, this assumes only that judges and lawyers believe they can reduce congestion by avoiding trials, not that congestion is actually reduced. In time series research, the theory leads to the prediction that more congestion is followed by fewer trials. In cross section research, it leads to the prediction that courts with more congestion have fewer trials, as the courthouse personnel react to the congestion. This is directly opposite to the prediction given above for cross section correlations, and it provides an initial suggestion of the complexities encountered.

2.2 Court management perspective.

The court management literature contains numerous arguments that an effective way to reduce delay is to increase the number of cases tried. The main rationale is that if more trials are scheduled (for example, it is argued, through improved management of judge and court room time), cases can be tried earlier. Furthermore, the backlog of all cases, not just those to be tried, will be reduced: because many lawyers initiate serious plea discussions only under the immediate threat of trial, the pleas will be entered sooner if trials are held sooner (Dodge & Hathaway 1985). The typical argument is the best answer to delay is firm scheduling of trials, with little chance of a continuance, along with case-flow management techniques that permit cases to be made ready for trial earlier and that permit the most efficient use of courtroom space and judge time (e.g., Lawyes Conference Task Force on Reduction of Litigation Costs and Delay 1986; Sipes et al. 1980). These contentions apparently have never been supported by credible empirical evidence, but because they should be given considerable respect because they are often advanced by those having first hand experience with court operations.

This hypothesis, again, also implies its opposite. Courts may react to delay by holding more trials, attempting to clear the docket. In all, the court management viewpoint leads to the prediction that in time series studies that more trials lead to less congestion in later years, and more congestion leads to more trials in later years. For cross section comparisons, one would expect more trails to be correlated both with more delay and less congestion, again making cross section comparisons inappropriate.

2.3 Economic perspective.

Economists have supplied a third theoretical perspective, with is based on the assumption that defendants act rationally according to their individual self-interests. Landis (1974) speculated that more delay in criminal courts leads to fewer trials because defendants in custody are more likely to plea bargain in order to gain speedier dispositions. This leads to the prediction in both time series and cross section research that congestion is negatively associated in the current year with congestion. On the other hand, this hypothesis may be partly countered by the fact that defendants on pretrial release may believe it in their self interest to delay trials (Rhodes 1976).

2.4 Summary. There are good theoretical reasons for hypothesizing that more trials can increase congestion, that more trials can reduce congestion, that more congestion can cause more trials, and that more congestion can reduce trials. Scholars have advanced one or more reasons for each of these possibilities, and we suspect that further thought on the matter would produce many other theoretical arguments. Researchers cannot assume sufficient acumen to uncover all the likely causal mechanisms in a complex social situation.

3. Variable Formulation.

The key variables, congestion and trials, can be operationalized in several ways consistent with the theories outlined above. These variations, when added to the causal possibilities, result in a complex body of theory. Throughout, of course, it is necessary to keep in mind that operationalization is always limited by the availability of data.

3.1 Congestion.

The most common measure of congestion in the research is caseloads, usually measured by the number of criminal cases filed in a year. But caseloads are inadequate measures of congestion, apparently only used because the data are most readily available. As Meeker and Pontell (1985) stress, caseloads fail as a measure of congestion because they do not take into account the capability of the court to deal with the cases; , and they suggest that caseloads be divided by the number of judges. Still, caseload per judge may not measure congestion because the variable still does not indicate whether the court is able to deal with the workload; high caseloads per judge may not mean congestion, for example, if the judges are more efficient. Per judge caseload measures, of course, ignore other resources, such as attorneys and court staff. Finally, most important, useable data on the number of judges handling criminal cases is seldom available because nearly all courts either assign judges to both civil and criminal cases [and according to Marvell and Dempsy (1985) civil cases dominate the docket], or, where courts have separate criminal divisions, the assignment of judges is generally flexible.

The best congestion variables, therefore, are those that directly measure whether the court can expeditiously deal with its caseload. The theories all demand some measure of the status of the docket, either the size of the backlog or the amount of delay. Backlog is the number of cases awaiting dispositions, and delay is the time cases have been pending or the time to disposition. Both are difficult concepts, and space limitations allow permit a short summary here.

Backlog, which represents pressure on the courts to dispose of cases, is the number of cases pending. Ideally, the measure should contain only cases available for court processing, excluding "inactive cases" in where defendants are unavailable, usually because they cannot be located. For the 11 states studied, six leave out inactive cases from pending figures; for the remaining states the pending figures somewhat overstate the the amount of actual caseload awaiting processing.

Backlog, of course, is relative; a given volume of pending cases represents a more serious problem in a small court than a large one. Pending figures, therefore, must be standardized. For reasons given above, per judge figures are not useful. We use two measures: 1) pending cases per capita and 2) the backlog index, pending cases at the end of the year divided by dispositions that year. The first measure, and the reasons for using per capita data (which are not obvious), are explained later in more detail. The second is a common measure of delay, since is approximates the time required for the court to dispose of its pending caseload (DonVito 1972: 63-64; Church _et al._ 1978b: 1-2; see also similar measures used by Clark and Merryman; Clark 1978).

In addition, when available, we use other measures of congestion, such as average time to trial and median time of cases pending. Table 1 lists the number of congestion measures available in the eleven states.

3.2 Trial Measures.

Selecting a measure for trials also presents several options. First one might use either the number of trials or a trial rate, the number of cases tried divided by the total number of dispositions (or by adjudicated dispositions, trials plus guilty pleas). Although most research uses only trial rates, both measures can be justified by theory. When addressing the impact of trials on congestion, the better formulation is the absolute number of trials. When focusing on the practices of courts and attorneys, the trial rate is the better measure. For a full presentation of the relevant theory, both measures must be used.

A second issue encountered when operationalizing trials what types of trials and dispositions to include. The first major option is whether to include the all trials or just jury trials.[1] We prefer the latter for two reasons. First, because jury trials are more time consuming than nonjury trials (Sipes and Oram 1988: 8-9), they presented a more pointed test of the various hypotheses. Second, the data for non-jury trials may be less

1. An additional issue, not explored in this study, is whether congestion is related to the frequency of nonjury trial, as opposed to jury trial.

accurate than jury trial data because courts occasionally count non-trial hearings, such as evidentiary hearings and sometimes even guilty plea hearings. Finally, with rare exceptions, the courts studied hold far more jury trials than non-jury trials. We use total trials, however, in alternate analyses and in the two states jury trial data are not available (see Table 2).

The next issue is the denominator for the trial rate, all dispositions or adjudicated dispositions, guilty pleas plus trials. There is no clear preference. Adjudicated dispositions has the conceptual appeal that the trial rate also represents the guilty plea rate; and thus seemingly is better when exploring the extent court participants emphasize plea arrangements to make dispositions. However, it is very likely that many non-adjudicated dispositions, which are largely dismissals, also result from plea agreements: prosecutors may agree to drop a case against a defendant as part of a plea agreement in another case, or the prosecutor may dismiss the case as part of prosecution diversion program. Most dismissals, however, occur because the defendant cannot be found or the prosecution decides that the case is not strong enough to proceed.

3.3 Variable Form.

If a variable has much greater variation in some courts than others, those courts will dominate the results. This problem has two facets. The first is heteroscedasticity, discussed below in the section on statistical analysis. The second arises from the fact that variables associated with state size have more variation in larger courts, simply because the numbers are larger. For example the year to year differences in the absolute number of filings or trials is greater in larger courts, such that the regression results are often largely determined by just a few courts. Therefore, all variables that reflect court size are divided by the population of the court district.

4. Research Design.

There are three major features of the research design used in this research, all of which are infrequently used in criminology research: 1) the time series-cross section analysis, 2) the Granger-Sims test, and 2) the strategy of multiple replication.

The complexity of the theory requires a research design that can deal with uncertain causation direction and the possibility of reciprocal causation. With few exceptions, the research so far has used either the cross-section or the before-and after research designs, even though they are generally considered inadequate for causal analysis (see generally Campbell and Stanley 1967; Cook and Campbell 1979; with respect to court research see Lempert 1966; Lind *et al*. 1980; Luskin 1978; Monahan and Walker 1985). The only studies using designs that provide any hope for proper analysis are the long time series studies, but there are problems in the implementation of these designs.

4.1 Cross-section research.

The cross-section design, which looks at correlations between trials and congestion, is inherently inadequate: correlations cannot be interpreted because the researcher cannot determine which variable causes which or whether there is reciprocal causation. Also, the sample size - the number of courts - is usually too small to provide statistically significant results or to control for other variables. The sample size in the time series studies is: 23 courts in Bureau of Justice Statistics (1984), 21 courts in Church (1978: 31-35), three courts in Eisenstein and Jacob (1977: 238-239), two courts in Feeley (1982), approximately 37 states in Flango, et al. (1983: 39-42), nine courts in Heumann (1978), and 18 to 24 courts in Miller (1978: 18-24). The only cross-section study approaching an adequate sample size is Gillespie (1977), which uses all federal district courts. All these studies found no relationship between trials and various measures of congestion, with the partial exception of Flango et al, who contend that there is relationship for the number of trials but not trial rates, but without any quantitative analysis.

Nardulli (1979) uses a very different kind of cross-section design, but does not avoid the specification problems. He found no significant relationship between the number of guilty pleas in the Chicago Criminal Division and the volume of pending cases in that or the previous month. Nardulli makes the common mistake of assuming that the only causal direction is that caseload pressures affect guilty pleas; but it is just as likely that judges have large backlogs because they receive relatively few pleas. The two causal mechanisms may well cancel each other.

4.2 Before and after design.

Four other studies use simple before-and-after designs, determining whether congestion increased after trial rates changed, typically accompanying plea bargaining reforms. The problems with this approach are that other factors may have caused the change measured and that the full impact may not be felt for some time after the change (Brereton and Casper 1982). The four before-and-after studies reach differing results, although this may be partly due to the differences in research objective. Heumann (1977:30-31, 168) noted that trial rates did not increase in most Connecticut courts when a jurisdiction change reduced the criminal caseload. Haney and Lowy (1979: 639) criticize this research on several grounds, including the possibility that the impact might be delayed. Second, research on the Alaska plea bargain ban found that the number of trials increased and delay declined after the ban (Rubenstein and White 1979 and Rubenstein et al. 1980: 102-106, 274), but the authors speculated that the delay reduction was caused by court administration changes rather than the increase in trials. That interpretation has been criticized on the grounds that only one of the three courts studied experienced the court administration changes mentioned (Cohen and Tonry 1983: 323-324). A third before-and-after study found that trial rates declined after a successful program in a New Jersey court to reduce delay, but the authors speculated that the decline may have been caused by a new criminal code (Ross et al. 1981). Likewise, a study of successful delay reduction efforts in Ohio found that guilty pleas rose moderately in two of the three courts studied (Grau and Sheskin 1982a: 166-168).

4.3 Long time series.

A third methodology is the long time series, which can be a valuable design when studying causation. This design also supplies most of the evidence for a relationship between trials and congestion. The major difficulties with time series analysis are 1) that it requires data for many years, at least 50 years, and 2) it requires sophisticated regressions analysis to deal with questions of autocorrelation. Two studies of federal courts compiled data for a sufficient period, both finding evidence of a relationship between congestion and trial rates (Jones 1979: 74-75, 194-195; Clark 1981: 113-117), but both simply present their impressions without conducting time series regressions. Also, Jones (1979: 74-85) contended that most evidence was against the relationship, since it was not seen in state court data and the most recent federal data. The third long time series is Meeker & Pontell (1985), who studied the impact of California legislation reducing the number of felony cases filed in the Superior Courts. The authors assumed that this change reduced congestion, and explored whether the trial rates were affected. They found such an impact only for capital cases, but the results for other felonies are uncertain because the jurisdiction change also affected the types of non-capital cases retained by the Superior Court. Moreover, it is charitable to call this a long time series, rather than a before-and-after, study because the authors had data for to few time periods to obtain adequately robust statistical results.

5 Time Series Cross-Section Design.

The present study uses the time series-cross section design, which has long been considered one of the best designs to study causation (see especially, Campbell and Stanley 1967; Lempert 1966; Marvell 1986). The model combines data from several units over several years, and the total number of observations (sample size) is the product of the number of units and the number of years. In the present research, which studies eleven states, the units vary from 7 to 88 court units (counties or multi-county districts), and there are 7 to 16 years. The overall number of observations varies from 86 to 1031; only two states have less than 100.

We use the fixed effects model, the standard econometric regression procedure for analyzing time series-cross section data (Pindyck and Rubinfeld 1981; Mundlak 1978). This model, which is an analysis of covariance, creates a dummy variable for each court unit in the analysis, and the coefficient associated with the variable is an estimate of the influence of specific factors ("fixed effects") unique to a court unit. Omission of these fixed effects, if they are significant, causes the estimates of the other variables to be biased. The fixed effects, of course, reduce the degrees of freedom by the number of court units included (and an additional degree of freedom for each court unit is lost when correction for autocorrelation is required). As discussed later, the fixed effect model also permits controlling for year effects by entering dummy variables for each year in the analysis.

Specifically, the form of the fixed effect model is as follows:

$$Y_{it} = a + bX_{it} + cY_{it} + g_2W_{2t} + g_3W_{3t} + \ldots + g_NW_{Nt}$$
$$+ d_2Z_{i2} + d_3Z_{i3} + \ldots + d_TZ_{iT} + e_{it}$$

where X_{it} and Y_{it} represent the continuous variables (e.g., the percent of cases going to trial) and dummy variables (e.g., whether a speedy trial law is operating in the district that year), and the court unit and year dummy variables are:

W_{it} = 1 for the i'th court, i = 2, . . ,N; otherwise W_{it} = 0, and

Z_{it} = 1 for the t'th year, t = 2, . . ,T; otherwise Z_{it} = 0.

And e_{it} is the error term.

Court unit dummies can be omitted if not significant as a group (see Pindyck and Rubinfeld 1981:255 for the test of significance). However, court unit dummies are highly significant for most analyses, indicating that there are major differences between courts. The only exceptions occurred in analyses with dispositions as the dependent variable, in which the court dummies were usually not significant.

The use of court unit dummies has several practical results. The variables in the analysis are transformed into the difference from the mean for the particular court. As a result, the fixed effect model produces a time-series analysis only; it combines the time series data from the several court units into one regression, but ignores within-year, across-court variations. Therefore, the analysis avoids the causal uncertainties inherent in cross section studies.

The use of court unit dummies permits one to combine courts into a single analysis even though individual courts have their own particular characteristics, such as different caseload mixes. This can be done because the dummy variables representing court units control for the differences. The year dummy does not control for differences that change substantially from year to year (in which case they would be controlled by the year dummies, if the trends are state-wide). Hence, the court dummies control for any variable that does not change significantly over time in any court district.

5.2 <u>Statistical Problems</u>.

<u>Autocorrelation</u>. Because it contains a time series element, the pooled time series-cross section design frequently encounters autocorrelation problems. The Durbin-Watson test can be used in the fixed effect model as long as there are gaps of missing values between the court units in the time series, such that error terms for the first year in court i are not compared to the last year for court i-1. The Durbin-Watson statistic occasionally indicated autocorrelation in the regressions here. When it did we corrected for it by calculating a separate

autocorrelation coefficients for each court unit, the standard procedure in the time series-cross section analysis (Pindyck & Rubinfeld 1982:258-59). The correction has the drawback of deleting one year from the analysis, reducing the degrees of freedom. As a general rule, corrections were made when the Durbin-Watson statistic was below 1.70 (figures below 1.57 and above 1.78 indicate the presence and absence of autocorrelation at the five percent significance level), although the point at which corrections were made depended on whether the analysis could easily afford to drop a year. The tables presenting the regressions in Appendices B and C give the Durbin-Watson statistic and indicate whether auto-correlation corrections were used. Corrections were not made in the Granger-Sims analysis (which enter lagged values of the dependent variable as independent variables) because the Durbin-Watson statistics were rarely below 1.9.

Heteroscedasticity. Heteroscedasticity is a likely problem in this research because many of the variables have more year-to-year variance in the small court districts. For example, since both the number pending and disposed have greater proportionate variation in small courts, the backlog index (pending divided by disposed) has much greater variation in small courts. The same problem applies to other ratio variables such as the portion of cases going to trial and the portion of trials ending in convictions. Therefore, error variance is greater in small courts; unless corrected, this problem would cause the results to be dominated by the small courts.

Using the Breush-Pagan test, we often discovered heteroscedasticity problems in states where the county is the court unit, but seldom in states with multi-county court districts. Heteroscedasticity was corrected by using weighted regressions; the weights were population, the square root of population, or the fourth root of population, which ever eliminated heteroscedasticity under the Breusch-Pagan test.

Coefficient Disparity. A similar problem is that variables that are not ratio variables have much greater variation in larger court units. For example, the year-to-year changes in number of trials is much greater in large counties, leading to greater variation (in the fixed effects model the variables are differences from their means). The same problems arise when using the number of filings or the number of trials as independent variables. The large courts, therefore, would dominate the results with respect to such variables; so variables that are absolute numbers are expressed in per capita terms.

Collinearity. Collinearity tests[2] were conducted in all analyses, and there were no problems except when entering successive lags of variables that changed little from year to year. There are two classes of such variables: 1) dummy

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2. We use the no intercept option for the collinearity test because there is high, spurious collinearity between the intercept and the court unit dummy variables.

variables, especially those applicable to only a few courts, and 2) the number of judges, which change little from year to year in most courts, particularly in states with counties as court units. The regressions therefore do not include only one year for these variables (whereas other independent variables are entered in two or more lagged versions).

Influence. We used influence analysis (Belsely, Kuh, and Welsh, 1980) to locate observations that have extreme impacts on the regression results. There were a few such observations in nearly all regressions. We assumed that these problems were probably caused by bad data and, thus, opted to delete the observations when feasible. The procedure used was 1) to delete the observation if it was in the first or last year of the court unit time series, 2) delete the court unit from the analysis if three or more observations for the court showed excessive influence (under the assumption that the data were probably bad for the court unit), or 3) otherwise, retain the observation in the analysis, but conduct a separate analysis without the court unit to determine if the regression results change (it never did). For most analyses one or two courts were dropped, along with a similar number of individual observations in courts otherwise included. A list of the observations deleted because of influence (and other data) problems is available from the authors. Two whole analyses were dropped because the existence of numerous influence problems suggested that the congestion measure is bad. These measures are the number of juries sworn in more than 60 days from indictment in California and the number of cases pending over 6 months in Arizona.

5.3 Variable Lags and the Granger-Sims Test.

As stressed earlier, the research encounters severe specification problems because the dependent variables, congestion and trial rate, may affect some of the independent variables. If the regression with congestion as the dependent variable and the trial rate as an independent variable found that the latter has a significant coefficient, one cannot conclude that trial rates affect congestion; the result may be due to the impact of congestion on trial rates. Initially, we should stress, it is not enough to assume that by lagging independent variables, any causal relationship must go from the right to left side of the equation. The lagged version of the independent variable is likely to be correlated with the current year version of that variable, causing a spurious relationship with the dependant variable. The fixed effect model mitigates this problem because the variables are transformed to differences from their means and, thus, are less likely to be correlated from year to year than the variables in their absolute form. Nevertheless, we must control for the possible impact of the other values of the independent variable.

We are aware of three ways to determine causal direction. The first is using simultaneous equations with two stage least square regressions, which involves the use of identifying variables that affect only one of the variables being explored.

We do not use this technique because, to the best of our knowledge, there are no such variables with available data. For example, there is no variable that, we can state with reasonable certainty, affects court congestion but does not affect the trial rate (and additionally, is not affected by changes in congestion or trial rate).

The analysis here uses two other means of determining causal direction. The first is to use successive lags of independent variables whenever they may be affected by the dependent variable. If both the current and lagged versions of the independent variable are included in the regression, any "backward" causation is probably controlled by the current year version, such that the results with respect to the lagged version indicate one way "forward" causation, from the left to right hand side of the equation. This, however, suffers from two drawbacks. 1) Any large current year impact may bias the results, because in my be in the reverse direction. 2) The analysis is limited to determining lagged impacts, since the coefficient for the current year value is not interpretable (unless, as discussed above, the Granger-Sims test indicates the absence of reverse causation).

The second approach is the Granger-Sims test, the standard econometric technique for determining causal direction. Separate tests were developed by Granger in 1969 and Sims in 1972 and then shown to be theoretically equivalent by Boussiou in 1986. We use the more common Granger version (Granger 1969). The test works as follows: Suppose we have reason to believe that two variables, y and x, are simultaneously determined. If this were true, a regression of y on lagged y and lagged x would reveal significance with respect to lagged x variables. That is, in the regression:

$$y_t = a_1 y_{t-1} + . . . + a_n y_{t-n} + b_1 x_{t-1} + . . . + b_n x_{t-n} + u_t$$

the coefficient $b_1, . . . , b_n$ can be expected to be jointly significant using an F test. If not, then x does not cause y. Similarly, if we regress x on itself lagged and lagged values of y, the coefficients on the lagged y will be significant if y causes x. Otherwise y does not cause x.

In the present research we use two lags (t-1 and t-2). More lags reduce the number of years in the time series, and adding preliminary exploration adding a third year did not produce different results. To give an example of the application of the Granger-Sims to a key issue in the present research, the relationship between congestion and trials, two regressions were conducted: 1) with congestion as the dependent variable, and with the prior year and two year's prior variables for both congestion and trials, and 2) the same regression with current year trials as the dependent variable.

The Granger-Sims test, however, may not locate causal effects if there is no significant lagged component and if there is little correlation between the current year and prior year versions of the independent variable. Such situations are unlikely here with respect to causal relationships that are not artifacts of variable measurement (see Chapter 4). Thus, in the absence of such measurement problems, rely on the results of the regular regressions when the Granger-Sims test suggests no backward causation.

5.4 Multiple replications and robustness tests.

The research strategy in this research is the analysis design is multiple replication.[3] The results of any one regression may be incorrect due to bad data, chance correlations, uncertain theoretical assumptions underlying the model, and many other problems. One can greatly reduce uncertainty by approaching a question from several different directions and by repeating the research at different sites, using separate data sets. The approach here is similar to metatheoretical analysis, except that we combine the different research efforts in a single study, rather than gather data sets from others' research, and analyze the data.

We research the same questions in eleven different states, using separate data sets. In each state we use two to ten measures of congestion and a similar number of trial rate measures. The most common congestion measures are the backlog size (number of pending cases per capita) and the backlog index (pending cases divided by dispositions). Also, four states have

3. This research strategy is derived largely from D. Campbell, "Science's Social System of Validity-Enhancing Collective Belief Change and the Problems of the Social Sciences," Pp. 108-135 in D. Fiske and R. Shweder, ed., Metatheory in Social Science (Chicago: University of Chicago Press, 1986); and E. Leamer, "Let's Take the Con out of Econometrics," 73 American Economic Rev. 31 (1983).

other congestion measures, such as the mean or median time to disposition, the time pending, and the percent of cases pending over six months. The major trial rate measures are the number of jury trials and jury trials divided by merit dispositions (trial plus pleas). Alternate analyses using total trials rather than jury trials almost always reached the same results even though non-jury trials are less time consuming and have less accurate statistics.

One of the most difficult tasks in this research is selecting which particular dependant and independent variables to focus on. State court statistics usually provided several measures of congestion, trial rates, judges, and other variables. There is seldom any overwhelming theoretical or common sense reasons to prefer one measure over the others. Also, as a general rule, it is dangerous to establish a specific model, based on theory or otherwise, without checking the robustness of the results because the assumptions behind the model, which may be incorrect, can influence the results (Leamer 1983). In fact, strong point of the research presented here is the ability to provide numerous robustness checks, employing the great variety of variables.

But the robustness checks cannot be limitless. In the states with the most copious statistics, analyzing each variation of the dependent variable with each variation of the independent variables amounts to an enormous number of regressions. Also, full scale treatment is only feasible for a few regressions in each state because the process of checking each regression is laborious and requires considerable computer use.[4]

Thus, our strategy is to select one or two basic models, which are subjected to the full checking, and the robustness checks are conducted by, first, substituting the alternate dependant variables and, second (using the original dependant variable or variables) with alternate versions of the independent variables. This leaves the very difficult problem of selecting the basic models out of numerous other possible models for the state. We have not been able to derive a simple criterion; rather we have made our selection by balancing several factors:

1) The first is to select variables that do not lead to spurious or uninteresting correlations (for example correlations resulting from common denominators in dependent and independent variables; hence, the analysis of the backlog index cannot use the current year trial rate as an independent variable).

2) We favored variations that are common to a large portion of the states studied, to facilitate comparisons between states.

3) Next is the theoretical or common sense importance of the various versions. There are a few exceptions to the general rule that a priori reasons for selecting one variable over others are

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4. These steps are determining whether to delete year effects; checking for, and if necessary correcting for, autocorrelation and heteroscedasticity; conducting influence analysis and determining whether to delete observations; checking for reciprocal causation; checking for multicollinearity; checking for lagged effects greater than two years; and if the state is large enough, conducting separate analyses for random samples of one-half of the courts.

absent. For example, the trial rate with the number of trials plus pleas in the denominator is preferred over the trial rate with all dispositions as the denominator because the latter figure includes dismissals (which are not involved in the defendant's selection of whether to go to trial). Also, quite often some variable versions appear to have slightly more theoretical merit or conform slightly more to common sense than others, and this judgment becomes one factor in the selection.

4) We favored variables with more observations - that is, variables that have data for more court units and more years.

5) We favored "middle of the road" variations, those that were more like the others in that they were more closely correlated with others and that the results of the regression produced less extreme variables. On a few occasions, this factor led us to change the model well after the regression started.

6) We favored variables that resulted in fewer statistical problems, especially autocorrelation and presence of year effects.

6. <u>Research Findings</u>.

6.1 <u>Trends</u>

Before exploring the regression results, we first describe the gross trends in court congestion and trial rates, which proved to be an interesting topic. Table 1 presents trends for ten years in states where data permit, otherwise for only eight or nine years. There are innumerable ways one can calculate trends, given the large number of congestion and trial rate measures and existence of several means to calculate changes for any one measure. Table 2 presents the change for the state total, such that backlog index, for example, is the total pending in the state divided by the total disposed. Alternative measures, such as the average yearly backlog ratio and trial rate, produce similar results. Table 2 gives the number of different trial rate and congestion measures used here (and in the regressions), and it gives the trend data for both the backlog index and the jury trial rate (based on merit dispositions), as well as the median rate for all measures.

The trends in trial rate are startling. Nine of the eleven states experienced large trial rate declines, generally in the 30% to 50% range, and different measures of trial rate produced similar trends. Only in Connecticut did trial rates increase, although only slightly and probably because in later years the felony court transferred many minor felonies to a lower court division. The trial rate for felony cases in Kansas remained steady (but increased if one includes misdemeanor cases).

Congestion trends are far less consistent; results differed greatly between states and within states different congestion measures often showed quit different results. Overall, there was a slight increase in congestion, with six states (Arizona, Michigan, Ohio, Oregon, and Pennsylvania suffering moderate to large increases, and only one state, Kansas, showing substantial improvement.

The eleven states, of course, constitute too small a sample to provide firm evidence that these trends exist nationwide. It is interesting to note, however, that the civil trial trends are very similar to the criminal trial trends, with large declines in seven of the in eight states with data. Civil congestion trends varied greatly from state to state, although in contrast to criminal cases, congestion was reduced in more states than it increased. (Marvell, Luskin, and Moody 1988: 6-49).

6.2 Analysis results.

As stressed earlier, the strategy in this research is to conduct as many robustness checks and replications as possible, conducting parallel analyses in eleven states, using the Granger-Sims tests for alternate causal direction, and using several measures for congestion and trial rate. This often leads to conflicting results, rendering some conclusions very uncertain; but when results are consistent, we have more confidence in our findings than we if we limited the analysis to one or a few a priori models.

This strategy presents a difficult problem when presenting the results. We conducted hundreds of regressions (presented in Marvell, Luskin, and Moody 1988; Appendices B and D); so we cannot use the usual format of presenting tables with the full results of each regression. Tables 2 through X improvise several mechanisms for presenting the results that do not require an excessive number of tables, yet summarizes the results in a way limits as little as possible the uncertainty due to the wide range of results. Still, numerous other consistency and statistical tests are not incorporated in the tables; they are not mentioned except when they suggest that qualification of the results is needed.

Because the table formats are unlike those used on social science research, we ask the reader to take time to understand the labels and definitions used. The large number of analyses makes significance tests harder than usual to interpret. With numerous analyses, the odds are that some will produce significant coefficients just by chance, even without a real causal connection. This, of course, is less likely if the level of significance is high, for example under .001. On the other hand, the numerous replications - numerous analyses addressing the same topic - mean that small effects, which are not statistically significant (i.e., significant to the .05 level) - can indicate a relationship if all or nearly all the replications produce the same results.[5] Furthermore, these points are confounded by the fact that relationships are more likely to

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5. These two problems - significant results by chance and the fact that non-significant results may be meaningful if found in several similar analyses - are not unique to the approach (multiple replication) taken here. They are encountered in all social science research if one views it as a body of research. There is a tendency to consider significance tests within the confines of individual research project, but in the real world there are numerous scholars addressing the same or similar issues. Some of the many studies on a topic are likely to reach significant results as a matter of chance, even in the absence of any relationship; and several studies may find that a particular variable is not significant, but the cumulative effect of the research may indicate a relationship.

produce significant coefficients when the sample is large; lack of results with small samples (e.g., less than 100) can be difficult to interpret. Table 2 is designed to answer these problems, and to summarize the results of numerous regressions in a way that shows the extent of consistency in results. Note especially that whenever the results for one or more regressions are shown as being significant (or nearly so), the results for other similar analyses are in the same direction - thus confirming the significant results - unless noted otherwise.

The significance levels associated with the F and T Ratios given in Table 2 here are as follows.

			F	T
n	.15	probability	1.46	1.91
m	.10	probability	1.65	2.40
N	.05	probability	1.97	3.02
M	.01	probability	2.66	4.90
X	.001	probability	3.30	7.10

That is, the F Ratio has a probability of .15 of it has a value of 1.46 to 1.65, and so on. The F and T Ratio levels are slightly higher in the states with fewer degrees of freedom (the number of observations, courts times years, less one more than the number independent variables, including the court and year dummies when entered). The T Ratio probability is that for a test of two variables (e.g., the current and lagged versions of a variable). When there are more variables, such as in the tests for year and state dummies, the probabilities for the T values are similar to those for the F values.

Only the capital letters (N, M, and X) represent results that are commonly considered statistically significant. But the lower level results may be important, especially if they are achieved in several analyses with different dependent variables.

Because analyses with larger sample sizes are more likely to find significant results when relationships exist, Table 2 gives the degree of freedom. This information should also be used when interpreting results presented in later tables.

7.3 Granger-Sims Tests.

The research found an uneven and differing relationship between congestion and trials. Table 2 gives an abbreviated summary of the myriad of findings resulting from the Granger-Sims test. We label the tests for different causal directions the "forward" and the "backward" analysis. The forward analysis is the impact of trials on court congestion; trials, the independent variable, are entered as one and two year lags (current year trials are not entered because 1) there may be reciprocal causation for the current year and 2) because spurious causation is common where variables have the same or similar denominators, such as backlog index and trial rate). Space limitations do not permit the listing of other independent variables. They include court congestion lagged one and two years, as required by the Granger-Sims test. Also, the regressions include several control variables which differ between state, but usually include the number of judges, criminal and civil filings, and the fixed effects dummy variables (A full listing of these variables can be found in Marvell, Luskin, and Moody 1988: Appendix B).

The backward analysis is the impact of court congest on trials. The regression is the same as the forward regression, except that the dependant variable is the current year trial measure, rather than congestion.

The regressions in Table 2 include from two to ten measures of congestion (the backlog index and number of pending per capita are always included, and the remaining vary from state to state), and two measures of trials, jury trials per capita and the jury trial rate (two exceptions are noted in the notes in Table 2). Analyses using other trial measures produced similar impacts on the backlog index (we did not combine all congestion measures with all trial measures because to do so would have caused an unmanageably large number of combinations.) Each line in TAble 2, therefore, summarizes the results of four times the number congestion measures, or from eight to forty regressions.

In the forward analyses, there are suggestions in about half the states that more trials lead to less congestion, although the relationships are very weak, except for trial rates in California and Connecticut. Possible contrary findings, again weak, occurred in Iowa and Michigan. The results are similar when using wither trials or trials per capita.

The backward analyses, see also in Table 2, show stronger indications of a relationship: more congestion seems to cause more trials, with very significant results in California, Connecticut, Iowa, Ohio, and Pennsylvania. But there is scant evidence of a similar relationship when using the trial rate variable, with only Iowa showing a significant positive relationship.

The results from the Granger-Sims test, in summary, provide strong evidence against the hypothesis that more trials increase congestion because they overburden the court. On the other hand, the Granger-Sims test provides only scant support for the hypothesis that more trials reduce congestion, either because they signal that the court is striving to dispose of more cases or because the court has improved caseflow procedures. Generally, the impact of trials or trial rates on congestion is not substantial.

Since more congestion quite often causes more trials, but seldom higher trial rates, the results support the court management contention that congestion prompts some courts to increase efforts to dispose of cases (a portion of which will be by trial). But the results do not support the contention that courts try to reduce congestion by increasing the portion of cases going to trial, through caseflow management. Most important, there is virtually no support for the argument that courts respond to congestion by reducing trials, emphasizing guilty pleas.

7.4 Other Regressions.

Table 3 presents the results of "regular" regressions, a term used here to denote the regressions other than Granger-Sims tests. Congestion is the dependent variable, and the number of trials per capita, current and prior year, are among the independent variables (Table 3).[6] The results for trials lagged are roughly consistent with the Granger-Sims tests, with Table 3 presenting results for both the backlog index and the number of pending cases as dependent variables. The current year relationships are sometimes significantly negative even when the Granger-Sims test indicates no causal relationship (especially in Michigan, North Carolina, Ohio, and Pennsylvania). These results, we believe, arise from a combination of two factors: 1) when dispositions increase, reducing the backlog index and pending cases, trials will also increase (unless the trial rate is reduced), and 2) the states with significant results here are those with large sample sizes and, thus, the analysis is sufficiently powerful to capture small impacts. Table T-5 shows that the number of trials is closely associated with the number of dispositions, both total dispositions and merit dispositions (trials plus guilty pleas). That relationship, however, applies only to current year trials; prior year trials have, if anything, a negative relationship with dispositions.

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6. Trial rates (trials divided by dispositions) are not used because of spurious relationships caused by the fact that the denominator of this measure is similar to the denominator of the backlog index.

Tables 4 and 5 summarize the results of the "regular" regressions, with congestion as dependent variables and current and prior year trials among the independent variables. These two tables support the general conclusions resulting from the prior tables. Table 4 concerns the impact of jury trial rates (trials divided by dispositions) on pending per capita.[7] The negative current year relationship seen in Table 2 largely disappears, although the negative lagged relationship remains for states where it was found in the Granger Sims test. The analyses of numerous other congestion measures (Table 5) show little indication that trials affect congestion; several results are significant, or marginally so, but the direction is not consistent.

7.5 Impact of Criminal Filings.

The number of criminal filings probably does not appreciably affect the amount of congestion, but a clear and consistent finding is that more filings lead to more dispositions and more pending cases. The latter relationships, summarized in Tables 7 and 8, are expected, and perhaps mundane, but their magnitude and significance levels are startling. This is especially true in the analysis of dispositions, where the combined coefficient for current year and prior year filings is close to one. It varies between .97 and 1.00 in nine of the twelve states, and the levels

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7. Because the dependent variable does not have dispositions in the denominator, the spurious relationships described in the above footnote are not likely.

of significance are astronomical.[8] The current year coefficients are usually two or ten times as large as the prior year coefficients (see Marvell, Luskin, and Moody 1987:Appendix B), suggesting that by and large the cases are processed fairly routinely. The only states with coefficients below .90, Illinois and Michigan, have complications that explain the comparatively low numbers (see the note in Table 7). In the seven states with data on merit dispositions, filings also have a very strong impact, although the sum of the coefficients is much smaller.

Pending cases, likewise, are greatly affected by filings (Table 8), with highly significant and consistent results. Obviously, more filings lead to more cases in the pipeline. It is interesting that the size of the coefficient is usually very similar to the backlog index (see Table 8).[9] The coefficient is increase in the number of cases pending for each filing added, whereas the backlog index is the number pending per cases disposed (times 100).

The impact of filings on pending cases, however, does not imply that more filings lead to more congestion. As seen in Table 7, the impact is largely limited to the current year; that

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8. The F Ratios for the current and prior year variables are not given because they are obviously significant; they range from 200 to 1000 in most states; the high is 7085 in Ohio and 66 in Michigan.

9. As discussed earlier, one cannot compare backlog indices from different states, especially because of differences in when cases are first counted as pending and in whether inactive cases are included,

is, more filings lead to a bulge in the number of cases being processed, and fewer filings lead to a trough, all without necessarily affecting the time to decision.

In fact, we found in most states that filing volume has virtually no impact on congestion.[10] This is even true of the backlog index: even through pending and disposition statistics are hugely affected by filings, their ratio is not (Table 8). There is a tendency for the backlog ratio to increase in the same year that filings increase, but that is probably due to the bulge in short-term pending cases. Likewise, there is a tendency for the backlog ratio to decline the year after filing increase, due to the increased number of dispositions resulting from the prior year filings. Overall, these two factors tend to balance out, and the sum of the coefficients for current and prior year filings is very small, and often in a negative direction (Table 8, first column).

8. <u>Conclusion</u>.

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10. There are obvious impacts on time frame statistics, such as the percent of cases pending over six months. More filings in the current year reduce the delay measure because there is a bulge of new cases, but more filings in the prior year cause the delay measure to increase because the bulge has progressed to the over-six-month category. See especially the North Carolina and Oregon analyses in Appendix B.

The research found that criminal case processing is dominated by the volume of filings, and most other factors studied have little or no impact. Regressions with dispositions as dependent variables, and filings for the current and prior years among dependent variables, found that the latter usually have combined coefficients of almost one, with extremely high significance levels. Criminal case flow, therefore, acts almost as though cases were funneled through a rigid pipeline: cases come into the system, are processed, and depart on such a regular basis that other factors appear to have little impact. When trying to explain congestion, most regressions had only modest R Squares, and most of the variance explained is probably due to the impact of court unit effects (dummy variables representing differences between courts) rather than the variables of interest.

A major purpose of the research was to illustrate a research strategy that we feel would prove useful in criminology. The first element of the strategy is to develop the theory surrounding the issues to the fullest extent possible, rather than extricate one or a few hypotheses out of a tangle of possible relationships. Although we cannot say for certain, in nearly every area of social research attempts to derive full theories will probably result in complex, intertangled hypotheses, at least as complex as those in the present study, which appeared - or at least researchers presentedit as - a simple issue. One area of criminology research that has undergone sophisticated theoretical scrutiny is macro-level deterrence research, with the result that a large body of research conducted under simplified hypotheses is now considered inadequate (Blumstein, et al. (1978); Cook (1982)).

Second, we urge that researcher concentrate on research designs and statistical analyses that can deal with causal uncertainty. As a practical matter, the designs are largely those advocated in the classic social science methodology texts (e.g. Cook and Campbell 1979), and the statistical methods are often econometric techniques. We emphasize the importance of regression diagnostics, especially influence analysis.

Third, because of the uncertainty inherent in social science research we advocate the use of multiple replications - doing the same or similar research in many different cites or on different data sets, and entering alternate forms of variables. It is probably the reader's experience, as well as ours, that differences in variables and site often produce different results. An accurate presentation of research requires that this uncertainty be elucidate.

A final issue that we continue to wrestle with - and will always wrestle with - is where to draw the line when attempting to adapt social sciences to the complexities of social life. We have gone far beyond the typical research study, but current technology permits further developments, and future technology will certainly produce more. As noted above, although it was possible for us to conducted analysis using all combinations of congestion and trial measures, we limited the combinations explored because we are not aware of software that would make analysis with all combinations practically feasible. Also, of course, we could have expanded the research to include non-linear relationships or the possibility that coefficients may vary with the direction variables are taking. Finally, the study of eleven sites, although unusual in its breadth, probably does not represent enough replications; after all, sample of eleven is not large. It does, however, permit us to illustrate the very important point that it is risky to translate the findings from one site to another.

Table 1 Trends in Trial Rates

		Trial Trends			Delay Trends		
		Number of Measures	Percent Change		Number of Measures	Percent Change	
	Years		Jury Trial Rate	Median Trial Measure		Backlog Index	Median Delay Measure
Arizona	78-87	4	-39%*	-40%	2	28%	38%
California	76-86	8	-51%	-46%	2	-2%	7%
Connecticut	79-87	2	12%*#	13%	6	-1%	-6%
Illinois	75-84	8	-45%	-35%	2	na	na
Iowa	77-87	2	-53%*	-52%	2	7%	9%
Kansas@	79-87	4	0%	2%	4	-23%	-76%
Michigan	78-86	4	-34%	-34%	2	22%	24%
N. Carolina	78-87	4	-48%	-44%	18	-10%	-4%
Ohio	76-86	8	-34%	-31%	2	5%	20%
Oregon	77-87	4	-42%*	-43%	4	58%	38%
Pennsylvania	76-86	8	-20%	-27%	2	45%	36%

Unless otherwise noted, the jury trial rate is the number of jury trials in the state divided by the number of merit dispositions (trials plus guilty pleas). Other trial rate measures use total trials or total dispositions. The backlog index is the number pending divided by the number disposed. The definition of pending cases in Illinois changed such that the backlog index is not comparable over the years.

* The trial rate is based on total dispositions, rather than merit dispositions.

Total trial rate is used instead of jury trial rate.

@ The Kansas figures are for felony cases. The analysis concentrated mainly on all criminal cases (which includes misdemeanors), and here the trial rate increased, 34% for the jury trial rate.

Table 2 Delay and Trial Rates - Granger-Sims Test
(causal direction between trials and delay)

	Degrees of Freedom	Number of Measures	Forward Analysis: Trials Affecting Congestion		Backward Analysis: Congestion Affecting Trials	
			Jury trials per capita[1]	Jury trial rate[2]	Jury trials per capita[1]	Jury trial rate[2]
Arizona	94	2	–	–	.	+nn*
Calif.	319	2	–nnmm	–MMMM	+NNMM	–NN
Conn.	71	8	–mN	–NMM	+nnNM	.
Illinois	121	2	.	.	–	.
Iowa	69	2	+	+mm	+NNNN	+MMMM
Kansas	183	5	.	.	+	.
Michigan	243	2	+mm	.	+	.
N. Car.	263	10	.	.	.	.
Ohio	837	2	.	.	+XXXX	–
Oregon	180	4	–NN*	–n	.	.
Penn.	551	2	–	–	+MMMM	+

1. Total trials in Connecticut
2. Trials divided by merit dispositions (trials plus pleas) except that it is trials divided by all dispositions in Arizona, Connecticut, Iowa, and Oregon.

Key:

No letter - 20% or less of delay measures	n -- Prob. = less than .15
One letter - over 20% of delay measures	m -- Prob. = less than .10
Two letters - over 40% of delay measures	N -- Prob. = less than .05
Three letters - over 60% of delay measures	M -- Prob. = less than .01
Four letters - over 80% of delay measures	X -- Prob. = less than .001

In the Granger-Sims test, independent variables include the "causing" variable lagged one and two years. The probabilities are for the two lags combined, as determined by an F test. The plus or minus sign is that for the larger coefficient of the two variables. Where a letter and sign are given, the sign applies to all analyses, not just that with a letter, except that there is a very slight, far from significant result the other way in the instances marketed by an asterisk (*). Where there is only a sign, without a letter, the analysis only hints a result in that direction (this judgment is based on all analyses, with different delay and trial measures). Dots indicate no evidence of causal connection.

Table 3 <u>Delay and Number of Trials</u>

	Forward Analysis DV = Backlog Index			Backward Analysis DV = Number of Pending		
	Total of Coeff- icients	T Ratios		Total of Coeff- icients	T Ratios	
		Current Year Trials	Prior Year Trials		Current Year Trials	Prior Year Trials
Arizona	−.28	−1.2	−.6	−.79	−1.4	−.3
California[#]	−.13[*]	−1.9	−1.2	−.20[*]	−.4	−1.7
Connecticut	−.83[*]	1.2	−2.1	−1.93[*]	.7	−2.4
Illinois	−.36	−.4	−.6	−.54	.4	−.9
Iowa	.27[*]	1.2	.0	2.99[*]	1.0	.4
Kansas	.01	−.4	.7	.31	.0	.8
Michigan	−.25[N]	−3.0	1.2	−.50[M]	−3.1	1.5
N. Carolina	−.11[N]	−2.9	.8	−.13	−1.7	1.0
Ohio	−.10[*]	−2.5	.7	−.41[*]	−3.3	−0.7
Oregon	−.11	−.8	−.6	−1.91[X]	−.7	−4.1
Pennsylvania	−.31[*]	−3.3	−1.2	−1.94[*]	−4.4	−3.1

Pending cases are cases pending trial.

* The Granger-Sims test indicates backward causation, making the current year results and the T Ratios difficult to interpret.

The backlog index and number pending per capita are dependent variables, and the results presented are for the number of jury trials per capita (all trials in Connecticut). The total of the coefficients is the sum of the coefficients for the current and prior year. The superscripts indicate whether the F ratios was significant (see Table T-1 for the key).

Table 4 Impact of Jury Trial Rate on Pending Cases

	T Ratio for Jury Trial Rate current year	prior year
Arizona	.73	-.13
California	-.21	-3.09^{M}
Connecticut	-.42	-2.71
Illinois	.52	-1.58^{n}
Iowa	2.62^{N*}	1.01
Michigan	-2.61^{N}	.20
Ohio	1.85^{m}	-1.02
Oregon	2.50^{N}	-1.88^{m}
Pennsylvania	.84	-1.08

The results here are for regressions with the number of pending (per 100,000 population) as dependant variables and the jury trial rate (jury trials divided by dispositions or merit dispositions) as independent variables. The significance levels of the T Ratios is according to the definitions in Table 2

* There is significant reverse causation.

Table 5 Trials and Other Measures of Delay

Delay Measures (dependant variable) and States	Jury Trials T Ratios Current Year	Jury Trials T Ratios Prior Year	Jury Trial Rate T Ratios Current Year	Jury Trial Rate T Ratios Prior Year
Percent pending over specific period				
Connecticut (6 mo.)	-.74	.32		
Kansas (12 mo.)	.57	.88		
N. Carolina (4 mo.)	.26	-.80		
N. Carolina (6 mo.)	-1.23	-.91		
Oregon (6 mo.)	-1.05	-1.67^{m}		
Pending, median time				
Connecticut	-.76	-.23		
N. Carolina	.14	-.58		
Pending, mean time				
N. Carolina	-1.78	-2.38^{N}		
Percent disposed over a specific period				
N. Carolina (4 mo.)	.21	.38	-.45	1.90^{n}
N. Carolina (6 mo.)	.55	.54	.60	1.81^{n}
Disposition, median time				
N. Carolina	.78	.52	.90	2.73^{M}
Disposition, mean time				
N. Carolina	1.37	-.45	1.63	1.15^{m}
Time to trial, mean time				
Oregon	1.29	-3.34^{M}	-.16	-1.50

This table presents the results for the analyses similar to those in the prior tables, using other available measures of delay. The delay measures are dependent variables, and the T Ratios are for independent variable jury trials per capita (all trials in Connecticut). The superscripts (as defined in Table T-1) indicate the significance of the F test for the current and prior year variables (not for the prior year only). Results are not given for trial rates (trials divided by dispositions) in the analysis involving pending cases because spurious relationships are possible.

Table 6 Dispositions and Trials

	DV = Total Dispositions			DV = Merit Dispositions		
	Total of	T Ratios		Total of	T Ratios	
	Coeff-icients	Current Year Trials	Prior Year Trials	Coeff-icients	Current Year Trials	Prior Year Trials
Arizona	.3	1.02	-.43			
California	1.2^{X}	5.18	.37	1.0^{X}	4.98	-.16
Connecticut	.3	.29	.08			
Illinois	1.0	.84	.61	2.5^{X}	4.00	1.59
Iowa	.2	-.59	.84			
Kansas	.6	.95	.11	3.3^{*}	4.86	.08
Michigan	1.5^{X}	4.03	-.84	1.9^{X}	6.14	.26
N. Carolina	$.2^{M}$	3.56	-2.61	.1	.60	-.13
Ohio	$.1^{X}$	4.70	-4.07	$.3^{X}$	4.53	-2.02
Oregon	.3	1.48	-.65			
Pennsylvania	$.4^{*}$	5.03	-3.40	1.6^{X}	6.51	.48

The dependent variables are the total number of dispositions and the number of merit dispositions (trials plus guilty pleas). The coefficients and T Ratios are for the number jury trials per capita (total trials in Connecticut) entered as separate independent variables for the current and prior years. The total of the coefficients is the sum of the coefficients for the two years. The superscripts indicate whether the F ratio is significant (see Table T-1 for the key). The asterisk (*) indicates that the Granger-Sims test shows backwards causation, rendering the results for the current year difficult to interpret.

Table 7 <u>Impact of Criminal Filings on Delay</u>

	Backlog Index: Total of Coeff-icients	Backlog Index: T Ratios: Current Year Filings	Backlog Index: T Ratios: Prior Year Filings	Number of Pending Cases: Total of Coeff-icients	Number of Pending Cases: T Ratios: Current Year Filings	Number of Pending Cases: T Ratios: Prior Year Filings
Arizona	$-.04^{N}$	.4	-2.6	$.51^{X}$ (73)	9.8	1.4
California	$-.01^{N}$	1.6	-2.5	$.09^{X}$ (14)	7.0	-2.2
Connecticut	-.08	.2	-1.3	$.37^{X}$ (67)	4.4	.4
Idaho	-.02	-.2	-1.2	$.39^{X}$ (40)	6.8	1.7
Illinois	-.02	-.7	.2	$.34^{X}$ (62)	3.2	1.8
Iowa	$-.01^{N}$	-2.5	1.3	$.30^{N}$ (46)	2.0	2.1
Kansas	.00	-.4	1.2	$.14^{X}$ (16)	4.4	1.7
Michigan	$.06^{X}$	4.6	-1.1	$.49^{X}$ (42)	11.3	-.7
N. Carolina	$.00^{N}$	2.7	-1.9	$.38^{X}$ (36)	11.1	.9
Ohio	$-.01^{X}$	5.7	-7.1	$.23^{X}$ (27)	20.1	-4.1
Oregon	.00	.7	-1.1	$.39^{X}$ (40)	8.7	2.3
Pennsylvania	$.01^{X}$	4.0	-2.1	$.46^{X}$ (47)	15.7	2.2

The results presented here are for two sets of analyses, one with the backlog index (pending divided by dispositions, times 100) as the dependent variable, and the other with the number of pending cases (which like filings is divided by 100,000 population). The total of the coefficients is the sum of the coefficients for the current and prior year. The number in parentheses is the backlog index, given to show its similarity with the total coefficients. The superscripts indicate whether the F ratios was significant (see Table T-1 for the key).

* In California the cases pending are those pending trial, rather than total pending.

In Illinois the filings are at the time of original complaint, rather than after finding of probably cause.

Table 8 Impact of Filings on Criminal Dispositions

	Total Dispositions			Merit Dispositions		
	Total of Coeff- icients	T Ratios: Current Year Filings	T Ratios: Prior Year Filings	Total of Coeff- icients	T Ratios: Current Year Filings	T Ratios: Prior Year Filings
Arizona	.93	13.3	8.2	na		
California	.97	15.8	6.9	.93	15.5	7.2
Connecticut	.97	12.8	3.1	na		
Idaho	.99	16.0	2.5	na		
Illinois*	.89	11.2	5.6	.12	2.2	1.6
Iowa	1.00	13.2	1.3	na		
Kansas	.98	26.2	.8	.51	11.5	.5
Michigan#	.83	8.8	4.9	.58	9.4	4.8
N. Carolina	.97	23.3	9.2	.65	12.6	5.5
Ohio	1.00	46.1	21.1	.80	31.1	14.2
Oregon	.97	20.2	6.1	na		
Pennsylvania	.98	14.3	12.2	.39	11.8	6.1

The results presented here are for two sets of analyses, one with total dispositions as the dependent variable, and the other with merit dispositions (trials plus guilty pleas). The total of the coefficients is the sum of the coefficients for the current and prior year. The F tests indicate that the combined effect of the two variables is significant to at least the .0001 level in all cases except for merit dispositions in Illinois, where is significant to the .01 level.

* In Illinois the filings are at the time of complaint; whereas in other states it is after a finding of probable cause.

In Michigan filings exclude, but dispositions include, cases refiled after returning from inactive status.

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