Document Title: Study of the Determinants of Case Growth in U.S. Federal District Courts, Final Report

Author(s): William F. Shughart, II ; Gokhan R. Karahan

Document No.: 204010

Date Received: February 2004

Award Number: 2000-IJ-CX-0042

A Study of the Determinants of Case Growth in U.S. Federal District Courts*

Sponsored by the National Institute of Justice
Grant # 2000-IJ-CK-0042

Principal Investigators:

William F. Shughart II and Gökhan R. Karahan

The University of Mississippi
Department of Economics
P. O. Box 1848
University, MS 38677-1848

Final Report
July 2003

FINAL REPORT
Approved By: M Battle
Date: 12/4/03

* John Barkoulas contributed significantly to the time series forecasting results reported herein. We also benefited from the research assistance of LiuQuing Mai and Yuncan Cen. Three peer reviewers supplied helpful comments and suggestions. As is customary, however, the principal investigators take full responsibility for the final product.

EXECUTIVE SUMMARY

This study analyzes the determinants of the explosion in the caseload of the U.S. federal district courts that commenced in 1960. Prior to that time, the federal judiciary's caseload grew at a rate averaging about 1.1 percent per year. Thereafter, the growth rate rose to 2.9 percent annually, nearly tripling the demands on the federal courts. In order to cope with a caseload that has been growing faster than the U.S. population, the budget of the federal judiciary has expanded by 170% over the past decade – about four times the corresponding increase in total government spending. The mounting burden on sitting federal judges has been driven primarily by an upsurge in civil cases, which have increased sevenfold since 1940. While criminal case filings have increased as well over time, especially so during the past few years, their volume has only doubled over the same period.

The analyses reported herein offer important insights into the judiciary's caseloads problem. First, using best-practice econometric techniques, we supply forecasts of future demands on the federal courts that are more accurate than those available previously. Forecast errors are reduced by taking account of the time series properties of the case data. In particular, strong evidence that the time series of federal civil and criminal cases are nonstationary (have unit roots) implies that the projections produced by deterministic linear trend models are unreliable.

Based on estimates of autoregressive time series models of civil and criminal cases, using annual data for the years 1904 to 1998 as well as several subperiods thereof, we generate out-of-sample forecasts through 2020 that differ substantially from the forecasts of the Judicial Conference of the United States. To illustrate, while the Judicial Conference estimates that total federal case filings will almost triple between 2000 and 2020, rising from 364,800 cases to 1,060,400 cases annually, our forecasts suggest that the burden on the federal courts will not reach half that number: our most generous estimate for 2020 is a total of 444,074 cases. Indeed, our out-of-sample forecasts for 1999, 2000, 2001 and 2002 are much closer to the numbers of case filings actually observed in those years than are the forecasts of the Judicial Conference of the United States (our estimates deviate by only about 10,000 cases per year, on the average, from the realized numbers). The following tables compare our out-of-sample forecasts with those contained in the Judicial Conference's *Long Range Plan for the Federal Courts* for five-year intervals, 2000–2020.

Five-Year Interval of Forecasts for Total Case Filings

Year	This Study						Observed Civil	JCUS 1940–95
	1904–98	1948–98	1960–98	1940–98	1940–95	Average of Forecasts		
2000	324,567	319,657	322,981	323,577	313,646	320,885	321,669	364,800
2005	334,462	336,471	350,662	345,771	328,893	339,252	NA	463,600
2010	350,522	361,305	381,487	362,216	348,306	360,767	NA	610,800
2015	366,821	382,708	409,940	384,750	366,981	382,240	NA	802,800
2020	382,740	408,259	444,074	411,714	385,903	382,240	NA	1,060,400

Five-Year Interval of Forecasts for Civil Case Filings

Year	This Study						Observed Civil	JCUS 1940–95
	1904–98	1948–98	1960–98	1940–98	1940–95	Average of Forecasts		
2000	259,946	257,592	259,347	258,240	263,855	259,796	259,517	317,000
2005	264,978	272,354	284,237	267,623	278,837	273,606	NA	409,400
2010	277,241	294,525	311,477	284,511	296,734	292,898	NA	548,000
2015	291,113	314,264	334,686	301,196	314,438	311,139	NA	731,100
2020	304,601	336,962	365,739	319,020	332,067	331,678	NA	976,500

Five-Year Interval of Forecasts for Criminal Case Filings

Year	This Study					Observed Criminal	JCUS 1940–95
	1904–98	1960–98	1940–1998	1940–1995	Average of Forecasts		
2000	60,553	59,157	58,509	46,010	56,057	62,152	47,800
2005	61,150	62,824	60,553	47,097	57,906	NA	54,200
2010	63,128	66,490	62,597	48,183	60,100	NA	62,000
2015	65,603	70,156	64,641	49,269	62,417	NA	71,700
2020	67,594	73,822	66,686	50,356	64,614	NA	83,900

The last of these tables suggests that both the JCUS and this study tend to underestimate future criminal caseloads. This finding is independent of the different baseline time horizons employed and the nature of the autoregressive process modeled. The overall evidence points toward the presence of significant nonlinearities in the federal criminal case series and therefore the potential for improved forecasting performance using an appropriately specified non-linear model. On the other hand, the recent increase in criminal case filings – by 4,218 additional cases from 2001 to 2002 – is quite unusual by historical standards.

The most recent data indicate that a total of 274,841 civil cases and 341,293 total cases actually were filed in 2002. Our forecasts deviate from these numbers by 6,180 cases (2.24 percent) and 22,763 cases (6.67 percent), respectively. Both of these observed figures are much lower than JCUS predicts for calendar year 2000.

The study's second contribution is to specify and estimate multivariate econometric models of the determinants of civil case filings over time and across geographic space using panel data techniques. These empirical models are run on three alternative datasets consisting of observations on statewide, district-wide, and circuit-wide U.S. civil, private civil, and total civil cases per capita, over the period 1960 to 1998. We find that federal civil case filings are influenced significantly by the socioeconomic characteristics of the relevant state, district, or circuit. In particular, holding other things constant, civil cases are positively related to per capita income, population density, the percentage of the population that is nonwhite, the unemployment rate, and the size of government.

We also find that the explanatory power of the panel data models is improved substantially by controlling for the geographical locations of the federal courts: other things being equal, except for the District of Columbia Circuit, significantly more civil cases are filed per capita in the Fifth Circuit than elsewhere. The fact that fixed effects models explain more of the variations in civil case filings than alternative models that do not take geographical location into account provides preliminary evidence pointing to the efficiency gains potentially flowing from reassessing the cross-circuit and cross-district allocation of judgeships and other scarce resources of the federal courts.

The importance of caseload management is reinforced by analyses of the impact of criminal cases on civil cases. We find that, holding constant the time between the filing and disposition of federal criminal cases, civil cases are disposed of more expeditiously in districts where there are more authorized judgeships per capita. On the other hand, holding authorized judgeships per capita constant, we also find that criminal cases impose a negative externality on civil cases: the more time federal judges take to dispose of criminal cases in a given district, the longer is the elapsed time between the date of filing and the date of disposition of civil cases. Moreover, the time to disposition of civil cases tends to be longer in districts where greater percentages of the criminal caseload involve alleged drug and immigration law violations.

Despite our finding that the time devoted to disposing of criminal cases slows the speed at which civil cases move through the courts, we also report evidence supporting the hypothesis that the time series of civil and criminal cases and the time series of authorized federal judgeships are not cointegrated. Two conclusions follow from this evidence. One is that the numbers of civil and criminal cases filed in the federal courts are generated by independent stochastic processes. In other words, information about the number of criminal cases filed in a given year does not allow one to predict the civil caseload, and vice-versa. The other conclusion is that the number of judges authorized to hear federal cases bears no statistically significant relation to the total caseload of the federal courts. Forces external to the courts, such as the political process, evidently play greater roles than caseload demands in determining the size of the federal judiciary.

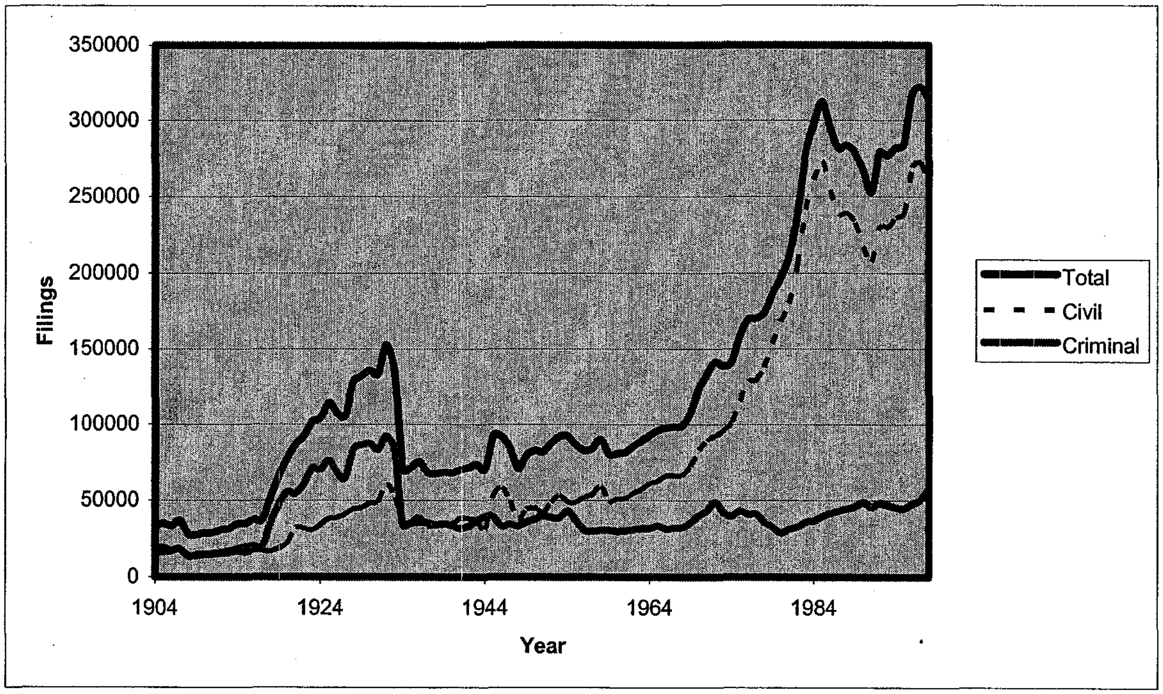
In sum, this study provides new, and we believe, more accurate forecasts of the future caseloads of the U.S. federal district courts than have been available hitherto. Grounded in best-practice econometric techniques, we project that the federal courts can at most expect to face a caseload of 444,075 civil and criminal cases by 2020, not a total exceeding one million such cases. The study also supplies evidence that the distribution of federal civil cases across states, districts, and circuits can be explained by empirical models that include standard socioeconomic variables, such as income, population density, and race, along with variables that control for fixed effects associated with the courts' geographic location. We thus present models that policymakers can use to forecast the future caseloads on the federal court system as a whole as well as to estimate how the total caseload can be expected to be distributed geographically.

Perhaps the most important policy implication of this study, however, is to demonstrate the problematic nature of caseload forecasts using models incorporating simple linear trends. Indeed, once the time series properties of federal civil and criminal case filings are taken into account, there is no evidence of a linear trend in the data. The failure appropriately to model the time series of cases explains why previous forecasts, such as those contained in the *Long Range Plan for the Federal Courts*, have tended to overestimate the future demands on the federal courts. Armed with the more accurate forecasts presented here, policymakers can more confidently assess the need for additional judgeships and, moreover, can address what seems to be a more pressing problem, namely the possible misallocation of judgeships across circuits and districts.

I. Introduction

The federal judiciary's caseload, especially its civil caseload, has grown dramatically since the 1960s. Between 1904 and 2002, civil cases grew at an average annual rate of about 3 percent. This growth rate is far greater than the growth of population and per capita income over the same period. By contrast, criminal case filings have increased much more slowly over the past 60 years – by about 0.5 percent annually, on the average. Consequently, as shown in Figure 1, the variation in the federal judiciary's total caseload is largely explained by variations in civil case filings. While it is possible that judicial resource allocation decisions are influenced by the composition of cases (civil versus criminal), it is clear that civil cases are where the action is in the U.S. federal courts.

Figure 1. Case Filings in U.S. Federal District Courts, 1904–1998



The long-run perspective of Figure 1 masks changes in the caseloads of the federal courts that exhibit much more variability over shorter time intervals. As reported in Table 1, for example, civil case filings increased by more than 103 percent during the 1980s, a time when criminal cases were rather markedly on the decline. On the other hand, criminal case filings have outpaced those of civil cases in recent years. It is nevertheless true that, for much of the twentieth century at least, the average annual rate of increase in civil caseloads has been significantly greater than that of criminal cases (see Table 2). Put in their starkest terms, criminal case filings have doubled over the past 60 years, while civil cases have increased sevenfold. The explosion in civil cases, a disproportionate contributor to the challenges now facing the federal courts, merits further study.

Table 1. Case Filings in U.S. Federal District Courts, by Decade

	Case Filings				Percentage Change from Previous Year		
Year	Civil	Criminal	Total		Civil	Criminal	Total
1940	34,734	33,401	68,135		—	—	—
1950	45,085	37,720	82,805		29.80%	12.93%	21.53%
1960	51,063	29,828	80,891		13.26%	–20.92%	–2.31%
1970	82,665	39,959	122,624		61.89%	33.96%	51.59%
1980	167,871	28,921	196,792		103.07%	–27.62%	60.48%
1990	217,421	48,904	266,325		29.52%	69.10%	35.33%
2000	259,517	62,152	321,669		19.36%	27.09%	20.78%
2002	274,841	66,452	341,293		5.90%	6.92%	6.10%

Table 2. Exponential Short-Run Growth Rates in Case Filings, Selected Intervals

Period	Civil	Criminal	Total
1940–1949	3.14%	0.50%	1.94%
1950–1959	2.28%	–3.52%	–0.10%
1960–1969	3.90%	1.38%	3.02%
1970–1979	6.83%	–2.39%	4.44%
1980–1989	3.57%	5.19%	3.81%
1990–2002	1.93%	3.12%	2.15%
1904–2002	3.09%	0.53%	2.15%
1960–2002	4.46%	1.44%	3.71%
1980–2002	1.15%	3.10%	1.46%

Rising case volumes have placed heavy demands on the resources of the judicial branch. Although the budget of the federal judiciary represents less than one-fifth of one percent of the

U.S. budget, it has grown over the past decade by 170%, a rate about four times the increase in total government spending.

In order to assist the federal judiciary in meeting the expected demands of the twenty-first century, the Judicial Conference of the United States (JCUS) recently produced a *Long Range Plan for the Federal Courts* (Judicial Conference of the United States 1995). That plan contained projections of future caseloads based on simple linear trend models.[1] Although such forecasts are fairly accurate over short time horizons, the weaknesses of such models are abundant. One could estimate what next year's caseload will be based on caseloads in earlier years, but linear projections will generally cause policymakers to err when making long-run resource-allocation decisions. For example, had it been used at the time, linear trend analysis would in all likelihood have prompted Congress to create many more judgeships and supporting personnel in response to the unprecedented rise in the criminal case filings during the Prohibition Era.[2] Linear trend estimates are sensitive to changes in the underlying determinants of caseloads and they consistently miss turning points in the data, overestimating growth when caseloads start to fall and underestimating it when caseloads start to rise.

In the not too distant future, the federal judiciary will no longer be able to play the game of the "mules and wagons".[3] Public concern with government spending will force the judicial system's planners to become more circumspect in assessing the system's human resource and

[1] The Judicial Conference of the United States may prepare other forecasts for internal use, including projections based on more sophisticated econometric techniques, but the forecasts published in the *Long Range Plan* are, to our knowledge, the only forecasts available to the public.

[2] The total number of criminal case filings reached its peak of 92,174 cases in 1932. After Prohibition ended, the criminal case volume declined steadily and, since 1970, has averaged about 40,000 criminal cases per year (Posner 1996, p. 391).

[3] The story is about a farmer trying to carry loads for himself and his neighbors. As the loads increase over time, the mules cannot carry the loads, and so the farmer adds a pair of mules, and later, one more wagon to his caravan. This goes on for a while and, at the end, he realizes that the lead pair of mules and the wagon is out of sight. Yet, he still adds more mules and wagons to solve the increased loads problem (Clark 1994).

physical capital needs. This may be possible by adopting more efficient management and budgeting processes. However, internal management and organization are only a part of the solution to the problem of increased caseloads. The demand for court services is not independent of the socioeconomic environment that surrounds the courts. That is the focus of this project. Specifically, this report specifies and estimates models of the determinants of the demand for federal district court cases. The contribution of this study is the following:

- It employs a more comprehensive dataset than any other previous study, a dataset comprising about 3,000 observations for each variable.
- It presents forecasts of annual case filings using best-practice econometric models and methods not employed by the Judicial Conference of the United States in its *Long Range Plan for the Federal Courts* (1995).[4]
- It constructs and exploits a dataset based on the annual case volumes of individual federal district courts. This minimizes the loss of information due to annualized aggregation of the case volumes across federal district courts at the state and circuit levels. However, empirical models are also presented using the statewide and circuit-wide data. Moreover, this study shows that the results are sensitive to the level of data aggregation.

In sum, this study provides new, and we believe, more accurate forecasts of the future caseloads of the U.S. federal district courts than have been available hitherto. Grounded in best-practice econometric techniques, we project that the federal courts can at most expect to face a caseload of 444,074 civil and criminal cases by 2020, not a total exceeding one million such

[4] In a footnote, the authors of the *Long Range Plan* write that "early investigations of alternatives to [linear] regression, notably ARIMA [autoregressive integrated moving average] modeling, generally produced projection results consistent with those obtained here" (Judicial Conference of the United States 1995, p. 145). Our findings cast doubt on that conclusion.

cases. The study also supplies evidence that the distribution of federal civil cases across states, districts, and circuits can be explained by empirical models that include standard socioeconomic variables, such as income, population density, and race, along with variables that control for fixed effects associated with the courts' geographic location. We thus present models that policymakers can use to forecast the future caseloads on the federal court system as a whole as well as to estimate how the total caseload can be expected to be distributed geographically.

II. Analysis of Case Growth Using Annual Time-Series Data

A. Univariate Time-Series Analysis

In this section we analyze the stochastic behavior of aggregate annual civil and criminal case filings spanning the 1904–1998 period, for a total of 94 yearly observations.[5] The objective is to ascertain whether the series in question are realizations of stationary versus nonstationary stochastic processes. Such a distinction regarding the underlying data generating process is crucial to the development of a univariate time-series forecasting model. Conceptually, for a nonstationary series an exogenous shock persists forever (has a permanent effect), but dissipates over time for a stationary series. The presence (or not) of a unit root in the autoregressive polynomial of a time series is critically important for forecasting purposes.

[5] We use raw numbers of cases throughout the analysis, ignoring the fact that cases differ considerably in terms of the amount of judges' time they absorb. That alternative is captured in so-called weighted filings, a workload metric in use since 1946 and updated most recently in 1993 that accounts for the different amounts of time judges require to resolve various types of civil and criminal actions. See, e.g., Southern District of Texas, "Explanation of Selected Terms", http://www.txs.uscourts.gov/statistics/cmsexp199.htm. It turns out, however, that in general we cannot reject the hypothesis of a one-to-one correspondence between "weighted" and "unweighted" cases either in time series or across federal judicial districts. This is true even in the Southwestern United States, where the correlation between weighted and unweighted cases over recent years (1988–2002) exceeds 0.9. What is more important, the various case weights are determined retrospectively. Forecasts of future caseloads therefore are essential for predicting future workloads and, in any event, "an increasing caseload translates into an increasing workload" (see "The Third Branch: Five-Year Retrospective Takes Stock", http://www.uscourts.gov/ttb/dec98ttb/stock.html, p. 4).

We subject our time series of civil and criminal cases to a variety of unit-root tests. These tests differ in terms of the parameterization of the alternative hypothesis or in how they define their null and alternative hypotheses. The reason for using a variety of unit-root tests is to obtain robust evidence regarding the degree of smoothness in the series under study. A brief description of unit-root tests is provided in Appendix 1.

Results of the univariate time-series analyses. A number of standard diagnostic tests are applied to each series in their levels, log-levels and first-logarithmic differences (growth rates). The detailed results are presented in Appendix 1. Table A.1.1 reports the ADF-GLS (augmented Dickey-Fuller, generalized least squares) test results. Civil cases, both in their levels and log-levels, are found to be nonstationary, but their growth rates (first-logarithmic differences) appear to be realizations of a stationary process. The findings for the criminal cases are similar. These results are reinforced by the Kwiatkowski-Phillips-Schmidt-Shin (KPSS) findings reported in Table A.1.2. The stationarity null hypothesis is strongly rejected for the level series but it is not rejected for their growth rates.

Table A.1.3 reports the structural-break tests, which allow for a double shift in the mean of the series as an alternative to the unit-root null. The δ estimates, indicating the importance of mean shifts, are uniformly distinguishable from zero. None of the unit-root test statistics approach the 5% critical value of –5.49 for the series in levels, indicating that unit roots in the civil and criminal cases cannot be rejected. Accounting for two level shifts therefore does not strengthen the evidence against the unit-root null.

Table A.1.4 reports the modified log-periodogram test results, which allow for a fractional exponent in the differencing process of the series. For civil cases in levels the results depend upon whether a trend is allowed or not. For the with-constant model, the degree of differencing appears to be in excess of a unit root (order of integration greater than one) but it appears to be around 0.5 (significantly less than one) for the with-trend model. However, the evidence obtained for the log-level series as well as for the growth rates series is consistent with unit-root behavior. Evidence in support of a single unit root is obtained for the criminal case series. Overall, the unit-root hypothesis appears to be robust to fractional alternatives.

Conclusions from the univariate time-series analyses. We conclude from the foregoing analysis that the time series of both civil and criminal cases appear to be realizations of nonstationary stochastic processes, which is consistent with the dynamic structure of many macroeconomic time series. Therefore, the presence of a unit root is an important element in modeling the temporal behavior of these series. The implication of this finding is that, in order to develop an adequate forecasting model for a time series, its underlying data generating process must be approximated and estimated as closely as possible. In the case of deterministic-trends models used in the *Long Range Plan*, it is assumed that the behavior of the series will be governed by these trends indefinitely into the future. However, such an assumption is precarious as these trends may be subject to shocks over time, which is a very realistic possibility for the caseload series. On the contrary, nonstationary models assume that shocks (e.g., economy-wide, macroeconomic shocks) continuously hit the series with permanent effects on its behavior; in such cases, trends are stochastic and nonstationary in nature. Therefore the dynamic behavior of the series is different depending upon whether trends are deterministic or stochastic in nature. If a series is nonstationary (has a unit root) but it is modeled as stationary around some deterministic trend, then such a model is misspecified and its estimates are inconsistent with direct implications for forecasting performance. There are important differences between trend-stationary and unit-root processes in terms of forecasts of the series, variance of the forecast error, dynamic multipliers, and transformations needed to achieve stationarity (see Hamilton 1994, pp. 438–44).

Next, we analyze the forecasting performance of a unit-root model relative to alternative baseline models used in the literature in general and the *Long Range Plan* in particular, which are primarily (deterministic) trend-based models, such as linear, log-linear, and exponential, to name the most common. In these models trends are of deterministic nature whereas in the non-stationary (unit-root) models trends are stochastic.[6]

Long-horizon forecasting is always a difficult task that requires continuous monitoring as, among other factors, the likelihood of parameter and other structural changes increases with the passage of time. Often a variety of forecasts is produced by alternative models. Because each model may capture part of the truth (i.e., the underlying data-generating process), combinations of forecasts frequently are superior to individual forecasts, a conclusion that is in general supported by the empirical evidence. Additionally, even though one model may dominate another model in terms of forecasting effectiveness on the basis of one forecasting criterion or metric, that dominance may not be invariant to the usage of alternative forecasting metrics ("one model does not always win"). For each forecasting experiment at hand, the relevant loss functions should be chosen and on the basis of those loss functions competing model forecasts should be evaluated. Higher moments should also be considered (conditional density forecasts).

B. Univariate Time-Series Forecasting

In this section, we generate out-of-sample forecasts for three time series (civil cases, criminal cases, and total cases) using nonstationary (unit-root) univariate models. The extensive

[6] The term deterministic relationship implies that, from the knowledge of the value of one variable, we are able to predict the value of another variable. For instance, suppose that Y = 2 + 10X. If X = 3, then Y = 2 + 10(3) = 32. However, owing to the omission of other relevant explanatory variables thought to influence Y, measurement errors, and so on, we may not be able to predict Y exactly. A stochastic relationship includes an additional term to account for the possible errors of prediction. "Stochos", in Greek, means bull's eye.

unit-root tests employed earlier strongly suggest that the time series possess stochastic trends, in particular, a single unit root.[7] After first-differencing the series, we model its short-run dynamics by fitting an autoregressive (AR) model using Box-Jenkins methods.[8]

The AR orders are selected on the basis of statistical significance of the coefficient estimates and Q-statistics for serial dependence: the most parsimonious representation is chosen so as to ensure serial independence for at least 12 lags in the corresponding residual vectors. For each series, we estimate autoregressive integrated (ARI) models of order (p, 1) using our sample observations and generate dynamic forecasts over the period 1999–2020.[9] To address the potential temporal instability of the underlying data generating process, we estimate AR models for five different sample periods: the full sample period (1904–1998), the post-World War II period (1948–1998), the period spanning the years 1960 through 1998 (these three are presented in Appendix 1), the 1940–1998 period and the 1940–1995 period (the latter two are presented in Appendix 4). The AR model estimates obtained from each sample period are then used to generate forecasted values for 1999–2020. Table A.1.5 presents the autoregressive models chosen for the civil, criminal, and total cases. The regression models used to generate the time series forecasts are shown in Table A.1.6. The out-of-sample dynamic forecasts generated by each model and for the first three series, 1904–1998, 1948–1998 and 1960–1998, are presented in Table A.1.9, the

[7] A process with a single unit root is referred to as integrated of order one, denoted by $I(1)$. More generally, a series is said to be integrated of order d, denoted by $I(d)$, if it is rendered stationary after differencing it d times.

[8] An extension of our approach could also consider moving average (MA) terms in modeling the short-run dynamical behavior of the series. However, adding a MA component would add complexity to the forecasting experiment while the forecasting improvements are doubtful, especially over longer horizons. An AR model with sufficient lag structure can very well approximate the MA components of the series.

[9] Dynamic forecasts are multi-step forecasts, where forecasts computed at earlier horizons are used for the lagged dependent variable terms at later horizons. For example, the forecasted value computed for time T will be used as the first-period lag value for computing the forecast at time $T + 1$, and so on.

forecasts for the 1940–1998 series are presented in Table A.4.6 and, finally, the forecasts for 1940–1995 are presented in Table A.4.9.[10]

To ensure that a linear structure is adequate to capture the essential features of our series, we perform the BDSL test suggested by Brock, Dechert, Scheinkman and LeBaron (1996) to the AR pre-filtered series.[11] Over the full sample period, 1904–1998, we observe the presence of an unspecified omitted nonlinear structure. For the sample period 1948–1998, evidence of nonlinearities is obtained for the criminal cases and weakly so for the civil cases. The overall evidence points toward the presence of significant nonlinearities in the series and therefore the potential for increased forecasting performance using an appropriately specified nonlinear model.

From the results presented in Tables A.1.9 and A.4.1 through A.4.9, we are unable to find evidence that civil and total cases will reach the levels projected by *Long Range Plan for the Federal Courts* even after accounting for the same time frame. The following tables summarize our forecasts for the three different series analyzed by this study (in five-year intervals). For purposes of comparison, the corresponding JCUS forecasts are shown within the same tables.

Our out-of-sample forecasts for total case filings are substantially lower than JCUS projects, even when we use the same 1940–1995 series as a baseline (see Table 3).[12] Table 4 pre-

---

[10] A linear trend was added to the ARI (unit-root) models. However, in none of the estimated models was the linear trend variable statistically significant.

[11] The BDSL-test checks the null hypothesis of independent and identical distribution (i.i.d.) in the data against an unspecified departure from i.i.d. A rejection of the i.i.d. null hypothesis in the BDSL-test is consistent with some type of dependence in the data, which would result from a linear stochastic system, a nonlinear stochastic system, or a nonlinear deterministic system. Under the null hypothesis, the BDSL-test statistic asymptotically converges to a standard normal variate. We applied the BDSL-test to the pre-whitened series with the AR filters in Table A.1.7 for the full sample period and for embedding dimensions of $m = 2, 3, 4$ and 5. For each m, ϵ (the distance) is set to 0.5, 1.0, and 1.5 standard deviations (σ) of the data. We perform the BDSL-test only for the full sample (1904–1998) and the sample spanning 1948–1998. We did not estimate the BDSL-statistic for the sample covering 1960–1998, as the number of observations decreases substantially, thus affecting the power and therefore the reliability of the test. The BDSL-test statistics are reported in Table A.1.7. The critical values corresponding to the sample sizes of the series were obtained from Kanzler (1999) in Table A.1.8.

[12] The JCUS estimates for total filings are the sum of the two estimates (civil and criminal). We treat total cases as a different series as the underlying autoregressive processes for civil and criminal cases are different. However, if we

sents our forecasts for civil case filings. Once again, the JCUS forecasts are substantially higher than ours. The most recent data indicate that a total of 274,841 civil cases and 341,293 total cases actually were filed in 2002. Our forecasts deviate from these numbers by 6,180 cases (2.24 percent) and 22,763 cases (6.67 percent), respectively. Both of these observed figures are much lower than JCUS predicts for calendar year 2000.

Table 3. Five-Year Interval of Forecasts for Total Case Filings

Year	1904–98	1948–98	1960–98	1940–98	1940–95	Average of Forecasts	Observed Civil	JCUS 1940–95
2000	324,567	319,657	322,981	323,577	313,646	320,885	321,669	364,800
2005	334,462	336,471	350,662	345,771	328,893	339,252	NA	463,600
2010	350,522	361,305	381,487	362,216	348,306	360,767	NA	610,800
2015	366,821	382,708	409,940	384,750	366,981	382,240	NA	802,800
2020	382,740	408,259	444,074	411,714	385,903	382,240	NA	1,060,400

Table 4. Five-Year Interval of Forecasts for Civil Case Filings

Year	1904–98	1948–98	1960–98	1940–98	1940–95	Average of Forecasts	Observed Civil	JCUS 1940–95
2000	259,946	257,592	259,347	258,240	263,855	259,796	259,517	317,000
2005	264,978	272,354	284,237	267,623	278,837	273,606	NA	409,400
2010	277,241	294,525	311,477	284,511	296,734	292,898	NA	548,000
2015	291,113	314,264	334,686	301,196	314,438	311,139	NA	731,100
2020	304,601	336,962	365,739	319,020	332,067	331,678	NA	976,500

In Table 5, we report our forecasts for criminal cases. Although our forecasts and JCUS forecasts are fairly close, it appears that both the JCUS and this study tend to underestimate criminal cases. This finding is independent of the different baseline time horizons employed and the nature of the autoregressive process modeled. As stated earlier, the overall evidence points toward the presence of significant nonlinearities in the series and therefore the potential for in-

were to add up the two numbers for calendar year 2000, the highest forecast we obtain would be about 320,499 cases (sum of the 259,946 civil and 60,553 criminal case forecasts for the 1904–98 series).

creased forecasting performance using an appropriately specified non-linear model. In fact, 66,452 criminal cases were observed in 2002, a number which our forecasts suggest will not occur until the 2010s. That forecast error may be a warning of things to come. If criminal case filings were to continue to increase at the same pace – by 4,218 additional cases – as they did from 2001 to 2002, simple arithmetic would tell us that by 2020, we will neither have 73,822 cases (the highest number forecast by this study) nor 83,900 cases (the number forecast by JCUS), but about 140,000 cases, a number twice as high as either projection. On the other hand, the recent increase in criminal case filings is quite unusual by historical standards.

Table 5. Five-Year Interval of Forecasts for Criminal Case Filings

Year	1904–98	1960–98	1940–1998	1940–1995	Average of Forecasts	Observed Criminal	JCUS 1940–95
2000	60,553	59,157	58,509	46,010	56,057	62,152	47,800
2005	61,150	62,824	60,553	47,097	57,906	NA	54,200
2010	63,128	66,490	62,597	48,183	60,100	NA	62,000
2015	65,603	70,156	64,641	49,269	62,417	NA	71,700
2020	67,594	73,822	66,686	50,356	64,614	NA	83,900

Tables 6 through 8 report validation estimates for the three case series analyzed. Alternately treating each of the five sample periods as a baseline, we generate "out-of-sample" forecasts for 1999, 2000, 2001 and 2002, and compare the forecasted values with the actual numbers of case filings observed in those years. As shown in Table 6, the forecasts for total case filings exhibit an average absolute deviation of about 10,000 cases annually, reinforcing the accuracy of the time series models. The corresponding forecasts for civil cases reported in Table 7 do not differ markedly from those reported in Table 6.

Table 8 shows the annual deviations between observed and forecasted criminal cases. Our models evidently do not do as well in forecasting criminal cases as they do in forecasting civil or total cases. This relatively poor performance is undoubtedly due to the unusually large number of criminal case filings in 2002. If this trend continues, then, as suggested earlier, we must consider alternative models other than those estimated here and by JCUS.

Table 6. "Out-of-Sample" Forecasts for 1999, 2000, 2001 and 2002 (Total Cases)

		Forecasted Total Case Filings					Average Value of Forecasts
Year	Observed	1904–98	1948–98	1960–98	1940–98	1940–95	
1999	319,522	315,351	305,712	306,675	311,677	306,265	309,136
2000	321,669	324,567	319,657	322,981	323,577	313,646	320,886
2001	313,041	324,282	320,537	325,247	327,532	314,692	322,458
2002	341,293	323,230	318,363	324,504	319,207	318,530	320,767
Average Annual Deviation		9,093	11,562	10,789	11,583	11,424	10,278
Average Percentage Annual Deviation		2.77%	3.52%	3.31%	3.54%	3.46%	3.13%

Table 7. "Out-of-Sample" Forecasts for 1999, 2000, 2001 and 2002 (Civil Cases)

		Forecasted Civil Case Filings					Average Value of Forecasts
Year	Observed	1904–98	1948–98	1960–98	1940–98	1940–95	
1999	260,271	249,785	246,381	245,534	247,635	269,132	251,693
2000	259,517	259,946	257,592	259,347	258,240	272,027	261,430
2001	250,907	258,870	257,745	261,180	257,659	256,787	258,448
2002	274,841	252,652	252,142	255,885	252,215	260,271	254,633
Average Annual Deviation		10,267	11,338	11,034	10,823	10,455	9,560
Average Percentage Annual Deviation		3.86%	4.27%	4.18%	4.07%	3.97%	3.60%

Table 8. "Out-of-Sample" Forecasts for 1999, 2000, 2001 and 2002 (Criminal Cases)

		Forecast Criminal Case Filings				Average Value of Forecasts
Year	Observed	1904–98	1960–98	1940–98	1940–95	
1999	59,251	60,098	58,424	58,100	45,793	55,604
2000	62,152	60,553	59,157	58,509	50,363	57,146
2001	62,134	61,973	59,891	58,918	46,010	56,698
2002	66,452	62,234	60,624	59,326	46,228	57,103
Average Annual Deviation		1,706	2,973	3,784	15,399	5,860
Average Percentage Annual Deviation		2.65%	4.65%	5.93%	24.52%	9.26%

C. Long-Memory Forecasting

As there is some evidence of a fractional order of integration in the civil cases time series, we generate out-of-sample forecasts based on an estimated ARFIMA (autoregressive fractionally integrated moving average) model.[13] Such a model incorporates the specific nonlinearity and represents a flexible and parsimonious way of modeling both the short- and long-term dynamical properties of the series.[14]

[13] The degree or order of integration for a time series indicates the degree of differencing required to render the time series stationary. The order of integration can be integer (can assume only integer values) or more generally fractional (can take any value on the real line). The most celebrated time series process is that of a pure random walk which is integrated of order one, that is, the series must be differenced once to make the series stationary. If a series is integrated of order, say, 0.75, then the series is fractionally integrated as the 0.75th difference of the series is stationary. The binomial expansion formula is used to define fractional differencing.

[14] Given the d estimate of approximately 1.25 for the level of the civil cases series over the full sample period, we approximate the short-run time series dynamics by fitting an AR model to the fractionally differenced series using Box-Jenkins methods. An AR representation of generally low order appears to be an adequate prediction of short-term dependence in the data. The AR order is selected on the basis of statistical significance of the coefficient estimates and Q-statistics for serial dependence. A question arises as to the asymptotic properties of the AR parameter estimates in the second stage of estimation. Conditioning on the d-estimate obtained in the first stage, Wright (1995) shows that the AR (p) fitted by the Yule-Walker procedure to the d-differenced series inherits the T^{δ}-consistency of the semiparametric estimate of d.

We forecast the civil cases series over the period 1999–2020 by casting the fitted fractional AR model in infinite autoregressive form, truncating the infinite autoregression at the beginning of the sample, and applying Wold's chain rule. These forecasts are truly ex ante, or dynamic, since they are generated recursively, conditioned only on the information available at the time the forecast is made.

The estimated ARFIMA model for the civil cases series is (t-statistics in parentheses):

$$\begin{aligned}(1-L)^{1.25} X_t = &\ 894.85 + 0.245X_{t-1} - 0.183X_{t-2} - 0.074X_{t-3} + 0.302X_{t-4} - 0.284X_{t-5} + \varepsilon_t \\ &\ (0.725)\quad (1.931)\quad (-1.456)\quad (-0.549)\quad (2.225)\quad (-2.062)\end{aligned}$$

The generated annual forecasts for 1999 to 2020 are reported in Table A.1.10. As can be seen there, the long-memory forecasts are generally in line with the linear ARI model forecasts.

D. Long-Run Relationship between Civil and Criminal Cases

Given the presence of a single unit root in the autoregressive polynomials of the civil and criminal cases series, their long-run relationship is estimated using the Johansen cointegration method (Johansen 1998; Johansen and Juselius 1990), a reduced rank regression technique. The Johansen method employs a vector autoregression (VAR) framework which incorporates both the short- and long-run dynamics of the system. To test the hypothesis that the number of cointegrating vectors is at most r, the trace test statistic is calculated. The asymptotic distribution for the trace test statistic is nonstandard and depends only on $(p - r)$, where p is the number of system variables. To account for the finite-sample bias toward over-rejection of the no-cointegration hypothesis (spurious cointegration), we correct the Johansen test statistic by multiplying it by the scale factor $(T - pk)/T$, where T is the number of observations and k the lag length of the VAR model. We estimate the trace test statistics for alternative lag lengths of the VAR model. We test for cointegration between civil and criminal cases over the full sample period (1904–1998), the post-World War II period (1948–1998), and the period spanning the years 1960 through 1998.

As Table A.1.11 reports, the Johansen procedure provides no evidence of cointegration between civil and criminal cases in any subsample. As expected, there is no (linear) long-run relationship between the series in question as they are driven by different stochastic trends.[15]

E. Long-Run Relationship between Authorized Judgeships and Civil and Criminal Cases

In this section we examine the relationship between the level of authorized judgeships and the levels of civil and criminal caseloads. Extensive unit-root testing suggests that authorized judgeships possess a single unit root. We therefore proceed to test for a cointegrating relationship in the system (authorized judgeships, civil cases, criminal cases) over the full sample period (1904–1998), the post-World War II period (1948–1998), and the period spanning the years 1960 through 1998.

We estimate the vector error correction model (VECM) for alternative lag lengths. The results are robust to the order of the VECM estimated. To conserve space we report the trace statistics corresponding to $K = 4$ in Table A.1.12. There is no evidence of a long-run equilibrium relationship among the system variables.[16]

In order to achieve parsimony in the estimation process, we also test for cointegration between the level of authorized judgeships and the level of total cases. Table A.1.13 reports the estimation results. Again, no evidence of a long-run equilibrium relationship between the two system variables is found.[17]

---

[15] Similar evidence is obtained if cointegration is tested between civil and criminal cases in their log-levels.

[16] This conclusion also holds if cointegration is tested among the log-levels of authorized judgeships, civil, and criminal cases.

[17] Once again, testing cointegration between the log-levels of authorized judgeships and total caseloads produces similar results.

III. Panel Data Analysis for Civil Cases using Statewide, District-Wide and Circuit-Wide Data

Other than the simple linear trend models estimated by staff at the Federal Judicial Center, there are few studies attempting to determine factors (other than the two most common variables used, time and population) associated with the heavier caseloads facing the federal courts. This is perhaps why social scientists dealing with caseload estimation conclude that estimation methodologies for planning purposes are at best tentative and arbitrary, and that there is no universally accepted empirical or theoretical model of caseload estimation (see, e.g., Boyum and Krislov 1990; Mangum 1995). That conclusion is reasonable in the light of the fact that when attempting to model complex relationships in society, the unit of analysis (the number of court cases, say) will capture only a portion of the socioeconomic factors (such as per capita income) responsible for placing demands on the courts. Moreover, as we use aggregated data (even at the district court level, which is a more proper unit of analysis), we are destined to lose valuable information hidden in each individual case. Posner (1996) argues that models explaining variations in caseloads as a function of social aggregates such as population or GNP are not adequate.

The economist's answer to the caseloads problem is simple: as long as the access to the courts is free, excess demand will result. That is, the absence of a price mechanism for allocating court services leads predictably to the overuse of the courts. Thus, as long as the full price (cost) of dispute resolution mechanisms is not borne by those who utilize them (judicial market participants such as private individuals, corporations, lawyers, and the government), there will be incentives to maintain the status quo, however inefficient it may be.[18] Of course, the solution suggested (and more often, implemented) seems to be to increase the court's resources (more judges,

[18] See Benson (1990) and Posner (1996). Posner suggests user fees to curtail the excess demand. However, given that judicial services are a monopoly of the state (a public good) and reasonably alternative private markets do not exist currently, the pricing of these services may be problematic.

more support personnel, and more courts). Moreover, the decision to expand the court's resources is not market-determined either. Although caseloads have a positive influence on the decision to expand the court's resources, political alignment at the very top of the government appears to have a significant influence on the timing of the decision to create more judgeships.[19]

A. Prior Literature

There have been a few previous studies employing both simple and multivariate models to explain increased federal caseloads. Some studies have focused on the caseload statistics of a single district court, an appellate court, or a combination of the two thereof. Only two studies employed a district-level dataset to estimate the demand for federal district court cases. This study takes that approach a little further and attempts to combine the time element into the district-wide analysis. Before proceeding to our empirical estimates, we summarize the most relevant of the existing literature.

The Posner study. Using simple statistics, Richard Posner (1996) analyzed a number of factors affecting judicial caseloads. Different types of case filings were compared between 1960 and 1983. His study did not control for any of the socioeconomic factors possibly relevant to caseload growth. Posner was, however, the first academic to recognize that civil cases were responsible for the rising demands on the federal judiciary. His data analysis points out that between 1958 and 1962 (1960 taken as the mid-point), federal civil cases took a sharp upward turn. Civil cases increased by more than 330% from 1960 to 1983. Between 1960 and 1995, criminal case filings dropped from 35.5% to about 16% of the total. During the same period, the percent-

[19] Figueiredo and Tiller (1996) suggest that institutional factors (caseload pressure) and political factors have positive influences on the decision to expand. They suggest that the cost of monitoring and disciplining the judiciary's behavior may encourage the legislature to appoint more "like-minded" judges (in order to avoid costly legislation to override undesired judicial decisions). Consequently, judicial expansion occurs when there is political alignment at the very top of the government ("alignment among the enacting House, Senate, president, and the nominating president and confirming senate"). According to their results, political alignment is a stronger determinant of the decision to expand than caseload pressure.

age of civil cases increased from 64.5% to about 84%. Between 1960 and 1983, the court services demanded by the U.S. government (U.S. civil filings) rose from 26.3% of the total to 34.6%. This percentage dropped to about 16% by 1995. Of the private civil filings, the percentage of so-called diversity cases was 21.5% in 1960 and 17.5% in 1995. However, by 1995, prisoners' petition cases accounted for the largest percentage of all the civil case filings.

Although it does not control for factors possibly influencing the growth in caseloads or examine year-to-year variations in the data, Posner provides a very good descriptive study highlighting not only historical increases in caseloads (especially the civil caseload) but also shifts in the composition of the caseloads. One can think of this study as a hedonic statistical analysis where the sum is explained using the parts which constitute that sum.

The Landes study. William Landes' (1971) estimation of the demand for federal criminal and civil cases was not an attempt to develop a predictive model. His model for federal civil cases instead sought to explain variations in the fraction of cases that commenced in 1957 disposed of by trial using the following independent variables: the length of the trial queue, a variable to account for differences in the distribution of types of cases across districts, and a dummy variable to account for regional differences (1 if South, 0 otherwise). Two of the variables, the trial queue length and the regional dummy, were statistically significant. The coefficient on the length of the trial queue was negative, as expected. The results also indicated that the fraction of cases commenced in 1957 that went to trial was lower in the South, ceteris paribus. The largest number of district courts analyzed in Landes' study was 84.

The Heydebrand and Seron study. Although Heydebrand and Seron (1990) do not employ panel data estimating techniques, their study is more comprehensive than the two studies summarized above and the one most relevant to this project. Heydebrand and Seron analyzed three decennial datasets. The number of federal court districts analyzed is 84 for 1960 and 1970, and 83 for 1950. The two major regression analyses conducted attempted to explain variations in total case filings and civil case filings per capita (filings divided by the district's population). The independent variables are population density, the number of corporations divided by the district's population, and the number of government employees divided by the district's population.

Population density is a measure of the urbanization of the society. Densely populated districts may indicate that the society is both more crowded and more heterogeneous and, thus, may entangle people in more complex (and more disputatious) relationships not only in social life but also in economic life. Alternatively, rural areas or closely knit societies may have fewer social conflicts. This may be true to some extent because it is possible that, in case of social conflicts, those involved may choose methods of dispute resolution other than bringing the matter to court. The influence of economic factors on the courts is measured by the per capita number of corporations with at least 100 employees. The theoretical reason given is that skewness in the distribution of wealth may create conflicts among the participating agents of the economy. A proxy for the government's demand for court services is measured by the government size, defined as the number of local, state, and federal government employees divided by the relevant jurisdiction's (district, state, or circuit) population. The government has become increasingly involved not only in social processes but also in the economy through stabilization policies and through policies aimed at allocating and distributing national income and wealth. To quote Heydebrand and Seron (1990, pp. 63–64), "the presence of government agencies in a district court's jurisdiction, be at the federal, state, or local level, should, we suggest, affect the business of that court in a variety of ways, *not the least of which are suits by and against the federal government*" (emphasis added).

The effect of population density diminished over the years (it was significant in 1950 and 1960 for total case filings, and in 1960 for civil case filings). The proxy variable for the size of the government was positively significant in 1960 and 1970 for both total and civil case filings. The number of corporations was significant in one year, in 1950, but only for civil case filings. Because the total filings include the criminal filings, which showed very little variation between 1950 and 1970, the differences in terms of significance of the independent variables make it difficult to draw any policy conclusions. Despite this, the Heydebrand and Seron study still stands as the most relevant contribution to the estimation of the demand for the federal court services as it correctly uses the caseload of a federal district court as the unit of analysis. However, it must be mentioned here that none of the above studies has reported any diagnostic statistics for their models.

B. Models and Data Sources

Cooter and Rubinfeld (1990, p. 450) summarize the current state of the attempts to model judicial caseloads in the following terms:

> Economists have done a great deal of modeling but little testing, whereas non-economists have offered statistical description but have not provided theoretical explanation.

This statement does not look so harsh in comparison with claims that "longitudinal study of the courts is without theory, with inadequate theory, or with wrong theory" (Sanders 1990, p. 241), or that longitudinal studies "lack concise theoretical explanation of variation" or that "studies have retrospective designs" (Reiss 1990, p. 345). This sentiment is also expressed by Krislov, yet he maintains that theoretical efforts should continue.

> The pursuit of practical objectives with weak or nonexistent theory is not unknown even in the physical sciences.... Only recently have scientists found previously unknown aspects of bumble bee anatomy to explain how they could fly in what had seemed defiance of aerodynamic theory. So case loaders need no apolo-

> gies if they proceed on ad hoc efforts to project as closely as possible and to fine-tune their "adhockery" with new and innovative adjustments and refinements. If it works, we probably will be able to understand why and learn from it. If it does not work, the richest theory will not resolve it. It seems to me that there is also a refined answer to the question of why practical caseloaders can come close to the mark even when using the crudest measure of all, simple population growth. (Krislov 1995, p. 46)

Among economists, Posner (1996) suggests a simple economic model to estimate the demand for court services. According to him, an individual would decide to litigate if the net expected benefit (the probability of a favorable judgment times the value of the judgment minus expected cost) is positive. This is surely a better model than those models which try to explain caseloads as a mechanical function of social and economic aggregates (Posner 1996, p. 88). However, empirical testing of such models is impossible given the lack of data.

Another possible empirical approach is to estimate a simultaneous equations model accounting not only for demand side but also for supply-side factors.[20] The models tested empirically by Landes and Heydebrand and Seron implicitly assumed that supply was fixed. This may not be so constraining on account of the fact that, although the quantity of judicial services is observed, the market price (filing fee) has rarely changed in the past 50 years.[21] Even if a measure of the price for filing is available, the supply side of the equation is determined not by market conditions but, to an extent, by the political ideology of the currently sitting legislature.[22]

The explosion of federal district court caseloads, especially for civil cases since 1960, has led to many and diverse proposed solutions, ranging from better management of the existing resources, to better prediction of caseloads and, unsurprisingly, to requests for more resources.

[20] Our attempts to do so were unsuccessful owing to the absence of a proxy for the price of court services, a critical variable in any demand-supply framework.

[21] The fee was $15 prior to 1978. It increased to $60 in 1978 and to $120 in 1986. This increase is greater than the rate of inflation prevailing during those years but, apparently, not enough to curtail demand (Posner 1996, p. 125).

[22] The U.S. Congress (the Appropriations Committee) apparently acts on the caseload statistics. Yet, when the Judicial Conference requests more resources in response to increased caseloads, the basis of the request may be questioned (Geyh 1995, p. 90).

Many commentators have suggested shifting diversity jurisdiction and prisoner petition cases from the federal district courts to the state courts.[23] However, such proposals assume that the state courts have resources that the federal courts do not have and that the competency with which the business of the state courts is conducted is not any different from that of the federal courts.

Preliminary data analysis. The annualized average growth rate of the civil cases between 1904 and 1998 is about 3 percent. This growth rate is far greater than the population growth and per capita income growth over the same period. The growth rate of the U.S. population is about 1.3 percent and the per capita income growth is about 1.7 percent. Cases grew at an average annual rate of about 1.1 percent prior to 1960. However, this rate subsequently increased to 2.9 percent. Figure 1 shows that civil cases now comprise a substantial fraction of the total cases and that criminal cases exhibit much lower growth rates (other than during the Prohibition Era and around the twenty-first century's turn). Also apparent in the figure is the enormous upward trend in civil cases starting about 1960.[24]

Multivariate statistical models. This study attempts to model the socioeconomic factors possibly associated with the increased civil case filings since the 1960s. Civil case filings are analyzed using three different categories: U.S. cases, private cases, and their summed total.

---

[23] See Posner (1996), Redish (1989) and Newman (1989). However, there does not appear to be a consensus. Campbell (1989) argues that, although increasing caseloads may result in lower quality of judicial services rendered and may destroy the coherence of the federal courts, eliminating diversity jurisdiction is not a solution. The question appears to go to the very heart of possible prejudice against out-of-state litigants. The U.S, Constitution, Article III, Section 2, Clause 1 provides that "The judicial Power [of the United States] shall extend to [i] all Cases, in Law and Equity, arising under this Constitution, the Laws of the United States, and Treaties made, or which shall be made, under their Authority; [ii] to all Cases affecting Ambassadors, other public Ministers or Consuls; [iii] to all Cases of admiralty and maritime Jurisdiction; [iv] to Controversies to which the United States shall be a Party; [v] to Controversies between two or more States; [vi] [Controversies] between a State and Citizens of Another State; [vii] Controversies] between Citizens of different States; [viii] [Controversies] between Citizens of the same State claiming Lands under Grants of different States, and [ix] [Controversies] between a State, or the Citizens thereof, and foreign States, Citizens or Subjects."

[24] As noted above, Posner (1996) identifies the period between 1958 and 1962 as the beginning of the upward trend. According to Redish (1984, p.41), Justice Scalia agrees with Posner's conclusion.

While analysis of federal district court cases at more disaggregated levels would have been useful, the lack of data for the 1961–1998 period limited our modeling to these three categories of cases. For each of the categories above the following models are estimated.

1. Statewide models using district-level observations aggregated to the state level.
2. District-wide models to analyze the influence of the factors at the district level (smallest unit of measurement).
3. Circuit-wide models using district-level observations aggregated to circuit level.

The general model to be estimated is:

Civil case filings = f(*population characteristics, economic factors, government size, geographic location*).

Population characteristics consist of population density and race; economic factors consist of per capita income and a proxy for labor market conditions; government size is a proxy variable to account for the government's demand for court services; and, finally, the geographic location of the district court accounts for possible regional differences in case-bringing activity. Theoretical reasons for the inclusion and *a priori* expectations of the independent variables are as follows.

Population density. As discussed earlier in summarizing the Heydebrand and Seron study, this variable is a measure of the urbanization of the society. As a very crude measure of social interactions and the complexity of social relationships, population density is hypothesized to have a positive impact on the civil case filings.

Race. Although we do not have *a priori* expectations on this variable, it is used as a measure of the heterogeneity of the society. This variable is constructed by dividing the population of non-white persons by the total population.

Per capita income. This is a measure of economic activity surrounding the federal court system. As the dollar volume of transactions grows larger, there will be more financial interests at stake, more interests to protect. Alternatively, if we assume that the good in question (civil cases) is a normal good, from a microeconomic perspective, there will be more demand for it as income increases. Moreover, a variable such as the number of corporations (the Hydebrand and Seron study) may not fully capture the influence of economic activity because a large percentage of the national income is generated by small businesses.

Unemployment rate. This variable is the percentage of the labor force that is not employed. It is a proxy for the influence a significant part of the civilian labor force exerts on the courts. It is a reasonable variable in light of the fact that one of the largest categories of cases arising under "federal question" jurisdiction consist of matters arising under existing labor laws. Moreover, it may indicate possible conflicts between employers and employees. Its effect is hypothesized to be positive.

Government size. As discussed above, the government involves itself in every aspect of social and economic life. It regulates the economy through its fiscal, monetary, and income policies. It regulates social processes by making laws and enforcing these laws through its bureaucratic agencies. Following the same methodology used by Heydebrand and Seron (1990), we measure the role of the government by entering a variable defined as the total number of federal, state and local government employees per jurisdiction (district, state, or circuit) divided by the jurisdiction's population. The expected relationship is positive.

Geographic location. Several indicator variables are employed to account for the effect of the geographic location of the district courts on the civil caseloads. In terms of identifying the future needs of each court and, thus, each circuit, the circuit-based geographic location may be more appropriate than the location variable, South versus non-South, used in the Landes study (Holloway 1989, p. 93). However, we will present our findings using both specifications. We do not have *a priori* expectations on these indicator variables.

Data sources. The unit of observation is the annual number of civil cases filed in each federal district court. The dependent variable is normalized by dividing cases (U.S. civil, private civil, or their total) by the relevant jurisdiction's population (state, district, or circuit). Socio-economic factors have been aggregated from the county level. Given that there are over 3,000 counties in the United States and that federal district court boundaries do not follow state boundaries for a majority of the district courts, the complexity of constructing the dataset has been enormous. When the data are transformed into a panel dataset accounting for the years between censuses, we have had to construct annual estimates of population density, race, income, government size, and working age population. Although such estimates may not be reliable, the information gained by examining annual cases may justify the use of such estimates (about 3,400 observations for each variable). Moreover, it is one of the objectives of this study to determine whether there exist substantial differences among models which use district-wide, state-wide, and circuit-wide data in terms of the models' explanatory power.

In estimating the demand for civil court cases, the following data sources have been used: *The County and City Data Book* for years 1962, 1967, 1972, 1977, 1983 and 1994; the U.S. Counties CD-ROM (since 1970); *Statistical Abstract of the United States* (various years); the *Reports* of the Administrative Office of the United States Courts (1961–1998); and *Federal Court Management Statistics* of the Administrative Office of the United States Courts (1968–1998).

Some may argue that the demand placed on the services of district courts may not be altogether externally determined. That is, the internal environment of the courts could also explain variations in caseloads. This argument is valid, but it is only relevant for cases already filed. In other words, the internal environment of the courts could determine what will happen to a case that is already filed. The question of interest here is, what prompts people to resolve their conflicts in courts in the first place?

C. Estimated Models and Results

Because there appears to be a systematic upward trend in total civil cases, normalizing the series by the size of the underlying population avoids a potential bias (more people = more cases). Thus, models using district-wide, statewide, and circuit-wide data are estimated for each of the following categories: per capita total civil case filings, per capita U.S. civil case filings, and per capita private civil case filings. Moreover, each of these categories has been also modeled using alternative geographical location dummy variables (South versus non-South and circuit dummies).

We have used a total of 10 indicator variables for the 12 geographical circuits, including the District of Columbia Circuit. In 1980, Congress decided to divide the Fifth Circuit (then comprised of districts in Alabama, Florida, Georgia, Louisiana, Mississippi and Texas) into two circuits, the Fifth Circuit and the Eleventh Circuit. Alabama, Florida and Georgia were assigned to the newly created Eleventh Circuit whereas Louisiana, Mississippi and Texas remained in the Fifth Circuit (see 94 Stat. 1994). For the sake of continuity, throughout this section, the Fifth Circuit and the Eleventh Circuit are treated as a single circuit, the Fifth circuit. However, we have estimated all the above models treating the Fifth and Eleventh Circuit as separate, starting in 1982. Those results, while not presented here, are highly consistent with the results reported below. The following states denote the regional indicator variable, Southern: Virginia, North Carolina, South Carolina, Georgia, Florida, Alabama, Mississippi, Louisiana, Texas and Arkansas.

Of the 18 models estimated (see Tables A.2.1 through A.2.6), several suffered from violations of the classical regression assumptions. The results are presented with proper corrections applied to these models. Moreover, asterisks next to the variables' p-values indicate if there is loss of significance after the corrections.

One can think of these models as "pseudo-fixed effects" models. A fixed effects model is one where the differences between cross-sectional units can be represented by intercept (parametric) shifts; this model is also known as *the least squares dummy variable model* (Greene 2000, p. 560). A fixed effects model is used when the variables are thought to be correlated with the observational units.[25] If, for example, we have reason to believe that one or more of our variables (population density, for instance), correlates with the districts then it is more appropriate to use a fixed effects model. In our case, there are two possibilities: district (or state, or circuit) effects and time effects. We have included circuit dummies to capture the cross-sectional effects. However, we have intentionally omitted time dummies from 1962 to1998. The reason is that per capita income very strongly correlates with time. When income is included in any model either as a single independent variable or with other variables, the marginal impact is positive. However, regardless of the type of model estimated (panel, time series, or cross-sectional), when in-

[25] A formal specification test was conducted in order to compare the fixed effects model with a random effects model. The test strongly rejected the random effects model at the state level. After running a fixed-effects model on the statewide data, we find the following results: For the sample period 1961–1998, the only two variables that lost significance or changed sign compared to the pseudo-fixed effects models, were per capita income, changing sign and significance to negative in U.S. cases, and race, changing sign to negative in private cases. The problem with running any kind of a fixed effects model with circuit dummies was the perfect collinearity associated with DC. In a sense, running a pseudo-fixed effects model rather than a true-fixed effects model was predicated by our objective of understanding better the variation accounted for by the geographic location of courts. The district-wide analysis cannot be performed due to the changing number of districts over the time period analyzed. However, some circuit-wide analyses may be modeled as a random effects model. The differences appear to be negligible.

cluded with time the income variable becomes negative and significant. To confirm this, we have run simple regressions of income against time (untransformed and log-transformed regression models) and observed adjusted coefficients of multiple determination (R^2) exceeding 0.88. The remaining regressors do not seem to be affected by time as much as income is. Thus, although cases seem to respond to time (capturing parametric level shifts through 37 time indicator variables) and space (that is, the 10 circuit dummies capturing spatial units' differences) positively, to present our findings more concisely (thereby avoiding reporting more than 90 right-hand side variables), we have chosen to exclude time. Nelson and Kang (1984) state that time as an explanatory variable may lead to spurious correlations, supplying another reason not to include it.[26]

Discussion. Of the 18 models estimated, the only model that did not perform according to our *a priori* expectations is the circuit-wide model without the circuit dummies. All of the remaining models have overall explanatory powers exceeding conventional significance levels. Based on the results in Tables A.2.1 through A.2.6, we arrive at the following general conclusions:

- Any geographical location dummy variable set seems to improve the models' performance, although some models suffer from multicollinearity.[27] The implication is that explanatory models not taking account of court location omit important information.

[26] We nevertheless estimated all of our models including time as an explanatory variable; the results are available upon request.

[27] Models with the circuit dummies exhibit symptoms of multicollinearity. The District of Columbia Circuit had especially strong correlations with population density. The choice was to use or not use the circuit dummies altogether. Because the models with the circuit dummies show a high degree of additional explanatory power, we have chosen to present our results both ways. Moreover, we have also run models including all the circuits except the District of Columbia Circuit (thrown out altogether from the data set). The results are identical with the population density variable having the correct (positive) sign.

- Per capita income, as hypothesized above, is able to explain a great deal of the variation in civil case filings per capita. That is, as the dollar volume of transactions rises, people tend to file more cases perhaps in an attempt to protect their economic interests. Interestingly, the impact of per capita income on private civil cases is stronger than it is on U.S. civil cases.
- Population density enters positively and significantly in a majority of the models estimated. It appears that, other things being the same, more cases tend to be filed in densely populated districts, states, and circuits.
- The size of the government also has a positive impact on case filings. This impact is more pronounced when the U.S. government is a party to a civil dispute either as a defendant or plaintiff. However, especially in private disputes, some models exhibited strong negative relationships, *ceteris paribus*, between government size and case filings.
- Filings in general seem to be higher in locations with higher unemployment rates and higher percentages of non-white populations.[28]
- It appears that southern states have more civil case filings per capita than other regions of the country. This is also apparent from the circuit level dummies. One

[28] Reliable unemployment rate data starting from 1970 were provided to us by the Bureau of Labor Statistics in personal correspondence with Thomas J. Krolik. For years prior to the 1970s, we have used the *Statistical Abstract of the United States* (U.S. Department of Commerce, various years) and linear regression to estimate the civilian labor force and unemployment rates. However, our models have also been run for 1976–1998, a period where very little estimation of these series was necessary. For the more reliable dataset, the statewide data, we report the results in Table A.2.7. For the years before 1970, we used any other available series to correlate with the series estimated. To avoid loss of generality and information, we used models with adjusted R^2s greater than 0.9. Certainly, some of these estimates may not be reliable, but the tradeoff was to avoid breaks in the continuity of the dataset and, through this, to be able to use more case data values. Moreover, we will not emphasize much the contribution of the race variable, as it is estimated based on available decennial data. We have used an exponential short-run growth rate model, $Y = Y_0e^{t*g}$, where Y = end-value of the series, Y_0 = beginning value of the series, t = time period involved (10 years) and g = growth rate. Models were also tried without the unemployment rate and non-white ratio; once again, the results are identical to those in tables 3.1 to 3.6 even though there were declines in the overall significance.

explanation for this may be Say's Law. It appears that there are more courts in the South than in the North, perhaps as means of enforcing the civil rights and voting rights acts of the 1960s. The only circuit that seems to have significantly higher case filings per capita than the Fifth Circuit is the District of Columbia Circuit. In general, all other circuits have significantly lower per capita filings than the Fifth Circuit (Texas, Louisiana, Mississippi) and the districts now comprising the Eleventh Circuit (Alabama, Florida, Georgia). This result, once again, implies that the circuit dummies add significant explanatory power to our models.

Table 9 summarizes the results of the 18 models estimated in a concise manner. Tables showing more detailed results can be found in Appendix 2.

Table 10 presents the elasticities of the three dependent case variables with respect to the continuous independent variables, calculated at the means of the data. After taking account of the influence of the other four independent variables, government size exhibits the largest elasticity. Per capita income ranks a close second. To repeat some earlier findings, government size appears to have the largest marginal effect in cases where the U.S. government is a party to a dispute either as a plaintiff or as a defendant. The impact of per capita income is larger in private civil cases.

These results are consistent with the hypotheses developed earlier. Naturally, there is more to be done in this uncharted territory. But we do now have confidence that, contrary to Posner, case filings can in fact be modeled as a function of standard socioeconomic aggregates. Of course, these findings are not conclusive. For example, finding case data on several subcategories of U.S. and private civil cases at the district or state level may reveal information that more aggregated series cannot reveal. There is admittedly the possibility that all these results, however free of the violations of the models assumptions, may be spurious. On the other hand, finding results consistent across data series leads us to believe that we have contributed to a field where very little has been done before.

Table 9. Summary of Findings: p-values

	MODELS without CIRCUIT DUMMIES								
	District-Wide			**Statewide**			**Circuit-Wide**		
Variables	Total	U.S.	Private	Total	U.S.	Private	Total	U.S.	Private
Constant	NS	NS	NS	[0.062]	[0.000]	0.003	NS	[0.000]	0.000
Density	0.000	0.000	0.000	0.000	0.000	0.000	0.000	[0.000]	0.000
Income	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000	[0.000]
Government	0.000	0.000	0.032	0.032	0.000	[0.000]	NS	0.000	[0.000]
U. Rate	0.000	0.000	0.000	0.000	0.000	0.000	NS	NS	[0.021]
Race	0.000	0.086	0.000	0.004	0.018	0.002	NS	0.000	NS
South	0.000	0.000	0.000	0.000	0.006	0.000			

	MODELS with CIRCUIT DUMMIES								
	District-Wide			**Statewide**			**Circuit-Wide**		
Variables	Total	U.S.	Private	Total	U.S.	Private	Total	U.S.	Private
Constant	0.000	0.000	0.001	0.000	NS	0.000	0.000	NS	0.000
Density	[0.042]	[0.000]	0.095	NS	[0.000]	0.000	0.000	[0.000]	0.000
Income	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.006	0.000
Government	NS	0.000	NS	NS	0.000	NS	NS	0.000	[0.000]
U. Rate	0.000	0.000	0.001	0.000	0.000	0.000	NS	NS	NS
Race	0.000	NS	0.000	NS	0.010	NS	[0.000]	NS	[0.000]
DC	0.000	0.000	0.000	0.002	0.000	NS	0.003	0.000	0.051
First	[0.000]	[0.001]	[0.000]	[0.016]	NS	[0.000]	[0.000]	NS	[0.000]
Second	[0.000]	[0.003]	NS	[0.000]	[0.016]	[0.000]	[0.000]	NS	[0.000]
Third	[0.000]	[0.002]	[0.000]	[0.000]	NS	[0.000]	[0.000]	NS	[0.000]
Fourth	[0.000]	[0.022]	[0.005]	[0.000]	NS	[0.000]	[0.002]	NS	0.004
Sixth	[0.000]	NS	[0.000]	[0.000]	NS	[0.000]	[0.000]	NS	[0.000]
Seventh	[0.000]	[0.000]	[0.000]	[0.002]	[0.000]	[0.000]	[0.000]	NS	[0.000]
Eighth	[0.000]	NS	[0.000]	[0.000]	NS	[0.000]	[0.000]	NS	[0.000]
Ninth	[0.000]	NS	[0.000]	[0.000]	[0.020]	[0.000]	[0.000]	NS	[0.000]
Tenth	[0.000]	NS	[0.003]	[0.000]	NS	[0.000]	[0.000]	NS	[0.000]

Note: Bracketed p-values indicate coefficients entering with negative algebraic signs; "NS" denotes not significant.

Table 10. Elasticities of Continuous Independent Variables[29]

	District-Wide			Statewide			Circuit-Wide		
Variables	Total	U.S.	Private	Total	U.S.	Private	Total	U.S.	Private
Density	NS	–0.02	0.02	NS	–0.35	0.03	0.05	–0.08	0.10
Income	0.44	0.31	0.53	0.42	0.26	0.49	0.37	0.29	0.41
Government	0.13	0.14	0.10	0.73	0.86	0.60	0.78	1.21	0.55
U. Rate	0.14	0.26	0.08	0.22	0.39	0.13	NS	0.42	NS
Race	0.20	0.16	0.23	0.13	0.14	0.14	0.23	0.23	0.24

Note: "NS" denotes not significant.

Summary. In this section, we have estimated models of civil case filings in the United States Federal District Courts over the years 1961 to 1998 at three levels of aggregation (district-wide, statewide, and circuit-wide). For each data series, six different regression models have been run. Our results confirm strongly our a priori expectations: higher numbers of case filings are associated with more densely populated districts, districts with higher per capita incomes, and districts with larger governments. Higher case volumes are also associated with higher unemployment rates and higher percentages of non-white persons. Interestingly, cases to which the United States government is a party are strongly associated with the size of the government of the particular district. These findings are tentative yet offer fruitful opportunities for future research.

IV. Interplay between Criminal and Civil Caseloads

The U.S. Constitution provides individuals charged with criminal offenses the right to speedy trials; defendants in civil cases have no such constitutional guarantee. Efficient caseload management therefore requires sitting federal judges to balance two competing demands on their

[29] "Elasticity" is a measure of the responsiveness of one variable to changes in the values of other variables. The income elasticity of total state-wide cases, 0.73, implies that when per capita income increases (decreases) by 1 percent, state-wide per capita total filings will increase (decrease) by 0.73 percent holding everything else constant.

limited time, trading off the need to dispose of criminal cases expeditiously while simultaneously handling a growing volume of civil cases. The empirical estimates reported in this section represent a preliminary attempt to shed light on the possibility of a link between criminal and civil caseloads in the U.S. federal district courts. More specifically, we ask whether the constitutional priority assigned to criminal cases plays a role in explaining the backlog observed in civil cases. In other words, if judges must allocate a certain amount of their fixed time to criminal cases, the time available to them to dispose of civil cases may be reduced. This in turn implies that the time lag between filing and disposition of civil cases may be lengthened, ceteris paribus.

Do criminal cases crowd out civil cases? To explore this question, we have collected panel data from 1968 to 1998. The unit of analysis is the federal district court and the data consist of the following variables: number of authorized judgeships per district, median time from filing to disposition of civil and criminal cases, and drug and immigration cases as a percent of total criminal filings.[30] Our empirical tests are based on two alternative regression specifications. Fixed effects models and least squares models were run on the panel data and a least squares regression was run using annual time series data from 1968 to 1998. The dependent variable in all regressions is the median time between filing to disposition of civil cases. The estimates are reported in Appendix 3.

The empirical results are consistent with the conjecture of crowding out. We find, in general, that the median time from filing to disposition of civil cases tends to be significantly longer in those districts where it also takes more time to dispose of criminal cases. Moreover, it appears that more time elapses between the filing and disposition of civil cases in districts where larger percentages of the criminal cases involve drug and immigration law violations. We find in addi-

[30] Although we reported evidence earlier that the time series of civil cases, criminal cases, and authorized judgeships are not cointegrated, it is still possible that the median time to disposition of civil cases is influenced either by the time spent disposing of criminal cases, the number of federal authorized judgeships, or both.

tion that federal judges have positive marginal products: other things being the same, civil cases are disposed of more expeditiously in districts with greater numbers of authorized judgeships.

Hence, while the number of federal criminal case filings has grown much more slowly than civil cases have grown over the past 60 years, the empirical evidence reported here suggests that the time required to dispose of such cases imposes a negative externality on the courts' civil caseload. As judges devote more time to disposing of criminal cases, the pace at which civil cases move through the federal courts is also slowed. The positive relation between time to disposition of civil and criminal caseloads has important implications for the management of the federal court system. It appears that efficient caseload management is not simply a matter of trading off less time for criminal cases in return for freeing more time for civil cases. Rather, efficient management requires adjusting on both margins simultaneously.

V. Summary and Conclusions

This study has explored the determinants of the explosion in the caseload of the U.S. federal district courts that commenced in 1960. Prior to that time, the federal judiciary's caseload grew at a rate averaging about 1.1 percent per year. The growth rate thereafter rose to 2.9 percent annually, nearly tripling the demands on the federal courts. Given that criminal case filings have increased much more slowly since the era of Prohibition, the mounting burden on sitting federal judges has been driven almost entirely by a sevenfold upsurge in civil cases.

The analyses reported herein offer important insights into the caseloads problem. First, using advanced econometric techniques, we have supplied forecasts of future demands on the federal courts that are more accurate than those available previously. Forecast errors are reduced by taking account of the time series properties of the case data. In particular, strong evidence that the time series of federal civil and criminal cases are nonstationary (have unit roots) implies that the projections produced by deterministic models with linear trends are unreliable. Based on estimates of autoregressive time series models of civil and criminal cases, using annual data for the years 1904 to 1998 as well as subperiods thereof, we generate out-of-sample forecasts through 2020 that differ substantially from the forecasts of the Judicial Conference of the United States. To illustrate, while the Judicial Conference estimates that total federal case filings will almost triple between 2000 and 2020, rising from 364,800 cases to 1,060,400 cases annually, our models project that the burden on the federal courts will not reach half that number: a total of 444,074 cases is our most generous estimate for 2020.

The study's second contribution is to specify and estimate multivariate econometric models of the determinants of civil case filings over time and across geographic space using panel data techniques. These empirical models are run on three alternative datasets consisting of observations on statewide, district-wide, and circuit-wide U.S. civil, private civil, and total civil cases per capita, over the period 1960 to 1998. We find that federal civil case filings are influenced significantly by the socioeconomic characteristics of the relevant state, district, or circuit. In particular, holding other things constant, civil cases are positively related to per capita income, population density, the percentage of the population that is nonwhite, the unemployment rate, and the size of government. We also find that the explanatory power of the panel data models is improved substantially by controlling for the geographical locations of the federal courts: other things equal, significantly more civil cases are filed per capita in the Fifth Circuit than elsewhere, except for the District of Columbia Circuit. The fact that fixed effects models explain variations in civil case filings better than alternative models that do not take geographical location into account provides preliminary evidence pointing to the efficiency gains potentially flowing from reassessing the cross-circuit and cross-district allocation of judgeships and other resources of the federal courts.

The importance of caseload management is reinforced by analyses of the impact of criminal cases on civil cases. We find that, holding constant the time between the filing and disposition of federal criminal cases, civil cases are disposed of more expeditiously in districts where there are more authorized judgeships per capita. On the other hand, holding authorized judgeships per capita constant, we also find that criminal cases impose a negative externality on civil cases: the more time federal judges take to dispose of criminal cases in a given district, the longer is the elapsed time between the date of filing and the date of disposition of civil cases. Moreover, the time to disposition of civil cases tends to be longer in districts where greater percentages of the criminal caseload involve alleged drug and immigration law violations.

Despite our finding that the time devoted to disposing of criminal cases slows the speed at which civil cases move through the courts, we also report evidence supporting the hypothesis that the numbers of civil and criminal cases and numbers of authorized federal judgeships are not cointegrated. Two conclusions follow from this evidence. One is that the numbers of civil and criminal cases filed in the federal courts are generated by independent stochastic processes. In other words, information about the number of criminal cases filed in a given year does not allow one to predict the civil caseload, and vice-versa. The other conclusion is that the number of judges authorized to hear federal cases bears no relation to the total caseload of the federal courts. Forces external to the courts, such as the political process, evidently play greater roles than caseload demands in determining the size of the federal judiciary.

In sum, this study provides new, and we believe, more accurate forecasts of the future workload of the U.S. federal district courts than have been available hitherto. Grounded in best-practice econometric techniques, we project that the federal courts can at most expect to face a caseload of 444,074 civil and criminal cases by 2020, not a total exceeding one million such cases. The study also supplies evidence that the distribution of federal civil cases across states, districts, and circuits can be explained by empirical models that include standard socioeconomic variables, such as income, population density, and race, along with variables that control for fixed effects associated with the geographical locations of the federal courts. We thus have models that policymakers can use to forecast the future caseloads on the federal court system as a whole as well as to estimate how the total caseload will be distributed geographically.

Perhaps the most important policy implication of this study, however, is that caseload forecasts using models incorporating simple linear trends are problematic. Indeed, once the time series properties of federal civil and criminal case filings are taken into account, there is no evidence of a linear trend in the data. The failure adequately to model the time series of cases explains why previous forecasts, such as those contained in the *Long Range Plan*, consistently have overestimated future demands on the federal courts. Armed with the more accurate forecasts presented here, policymakers can more confidently assess the need for additional judgeships and, moreover, can address what seems to be a more pressing problem, namely the possible misallocation of judgeships across circuits and districts.

This final observation points to a useful area for future research. Although beyond the scope of the present study, it would be informative to conduct time series analyses of the case filings in individual federal district courts. Such a study would allow one to explore the causes and consequences of the growth of criminal caseloads, especially those involving drug and immigration law violations, which have recently increased at historically unprecedented rates in the Southwestern United States.

Appendix 1

Unit Root Tests

The augmented Dickey-Fuller-generalized-least-squares (ADF-GLS) test is the Elliott-Rothenberg-Stock (1996) efficient test for an autoregressive unit root. This test is similar to an (augmented) Dickey-Fuller *t*-test, as it applies GLS detrending before the series is tested via the Dickey-Fuller regression. Compared with the ADF tests, the ADF-GLS has the best overall performance in terms of small sample size and power. It "has substantially improved power when an unknown mean or trend is present" (Elliott et al. 1996, p. 813). The null hypothesis is that the series is level (or trend) stationary with the alternative of a single unit root.

The Kwiatkowski-Phillips-Schmidt-Shin (KPSS) test is introduced in Kwiatkowski et al. (1992) and differs from those in common use by having a null hypothesis of stationarity. The test may be conducted under the null hypothesis of either trend stationarity or level stationarity. It can be used in conjunction with the ADF-GLS tests to obtain insights into the low-frequency behavior of time series given small sample sizes.

The structural break tests are the ones proposed by Clemente et al. (1998), which represent extensions of the Perron-Vogelsang methodology to allow for double mean shifts. Perron and Vogelsang (1992) propose a class of test statistics that captures two alternative forms of change: the "additive outlier" (AO) model, allowing for the possibility of a sudden change, and the "innovational outlier" (IO) model, appropriate for modeling a gradual shift in the mean of the series. The test statistics do not require a priori knowledge of the breakpoint, as their computation involves a two-dimensional grid search for breakpoints over the sample.

The modified log-periodogram test is a test for fractional integration proposed by Phillips (1999). Phillips' estimator is an extension of the well-known contribution of Geweke and Porter-

Hudak (1983) that addresses some of the weaknesses of the GPH test. We use Phillips' test as we want to consider the possibility that the order of integration in our series may be fractional, $I(d)$, rather than integer, $I(1)$ versus $I(0)$. The previous tests allow only for integer orders of integration, creating a knife-edged unit-root versus stationarity distinction. The series is said to be fractionally integrated if the differencing parameter is found to be of noninteger value.

Table A.1.1. ADF-GLS Unit-Root Test Results

	Series		
	Panel A		
Lag Order	Civil	Log(civil)	Δ Log(civil)
With constant			
$k = 2$	0.945	1.488	–5.515***
$k = 4$	0.197	0.740	–3.444***
$k = 6$	0.961	0.593	–2.585**
$k = 8$	0.835	0.627	–2.428**
Min. MAIC	0.740 (5)	0.740 (4)	–2.585** (6)
Sequential-t	0.213 (9)	0.740 (4)	–3.704*** (3)
With constant and trend			
$k = 2$	–1.045	–1.775	–5.686***
$k = 4$	–1.556	–2.213	–3.601**
$k = 6$	–0.939	–2.261	–2.734*
$k = 8$	–0.962	–2.191	–2.602*
Min. MAIC	–1.105 (5)	–1.775 (2)	–2.734* (6)
Sequential-t	–1.311 (9)	–2.213 (4)	–3.849*** (3)
	Panel B		
	Criminal	Log(criminal)	Δ Log(criminal)
With constant			
$k = 2$	–1.267	–0.888	–4.583***
$k = 4$	–1.548	–1.128	–3.306***
$k = 6$	–1.260	–1.046	–2.751***
$k = 8$	–1.483	–1.149	–2.689***
Min. MAIC	–1.446 (1)	–0.939 (1)	–2.751** (6)
Sequential-t	–1.617 (7)	–0.939 (1)	–8.152*** (0)
With constant and trend			
$k = 2$	–1.815	–1.759	–4.571***
$k = 4$	–2.152	–2.073	–3.296**
$k = 6$	–1.864	–2.006	–2.741*
$k = 8$	–2.150	–2.191	–2.667*
Min. MAIC	–2.003 (1)	–1.816 (1)	–2.741* (6)
Sequential-t	–2.281 (7)	–1.816 (1)	–8.150*** (0)

Notes: The ADF-GLS test is the one suggested by Elliott, Rothenberg and Stock (1996) for an autoregressive unit root. MAIC is the modified Akaike information criterion proposed by Ng and Perron (2001) (the optimal lag order is shown in parentheses). They have established that the MAIC criterion may provide huge size improvements in the ADF-GLS test. The sequential-t criterion was proposed by Ng and Perron (1995) and is based on a sequential t-test on the highest lag order coefficient, stopping when that coefficient's t-value is less than 0.10 (the optimal lag order is given in parentheses). Asterisks indicate statistical significance at the 1% (***), 5% (**), and 10% (*) levels, respectively.

Table A.1.2. Kwiatkowski-Phillips-Schmidt-Shin (KPSS) Unit-Root Test Results

	Null Hypothesis	
Series	Level Stationarity (Lag)	Trend Stationarity (Lag)
Civil	1.908*** (3)	0.496*** (3)
Log(civil)	2.241*** (3)	0.262*** (3)
Δ Log(civil)	0.072 (3)	0.063 (3)
Criminal	0.164 (3)	0.174** (3)
Log(criminal)	0.312 (3)	0.198** (3)
Δ Log(criminal)	0.095 (3)	0.085 (3)

Notes: The test statistics are the KPSS test statistics for the null hypothesis of level (or trend) stationarity. The order of serial correlation (lag) is chosen according to an automatic bandwidth selection which specifies the selection of the serial correlation allowed in the estimation of the "long-run" covariance by the automatic bandwidth selection proposed by Newey and West (1994) in conjunction with the usage of the quadratic spectral kernel to weight the empirical autocovariance function as suggested by Hobijn et al. (1998). It is in conjunction that Hobijn et al. found the greatest improvement in the test: "Our Monte Carlo simulations show that the best small sample results of the test in case the process exhibits a high degree of persistence are obtained using both the automatic bandwidth selection procedure and the quadratic spectral kernel" (1998, p. 14). Asterisks denote significance at the at the 1% (***), 5% (**) , and 10% (*) levels, respectively.

Table A.1.3. Structural Break Unit-Root Test Results

Series	Model	δ_1	T_{b1}	δ_2	T_{b2}	M	k	$\alpha - 1$
Civil	IO	16.203 (3.60)	1970	15.528 (2.25)	1980	7.069	5	–0.155 (–3.08)
	AO	72.258 (11.69)	1967	127.69 (17.26)	1980	37.93	12	–0.351 (–1.87)
Log(civil)	IO	0.061 (0.99)	1921	0.129 (1.72)	1973	0.80	0	–0.078 (–1.63)
	AO	1.049 (13.77)	1921	1.41 (21.06)	1972	9.72	0	–0.335 (–4.22)
Δ Log(civil)	IO	–0.160 (–3.26)	1931	0.147 (3.20)	1939	0.06	1	–1.324 (–9.31)
	AO	–0.10 (–2.03)	1930	0.089 (1.92)	1937	0.05	10	–0.883 (–2.54)
Criminal	IO	9.105 (1.42)	1920	–7.589 (–1.75)	1935	9.87	7	–0.289 (–2.63)
	AO	50.194 (14.91)	1920	–34.583 (–12.56)	1934	22.35	7	–0.525 (–2.38)
Log(criminal)	IO	–0.027 (–0.22)	1919	0.007 (0.11)	1935	0.54	0	–0.049 (–0.61)
	AO	1.292 (16.95)	1919	–0.617 (–10.15)	1934	9.86	0	–0.432 (–4.85)
Δ Log(criminal)	IO	–0.444 (–4.54)	1932	0.372 (4.09)	1936	0.08	1	–1.281 (–8.18)
	AO	–0.225 (–3.46)	1931	0.177 (2.84)	1936	0.06	2	–1.057 (–6.06)

Notes: The unit-root tests are those proposed by Clemente at al. (1998) for the innovational outlier (IO) and additive outlier (AO) models of a unit root in the presence of double mean shifts. The 5% critical value for the test of ($\alpha - 1$) is –5.49 for both innovational and additive outlier models. The t-statistics for μ, δ_1, and δ_2 follow a standard t-distribution under the null; k is the autoregressive lag order chosen.

Table A.1.4. Modified Log-Periodogram Test Results

Series	*t*-statistics for Bandwidth Window		
	0.70	0.80	0.90
Civil			
With Trend	1.161 (1.23)	1.248** (2.38)	1.332*** (4.01)
No Trend	0.502*** (–3.80)	0.434*** (–5.44)	0.385*** (–7.43)
Log(civil)			
With Trend	0.996 (–0.03)	1.042 (0.42)	1.081 (0.98)
No Trend	0.873 (–0.97)	0.831 (1.63)	0.793 (–2.50)
Δ Log(civil)			
With Trend	0.040 (0.17)	0.065 (0.34)	0.029 (0.21)
No Trend	–0.047 (–0.20)	–0.002 (–0.01)	–0.018 (–0.13)
Criminal			
With Trend	1.037 (1.29)	0.994 (–0.06)	1.008 (0.09)
No Trend	1.089 (0.68)	0.940 (–0.58)	0.865 (–1.63)
Log(criminal)			
With Trend	1.036 (0.28)	1.017 (0.16)	0.970 (–0.36)
No Trend	0.960 (–0.30)	0.935 (–0.62)	0.906 (–1.13)
Δ Log(criminal)			
With Trend	0.133 (0.92)	0.151 (1.36)	0.139 (1.62)
No Trend	0.166 (1.19)	0.176 (1.62)	0.160* (1.89)

Notes: The modified log-periodogram test computes a modified form of the Geweke-Porter-Hudak (GPH) estimate of the long memory parameter d of a time series, proposed by Phillips (1999). The estimator gives rise to a test statistic for $d = 1$, which is a standard normal variate under the null (for the log-differenced series the test statistic for $d = 0$ is reported). Phillips suggests that deterministic trends should be removed from the series before application of the estimator. Asterisks denote statistical significance at the 1% (***), 5% (**), and 10% (*) levels, respectively.

Table A.1.5. Autoregressive Models Chosen for the Civil, Criminal and Total Cases

	Sample Period		
Series	1904–1998	1948–1998	1960–1998
Civil Cases	ARI(5, 1)	ARI(5, 1)	ARI(5, 1)
Criminal Cases	ARI(7, 1)	ARI(0, 1)	ARI(0, 1)
Total Cases	ARI(5, 1)	ARI(5, 1)	ARI(5, 1)

Notes: ARI(p, 1) stands for a unit-root (integrated of order one) process of autoregressive order p. The chosen model for the criminal cases over the 1948–1990 period is a driftless random walk; for the 1960–1990 period it is a random walk with drift.

Table A.1.6. Regression Results

CIVIL CASES 1

Estimation by Least Squares	
Dependent Variable	**First Differenced Civil Cases 1904–1998**
Usable Observations	89
Degrees of Freedom	83
Centered R^2	0.24
Adjusted R^2	0.2
Uncentered R^2	0.31
$T*R^2$	27.19
Mean of Dep. Variable	2737.75
St. Error of Dep. Variable	9319.37
St. Error of Estimate	8388.95
Sum of Squared Errors	5771777548
F-Statistic	5.38
Significance of F	0.00024618
D-W	2.02

Variable	Coefficient	St. Error	*t*-stat	p-value
Constant	1734.51338	980.84980	1.76838	0.08067
DCVL{1}	0.45624	0.10751	4.24384	0.00006
DCVL{2}	–0.13834	0.11302	–1.22401	0.22441
DCVL{3}	–0.00584	0.12220	–0.04777	0.96201
DCVL{4}	0.34071	0.12339	2.76124	0.00709
DCVL{5}	–0.31250	0.11596	–2.69480	0.00852

CIVIL CASES 2

Estimation by Least Squares	
Dependent Variable	**First Differenced Civil Cases 1948–1998**
Usable Observations	45
Degrees of Freedom	39
Centered R^2	0.32
Adjusted R^2	0.23
Uncentered R^2	0.41
$T*R^2$	18.42
Mean of Dep. Variable	4518.18
St. Error of Dep. Variable	11447.35
St. Error of Estimate	10060.94
Sum of Squared Errors	3947690221
F-Statistic	3.59
Significance of F	0.00909316
D-W	2.07

Variable	Coefficient	St. Error	*t*-stat	p-value
Constant	2886.14946	1808.43525	1.59594	0.11858
DCVL{1}	0.55028	0.15450	3.55951	0.00090
DCVL{2}	–0.14599	0.16657	–0.87649	0.38613
DCVL{3}	–0.09013	0.18453	–0.48845	0.62797
DCVL{4}	0.43503	0.19031	2.28587	0.02778
DCVL{5}	–0.42987	0.17137	–2.50843	0.01640

CIVIL CASES 3

Estimation by Least Squares	
Dependent Variable	**First Differenced Civil Cases 1960–1998**
Usable Observations	33
Degrees of Freedom	27
Centered R^2	0.36
Adjusted R^2	0.24
Uncentered R^2	0.47
$T*R^2$	15.59
Mean of Dep. Variable	5882.33
St. Error of Dep. Variable	12929.89
Sum of Squared Errors	345384897
F-Statistic	3.03
Significance of F	0.02669406
D-W	2.08

Variable	Coefficient	St. Error	*t*-stat	p-value
Constant	4034.15511	2494.00650	1.61754	0.11739
DCVL{1}	0.58619	0.18063	3.24534	0.00312
DCVL{2}	–0.19142	0.19703	–0.97153	0.33991
DCVL{3}	–0.11961	0.22217	–0.53839	0.59472
DCVL{4}	0.49915	0.23147	2.15644	0.04012
DCVL{5}	–0.51764	0.20437	–2.53283	0.01743

CRIMINAL CASES 1

Estimation by Least Squares	
Dependent Variable	**First Differenced Criminal Cases 1904–1998**
Usable Observations	87
Degrees of Freedom	79
Centered R^2	0.12
Adjusted R^2	0.04
Uncentered R^2	0.12
$T*R^2$	10.56
Mean of Dep. Variable	490.05
St. Error of Estimate	6787.84
Sum of Squared Errors	3639903176
F-Statistic	1.49
Significance of F	0.18174608
D-W	1.95

Variable	Coefficient	St. Error	*t*-stat	p-value
Constant	409.54144	733.35476	0.55845	0.57812
DDEPVAR{1}	0.22572	0.11151	2.02430	0.04632
DDEPVAR{2}	–0.13723	0.11112	–1.23495	0.22051
DDEPVAR{3}	0.13289	0.11195	1.18707	0.23876
DDEPVAR{4}	–0.02935	0.11245	–0.26103	0.79475
DDEPVAR{5}	0.07089	0.11140	0.63640	0.52636
DDEPVAR{6}	–0.23180	0.11065	–2.09480	0.03939
DDEPVAR{7}	0.18063	0.11124	1.62382	0.10840

CRIMINAL CASES 2

Estimation by Least Squares	
Dependent Variable	**Criminal Cases (Levels) 1948–1998**
Usable Observations	50
Degrees of Freedom	48
Centered R^2	0.81
Adjusted R^2	0.8
Uncentered R^2	0.99
$T*R^2$	49.71
Mean of Dep. Variable	38765.24
St. Error of Dep. Variable	6839.77
St. Error of Estimate	3044.06
Sum of Squared Errors	444781588
F-Statistic	199.38
Significance of F	0
D-W	1.54

Variable	Coefficient	St. Error	*t*-stat	p-value
Constant	1527.11523	2672.09824	0.57150	0.57032
CML{1}	0.97285	0.06890	14.12037	0.00000

CRIMINAL CASES 3

Estimation by Least Squares	
Dependent Variable	**First Differenced Criminal Cases 1948–1998**

The chosen model is a random walk without drift.

CRIMINAL CASES 4

Estimation by Least Squares	
Dependent Variable	**First Differenced Criminal Cases 1960–1998**
Usable Observations	38
Degrees of Freedom	37
Centered R^2	0
Adjusted R^2	0
Uncentered R^2	0.06
$T*R^2$	2.279
Mean of Dep. Variable	733.24
St. Error of Dep. Variable	2941.73
St. Error of Estimate	2941.73
Sum of Squared Errors	320189977
D-W	1.59

Variable	Coefficient	St. Error	*t*-stat	p-value
Constant	733.23684	477.21184	1.53650	0.13292

TOTAL CASES 1

Estimation by Least Squares	
Dependent Variable	**First Differenced Total Cases 1904–1998**
Usable Observations	89
Degrees of Freedom	83
Centered R^2	0.13
Adjusted R^2	0.08
Uncentered R^2	0.19
$T*R^2$	16.88
Mean of Dep. Variable	3222.99
St. Error of Dep. Variable	12469.9
St. Error of Estimate	11942.49
Sum of Squared Errors	11837706396
F-Statistic	2.59
Significance of F	0.03161627
D-W	2.04

Variable	Coefficient	St. Error	*t*-stat	p-value
Constant	2357.67209	1379.91854	1.70856	0.09127
DTOTAL{1}	0.32059	0.10850	2.95483	0.00407
DTOTAL{2}	–0.11950	0.11210	–0.06596	0.28953
DTOTAL{3}	0.06878	0.11700	0.58791	0.55819
DTOTAL{4}	0.16877	0.11704	1.44200	0.15306
DTOTAL{5}	–0.17562	0.11241	–1.56231	0.12202

TOTAL CASES 2

Estimation by Least Squares	
Dependent Variable	**First Differenced Total Cases 1948–1998**
Usable Observations	45
Degrees of Freedom	39
Centered R^2	0.32
Adjusted R^2	0.23
Uncentered R^2	0.42
T^*R^2	18.97
Mean of Dep. Variable	4944.56
St. Error of Dep. Variable	11838.15
St. Error of Estimate	10382.03
Sum of Squared Errors	4203679124
F-Statistic	3.64
Significance of F	0.00846113
D-W	2.09

Variable	Coefficient	St. Error	*t*-stat	p-value
Constant	3511.41132	1893.03287	1.85491	0.07118
DTOTAL{1}	0.52118	0.14993	3.47626	0.00126
DTOTAL{2}	–0.11115	0.15845	–0.70152	0.48715
DTOTAL{3}	–0.18412	0.17389	–1.05880	0.29621
DTOTAL{4}	0.48110	0.18080	2.66094	0.01125
DTOTAL{5}	–0.44741	0.16606	–2.69428	0.01035

TOTAL CASES 3

Estimation by Least Squares	
Dependent Variable	**First Differenced Total Cases 1960–1998**
Usable Observations	33
Degrees of Freedom	27
Centered R^2	0.33
Adjusted R^2	0.2
Uncentered R^2	0.46
$T*R^2$	15.34
Mean of Dep. Variable	6620.42
St. Error of Dep. Variable	13212.93
St. Error of Estimate	11806.96
Sum of Squared Errors	3763913105
F-Statistic	2.62
Significance of F	0.04712531
D-W	2.11

Variable	Coefficient	St. Error	*t*-stat	p-value
Constant	5340.195063	2733.84292	1.95337	0.061211
DTOTAL{1}	0.507557	0.177404	2.86102	0.008055
DTOTAL{2}	–0.143759	0.188102	–0.76426	0.451341
DTOTAL{3}	–0.199538	0.20776	–0.96043	0.345363
DTOTAL{4}	0.476682	0.21761	2.19053	0.037298
DTOTAL{5}	–0.498298	0.198461	–2.51081	0.018336

Table A.1.7. BDSL Results

CIVIL CASES 1904–1998						
Initial Obs.: 1, Num Obs.: N = 89, SD/Spread = 1.6078E–0001						
Epsilon	m	C1	Cm	BDSL	SD	BDSL/SD
0.1608	2	2240	1440	4.59E–01	9.66E–02	4.7571E+0000*
0.1608	3	2240	1013	7.80E–01	1.26E–01	6.1722E+0000*
0.1608	4	2240	728	8.54E–01	1.24E–01	6.8889E+0000*
0.1608	5	2240	529	8.02E–01	1.07E–01	7.5214E+0000*
0.0804	2	1289	577	3.98E–01	5.22E–02	7.6113E+0000*
0.0804	3	1289	318	4.60E–01	4.02E–02	1.1441E+0001*
0.0804	4	1289	178	3.43E–01	2.33E–02	1.4732E+0001*
0.0804	5	1289	95	2.10E–01	1.18E–02	1.7780E+0001*
0.2412	2	2927	2260	2.97E–01	8.86E–02	3.3584E+0000*
0.2412	3	2927	1793	5.76E–01	1.50E–01	3.8428E+0000*
0.2412	4	2927	1457	8.02E–01	1.90E–01	4.2287E+0000*
0.2412	5	2927	1190	9.22E–01	2.10E–01	4.3846E+0000*

CRIMINAL CASES 1904–1998						
Initial Obs.: 1, Num Obs.: N = 87, SD/Spread = 1.1107E–0001						
Epsilon	m	C1	Cm	BDSL	SD	BDSL/SD
0.1111	2	2605	1908	3.44E–01	1.03E–01	3.3327E+0000*
0.1111	3	2605	1413	5.36E–01	1.63E–01	3.2810E+0000*
0.1111	4	2605	1041	5.82E–01	1.94E–01	3.0053E+0000*
0.1111	5	2605	764	5.54E–01	2.01E–01	2.7559E+0000*
0.0555	2	1580	817	4.19E–01	8.08E–02	5.1800E+0000*
0.0555	3	1580	436	4.31E–01	7.92E–02	5.4453E+0000*
0.0555	4	1580	238	3.34E–01	5.83E–02	5.73E+00
0.0555	5	1580	126	2.15E–01	3.76E–02	5.7132E+0000*
0.1666	2	3138	2641	1.76E–01	8.05E–02	2.1839E+0000*
0.1666	3	3138	2247	3.62E–01	1.52E–01	2.3750E+0000*
0.1666	4	3138	1903	4.66E-01	2.16E–01	2.1615E+0000*
0.1666	5	3138	1601	5.03E–01	2.67E–01	1.88E+00

TOTAL CASES 1904–1998						
Initial Obs.: 1, Num Obs.: N = 89, SD/Spread = 1.3416E–0001						
Epsilon	m	C1	Cm	BDSL	SD	BDSL/SD
0.1342	2	2429	1593	2.95E–01	9.97E–02	2.9539E+0000*
0.1342	3	2429	1043	3.75E–01	1.41E–01	2.6557E+0000*
0.1342	4	2429	707	4.21E–01	1.50E–01	2.8155E+0000*
0.1342	5	2429	494	4.29E–01	1.39E–01	3.0911E+0000*
0.0671	2	1358	553	2.27E–01	5.52E–02	4.1143E+0000*
0.0671	3	1358	256	2.49E–01	4.47E–02	5.5834E+0000*
0.0671	4	1358	122	1.75E–01	2.71E–02	6.4662E+0000*
0.0671	5	1358	61	1.11E–01	1.44E–02	7.7039E+0000*
0.2013	2	3047	2416	2.41E–01	8.44E–02	2.8595E+0000*
0.2013	3	3047	1898	3.38E–01	1.48E–01	2.2822E+0000*
0.2013	4	3047	1542	5.13E–01	1.95E–01	2.6296E+0000*
0.2013	5	3047	1275	6.63E–01	2.25E–01	2.9492E+0000*

CIVIL CASES 1948–1998						
Initial Obs.: 1, Num Obs.: N = 45, SD/Spread = 2.0175E–0001						
Epsilon	m	C1	Cm	BDSL	SD	BDSL/SD
0.2018	2	533	304	2.09E–01	7.87E–02	2.66E+00
0.2018	3	533	175	2.48E–01	9.77E–02	2.53E+00
0.2018	4	533	114	3.14E–01	9.11E–02	3.4428E+0000*
0.2018	5	533	79	3.27E–01	7.43E–02	4.4034E+0000*
0.1009	2	277	86	8.37E–02	3.34E–02	2.51E+00
0.1009	3	277	30	7.42E–02	2.25E–02	3.30E+00
0.1009	4	277	11	4.31E–02	1.14E–02	3.78E+00
0.1009	5	277	2	4.64E–03	5.05E–03	9.19E-01
0.3027	2	722	528	1.74E–01	7.39E–02	2.36E+00
0.3027	3	722	386	2.60E–01	1.22E–01	2.12E+00
0.3027	4	722	290	3.50E–01	1.52E–01	2.31E+00
0.3027	5	722	221	4.05E–01	1.65E–01	2.46E+00

CRIMINAL CASES 1948–1998						
Initial Obs.: 1, Num Obs.: N = 50, SD/Spread = 2.1635E–0001						
Epsilon	m	C1	Cm	BDSL	SD	BDSL/SD
0.2164	2	674	415	3.51E–01	8.51E–02	4.1256E+0000*
0.2164	3	674	265	4.74E–01	1.08E–01	4.3911E+0000*
0.2164	4	674	168	4.37E–01	1.03E–01	4.2644E+0000*
0.2164	5	674	106	3.53E–01	8.53E–02	4.1334E+0000*
0.1082	2	365	138	2.00E–01	3.89E–02	5.14E+00
0.1082	3	365	54	1.48E–01	2.77E–02	5.36E+00
0.1082	4	365	22	8.55E–02	1.48E–02	5.79E+00
0.1082	5	365	14	7.58E–02	6.91E–03	1.10E+01
0.3246	2	883	679	4.05E–01	8.07E–02	5.0169E+0000*
0.3246	3	883	527	6.42E–01	1.32E–01	4.8643E+0000*
0.3246	4	883	403	7.05E–01	1.62E–01	4.3561E+0000*
0.3246	5	883	304	6.72E–01	1.74E–01	3.8707E+0000*

TOTAL CASES 1948–1998						
Initial Obs.: 1, Num Obs.: N = 45, SD/Spread = 2.1491E–0001						
Epsilon	m	C1	Cm	BDSL	SD	BDSL/SD
0.2149	2	531	293	1.46E–01	8.56E–02	1.71E+00
0.2149	3	531	166	1.94E–01	1.06E–01	1.83E+00
0.2149	4	531	100	2.16E–01	9.86E–02	2.19E+00
0.2149	5	531	55	1.45E–01	8.03E–02	1.81E+00
0.10745	2	300	86	–6.09E–03	5.39E–02	–1.13E–01
0.10745	3	300	28	2.09E–02	3.94E–02	5.29E–01
0.10745	4	300	8	5.57E–03	2.16E–02	2.57E–01
0.10745	5	300	2	–7.44E–04	1.04E–02	–7.14E–02
0.32235	2	705	500	1.42E–01	7.95E–02	1.79E+00
0.32235	3	705	355	2.10E–01	1.29E–01	1.63E+00
0.32235	4	705	263	3.13E–01	1.56E–01	2.01E+00
0.32235	5	705	195	3.50E–01	1.66E–01	2.11E+00

Notes: $C_m(\epsilon)$ is the correlation integral which measures the number of vectors within ϵ distance from one another and is given by

$$C_m(\varepsilon) = \lim_{T \to \infty} \frac{1}{T^2} \times \# \left\{ j, k \middle| \left\| y_t^m - y_k^m \right\| < \varepsilon \right\}; \ m = 2, 3, \ldots,$$

where $\#\{\cdot\}$, $\|\cdot\|$, T, and ϵ denote the cardinality of the set $\{\cdot\}$, some norm, the number of m histories, and the embedding dimension, respectively. We let $\|\cdot\|$ measure Euclidean distance. The sequence of m histories of the series is defined as

$$y_j^m = \left(y_j, \ldots, y_{j+m-1}\right),$$

that is, the *m*-dimensional vectors obtained by putting *m* consecutive observations together.

BDSL show that under the null hypothesis the time series y_t is independently and identically distributed (i.i.d.) with a nondegenerate density G, $C_m(\varepsilon) \rightarrow C_1(\varepsilon)^m$ with probability one as $T \rightarrow \infty$ for any fixed *m* and ϵ. They show that the test statistic $\sqrt{T}\left(C_m(\varepsilon) - C_1(\varepsilon)^m\right)$ has a normal limiting distribution with zero mean and variance *V* (see Brock, Dechert, Scheinkman and LeBaron 1996 for definition of the variance *V*). In the table providing the results, C1 is the correlation integral for embedding dimension 1, Cm is the correlation integral for embedding dimension *m*, BDSL is $\sqrt{T}\left(C_m(\varepsilon) - C_1(\varepsilon)^m\right)$, SD is $\sqrt{V}$, and BDSL/SD is the BDSL test statistic. Simulations show that the BDSL test has good power against simple nonlinear deterministic systems as well as nonlinear stochastic processes. An asterisk (*) denotes significance at the 5% level of statistical confidence.

Table A.1.8. Finite-Sample Critical Values for the BDSL Test Statistic

	$\varepsilon/\sigma = 0.5$			
Sample Size (n)	$m=2$	$m=3$	$m=4$	$m=5$
n=50	5.66	6.88	9.14	13.14
n =100	3.24	3.83	4.77	6.61
	$\varepsilon/\sigma = 1.0$			
	$m=2$	$m=3$	$m=4$	$m=5$
n=50	2.76	2.89	3.03	3.27
n =100	2.16	2.20	2.28	2.41
	$\varepsilon/\sigma = 1.5$			
	$m=2$	$m=3$	$m=4$	$m=5$
n=50	2.46	2.46	2.47	2.46
n =100	2.02	2.02	2.00	2.03

Notes: ϵ is distance, σ is the standard deviation of the data, and m is the embedding dimension. The critical values of the BDSL test statistic for the specific sample sizes were obtained from Kanzler (1999) and they correspond to a right-tailed test at the 5% level.

Table A.1.9. Forecasts for Total, Civil, and Criminal Cases

Total Cases

Year	Full Sample AR(5)	1948–1998 AR(5)	1960–1998 AR(5)
1999	315,351	305,712	306,675
2000	324,567	319,657	322,981
2001	324,282	320,537	325,247
2002	323,230	318,363	324,504
2003	327,455	317,398	326,110
2004	332,675	331,117	343,581
2005	334,462	336,471	350,662
2006	336,932	339,985	355,280
2007	341,125	342,716	359,596
2008	344,794	353,306	372,578
2009	347,381	357,824	377,635
2010	350,522	361,305	381,487
2011	354,103	363,921	385,222
2012	357,294	371,450	394,932
2013	360,226	375,389	399,837
2014	363,503	379,288	404,842
2015	366,821	382,708	409,940
2016	369,967	389,296	418,938
2017	373,100	393,669	424,612
2018	376,342	398,211	430,464
2019	379,571	402,292	436,099
2020	382,740	408,259	444,074

Civil Cases

Year	Full Sample AR(5)	1948–1998 AR(5)	1960–1998 AR(5)
1999	249,785	246,381	245,534
2000	259,946	257,592	259,347
2001	258,870	257,745	261,180
2002	252,652	252,142	255,885
2003	254,016	252,937	257,084
2004	262,889	266,414	275,336
2005	264,978	272,354	284,237
2006	264,647	273,966	286,260
2007	268,298	278,411	290,933
2008	274,329	288,494	304,743
2009	276,251	292,925	310,732
2010	277,241	294,525	311,477
2011	280,474	297,976	314,434
2012	284,448	304,605	323,818
2013	286,314	308,083	328,538
2014	288,067	310,396	330,460
2015	291,113	314,264	334,686
2016	294,327	320,027	343,418
2017	296,490	323,975	349,031
2018	298,764	327,355	352,694
2019	301,707	331,693	357,871
2020	304,601	336,962	365,739

Criminal Cases

Year	Full Sample AR(7)	1960–1998 Random Walk With Drift
1999	60,098	58,424
2000	60,553	59,157
2001	61,973	59,891
2002	62,234	60,624
2003	62,443	61,357
2004	61,512	62,090
2005	61,150	62,824
2006	61,621	63,557
2007	61,746	64,290
2008	62,052	65,023
2009	62,473	65,757
2010	63,128	66,490
2011	63,782	67,223
2012	64,196	67,956
2013	64,677	68,690
2014	65,164	69,423
2015	65,603	70,156
2016	65,991	70,889
2017	66,356	71,623
2018	66,777	72,356
2019	67,193	73,089
2020	67,594	73,822

Table A.1.10. Long-Memory Forecasting Results for Civil Cases

Year	Civil Cases
1999	249,560
2000	260,693
2001	260,554
2002	256,275
2003	259,423
2004	269,392
2005	271,430
2006	271,723
2007	276,004
2008	282,169
2009	283,885
2010	285,614
2011	289,748
2012	294,257
2013	296,550
2014	299,332
2015	303,350
2016	307,189
2017	309,954
2018	313,183
2019	316,980
2020	320,515

Table A.1.11. Johansen Cointegration Results for the System of Civil and Criminal Cases

	Trace Test Statistics					
VAR Lag Length	1904–1998		1948–1998		1960–1998	
	H_0: $r = 0$	H_0: $r \leq 1$	H_0: $r = 0$	H_0: $r \leq 1$	H_0: $r = 0$	H_0: $r \leq 1$
$k = 3$	4.7965	0.1414	4.7503	0.2498	3.8713	1.0196
$k = 4$	5.6981	0.0447	6.8527	0.0665	4.1987	0.7785
$k = 5$	6.6612	0.0374	5.1926	0.0286	2.9353	0.5259

Notes: The system variables are civil cases and criminal cases. The asymptotic critical values (without a drift in the data generating process), obtained from Osterwald-Lenum (1992), are presented in the following table, in which p is the number of system variables and r is the cointegration rank:

Trace		
$p - r$	1%	10%
1	11.65	6.50
2	23.52	15.66

Table A.1.12. Johansen Cointegration Results for the System of Authorized Judgeships, Civil, and Criminal Cases

Trace Test Statistics			
Sample Period	H_0: $r \leq 2$	H_0: $r \leq 1$	H_0: $r = 0$
1904–1998	19.4720	7.3299	1.2491
1948–1998	17.5770	6.4597	0.4495
1960–1998	12.4740	4.7186	0.8310

Notes: The system variables are authorized judgeships, civil cases and criminal cases. The asymptotic critical values (without a drift in the data generating process), obtained from Osterwald-Lenum (1992), are presented in the following table, in which p is the number of system variables and r is the cointegration rank:

Trace			
$p-r$	1%	5%	10%
1	11.65	8.18	6.50
2	23.52	17.95	15.66
3	37.22	31.25	28.71

Table A.1.13: Johansen Cointegration Results for the System of Authorized Judgeships and Total Case Loads

Trace Test Statistics						
VAR Lag Length	1904–1998		1948–1998		1960–1998	
	H_0: $r = 0$	H_0: $r \leq 1$	H_0: $r = 0$	H_0: $r \leq 1$	H_0: $r = 0$	H_0: $r \leq 1$
$k = 3$	8.1905	1.0683	8.1074	0.0251	7.7622	0.5609
$k = 4$	10.9772	1.3486	8.9318	0.0312	8.3884	0.7002
$k = 5$	12.2222	2.8774	12.3908	0.0028	15.6629	0.6871

Notes: The system variables are civil cases and criminal cases. The asymptotic critical values (without a drift in the data generating process), obtained from Osterwald-Lenum (1992), are presented in the following table, in which p is the number of system variables and r is the cointegration rank:

Trace			
$p - r$	1%	5%	10%
1	11.65	8.18	6.50
2	23.52	17.95	15.66

APPENDIX 2

Table A.2.1. District-wide Models with South v. Non-South Indicator Variable

Total District-wide w/o Circuit Dummies				U.S. District-wide w/o Circuit Dummies			Private District-wide w/o Circuit Dummies		
Variables	Coefficient Estimates	*t*-values	p-value	Coefficient Estimates	*t*-values	p-value	Coefficient Estimates	*t*-values	p-value
Constant	–0.00001	–0.25	NS	–0.00001	–0.38	NS	–0.00006	–1.27	NS
Density	0.00068	20.83	0.000	0.00024	24.15	0.000	0.00049	6.47	0.000
Income	0.00000	14.67	0.000	0.00000	6.58	0.000	0.00000	6.44	0.000
Government	0.00090	9.04	0.000	0.00056	17.84	0.000	0.00026	2.15	***0.032
U. Rate	0.00406	6.80	0.000	0.00178	9.34	0.000	0.00270	4.71	0.000
Race	0.00066	4.93	0.000	0.00007	1.72	***0.086	0.00061	6.82	0.000
South	0.00032	8.97	0.000	0.00009	7.30	0.000	0.00018	5.82	0.000
Overall Significance = 0.000				Overall Significance = 0.000			Overall Significance = 0.000		
N = 3,398				N = 3,398			N = 3,398		

Note: Asterisks (***) denote standard errors affected by corrections for violations of the assumptions of the classical linear model.

Table A.2.2. District-wide Models with South v. Non-South Indicator Variable

	Total District-wide with Circuit Dummies			U.S. District-wide with Circuit Dummies			Private District-wide with Circuit Dummies		
Variables	Coefficient Estimates	*t*-values	p-value	Coefficient Estimates	*t*-values	p-value	Coefficient Estimates	*t*-values	p-value
Constant	0.00049	6.95	0.000	0.00015	8.35	0.000	0.00022	3.22	0.001
Density	–0.00013	–2.03	0.042	–0.00008	–4.74	0.000	0.00012	1.67	***0.095
Income	0.00000	11.69	0.000	0.00000	6.10	0.000	0.00000	6.65	0.000
Government	0.00015	1.17	NS	0.00016	5.05	0.000	–0.00010	–0.77	NS
U. Rate	0.00318	3.83	0.000	0.00083	4.69	0.000	0.00209	3.46	0.001
Race	0.00048	5.02	0.000	0.00003	0.89	NS	0.00058	6.35	0.000
DC	0.00459	15.81	0.000	0.00203	24.80	0.000	0.00207	6.47	0.000
First	–0.00043	–9.01	0.000	–0.00008	–3.31	0.001	–0.00024	–7.75	0.000
Second	–0.00019	–2.76	0.006	–0.00007	–3.00	0.003	–0.00011	–1.16	NS
Third	–0.00025	–4.53	0.000	–0.00005	–2.40	0.002	–0.00012	–3.80	0.000
Fourth	–0.00016	–2.17	0.003	–0.00004	–2.30	0.022	–0.00013	–2.81	0.005
Sixth	–0.00031	–7.69	0.000	–0.00000	–0.07	NS	–0.00022	–9.30	0.000
Seventh	–0.00038	–7.90	0.000	–0.00008	–3.71	0.000	–0.00020	–6.88	0.000
Eighth	–0.00029	–6.40	0.000	–0.00000	–0.12	NS	–0.00017	–6.71	0.000
Ninth	–0.00036	–8.27	0.000	–0.00001	–0.66	***NS	–0.00025	–9.88	0.000
Tenth	–0.00019	–4.40	0.000	–0.00000	–0.23	NS	–0.00008	–2.94	0.003
Overall Significance = 0.000				Overall Significance = 0.000			Overall Significance = 0.000		
N = 3,398				N = 3,398			N = 3,398		

Note: Asterisks (***) denote standard errors affected by corrections for violations of the assumptions of the classical linear model.

Table A.2.3. Statewide Models with Southern v. Non-South Indicator Variable

	Total Statewide w/o Circuit Dummies			U.S. Statewide w/o Circuit Dummies			Private Statewide w/o Circuit Dummies		
Variables	Coefficient Esti-mates	*t*-values	p-value	Coefficient Esti-mates	*t*-values	p-value	Coefficient Esti-mates	*t*-values	p-value
Constant	–0.07390	–1.87	0.062	–0.15300	–9.74	0.000	0.0901	3.02	0.003
Density	0.00106	37.82	0.000	0.00032	30.68	0.000	0.00075	34.84	0.000
Income	0.00003	17.43	0.000	0.00001	9.18	0.000	0.00002	18.25	0.000
Government	0.67350	2.14	0.032	1.28960	10.88	0.000	–0.89720	–3.69	***0.000
U. Rate	0.04930	10.12	0.000	0.03080	16.03	***0.000	0.0194	5.25	0.000
Race	0.00283	2.92	0.004	0.00086	2.37	0.018	0.00229	3.05	0.002
South	0.27220	9.10	0.000	0.03240	2.77	0.006	0.2155	9.48	0.000
Overall Significance = 0.000				Overall Significance = 0.000			Overall Significance = 0.000		
N = 1,938				N = 1,938			N = 1,938		

Note: Asterisks (***) denote standard errors affected by corrections for violations of the assumptions of the classical linear model.

Table A.2.4. Statewide Models with Circuit Dummies

	Total Statewide with Circuit Dummies			U.S. Statewide with Circuit Dummies			Private Statewide with Circuit Dummies		
Variables	Coefficient Estimates	*t*-values	p-value	Coefficient Esti-mates	*t*-values	p-value	Coefficient Estimates	*t*-values	p-value
Constant	0.43587	5.10	0.000	0.02640	1.58	NS	0.42055	5.62	0.000
Density	0.00036	1.24	***NS	–0.00062	–28.00	0.000	0.00107	3.67	0.000
Income	0.00003	18.41	0.000	0.00000	7.45	0.000	0.00002	21.11	0.000
Government	–0.29087	–0.31	NS	0.49130	6.23	0.000	–1.09977	–1.18	***NS
U. Rate	0.04949	11.21	0.000	0.02470	17.14	0.000	0.02114	6.26	0.000
Race	0.00101	1.06	NS	0.00059	2.59	0.010	0.00090	1.18	NS
DC	3.00630	3.07	0.002	4.26790	45.88	0.000	–1.54659	–1.61	NS
First	–0.57132	–2.40	0.016	–0.02120	–1.28	NS	–0.52454	–8.39	0.000
Second	–0.35206	–4.84	0.000	–0.04160	–2.42	***0.016	–0.38557	–7.12	0.000
Third	–0.47717	–5.97	0.000	0.01320	0.78	NS	–0.41306	–6.09	0.000
Fourth	–0.35102	–6.90	0.000	0.00523	0.39	NS	–0.33046	–9.84	0.000
Sixth	–0.40787	–7.69	0.000	0.01110	0.79	NS	–0.36465	–10.05	0.000
Seventh	–0.53621	–3.10	0.002	–0.06860	–4.14	0.000	–0.40195	–11.12	0.000
Eighth	–0.35979	–6.64	0.000	–0.01230	–0.79	NS	–0.30705	–9.30	0.000
Ninth	–0.35056	–6.73	0.000	–0.03360	–2.32	0.020	–0.28640	–8.40	0.000
Tenth	–0.26847	–4.94	0.000	–0.02240	–1.54	NS	–0.20629	–6.46	0.000
Overall Significance = 0.000				Overall Significance =0.000			Overall Significance = 0.000		
N = 1,938				N = 1,938			N = 1,938		

Note: Asterisks (***) denote standard errors affected by corrections for violations of the assumptions of the classical linear model.

Table A.2.5. Circuit-wide Models without Circuit Dummies and South v. Non-South Indicator Variable

Total Circuit-wide w/o Circuit Dummies				U.S. Circuit-wide w/o Circuit Dummies			Private Circuit-wide w/o Circuit Dummies		
Variables	Coefficient Estimates	*t*-values	p-value	Coefficient Estimates	*t*-values	p-value	Coefficient Estimates	*t*-values	p-value
Constant	0.33967	1.49	NS	–0.356	–11.03	0.000	0.71588	8.28	0.000
Density	1.13932	5.65	0.000	–0.1143	–4.89	0.000	1.26827	19.23	0.000
Income	0.00003	7.91	0.000	0.00001	5.12	0.000	0.00002	6.63	0.000
Government	0.15557	0.08	***NS	5.8448	23.74	0.000	–5.60876	–8.08	0.000
U. Rate	–1.56197	–0.64	***NS	0.5774	1.55	***NS	–2.37113	–2.31	0.021
Race	0.23233	0.54	***NS	0.4635	3.57	0.000	–0.39061	–1.05	NS
Overall Significance = 0.000				Overall Significance = 0.000			Overall Significance = 0.000		
N = 418				N = 418			N = 418		

Note: Asterisks (***) denote standard errors affected by corrections for violations of the assumptions of the classical linear model.

Table A.2.6. Circuit-wide Models with Circuit Dummies and without South v. Non-South Indicator Variable

Total Circuit-wide with Circuit Dummies				U.S. Circuit-wide with Circuit Dummies			Private-wide with Circuit Dummies		
Variables	Coefficient Estimates	*t*-values	p-value	Coefficient Estimates	*t*-values	p-value	Coefficient Estimates	*t*-values	p-value
Constant	1.48620	8.73	0.000	0.01680	0.41	NS	1.51033	9.40	0.000
Density	1.04860	7.05	0.000	–0.73580	–21.44	0.000	0.00004	10.43	0.000
Income	0.00005	10.18	0.000	0.00000	2.77	0.006	1.81698	12.92	0.000
Government	–0.95900	–0.98	NS	2.43440	10.47	0.000	–3.40708	–3.71	0.000
U. Rate	–0.01900	–0.02	NS	0.26190	1.00	***NS	–0.52359	–0.52	NS
Race	–4.37220	–5.80	0.000	0.16190	0.96	***NS	–4.72626	–6.56	0.000
DC	2.50900	3.04	0.003	4.00860	20.89	0.000	–1.52727	–1.96	0.051
First	–1.27890	–7.32	0.000	–0.05430	–1.29	NS	–1.25009	–7.55	0.000
Second	–0.64480	–5.33	0.000	–0.01340	–0.43	NS	–0.65485	–5.76	0.000
Third	–0.91400	–6.57	0.000	0.00693	0.21	NS	–0.91807	–6.98	0.000
Fourth	–0.31750	–3.11	0.002	–0.01960	–0.88	NS	–0.28828	–2.90	0.004
Sixth	–0.78910	–5.99	0.000	0.01930	0.67	NS	–0.81590	–6.40	0.000
Seventh	–0.89250	–6.60	0.000	–0.04710	–1.46	NS	–0.82915	–6.48	0.000
Eighth	–0.92820	–6.18	0.000	–0.02540	–0.70	NS	–0.91723	–6.45	0.000
Ninth	–0.58970	–5.20	0.000	–0.05830	–1.97	NS	–0.51816	–4.87	0.000
Tenth	–0.69790	–5.02	0.000	–0.03550	–1.02	NS	–0.67345	–5.13	0.000
Overall Significance = 0.000				Overall Significance = 0.000			Overall Significance = 0.000		
N = 418				N = 418			N = 418		

Note: Asterisks (***) denote standard errors affected by corrections for violations of the assumptions of the classical linear model.

Table A.2.7. Statewide Models with Circuit Dummies, 1976–1998

	Total Statewide with Circuit Dummies			U.S. Statewide with Circuit Dummies			Private Statewide with Circuit Dummies		
Variables	Coefficient Estimates	*t*-values	p-value	Coefficient Estimates	*t*-values	p-value	Coefficient Estimates	*t*-values	p-value
Constant	0.5891	7.33	<.0001	0.08433	3.16	0.0016	0.40721	7.71	<.0001
Density	–0.0014	–8.42	<.0001	–0.00038	–7.23	<.0001	–0.00066	–6.29	<.0001
Income	1.7E–05	8.03	<.0001	–1.2E–06	–1.79	0.0742	1.75E–05	12.85	<.0001
Government	0.3195	0.88	0.3807	0.37003	3.06	0.0023	–0.36729	–1.54	0.125
U. Rate	0.054	8.53	<.0001	0.0302	14.35	<.0001	0.02948	7.09	<.0001
Race	0.0064	5.16	<.0001	0.00086	2.09	0.0365	0.00615	7.54	<.0001
DC	8.5842	14.56	<.0001	3.55576	18.15	<.0001	4.01095	10.36	<.0001
First	–0.3412	–5.17	<.0001	–0.01252	–0.57	0.5683	–0.29242	–6.75	<.0001
Second	–0.1885	–2.93	0.0035	–0.01499	–0.70	0.4837	–0.27024	–6.39	<.0001
Third	–0.2211	–3.32	0.0009	–0.02319	–1.05	0.295	–0.11183	–2.56	0.0107
Fourth	–0.3606	–7.19	<.0001	0.03426	2.06	0.04	–0.34141	–10.37	<.0001
Sixth	–0.3759	-6.75	<.0001	0.00565	0.31	0.7601	–0.3273	–8.95	<.0001
Seventh	–0.5518	–9.07	<.0001	–0.07222	–3.57	0.0004	–0.37937	–9.49	<.0001
Eighth	–0.3875	–7.40	<.0001	0.03195	1.84	0.0665	–0.32618	–9.49	<.0001
Ninth	–0.4537	–10.26	<.0001	–0.02172	–1.48	0.1397	–0.38099	–13.12	<.0001
Tenth	–0.3474	–6.76	<.0001	0.00172	0.1	0.9197	–0.2585	–7.66	<.0001
Overall Significance = 0.000				Overall Significance = 0.000			Overall Significance = 0.000		
N = 1173				N = 1173			N = 1173		

APPENDIX 3

Table A.3.1. District-Wide Data (1968–1998) with No Lags

A. Fixed effects models to explain variations in median time from filing to disposition (in months) of civil cases (fixed effects coefficients are excluded)

1. Dependent Variable = Filing to Disposition (in months) of Civil Cases

Variable	Estimate	St. Error	*t*-stat	p-value
Intercept	4.61838	0.66144	6.98	<.0001
Criminal Filing to Disposition	0.27568	0.06211	4.44	<.0001

$R^2 = 0.48$
N = 1,530

2. Dependent Variable = Filing to Disposition (in months) of Civil Cases

Variable	Estimate	St. Error	*t*-stat	p-value
Intercept	6.48528	0.67911	9.55	<.0001
Number of Authorized Judgeships	–0.1419	0.08744	–1.62	0.1048
Criminal Filing to Disposition	0.28287	0.06223	4.55	<.0001

$R^2 = 0.48$
N = 1,530

3. Dependent Variable = Filing to Disposition (in months) of Civil Cases

Variable	Estimate	St. Error	*t*-stat	p-value
Intercept	7.07582	1.41387	5.00	<.0001
Number of Authorized Judgeships	–0.15446	0.08776	–1.76	0.0786
Criminal Filing to Disposition	0.27946	0.06224	4.49	<.0001
Percent Drug & Immigration	–1.18637	0.75466	–1.57	0.1162

$R^2 = 0.49$
N = 1,530

B. Least squares models to explain variations in median time from filing to disposition (in months) of civil cases

1. Dependent Variable = Filing to Disposition (in months) of Civil Cases

Variable	Estimate	St. Error	*t*-stat	p-value
Intercept	8.37611	0.25490	32.86	<.0001
Criminal Filing to Disposition	0.14501	0.04306	3.37	0.0008

$R^2 = 0.01$
N = 1,530

2. Dependent Variable = Filing to Disposition (in months) of Civil Cases

Variable	Estimate	St. Error	*t*-stat	p-value
Intercept	8.21045	0.27269	30.11	<.0001
Number of Authorized Judgeships	0.13186	0.04372	3.02	0.0026
Criminal Filing to Disposition	0.90208	0.52827	1.71	0.0879

$R^2 = 0.01$
N = 1,530

3. Dependent Variable = Filing to Disposition (in months) of Civil Cases

Variable	Estimate	St. Error	*t*-stat	p-value
Intercept	8.59290	0.26991	31.84	<.0001
Number of Authorized Judgeships	–0.13161	0.01517	–8.68	<.0001
Criminal Filing to Disposition	0.19900	0.04339	4.59	<.0001
Percent Drug & Immigration	1.27354	0.51763	2.46	0.0140

$R^2 = 0.06$
N = 1,530

Table A.3.2. District-Wide Data (1968–1998) with Lags

A. Fixed effects model to explain variations in median time from filing to disposition (in months) of civil cases with one-period lagged value of time to disposition of criminal cases (fixed effects coefficients are excluded)

Dependent Variable = Filing to Disposition (in months) of Civil Cases

Variable	Estimate	St. Error	*t*-stat	p-value
Intercept	6.52466	1.44170	4.53	<.0001
Number of Authorized Judgeships	–0.19918	0.09067	–2.20	0.0282
Criminal Filing to Disposition (t)	0.17319	0.07842	2.21	0.0274
Criminal Filing to Disposition (t–1)	0.17869	0.07962	2.24	0.0250
Percent Drug and Immigration Cases	–0.68612	0.75048	–0.91	0.3608

$R^2 = 0.51$
N = 1,440

B. Least squares model to explain variations in median time from filing to disposition (in months) of civil cases with one-period lagged value of time to disposition of criminal cases using district-wide data, 1968–1998

Dependent Variable = Filing to Disposition (in months) of Civil Cases

Variable	Estimate	St. Error	*t*-stat	p-value
Intercept	8.75419	0.27910	31.37	<.0001
Number of Authorized Judgeships	–0.13640	0.01508	–9.04	<.0001
Criminal Filing to Disposition (t)	0.10129	0.08414	1.20	0.2288
Criminal Filing to Disposition (t–1)	0.09522	0.08791	1.08	0.2789
Percent Drug & Immigration	1.07758	0.52137	2.07	0.0389

$R^2 = 0.06$
N = 1,440

C. Least squares model to explain variations in median time from filing to disposition (in months) of civil cases with one-period lagged value of time to disposition of criminal cases using annual series, 1968–1998

Dependent Variable = Filing to Disposition (in months) of Civil Cases

Variable	Estimate	St. Error	*t*-stat	p-value
Intercept	11.8442	0.8498	13.94	<.0001
Number of Authorized Judgeships	–0.0116	0.0029	–4.02	0.0005
Criminal Filing to Disposition (t)	1.0597	0.5156	2.06	0.0504
Criminal Filing to Disposition (t–1)	–0.6328	0.4843	–1.31	0.2032

$R^2 = 0.43$
N = 30

APPENDIX 4

In this appendix, we present details on in-sample and out-of-sample forecasting. In addition to presenting in-sample estimates for the full baseline period (1904–1998), we analyze three other time periods, 1904–1990, 1940–1998 and 1940–1995. The last of these is the baseline period used by the Judicial Conference of the United States for the forecasts reported in its *Long Range Plan for the Federal Courts.*

Presentation of the in-sample estimates for the 1904–1998 period. Table A.4.1 presents the in-sample forecasts based on the autoregressive integrated (ARI) models estimated over the full sample for civil, criminal and total caseloads. We also present forecasting accuracy metrics such as root mean squared error (RMSE) and mean absolute deviation (MAD) in Table A.4.1.

Estimation over the 1904–1990 period, then forecasting over 1991–1998. We perform a model validation exercise by estimating the model over the 1904–1990 period (our "training set") and then generating forecasts over the remainder of the sample period 1991–1998 (our validation test). We subsequently compare the generated forecasts to the actual, observed values and comment on the forecasting performance of the estimated models. Table A.4.2 presents the selected ARI models over the sample period 1904–1990. The model selection strategy is similar to the one for the other sample periods considered earlier. Each series possesses a single unit root (integrated of order one) and an autoregressive filter is applied to the first-differenced series. The forecasted values over the period 1991-1998 are presented with the realized values of the civil, criminal, and total caseloads over the same subperiod in Table A.4.3.

Out-of-sample forecasts based on models estimated over the 1940–1998 period. Table A.4.4 presents the selected ARI models over the sample period 1904–1998. In Table A.4.5, we present the out-of-sample forecasts based on ARI models estimated over the sample period 1940-

1998 for civil, criminal, and total caseloads. This choice of sample period is made to coincide with the start of the sample period employed in the JCUS study (1940–1995). The model selection strategy is similar to the one for the other sample periods considered earlier. Each series possesses a single unit root (integrated of order one) and an autoregressive filter is applied to the first-differenced series. The multi-step-ahead dynamic forecasts over the period 1999–2020 generated by each model and for each series presented in Table A.4.5 are broadly consistent with those based on alternative sample periods.

Out-of-sample forecasts based on models estimated over the 1940–1995 period. Table A.4.6 presents the selected ARI models over the sample period 1940–1995. In Table A.4.5, we present the in-sample forecasts based on ARI models estimated over the sample period 1940–1995 for civil, criminal, and total caseloads and Table A.4.8 presents the out-of-sample forecasts based on ARI models estimated over the sample period 1940–1995 for civil, criminal, and total caseloads. This choice of sample period is made to coincide with the sample period employed in the JCUS study (1940–1995). The model selection strategy is similar to the one for the other sample periods considered earlier. Each series possesses a single unit root (integrated of order one) and an autoregressive filter is applied to the first-differenced series. The forecasts for the period 1999–2020 are multi-step ahead forecasts which are based solely on the observed values during the sample period 1940–1995.

Table A.4.1. Regression Results for the 1904–1990, 1940–1998 and 1940–1995 Periods

CIVIL CASES 1

Estimation by Least Squares	
Dependent Variable	**First Differenced Civil Cases 1904–1990**
Usable Observations	83
Degrees of Freedom	79
Centered R^2	0.27
Adjusted R^2	0.24
Uncentered R^2	0.32
T^*R^2	26.91
Mean of Dep. Variable	2397.43
St. Error of Dep. Variable	8548.37
St. Error of Estimate	7438.92
Sum of Squared Errors	4371668179
F-Statistic	9.76
Significance of F	0.000001499
D-W	1.99

Variable	Coefficient	St. Error	*t*-stat	p-value
Constant	978.7414781	890.3636	1.09926	0.27499
DDEPVAR{1}	0.5766388	0.11283	5.1107	2.2E-06
DDEPVAR{2}	-0.2242973	0.12766	-1.75699	0.08279
DDEPVAR{3}	0.1909498	0.113578	1.68122	0.09667

CIVIL CASES 2

Estimation by Least Squares	
Dependent Variable	**First Differenced Civil Cases 1940–1998**
Usable Observations	53
Degrees of Freedom	47
Centered R^2	0.31
Adjusted R^2	0.24
Uncentered R^2	0.39
T^*R^2	20.58
Mean of Dep. Variable	3840.58
St. Error of Dep. Variable	10988.71
St. Error of Estimate	9586.19
Sum of Squared Errors	4319071295
F-Statistic	4.27
Significance of F	0.00279529
D-W	1.91

Variable	Coefficient	St. Error	*t*-stat	p-value
Constant	2272.57896	1550.83208	1.46539	0.14947
DCVL{1}	0.51900	0.13388	3.87668	0.00033
DCVL{2}	-0.15046	0.14009	-1.07401	0.28830
DCVL{3}	-0.05055	0.15284	-0.33073	0.74232
DCVL{4}	0.39310	0.15551	2.52790	0.01489
DCVL{5}	-0.38338	0.14499	-2.64418	0.01110

CIVIL CASES 3

Estimation by Least Squares	
Dependent Variable	**First Differenced Civil Cases 1940–1995**
Usable Observations	50
Degrees of Freedom	44
Centered R^2	0.29
Adjusted R^2	0.21
Uncentered R^2	0.37
T^*R^2	18.68
Mean of Dep. Variable	3710.56
St. Error of Dep. Variable	10311.06
St. Error of Estimate	9162.88
Sum of Squared Errors	3694165802
F-Statistic	3.61
Significance of F	0.00802046
D-W	1.92

Variable	Coefficient	St. Error	*t*-stat	p-value
Constant	2134.16926	1503.05177	1.41989	0.16269
DCVL{1}	0.50806	0.13721	3.70274	0.00059
DCVL{2}	-0.08254	0.15092	-0.54689	0.58722
DCVL{3}	-0.02162	0.15811	-0.13671	0.89188
DCVL{4}	0.26910	0.17463	1.54101	0.13048
DCVL{5}	-0.27799	0.15810	-1.75834	0.08564

CRIMINAL CASES 1

Estimation by Least Squares	
Dependent Variable	**Criminal Cases (Levels) 1904-1990**
Usable Observations	79
Degrees of Freedom	71
Centered R^2	0.13
Adjusted R^2	0.04
Uncentered R^2	0.13
$T*R^2$	10.33
Mean of Dep. Variable	428.44
St. Error of Dep. Variable	7194.95
St. Error of Estimate	7043.62
Sum of Squared Errors	3522494501
F-Statistic	1.48
Significance of F	0.1869571
D-W	1.96

Variable	Coefficient	St. Error	*t*-stat	p-value
Constant	338.0621593	797.0368066	0.42415	0.67274
DDEPVAR{1}	0.2328821	0.1165457	1.9982	0.049525
DDEPVAR{2}	-0.1491343	0.1162436	-1.28295	0.203683
DDEPVAR{3}	0.1434511	0.1170792	1.22525	0.224531
DDEPVAR{4}	-0.0343099	0.1176772	-0.29156	0.771474
DDEPVAR{5}	0.084421	0.1165375	0.72441	0.471194
DDEPVAR{6}	-0.2402088	0.1156338	-2.07732	0.04139
DDEPVAR{7}	0.1911306	0.1160747	1.64662	0.104056

CRIMINAL CASES 2

Estimation by Least Squares	
Dependent Variable	**First Differenced Criminal Cases 1940-1998**
Usable Observations	57
Degrees of Freedom	55
Centered R^2	0.02
Adjusted R^2	0.003
Uncentered R^2	0.02
$T*R^2$	2.443
Mean of Dep. Variable	453.82
St. Error of Dep. Variable	3020.28
St. Error of Estimate	3015.67
Sum of Squared Errors	500182977
F-Statistic	1.17
Significance of F	0.28
D-W	1.9

Variable	Coefficient	St. Error	*t*-stat	p-value
Constant	408.84340	401.59056	1.01806	0.3131
DCML{1}	0.15116	0.13965	1.08240	0.2838

CRIMINAL CASES 3

Estimation by Least Squares	
Dependent Variable	**First Differenced Criminal Cases 1940-1995**
Usable Observations	57
Degrees of Freedom	55
Centered R^2	0.01
Adjusted R^2	-0.01
Uncentered R^2	0.02
$T*R^2$	1.072
Mean of Dep. Variable	242.61
St. Error of Dep. Variable	2916.41
St. Error of Estimate	2925.21
Sum of Squared Errors	444956581
F-Statistic	0.68
Significance of F	0.41
D-W	1.98

Variable	Coefficient	St. Error	*t*-stat	p-value
Constant	217.2610941	399.2535597	0.54417	0.588652
DCML{1}	0.11339471	0.13736932	0.82547	0.412874

TOTAL CASES 1

Estimation by Least Squares	
Dependent Variable	**Total Cases (Levels) 1904-1990**
Usable Observations	83
Degrees of Freedom	79
Centered R^2	0.15
Adjusted R^2	0.12
Uncentered R^2	0.2
$T*R^2$	16.34
Mean of Dep. Variable	2765.77
St. Error of Dep. Variable	12076.87
St. Error of Estimate	11315.69
Sum of Squared Errors	10115545786
F-Statistic	4.8
Significance of F	0.004
D-W	1.97

Variable	Coefficient	St. Error	*t*-stat	p-value
Constant	1605.569104	1323.281722	1.21332	0.228621
DDEPVAR{1}	0.402242	0.111551	3.60589	0.000543
DDEPVAR{2}	-0.189512	0.118397	-1.60065	0.113447
DDEPVAR{3}	0.179931	0.111747	1.61016	0.111352

TOTAL CASES 2

Estimation by Least Squares	
Dependent Variable	**First Differenced Total Cases 1940-1995**
Usable Observations	51
Degrees of Freedom	43
Centered R^2	0.38
Adjusted R^2	0.28
Uncentered R^2	0.46
$T*R^2$	23.64
Mean of Dep. Variable	4515.86
St. Error of Dep. Variable	11446.97
St. Error of Estimate	9732.15
Sum of Squared Errors	4072731981
F-Statistic	3.74
Significance of F	0.003
D-W	1.93

Variable	Coefficient	St. Error	*t*-stat	p-value
Constant	2898.3719	1730.8740	1.6745	0.1013
DTOTAL{1}	0.4731	0.1482	3.1932	0.0026
DTOTAL{2}	0.0470	0.1610	0.2918	0.7719
DTOTAL{3}	-0.2760	0.1523	-1.8125	0.0769
DTOTAL{4}	0.4506	0.1485	3.0352	0.0041
DTOTAL{5}	-0.2969	0.1619	-1.8342	0.0736
DTOTAL{6}	-0.2948	0.1714	-1.7198	0.0927
DTOTAL{7}	0.2706	0.1567	1.7264	0.0914

TOTAL CASES 3

Estimation by Least Squares	
Dependent Variable	**First Differenced Total Cases 1940-1995**
Usable Observations	50
Degrees of Freedom	44
Centered R^2	0.38
Adjusted R^2	0.24
Uncentered R^2	0.16
T^*R^2	0.33
Mean of Dep. Variable	16.454
St. Error of Dep. Variable	3820.46
St. Error of Estimate	10801.06
Sum of Squared Errors	9914.37
F-Statistic	4324970637
Significance of F	0.027
D-W	1.88

Variable	Coefficient	St. Error	*t*-stat	p-value
Constant	2559.7151	1650.5514	1.5508	0.1281
DTOTAL{1}	0.4531	0.1395	3.2473	0.0022
DTOTAL{2}	-0.0268	0.1483	-0.1804	0.8576
DTOTAL{3}	-0.1369	0.1533	-0.8929	0.3767
DTOTAL{4}	0.3148	0.1750	1.7994	0.0788
DTOTAL{5}	-0.2840	0.1641	-1.7304	0.0906

Table A.4.2. In-sample Estimates: Full Sample 1904–1998

Civil Cases

Year	Civil	Civil Forecasts	RMSE	MAD
1904	14,888	NA	NA	NA
1905	16,002	NA	NA	NA
1906	15,986	NA	NA	NA
1907	18,434	NA	NA	NA
1908	14,905	NA	NA	NA
1909	13,127	NA	NA	NA
1910	13,788	14,171	146,409	383
1911	14,001	16,930	8,577,362	2,929
1912	14,993	13,784	1,461,042	1,209
1913	14,935	17,644	7,337,617	2,709
1914	16,288	17,285	994,832	997
1915	15,268	18,508	10,497,929	3,240
1916	17,352	16,622	533,277	730
1917	17,551	19,841	5,243,021	2,290
1918	16,756	19,573	7,935,925	2,817
1919	18,800	17,318	2,197,007	1,482
1920	22,109	22,605	245,697	496
1921	32,175	24,492	59,033,782	7,683
1922	31,745	37,699	35,453,760	5,954
1923	30,716	32,816	4,411,599	2,100
1924	34,211	32,470	3,029,590	1,741
1925	38,035	40,080	4,183,994	2,045
1926	38,721	37,745	953,378	976
1927	40,856	40,003	727,814	853
1928	44,445	44,960	264,933	515
1929	45,287	47,728	5,960,009	2,441
1930	48,325	45,935	5,709,935	2,390
1931	49,332	51,821	6,196,071	2,489
1932	60,515	51,656	78,474,787	8,859
1933	52,453	66,360	193,403,183	13,907
1934	35,959	49,728	189,595,516	13,769
1935	36,082	30,612	29,921,325	5,470
1936	39,391	43,697	18,540,692	4,306
1937	32,899	36,473	12,773,482	3,574
1938	33,591	28,113	30,010,803	5,478

Year	Civil	Civil Forecasts	RMSE	MAD
1939	33,810	41,716	62,508,777	7,906
1940	34,734	36,676	3,769,731	1,942
1941	38,477	33,610	23,689,764	4,867
1942	38,140	44,055	34,982,970	5,915
1943	36,789	39,056	5,139,033	2,267
1944	30,896	38,178	53,031,587	7,282
1945	53,236	31,117	489,238,037	22,119
1946	58,454	64,702	39,031,860	6,248
1947	49,606	59,158	91,244,270	9,552
1948	37,420	44,866	55,440,036	7,446
1949	44,037	44,241	41,752	204
1950	45,085	45,324	57,353	239
1951	41,938	41,808	16,849	130
1952	48,442	40,666	60,463,192	7,776
1953	53,469	59,636	38,028,580	6,167
1954	49,058	54,905	34,186,729	5,847
1955	49,056	46,647	5,803,610	2,409
1956	52,174	54,570	5,740,208	2,396
1957	54,143	55,037	799,895	894
1958	59,308	53,271	36,448,686	6,037
1959	49,586	64,486	222,015,006	14,900
1960	51,063	47,222	14,754,059	3,841
1961	51,225	54,483	10,612,058	3,258
1962	54,615	54,030	341,845	585
1963	57,028	52,939	16,722,498	4,089
1964	61,093	62,935	3,392,472	1,842
1965	62,670	63,922	1,567,948	1,252
1966	66,144	65,652	242,089	492
1967	66,197	68,984	7,769,478	2,787
1968	66,740	68,097	1,841,028	1,357
1969	72,504	67,962	20,633,079	4,542
1970	82,665	77,484	26,846,000	5,181
1971	89,318	87,167	4,625,559	2,151
1972	92,385	92,817	186,676	432
1973	96,056	96,333	76,906	277
1974	101,345	100,663	465,134	682
1975	115,098	104,058	121,875,020	11,040
1976	128,361	121,320	49,575,270	7,041

Year	Civil	Civil Forecasts	RMSE	MAD
1977	128,899	134,506	31,433,581	5,607
1978	137,707	129,619	65,419,234	8,088
1979	153,552	146,341	51,995,043	7,211
1980	167,871	161,515	40,397,073	6,356
1981	179,803	169,934	97,402,501	9,869
1982	205,525	187,741	316,273,107	17,784
1983	241,159	219,907	451,654,151	21,252
1984	260,785	255,450	28,457,698	5,335
1985	273,056	265,985	50,001,627	7,071
1986	254,249	282,501	798,181,922	28,252
1987	238,394	249,694	127,683,613	11,300
1988	239,010	230,976	64,544,147	8,034
1989	232,921	241,376	71,494,292	8,455
1990	217,421	221,642	17,819,923	4,221
1991	207,094	213,398	39,736,290	6,304
1992	230,212	211,461	351,592,740	18,751
1993	229,440	241,746	151,434,995	12,306
1994	236,149	224,306	140,249,033	11,843
1995	238,764	242,241	12,092,831	3,477
1996	269,132	251,872	297,916,263	17,260
1997	272,027	276,833	23,101,242	4,806
1998	256,787	273,393	275,765,429	16,606
			Forecast MAD	5,814
			Forecast MSE	8,053

Notes: NA indicates that the values for those years were part of the conditioning set in the estimation process.

Criminal Cases

Year	Criminal	Criminal Forecasts	RMSE	MAD
1904	18,488	NA	NA	NA
1905	18,900	NA	NA	NA
1906	17,435	NA	NA	NA
1907	18,332	NA	NA	NA
1908	13,345	NA	NA	NA
1909	14,505	NA	NA	NA
1910	14,864	NA	NA	NA
1911	15,057	NA	NA	NA
1912	15,935	16,239	92,399	304
1913	16,753	15,704	1,100,799	1,049
1914	18,399	18,642	59,073	243
1915	19,868	18,035	3,361,339	1,833
1916	20,243	20,606	131,901	363
1917	19,628	20,813	1,403,473	1,185
1918	35,096	19,883	231,420,158	15,213
1919	47,443	39,174	68,379,427	8,269
1920	55,587	48,294	53,180,606	7,293
1921	54,487	58,197	13,767,745	3,710
1922	60,722	54,852	34,453,572	5,870
1923	71,077	64,717	40,454,432	6,360
1924	70,168	69,762	164,985	406
1925	76,136	70,322	33,807,915	5,814
1926	68,582	79,475	118,657,195	10,893
1927	64,614	68,211	12,936,601	3,597
1928	83,372	65,074	334,800,940	18,298
1929	86,348	86,043	93,241	305
1930	87,305	87,054	63,120	251
1931	83,747	88,048	18,501,609	4,301
1932	92,174	85,615	43,024,444	6,559
1933	82,675	95,899	174,871,910	13,224
1934	34,152	74,429	1,622,251,975	40,277
1935	35,365	28,903	41,758,543	6,462
1936	35,920	41,261	28,525,503	5,341
1937	35,475	31,714	14,146,547	3,761
1938	34,202	34,024	31,713	178
1939	34,808	34,707	10,102	101
1940	33,401	45,071	136,197,321	11,670

Year	Criminal	Criminal Forecasts	RMSE	MAD
1941	31,823	24,247	57,393,796	7,576
1942	33,294	32,246	1,097,802	1,048
1943	36,588	34,161	5,892,688	2,427
1944	39,621	37,628	3,970,205	1,993
1945	39,429	40,035	366,957	606
1946	33,203	40,097	47,531,259	6,894
1947	34,563	32,756	3,265,857	1,807
1948	33,300	35,627	5,414,412	2,327
1949	35,686	32,133	12,622,086	3,553
1950	37,720	37,049	449,895	671
1951	39,830	38,204	2,642,429	1,626
1952	39,022	42,296	10,717,512	3,274
1953	38,504	37,630	763,029	874
1954	43,196	39,836	11,291,136	3,360
1955	37,123	43,929	46,327,054	6,806
1956	30,653	35,582	24,293,154	4,929
1957	30,078	30,895	667,935	817
1958	30,737	30,833	9,129	96
1959	30,707	30,999	85,491	292
1960	29,828	29,521	94,170	307
1961	30,268	31,944	2,809,829	1,676
1962	31,017	31,236	48,032	219
1963	31,746	30,431	1,730,225	1,315
1964	31,733	32,043	95,999	310
1965	33,334	32,190	1,309,050	1,144
1966	31,494	34,411	8,509,637	2,917
1967	32,207	31,038	1,367,230	1,169
1968	32,571	33,201	396,484	630
1969	35,413	32,639	7,696,561	2,774
1970	39,959	36,811	9,909,595	3,148
1971	43,157	40,528	6,910,506	2,629
1972	49,054	44,798	18,115,218	4,256
1973	42,434	50,405	63,531,392	7,971
1974	39,754	41,077	1,751,547	1,323
1975	43,282	40,886	5,740,214	2,396
1976	41,020	43,489	6,096,629	2,469
1977	41,464	40,771	480,423	693
1978	35,983	41,573	31,249,355	5,590

Year	Criminal	Criminal Forecasts	RMSE	MAD
1979	32,688	37,100	19,465,339	4,412
1980	28,921	32,907	15,887,393	3,986
1981	31,287	26,729	20,777,577	4,558
1982	32,682	33,664	963,562	982
1983	35,872	31,778	16,762,670	4,094
1984	36,845	38,352	2,271,817	1,507
1985	39,500	36,659	8,071,083	2,841
1986	41,490	41,204	81,774	286
1987	43,292	40,890	5,769,203	2,402
1988	44,585	44,490	9,093	95
1989	45,995	44,807	1,410,961	1,188
1990	48,904	47,265	2,685,247	1,639
1991	45,735	49,597	14,915,195	3,862
1992	48,366	45,325	9,244,683	3,041
1993	46,786	50,183	11,539,054	3,397
1994	45,473	46,097	389,453	624
1995	44,924	46,359	2,058,065	1,435
1996	47,889	44,458	11,769,496	3,431
1997	50,363	50,362	2	1
1998	57,691	49,595	65,538,995	8,096

Forecast MAD	3,781
Forecast MSE	6,468

Total Cases

Year	Total	Total Forecasts	RMSE	MAD
1904	33,376	NA	NA	NA
1905	34,902	NA	NA	NA
1906	33,421	NA	NA	NA
1907	36,766	NA	NA	NA
1908	28,250	NA	NA	NA
1909	27,632	NA	NA	NA
1910	28,652	30,521	3,494,345	1,869
1911	29,058	31,649	6,715,317	2,591
1912	30,928	29,357	2,468,856	1,571
1913	31,688	35,298	13,032,400	3,610
1914	34,687	34,374	97,680	313
1915	35,136	37,933	7,824,937	2,797
1916	37,595	37,576	368	19
1917	37,179	40,693	12,351,604	3,514
1918	51,852	39,513	152,250,118	12,339
1919	66,243	58,682	57,175,093	7,561
1920	77,696	71,768	35,136,467	5,928
1921	86,662	82,513	17,215,260	4,149
1922	92,467	94,065	2,552,825	1,598
1923	101,793	96,254	30,679,992	5,539
1924	104,379	106,469	4,368,782	2,090
1925	114,171	106,352	61,129,422	7,819
1926	107,303	119,405	146,469,491	12,102
1927	105,470	107,021	2,406,183	1,551
1928	127,817	107,533	411,443,826	20,284
1929	131,635	138,284	44,207,988	6,649
1930	135,630	129,541	37,070,246	6,089
1931	133,079	141,246	66,700,255	8,167
1932	152,689	138,498	201,397,509	14,191
1933	135,128	158,633	552,479,710	23,505
1934	70,111	129,341	3,508,174,088	59,230
1935	71,447	53,940	306,482,655	17,507
1936	75,311	84,552	85,395,512	9,241
1937	68,374	67,868	255,923	506
1938	67,793	60,249	56,914,503	7,544
1939	68,618	82,703	198,374,283	14,085
1940	68,135	71,250	9,702,925	3,115

Year	Total	Total Forecasts	RMSE	MAD
1941	70,300	68,350	3,802,756	1,950
1942	71,434	74,586	9,937,517	3,152
1943	73,377	74,105	529,335	728
1944	70,517	76,145	31,669,617	5,628
1945	92,665	72,254	416,615,830	20,411
1946	91,657	102,410	115,618,019	10,753
1947	84,169	90,977	46,348,651	6,808
1948	70,720	84,946	202,381,128	14,226
1949	79,723	73,832	34,706,244	5,891
1950	82,805	81,999	649,111	806
1951	81,768	83,063	1,677,252	1,295
1952	87,464	83,089	19,137,373	4,375
1953	91,973	95,865	15,147,107	3,892
1954	92,254	93,963	2,921,752	1,709
1955	86,179	93,838	58,667,551	7,659
1956	82,827	88,009	26,854,349	5,182
1957	84,221	84,616	156,049	395
1958	90,045	86,264	14,297,149	3,781
1959	80,293	92,798	156,374,642	12,505
1960	80,891	79,425	2,147,996	1,466
1961	81,493	85,830	18,811,527	4,337
1962	85,632	84,040	2,535,864	1,592
1963	88,774	86,617	4,652,115	2,157
1964	92,826	93,499	453,310	673
1965	96,004	96,389	147,842	385
1966	97,638	99,705	4,273,475	2,067
1967	98,404	100,222	3,304,658	1,818
1968	99,311	101,163	3,428,645	1,852
1969	107,917	101,805	37,355,708	6,112
1970	122,624	112,696	98,573,022	9,928
1971	132,475	128,573	15,226,777	3,902
1972	141,439	136,844	21,115,583	4,595
1973	138,490	147,798	86,638,926	9,308
1974	141,099	140,479	383,807	620
1975	158,380	144,342	197,069,535	14,038
1976	169,381	165,546	14,707,235	3,835
1977	170,363	171,308	892,944	945
1978	173,690	173,868	31,598	178

Year	Total	Total Forecasts	RMSE	MAD
1979	186,240	180,212	36,337,159	6,028
1980	196,792	191,113	32,252,524	5,679
1981	211,090	199,495	134,433,515	11,595
1982	238,207	218,023	407,403,766	20,184
1983	277,031	249,809	741,034,601	27,222
1984	297,630	289,155	71,824,216	8,475
1985	312,556	304,377	66,891,135	8,179
1986	295,739	321,973	688,239,163	26,234
1987	281,686	294,129	154,826,341	12,443
1988	283,595	279,233	19,025,443	4,362
1989	278,916	285,989	50,025,069	7,073
1990	266,325	273,119	46,164,770	6,794
1991	252,829	265,918	171,326,158	13,089
1992	278,578	254,833	563,830,292	23,745
1993	276,226	288,812	158,411,558	12,586
1994	281,622	272,521	82,825,465	9,101
1995	283,688	287,695	16,056,865	4,007
1996	317,021	292,617	595,543,700	24,404
1997	322,390	325,270	8,295,353	2,880
1998	314,478	323,952	89,748,737	9,474

Forecast MAD	7,771
Forecast MSE	11,533

Table A.4.3. Autoregressive Models Chosen for the Civil, Criminal and Total Cases Time Series over the 1904–1990 Period

Series	Sample Period 1904–1990
Civil Cases	ARI(3, 1)
Criminal Cases	ARI(7, 1)
Total Cases	ARI(3, 1)

Table A.4.4. Estimation over the 1904–1990 Period and Forecasting 1991–1998

Year	**Civil**	**Civil Forecasts**	**MAD**	**Criminal**	**Criminal Forecasts**	**MAD**	**Total**	**Total Forecasts**	**MAD**
1991	207,094	210,945	3,851	45,735	49,549	3,814	252,829	264,096	11,267
1992	230,212	204,432	25,780	48,366	45,241	3,125	278,578	250,550	28,028
1993	229,440	243,878	14,438	46,786	50,215	3,429	276,226	290,833	14,607
1994	236,149	222,816	13,333	45,473	45,962	489	281,622	269,577	12,045
1995	238,764	245,584	6,820	44,924	46,381	1,457	283,688	290,477	6,789
1996	269,132	239,598	29,534	47,889	44,316	3,573	317,021	284,679	32,342
1997	272,027	288,317	16,290	50,363	50,405	42	322,390	332,614	10,224
1998	256,787	268,363	11,576	57,691	49,430	8,261	314,478	320,210	5,732
MAD			**15,203**			**3,024**			**15,129**

Table A.4.5. Autoregressive Models Chosen for the Civil, Criminal and Total Cases over the 1940–1998 Period

Series	Sample Period 1940–1998
Civil Cases	ARI(5, 1)
Criminal Cases	ARI(1, 1)
Total Cases	ARI(7, 1)

Table A.4.6. Out-of-Sample Forecasts for the 1940–1998 Period

Year	**Civil**	**Criminal**	**Total**
1999	247,635	58,100	311,677
2000	258,240	58,509	323,577
2001	257,659	58,918	327,532
2002	251,396	59,326	319,207
2003	252,215	59,735	323,593
2004	263,562	60,144	337,062
2005	267,623	60,553	345,771
2006	268,015	60,962	343,020
2007	272,030	61,371	347,811
2008	280,268	61,779	358,735
2009	283,438	62,188	364,166
2010	284,511	62,597	362,216
2011	287,875	63,006	365,485
2012	293,271	63,415	375,008
2013	295,872	63,824	380,150
2014	297,718	64,232	380,612
2015	301,196	64,641	384,750
2016	305,696	65,050	393,573
2017	308,641	65,459	398,711
2018	311,318	65,868	400,072
2019	314,968	66,277	404,209
2020	319,020	66,686	411,714

Table A.4.7. Autoregressive Models Chosen for the Civil, Criminal and Total Cases over the 1940–1995 Period

Series	Sample Period 1940–1995
Civil Cases	ARI(5, 1)
Criminal Cases	ARI(1, 1)
Total Cases	ARI(5, 1)

Table A.4.8. In-Sample Forecasts for the 1940–1995 Period

Civil Cases

Year	Civil	Civil Forecast	MAD
1940	NA	34,734	NA
1941	NA	38,477	NA
1942	NA	38,140	NA
1943	NA	36,789	NA
1944	NA	30,896	NA
1945	NA	53,236	NA
1946	66,105	58,454	7,651
1947	61,253	49,606	11,647
1948	45,121	37,420	7,701
1949	41,630	44,037	2,407
1950	45,924	45,085	839
1951	43,637	41,938	1,699
1952	41,424	48,442	7,018
1953	59,286	53,469	5,817
1954	56,131	49,058	7,073
1955	47,257	49,056	1,799
1956	54,070	52,174	1,896
1957	55,533	54,143	1,390
1958	54,436	59,308	4,872
1959	65,062	49,586	15,476
1960	47,152	51,063	3,911
1961	54,301	51,225	3,076
1962	54,372	54,615	243
1963	54,374	57,028	2,654
1964	63,205	61,093	2,112
1965	64,653	62,670	1,983
1966	66,085	66,144	59
1967	69,532	66,197	3,335
1968	68,460	66,740	1,720
1969	68,365	72,504	4,139
1970	78,017	82,665	4,648
1971	88,523	89,318	795
1972	94,000	92,385	1,615
1973	96,709	96,056	653
1974	100,790	101,345	555

Year	Civil	Civil Forecast	MAD
1975	104,763	115,098	10,335
1976	122,680	128,361	5,681
1977	136,119	128,899	7,220
1978	130,317	137,707	7,390
1979	146,216	153,552	7,336
1980	162,744	167,871	5,127
1981	172,240	179,803	7,563
1982	188,696	205,525	16,829
1983	221,249	241,159	19,910
1984	258,465	260,785	2,320
1985	268,624	273,056	4,432
1986	282,639	254,249	28,390
1987	247,830	238,394	9,436
1988	229,136	239,010	9,874
1989	241,019	232,921	8,098
1990	223,781	217,421	6,360
1991	213,131	207,094	6,037
1992	209,966	230,212	20,246
1993	243,469	229,440	14,029
1994	227,019	236,149	9,130
1995	242,786	238,764	4,022

MAD			6,371

Criminal Cases

Year	Criminal	Criminal Forecast	MAD
1940	33,401	NA	NA
1941	31,823	NA	NA
1942	33,294	31,861	1,433
1943	36,588	33,678	2,910
1944	39,621	37,179	2,442
1945	39,429	40,182	753
1946	33,203	39,624	6,421
1947	34,563	32,714	1,849
1948	33,300	34,934	1,634
1949	35,686	33,374	2,312
1950	37,720	36,174	1,546
1951	39,830	38,168	1,662
1952	39,022	40,287	1,265
1953	38,504	39,148	644
1954	43,196	38,663	4,533
1955	37,123	43,945	6,822
1956	30,653	36,652	5,999
1957	30,078	30,137	59
1958	30,737	30,230	507
1959	30,707	31,029	322
1960	29,828	30,921	1,093
1961	30,268	29,946	322
1962	31,017	30,535	482
1963	31,746	31,319	427
1964	31,733	32,046	313
1965	33,334	31,949	1,385
1966	31,494	33,733	2,239
1967	32,207	31,503	704
1968	32,571	32,505	66
1969	35,413	32,830	2,583
1970	39,959	35,953	4,006
1971	43,157	40,692	2,465
1972	49,054	43,737	5,317
1973	42,434	49,940	7,506
1974	39,754	41,901	2,147
1975	43,282	39,667	3,615
1976	41,020	43,899	2,879

Year	Criminal	Criminal Forecast	MAD
1977	41,464	40,981	483
1978	35,983	41,732	5,749
1979	32,688	35,579	2,891
1980	28,921	32,532	3,611
1981	31,287	28,711	2,576
1982	32,682	31,773	909
1983	35,872	33,057	2,815
1984	36,845	36,451	394
1985	39,500	37,173	2,327
1986	41,490	40,018	1,472
1987	43,292	41,933	1,359
1988	44,585	43,714	871
1989	45,995	44,949	1,046
1990	48,904	46,372	2,532
1991	45,735	49,451	3,716
1992	48,366	45,593	2,773
1993	46,786	48,882	2,096
1994	45,473	46,824	1,351
1995	44,924	45,541	617

MAD			2,227

Total Cases

Year	Total	Total Forecast	MAD
1940	68,135	NA	NA
1941	70,300	NA	NA
1942	71,434	NA	NA
1943	73,377	NA	NA
1944	70,517	NA	NA
1945	92,665	NA	NA
1946	91,657	104,813	13,156
1947	84,169	93,849	9,680
1948	70,720	78,878	8,158
1949	79,723	75,310	4,413
1950	82,805	81,139	1,666
1951	81,768	86,291	4,523
1952	87,464	80,435	7,029
1953	91,973	98,865	6,892
1954	92,254	94,979	2,725
1955	86,179	92,839	6,660
1956	82,827	87,449	4,622
1957	84,221	83,794	427
1958	90,045	87,142	2,903
1959	80,293	93,673	13,380
1960	80,891	78,758	2,133
1961	81,493	84,576	3,083
1962	85,632	87,082	1,450
1963	88,774	85,245	3,529
1964	92,826	95,522	2,696
1965	96,004	96,591	587
1966	97,638	100,597	2,959
1967	98,404	100,112	1,708
1968	99,311	101,215	1,904
1969	107,917	101,887	6,030
1970	122,624	113,859	8,765
1971	132,475	131,270	1,205
1972	141,439	137,994	3,445
1973	138,490	148,235	9,745
1974	141,099	140,311	788
1975	158,380	142,617	15,763
1976	169,381	169,128	253

Year	Total	Total Forecast	MAD
1977	170,363	172,631	2,268
1978	173,690	172,366	1,324
1979	186,240	180,924	5,316
1980	196,792	192,818	3,974
1981	211,090	200,526	10,564
1982	238,207	218,896	19,311
1983	277,031	254,232	22,799
1984	297,630	294,256	3,374
1985	312,556	306,276	6,280
1986	295,739	320,488	24,749
1987	281,686	291,980	10,294
1988	283,595	271,742	11,853
1989	278,916	288,547	9,631
1990	266,325	271,695	5,370
1991	252,829	263,396	10,567
1992	278,578	254,844	23,734
1993	276,226	292,874	16,648
1994	281,622	276,244	5,378
1995	283,688	282,491	1,197

MAD			6,938

Table A.4.9. Estimation over the 1940–1995 Period and Forecasting 1996–2020

Civil Cases

Year	Civil	Civil Forecast	MAD
1996	269,132	250,782	18,350
1997	272,027	252,026	20,001
1998	256,787	255,764	1,023
1999	260,271	258,274	1,997
2000	259,517	263,855	4,338
2001	250,907	265,531	14,624
2002	274,841	268,661	6,180
2003	NA	271,763	NA
2004	NA	275,983	NA
2005	NA	278,837	NA
2006	NA	282,382	NA
2007	NA	285,956	NA
2008	NA	289,824	NA
2009	NA	293,147	NA
2010	NA	296,734	NA
2011	NA	300,308	NA
2012	NA	303,938	NA
2013	NA	307,363	NA
2014	NA	310,902	NA
2015	NA	314,438	NA
2016	NA	317,985	NA
2017	NA	321,466	NA
2018	NA	325,000	NA
2019	NA	328,533	NA
2020	NA	332,067	NA

MAD			9,502

Criminal Cases

Year	Criminal	Criminal Forecast	MAD
1996	47,889	45,141	2,748
1997	50,363	45,359	5,004
1998	57,691	45,576	12,115
1999	59,251	45,793	13,458
2000	62,152	46,010	16,142
2001	62,134	46,228	15,906
2002	66,452	46,445	20,007
2003	NA	46,662	NA
2004	NA	46,879	NA
2005	NA	47,097	NA
2006	NA	47,314	NA
2007	NA	47,531	NA
2008	NA	47,748	NA
2009	NA	47,966	NA
2010	NA	48,183	NA
2011	NA	48,400	NA
2012	NA	48,617	NA
2013	NA	48,835	NA
2014	NA	49,052	NA
2015	NA	49,269	NA
2016	NA	49,486	NA
2017	NA	49,704	NA
2018	NA	49,921	NA
2019	NA	50,138	NA
2020	NA	50,356	NA

MAD	12,197

Total Cases

Year	Total	Total Forecast	MAD
1996	317,021	299,301	17,720
1997	322,390	300,088	22,302
1998	314,478	304,670	9,808
1999	319,522	306,265	13,257
2000	321,669	313,646	8,023
2001	313,041	314,692	1,651
2002	341,293	318,530	22,763
2003	NA	320,990	NA
2004	NA	326,290	NA
2005	NA	328,893	NA
2006	NA	333,064	NA
2007	NA	336,403	NA
2008	NA	340,977	NA
2009	NA	344,263	NA
2010	NA	348,306	NA
2011	NA	351,850	NA
2012	NA	355,949	NA
2013	NA	359,453	NA
2014	NA	363,345	NA
2015	NA	366,981	NA
2016	NA	370,888	NA
2017	NA	374,527	NA
2018	NA	378,363	NA
2019	NA	382,068	NA
2020	NA	385,903	NA

MAD	13,646

REFERENCES

Administrative Office of the United States District Courts (various years). *Annual Report of the Director*. Washington, DC: Administrative Office of the United States District Courts.

Administrative Office of the United States District Courts (various years). *Federal Court Management Statistics*. Washington, DC: Administrative Office of the United States District Courts.

Administrative Office of the United States District Courts (1995). *Judicial Business of the United States Courts: Report of the Director*. Washington, DC: Administrative Office of the United States District Courts.

Benson, Bruce L. (1990). *The Enterprise of Law: Justice without the State*. San Francisco, CA: Pacific Research Institute for Public Policy.

Boyum, Keith O. and Sam Krislov (Eds.) (1990). *Forecasting the Impact of Legislation on Courts*. Washington, D.C: National Academy Press.

Brock, W., W.D. Dechert, J. Scheinkman and B. LeBaron (1996). A test for independence based on the correlation dimension. *Econometric Reviews* 15(3): 197–235.

Clark, Charles. (1994). Mules and wagons – A plea for jurisdictional reform. *Mississippi College Law Review* 14: 263–269.

Clemente, J., A. Montanes, and M. Reyes (1998). Testing for a unit root in variables with a double change in the mean. *Economics Letters* 59: 175–82.

Cooter, Robert D. and Daniel L. Rubinfeld (1990). Trial courts: An economic perspective. *Law and Society Review* 24: 533–46.

Elliott, G., T.J. Rothenberg, and H.J. Stock (1996). Efficient tests for an autoregressive unit root. *Econometrica* 64: 813–36.

Figueiredo, John M. de and Emerson H. Tiller (1996). Congressional control of the courts: A theoretical and empirical analysis of expansion of the Federal Judiciary. *Journal of Law and Economics* 39: 435–62.

Geweke, J. and J. Porter-Hudak (1983). The estimation and application of long memory time series models. *Journal of Time Series Analysis* 4: 221–38.

Geyh, Charles G. (1995). Overcoming the competence/credibility paradox and the politics of interbranch relations. In A. Fletcher Magnum (Ed.), *Conference on Assessing the Effects of Legislation on the Workload of the Courts: Papers and Proceedings*, 79–94. Washington, DC: Federal Judicial Center.

Greene, William H. (2000). *Econometric Analysis*, 4th ed. Upper Saddle River, NJ: Prentice-Hall.

Hamilton, James D. (1994). *Time Series Analysis*. Princeton, NJ: Princeton University Press.

Hausman, J.A. (1978). Specification tests in econometrics. *Econometrica* 46(6): 1251–71.

Heydebrand, Wolf and Carroll Seron (1990). *Rationalizing Justice: The Political Economy of Federal District Courts.* Albany, NY: State University of New York Press.

Hobijn, B., P.-H. Franses and M. Ooms (1998). Generalizations of the KPSS test for stationarity. Econometric Institute Report 9802/A, Econometric Institute, Erasmus University, Rotterdam.

Holloway, William J. Jr. (1989). Certifying questions to state supreme courts. In Cynthia Harrison and Russell R. Wheeler (Eds.), *The Federal Appellate Judiciary in the 21st Century*, 93–96. Washington, DC: Federal Judicial Center.

Johansen, S. (1988). Statistical analysis of cointegrating vectors. *Journal of Economic Dynamics and Control* 12: 231–54.

Johansen, S. and K. Juselius (1990). Maximum likelihood estimation and inference on cointegration-with applications to the demand for money. *Oxford Bulletin of Economics and Statistics* 52: 169–210.

Judicial Conference of the United States (1995). *Long Range Plan for the Federal Courts*. Washington, DC: Judicial Conference of the United States.

Kanzler, L. (1999). Very fast and correctly sized estimation of the BDS statistic. Unpublished working paper, Oxford University.

Krislov, Samuel (1995). Caseloading in the balance. In A. Fletcher Mangum (Ed.), *Conference on Assessing the Effects of Legislation on the Workload of the Courts: Papers and Proceedings*, 33–46. Washington, DC: Federal Judicial Center.

Kwiatkowski, D., P.C. Phillips, P. Schmidt and Y. Shin (1992). Testing the null hypothesis of stationarity against the alternative of a unit root: How sure are we that economic time series have a unit root? *Journal of Econometrics* 54: 159–78.

Landes, William M. (1971). An economic analysis of the courts. *Journal of Law and Economics* 14: 61–109.

Nelson, Charles R. and Heejoon Kang (1984). Pitfalls in the use of time as an explanatory variable in regression. *Journal of Business and Economic Statistics* 2(1): 73–82.

Newey, W.K. and K.D. West (1994). Automatic lag selection in covariance matrix estimation.

Review of Economic Studies 61: 631–53.

Newman, Jon O. (1989). Discretionary access to the Federal Courts. In Cynthia Harrison and Russell R. Wheeler (Eds.), *The Federal Appellate Judiciary in the 21st Century*, 63–67. Washington, DC: Federal Judicial Center.

Ng, S. and P. Perron (1995). Unit root test in ARMA models with data-dependent methods for the selection of the truncation lag. *Journal of the American Statistical Association* 90: 268–281.

Ng, S. and P. Perron (2001). Lag length selection and the construction of unit root tests with good size and power. *Econometrica*, in press.

Osterwald-Lenum, M. (1992). A note with quantiles of the asymptotic distribution of the maximum likelihood cointegration rank test statistics. *Oxford Bulletin of Economics and Statistics* 54: 461–72.

Perron, P. and T. Vogelsang (1992). Nonstationarity and level shifts with an application to purchasing power parity. *Journal of Business and Economic Statistics* 10: 301–20.

Phillips, P.C.B. (1999). Unit root log periodogram regression. Unpublished working paper no. 1244, Cowles Foundation for Research in Economics, Yale University.

Posner, Richard A. (1986), *Economic Analysis of Law*, ed. Boston, MA: Little Brown.

Posner, Richard A. (1996), *The Federal Courts: Challenge and Reform.* Cambridge, MA: Harvard University Press.

Redish, Martin H. (1989). The role of the Federal Judiciary and the allocation of jurisdiction. In Cynthia Harrison and Russell R. Wheeler (Eds.), *The Federal Appellate Judiciary in the 21st Century*, 39–50. Washington, DC: Federal Judicial Center.

Reiss, Albert J. Jr. (1990). Longitudinal study of trial courts: A plea for development of explanatory variables. *Law and Society Review* 24: 545–52.

Sanders, Joseph. (1990). The interplay of micro and macro processes in the longitudinal study of courts: Beyond the Durkheimian tradition. *Law and Society Review* 24: 240–48.

SAS Institute, Inc. (1999). *SAS/ETS User's Guide*, Version 8. Cary, NC: SAS Institute, Inc.

U.S. Department of Commerce (various years). *Statistical Abstract of the United States*. Washington, DC: U.S. Government Printing Office.

Wright, J. H. (1995). Stochastic orders of magnitude associated with two-stage estimators of fractional ARIMA systems. *Journal of Time Series Analysis* 16: 119–25.