B. Courts as Complex Organizations

PLEA BARGAINING AND PROHIBITION IN THE FEDERAL COURTS, 1908–1934

JOHN F. PADGETT

This article documents and explains the emergence of implicit plea bargaining in the federal district courts during the Progressive and Prohibition periods. Three competing explanations for plea bargaining are tested statistically—the caseload, the substantive justice, and the evidentiary quality arguments. All three receive qualified support. The historical operation of each of these causal paths, however, was shaped by the preoccupation of more elite federal judges with their own professional self-image in the face of Prohibition. Implicit plea bargaining in the federal courts emerged reflexively as an unintended consequence of the failed Progressive assault on the "corrupt" explicit plea bargaining practices of lower state and county courts.

> [Historical studies] agree that plea bargaining was probably nonexistent before 1800, began to appear during the early or mid-nineteenth century, and became institutionalized as a standard feature of American urban criminal courts in the last third of the nineteenth century. During the twentieth century there may have been periods of renewed growth of plea bargaining: in the 1920s, especially in the federal courts faced with large numbers of prohibition cases, and in the 1960s, perhaps related to the growth of street crime. (Haller, 1979: 273)

I. INTRODUCTION

Due in part to the Crime Commission reports of the 1920s (Alschuler, 1979: 26ff.), many researchers and practitioners have long had the impression that plea bargaining first emerged in force from deep within the bowels of urban criminal courts. Teeming with caseload, tainted by corruption, and staffed largely by ethnics with little professional training, these courts were considered to be

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ideal breeding grounds for "bargaining with crime." Today, many forget that these excellent statistical reports were themselves political documents—rallying cries for Progressives in their early twentieth-century assault on political machines. Based on the analyses in this article, however, my conclusion will be this: Implicit plea bargaining emerged in the federal courts as a reflexive consequence of federal judges' own professionalizing attempt to abolish explicit plea bargaining in the lower municipal courts.

In this article, I investigate the anatomy and the causes of the rise of plea bargaining in the federal district courts in the first third of this century. Using data from U.S. Attorney General annual reports and from the American Law Institute's 1934 study of thirteen district courts, I test alternative hypotheses about the historical emergence of plea bargaining. I also determine the precise form of plea bargaining that emerged.

Three families of hypotheses about the rise of plea bargaining exist in the literature (cf. Padgett, 1985):

1. The administrative capacity argument (e.g., Blumberg, 1967; Alschuler, 1976) is that plea bargaining arises in response to the fact that incoming caseload often overwhelms the trial "carrying capacity" of criminal courts. Raw caseload itself can be affected by not only crime but also by changing arrest and dismissal practices, and expansion in the criminal law. Equally important to the argument, the administrative carrying capacity of courts can be affected not only by the raw numbers of court personnel but also by the average length of trials. Langbein (1979), for example, has argued that plea bargaining emerged as a response to the increasing procedural complexity and prolixity of American jury trials.

Prohibition in the federal courts presents an almost ideal quasi-experimental setting for evaluation of this caseload argument. As can be seen in Figure 1, the liquor cases of Prohibition represented a massive, very discrete, and moderately long-lasting shock to the system, unparalleled in history before or since. Cross-sectionally, there was wide variation in the popularity and, hence, in the enforcement of Prohibition. Causal effects can be distinguished from compositional effects by examining the impact of liquor caseload on the guilty plea processing of nonliquor cases. Hard-to-find data on the distribution of trial lengths are reported in the ALI study.

2. The substantive justice argument (e.g., Feeley, 1979) begins with the micro observation that plea bargaining is an effort to substitute flexible sentencing standards, which remain sensitive to the idiosyncratic background of the crime or the criminal, for the rigid and often harsh provisions of the criminal code. The content of these substantive sentencing standards, however, varies according to "local legal cultures" (Church, 1982). In particular, judges appointed through bar association sponsorship may be harsher and

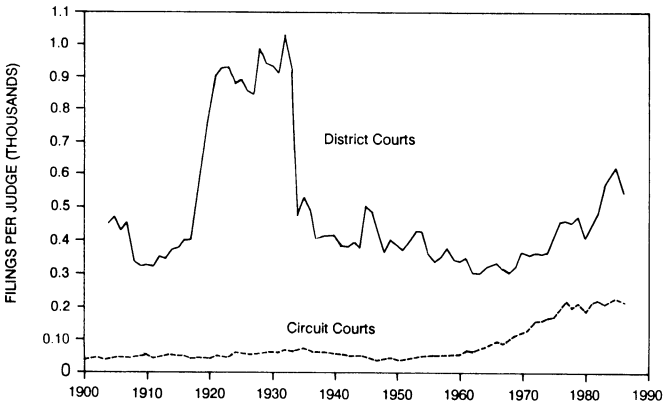

Figure 1. Federal caseload pressure, criminal and civil cases, standardized, 1900–1985

more routinized in their sentencing than judges elected through sponsorship by political machines (Levin, 1971).

Historically, the local legal culture of federal courts differed sharply from contemporaneous state and county criminal courts precisely on this dimension of professionalism. Haller (1970) argues that plea bargaining emerged in the late nineteenth century originally because it was embedded in urban political machines. In contrast, the federal courts, along with state appellate courts, elite law schools, and bar associations, were the bastions from which nineteenth- and early twentieth-century professionalizing assaults on the lower, more democratic, and often more ethnic reaches of the legal profession were carried out (Auerbach, 1976; Friedman, 1973; Horwitz, 1977). All these elite institutions vigorously and uniformly opposed the practice of plea bargaining, without success (Alschuler, 1979).

3. The strength of state's case argument (e.g., Heumann, 1978) is that plea bargaining is a method whereby the prosecutor can secure at least some punishment for factually guilty defendants, whom the prosecutor is not sure he can convict. In the aggregate, average strength of case is a function not only of investigative excellence by the police, but also of the thoroughness of pretrial screening and dismissal by the magistrate or the prosecutor. Thus the nineteenth- and early twentieth-century rise of professional police and prosecutors in America may explain the decline of trials, simply as a compositional effect (Mather, 1979b).[1]

The plan of this article is as follows: After a brief discussion of the federal court data, I will assess these three causal arguments about the historically changing volume of plea bargaining empirically, in bivariate fashion, more or less in the order just presented. Then I will analyze determinants of sentencing and sentence dis-

[1] The sign of this predicted relationship depends on the form of plea bargaining in place, however (see below, in sec. VII).

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counting, and will present time-series regressions in order to evaluate a subset of the bivariate findings in multivariate context. Finally, to frame these statistical results in historical context, a closing section will discuss crucial doctrinal, institutional and political changes in the court system during this Progressive era.

The organizationally interesting complication in these various tests is that plea bargaining is not a homogeneous phenomenon. "Plea bargaining consists of the exchange of official concessions for a defendant's act of self-conviction" (Alschuler, 1979: 3). In the United States, there are four different systems of bargaining within which this exchange takes place (Alschuler, 1976: Miller *et al.*, 1978):

a) In implicit plea bargaining, the defendant simply pleads guilty to the original charge in the expectation of receiving a more lenient sentence thereby. The ratio of the expected guilty plea sentence to the expected trial conviction sentence is called the sentence discount.
b) In charge reduction plea bargaining, the prosecutor downgrades or eliminates charges in exchange for a guilty plea to the reduced charge(s).
c) In sentence recommendation plea bargaining, the prosecutor in exchange for a guilty plea recommends a particular disposition to the judge, who then (usually) imposes the sentence recommended.
d) In judicial plea bargaining, the judge, after consultation with prosecutor and defense attorney in conference, offers the defendant a specific guilty plea sentence.

Modes (b), (c), and (d) are explicit bargaining, in the sense that overt promises are made in conversations between the defense and state officials. Implicit plea bargaining, in contrast, operates through tacit signals about the "going rate," communicated to knowledgeable insiders by a judge's past sentencing behavior.

In the special case of the federal courts, we have not known which system of plea bargaining predominated. Indeed, the historical existence of plea bargaining was and is a matter of some controversy. Whether "plea bargaining" in federal courts is recognized as existing depends on how it is defined. Since the 1975 revision of the Federal Rules of Criminal Procedure,[2] prosecutorial sentence bargaining has become common, especially among "proactive" U.S. Attorneys (Hagan and Bernstein, 1979). Before that, however, in 1967–68 (Alschuler (1976) found both that most federal judges did not overtly bargain with criminal defendants[3] and that prosecutorial bargaining was restrained.[4] Instead,

[2] These revisions, providing guidelines for explicit plea bargaining, were provoked by the Supreme Court's "legalization" of plea bargaining in 1970. Before this time, appellate courts had held plea bargaining to be illegitimate (Alschuler, 1979), even though it was practiced widely.

[3] "The situation was summarized by an Assistant United States Attorney

implicit plea bargaining was widespread, in which defendants "threw themselves on the mercy of the court" in the expectation (but not the explicit promise) of a lighter sentence thereby. Thus, plea bargaining was present in the federal courts if the definition of plea bargaining includes the reliable exchange of sentence benefit for plea. But it was largely absent (in 1967–68) if one insists on an explicit pre-plea promise as part of the definition.

Before the 1960s, no direct case studies of plea bargaining in federal district courts exist. The only general assessment I could find about earlier federal plea practices was the following assertion by Franklin Roosevelt's Attorney General:

> In the field of criminal prosecutions, except where Congress has provided otherwise in tax prosecutions, quite another rule prevails. "Enter into no compromise with offenders against the United States," directed Attorney General Devens in 1880. "I want no trafficking or guarantees, but a judicial investigation." Sometimes, however, the courts themselves, with or without the recommendation of counsel, grant a type of compromise by imposing slight sentences in return for pleas of guilty or for giving evidence against accomplices. (Cummings and McFarland, 1937: 509)

This statement of policy clearly implies the existence of implicit plea bargaining in the 1930s. Furthermore, explicit promises clearly were considered illegitimate. However, the phrase "with the recommendation of counsel" leaves ambiguous the status of prosecutorial bargaining (cf. note 4).

Elsewhere (Padgett, 1985), I have developed stochastic models of each of the four types of plea bargaining. These models operationalized and synthesized existing causal arguments in the field, in order to derive "sentence discount schedules" (that is, predicted equilibrium relationships between sentence discounts and aggregate guilty plea rates) for each of the four types of plea bargaining. An important conclusion of that analysis was that, for any given guilty plea rate (with one exception), implicit plea bargaining entails much heavier sentence discounting than does any of the explicit systems (see Fig. 4 below).

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in Chicago who said, 'There may be one or two judges in the Northern District of Illinois who make sentence promises in advance of trial, but there may also be one or two judges who take bribes. Neither activity is really considered a suitable part of the judicial process' " (Alschuler, 1976: 1078).

[4] Alschuler (1976: 1078–79) reports:

> Although federal prosecutors were usually willing to discuss plea agreements with defense attorneys, they commonly made available only insubstantial concessions. In some federal courts, pretrial bargaining focused on the prosecutors' sentence recommendations; but most federal prosecutors did not make sentence recommendations, and others made recommendations that were not subject to negotiation. . . . In most offense areas, moreover, the Federal Criminal Code was not well adapted to the patterns of charge reduction that characterized bargaining in many state courts.

In the absence of qualitative case studies to provide the information directly, derived sentence discount schedules can be used to estimate statistically the dominant mode of plea bargaining in place. In particular, I am interested in whether federal courts during Prohibition coped with their crushing caseload through an intensification of implicit plea bargaining techniques or whether, contrary to Attorney General Cummings's impression, they shifted temporarily to one of the other more efficient, but procedurally less acceptable, forms of explicit plea bargaining. Conversely, of course, federal data on guilty plea rates during Prohibition can be used to test my models.

II. DATA

There are two primary sources of data for this study. The primary time-series data base was the annual reports of the U.S. Attorney General from 1908 to 1934 (U.S. Department of Justice, Office of the Attorney General, 1908–34). These reports list, by district by offense, basic mortality information: cases commenced, terminated, and pending; and of those terminated, cases dismissed by *nolle prosequi* or quashed, cases convicted by guilty plea and by trial, and cases acquitted by trial. I coded this information for the nation and for the thirteen district courts covered in the American Law Institute Study. Guilty pleas were first tabulated in 1908; the last year in which data were reported within the same accounting scheme was 1934.[5]

The second source of data for this study was a 1934 American Law Institute (ALI) cross-sectional study of thirteen federal district courts. This excellent study was initiated, following earlier crime commission models, by a star-studded cast of nationally prominent lawyers and scholars (including Robert M. Hutchins, Charles E. Clark, Owen J. Roberts, and William O. Douglas) and was directed and written by Thurman Arnold and Charles Samenow. The study coded for the thirteen districts, according to a very detailed coding scheme, all civil cases, all nonliquor criminal cases, and most liquor criminal cases terminated during the period fiscal

[5] Before 1934 the annual reports were compilations of self-reports by the various district attorneys. After 1934, statistics were compiled centrally. While this change in data collection procedure probably improved the accuracy of national statistics, it had the unfortunate consequence for my purposes of decreasing the detail of district-by-district breakdowns.

Another unfortunate shift in these reports' accounting, at the district level of aggregation, should be noted. Before 1922, offenses were classified by legal charge; after 1922, charges were aggregated into administrative categories. In particular, Volstead Act liquor cases were included in "Public Health and Safety" along with internal revenue liquor cases, narcotics cases, white slavery (i.e., prostitution) cases, peonage cases, and a few others. However, since Volstead Act cases consistently comprise about 90 percent of this category at the national level, in district-specific time-series analyses below, I will treat "Public Health and Safety" as equivalent to liquor cases. This accounting complication has been circumvented for national data.

1927 to fiscal 1930. Unlike the Attorney General reports, individuals, not indictments/informations, were the units of analysis. The final study aggregated its criminal statistics by the classification Nonliquor cases and Liquor cases, which included both Prohibition (Volstead Act) and Internal Revenue Service liquor cases.

For my purposes, the most valuable (and rare) data in this study were average length of trial and sentencing by plea. Average length of trial is a crucial variable in the caseload argument. Sentencing data by plea and offense were used to calculate sentence discounts (see sec. IV below).

Finally, as elaborated below, these two primary sources of data were supplemented in a variety of ways: district judge identities were obtained from the *Federal Record;* biographical information on district judges was obtained from *Who's Who;* public sentiment about prohibition was measured with roll call voting data from the *Congressional Record;* Volstead Act arrest information was obtained from the annual reports of the Bureau of Prohibition. In all, this is a more complete data base on federal courts during this period than has ever before been assembled.

III. GUILTY PLEAS AS A RESPONSE TO CASELOAD

My first task is to evaluate the administrative capacity argument. Figures 2a and 2b present the behavioral and criminal caseload time trends for the federal district courts as a whole.[6] Figure 2a shows trends in court behavior: percentage guilty pleas for Volstead Act cases and for all other cases, and percentage dismissals for the same two classes of cases.[7] Figure 2b shows trends in the criminal caseload impinging on the courts.[8] Again, Volstead Act prohibition cases are separated from the rest.

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[6] Actually, these national data exclude the District of Columbia district court because for this court reporting was not consistent over time. In particular, the number of both guilty pleas and trials was reported to be zero for 1910–15, even though convictions annually were reported in the thousands. One result of including data from the District of Columbia is the misleading plot presented in the American Law Institute's own study (p. 58), wherein a very sharp increase in the guilty plea rate is shown for 1916.

[7] Guilty plea percentages are measured here and throughout not as percentage of convictions but rather as the probability, on average, that defendants choose guilty pleas. That is,

(# guilty pleas / # guilty pleas + # trials).

Dismissal percentages include both *nolle prosequi* dismissals by prosecutors and "quashed, dismissed, demurrer, etc." by judges:

(# nol pros + # quashed / # cases terminated).

Of the two modes of dismissal, *nolle prosequi* by prosecutors are by far (70–90 percent) the predominant type in this data set.

[8] In the period under study here, criminal cases predominated in the federal courts, due to Prohibition. Sample distributions of the overall commenced-case mix at the beginning and end of the study period are as follows:

	1908	1932	1934
Criminal cases to which U.S. was a party	13,345	92,174	34,152
Civil cases to which U.S. was a party	3,202	34,189	9,487
Civil cases to which U.S. was not a party	11,703	26,326	26,472

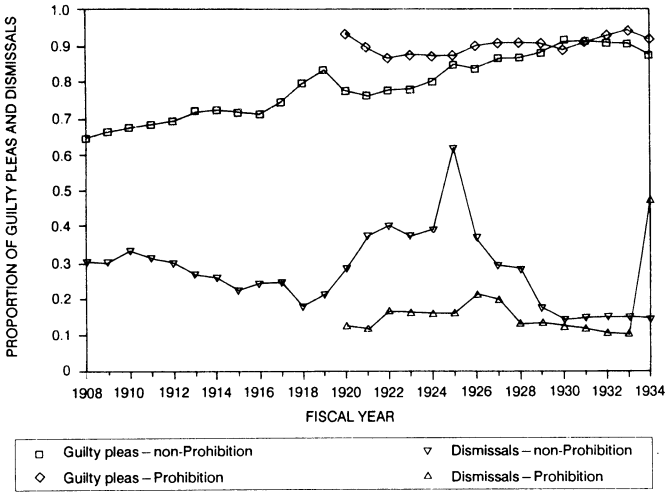


Figure 2a. Disposition of cases in federal district courts, 1908–1934 (District of Columbia excluded)

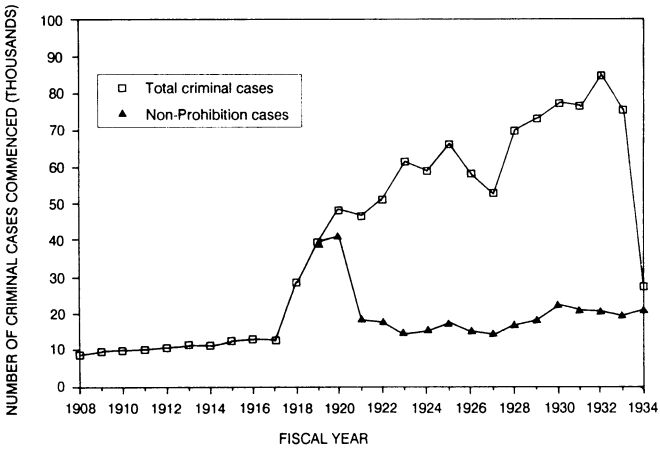


Figure 2b. Federal district court caseload, 1908–1934 (District of Columbia excluded)

As is clear in Figure 2b, actually two caseload shocks hit the system during this period. Before the massive flood of Prohibition cases began in earnest in fiscal 1921, there was a prefiguring of the caseload to come—a short but intense burst, from fiscal 1918 to fiscal 1920, of World War I draft resistance cases, which tripled the number of incoming criminal cases. These cases caused a transient increase in the nonliquor guilty plea rate, but mostly they remained in the pending docket, eventually to be dismissed after the fervor of the war had passed (compare Fig. 2a with Figs. 6a and

Moreover, as is apparent, many of the "civil cases to which U.S. was a party" were in fact Prohibition related. The American law Institute (1934) study labels these "quasi-criminal" civil cases.

6b). The long-term caseload effect of World War I on plea bargaining hence was minimal.

During the post-1920 Prohibition period per se, there would seem to be clear evidence for the impact of caseload on guilty plea rates. From the beginning, Prohibition liquor cases were plea bargained at a higher rate than were nonliquor cases. However, from 1920 onward, the nonliquor guilty plea rate gradually rose until it equaled the liquor guilty plea rate. At that point, it flattened out.

On the other hand, as Figure 2s also shows, this rise in the nonliquor guilty plea rate was a trend of long standing. Indeed, apart from the transient World War I effect, nonliquor guilty plea rates for the nation as a whole rose smoothly and linearly from 65 percent in 1908 to about 90 percent in the early 1930s. Obviously, this raises a question about whether the apparent caseload effect on guilty pleas might be spurious.

Apart from the rather massive delayed dismissal of World War I draft cases, an almost mirror image decline in nonliquor dismissal rates is also observed: from 30 percent in 1908 to about 15 percent at the end of our period (see also Figs. 6a and 6b). Liquor dismissal rates declined in parallel manner (except, of course, for the wholesale dismissal of cases in fiscal 1934, when Prohibition was abolished by the twenty-first amendment).

Ambiguity in the time-series support for the caseload hypothesis can be resolved with the help of the ALI study. A cross-sectional test is more rigorous here because the underlying data are more refined.

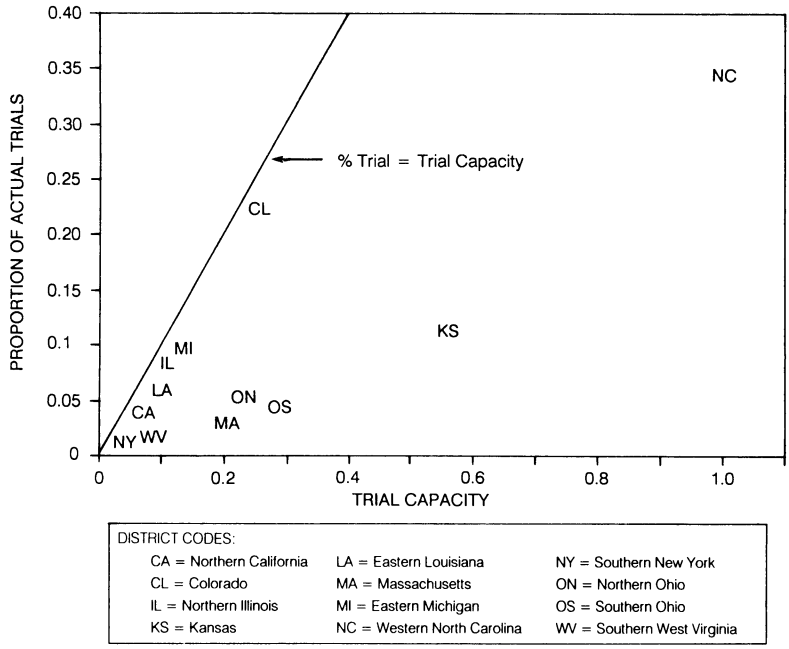
The caseload argument actually refers not to raw caseload alone, but rather to caseload relative to trial "carrying capacity" (Padgett, 1985: 761–62). Trial carrying capacity is the percentage of trials, jury and bench, that a court can process if it works full time at this task. This percentage constraint is (*ibid.*, p. 771)

$$\text{Prob (Trial)} \leq (k/nl),$$

where n is raw caseload, k is the number of judges in the court, and l is the average length of trial (defined in annual units). If we further assume that judges devote all their time to trials, then the above constraint becomes a predicted equality. That is, if the caseload argument were completely true and if judges had no preference for "free time" (including other court business), then the predicted percentage of trials (equals one minus the percentage of guilty pleas) would be exactly the quantity indicated.

Given the ALI study, this k/nl quantity can be calculated for each of the thirteen courts.[9] Figure 3 presents the scatterplot of

[9] The ALI study (1934) reports the distribution of trial lengths, in days, from which an average was calculated (using midpoints of intervals). I consulted the *Federal Reporter* (for January of each year) in order to code the names and number of full-time district judges on duty in all eighty-two district courts over the entire 1908–32 period. From these names, k was calculated as the average number of the thirteen courts' judges on duty for the three years of the ALI study. For purposes of the trial capacity calculation, raw caseload n



DISTRICT CODES:

CA = Northern California	LA = Eastern Louisiana	NY = Southern New York
CL = Colorado	MA = Massachusetts	ON = Northern Ohio
IL = Northern Illinois	MI = Eastern Michigan	OS = Southern Ohio
KS = Kansas	NC = Western North Carolina	WV = Southern West Virginia

Figure 3. Caseload constraint test (Connecticut outlier to right).

these cross-sectional data (except for one Connecticut outlier, off to the right of the graph). The full capacity line, defined by Prob(Trial) = Trial Capacity, is also presented for comparison. Visually, it is obvious that the data do *not* fall along the predicted full-capacity line.[10]

The line graphed in Figure 3 is a prediction of the caseload argument, however, only under the auxiliary assumption that judges have no preference for "free time" (Padgett 1985: 771). Posed this way, the caseload argument is clearly wrong. There are a number of courts with quite light caseload pressure (depicted on the right of the scatterplot) that have far more guilty pleas than can be explained by caseload alone.

On the other hand, it is equally apparent that each court, bar none, falls below the trial capacity constraint. In other words, the inequality version of the caseload argument, which drops the aux-

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was taken to be the sum of criminal and civil cases commenced, averaged over the same three years. To translate trial length into appropriate annual units, an estimate of the number of annual judge-days available for trial duty is required. I chose 200 days (equals 50 weeks $\times$ 4 days per week), under the assumption that at least one day per week must be available for other court business. Given all this, trial capacity was calculated as:

$$\text{Trial Capacity} = \frac{[(\#\text{ judges}) * (200/\text{mean trial length})]}{\#\text{ criminal} + \#\text{ civil cases commenced})}$$

[10] A simple regression through the data in Fig. 3 generates a significant relationship ($R^2 = .668$; $t = 4.48$). However, this relationship is destroyed by the Connecticut outlier ($R^2 = .018$; $t = 0.45$).

Table 1. Caseload Pressure

	% Guilty Pleas			Average Filings per Judge	Average Trial Length (Days)		
	All Cases	Liquor Cases	Non-liquor Cases		All Cases	Liquor Cases	Non-liquor Cases
Northern California	93.6	96.6	87.9	843	3.635	3.691	3.609
Colorado	76.8	75.5	79.8	522	1.575	1.374	2.184
Connecticut	98.8	98.7	99.4	156	0.600	0.611	0.500
Northern Illinois	91.1	76.9	95.1	1,159	1.749	0.667	3.055
Kansas	86.5	81.4	86.9	220	1.638	2.611	1.514
Eastern Louisiana	88.6	90.7	86.7	1,429	1.408	1.797	1.136
Massachusetts	95.7	96.5	93.0	428	2.277	2.268	2.293
Eastern Michigan	87.0	91.7	83.9	1,071	1.478	1.505	1.468
Southern New York	90.9	96.6	82.7	1,494	4.510	3.905	4.646
Western North Carolina	57.2	56.4	61.0	384	0.520	0.506	0.596
Northern Ohio	94.6	96.6	92.9	506	1.748	1.477	1.851
Southern Ohio	92.2	96.0	83.5	436	1.599	1.566	1.616
Southern West Virginia	97.9	98.0	97.3	2,408	1.098	1.118	1.000

SOURCE: Percentage of guilty pleas and average trial length from American Law Institute (1934) study; filings per judge from U.S. Department of Justice, Office of the Attorney General (1908–34) *Annual Reports* and *Federal Reporter*.

iliary "no free time" assumption, is indeed satisfied. Posed this way, the caseload argument is correct. With light caseload pressure, courts are free to do whatever they want, including generating guilty pleas. With heavy caseload pressure, however, courts are forced, by one means or another, to generate a high rate of guilty pleas.

As I have described elsewhere (Padgett, 1985), the caseload argument has been the source of much controversy in the literature (Heumann, 1978; Feeley, 1979). Figure 3 demonstrates why both sides have been correct. Statistical tests that compare guilty plea rates of low- and high-caseload courts are very likely to find the caseload argument to be false, because of high guilty plea rates in low-caseload courts. On the other hand, qualitative reports by both researchers and practitioners about the impact of caseload in high-caseload courts are very likely to be accurate, because of the absence of low guilty plea rates in high-caseload courts.

In interpreting this conclusion, it is important to bear in mind that "caseload pressure" is a composite concept. Table 1 breaks down the Figure 3 data into its parts: caseload per judge and average length of trial. These two components are not correlated (r^2=.048). That is, federal judges on the whole did *not* respond to

increased caseload by speeding up their trials, even though there clearly is wide cross-sectional variation.

In particular, according to the ALI study itself (p. 131), bench trials were uncommon in the federal courts during this period: only the Western North Carolina and Northern Illinois districts (for liquor cases only) used them. All other districts relied exclusively on jury trials, regardless of charge. Counterarguments notwithstanding (Frankfurter and Corcoran, 1926), jury trials were conceived of in this period as constitutional rights not to be infringed, even in those districts with heavy caseloads. As is implied in Table 1, however, this does not mean that all districts administered their jury trials with equal procedural care (cf. Langbein 1979).

IV. GUILTY PLEAS AS A RESPONSE TO SENTENCING

Next I estimate the form of plea bargaining employed in the federal courts during this period, and lay the groundwork for an evaluation of the substantive justice argument. I earlier derived a number of predictions about the relationship between plea bargaining and sentencing policies (Padgett, 1985). One such conclusion was that no aggregate statistical relationship should exist between guilty plea rates and "sentencing severity" (i.e., the mean level of sentencing). This conclusion contradicts the intuition of a number of authors reviewed there. Whether a judge is a hanging judge or a soft judge should make no difference to a defendant's plea calculations (as opposed to happiness), because even though the defendant is risk-averse, he is making a relative or ratio choice between two unpleasant alternatives.[11]

In contrast, the models showed the relationship between guilty plea rates and sentence discounts to depend strongly on the type of plea bargaining in place. For charge reduction plea bargaining, the predicted relationship was so modest that statistical studies are unlikely to find anything significant. For sentence recommendation or judicial plea bargaining, the predicted relationship was not continuous: no sentence discounts up to guilty plea rates of about 85 percent, and steeply increasing discounts thereafter. For implicit plea bargaining, the model predicted a strong and continuous relationship between sentence discounts and guilty plea rates throughout, the slope of which depends on average strength of case (see Fig. 4).

To test the first hypothesis, "sentencing severity" was calculated as the expected prison sentence[12] if convicted, whether by

[11] Misdemeanor crimes are an exception to this statement (Feeley, 1979). When prison is not an issue, transaction or "process" costs that otherwise are relatively trivial can loom large. Felony liquor crimes involving mere possession, however, might count in this category, since (depending on the district) prison in these cases was often only a remote possibility.

[12] That is,

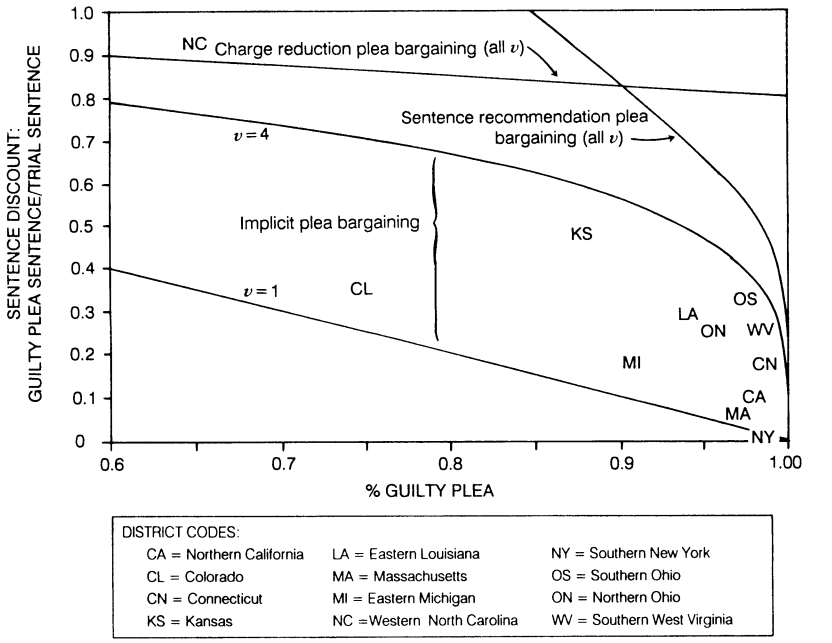

Figure 4a. Sentence discount schedules, liquor cases

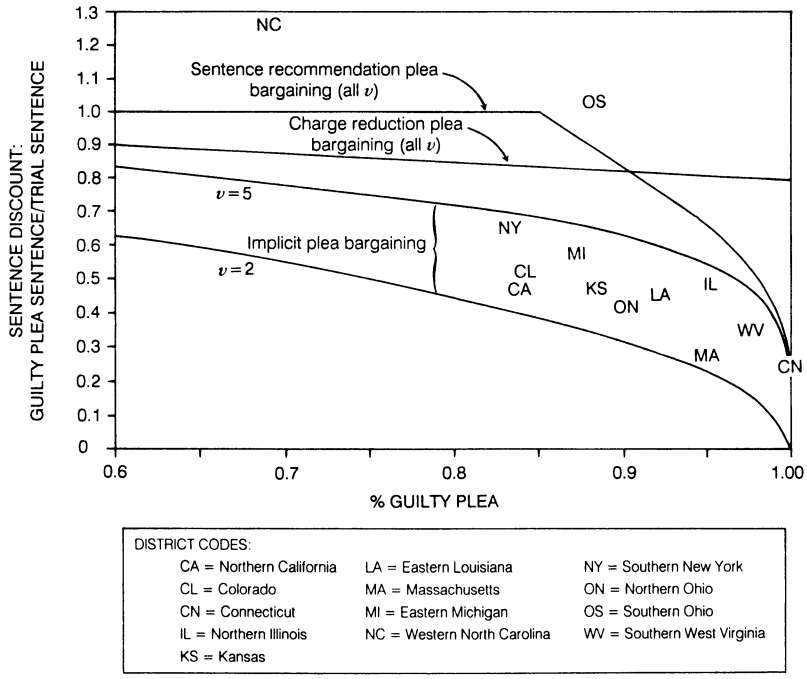

Figure 4b. Sentence discount schedules, nonliquor cases

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guilty plea or by trial. Although I do not present the data visually here, the predicted nonrelationship between sentencing severity and guilty plea rates was confirmed: for liquor cases, $r^2=.079$; for nonliquor cases, $r^2=.001$. Contrary to intuition, "hanging judge" courts did not generate higher guilty plea rates than lenient courts did.

Data on sentence discounts are more interesting. Figures 4a and 4b present the cross-sectional scatterplot of the ALI's thirteen courts. Sentence discounts were calculated as the ratio of expected prison sentence given guilty plea to the expected prison sentence given conviction at trial.[13] In this data set, sentencing severity and sentence discounts are statistically independent of each other: for liquor cases, $R^2=.191$ and $t=1.536$; for nonliquor cases, $R^2=.016$ and $t=-0.427$.

Superimposed onto the scatterplot are the sentence discount schedules derived in my earlier article (Padgett, 1985) for three of the four types of plea bargaining.[14] The parameter v is a measure of court-wide average strength of case: the higher the v, the stronger on average are the state's cases.[15]

It is clear from inspection that the data fit neatly within the discount schedule band for implicit plea bargaining and do not fit the other types. On the one hand, this represents a confirmation of the implicit plea bargaining model. On the other hand, it represents a confirmation (and clarification) of Attorney General Cummings's impressions. The apparent answer to one of our opening questions is that federal district courts coped with the crush of their Prohibition cases by intensifying sentence discounts within the preexisting implicit plea bargaining framework, rather than either by speeding up trials or by shifting to a more efficient bargaining mode.

The evidence of a statistical relationship between guilty pleas and sentence discounts in terms of traditional statistics is clearcut: for liquor cases, $r^2=.707$;[16] and for nonliquor cases, $r^2=.651$. There

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Expected Prison Sentence =
Prob (G.P.)*Prob(Prison | G.P.)*(Exp.Prison Sent. | Pris. & G.P.)
+ Prob (Trial)*Prob(Prison | Trial)*(Exp.Prison Sent. | Pris. & Trial)
The American law Institute study (1934) reports both fines and imprisonments as sentences, but to avoid scaling difficulties only imprisonment sentences were used here.

[13] That is,

$$\text{Sentence Discount} = \frac{\text{Prob(Prison}\,|\,\text{G.P.)*(Exp.Prison Sent.}\,|\,\text{G.P. \& Pris.)}}{\text{Prob(Prison}\,|\,\text{Trial)*(Exp.Prison Sent.}\,|\,\text{Trial \& Pris.)}}$$

Note that when the sentence discount equals 1, there is no difference between trial and guilty plea sentences.

[14] The sentence discount schedule for judicial plea bargaining is similar to that for sentence recommendation plea bargaining. I suppress it here only to improve visual clarity.

[15] In particular, $E(\text{Acquittal}\,|\,\text{Trial}) = (1/1+v)$.

[16] Northern District of Illinois (Chicago) had a sentence discount for liquor cases that was so extreme that I deleted it from all statistical analyses. It had a calculated value of 2.57, which means that guilty pleas were punished

are strong reasons for believing that this relationship is causal.[17] The district courts' data for liquor cases are bounded by $v=1$ and $v=4$, while for nonliquor cases they are bounded by $v=2$ and $v=5$. This means that in general liquor cases were weaker than nonliquor cases—a conclusion that is consistent with qualitative reports from the period (Assistant Attorney General Willebrant 1929: 120).

The magnitude of the sentence discounts indicated in these data are startling. For nonliquor cases, expected prison sentences after trial conviction ranged from 50 percent to four times higher than expected sentences after pleading guilty. For liquor cases, discounts were even more extreme—trial convictions from two to ten times higher than guilty pleas. Federal judges during this period clearly were willing to treat similar crimes very differently solely on the basis of the defendant's choice of plea. Indeed, given an apparently successful administrative ban against alternative plea bargaining procedures, judges may have had no other option (except perhaps summary bench trials, which few of them chose).

The empirical analysis, however, highlights one major flaw in the analysis in Padgett (1985). Earlier, I had posited that judges try to minimize sentence discounts. This hypothesis was motivated by "the common observation that trial judges (and prosecutors) are concerned with substantive rather than formal justice" (Padg-

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two and a half times more severely than convictions at trial! I strongly suspect the reliability of this particular datum.

[17] A potential complication is whether these sentence discounts are an artifact of charge composition effects. The ALI study did not disaggregate sentencing by charge. There are four reasons why the discounts measured here are not artifacts:

1. As long as district discounting percentages are constant across charges, then variation across districts in charge composition or in sentencing severity is completely irrelevant to the estimation of discounts. For the example of two charges, where d is the discount, p is the percentage of cases in the first charge class, and m_1, m_2 are mean sentences per charge:

$$\frac{dpm_1}{pm_1} + \frac{d(1-p)m_2}{(1-p)m_2} = d.$$

While perfect constancy is an idealization, modest deviation affects this conclusion only slightly.

2. Case docket studies of federal courts' sentencing in the 1960s and early 1970s have demonstrated strong effects of plea on sentence, even after controlling for many variables not available to me—charge, type of trial, legal representation, defendant's prior record, age, and race (Tiffany *et al.*, 1975; Cook, 1973). While not from my period, these studies do cover a period of known implicit plea bargaining.

3. Surveys of judges in the late 1930s (U.S. Department of Justice, Office of the Attorney General, 1939) and in the 1950s (*Yale Law Journal*, 1956) revealed that federal judges admitted and defended their differential sentencing of defendants by plea, largely on the grounds (a) that guilty pleas are evidence of remorse and/or (b) that the state is saved time and cost.

4. The only charge disaggregation of the ALI data we have is for liquor cases in Connecticut (Wickersham Commission, 1931a: 24). The number of trials is far too small to be definitive, but these data do not show that liquor trials involved more severe charges than guilty pleas.

ett 1985: 765). Indeed, this was the mechanisms that caused judges to switch plea bargaining role structures:

> I hypothesize that judges, consistent with their micro concern with the equity of individual dispositions, are concerned at the aggregate level with maintaining the integrity of their own internalized sentencing standards (whatever these might be). . . . To the extent to which a plea bargaining structure forces judges and prosecutors to generate guilty plea sentences that are on average at variance with their internalized sentencing norms, judges and prosecutors will be tempted to abandon that structure. Assuming (holding aside statutory code restrictions) that conviction at trial offers an unconstrained opportunity to apply these internalized standards directly, the sentence discount is a direct measure of this normative inconsistency. (*ibid.*)

The data, for federal district courts at least, show decisively this hypothesis to be wrong. Federal district judges during this period cared more about maintaining correct "no compromise" procedure than about maintaining substantive equity across plea classes.

V. CONTEMPORANEOUS ACCOUNTS OF FEDERAL PLEA BARGAINING

A major finding in the statistical analysis thus far is that federal judges responded to the crisis of Prohibition not by altering the form of plea bargaining but rather by intensifying sentence discounting within a preexisting implicit plea bargaining framework. Is there any qualitative evidence from this period that can help both to corroborate this conclusion and to provide clues about the reasons for this type of response?

First-hand accounts are extremely fragmentary, but some do exist. The Senate Judiciary Committee, for example, interviewed a few U.S. District Attorneys in their 1926 hearings on Prohibition (U.S. Senate, Judiciary Committee, 1926). District Attorneys from the Northern Illinois and Northern Ohio districts stated frankly that implicit plea bargaining, with its attendant sentence discounting, was an accepted practice in their districts:

> We do not bargain with liquor criminals and their lawyers . . . Yes, [pleading guilty] is to their advantage in this way: They have found that when they take the jury trial and the jury brings in a verdict of guilty the judge either gives them the limit provided by the law or somewheres near it. That when they plead guilty and they save the time of the court, he takes that into consideration and he always gives them a less punishment. (Edwin Olson, No. Dist. of Ill., in *ibid.*, p. 1233, 1235)

> The courts impose heavier sentences than when a man pleads guilty. If a man pleads guilty the sentence is not so

> heavy as if he is found guilty by a jury. (A.E. Bernsteen, No. Dist. of Ohio, in *ibid.*, p. 1268).

A particularly extreme version of implicit plea bargaining, which received much notoriety at the time (Pound, 1930: 184), was the so-called bargain days. In this technique, large numbers of defendants appeared before the judge on set days in order to plead guilty, with the clear understanding (but no explicit promise) that only fines would be forthcoming. A U.S. Attorney from New York City explained:

> The pleas of guilty that have been made in my district are from sheer necessity. . . . I have been criticized for clearing up to two to three thousand cases on a so-called bargain sale. Let me tell you about that. One year before, . . . in the same way, with the same judge and about the same number of defendants, the calendars were cleared. [Sen. Reed: And how are they cleared?] By letting the men plead guilty with the understanding that if they pleaded guilty the court would impose a fine. Otherwise, with the docket of 3,000 cases with which I started, I would be some six years trying those cases; even if the prohibition law had been repealed five minutes after I came into office. The reason why I dramatized it and called attention to it, although it was exactly the same institution that had always existed and always must exist if these petty violators are arrested, was simply to point out that this is not law enforcement. (Emory Buckner, So. Dist. of N.Y., in U.S. Senate, Judiciary Committee, 1926: 186)

In addition to the district of Southern New York, this practice also was apparently common in Massachusetts (U.S. Department of Treasury, Bureau of Prohibition, 1930: 39). Such "bargain days" account for the extreme positions of New York and Massachusetts in Figure 4a.

One proximate reason for the persistence of implicit plea bargaining was that many practitioners in the period drew a firm normative line between sentence discounting and explicit bargaining (cf. Olson's quotation above). No matter how regular or extensive the modification of sentencing, legal fictions were stoutly maintained that no precommitments of state action were granted and that judicial sentencing discretion was preserved.

No more powerful example of this can be given than the behavior of Northern Illinois District Judge Wilkerson in the famous Al Capone case. As mentioned in Attorney General Cummings's quote above, internal revenue violations were the one area in which Congress had expressly authorized the "compromise" of federal criminal cases. Based on this statute, Al Capone's lawyers had struck an explicit sentence bargain with U.S. District Attorney Johnson—namely, that Johnson would recommend to Judge Wilkerson a sentence of two and one-half years in exchange for a series of Capone guilty pleas. Given the importance of this case,

Johnson had carefully obtained explicit authorization for his offer from both the U.S. Attorney General and the Assistant Secretary of the Treasury.

In spite of the impressive credentials supporting this plea bargain, however, Judge Wilkerson in effect rejected the deal by refusing to promise or even imply in hearing that he would accept the prosecutor's recommendation. In his ruling on the plea, Judge Wilkerson explained his legal reasons for this action:

> Of course, the Court will receive [the prosecutor's] suggestions, and give to them the weight to which the views of counsel for the Government are entitled. It is always understood, however, that in consenting to receive these suggestions, the Court does not bind itself to adopt them, or to enter judgments in conformity therewith. There can be no exception to this rule. The parties to a Criminal case may not stipulate as to the judgment to be entered. That duty rests on the Court, and no one may relieve the Court of that responsibility. . . . This defendant must understand that he cannot have an agreement as to the judgment to be entered in this case. Pleas of guilty must be taken as unqualified admissions of the essential facts charged in the indictment. . . . They may not be acted upon by the Court, unless they are voluntary and unconditional. The power to compromise cases involving criminal liability when it exists under Federal Law, is not vested in the Courts. (*United States v. Alphonse Capone*, July 30, 1931: 4–5)

This confession construction of a guilty plea essentially rules out of court any effective explicit plea bargaining, even when authorized by congressional statute. Later, in response to the defense attorney's vigorous protest to this ruling, Judge Wilkerson elaborated his position with passion:

> Certainly, it is an unheard thing in a criminal proceeding, that anybody, even the court itself, could bind the court to the judgment which is to be entered after the hearing. . . . The court will listen, as I said this morning, to the recommendation of the District Attorney. But the thing is that the defendant cannot think, must not think—the thing about which there must be no misunderstanding is that in the end, the duty of the court is to enter judgment upon the record as it exists, at the close of the hearing. . . . It was high time that somebody bring to [Al Capone's] attention, and bring to his attention forcibly, the fact that it is utterly impossible to bargain with the Federal Court with respect to the judgment to be entered in a criminal case. (*ibid.*, pp. 13–15, 28)

In the end, after Al Capone's jury trial conviction, Judge Wilkerson sentenced him to eleven, not two and one-half, years.

Such vigorous assertions of judicial prerogatives are especially striking in comparative context. In the same city at almost the same time as the Capone trial, Chicago municipal judges defended their own deference to prosecutorial discretion. In 1928, the Chi-

cago Crime Commission brought suit to have three municipal judges dismissed on the grounds of permitting charge-reduction plea bargaining in their courts. But a panel of municipal judges justified their long-time practice of routinely rubber-stamping the state district attorney's downgrading of felonies to misdemeanors in the following terms:

> The judges of our State have acted on the assumption that it was entirely for the State's Attorney, the legal representative of the State, to determine whether he would continue the prosecution or discontinue it, or whether he would ask conviction on one count or charge contained in the indictment or another. . . . Without, however, expressing a definite opinion as to whether under our system of jurisprudence the Court has any discretion in the matter or not, or whether the exercising of the power to refuse the granting of such a motion has any practical value, we are of the opinion that no just criticism can be made of any judge who accepts the recommendation of the State's Attorney with or without inquiry into the reasons that prompt him to waive the felony in any case. We must call attention also to the fact that it would be rather a dangerous rule of practice to adopt that the judges should look with suspicion upon the action of the State's Attorney or treat his official acts as if no presumption of his integrity might be indulged in. (Illinois, Criminal Court of Cook County, 1928: 19, 23)

Later I will suggest that the juxtaposition, temporally, of these two contradictory self-images of judicial autonomy was no coincidence. The existence of one shaped the other.

VI. SENTENCING AS A RESPONSE TO JUDGES' BACKGROUND AND PUBLIC OPINION

In view of the importance of sentence discounts in the federal district courts during our period, a deeper investigation into the determinants of sentencing seems desirable. Three potential causal factors, derived from the substantive justice perspective, are explored here. To test the argument of Levin (1971), and indirectly the argument of Haller (1970), I have coded indicators of the professionalism and politicization of district court judges. Levin argued that local judges selected by bar associations are both more harsh and more standardized in their sentencing than are judges selected by political machines. Given the extreme variation in public support for Prohibition around the country, I also coded indicators of public opinion in the twelve states. The simple hypothesis is that judges' liquor sentencing policies were responsive to the public opinion about Prohibition in their states.

The simplest practical measure of professionalism from these sources is the percentage of judges who attended law schools

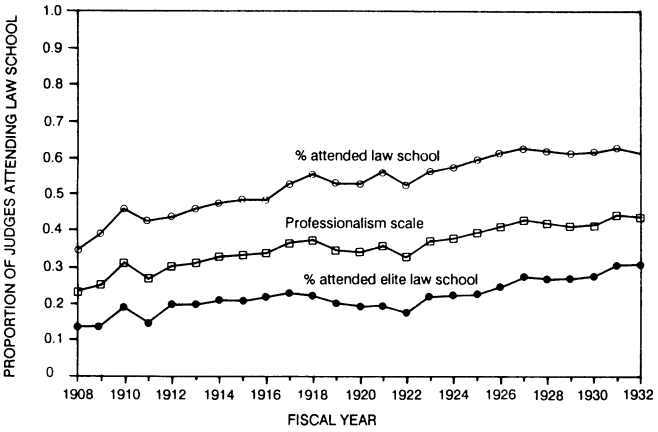

Figure 5. Judicial professionalism, national average, all district courts, 1908–1932

(rather than who only apprenticed in private law offices).[18] However, numerous sources reveal that law schools in this period varied widely in their commitment to the image of law as science and scholarship (Reed, 1921; Johnson, 1978). Only a few law schools led the way to professional "higher standards," primarily through the means of the case method. The Carnegie Foundation for the Advancement of Teaching (Reed, 1928) provided a contemporary itemization of these elite schools.[19] The percentage of judges who have attended elite law schools thus provides an alternative operationalization. A compromise is to combine these two measures into a professional training scale.[20] Figure 5 presents all three measures over time for the nation as a whole. As one may have suspected, professional training was incrementally on the increase throughout our period.

With career information, "politicization" of district courts can

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[18] Indicators of professionalism and politicization are based on data about judges' career histories. For all district judges listed in the *Federal Reporter* from 1908 to 1932, I consulted the two legally oriented *Who's Who*s published in the era: *Who's Who in Jurisprudence, 1925* and *Who's Who in Law, 1937*. Between them I located information on 207 of the 302 judges.

This was done at the suggestion of Albert Alschuler. Chris Ansell provided able research assistance in this task.

[19] The Carnegie Foundation's operational definition of elite law schools was "Group 1. Full-Time Schools requiring, after the High School, a Minimum of More than Five Academic Years" (Reed, 1928: 169). These Group 1 law schools, rank-ordered by the number of judges in my data set attending them, were as follows: Harvard (16), Michigan (16), Columbia (10), Yale (4), Pennsylvania (4), Wisconsin (2), Chicago (1), Northwestern (1), California at Berkeley (1), Cornell (1), Stanford (0), Pittsburgh (0), William and Mary (0), and Western Reserve (0).

[20] The following scale, applied to each judge and then averaged over judge-years by district or by nation, was used here: received LL.B. or J.D. from elite law school (1.00); attended elite law school but did not receive degree (0.75); received LL.B. or J.D. from non-elite law school (0.50); attended non-elite law school but did not receive degree (0.25); did not attend law school (0.00).

be measured two ways: the percentage of judges with a background in state or national legislatures, and the percentage of judges with job experience either in legislatures or in state parties.[21] These data are not reported graphically here, since there was no historical trend. The first measure remained constant at about 25 percent; the second measure stayed constant at 35 percent. Unlike what one may have expected, increased professionalism was *not* associated with a decline in politization.

For the public opinion hypothesis, I coded congressional votes from the *Congressional Record* on the only two direct roll call votes taken on the eighteenth amendment.[22] This may be a more accurate measure of organized opinion than of mass opinion.

Cross-sectional regressions of sentencing severity and sentence discount on best fitting measures of professionalism, politicization, and prohibition public opinion are presented in Table 2. The best fitting measure of professionalism was the professional training scale, most likely because it contains the most refined information. The best fitting measure of politicization was legislative background, perhaps because it contains the more reliable information.

The judicial background hypotheses fare as follows. Neither law school training nor political background has any significance in the explanation of sentencing severity. This negative finding is inconsistent with Levin's (1971) comparison of Minneapolis and Pittsburgh. However, in the explanation of sentence discounts, professional training is significant, whereas political background is not. Judges from law schools, and especially judges from elite law schools, relief more heavily on sentence discounts than did judges with only apprenticeship background.[23]

Extremes can be illustrated by example. Massachusett's dis-

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[21] Background in state parties, however, was not recorded as reliably in *Who's Who* as was background in legislatures. (I infer this from listings for judges in both sources.) Both measures are conservative in the sense that I did not include prosecutorial background in either of them. I had no way of ascertaining when prosecutor positions were highly politicized and when not.

[22] In particular, I coded the votes of my twelve state delegations in the House of Representatives on (a) the December 17, 1917, vote to send the eighteenth amendment to the states for ratification, which passed 282 dry to 128 wet, and (b) the March 14, 1932, preelection vote to extract a nullification bill from committee, which failed 227 dry to 187 wet. For my final measure, I averaged these two votes at the beginning and end of Prohibition.

I chose congressional votes over public referenda as an indicator in order to hold the question constant. I chose congressional votes over state legislature ratification votes because only finally successful votes were available (U.S. Department of the Treasury, Bureau of Industrial Alcohol, 1924–33). This source provides only a truncated sample of states, with votes that span numerous years. Scientific public opinion polls do not exist for this period.

[23] Technically of course, these are ecological regressions, and hence this leap to individual inference may be challenged. However, in this case the number of judges per court is often small: for the cross-sectional fiscal year 1928–30 period, four of the thirteen courts had only one judge, and three of the thirteen had only two. The maximum number, for the district of Southern New York, was six. The ALI study did not break down sentencing by judge.

Table 2. Cross-sectional Sentencing Regressions on Judges' Background and Public Opinion

		Sentence Severity (Mean Prison Sentence)				Sentence Discount (Guilty Plea Sentence/ Trial Sentence)			
		Liquor Cases							
Regression no.		(1)	(2)	(3)	(4)	(1)	(2)	(3)	(4)
Intercept	*a*	4.523	2.909	−0.033	−0.645	.625	.226	.003	.243
Professionalism: law school scale	*b*	−2.082			0.221	−.526**			−.359*
	(S.E.)	(2.380)			(1.974)	(.201)			(.188)
Politicization: Legislative job	*b*		0.930		1.116		.177		.181
	(S.E.)		(1.646)		(1.243)		(.130)		(.111)
Public opinion: Eighteenth amendment vote	*b*			5.784**	5.939**			.499**	.373*
	(S.E.)			(1.714)	(1.962)			(.197)	(.178)
	R^2	.065	.028	.509	.549	.391	.105	.392	.667
		Nonliquor Cases							
Regression no.		(1)	(2)	(3)		(1)	(2)	(3)	
Intercept	*a*	16.636	17.343	17.853		.874	.549	.863	
Professionalism: Law school scale	*b*	−0.637		−0.812		−.501*		−.499*	
	(S.E.)	(6.934)		(7.125)		(.265)		(.277)	
Politicization: Legislative job	*b*		−3.164	−3.185			.041	.029	
	(S.E.)		(4.609)	(4.834)			(.207)	(.188)	
	R^2	.001	.041	.042		.245	.004	.247	

* $p < .10$
** $p < .05$

trict court was dominated by judges from Harvard; Connecticut's court was dominated by Yale; the Eastern Michigan district was dominated by the University of Michigan; and the Southern New York court by a mixture of Columbia and Harvard. All these courts had high guilty plea rates based on extensive discounting, especially for liquor cases. At the other extreme was the district of Western North Carolina. This district was run by an apparently crusty old ex-Confederate soldier with no formal legal training. This judge believed in summary bench trials, with no differential treatment by plea, rather than in implicit plea bargaining.

Table 2 also presents data on the (organized) public opinion hypothesis. Federal judges' sentencing of liquor defendants was extremely sensitive to public opinion about Prohibition, both in severity level and in discounting. Federal judges from wet states simply did not severely punish Volstead Act offenders, especially those who pleaded guilty. This was a source of much controversy and, for some, consternation at the time. On the other hand, God help the poor liquor offender caught in a dry state (in particular, West Virginia).

Even the exceptions prove the rule. In Kansas, Judge John C. Pollock was well known to be out of sympathy with the dry sentiment in his state and to impose "inadequate" liquor sentences (Wickersham Commission, 1931e: 479). The net effect was that federal and state Prohibition enforcement officials prosecuted their cases in state rather than in federal court. (Kansas had a "bone dry" Prohibition law of her own.) Eventually in 1930, the federal court was expanded by one; the "guiding spirit" of the Kansas Anti-Saloon League was appointed to the vacancy (*ibid.*: p. 479), and federal liquor caseload immediately picked up.

Professionalism and Prohibition public opinion were correlated in this period: districts with elite judges tended also to be in wet states. Hence, significance levels of both coefficients decline in the discount multiple regression for liquor cases. Neither bivariate effect, however, is a spurious consequence of the other. Both magnitudes and significance levels remained considerable even in the presence of multicollinearity.

The professionalism findings here contradict the usual impression that, historically, plea bargaining was associated with the lower reaches of the legal profession. Political background per se was irrelevant, but the less well educated judges in this data set were *not* the ones to engage in implicit plea bargaining. (Apparently almost no one engaged in sustained explicit plea bargaining.) Quite the opposite: it was judges from Harvard, Columbia, and Michigan who led the way to massive sentence discounting in the federal courts.

VII. GUILTY PLEAS AS A RESPONSE TO STRENGTH OF STATE'S CASE

There is a second mechanism through which professionalization may have impinged on plea bargaining in the federal courts. Mather (1979b) has argued that professionalization of police and prosecutors improved the evidentiary quality of cases flowing into the court.

The strength of state's case explanation of plea bargaining has become common in the literature since its first introduction by Heumann (1978), but the conclusions drawn are sometimes contradictory because researchers have taken the perspective of different actors. For prosecutors, the incentive is to bargain away weak cases. That way, prosecutors can achieve at least some punishment instead of risking a loss at trial. Hence, the weaker the cases flowing through the court on average, the higher the guilty plea rate. For defendants, however, the incentive is to take weak cases to trial. *Ceteris paribus*, the weaker the average strength of the state's case, the lower the guilty plea rate.

As derived in my earlier article (Padgett, 1985), the net prediction depends on the particular form of plea bargaining in place. For sentence recommendation and charge reduction plea bargaining, essentially the two incentives cancel each other, and the net prediction is no aggregate relationship between average strength of case and guilty plea rates. For implicit and judicial plea bargaining, however, the prosecutor essentially is irrelevant. The net prediction thus is a positive relationship between average strength of case and guilty plea rates. If this argument is correct, as the average quality of cases increases over time (either because of better police work or because of better screening), so should guilty plea rates.

Strength of the state's case, or probability of conviction at trial, however, is one of those notoriously complex variables that experienced participants claim to be expert at estimating, but that researchers have a hard time measuring directly, even with case docket data. Therefore, I must proceed indirectly.

Figures 6a and 6b plot national dismissal and trial acquittal rates over time, after the misleading World War I selective service cases have been removed. Figure 6b separates Volstead Act cases from the rest, but Figure 6a pools them, since the trends are similar in both sets of cases. The graphs show an unambiguous and parallel decline in both dismissal and acquittal rates, both being almost a mirror image of the guilty plea trend.

There are a number of possible mechanisms that could induce a relationship between guilty plea rates and dismissal rates, strength of the state's case being only one. (1) Guilty pleas and dismissals are both possible ways of responding to caseload pressure. Under this scenario, however, dismissals should be increas-

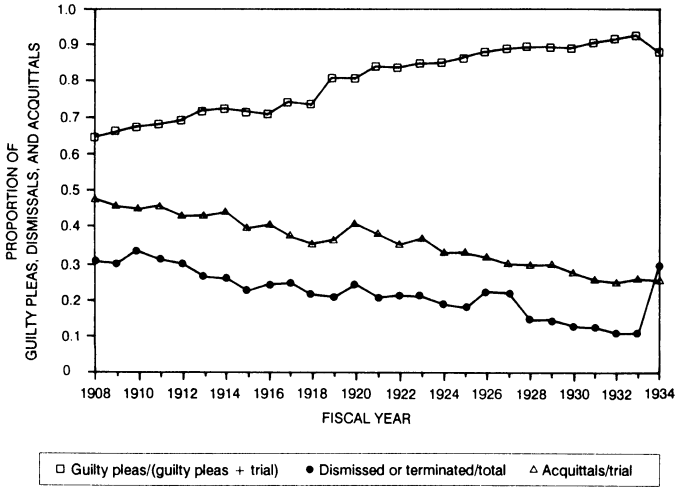

Figure 6a. Disposition of cases in federal court, 1908–1934 (Selective Service cases and District of Columbia excluded)

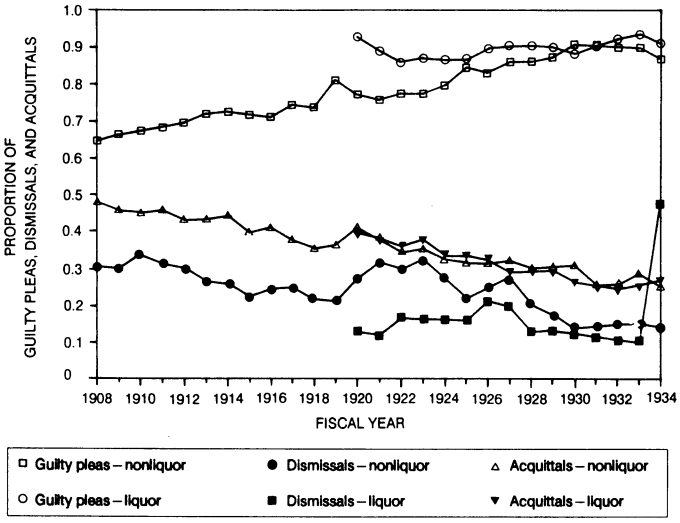

Figure 6b. Disposition of liquor and nonliquor cases in federal court, 1908–1934 (Selective Service cases and District of Columbia excluded)

ing not decreasing over time. (2) Improvements in the accuracy of filtering within the court dismissal process itself would generate increased guilty plea rates under implicit plea bargaining. Under this scenario, however, there is no reason to expect a decline in dismissal rates. (3) Another selection argument is that exogenously induced increases in plea bargaining cause progressively weaker cases to be creamed off from trials into pleas (Finkelstein, 1975). This may explain a decline in trial acquittal rates. However, this is an argument exclusively from the perspective of the

prosecutor. For implicit plea bargaining, a heavier use of sentence discounts results in weaker, not stronger, cases going to trial (Padgett, 1985: 768). (4) Finally, there is the straight substitution argument that, due to public objection to dismissals, state officials gradually began to plea bargain just those cases that earlier they would have dismissed (Moley, 1928: 102). For implicit plea bargaining, there is only one scenario under which this can occur: prosecutors stop dismissing, and judges start discounting. But then, trial acquittal rates increase, not decrease.

The most parsimonious explanation for all three of the historical trends observed in Figures 6a and 6b is very simple—a gradual and exogenous increase in the quality of criminal cases entering the court. Observed dismissal rates would decline under this scenario if prosecutors (and judges) behaved nonstrategically: They dismiss fewer cases because there are fewer bad cases to dismiss. Guilty plea rates would rise under this scenario because, with "dead-bang" cases, under implicit plea bargaining defendants have nowhere else to turn. Acquittal rates would decline under this scenario because, even though observed trials under implicit plea bargaining involve cases with much higher probabilities of acquittal than observed guilty plea cases, an historical trend in the quality of all cases would also cause a parallel (but upshifted) trend in the quality of trial cases, *ceteris paribus* (Padgett, 1985: 766–69). All this, of course, is exactly what is observed in Figures 6a and 6b.

Therefore, even though we have no direct measure of average strength of state's case (i.e., v), indirect evidence at the national level is strong that historical changes in this variable are implicated in the historical increase in guilty plea rates.

Given the above nonstrategic scenario as our simple "model" of causality, observed dismissal rates can be treated as a proxy variable[24] for average strength of case in order to investigate further, both in cross-sectional and in multivariate time-series analyses. Cross-sectional analysis of our thirteen districts confirms a negative relationship between guilty plea and dismissal rates. For liquor cases, the bivariate relationship was significantly negative at .05 ($R^2 = .321$; $t = -2.280$). In particular, the districts of Western North Carolina and Colorado had high dismissal rates and low guilty plea rates. For nonliquor cases, the bivariate relationship was negative but just short of significant at the .10 level ($R^2 = .230$; $t = -1.754$). Again, Western North Carolina was the most extreme high dismissal/low guilty plea district.

Now that the empirical plausibility of the case strength argu-

[24] Proxy variables are not intended as unbiased estimators of the unobserved variable of interest; rather they are observed variables expected to covary strongly with the unobserved variable. According to Figs. 6a and 6b, observed trial acquittal rates could also serve for this purpose, but the number of observed trials in some districts becomes too small once we disaggregate to the district level.

ment has been demonstrated (see also the multivariate analyses in sec. VIII), we can turn our attention to what caused this apparent increase in the evidentiary quality of the cases coming into the district courts. Two options present themselves: (a) The quality of police investigation and/or arrest practices improved over time, and (b) the commissioners improved their filtering at the preliminary hearing, even before cases reached the court.

Information can be assembled to assess the second possibility directly, for liquor cases only. Like most criminal courts, the federal courts have a preliminary hearing stage at which decisions are made about whether to bind the defendant over to the district attorney for indictment, but also about search and arrest warrants, subpoenas and bail (U.S. Department of Justice, Office of the Attorney General, 1918). Unlike most criminal courts, however, these federal commissioner hearings were staffed (at least during this period) by autonomous political appointees, who were not part of the judicial or police hierarchy. Unfortunately, the Attorney General's annual report provides no information about the actions taken at this preliminary stage, because the report was compiled from district attorney records.

A not entirely successful attempt was made to triangulate estimates of dismissals at commissioner hearings, for Volstead Act liquor cases only, by collecting time-series data on Prohibition arrests by state from the Bureau of Prohibition's annual reports. However, data comparability problems thwarted state level comparisons.[25] Fortunately, national statistics are less problematic.[26]

Figures 7a and 7b present the national data, both Bureau of Prohibition federal arrests and Volstead Act cases commenced in district courts. The change in accounting from the Treasury's indi-

[25] One accounting problem was that the Bureau reported Volstead Act liquor arrests, whereas at the district level of aggregation after 1922, the Justice Department reported "Public Health and Safety" cases commenced. This category can be disaggregated with national data only. Another, more intractable accounting problem was that, until the Bureau of Prohibition was transferred from Treasury to the Justice Department in fiscal year 1931, it reported arrests in terms of individuals arrested, rather than in terms of cases in which arrests were made.

Perhaps the most bedeviling barrier to cross-sectional comparisons, however, was alluded to in the Kansas vignette. For the most part federal arrests were prosecuted in federal courts, but sometimes federal arrests were prosecuted in state courts under local prohibition statutes, and sometimes state arrests were handed over to federal authorities. I had no way of controlling for these jurisdictional crossovers in order to estimate true commissioner dismissal rates. These problems with federal court statistics were well known at the time (Wickersham Commission, 1931c).

[26] The greatest potential problem with the national data was temporal shifts in jurisdictional crossovers. Federal officials at the time were constantly trying to get recalcitrant states to absorb more of the Prohibition burden.

A systematic national trend by state authorities into or out of federal courts would probably be reflected in cooperation patterns at the enforcement level as well. The latter statistics were reported in comparable units by the Bureau of Prohibition from 1925 to 1930 (U.S. Department of the Treasury, Bureau of Prohibition, 1924–33):

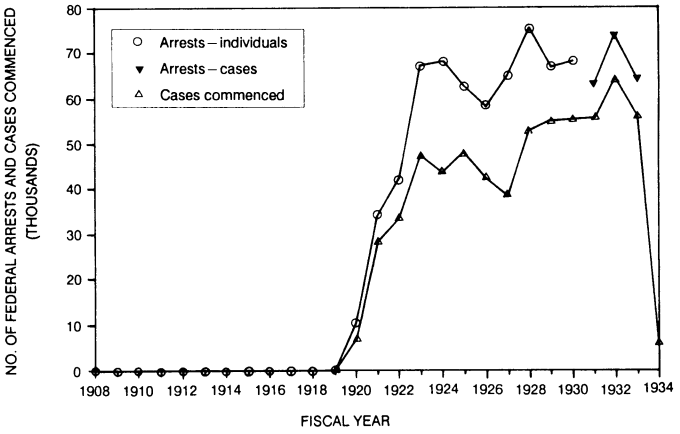

Figure 7a. Prohibition arrests for all federal courts, 1908–1934

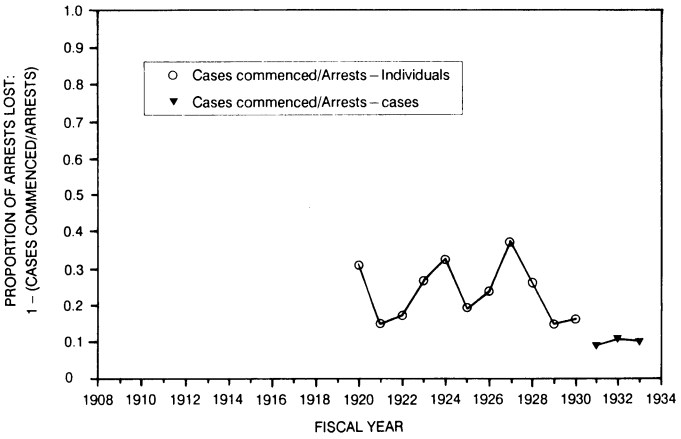

Figure 7b. Prohibition arrests not prosecuted, all federal coursts, 1908–1934

vidual arrests to the Justice Department's case arrests is apparent. Not surprisingly, data on cases arrested track data on cases commenced more closely than they do data on individual arrests. Especially when measured in comparable units, the data show that

	Persons Arrested by Federal Officers	Persons Arrested by State Officers, with Federal Assistance
1925	62,747	14,391
1926	58,391	14,332
1927	64,986	15,093
1928	75,307	14,166
1929	66,878	13,569
1930	68,173	17,027

No systematic temporal trends in arrest allocation are observed: Federal efforts to increase state participation in the enforcement of Prohibition were a failure. The troublesome variation in jurisdictional crossover, hence, is cross-sectional, not temporal.

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commissioner dismissal rates were extremely low. Moreover, percentage "arrest loss" (defined as 1 minus Cases Commenced/Individual Arrests) shows no systematic trend over time. For liquor cases at least, in other words, it is extremely unlikely that long-term increases in the evidentiary quality of the state's cases were due to improvements in the commissioners' filtering of cases at the preliminary hearing.

This conclusion is bolstered by the historical observation that, for commissioners, nothing structural changed during this period. Commissioners were patronage employees at the beginning of our period, and they were patronage employees at the end of our period. Given the commissioners' organizational autonomy, no formal mechanism existed to coerce a change in their behavior in order to assist the federal district courts.

By a process of elimination, this leaves only one possible explanation for the observed historical trend in strength of case: police work improved.[27]

As to which specific aspect of police work improved, we have

[27] Once we conclude that police work improved, however, it is not at all obvious about whom we are talking. As was described by numerous contemporary authors (e.g., Millspaugh, 1937), federal police work was organizationally very complex because there were so many special-purpose investigatory agencies. The Bureau of Prohibition took care of Volstead Act cases, and the Bureau of Narcotics (later folded into the Prohibition Bureau) took care of its namesake. The Alcohol Tax Unit (the "revenooers") of the Internal Revenue Service took care of traditional liquor cases. Postal inspectors dealt with violations involving the mails. The Bureau of Customs and the Coast Guard cooperated on smuggling cases. The Labor Department's Immigration and Naturalization Service took care of the smuggling of people. The Secret Service division of the Treasury occupied itself primarily with counterfeiting (rather than the protection of presidents). And the Departments of Interior and Agriculture had their own investigatory units in order to cope with land fraud cases and with food & drug and meat inspection cases, respectively. Even the FBI, currently our only general-purpose federal police force, was at this time only just beginning. It occupied itself (to its virtual disgrace) with prostitution, selective service, and espionage cases. No central compilation of statistics exists about the arresting behavior of all these various police.

This special-purpose profusion of federal enforcement agencies during this period, however, can be used to our advantage in sorting through police effects. For when we disaggregate national court data by legal offense, we are disaggregating by police agency as well. I have plotted national time-series data on guilty plea, trial acquittal, and dismissal rates (available on request), disaggregated by legal offense cum police agency. Once again, I used dismissal and acquittal rates as proxy variables for average strength of case.

The time-series plots reveal that, while the Immigration Service and the Interior Department showed dramatic improvement over time, the more typical pattern was slow incremental improvement. The Customs Bureau, the Post Office, the ICC, the FBI (after 1920), the IRS, and the Bureau of Prohibition all showed gradual, steady declines in acquittal rates, dismissal rates, or usually both. The IRS was a somewhat special case, because superimposed onto its long-term evidentiary improvement was a dramatic short-term reaction to Prohibition—massive dismissal of older liquor (and related) case backlog, and a fairly rapid modification of its guilty plea rates to equal that of the newer Bureau of Prohibition. Furthermore, while I do not present the details here, bivariate negative relationships between dismissal and guilty plea rates were statistically significant (at .05) for Prohibition, other liquor, customs, post office, banking, interstate commerce, land, immigration, and white slavery

little direct evidence. It could be that investigatory quality improved directly, or it could be that police were more selective in whom they arrested. The Wickersham Commission's case study of Connecticut (1931d: 19) concluded that federal agencies in this district were very selective in their arrests. The basis of this conclusion was the fact that 53 percent of the search and arrest warrants issued by commissioners at the behest of federal enforcement agencies did not result in defendants being brought forward to preliminary hearing. Without time-series or comparative data, however, it is hard to know how to interpret this statistic.

We are on firmer ground with the qualitative case studies of the period. Of the various federal police, the Bureau of Prohibition and the FBI have been studied the most extensively.

The Bureau of Prohibition in its early days experienced numerous scandals of corrupt profiteering and civilian killings and of high rates of personnel terminations "for cause." Formal reorganizations were almost continuous, first within the Treasury and then in the Justice Department. Eventually, however, control over agent appointment shifted from a political to a civil service basis; centralized training schools were established; and an internal investigation unit was created to ferret out corruption (Schmeckebier, 1929; Willebrant, 1929; Merz, 1931; Sinclair, 1962). District attorneys at the time reported a noticeable improvement in the quality of their liquor cases.

J. Edgar Hoover, who took over the FBI in 1924, made professionalization of the bureau into a national mystique. However, there was some truth to his extravagant claims. Hoover purged the FBI of its political appointees, instituted a strict merit system, and even promulgated behavior and dress codes for agents after hours (Lowenthal, 1950; Whitehead, 1956).

Thus, while the detailed mechanisms remain unclear, it seems likely that the historical trend toward stronger cases was rooted in the reform efforts by Progressives to substitute civil service and meritocratic standards for political patronage. This conclusion is bolstered by the empirical implication that, if indeed this was the cause, case strengths should have improved across the board, since the Progressives' reforms touched many parts of the executive branch of the federal government. In fact, federal police forces uniformly improved (see note 27).

VIII. MULTIVARIATE TIME-SERIES ANALYSIS

Some but not all of the above findings can now be assembled in a multivariate context, using available time-series data at both the national and the district levels. Guilty plea rates will be regressed on (a) standardized caseload, (b) dismissal rates, and

cases. In other words, consequential improvement in police work was not concentrated in a few agencies. It was virtually across the board.

Table 3. Time-Series Regressions, National-Level Data

Independent Variables		Dependent Variable: (Guilty Plea) Volstead Act Liquor Cases ($n=13$: 1920–32)	Nonliquor Cases[a] ($n=25$: 1908–32)
Intercept	a	1.020	0.609
(Total cases/# judges)$_t$	b	−.000283	.000174**
	(S.E.)	(.000170)	(.000034)
(% dismissals)$_t$	b	−.445*	−.406**
	(S.E.)	(.207)	(.098)
(Professional training)$_t$	b	.208	.588**
	(S.E.)	(.145)	(.140)
Durbin-Watson statistic		1.83	1.88
	R^2	.45	.95

NOTE: The b's are unstandardized ordinary least squares regression coefficients.

[a] Excludes Selective Service cases.

* $p < .10$

** $p < .05$

(c) judicial professionalism. Standardized caseload here means the total number of incoming criminal cases, divided by the number of district judges. (Information on average length of trial is not available across time.) Dismissal rates, as above, are used as a proxy for average strength of case. As should be clear from Figure 5, it does not matter statistically which particular measure of judges' law school background is used. Here, I will use the professional training scale to be consistent with the earlier cross-sectional analysis.

For national data, the time-series regression results, for both liquor cases and nonliquor cases, appear in Table 3. Durbin-Watson statistics, also reported in Table 3, indicate that autoregression is not an issue here; hence, ordinary OLS regression procedures are employed throughout.

For nonliquor cases, all three variables are significant at the .05 level. Thus, the bivariate findings reported above about caseload pressure, professionalism, and strength of case are *not* spurious consequences of each other, even though multicollinearity is present, due to trending. For liquor cases, only dismissal rate is significant at the .10 level, but the time series is very short in this analysis.

All three schools of thought about the emergence of plea bargaining can find comfort in these results. Given that most of the caseload in these regressions involves liquor cases, the finding of a

significant caseload effect for nonliquor cases implies a causal, rather than a compositional, interpretation. Given the information in Figure 7a and 7b, the finding of a significant strength of case effect should come as no surprise. The strong professionalism effect, even after controlling for other trending variables, provides reassuring cross-validation of the earlier comparative sentence discount finding.

These aggregate national conclusions were also confirmed in more detailed court-specific analyses, not reported here. The above OLS time-series regressions, applied to pooled liquor and nonliquor data, were repeated for each of the thirteen courts individually. The results were as follows. Controlling for other effects, increased caseload always increased guilty plea rates; not only that but, consistent with my constraint-only interpretation, the eight significant coefficients occurred in precisely those courts identified as high caseload pressure in Figure 3. Moreover, with two exceptions, fewer dismissals were associated with more guilty pleas (three times significantly so). And with three exceptions, more law school training for judges led to higher guilty plea rates (seven times significantly so).

IX. HISTORICAL CONTEXT

So far, we have derived the following conclusions:

1. The massive caseload of prohibition did increase federal plea bargaining but only in those districts with high caseload pressure.
2. Federal courts did not respond to increased prohibition caseload by speeding up their processing of trials.
3. Federal courts did respond to increased caseload by a heavy reliance on massive sentence discounts. Implicit, rather than explicit, plea bargaining was the predominant form of plea bargaining employed.
4. Judges from elite law schools led the way in the federal courts' development of implicit plea bargaining. (This conclusion holds even after controlling for correlates, such as caseload, dismissal rates and prohibition public opinion.)
5. In their sentencing of liquor violators, federal judges were very sensitive to public opinion about Prohibition in their states.
6. Guilty plea rates also rose during this period because of improvement in the evidentiary quality of cases. The most likely cause of this was professionalization among federal police.

The interpretive question now arises of how to make sense of these various empirical facts. That is, what is the historical scenario within which all of these facts cohere? My conclusion will be that implicit plea bargaining emerged as the hidden underside of elite lawyers' struggle to professionalize the courts.

The first point to make about the historical context of this pe-

riod is the vigor with which the elite bar attacked plea bargaining in the municipal courts. Appellate courts and crime commissions uniformly rejected plea bargaining as "paltering with crime" (Alschuler, 1979). But overlapping groups in alliance had somewhat different grounds for their opposition.

Law-and-order conservatives focused on the fact that plea bargaining gave undeserved leniency to criminals, especially to gangster criminals. The Illinois Association for Criminal Justice (1929: 318) argued that plea negotiation "gives notice to the criminal population of Chicago that the criminal law and the instrumentalities for its enforcement do not really mean business. This, it would seem, is a pretty direct encouragement to crime." As a consequence of this report, as mentioned above, the Chicago Crime Commission brought to administrative trial three Chicago judges who tolerated charge bargaining in their courts. One of their charges:

> All three entered orders waiving felony charges and accepted pleas of guilty to lesser offenses, so that the accused escaped with less punishment (or none at all) than was prescribed by the statutes. (Illinois, Criminal Court of Cook County, 1928: 4)

The New York State appellate court also took this law-and-order view.[28]

Given the magnitude of the sentence discounts observed in the federal courts, however, it seems fair to conclude that this criminology consideration was not the one that was foremost in the minds of the federal judges.

A second more legal objection to plea bargaining, commonly found in appellate court opinions of the time, was that guilty pleas were like confessions. English common law and the U.S. Supreme Court had held that "a confession, in order to be admissible, must be free and voluntary: that is, must not be extracted by any sort of threats or violence, nor obtained by any direct or implied promises, however slight" (*Bram v. United States*, 168 U.S., 1897: 542). Such a doctrine, when applied to plea bargaining, gave rise to the following state appellate court conclusions (Alschuler, 1979: 20):

> The least surprise or influence causing [the defendant] to plead guilty when he had any defense at all should be sufficient cause to permit a change of plea from guilty to not guilty. (*State v. Williams*, 45 La. Ann. 1356 (1893)

> No sort of pressure can be permitted to bring the party to

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[28] "[Through] acceptance of a plea of a lesser degree than that for which the defendant was indicted, those deserving of extreme punishment are permitted to escape with a suspended sentence or with punishment all too inadequate for the crime committed. We deplore the tendency of some district attorneys, following the course of least resistance, thus to relax the rigid enforcement of our penal statutes." (*People v. Gowasky*, 219 N.Y. A.D. 19 (1926)).

> forego any right or advantage however slight. The law will not suffer the least weight to be put in the scale against him. (*O'Hara v. People*, 41 Mich. 623 (1879)
>
> As the plea of guilty is often made because the defendant supposes that he will thereby receive some favor of the court in the sentence, it is the English practice not to receive such a plea unless it is persisted in by the defendant after being informed that such plea will make no alteration in the punishment . . . . All courts should so administer the law as to secure a hearing upon the merits if possible. (*Deloach v. State*, 77 Miss. 691 (1900)

In a landmark 1927 case, however, the U.S. Supreme Court expressly backed away from its earlier position:

> A plea of guilty differs in purpose and effect from a mere admission or an extra-judicial confession; it is itself a conviction. Like a verdict of the jury it is conclusive. More is not required; the court has nothing to do but give judgement and sentence. (*Kercheval v. United States*, 274 U.S. 223 (1927))

Judge Wilkerson's behavior in the Al Capone case indicates that not all federal judges agreed with this retreat. However, the fact that the Supreme Court chose as it did, plus the widespread practice of discounting without serious inquiry into factual basis, probably indicates that Judge Wilkerson's objection was also not the modal one among federal judges.

The modal objection to explicit plea bargaining among federal judges was rooted, in my opinion, in the threat that Prohibition posed to the public honor and dignity of the federal courts. The famous Wickersham Commission reports:

> Lawyers everywhere deplore, as one of the most serious effects of prohibition, the change in the general attitude toward the federal courts. Formerly these tribunals were of exceptional dignity, and the efficiency and dispatch of their criminal business commanded wholesome fear and respect. The professional criminal, who sometimes had scanty respect for the state tribunals, was careful so to conduct himself as not to come within the jurisdiction of the federal courts. The effect of the huge volume of liquor prosecutions, which has come to these courts under prohibition, has injured their dignity, impaired their efficiency, and endangered the wholesome respect for them which once obtained. Instead of being impressive tribunals of superior jurisdiction, they have had to do the work of police courts and that work has chiefly been in the public eye. (Wickersham Commission, 1931b: 56)

The connection between the professional dignity of judges and explicit bargaining was well articulated by the Wisconsin Supreme Court, in a comment on the very congressional statute which authorized the U.S. Attorney to compromise with Al Capone:

> We were educated, politically and professionally, in too high a reverence for federal authority in its sphere to have thought possible such a provision in a federal statute. . . . It commits the power [to compromise] exclusively to revenue officers; fitter, it seems to be assumed, for such a function than members of a profession educated in the morality of the common law. The section imposes none of its dirty work upon the bar. It authorizes no member of the profession to negotiate or contract with criminals for compounding their crimes. That seems to be taken as more in the way of revenue officials. . . . The section seems to have been framed in our view of the character and function of the profession of the law. Surely it needs no argument to show that it is unprofessional to compound crime, unprofessional to advise in or be privy to the compounding of crime. (*Wight v. Rindskopf*, 43 Wis. 361, 366 (1877))

"Compounding," by the way, is the common law conceptualization of charge reduction—a promise not to prosecute a crime.

Prohibition was also the period, don't forget, of continued Progressive assault on the political machines. The second charge made by the Chicago Crime Commission against the three municipal judges, mentioned above, was this:

> These three judges were put on the bench by the politicians against the protest and vote of the Chicago Bar Association. . . . The three judges have been improperly actuated and influenced by considerations emanating from the political condition which has existed in Cook County for a number of years. (Illinois, Criminal Court of Cook County, 1926: 1, 6)

Dean Harno (1928: 103) of the University of Illinois Law School elaborates on how explicit plea bargaining was perceived during this period, at least by the elite bar:

> When the plea of guilty is found in records it is almost certain to have in the background a session of bargaining with the State's Attorney. . . . These approaches, particularly in Cook County, are frequently made through another person called a "fixer." This sort of person is an abomination, and it is a serious indictment against our system of criminal administration that such a leech not only can exist but thrive. . . . As to qualifications, he has none, except that he may be a person of some small political influence.

Thus, Prohibition and political machines made the subject of explicit plea bargaining in the federal courts a highly charged subject among the insiders who cared. The elite bar, federal judges included, was struggling desperately to maintain the perceived dignity of their profession against the public's negative and undifferentiated perception of the legal system, which was developing at the time. This was the historical context for federal judges drawing an urgent but fine normative line between implicit and explicit plea bargaining.

So much for their rejection of explicit bargaining, but why did

federal judges choose implicit plea bargaining? "Choose" may be the wrong word, however, when implicit plea bargaining is the path of least structural resistance in a legal system dominated by judges with wide discretion. Federal judges were and are unusual in America in that they have held onto the common law right to actively question and comment on witnesses during jury trial (Cleary, 1984: ch. 2, p. 15). In the nineteenth century, by contrast, most state legislatures hemmed in judges' power to comment on the weight of the evidence (Friedman, 1973). On the sentencing side, moreover, judicial discretion to individualize sentences was given "scientific" justification by the leading psychologists of the time (Pound, 1930: 214).

On the other role front, since the creation of the U.S. Department of Justice in 1870, federal prosecutors are unusual in being embedded in national bureaucratic control. In contrast, state prosecutor offices have been elected autonomous posts since the Jacksonian era.

The federal courts, therefore, were structurally predisposed toward adaptation by judges. The caseload of Prohibition was simply the catalyst. Nor was reaction to liquor caseload merely a matter of independent and decentralized adaptation. One of the first acts of the newly created Judicial Conference[29] was to urge in 1923 a speedier processing of federal cases, to be enforced by the mandatory dismissal from the dockets of cases not settled after one year (Frankfurter and Landis, 1927: 248). In 1926 the "Conference was specially emphatic in urging the district judges to exercise their powers for the dispatch of criminal business" (*ibid.*). "Bargain days," but without explicit bargaining, was the unintended but structurally constrained result.

This historical scenario makes it more clear why it was judges from elite law school backgrounds who led the way in institutionalizing sentence discounts. These were the judges most preoccupied with public dignity and with procedural niceties in their courts. Hence, these were the judges least likely to adapt through summary bench trials or (God forbid!) explicit plea bargaining. These were also the judges most likely to insist on judicial prerogatives, whatever the cost. Finally, these were the judges most likely to respond to efficiency orders from the very pinnacle of the federal appellate hierarchy: in effect, "do not tolerate tardy justice, but also do not sully your hands."

Substantive justice and criminal deterrence issues took a back seat to such professional self-conceptions.

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[29] Established by Congress in 1922 at the initiative of Chief Justice Taft, the Judicial Conference was an administrative board composed of the Chief Justice from the Supreme Court and of the senior judges from the nine circuit courts. Its centralizing purpose was to provide a clearinghouse for legislative proposals to Congress, and to oversee the administration of the federal courts.

X. CONCLUSION

Largely because of reports sponsored by the legal profession itself, many had and have the impression that plea bargaining was a sordid activity that emerged from deep within those ethnic and political machine enclaves far removed from appellate review. During the Professive period, the legal elite, through bar associations and crime commissions, launched vigorous assaults on the lower reaches of municipal courts. However, they never pointed the finger at themselves.[30]

Perhaps the primary conclusion to emerge from this study, however, is that plea bargaining, in the federal courts at least, was a consequence of professionalization—professionalization of police, professionalization of trials (cf. Langbein, 1979), and professionalization of legal training. Such a revisionist conclusion raises once again all the standard disputes over the definition of plea bargaining. The fact remains, however, that federal courts during this period treated similar crimes far more inequitably than did those "corrupt" courts that used more explicit forms of plea bargaining.[31] Disputes over definitions should be seen for what they are: struggles for control over the courts.

This is not to say that other factors were not important. The caseload and public opinion induced by Prohibition were both catalysts in the institutionalization of sentence discounts. However, the time-series data clearly show a long-term trend quite apart from the fluctuations in these variables. Part of this trend was due to improvements in average strength of the state's case; part of it was due to increases in average length of jury trial; and part of it was due to increased recruitment of judges trained in law schools, especially in elite law schools. Elite judges were less predisposed against sentence discounts than were their less well educated brethren.

It is easy to speculate that this normative indifference to discounts is related to the substantive versus formal justice distinction. I suggested earlier (Padgett, 1985) that judges care more about the substantive justice of final punishments than they do about the formal justice of proper procedure. For federal judges, this assumption is certainly false. Administrative and professional barriers against explicit quid pro quo bargains with defendants were effective during this period, even though experienced participants no doubt knew full well what was going on. The image of a judge far removed from the fray, impartially enforcing procedures

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[30] The 1934 ALI study relied upon so heavily in this article is typical. The research was too good not to note in passing the existence of sentence discounts, but no critical or causal conclusions were drawn from this fact. As is also apparent in Attorney General Cummings's remarks, cited at the outset, implicit plea bargaining was accepted as normatively legitimate.

[31] This comparative statement is based both on this study and on the derivations in Padgett (1985).

"by the book," was carefully nurtured and protected, at great substantive cost, even in the face of extreme caseload pressure.

The coincidence of this institutionalization of implicit plea bargaining with Progressive crime commission and appellate assaults by the legal elite on lower municipal criminal courts was no accident. Victory in this struggle for control depended, in part, upon the maintenance of plausible evidence that the federal courts were paragons of higher virtue—operationally (and polemically) defined as the inverse of municipal court behavior. Federal district courts were molded reflexively by their opposition to lower courts.

Moreover, lower courts were shaped in part by their subversion of higher courts (Padgett, 1985: 794). Federal and lower courts thus mutually constructed each other, through an oppositional dynamic sustained by social cleavage (cf. Auerbach, 1976; Heinz and Laumann, 1983). In America, a vigorous insistence on professional ideals infused into the federal system a transposed version of the very object of those ideals' disdain. Bargain days emerged through denying bargaining.