

An Evaluation
of Judicial Case
Management Under
the Civil Justice
Reform Act

RAND

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CONTENTS

Preface	v
Figures	xi
Tables	xiii
Summary	xvii
Acknowledgments	xxxvii
Chapter One	
INTRODUCTION	1
Background to the Legislation	1
Overview of the CJRA Pilot Program	3
The Six Case Management Principles	3
The Six Case Management Techniques	4
Preview of Findings	4
Outline of This Report	5
Chapter Two	
EVALUATION DESIGN AND INTERPRETATION OF FINDINGS	7
Overview of the RAND Evaluation Design	7
Data Sources	9
Major Evaluation Measures	11
Time to Disposition	11
Litigant Costs and Judicial Resource Requirements	12
Satisfaction with Case Management and Views of Its Fairness	15
Representativeness and Comparability of Pilot and Comparison Districts	15
Levels and Methods of Analysis	19
Levels	19
Methods	19
General Civil Cases with Issue Joined	20
Interpretation of Findings	21
Chapter Three	
IMPLEMENTATION OF THE CJRA PILOT PROGRAM	23
The Advisory Group Process	23
How the Districts Implemented Their CJRA Plans	25

Differential Case Management	25
Early Judicial Case Management	27
Discovery Management	28
Alternative Dispute Resolution	30
Assessment of Implementation	31
Impediments to Change	33
The Process and Definition of Policy Change	33
Organizational Factors	34
Implementation Factors	35
Local Legal Culture	36
Understanding the Implementation of Change Under CJRA	37
The Challenge of Changing Organizations	37
Implementation Strategies and Principles	39
Facilitating Behavior Change	44
Chapter Four	
EFFECTS OF DIFFERENTIAL CASE MANAGEMENT	47
Introduction	47
Assessment	49
Chapter Five	
EFFECTS OF EARLY JUDICIAL CASE MANAGEMENT	51
Introduction	51
Estimated Effects of Early Case Management	52
Time to Disposition	52
Litigation Costs	54
Satisfaction	55
Views of Fairness	55
Assessment	55
Chapter Six	
EFFECTS OF DISCOVERY MANAGEMENT	61
Introduction	61
Estimated Effects of Discovery Management	62
Time to Disposition	63
Litigation Costs	64
Satisfaction	65
Views of Fairness	66
Assessment	67
Chapter Seven	
EFFECTS OF ALTERNATIVE DISPUTE RESOLUTION	71
Introduction	71
Estimated Effects of Alternative Dispute Resolution Referral	74
Estimated Effects of Mandatory Arbitration Referral	74
Effects of Other Types of ADR Referral	75
Assessment	75

Chapter Eight	
USE OF MAGISTRATE JUDGES AND OTHER CASE MANAGEMENT TECHNIQUES	77
Introduction	77
Estimated Effects of Having Litigants Available for Settlement Conferences	78
Estimated Effects of Increased Use of Magistrate Judges in the Civil Pretrial Process	78
Assessment	80
Chapter Nine	
EFFECTS OF THE PILOT PROGRAM AS A PACKAGE	81
Introduction	81
Statistical Analysis of Difference Between Pilot and Comparison Districts	81
Judicial Time Study Results	83
Survey of Judges About Difference in Case Management	84
Assessment	85
Chapter Ten	
CONCLUSIONS	87
The CJRA Principles and Techniques	87
Principles and Techniques We Could Not Evaluate	88
Results of Evaluation of Judicial Case Management	88
Effects on Time to Disposition	89
Effects on Costs (Lawyer Work Hours)	90
Effects on Satisfaction	90
Effects on Views of Fairness	91
A Promising Case Management Package	91
Implementing Change	92
Appendix	
A. Case Sample Design and Weighting	95
B. Surveys Conducted and Nonresponse Weighting	115
C. Recent Trends in Federal District Courts	129
D. Analysis of Time to Disposition	155
E. Analysis of Lawyer Work Hours	189
F. Analysis of Satisfaction	213
G. Analysis of Views of Fairness	229
H. Analysis of Time Expended by Judicial Officers	243
I. Judge Survey Responses	255
J. Lawyer Survey Responses	265
K. Litigant Survey Responses	287

I. Annotated Bibliography of Recent Research Literature on Issues of Delay and Cost of Litigation	303
BIBLIOGRAPHY	321
[C] Publications	333

FIGURES

S.1.	Effects of Early Judicial Management on Time to Disposition: 1992–93 General Civil Cases with Issue Joined	xxv
S.2.	Effects of Early Schedule for Trial on Time to Disposition: 1992– 93 General Civil Cases with Issue Joined	xxv
2.1.	Median Time to Disposition in Pilot and Comparison Districts	18
5.1.	1992–93 Time to Disposition with and Without Early Judicial Management: General Civil Cases with Issue Joined	53
5.2.	1992–93 Time to Disposition with and Without Early Schedule for Trial: General Civil Cases with Issue Joined and Early Management.	54
6.1.	1992–93 Time to Disposition with Shorter and Longer Days from Discovery Schedule to Cutoff: General Civil Cases with Issue Joined	63
C.1.	Full Time Equivalent Judicial Officers: RY1987–RY1995	133
C.2.	Civil Filings in Federal District Court: RY1987–RY1995	134
C.3.	Civil Terminations in Federal District Court: RY1987–RY1995	135
C.4.	Number of Civil Cases Pending at End of Year: RY1987–RY1995	136
C.5.	Growth in Number of Civil Cases Pending at End of Year Relative to RY1987	137
C.6.	Number of Cases Pending Over 3 Years for All Judicial Officers: 1991–1995	138
C.7.	Types of Civil Cases Filed: RY1987–RY1995	139
C.8.	Basis of Federal Court Jurisdiction: FY1987–FY1995	141
C.9.	Percent of Cases Disposed of During or After Trial: FY1987– FY1995	143
C.10.	Time to Disposition: RY1987–RY1995	144
C.11.	Percent of Cases Terminated Within 12 Months and Over 36 Months: RY1987–RY1995	145
C.12.	Number of Defendant-Cases Commenced, All Filing Levels: RY1987–RY1995	149
C.13.	Criminal Defendant Filings by Type of Offense, All Levels: RY1987–RY1995	150
C.14.	Criminal Offense Level at Time of Filing: RY1987–RY1995	152

TABLES

S.1. Summary of Statistical Evaluation of CJRA Principles and Techniques	xxxi
2.1. Sample Size and Responses to Surveys	11
2.2. Pilot and Comparison Districts for the CJRA Pilot Program	17
3.1. Differential Case Management: From Advice to Implementation	27
3.2. Pilot and Comparison Districts with Various Types of Discovery Limitations Before and After CJRA Plan Implementation	29
3.3. Early Disclosure of Information: From Advice to Implementation	30
3.4. ADR: From Advice to Implementation	31
5.1. Cases with and Without Early Judicial Management: 1991 Closed General Civil Cases with Issue Joined	56
5.2. Cases with and Without Early Judicial Management: 1992–93 Closed General Civil Cases with Issue Joined	57
5.3. Cases with and Without Early Schedule for Trial Date: 1991 Closed General Civil Cases with Issue Joined and Early Judicial Management.	58
5.4. Cases with and Without Early Schedule for Trial Date: 1992–93 Closed General Civil Cases with Issue Joined and Early Judicial Management.	59
6.1. Cases in Districts with Shorter and Longer Time from Discovery Schedule to Cutoff: 1991 Closed General Civil Cases with Issue Joined	66
6.2. Cases in Districts with Shorter and Longer Time from Discovery Schedule to Cutoff: 1992–93 Closed General Civil Cases with Issue Joined	67
7.1. Volume of ADR Referrals by District, Before and After CJRA	73
9.1. Cases in Pilot and Comparison District Samples: 1991 Closed General Civil Cases with Issue Joined	82
9.2. Cases in Pilot and Comparison District Samples: 1992–93 Closed General Civil Cases with Issue Joined	83
9.3. Average Judicial Time Reported Spent per Civil and Criminal Filing	84
10.1. Summary of Statistical Evaluation of CJRA Principles and Techniques	89
A.1. High, Medium, and Low Categories of Cases by Nature of Suit for Sample Selection Purposes	98

A.2.	Percentage of Cases and Judicial Officer Work Time in High, Medium, and Low Categories of Cases	99
A.3.	1991 Terminated Civil Cases in CJRA Sample	103
A.4.	1992–93 Filed Civil Cases in CJRA Sample	105
A.5.	“District Size” Sample Weights: CY1991 Terminated Civil Cases	107
A.6.	“Equal Districts” Sample Weights: CY1991 Terminated Civil Cases Average Sample Case Weight = 1.00	108
A.7.	“District Size” Sample Weights: 1991–93 Filed Civil Cases	109
A.8.	“Equal Districts” Sample Weights: 1992–93 Filed Civil Cases, Average Sample Case Weight = 1.00	110
A.9.	1993–94 Filed Criminal Cases in CJRA Sample	112
A.10.	Sample Weights: RY1993–94 Filed Criminal Cases	113
B.1.	Sample Sizes for Survey Data Collection	115
B.2.	Judge Survey Response Data for 1992–93 Civil Case Sample	116
B.3.	Lawyer Survey Response Data for 1991 Civil Case Sample	118
B.4.	Lawyer Survey Response Data for 1992–93 Civil Case Sample	119
B.5.	Litigant Survey Response Data for 1991 Civil Case Sample	120
B.6.	Litigant Survey Response Data for 1992–93 Civil Case Sample	121
C.1.	Full-Time-Equivalent Judicial Officers: RY1987–RY1995	133
C.2.	Civil Filings in Federal District Court: RY1987–RY1995	135
C.3.	Terminations in Federal District Court: RY1987–RY1995	135
C.4.	Number of Civil Cases Pending at End of Year: RY1987–RY1995	136
C.5.	Number of Cases Pending Over 3 Years, All Judicial Officers: 1991–1995	138
C.6.	Types of Civil Cases Filed, All Districts: RY1987–RY1995	139
C.7.	Filings by High, Medium, and Low Judicial Workload Category: RY1987–RY1995	140
C.8.	Civil Case Point of Disposition: RY1987–RY1995	142
C.9.	Mean and Percentiles for Time to Disposition: RY1987–RY1995	145
C.10.	Distribution of Months from Filing to Disposition: RY1987–RY1995	146
C.11.	Number of Defendant/Cases Commenced, All Filing Levels: RY1987–RY1995	149
C.12.	Criminal Defendant Filings by Type of Offense, All Levels: FY1987–FY1995	151
C.13.	Criminal Offense Level at Time of Filing: RY1987–RY1995	153
D.1.	Sample of General Civil Cases and Issue Joined Cases Used in TTD Analyses, by District and Sample Year	157
D.2.	Control Variables Used for Modeling Time to Disposition	160
D.3.	Policy and Procedure Variables Used in Modeling Time to Disposition	163
D.4.	Model for Time to Disposition: 1992–93 Sample, All General Civil Cases with Issue Joined	170
D.5.	Model for Time to Disposition: 1992–93 Sample, General Civil Cases with Time to Disposition over 270 Days	171
D.6.	Model for Time to Disposition: 1991 Sample, All General Civil Cases with Issue Joined	172

D.7.	Model for Time to Disposition: 1991 Sample, General Civil Cases with Time to Disposition over 270 Days	173
D.8.	Estimated Effects of the Components of Early Management on General Civil Litigation Cases	175
D.9.	Estimated Effect Sizes in Days for Selected Policy Variables for the Over 270 Day Sample	184
E.1.	Attorneys Responding with Work Hours on General Civil Cases by District and Sample Year: All Issue Joined and Time to Disposition Over 270 Day Samples	191
E.2.	Additional Control Variables Used for Modeling Lawyer work Hours	193
E.3.	Model for Lawyer Work Hours: 1992–93 Sample, All General Civil Cases with Issue Joined	195
E.4.	Model for Lawyer Work Hours: 1992–93 Sample, All General Civil Cases with Time to Disposition over 270 Days	196
E.5.	Model for Lawyer Work Hours: 1991 Sample, All General Civil Cases with Issue Joined	197
E.6.	Model for Lawyer Work Hours: 1991 Sample, All General Civil Cases with Time to Disposition over 270 Days	198
E.7.	Estimated Effects of the Components of Early Management on General Civil Litigation Cases	201
E.8.	Estimated Effect for Selected Policy Variables Measured in Lawyer Work Hours for the Over 270 Day Sample	209
F.1.	Attorneys Responding on General Civil Cases by District and Sample Year: All Issue Joined and Time to Disposition Over 270 Day Samples	216
F.2.	Model for Attorney Satisfaction: 1992–93 Sample, All General Civil Cases with Issue Joined	219
F.3.	Model for Attorney Satisfaction: 1992–93 Sample, General Civil Cases with Time to Disposition over 270 Days	219
F.4.	Model for Attorney Satisfaction: 1991 Sample, All General Civil Cases with Issue Joined	220
F.5.	Model for Attorney Satisfaction: 1991 Sample, General Civil Cases with Time to Disposition over 270 Days	220
F.6.	Estimated Effects of the Components of Early Management on General Civil Litigation Cases	222
G.1.	Sample Size by District for Modeling Attorney Response, All Issue Joined and Time to Disposition Over 270 Day Samples	231
G.2.	Model for Attorney Views on Fairness: 1992–93 Sample, All General Civil Cases with Issue Joined	233
G.3.	Model for Attorney Views on Fairness: 1992–93 Sample, General Civil Cases with Time to Disposition over 270 Days	234
G.4.	Model for Attorney Views on Fairness: 1991 Sample, All General Civil Cases with Issue Joined	234
G.5.	Model for Attorney Views on Fairness: 1991 Sample, General Civil Cases with Time to Disposition over 270 Days	235

G.6.	Estimated Effects of the Components of Early Management on General Civil Litigation Cases	236
II.1.	1989 FJC and 1992–93 CJRA Civil Time Studies: Total Minutes Reported in Cases with One or More Time Study Reports—All Judicial Officers, All Origins, 19 Reporting Districts	249
H.2.	1989 FJC and 1993–94 CJRA Criminal Time Studies: Total Minutes Reported for Defendants with One or More Time Study Reports—All Judicial Officers, 16 Reporting Districts	251

The Civil Justice Reform Act (CJRA) of 1990 emerged from a multiyear debate about ways to reduce delay and the cost of litigation in federal courts. Based on recommendations from Senator Joseph Biden's Task Force on Civil Justice Reform, the legislation required each federal district court to develop a case management plan to reduce costs and delay. The legislation also created a pilot program to test six principles of case management and required an independent evaluation to assess the effects of these principles and other related case management techniques. The Judicial Conference and the Administrative Office of the U.S. Courts asked RAND's Institute for Civil Justice to conduct the evaluation.

This document reports our evaluation of the effects of the CJRA case management principles and techniques on time to disposition, costs, and participants' satisfaction and views of fairness. The implementation of the act in the 20 pilot and comparison districts is described in a companion report.¹

OVERVIEW OF THE CJRA PILOT PROGRAM

The pilot program required ten districts to incorporate certain case management principles into their plans. The evaluation included ten other districts to permit comparisons. The 20 districts were selected rather than being volunteers, and all those selected were required to participate in the study.

The ten pilot districts, selected by the Committee on Court Administration and Case Management of the Judicial Conference of the United States, were: California (S), Delaware, Georgia (N), New York (S), Oklahoma (W), Pennsylvania (E), Tennessee (W), Texas (S), Utah, and Wisconsin (E).

The Judicial Conference, in consultation with RAND, also selected the following ten comparison districts: Arizona, California (C), Florida (N), Illinois (N), Indiana (N), Kentucky (E), Kentucky (W), Maryland, New York (E), and Pennsylvania (M).

The pilot districts were required to implement their plans by January 1992; the other 84 districts, including the comparison districts, could implement their plans any time before December 1993.

¹Kakalik et al. (1996a).

The Six Case Management Principles

The act directs each pilot district to incorporate the following principles into its plan:

1. Differential case management;
2. Early judicial management;
3. Monitoring and control of complex cases;
4. Encouragement of cost-effective discovery through voluntary exchanges and co-operative discovery devices;
5. Good-faith efforts to resolve discovery disputes before filing motions; and
6. Referral of appropriate cases to alternative dispute resolution programs.²

Pilot districts *must* incorporate these principles, while other districts *may* do so.

The Six Case Management Techniques

The act directs each district to *consider* incorporating the following techniques into its plan, but no district is *required* to incorporate them:

1. Joint discovery/case management plan;
2. Party representation at pretrial conferences by an attorney with authority to bind that party regarding all matters previously identified by the court for discussion at the conference and all reasonably related matters;
3. Required signature of attorney and party on all requests for discovery extensions or trial postponements;
4. Early neutral evaluation;
5. Party representatives with authority to bind to be present or available by telephone at settlement conferences; and
6. Other features that the court considers appropriate.

FEATURES OF THE RAND EVALUATION

The evaluation is designed to provide a quantitative and qualitative basis for assessing how the case management principles and techniques identified in the CJRA affect costs to litigants, time to disposition, participants' satisfaction with the process,³ views of fairness of the process, and judge work time required.

²Our evaluation of mediation and neutral evaluation, based on a supplemental study, is the subject of a companion report. See Kakalik et al. (1996b).

³Satisfaction and views of fairness were measured by responses to the following questions: How satisfied were you with the court management and procedures for this case for your party or parties? How fair do you think the court management and procedures were for this case for your party or parties?

Comparisons are made between the ten pilot and ten comparison districts using data from cases terminated in 1991 before CJRA and separately using data from cases filed in 1992–93 after implementation of the pilot program plans. Because there are differences between our pre-CJRA and post-CJRA data unrelated to CJRA and difficult to properly account for, we focus on separate pre- and post-CJRA analyses. The results of our qualitative analysis, combined with our separate pre- and post-CJRA quantitative analyses, provide ample evidence concerning the effects of the act.

The evaluation also uses quantitative analyses to compare cases managed in different ways to determine the effect of such management practices on costs to litigants, time to disposition, participants' satisfaction with the process, and views of fairness. To explore the effects of management practices, the quantitative analyses exploit natural variation in judges' management practices, rather than making an experimental random assignment of management practices to cases.

Representativeness and Comparability of Pilot and Comparison Districts

Ideally, the pilot and comparison districts would be similar in every respect except case management policies. Factors for which data were available, such as district size, workload per judge, the number of criminal and civil filings, and the time to disposition in civil cases, were therefore used to identify suitable comparison districts.

Using aggregate 1991 data, we examined the districts along these dimensions and concluded that the pilot districts were comparable to the comparison districts. Based on additional data collected in our surveys, our multivariate statistical analysis later confirmed that there was no statistically significant difference between the pilot and comparison district groups in 1991 in either time to disposition or cost per litigant.

We concluded that the pilot and comparison districts adequately represent the range of districts in the federal judicial system. Together, the 20 study districts have about one-third of all federal judges and one-third of all federal case filings and represent the full spectrum of the system with respect to the critical variables identified above.

Data Sources

The evaluation is based on extensive and detailed case-level data from the period January 1991 through December 1995. Data sources include:

- Court records;
- Records, reports, and surveys of CJRA advisory groups;
- The districts' cost and delay reduction plans;
- Detailed case processing and docket information on a sample of cases;
- Surveys of judicial officers, and reports on their activities, time expenditures, and views of CJRA;

- Mail surveys of attorneys and litigants about costs, time, satisfaction, and views of the fairness of the process; and
- Interviews in person with judges, court staff, and lawyers in each of the 20 districts.

Similar data were collected for a special supplementary analysis of ADR programs in the six study districts with a sufficiently high volume of ADR cases to permit evaluation.

We used CJRA advisory group reports, documents, and meeting minutes to assess the advisory group process and findings; we used the districts' plans and proposed local rule changes to assess what the district said it would do under CJRA; we used the dockets for a large sample of cases to help us understand what was actually done on cases and when (such as schedule setting, assignment to management tracks, or referral to ADR); we used court records to assess the basic characteristics of the cases and court actions, such as referral to ADR, that were not always on the court docket; we used the judicial surveys on our sample of cases to get judges' views on whether they had changed how they manage cases as a result of CJRA; we used extensive mail surveys of thousands of lawyers and litigants on our sample of cases to get their views on how cases were managed and information on litigation costs, satisfaction, and views of fairness; and we used extensive semi-structured interviews with judges, court staff, advisory group members, and lawyers to better understand both the implementation of CJRA and case management in the districts before and after CJRA.

In total, more than 10,000 cases were selected for intensive study, and we attempted to survey more than 60,000 people. We received completed survey responses from judges on 3,280 cases (which are about two-thirds of all closed cases in our post-CJRA sample), from about 9,000 lawyers (about one-half of the lawyers surveyed), and from about 5,000 litigants (about one-eighth of the litigants we attempted to survey). Because of the low litigant response rate, we were limited in our ability to analyze litigants' hours spent, satisfaction, and views of fairness.

We base our primary assessment of case management policies and procedures on data from general civil litigation cases⁴ with issue joined.⁵ We also analyze the subset of these cases that took longer than nine months to disposition.

⁴In practice, federal district courts split the civil caseload into two categories—those cases that usually receive minimal or no management, and those general civil litigation cases to which the district's standard case management policies and procedures apply (and which are of primary concern for evaluation of CJRA case management principles and techniques). Minimal management is usually applied to prisoner cases (other than death penalty cases), administrative reviews of Social Security cases, bankruptcy appeals, foreclosure, forfeiture and penalty, and debt recovery cases.

⁵Issue is considered joined after the defendants have answered the complaint in accordance with F.R.Civ.P. Rule 12(a) or as mandated otherwise by the court (Administrative Office of the United States Courts (1995), Chapter 5, p. 15).

HOW THE CJRA WAS IMPLEMENTED

The CJRA required districts to appoint an advisory group to identify causes of delay and excess costs and to make recommendations to the court. In general, these groups approached their missions with dedication and conscientiousness. Most courts incorporated most of their advisory group's recommendations into their CJRA plans.

All pilot districts complied with the statutory language in the act, which provides loosely defined principles but leaves operational interpretation of them to the discretion of individual districts and judicial officers. Many pilot and comparison districts interpreted some or all of their current and past practices to be consistent with the language of the act and continued those practices unchanged. However, if the spirit of the act is interpreted to mean experimentation and change focusing on the six CJRA principles, then the pilot districts met that spirit to varying degrees. Comparison districts, which were required to consider but were not required to adopt the six CJRA principles in their plans, generally made fewer changes than pilot districts.

Even in pilot districts whose plans suggested major changes, implementation often fell short. Thus, there was less change in case management after CJRA than one might have expected from reading the plans. However, implementing the pilot plans may have heightened the consciousness of judges and lawyers and brought about some important shifts in attitude and approach to case management on the part of the bench and bar. For example, our interviews suggested, and the case-level data we collected confirmed, that the fraction of cases managed early has increased and that the median time to discovery cutoff has shortened.

EFFECTS OF THE CJRA CASE MANAGEMENT POLICIES

The evaluation presented in this section is based on quantitative analyses that compare cases managed in different ways to determine the effects of different management practices. These analyses exploit observational data resulting from the naturally occurring variation in judges' management practices, rather than data from an experimental random assignment of management practices to cases. These observational data have certain inherent constraints. In particular, judges and districts choose to use certain case management policies and practices, and we must assume that these judges and districts could differ from other judges and districts choosing not to use them. Because of these potential differences, our observed effects of a particular case management practice should be treated as an upper bound to what might occur if other judges and districts were asked to implement that practice.

One issue that has been raised regarding the CJRA concerns the appropriateness and effectiveness of national uniform standardized rules and procedures.⁶ Some people see CJRA as a "top down" reform started by Congress. Others see CJRA with its local advisory groups and local rule revisions as an attempt to tailor management to the

⁶For an interesting paper related to this issue, see Carrington (1996).

local legal needs and culture. Our research design did not address the debate over national versus local rules and procedures. Instead, we report here what happened as a result of CJRA and the application of management principles and techniques identified in the act; we leave it to others to draw conclusions on the issue of uniformity of rules and procedures.

The six principles and six techniques specified in the act can be usefully assigned to four case management categories: differential case management, early judicial case management, discovery management, and alternative dispute resolution. We use these categories in our discussion below.

Differential Case Management

The essence of the differential case management (DCM) concept is that different types of cases need different types and levels of judicial management. One way to implement DCM is to create a number of separate “tracks,” each of which implies a certain approach to case scheduling and management, and to assign cases early to these tracks. One such tracking approach is to define expedited, standard, and complex tracks, each with different levels and types of management.

Before CJRA, all courts had special management procedures for “minimal management” cases such as prisoner petitions other than death penalty cases, Social Security appeals, government loan recovery, and bankruptcy appeals. After CJRA, all courts retained their procedures for these cases with little modification. Minimal management cases are typically disposed of relatively quickly and cheaply with little or no judicial management necessary. Since districts made few changes in their procedures for minimal management types of cases, and since almost none of these cases are managed using the policies and procedures that apply to general civil litigation and are the focus of the CJRA, they could not inform our evaluation of the procedures of concern in the CJRA. For all of these reasons, we exclude the category of minimal management cases from our statistical analyses.

Another approach to case management is the “judicial discretion” model, in which judges make management decisions for general civil cases on a case-by-case basis. This was the predominant approach to case management in all 20 study districts before CJRA. Complex cases, for example, can receive individualized, specialized management within a framework enunciated in the Federal *Manual for Complex Litigation*.

Six of the ten pilot districts planned to replace the judicial discretion model with a track model, but that model proved difficult to implement. Most districts that included tracking in their plan actually assigned the traditional group of minimal management case types listed above to an expedited track. Five of the six pilot districts whose plans contained a track model assigned 2 percent or less of their cases to the complex track. Pennsylvania (E), which assigned 7 percent of its general civil cases to

the complex track, was the sole exception.⁷ The consequence was that almost all general civil cases to which CJRA procedural principles might be relevant were placed in the standard track, if any track assignment was made. This meant that there was little actual “differential” tracking of general civil cases in most districts that adopted a track model in their CJRA plan. Consequently, we have no basis for evaluating how the track method of DCM might have affected time, cost, satisfaction, and views of fairness.

Interviews with judges and lawyers suggest some reasons for the lack of experimentation with and successful implementation of a tracking system of DCM for general civil litigation. They include (1) the difficulty in determining the correct track assignment for most civil litigation cases using data available at or soon after case filing; and (2) judges’ desire to tailor case management to the needs of the case and to their style of management rather than having the track assignment provide the management structure for a category of cases.

With respect to the difficulty in determining the correct track assignment for a case, our statistical analysis indicates that the objective data available at the time of filing (such as nature of suit category, origin, jurisdiction, and number of parties) are not particularly good predictors of either time to disposition or cost of litigation. They apparently do not capture the real complexity of the case very well. This does not mean that a track system is not viable; rather, it suggests that if a track model is to be implemented, decisions about track assignments should be supplemented with subjective information from the lawyers or judge.

Special management of complex cases, the third CJRA principle, is a subset of differential case management. This principle lacked an implementation sufficiently consistent and well documented to permit evaluation.

Early Judicial Management

Early judicial case management includes the CJRA principle of early and ongoing judicial control of pretrial processes, as well as the optional technique of requiring that counsel jointly present a discovery/case management plan at the initial pretrial conference.

In our statistical analyses, we defined early judicial case management as any schedule, conference, status report, joint plan, or referral to ADR within 180 days of case filing. This definition gives time for nearly all cases to have service and answer or other appearance of the defendants (which legally can take up to six months), so issue is joined and it is appropriate to begin management if the judge wants to do so.

Early judicial case management is associated with both significantly reduced time to disposition and significantly increased lawyer work hours. We estimate a 1.5 to two month reduction in median time to disposition for cases that last at least nine

⁷ PA(E) also implemented other changes, the results of which we cannot reliably separate from the effects of the track system.

months, and an approximately 20-hour increase in lawyer work hours. Our data show that the costs to litigants are also higher in dollar terms and in litigant hours spent when cases are managed early. These results debunk the myth that reducing time to disposition will necessarily reduce litigation costs.

Lawyer work hours may increase as a result of early management because lawyers need to respond to the court's management—for example, talk to the litigant and to the other lawyers in advance of a conference with the judge, travel, and spend time waiting at the courthouse, meeting with the judge, and updating the file after the conference. In addition, once judicial case management has begun, a discovery cutoff date has usually been established, and attorneys may feel an obligation to begin discovery. Doing so could shorten time to disposition, but it may also increase lawyer work hours on cases that were about to settle when the judge began early management.

Early management has no significant effect on lawyer satisfaction or views on fairness. Litigant data showed mixed results for satisfaction with early management, higher in the pre-CJRA sample and lower in the post-CJRA sample.

We also explored alternative definitions of “early” using time periods other than six months, with results similar to those reported here. This finding suggests that the *fact* of management adds to the lawyer work hours, not the “earliness” of the management. However, starting earlier than six months means that more cases would be managed because more cases are still open, so more cases would incur the predicted increase in lawyer work hours. Early management involves a tradeoff between shortened time to disposition and higher lawyer work hours.

In terms of predicting reduced time to disposition, setting a schedule for trial early was the most important component of early management. Including early setting of trial date as part of the early management package yields an additional reduction of 1.5 to two months in estimated time to disposition but no further significant change in lawyer work hours.

No other aspect of early judicial management had a statistically significant effect on time to disposition, cost of litigation, attorney satisfaction, or views of fairness.

Figures S.1 and S.2 graphically illustrate the effects of early judicial management and early schedule for trial on time to disposition for the cases in the 1992–93 sample. In Figure S.1, the “not early” line is higher than the “early” line for the first six months because the former category includes cases that close almost immediately, before the judge has a chance to manage them.

Discovery Management

Discovery management policies include the CJRA principles of early and ongoing judicial control of pretrial processes, exchanging information early without formal discovery, and requiring good-faith efforts to resolve discovery disputes before filing motions.

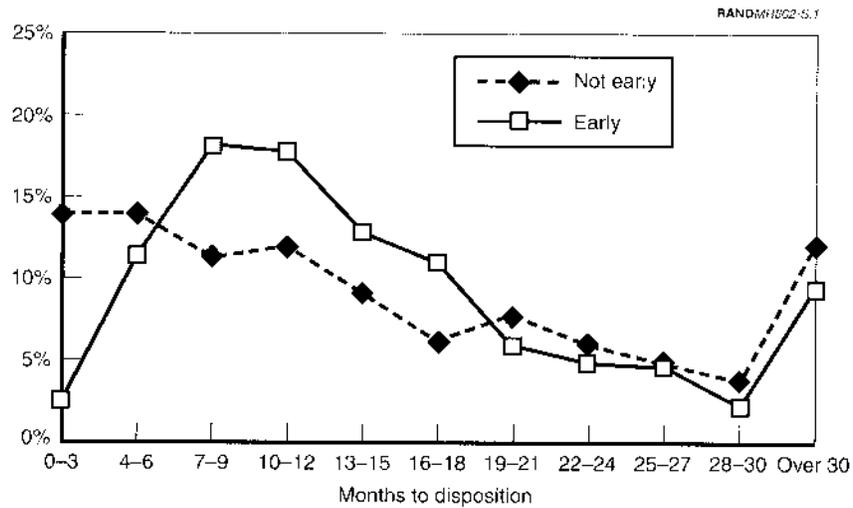


Figure S.1—Effects of Early Judicial Management on Time to Disposition: 1992-93 General Civil Cases with Issue Joined

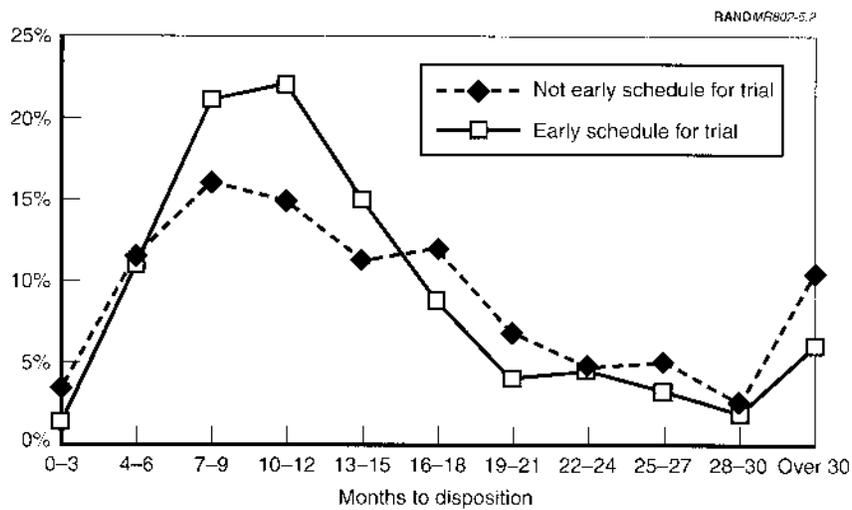


Figure S.2—Effects of Early Schedule for Trial on Time to Disposition: 1992-93 General Civil Cases with Issue Joined

Before CJRA, most districts left court control of the volume and timing of discovery to the judge in each case; CJRA had little effect on this arrangement. However, the median district times to discovery cutoff were lowered in some of the study districts. For example, in 1991 the fastest and slowest districts' median days from schedule to discovery cutoff were 100 and 274 days, respectively. In 1992-93, these medians had fallen to 83 and 217 days, respectively.

CJRA brought about substantial change in early disclosure. Only one district required it before CJRA; after CJRA, all pilot and comparison districts have adopted one of five approaches providing either voluntary or mandatory exchange of information by lawyers, sometimes only for specified types of cases.

All districts retained or strengthened their existing requirements that lawyers certify good-faith efforts to resolve discovery disputes.

Shorter time from setting a discovery schedule to discovery cutoff is associated with both significantly reduced time to disposition and significantly reduced lawyer work hours. If a district's median time to discovery cutoff is reduced from 180 days to 120 days, the estimated median time to disposition falls by about 1.5 months for cases that last at least nine months. In addition, lawyer work hours fall by about 17 hours—about 25 percent of their median work hours. These benefits are achieved without any significant change in attorney satisfaction or views of fairness. The data on litigant costs in terms of dollars and litigant hours spent appear consistent with the data on lawyer work hours. Litigant data also show little difference in satisfaction between shorter and longer times to discovery cutoff.

Neither mandatory nor voluntary early disclosure significantly affects time or costs. Furthermore, we found that cases from districts with a policy of mandatory disclosure of information bearing on *both* sides of the case did not differ significantly in terms of time to disposition from other cases. But the type of disclosure influences lawyer satisfaction. Lawyers are significantly less satisfied when a district has a policy of mandatory disclosure. However, they tend to be significantly more satisfied when they actually participate in early disclosure on *their* cases.

According to our analysis of dockets on more than 5,000 cases, and according to judges we have interviewed in pilot and comparison districts that implemented their plans in December 1991, motions regarding early mandatory disclosure of information are extremely rare. Despite the dire warnings of critics of early mandatory disclosure, we did not find any explosion of ancillary litigation and motion practice related to disclosure in any of the pilot or comparison districts using mandatory disclosure.

Alternative Dispute Resolution

All of the 20 pilot and comparison districts permit the use of ADR techniques in their CJRA plans. The three study districts that used mandatory arbitration before CJRA have continued to do so, and two of the three study districts authorized to use voluntary arbitration have started doing so. However, there has been a marked shift in half of the pilot districts toward other ADR programs—especially mandatory or voluntary mediation and early neutral evaluation—involving between 2 and 19 percent of all their civil case filings. And one district uses magistrate judge early neutral evaluation on 50 percent of its cases.

Some districts with structured programs have only 2 to 4 percent of their cases referred to ADR, so structure appears to be a necessary but not sufficient feature for a

volume ADR program. However, districts that permit ADR of some kind without a structured and administratively supported program have attracted few cases.

Our statistical analyses of cases referred to mandatory arbitration detected no major effect of arbitration on time to disposition, lawyer work hours, or lawyer satisfaction. The findings for views of fairness were inconclusive. However, the small sample of arbitration referrals allows us to detect only major effects, not more modest ones.

Neither lawyers nor judges have used any type of ADR extensively when its use is voluntary.

Using our primary sample data, we cannot statistically analyze the effects of the other types of ADR used in the districts. The volume of cases referred to ADR was too small to generate a large enough sample when all cases were sampled at random. And each of the various mediation and neutral evaluation programs was sufficiently different to make pooling the data problematic.

This CJRA evaluation has a separate supplemental ADR component in which we examine the mediation and early neutral evaluation programs in six of the study districts that use those ADR techniques for at least 5 percent of their civil filings. This analysis is reported separately.⁸

Magistrate Judges and Other Techniques

Of the four recommended techniques that were not discussed above, two could not be evaluated:

- None of the 20 districts required the signature of the attorney and the party on requests for discovery extensions or postponements of trial.
- Both before and after CJRA, all districts required, or allowed judges to require, that parties be represented at pretrial conferences by an attorney with authority to bind that party regarding all matters previously identified by the court for discussion at the conference and all reasonably related matters. Since there was no variation in policy between districts, we could not evaluate the policy's effect.

The technique of having litigants at or available for settlement conferences is a statistically significant predictor of reduced time to disposition. However, it has no significant effect on lawyer work hours or satisfaction and no consistent statistically significant effects on attorneys' views of fairness.

The last CJRA technique, "other features," was intended to give districts some latitude in their plans. One case management approach included here is the use of magistrate judges in the civil pretrial process.

Districts vary in the roles assigned to magistrate judges on civil cases. Virtually all districts' magistrate judges conduct felony preliminary proceedings and try misde-

⁸Kakalik et al. (1996b).

meanor and petty offense cases. In some districts, magistrate judges are also given felony pretrial duties, including motions, pretrial conferences, and evidentiary hearings. Prisoner cases are routinely referred to magistrate judges in many districts for pretrial management and the preparation of reports and recommendations.

With respect to other civil cases, magistrate judges conduct almost all civil pretrial proceedings in some courts, preparing the case for trial before the assigned district judge. In other courts, they are assigned duties in non-prisoner civil cases on a selective basis in accordance with the preferences of the assigning district judge. In addition, magistrate judges conduct jury and nonjury trials and dispose of civil cases with the consent of the litigants. In two of our study districts—CA(S) and NY(E)—magistrate judges actively manage all aspects of the pretrial process and usually make early attempts to settle cases. This style of case management differs markedly from the traditional approach used in most other districts before CJRA.

We found that *increased* magistrate judge activity on civil cases had no significant effect on time to disposition or on lawyer work hours, and no consistently significant effect on views of fairness associated with changing the level of magistrate judge activity. This *does not* mean that what magistrate judges *do* to manage cases has no significant effect. We believe that districts with higher levels of magistrate judge activity on civil cases are usually using them to conduct pretrial processing that usually would otherwise be conducted by a district judge. Hence, we believe our statistical findings mean that using magistrate judges *instead of district judges* to conduct pretrial civil case processing does not significantly affect time to disposition, lawyer work hours, or attorney views of fairness.

In the post-CJRA data, we find that increased magistrate judge activity on civil cases is a strong and statistically significant predictor of greater attorney satisfaction. Our interviews with lawyers suggest that one reason they are more satisfied with magistrate judges is that they find them more accessible than district judges.

These findings suggest that some magistrate judges may be substituted for district judges on non-dispositive pretrial activities without drawbacks and with an increase in lawyer satisfaction.

EFFECTS OF THE CJRA PILOT PROGRAM AS A PACKAGE

Did requiring pilot districts to adopt the package of broadly defined case management principles alter time to disposition, litigation costs, satisfaction, and views of fairness for civil cases?

We conclude that the CJRA pilot program, as the package was implemented, had little effect.

We base this assessment on statistical analysis of cases in pilot and comparison districts, on the results of judicial time studies, and on our survey of judges about how they managed cases before and after CJRA.

Statistical Analysis of Cases in Pilot and Comparison Districts

In 1991, before the pilot program was implemented, we detected no significant difference between pilot and comparison district cases in time to disposition, attorney work hours, attorney satisfaction, or views of fairness.

In 1992–93, after the pilot program was implemented but before eight of the comparison districts had implemented their CJRA plans, we still found no significant difference between pilot and comparison district cases in time to disposition, attorney work hours, attorney satisfaction, or views of fairness.

We believe there are at least four reasons why we did not see a significant difference between pilot and comparison districts after the pilot program was implemented.

- Some pilot districts' plans, as implemented, did not result in any major change in case management.
- Some pilot districts' plans that resulted in major change in management at the case level did not apply that change to a large percentage of cases within the district.
- Some changes that were more widely implemented (such as early mandatory disclosure of information) did not significantly affect time, cost, satisfaction, or views of fairness.
- Some case management practices that we have identified as significant predictors are implemented at the case level by judges, not at the district level, and there is much variation among judges in case management within both pilot and comparison districts.

Results of Judicial Time Study

One concern raised about implementing new case management policies is that benefits such as faster time to disposition may come at the cost of increased time spent by judicial officers. To see if the judicial case management principles and techniques of the Civil Justice Reform Act increased the amount of judicial time spent on civil cases, we conducted a "judicial time study" on the cases in our sample of 1992–93 civil filings and compared the results with data from the judicial time study conducted by the Federal Judicial Center in the late 1980s.

We found almost no difference in the time spent by judicial officers per civil case in 1992–93 when compared to 1989. The difference in the median time reported per civil case was only one minute; the difference in the mean was six minutes.

Survey of Judges About Case Management Approach

In the 1992–93 sample of cases, we surveyed judges after case closure and received over 3,000 responses. One question concerned the difference in case management before and after CJRA: "Was there a difference in how you and any other judicial of-

ficer managed this case, compared to how you would have managed it if it had been disposed of *prior* to January 1, 1992?”

The vast majority of the judges (85 percent in pilot districts, 92 percent in comparison districts) answered “no difference.”

Effect of Public Reporting?

Although the pilot program has had no significant effect on time to disposition, attorney work hours, satisfaction, or views of fairness, there is some evidence that another part of the CJRA may have affected the number of cases pending for three years or more in both pilot and comparison districts. The CJRA requires that “The Director of the Administrative Office of the United States Courts shall prepare a semiannual report, available to the public, that discloses for each judicial officer . . . the number and names of cases that have not been terminated within three years after filing.” Since public reports on each judge were required, the number of all civil cases pending has increased, but the number of cases pending over three years has dropped by about 25 percent from its pre-CJRA level. Nationwide, about 6 percent of all terminations (excluding asbestos cases) are more than three years old. In the pilot and comparison districts, the percentage of terminated cases more than three years old has drifted downward since the passage of the CJRA, from 6.8 percent in 1990 to 5.2 percent in 1995.

RESULTS OF EVALUATION OF JUDICIAL CASE MANAGEMENT

We conclude that the CJRA pilot program, as implemented, had little effect. But that finding does not imply that *case management* has no significant effect. Because case management varies across judges and districts, we were able to assess the effects of specific case management procedures and techniques on time, cost, satisfaction, and fairness. This assessment clearly shows that what judges do to manage cases matters.

Table S.1 summarizes the estimated effects of various case management principles and techniques. Those CJRA case management principles that we could not evaluate, because of the way in which the CJRA was implemented, may or may not affect costs and time.

Effects on Time to Disposition

Four case management procedures showed consistent statistically significant effects on time to disposition: (1) early judicial management; (2) setting the trial schedule early; (3) reducing time to discovery cutoff; and (4) having litigants at or available on the telephone for settlement conferences. For general civil cases with issue joined that do not close within the first nine months, we estimate that these procedures have the *combined effect* of reducing median time to disposition by about four to five months in our post-CJRA sample.

Table S.1
Summary of Statistical Evaluation of CJRA Principles and Techniques

Principle or Technique	Time to Disposition	Cost (Lawyer Work Hours)	Lawyer Satisfaction	Lawyer Perception of Fairness
Early judicial management of any type	S -	S +	0	0
Effect of including trial schedule set early as part of early management	S -	0	0	0
Effect of including pretrial conference as part of early management	0	0	0	0
Effect of including joint discovery/case management plan or status report as part of early management	0	0	0	0
Effect of including referral to mandatory arbitration as part of early management	0	0	0	0
Discovery: limiting interrogatories	0	0	0	0
Discovery: limiting depositions	NE	NE	NE	NE
Discovery: shortening time to cutoff	S -	S -	0	0
Mandatory early disclosure	0	0	S - district S + case	0
Voluntary early disclosure	0	0	0	0
Good-faith efforts before filing discovery motion	0	0	0	0
Litigants available at settlement conferences	S -	0	0	0
Increase use of magistrate judges to conduct civil pretrial case processing	0	0	S +	0
Track model of DCM	NE	NE	NE	NE
Complex case management	NE	NE	NE	NE
Party and lawyer sign continuance requests	NE	NE	NE	NE
Person with authority to bind at conferences	NE	NE	NE	NE

S + = significant increase; 0 = no significant effect; S - = significant decrease; NE = not evaluated (see the text for reasons).

Case management procedures have a substantial effect on predicted time to disposition. Of the total variance explained in our time to disposition analyses, only about half was explained by the case characteristics and other control variables; case management variables accounted for the rest.

Effects on Lawyer Work Hours

Of all the policy variables we investigated as possible predictors of reduced lawyer work hours, only judicial management of discovery was significantly associated with the desired effect. Cases from districts with shorter median time to discovery cutoff tend to require fewer lawyer hours; in contrast, cases with early management tend to require more.

Several attorney and case characteristics—especially case stakes and case complexity—explain much more of the variance in lawyer work hours than do the case management variables. This finding suggests that lawyer work hours are driven predominantly by factors other than case management policy. When time to disposition is cut, lawyers seem to do much the same work, but in a shorter time period.

We estimate that the combined effects of early management, setting the trial schedule early, and reducing time to discovery cutoff tend to offset their respective effects on lawyer work hours; consequently, we estimate that cases with all three policies will not differ much from cases that receive none of the policies—a reduction of only one hour in lawyer work hours.

Our findings that lawyer work hours appear to be driven predominantly by factors other than judicial case management policy and that only judicial management of discovery was significantly associated with reduced lawyer work hours suggest that procedural reform of the type specified in the CJRA may have a limited role to play in reducing litigation costs.

Effects on Satisfaction

In our explorations of attorney satisfaction, we find that early management, median days to discovery cutoff, and setting a trial schedule early in the case—the policies that had the greatest effects on predicted time to disposition and lawyer work hours—have no statistically significant effect on lawyer satisfaction. However, a higher degree of case management is associated with higher lawyer satisfaction.

The increased use of magistrate judges in civil pretrial management is associated with significantly increased attorney satisfaction. However, as noted above, we believe that whether magistrate judges or district judges conduct pretrial management activities does not significantly affect time to disposition, lawyer work hours, or attorney views of fairness. These findings suggest that some magistrate judges may be substituted for district judges on non-dispositive pretrial activities without significant drawbacks and with an increase in lawyer satisfaction.

Attorneys from cases where early disclosure occurs report greater satisfaction. However, attorneys from districts with a policy of requiring early disclosure for all cases were less likely to report satisfaction with case management. Lawyers apparently do not like blanket disclosure policies that apply to all cases, but they like the results when they participate in early disclosure on *their* cases.

Effects on Views of Fairness

We found no consistent statistically significant effects of case management on attorney views of fairness. Over 90 percent of attorneys report that case management was fair.

A PROMISING CASE MANAGEMENT PACKAGE

These findings suggest a package of case management policies with the potential to reduce time to disposition without changing costs, attorney satisfaction, and views of fairness. The package includes discovery control, the only CJRA case management practice that seemed to be effective in reducing costs:

If early case management and early setting of the trial schedule are combined with shortened time to discovery cutoff, the increase in lawyer work hours predicted by early management can be offset by the decrease in lawyer work hours predicted by judicial control of discovery. We estimate that under these circumstances, litigants in general civil cases that do not close within the first nine months would pay no significant cost penalty for a reduced time to disposition of approximately four to five months—about 30 percent of their current median time to disposition. And as we have seen, none of these policies has any significant effect on lawyers' satisfaction or perceptions of fairness.

Our analysis suggests that the following approach to early management of general civil litigation cases should be considered by courts and judges not currently using this approach and reemphasized by courts and judges that are using it. The powers to use this approach already exist under the Federal Rules of Civil Procedure.

- For cases that do not yet have issue joined, have a clerk monitor them to be sure deadlines for service and answer are met, and begin judicial action to dispose of the case if those deadlines are missed.
- For cases that have issue joined, wait a short time after the joinder date, perhaps a month, to see if the case terminates and then begin judicial case management.
- Include setting of a firm trial date as part of the early management package, and adhere to that date as much as possible.
- Include setting of a reasonably short discovery cutoff time tailored to the case as part of the early management package.

For nearly all general civil cases, this policy should cause judicial case management to begin within six months or less after case filing.

IMPLEMENTING CHANGE

Given our understanding of how the civil justice system operates, we believe that the package of case management policies has a high probability of reducing time to disposition if implemented, without negatively affecting litigation costs or attorney views of satisfaction and fairness. However, our estimated effect should be treated as an upper bound to the effects that could be anticipated if the policies were implemented more widely.

Our estimate is an upper bound rather than a precise estimate because our quantitative analyses exploited observational data on the naturally occurring variation in judges' management practices, rather than data resulting from an experimental ran-

dom assignment of management practices to cases. We believe we have accurately estimated the effect of a given management practice among districts and judges who currently use it. However, any effects we observe must be interpreted in light of the constraints imposed by observational data.

In particular, judges and districts *choose* to use certain case management policies and practices, and we must assume that these judges and districts may differ from other judges and districts who choose *not* to use the same policies and practices. For example, judges who currently use early management may use it with greater intensity or effectiveness than other judges who may be asked to start using it in the future. Judges who use early management now might be using it in combination with other practices for which we do not have data (such as settlement discussion during the initial case management conference) and which other judges may not choose to use. Also, judges who do not use a particular case management practice now may continue not using it even if they are asked to start using it in the future.

Thus, successful use of a case management procedure by some judges in some districts does not necessarily mean it will be equally effective if all judges are asked to use the procedure in all districts. However, the limitations of observational data notwithstanding, practices that we have identified as effective among judges who currently use them are good candidates for practices that could be beneficial if more widely implemented.

The judiciary's ability to ensure widespread implementation of these promising practices is the key to achieving the positive effects we observe. Effective implementation of new policies can be enhanced by examining why the CJRA pilot program had little effect and by learning from prior court and organization research on implementation of change.

Implementation factors that may have contributed to the pilot program's having little effect include: the vague wording of the act itself; the fact that some judges, lawyers, and others viewed the procedural innovations imposed by Congress as curtailing judicial independence accorded judges under Article III of the Constitution and as unduly emphasizing speed and efficiency at the possible expense of justice; and the lack of effective mechanisms for ensuring that the policies contained in district plans were carried out on an ongoing basis.

Prior research on implementation indicates that change is not something "done" to members of an organization; rather, it is something they participate in, experience, and shape. Studies of change in the courts and in other organizations provide some guidelines for improving implementation. They include: clearly articulating what the change is to accomplish and generating a perceived need for it; a governance structure and process that coordinates individuals' activities and assigns accountability for results; and meaningful performance measures to help both implementers and overseers gauge progress.

Studies of change also document that members of organizations are more likely to change their behavior when leadership and commitment to change are embedded in the system, appropriate education is provided about what the change entails, relative

performance is communicated across all parts of the organization, all supporting elements in the organization also make desired changes, and sufficient resources are available.

Future efforts to change the federal civil justice system could be substantially enhanced by incorporating such guidelines.

ACKNOWLEDGMENTS

We greatly appreciate the cooperation we have received from the Judicial Conference Committee on Court Administration and Case Management; from the Administrative Office of the United States Courts; from the Federal Judicial Center; and from the judges, clerks, advisory group members, and attorneys in the 20 pilot and comparison district courts. Without their cooperation and assistance, this study would not have been possible.

We are also indebted to the thousands of attorneys and litigants who responded to our surveys. They contributed a wealth of information that previously has not been available on litigation time, lawyer hours, costs, satisfaction with the court's management, and views on fairness.

Many people at RAND contributed to this five-year effort. Deborah Hensler, Director of RAND's Institute for Civil Justice, provided sound advice and strong support throughout the study. Dr. Hensler was technical advisor to the Brookings Task Force. RAND's Survey Research Group conducted our surveys and coded over 10,000 case dockets under the direction of Laural Hill; we especially thank Eva Feldman, Jo Levy, Don Solosan, and Tim Vernier for their extraordinary efforts and care. Data file design and analyses were conducted under the direction of Marian Oshiro, with great and timely assistance from Lori Parker and Deborah Wesley. Beth Benjamin helped to prepare the chapter on implementation of change in the court system. The insightful comments of RAND reviewers John Adams, Lloyd Dixon, and Peter Jacobson significantly improved the final report. Stephen B. Burbank, Paul D. Carrington, Loren Kieve, Robert S. Peck, Judith Resnik, Bill Wagner, and H. Thomas Wells, Jr., provided independent outside reviews and many useful comments on drafts of our report. We received excellent secretarial support from Rosa Morinaka and Sharon Welz, especially in dealing with calls from survey recipients. Finally, we appreciate the expert assistance of Patricia Bedrosian, who edited and oversaw production of the final version of this document.

RAND's Institute for Civil Justice evaluated the pilot program of the Civil Justice Reform Act of 1990 (CJRA), at the request of the Judicial Conference of the United States. The general objective of the evaluation was to identify effective approaches to cost and delay reduction for civil cases in federal district courts. The specific objective was to evaluate the implementation and effects of the CJRA case management principles and techniques in ten pilot and ten comparison districts.

This document describes the evaluation of the effects of the CJRA case management principles and techniques on time to case disposition, litigation costs, and participants' satisfaction and views of fairness in pilot and comparison districts. Separate RAND reports describe the implementation of the CJRA and our supplemental evaluation of alternative dispute resolution programs.¹

BACKGROUND TO THE LEGISLATION

Concerns that civil litigation costs too much and takes too long have been at the forefront of the civil justice reform debate for more than a decade.² Both federal and state courts are thought to be increasingly overburdened; as a consequence, according to the oft-heard indictment of the civil justice system, individuals are denied access to justice, and U.S. businesses pay too much, directly and indirectly, to resolve their disputes.

In the late 1980s, several groups, including the Federal Courts Study Committee³ and the Council on Competitiveness,⁴ began formulating reform proposals. One of these—the Task Force on Civil Justice Reform, which was initiated by Senator Joseph Biden and convened by The Brookings Institution—produced a set of recommendations that ultimately led to legislation.⁵

¹For details of each district's CJRA plan and its implementation, see Kakalik et al. (1996a). For our ADR evaluation see Kakalik et al. (1996b).

²See, for example, Chapper et al. (1984); The Brookings Institution (1989); The Federal Courts Study Committee (1990); and President's Council on Competitiveness (1991).

³The Federal Courts Study Committee (1990).

⁴President's Council on Competitiveness (1991).

⁵The Brookings Institution (1989).

In 1988, Senate Judiciary Committee Chairman Joseph Biden requested that the Foundation for Change and The Brookings Institution convene a task force of authorities to recommend ways to alleviate the excessive cost and delay attending litigation. The task force comprised leading litigators from the plaintiffs' and defense bar, civil and women's rights lawyers, attorneys representing consumer and environmental organizations, representatives of the insurance industry, general counsels of major corporations, former judges, and law professors. A separate survey of judges and lawyers conducted for The Foundation for Change bolstered the belief that the federal courts urgently needed reform. In its final report, the Brookings task force made extensive recommendations for expanding federal judicial resources and for instituting procedural reform.⁶

The Federal Courts Study Committee, appointed by the Chief Justice at the behest of Congress, also began work in 1988 on a 15-month study of the problems facing the federal courts.⁷ Rather than focusing on changes in the substantive law, the committee explored institutional and managerial solutions. Specifically, the committee recommended reallocating cases between the state and federal systems, creating non-judicial branch forums for business currently in the federal courts, expanding the capacity of the judicial system, dealing with the appellate caseload, reforming sentencing procedures, protecting against judicial bias and discrimination, improving federal court administration, reducing the complexity of litigation, and expediting the movement of cases through the system. To achieve the last objective, the committee recommended sustained experimentation with alternative and supplemental dispute resolution techniques. To control the pace and cost of litigation, it also encouraged early judicial involvement, phased discovery, the use of locally developed case management plans, and additional training of judges in techniques of case management.

Concurrently, President Bush created a Council on Competitiveness to propose reforms, although its formal report was not issued until after Congress enacted the Civil Justice Reform Act. That report⁸ recommended reforming expert evidence procedures, creating incentives to reduce litigation, reducing unnecessary burdens on federal courts, eliminating litigation caused by poorly drafted legislation, reducing punitive damage awards, improving the use of judicial resources through efficient case management techniques, streamlining discovery, making trials more efficient, and increasing the use of voluntary alternative dispute resolution programs.

Whatever their other differences, the studies by each branch of government stood united in their emphasis on case management techniques and procedural reform. In the end, it was The Brookings Institution report, derived from initiatives largely sponsored by Senator Biden, that detailed many of these procedural and managerial reforms and in time formed the blueprint for draft legislation. Its goal, in brief, was

⁶The Brookings Institution (1989).

⁷The Federal Courts Study Committee (1990).

⁸President's Council on Competitiveness (1991).

to prompt the federal courts to impose rules and procedures on themselves and on lawyers that would ameliorate the perceived twin problems of cost and delay.

The ensuing debate about the draft legislation was energetic, to say the least. It resulted in a compromise under which the main themes of procedural reform were sustained but some of the detailed statutory controls contemplated by the Brookings task force were deleted. Replacing them was an agreement that each district court would accept the responsibility for developing a cost and delay reduction plan tailored to its own needs.

The new legislation, the CJRA, required each federal district court to conduct a self-study with the aid of an advisory group, and to develop a plan for civil case management to reduce costs and delay. It created a pilot program requiring ten districts to incorporate six principles of pretrial case management into their plans and to consider incorporating six other case management techniques. Ten other districts, although they were left free to develop their own plans that did not have to contain any of the CJRA principles or techniques, were included in the program to permit comparisons.

To generate reliable information about the effects of the case management principles and techniques, Congress provided for an independent evaluation of the activities in these 20 pilot and comparison districts. The Judicial Conference and the Administrative Office of the U.S. Courts asked RAND's Institute for Civil Justice to evaluate the implementation and the effects of the CJRA in these 20 districts.

This document is the main report of that evaluation.

OVERVIEW OF THE CJRA PILOT PROGRAM

The ten pilot districts selected by the Committee on Court Administration and Case Management of the Judicial Conference of the United States were: California (S), Delaware, Georgia (N), New York (S), Oklahoma (W), Pennsylvania (E), Tennessee (W), Texas (S), Utah, and Wisconsin (E).

The Judicial Conference, in consultation with RAND, selected the following ten comparison districts: Arizona, California (C), Florida (N), Illinois (N), Indiana (N), Kentucky (E), Kentucky (W), Maryland, New York (E), and Pennsylvania (M).

The pilot districts were required to implement their plans by January 1992; the other 84 districts, including the comparison districts, could implement their plans any time before December 1993.

The Six Case Management Principles

The act directs each pilot district to incorporate the following principles into its plan:

1. Systematic, differential case management tailored to the characteristics of different categories of cases (the act specifies several factors such as case complexity that may be used to categorize cases);

2. Early and ongoing control of the pretrial process through involvement of a judicial officer in assessing and planning the progress of the case, setting an early and firm trial date, controlling the extent and timing of discovery, and setting timelines for motions and their disposition;
3. For complex and other appropriate cases, judicial case monitoring and management through one or more discovery and case management conferences (the act specifies several detailed case management policies, such as scheduling and limiting discovery);
4. Encouragement of cost-effective discovery through voluntary exchanges and cooperative discovery devices;
5. Prohibition of discovery motions until the parties have made a reasonable, good-faith effort to resolve the discovery dispute; and
6. Referral of appropriate cases to alternative dispute resolution programs.

Pilot districts *must* incorporate these principles, while other districts *may* do so.

The Six Case Management Techniques

The act directs each pilot district to *consider* incorporating the following techniques into its plan, but no district is *required* to incorporate them:

1. Require that counsel jointly present a discovery/case management plan at the initial pretrial conference, or explain the reasons for their failure to do so;
2. Require that each party be represented at pretrial conferences by an attorney with authority to bind that party regarding all matters previously identified by the court for discussion at the conference and all reasonably related matters;
3. Require the signature of the attorney and the party on all requests for discovery extensions or postponements of trial;
4. Offer an early neutral evaluation program;
5. Require party representatives with authority to bind to be present or available by telephone at settlement conferences; and
6. Incorporate such other features as the district court considers appropriate.

PREVIEW OF FINDINGS

The ICJ evaluation was designed to provide a quantitative and qualitative basis for assessing how the adoption of these management principles and techniques in the pilot and comparison districts affected time to disposition, costs to litigants, and participants' satisfaction and views of fairness.

To preview the key findings of the evaluation:

The pilot program, as implemented, had little effect on time to disposition, costs, and attorneys' satisfaction and views of fairness. However, case management approaches varied widely across districts and judges. Our analysis of those approaches identified a package of policies with the potential to reduce time to disposition without increasing costs or affecting satisfaction or views of fairness. The only CJRA case management practice that seemed to be effective in reducing the cost of litigation was discovery control.

OUTLINE OF THIS REPORT

This report has two dimensions, designed for different audiences. The main text focuses on policy evaluation and is intended for policymakers and policy-users. The extensive appendices, which focus on the details of our surveys and statistical analyses behind the policy evaluation, are intended for staff members and others who want to know methodological and empirical details of how we reached our conclusions.

The discussion is organized as follows. In Chapter Two we briefly review the study's objectives, methods, and data sources. Chapter Three summarizes the findings of our separate report on how the pilot program was implemented and discusses the challenge of implementing change in the federal courts. In Chapters Four through Eight, we present our findings on the effects of different CJRA case management policies and procedures: differential case management, early judicial case management, discovery management, alternative dispute resolution, use of magistrate judges, and other case management policies. Our assessment of the pilot program as a package is described in Chapter Nine. The last chapter summarizes our conclusions.

EVALUATION DESIGN AND INTERPRETATION OF FINDINGS

OVERVIEW OF THE RAND EVALUATION DESIGN

The CJRA mandates that ten pilot districts adopt “civil justice expense and delay reduction plans,” for the purpose of facilitating deliberate adjudication of civil cases on the merits, monitoring discovery, improving litigation management, and ensuring just, speedy, and inexpensive resolution of civil disputes.¹ At the outset of this study, RAND in consultation with the Judicial Conference decided that the evaluation should include not only litigation cost and time to disposition, but also “justice” as measured by participants’ satisfaction with the process and views of the fairness of the process.²

This five-year evaluation ends in December 1996³ with a report to the Judicial Conference of the United States, the governing body of the federal court system. The Judicial Conference is to submit a report to Congress documenting the evaluation’s results and including the conference’s recommendations about which of the CJRA pilot program principles, or alternative and more effective programs, should be adopted in the federal district court system.

The CJRA did not require experimental random assignment of case management policies and procedures to districts or to cases. Rather, the act assigned ten districts to a pilot program, and the pilot program required that these districts adopt plans to implement policies consistent with the principles of the legislation. The CJRA influenced the district’s choice of policies, but the districts chose the policies we observed. Similarly, the judges in our study districts responded to district policies according to their own beliefs about case management and their understanding of the

¹28 U.S.C. §471.

²We believe that participants’ satisfaction and views of fairness are the best available measures of “justice.” We also have data on the litigants’ opinion of whether they won or lost or had a mixed result, as well as data on the monetary and nonmonetary outcomes as reported by the lawyers and litigants. Because less than 100 percent of the lawyers and litigants we surveyed responded, it is difficult to aggregate these data from the level of the individual lawyer and litigant to the case level. Although outcome data are important in their own right and outcome data collected for this CJRA study are a rich source of information for possible future research, monetary outcomes are not a focus of this CJRA evaluation.

³The CJRA originally called for the pilot program to end in December 1994, with a report due to Congress in December 1995. These dates were extended for one additional year by the Judicial Amendments Act of 1994, Public Law No. 103-420.

requirements of individual cases. Together, the district policies and the judicial discretion create substantial variation in case management policies within each district and among districts.

This evaluation is designed to provide a quantitative and qualitative basis for assessing how the case management principles and techniques identified in the CJRA affect costs to litigants, time to disposition, participants' satisfaction with the process, views of fairness of the process, and judge work time required.

Comparisons are made between the ten pilot and ten comparison districts using data from cases terminated in 1991 before CJRA, and separately using data from cases filed in 1992–93 after implementation of the pilot program plans. Because there are differences between our pre-CJRA and post-CJRA data unrelated to CJRA⁴ and difficult to properly account for, we focus on separate pre- and post-CJRA analyses. The results of our qualitative analysis combined with our separate pre- and post-CJRA quantitative analyses provide ample evidence on the effects of the act.

The evaluation also uses quantitative analyses to compare cases managed in different ways to determine the effect of such management practices on costs to litigants, time to disposition, participants' satisfaction with the process, and views of fairness. The quantitative analyses exploit natural variation in judges' management practices, rather than an experimental random assignment of management practices to cases, to explore the effects of management practices.

Any effects we observe must be interpreted in light of the constraints of observational data. In particular, judges and districts choose to use certain case management policies and practices, and we must assume that these judges and districts could differ from other judges and districts that choose not to use the same policies and practices. Because of these potential differences between judges and districts, our observed effects of a particular type of case management should be treated as an upper bound to what might occur if a decision were made to ask other judges and districts to implement that particular type of case management.

This study is concerned with evaluation of case management policies and procedures and not with evaluation of individual district performance under CJRA. The evaluation assesses differences in time to disposition, litigation costs, satisfaction, and views of fairness at the case level, not at the individual district level. The effects of district policies, as well as individual case management procedures, are taken into account using statistical models to explore the variation in effects between cases (or parties from those cases).

Evaluating *case-level* effects rather than aggregated district statistics is advantageous because we can estimate effects while controlling for the many case-specific factors,

⁴See Appendix C for a discussion of trends in the federal court system over the last decade.

such as case type, complexity, and stakes, as well as for the actual management procedures used on each individual case.⁵

Although the act required pilot districts to develop and implement case management plans that included six principles of case management, it did not require that comparison districts remain static during the entire study period. Comparison districts had the obligation to adopt their own CJRA plans, although with freedom of choice as to content and on a different implementation schedule from that established for pilot districts. The act required pilot districts to institute plans by January 1992; comparison districts could implement any time before December 1993. We selected our sample of cases for intensive study beginning in mid-1992 and we finished sample selection in nearly all districts by mid-1993. Since eight of the ten comparison districts did not implement their CJRA plans until December 1993, they effectively retained their status quo policies during the early months of the lives of our sample of cases.⁶

After our sample was selected, the December 1993 amendments to the Federal Rules of Civil Procedure made some changes in pretrial procedures. Some of these changes were similar to rules implemented earlier by pilot districts; some were not. Districts were allowed to opt out of some of the rule changes.⁷ By December 1993, our sample cases were usually at least several months old and beyond the age when, for example, the revised Rule 26 requiring early disclosure of information would have been applied.

In the remainder of this chapter, we describe the data we collected to allow us to identify the particular case management procedures and case-level measures used in our analyses. We also discuss the representativeness and comparability of the pilot and comparison districts. Finally, we discuss what our study approach implies for the interpretation of our findings and the adoption of promising case management policies and procedures by other judges in other districts.

DATA SOURCES

To conduct the evaluation, we collected a vast amount of subjective and objective case-level data from a wide variety of sources. Data are drawn from January 1991 through December 1995. Data sources include:

⁵See Appendix I for an annotated bibliography of major recent studies. Prior research usually relied upon existing data collected by courts rather than on detailed case-specific information about the case management practices that individual judges and courts actually use on individual cases.

⁶Ideally, we would have liked comparison districts to be excused from complying with CJRA requirements so that they could maintain their prior policies for the duration of the evaluation. However, that would have required revising the CJRA, and this proved to be infeasible. For the reason cited in the text, this turned out to be a smaller problem than originally anticipated.

⁷See Stienstra (1995).

- Court records, including summary data on every civil and criminal case filed from 1971 through 1995,⁸ trial/bench time data files for 1986 through 1995,⁹ magistrate judge monthly work data for 1986 through 1995,¹⁰ and information on vacant judgeship months, magistrate judge positions, and senior judges from 1986 through 1995;
- Records, reports, and surveys of CJRA advisory groups;
- Pilot and comparison districts' cost and delay reduction plans;
- Detailed case processing and complete docket information on our samples of cases;¹¹
- Surveys of judicial officers on more than 5,000 cases, and reports on their activities, time expenditures, and views of CJRA;
- Mail surveys of attorneys and litigants about costs, time, satisfaction, and views of the fairness of the process; and
- Interviews in person with judges, court staff, and lawyers during at least three site visits to each of the 20 districts.

We selected a stratified random sample of about 250 cases from each time period and for each district for intensive study for a total of approximately 10,000 cases.¹² To collect information on case costs and on the perceptions of lawyers and litigants on both sides, we attempted to survey the lawyers and litigants in all of the cases selected for the sample—a total of about 60,000 persons.

Data collection began in 1992 when the 5,000-case pre-CJRA sample was drawn from cases that terminated in the last half of 1991. Selection of the 5,000 post-CJRA cases began with cases filed in late 1992 and early 1993. It did not begin earlier because, although each of the pilot districts met the January 1, 1992, deadline, the full implementation of plan provisions did not occur in most districts until well into 1992. Our sample selection of filed cases was done after the pilot district plan had been in place for at least six months, so that we were past the initial startup phase of the plan.

We followed the 5,000+ cases filed after the CJRA became law until December 1995, as long as the Congressionally established reporting deadlines permitted. At the end of 1995, 93 percent of the main post-CJRA sample cases were closed, and only 7 percent were still open. After a case concluded and the period allowed for appeal ex-

⁸The Integrated Federal Courts Data Base, developed by the Administrative Office of the United States Courts and the Federal Judicial Center, contains summary information on each civil and criminal case based on forms submitted by each district at the time of case filing and again at the time of case disposition.

⁹JS-10 trial reports contain information on each trial based on forms submitted by each district.

¹⁰JS-43 reports are submitted monthly by each magistrate judge.

¹¹Most docket information came from the Integrated Case Management System (ICMS), a computerized database maintained in each district containing detailed information about each case, including full docket information. This system was still being implemented by some districts in the early stages of our evaluation. In those districts, we also used paper records such as docket sheets and staffing level reports.

¹²We also conducted a supplemental alternative dispute resolution study which involved similar surveys on 1,823 additional cases. For details, see Kafalik et al. (1996b).

pired, surveys were sent to judges, lawyers, and litigants. For open cases, we also surveyed the lawyers in early 1996. Data were collected for about a three-year period because, although the median time to disposition for civil cases is less than a year, a number of cases remain in the courts much longer.

Table 2.1 shows the number surveyed, the number of responses received, and the percentage of completed surveys for closed cases.¹³ About two-thirds of the judges, one-half of the lawyers, and one-eighth of the litigants responded to the surveys.¹⁴ This completion rate for litigants is low in part because of the difficulty in finding the right person in an organization to send the survey to and in part because of the existence of litigants who were named on the case but who had very little involvement because they were not principal litigants. Appendix B provides details of the completion rates by survey and by district.

Table 2.1
Sample Size and Responses to Surveys

Survey Type	Sample Size	Responses	Percent Complete for Closed Cases
Case dockets			
1991	5,149	5,149	100
1992-93	5,222	5,222	100
Judges ^a			
1992-93	5,222	3,280	67
Lawyers			
1991	9,777	4,870	50
1992-93	9,423	4,061	47
Litigants			
1991	19,949	2,824	14
1992-93	20,272	2,264	13

NOTE: See Appendices A and B for details.

^aJudges on cases terminated in 1991 were not surveyed.

MAJOR EVALUATION MEASURES

Time to Disposition

We chose to analyze “time to disposition” rather than “delay,” since delay is a subjective term that cannot be objectively defined without reference to some currently unavailable standard of how long civil cases *should* take to resolve. Time to disposition is defined as the interval from first filing of the case to final closing in the study district court, an objective term that can be accurately measured for every case. Since reducing time to disposition will probably reduce delay, regardless of how delay is defined, this approach will, by inference, allow us to predict the effects of case management on delay.

¹³Litigants are more numerous than lawyers because some lawyers represent more than one litigant, and some litigants have no identified lawyers (not only pro se litigants but litigants whose cases close before they hire a lawyer or before the court is notified of the lawyer’s name).

¹⁴See Appendices A and B for detailed sample selection and response information.

Litigant Costs and Judicial Resource Requirements

We analyze “cost” rather than “excess cost,” since excess cost is a subjective term that cannot be objectively defined without reference to some currently unavailable standard of how much civil cases should cost to resolve. The issue of concern is how the cost of litigation to litigants is affected by the type of management practiced by the court.

We focus on two types of costs: those borne by litigants and those borne by the federal court system. The former are defined in this study in both monetary and work hour terms, and the latter are defined in terms of judicial work hours only. Litigant costs include attorney work hours and fees—whether contingent or hourly or determined by some other billing method—direct dollar expenditures for expert witnesses, filing fees, copying services and the like, and litigant time spent working on the case.

Lawyer work hours are our most informative measure of litigation costs. We define lawyer work hours as all hours spent by attorneys on the case before and after filing, excluding time spent on related state cases, government administrative proceedings, and appeals. We use this as our measure of costs in the statistical analyses because it has uniform meaning regardless of attorney fee structure¹⁵ or geographic variations in attorney fee rates. Also, attorneys provided information on work hours a little more often than they provided information on their fees charged.¹⁶

Actual dollars spent on the case are affected by both fee structure and variations in attorney fee rates. Contingent fee arrangements, fixed fees, and prepaid legal insurance fees create the possibility that fees paid do not correspond to attorney workload related to policy variables or case characteristics. Also there is considerable district-to-district variation in average attorney fees that would bias estimates of policy effects on dollar costs if the interdistrict fee rate variation was confounded with policy differences.

Using lawyer work hours reflects the cost of the lawyers (both in-house lawyers and outside counsel) but does not capture expenses such as expert witness fees, travel costs, court and transcript fees, and the costs of investigators. However, our survey of lawyers shows that those expenses constitute only 10 percent of the total combined cost of the lawyers’ time plus expenses.

Using only lawyer work hours in our statistical analyses underestimates costs because it does not include non-lawyer time spent by the litigants. In subsequent chapters of this report, we present some tabular descriptive information on non-lawyer litigant hours spent. Those tables suggest that the number of litigant hours spent is related to the number of lawyer hours spent, and generally moves in a direction consistent with our findings on policy effects obtained from analysis of lawyer hours. However, due to the low litigant response rate and possibly biased re-

¹⁵Under some fee structures, such as contingent fees, changes in lawyer work hours that may result from changes in court management are not necessarily reflected in the fees charged to clients.

¹⁶Refer to Appendix J for the survey questions about lawyer hours and dollar costs.

sponses, as discussed in the “methods” subsection later in this chapter and in Appendix B, we did not conduct inferential statistical analyses using litigant cost data.

Because the value of money changes over time with inflation, we convert all expenditures to constant 1995 value dollars. We used the Consumer Price Index for all Urban Consumers (CPI-U) to adjust the expenditures for all cases.¹⁷ To have a meaningful comparison between legal fees charged by outside counsel and the costs of in-house counsel, we used the *1994 Law Department Salary Survey*¹⁸ and the *Law Department Function and Expenditure Report, 1994 Survey*,¹⁹ by Altman Weil Pensa, Inc., to estimate factors for the costs of fringe benefits and overhead as functions of legal staff salaries for in-house legal departments.

¹⁷As of December 1995, CPI-U data were available through October 1995 from the U.S. Bureau of Labor Statistics (1995). There is a delay in the release of the CPI-U and numbers were not available for the last few months of our study. For these months we estimated the CPI-U using the recent annual rate of inflation of 2.5 percent. Although we were dealing specifically with the monetary outcome of litigation, plus legal fees and related costs, we chose to use a general price index to adjust to constant value dollars rather than an index based only on legal expenditures. Our rationale for this choice is twofold. First, we believe that for the monetary outcome of litigation, and for contingent fees that are a percentage of the monetary outcome, the inflationary effects are best represented by changes in a general price index. Second, we do not believe that any legal price index can successfully capture differences in the price of equal quality legal services. In other words, if one looks only at the cost per hour of legal service, without adjusting for changes over time in the quality of that hour of legal services, then the index displays a combination of changes in price per hour and changes in quality of service per hour of work. Because of changing fee structures and competitive pressures, we felt that the average hour of attorney time spent in 1995 might be of different quality than it was in earlier years such as 1991. If this is so, then changes in the average attorney fee per hour represent both changes in quality as well as changes in the per hour price of equal quality time. We felt that adjusting by a general price index would capture the inflationary effects on legal fees while avoiding changes in quality that could be implicit in legal price indices. To the extent that changes in the true price of equal quality legal time differ from changes in the CPI-U, then changes we see after converting to constant 1995 value dollars will reflect changes in both legal prices and legal service quality.

¹⁸Altman Weil Pensa, Inc. (1993a). The reports use data from a sample of 184 law departments from employers in 15 industries, including finance, manufacturing, retail, banking, and government. The departments range in size from two lawyers to over 40 lawyers and were selected from all regions of the country. The *1994 Law Department Salary Survey* provided a table with fringe benefits as a percentage of payroll for all surveyed departments. The percentage was given as a range (e.g., 10–14.9, 15–19.9) and the number of firms with costs in this range was also included in the table. Using the midpoint of the range for the costs for each department, we found that the average was 30 percent for all departments.

¹⁹Altman Weil Pensa, Inc. (1993b). This provided information on the total in-house expenditure for departments by size (2–10, 11–25, 26–40, or 41 or more lawyers). The *1994 Law Department Salary Survey* provided estimates of the average total compensation expense for departments in the same categories. Dividing average total expenditures by average compensation expenditures (including fringe benefits) yielded our estimate of the overhead rate for each category. We used a weighted average of the four rates to produce our final estimate of a 78 percent overhead rate. The overhead rate was roughly constant for the in-house legal departments with 2–10, 11–25, or 26–40 lawyers. The weighted average of these rates was 62 percent. The rate for large departments (over 40 lawyers) was higher. Including the data from these large firms gives us an estimated overhead rate of 78 percent. Although the sample of large departments was small and the variation of expenditures within this group was large, we decided to include these data in our estimate of the overall overhead rate to avoid biasing our estimate in favor of small firms. We compared our estimate for in-house legal departments with an estimate of the overhead rate for law firms. We found that average total expenses, not including compensation and fringe benefits, were 44 percent of total income for law firms. (See Altman Weil Pensa, Inc., 1995, for details.) Thus, we have an overhead rate of 79 percent for law firms, which is similar to the 78 percent we estimated for in-house legal departments. This does not ensure that our estimated rate for in-house legal departments is precisely accurate, but it does provide confirmatory evidence that it is a reasonable estimate. For litigation involving in-house attorneys (for government or private organizations), we estimate the cost of an hour of attorney time as salary per hour, plus 30 percent of salary for fringe benefits, plus 78 percent of salary and fringe benefits for overhead.

We also analyze court costs in terms of judicial time spent. The premise of CJRA is that more active, early management by the court is likely to be better management, and the act explicitly calls for increased court involvement in the early stages of civil litigation. It seems inevitable that this will require more time and effort by judges, magistrate judges, and clerical staff early in the life of a case. The question is whether this early judicial effort is offset by a reduction in the level of judicial effort required later in the case.²⁰

To develop some information pertaining to these issues, an agreement was reached between the Judicial Conference, the Federal Judicial Center, and the 20 participating districts to continue the judicial time survey that the center has been conducting in recent years. Begun in 1989 with a nationwide data collection effort, that survey involves the development of “case weights” for the district court system. These case weights show the relative amount of time district judges spend on different types of cases. During the study, both district judges and magistrate judges reported how much time they spent on each case—from filing to final disposition. We use an identical methodology for 5,000 of the cases selected for the CJRA evaluation. The Federal Judicial Center assisted in administering and managing the extended survey. In addition, with the agreement of the judiciary, the FJC provided its earlier data to RAND to assist in establishing baseline measures for the CJRA evaluation.²¹

We note that we use the number of lawyers in the legal firm or legal department as a variable in our statistical analyses. However, we did not do a separate in-depth analysis of cases that involved solo practitioners or lawyers from very small legal firms. The potential differential effect of case management policy on litigants represented by those solo practitioners or lawyers from very small firms is an important issue because they may not be able to effectively comply with judicial case management orders in as timely a fashion as larger firms and legal departments. This is an important question, and data collected for this CJRA study are a rich source of information for possible future research into this issue.

We also use the type of lawyer fee structure (such as contingent or hourly fee) as a variable in our statistical analyses. However, we did not do a separate in-depth analysis of cases that involved a lawyer paid by contingent fee. If case management policies and procedures were to increase the amount of work time required of contingent fee lawyers, this has the possibility of lessening access to the justice system for litigants with marginal claims. This is an important issue, and data collected for this CJRA study could be used as one source of information for possible future research into this area.

Finally, we note that we use total lawyer work hours in our analyses, as a measure of the total litigation costs. In addition to the total hours, we collected the components of lawyer work hours, as a method of helping improve the accuracy of the lawyers’ responses (see Appendix J, especially question 9). Hence, we have data on lawyer time

²⁰Such a later reduction might occur if the case closes earlier, or fewer motions are filed, or fewer cases go to trial.

²¹Appendix H contains a detailed discussion of this judicial time study.

spent for each sample case on trial, ADR, discovery, motion practice, conferences, and other time spent both before and after filing. Those components of total lawyer work hours are a rich source of information for possible future research into the components of the cost of litigation. We did not collect data on when in the life of the case the lawyer hours were expended.

Satisfaction with Case Management and Views of Its Fairness

Increasing the judicial management of civil litigation is not universally accepted as the most appropriate way to deal with perceived cost and time to disposition problems. Some commentators are concerned about negative effects if judges try to push cases through the courts faster and exert pressure on litigants to settle early. They argue that litigants and their lawyers are the best judges of the speed at which a case should move because only they have the knowledge about the case that is necessary to determine the most suitable pace. With respect to settlement, the argument is made that if the parties to a case wish to settle, they will do so. If they do not settle, the court should not pressure them. Such pressure, it is said, may favor one side or the other.

We do not propose to provide a definitive philosophical or doctrinal answer to these difficult questions. But to date, the debate about managerial judging has lacked empirical information about how variation in case management actually affects participants' satisfaction with case management and their views of the fairness of case management. Our survey responses provide some information on this complex issue.

REPRESENTATIVENESS AND COMPARABILITY OF PILOT AND COMPARISON DISTRICTS

Ideally, the pilot and comparison districts would be similar in every respect except case management policies. However, since these policies were not known at the time the pilot and comparison districts were selected, the Judicial Conference used factors for which comparable data were available, such as district size, workload per judge, the number of criminal and civil filings, and the time to disposition in civil cases, to select the comparison districts. Factors such as the cost of litigation, participant satisfaction, and views of fairness could not be used because data were not available.

Together, the 20 study districts have about one-third of all federal judges and one-third of all federal case filings. However, since the program involves only 20 of the 94 federal districts, the representativeness of the pilot and comparison districts becomes critical. Obviously, the more representative they are, the greater the likelihood that they will yield valid generalizations about the system as a whole. A second concern is whether the ten pilot and ten comparison districts are sufficiently similar

to be considered comparable. Using data from Statistical Year 1990²²—the year used to select the comparison districts—we present the characteristics of the two groups in Table 2.2.

We note that although the table shows one pilot district paired with one comparison district, the pairing was done for selection of the comparison districts only. We did not conduct our pilot program evaluation and analysis by directly comparing one pilot district with one comparison district.

Because no two federal districts are identical, perfect representativeness and comparability are ideals that are difficult to achieve. But having considered three important types of characteristics in selecting the districts—judicial resources, number of filings, and time to disposition—we found considerable similarity and representativeness.

First, consider the number of authorized judges in each of the pilot and comparison districts in 1990. The two groups had 193 authorized positions in 1990—about one-third of the 575 judgeships authorized for the entire district court system in that year.²³ There is a roughly even split between the pilot and comparison groups with respect to both the total number of positions and the variation in size between the districts in each group. The four largest districts in the federal court system are participants in the program (two pilot, two comparison, each with 19 or more positions), and smaller districts (fewer than five judges) are also represented.

On the dimension of workloads, as measured by the total number of civil and criminal cases filed in 1990, the pilot and comparison districts also look comparable to each other and to the system as a whole. The number of filings nationally was about 251,000 in FY90, of which about 32,000 were felony criminal cases. The study districts contained about one-third of the total filings, split roughly equally between pilot and comparison districts. Again, some of the largest and smallest districts are found in both the pilot and comparison groups. Using other workload measures—just civil filings, just criminal filings, case mixture, or filings per judgeship—the picture looks much the same.

Finally, since this study concerns itself with time to disposition, among other factors, we consider the median time to dispose of civil cases. Figure 2.1 shows the ten pilot districts on the left, and the ten comparison districts on the right. The median was nine months nationally in 1990 and was about the same for both pilot and comparison groups. But also note the wide variation among the 20 districts, ranging from a low of five months to a high of 14. This provides a range that is representative of the differing times to disposition in all federal districts. Using other statistics yields similar results. For example, about 10.6 percent of the civil cases pending nationally in

²²July 1, 1989, through June 30, 1990.

²³The CJRA of 1990 increased the number of authorized judgeships to 649. Note that there are always some authorized judgeships unfilled because of the length of time consumed in the selection and confirmation process. For example, of the 649 authorized in FY92, 109 positions remained open nationwide.

Table 2.2
Pilot and Comparison Districts for the CJRA Pilot Program

Pilot and Comparison Districts	District	Number of Judgeships	Weighted Filings ^a			Unweighted Cases per Judgeship			Median Civil Time Intervals (months)		
			Total	Per Judgeship	Civil Filings	Felony Criminal Filings	Pending Cases	Filing to Dispose	Issue to Trial ^b	% of Civil Cases over 3 Yrs Old	
Pilot	CA(S)	7	3,080	440	275	131	591	12	18	12.7	
Comparison	AZ	8	3,296	412	358	104	538	9	20	11.5	
Pilot	DE	4	924	231	195	28	249	10	17	8.6	
Comparison	FL(N)	3	984	328	358	70	454	9	23	7.3	
Pilot	GA(N)	11	4,169	379	312	35	330	10	19	4.0	
Comparison	MD	10	4,000	400	350	38	378	9	11	10.2	
Pilot	NY(S)	27	11,043	409	325	29	505	9	19	12.8	
Comparison	IL(N)	21	10,248	488	380	36	346	5	12	11.6	
Pilot	OK(W)	5	2,220	444	458	49	287	7	11	3.2	
Comparison	PA(M)	5	2,305	461	447	53	300	8	10	5.3	
Pilot	PA(F)	19	12,122	638	488	26	537	7	12	2.1	
Comparison	CA(C)	22	10,714	487	401	48	471	7	12	8.6	
Pilot	TN(W)	4	1,408	352	325	78	514	14	30	14.5	
Comparison	KY(W)	4.5	1,503	334	361	35	442	13	19	11.7	
Pilot	TX(S)	13	7,631	587	460	181	816	11	23	13.2	
Comparison	NY(E)	12	5,940	495	369	80	589	9	19	13.1	
Pilot	UT	4	1,620	405	310	60	481	11	14	12.3	
Comparison	IN(N)	5	1,530	306	277	41	325	12	15	12.0	
Pilot	WI(E)	4	1,684	421	369	61	386	7	20	6.0	
Comparison	KY(E)	4.5	1,606	357	385	48	389	8	18	5.0	

NOTE: Data used to select districts are from Statistical Year 1990.

^aCase weights, approved by the Judicial Conference of the United States, give the relative amount of judicial work time per case of each type.

^bMonths from the point in the case when the issue was joined to the start of the trial.

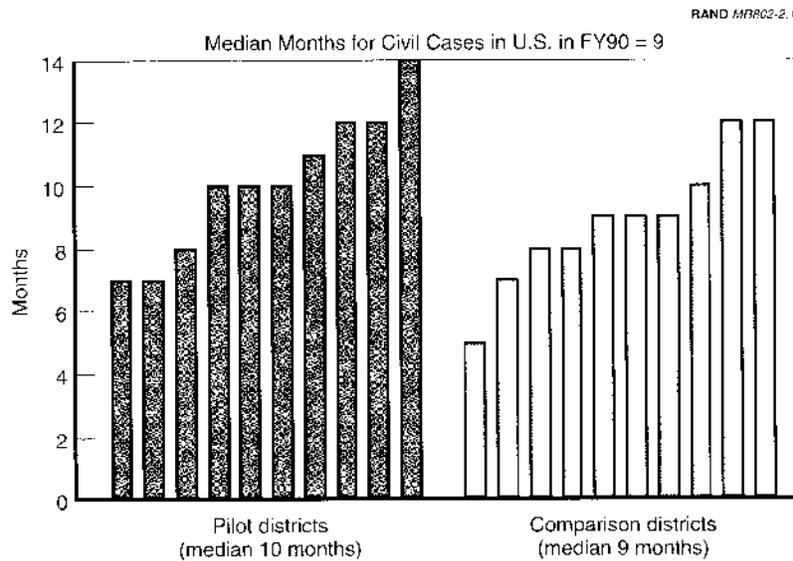


Figure 2.1—Median Time to Disposition in Pilot and Comparison Districts

1990 were over three years old, and the averages for both pilot districts (9.2 percent) and comparison districts (9.7 percent) approximate the national figure.

This examination of aggregate 1990 data pertaining to judgeships, filings, and time to disposition in the 20 districts suggests that they well represent the range of districts in the United States. Furthermore, the pilot and comparison district groups are reasonably comparable to each other, at least along these dimensions.

In 1996, after all of the study's survey data had been collected, we used additional data to conduct a multivariate statistical analysis to see if pilot and comparison districts as a group were different from one another. We controlled for differences in case characteristics among districts in the analysis. Our conclusion was that there was no statistically significant difference between pilot and comparison districts in 1991 before CJRA, in either the time to disposition or the cost per litigant.²⁴

We believe that the pilot and comparison districts represent the range of districts in the United States and are comparable to one another.

²⁴See Chapter Nine and Appendix D for details.

LEVELS AND METHODS OF ANALYSIS

Levels

In this study, we conduct analyses at two levels: the district and the case. For example, we compare ten pilot versus ten comparison *districts* so that we can evaluate the pilot program as a package. We also compare *cases* managed with and without certain policies and procedures. Case-level analysis is preferable because there is much variation in judicial case management within each district that is only visible at the case level. We can learn a lot more from 10,000 cases and how they were actually managed than we can from 20 districts with district-level packages of policies.

Methods

Our evaluation uses both descriptive tabulations and multivariate statistical analyses. Descriptive tabulations show data on time, cost, satisfaction, and fairness measures for cases managed with and without a procedure. Simple tabulations do not hold “other factors equal,” but they do demonstrate relationships found by the multivariate statistical analysis in a way that is easier for a non-statistician to understand. Multivariate statistical analyses estimate relationships between case management and time, cost, satisfaction, and fairness, while controlling for other factors such as district and case characteristics (e.g., stakes and complexity).²⁵ We use interviews with judges and lawyers, and prior research, to help interpret the empirical findings.

The purpose of statistical modeling is to summarize the voluminous data in a manner that can be more easily interpreted, and to develop estimates that reflect the true effects of case management policies on the cases being studied. From these statistical data summaries, we infer the effects of policy and evaluate the pilot program. Such inference is appropriate when we believe that the data are representative of the general population of civil cases, or attorneys, or litigants involved in civil cases.

We believe that our cases and responding attorneys are representative and that they are appropriately used for inferential statistical analyses. However, we do not believe that such inferential statistical analysis is appropriate for our litigant survey data, because less than 15 percent of litigants responded.²⁶ Although we have no strong evidence that the responding litigants are systematically different from other litigants, the low response rate does not justify our assuming that they do not differ. Hence, we report only aggregate tabulations of numbers for litigants, and we recommend that the litigant survey data be viewed as merely suggestive and supportive of our other analyses.

²⁵See Appendices D through G for information on the various types of multivariate analysis techniques that we employed. We use the statistical term “significant” consistently throughout this study to mean the $p=.05$ level of significance.

²⁶The actual percentage ranged from 10 to 14, depending on the sample being considered. See Appendix B for details.

General Civil Cases with Issue Joined

In practice, federal district courts split the civil caseload into two categories—types of cases that usually receive minimal or no management and general civil litigation cases to which the district’s standard case management policies and procedures apply. The definition of the former category, which we call “minimal management” cases for convenience, varies somewhat across districts, but typically it includes prisoner cases,²⁷ administrative reviews of Social Security cases, bankruptcy appeals, foreclosure, forfeiture and penalty, and debt recovery cases.²⁸

We sampled minimal management cases at a low rate (they constitute only about 20 percent of our entire sample) in case the districts decided to make major management policy changes for them as a result of CJRA. However, districts made few changes in their procedures for these cases. In addition, almost none of these cases are managed using the policies and procedures that apply to general civil litigation and that are the focus of the CJRA; hence, they could not inform our evaluation of the procedures of concern in the CJRA. For all of these reasons, we exclude the category of minimal management cases from our statistical analyses.

After reviewing the case management policies and procedures set forth in CJRA, we concluded that we could best estimate their effects by using data from general civil litigation cases with issue joined. Issue is considered joined after the defendants have answered the complaint in accordance with F.R.Civ.P. Rule 12(a) or as mandated otherwise by the court.²⁹ Cases in which issue is not joined usually are not available for and do not receive judicial case management. Thus, the policies and procedures of concern in the CJRA apply to cases with issue joined, and we exclude from our analyses cases that did not have issue joined.

In addition to analyzing general civil cases with issue joined, we also analyze the subset of general civil cases with issue joined that took longer than nine months to disposition. This is done for two reasons. First, about half the cases take longer than nine months, and this lets us see the effects of policy on the half of cases that take longer. Second, using all cases with issue joined puts some cases that close soon after defendants’ answer and before they can be managed into the “not managed early” category. Thus, the “not managed early” set of data includes some cases that closed too early to be managed as well as some that began management later than six months, which may distort the findings for all issue joined cases.³⁰ Using cases closed after nine months eliminates those cases that close almost immediately after issue is joined, and before the judge has a chance to manage them.

²⁷A small fraction of prisoner cases are death penalty cases, which, in contrast to the typical minimal management case, are complicated and time consuming to resolve. Our 1991 sample includes only two such cases; there are none in the 1992–93 sample.

²⁸To ensure consistency in our analysis, we have used these six categories to define minimal management cases in all districts.

²⁹Administrative Office of the United States Courts (1995), Chapter 5, p. 15.

³⁰See Appendix D for a discussion of this point.

INTERPRETATION OF FINDINGS

It is important to remember that in our evaluation design, we are not evaluating case management policies and procedures assigned to cases at random in an experiment. Although the pilot program was a designed intervention, the case management policy and procedure measures we observed being used were dictated by the choices made by districts and individual judges.

Given the observational nature of our data, one should not treat our statistical results as exact estimates of causal effect. For instance, if we find that using a particular case management procedure predicts a 60-day reduction in time to disposition in our sample of cases, this should not be interpreted to mean that if all judges were to use that particular procedure on every case, we would find exactly a 60-day reduction in time to disposition. Rather, our statistical analyses summarize the differences observed in our sample of cases. We have made every attempt to ensure that our estimates clearly represent effects in our observed data, but since the pilot program did not randomly assign case management procedures to cases using an experimental design, we cannot say definitively that our observed effects correspond to causal effects among the studied cases and districts. Thus, interpretation of our statistical results should take place only in the context of an understanding of how the judicial system functions in practice.

Given our understanding of how the civil justice system operates, we believe that the estimated effect that we will show later in this report should be treated as an upper bound to the effects that could be anticipated if the policies were implemented more widely.

Our estimate is an upper bound rather than a precise estimate because our quantitative analyses exploited observational data on the naturally occurring variation in judges' management practices rather than data resulting from an experimental random assignment of management practices to cases. We believe that we have accurately estimated the effect of a given management practice among districts and judges who currently use it. However, any effects we observe must be interpreted in light of the constraints imposed by observational data.

In particular, judges and districts *choose* to use certain case management policies and practices, and we must assume that these judges and districts may differ from other judges and districts who choose *not* to use the same policies and practices. For example, judges who currently use early management may use it with greater intensity or effectiveness than other judges who may be asked to start using it in the future. Judges who use early management now might be using it in combination with other practices for which we do not have data (such as extensive settlement discussion during the initial case management conference). On the other hand, judges who do not use a particular case management practice now may continue not using it even if they are asked to start using it in the future.

Thus, successful use of a case management procedure by some judges in some districts does not necessarily mean that it will be equally effective if all judges are asked to use the procedure in all districts. However, the limitations of observational data

notwithstanding, practices that we have identified as effective among judges who currently use them are good candidates for practices that could be beneficial if more widely implemented.

One issue that has been raised regarding the CJRA concerns the appropriateness and effectiveness of national uniform standardized rules and procedures.³¹ Some people see CJRA as a “top down” reform started by Congress. Others see CJRA with its local advisory groups and local rule revisions as an attempt to tailor management to the local legal needs and culture. Our research design did not address the debate over national versus local rules and procedures. Instead, we report what happened as a result of CJRA and the application of management principles and techniques identified in the act; we leave it to others to draw conclusions on the issue of uniformity of rules and procedures.

³¹For an interesting paper related to this issue, see Carrington (1996).

IMPLEMENTATION OF THE CJRA PILOT PROGRAM

In this chapter, we summarize how the Civil Justice Reform Act was implemented in the pilot and comparison districts.¹ We describe how the CJRA advisory groups were created, what the groups recommended to the court, what elements the courts' plans contained, and how those plans were implemented. We summarize how past practices, the advisory group recommendations, and the courts' implemented plans differ. We also explore some possible reasons for the variable nature of the act's implementation and discuss several concepts from the court and organization change research literature that shed light on the difficulties encountered when implementing the pilot program.

Our discussion draws on a wide range of data. We used CJRA advisory group reports, documents, and meeting minutes to assess the advisory group process and findings; we used the districts' plans and proposed local rule changes to assess what the districts said they would do under CJRA; we used the dockets for a large sample of cases to help us understand what was actually done on cases and when (such as schedule setting, assignment to management tracks, or referral to ADR); we used court records to assess the basic characteristics of the cases and court actions, such as referral to ADR, that were not always on the court docket; we used the judicial surveys on our sample of cases to get judges' views on whether they had changed how they manage cases as a result of CJRA; we used extensive mail surveys of thousands of lawyers and litigants on our sample of cases to get their views on how the case was managed and information on litigation costs, satisfaction, and views of fairness; and we used extensive semi-structured interviews with judges, court staff, advisory group members, and lawyers to better understand both the implementation of CJRA and case management in the districts before and after CJRA.

THE ADVISORY GROUP PROCESS

The CJRA required each district to appoint an advisory group to assess the condition of the civil and criminal dockets, identify the principal causes of delay and excess cost, and make recommendations for dealing with these problems. Each district court could accept, modify, or reject the advisory group recommendations when it adopted its CJRA plan. Following implementation of the court's plan, the advisory

¹A detailed discussion appears in Kakalik et al. (1996a).

group was also to monitor the implementation of the plan and provide input to an annual assessment.

The advisory groups were created in a timely fashion by each district's chief judge. The membership was predominantly members of the federal bar, and the chairperson was usually a senior member of the bar.

The act calls for advisory groups to be balanced and to include attorneys and other persons who represent major categories of litigants. One interpretation of this requirement is that the lawyers in the advisory group can help achieve balance because they represent different types of litigants,² and lawyers appear to have contributed to the group's balance in this way. However, "other persons" were minimally represented. Limited by their lack of familiarity with the federal district court system, lay people usually played only a very modest role in advisory group meetings.

The judges' role on the advisory group varied widely among districts. Research on court and organizational change clearly indicates that the judges should be involved in the process. On the basis of that research and our interviews, we believe the process would work best when judges play a moderate role rather than a very active or a very passive role. If judges dominate while attending all meetings and even chair them, it may stifle consideration of new ideas for change or at least create the appearance that the advisory group may not be offering independent advice. On the other hand, if the judges are almost totally uninvolved before receiving the advisory group's report, then the advisory group does not get the full benefit of the wisdom of judicial officers about the practicality of certain proposed changes, and judges do not get the full benefit of the wisdom of the advisory group members about problems and proposed solutions.

Advisory groups lacked the time and money to conduct extensive research on the causes of and solutions to cost and delay. Their contribution was to analyze available statistics on time to disposition and assess subjective information collected in interviews and surveys of people who run and use the court system. The recommendations they made generally flowed from either the identified causes of cost and delay or from the CJRA mandate that certain principles and techniques of case management be included or considered.

When the advisory groups found causes of cost and delay that were unrelated to case management, they generally pointed those out and made recommendations related to those other causes. When they perceived that the court had no major cost or delay problem, or was already using a CJRA principle of case management, they said so, but usually they also made suggestions to further refine and improve case management.

²Many of the attorneys in the groups consistently represent the same types of litigants in federal litigation. The U.S. Attorney is an obvious example, but it is also true for those who work for public interest firms and those who act as corporate or other organization counsel. In this sense, the lawyers themselves can be representative of major categories of litigants.

We were impressed by the dedication and conscientiousness with which the advisory groups approached developing recommendations. The various members of the advisory groups volunteered many hours of time. Advisory group members professed independence from the court, indicating that they felt free to express criticisms that reflected group consensus. Final reports generally reflected considerable independence from the court.

The act gives district courts the authority to accept, reject, or modify the recommendations of their advisory groups, but most district courts responded positively to most or all of the recommendations. Indeed, more than three-quarters of the major recommendations of the pilot and comparison advisory groups were adopted into the courts' CJRA plans.³ Circuit and Judicial Conference review of the plans after adoption resulted in few changes.

The quality of the required annual reassessments varies markedly from district to district. Although the act does not require a written assessment, seven of the 20 districts in this study have done written reassessments at least twice. Six of the 20 districts had no written documentation of the results of any annual assessment when we inquired in January 1996.

Whatever the content of the courts' CJRA plans, our interviews indicate that the process of generating the plans has made courts more cognizant of case management problems and opportunities. Bench-bar understanding reportedly has also been improved. That benefit alone probably justifies the advisory groups' work.

A majority of advisory group members whom we interviewed—especially the principal players among them—saw the advisory group process as valuable and believed that they had accomplished something worthwhile.

Our conclusion is that the CJRA advisory group process was useful, and the great majority of advisory group members thought so too.

HOW THE DISTRICTS IMPLEMENTED THEIR CJRA PLANS

The six principles and six techniques specified in the act fall into four general categories of procedures: differential case management, early judicial case management, discovery management, and alternative dispute resolution. We use these general categories in our discussion below.

Differential Case Management

This category of procedures includes differential management of cases as well as special judicial control of complex cases.

³The deadline for plan adoption was January 1, 1992, for pilot districts, and December 1993 for other districts.

Before CJRA, all pilot and comparison districts had special procedures for processing cases that require minimal management—typically, prisoner petitions, Social Security appeals, government loan recoveries, and bankruptcy filings. For other cases, nearly all districts relied on “judicial discretion”—judges making case management decisions case by case according to their own schedules and procedures. Hereafter, we refer to this as the judicial discretion model of case management. Complex cases, for example, can receive individualized, specialized management within a framework enunciated in the *Federal Manual on Complex Litigation*.

In response to the CJRA, six of the ten pilot district plans replaced the judicial discretion model with a track model of differential case management. Implementing a track model implies having separate tracks for different types of general civil cases, setting guidelines for managing the cases in each track, and assigning cases to each track at or near case filing. A common formulation is to have three tracks: expedited, standard, and complex. Six of the ten pilot districts planned to replace the judicial discretion model with a track model, but the track model proved difficult to implement. Most districts that included tracking in their plan actually assigned the traditional group of minimal management case types listed above to an expedited track. Five of the six pilot districts whose plans contained a track model assigned 2 percent or less of their cases to the complex track. Pennsylvania (E), which assigned 7 percent of its general civil cases to the complex track, was the sole exception.⁴ The consequence was that almost all general civil cases to which CJRA procedural principles might be relevant were placed in the standard track, if any track assignment was made. This meant that there was little actual “differential” tracking of general civil cases in most districts that adopted a track model in their CJRA plans. Consequently, we have no basis for evaluating how the track method of DCM might have affected time, cost, satisfaction, and views of fairness.

Using the act’s flexible definition of differential case management, four of the pilot districts interpreted the CJRA’s requirement as being fulfilled by a continuation of the judicial discretion model.

Table 3.1 traces the types of differential case management in the pilot and comparison districts from before CJRA to implementation of the district plans.

Judicial discretion is defined above.

Pretrial management by magistrate judges refers to a system under which all cases are automatically assigned to a magistrate judge, who manages a case through all its pretrial phases.

In *judicial-officer-selected tracking*, the judicial officer makes the initial track decision for a case.

Rule-based tracking involves assigning a case on the basis of its objective characteristics—such as the nature of suit—known at the time of filing.

⁴PA(E) also implemented other changes, the results of which we cannot reliably separate from the effects of the track system.

Table 3.1
Differential Case Management: From Advice to Implementation

Stage in Process	Judicial Discretion Model		Track Model		
	Standard	Magistrate Judges	Judicial-Officer-Selected	Rule-Based ^a	Attorney-Selected
Pilot Districts					
12/91 before CJRA	10 districts				
Advisory group recommendation	TN(W), UT, WI(E), GA(N), OK(W)	CA(S)	NY(S), DE, TX(S)		PA(E)
District plan	TN(W), UT, WI(E)	CA(S)	NY(S), DE, TX(S), OK(W)	GA(N)	PA(E)
District implementation	TN(W), UT, WI(E)	CA(S)	DE, OK(W), TX(S) (each <1%)	GA(N)(2%)	NY(S) (1%), PA(E) (7%)
Comparison Districts					
12/91 before CJRA	9 districts				
Advisory group recommendation	FL(N), IL(N), IN(N), KY(E), KY(W), MD	NY(E)	AZ, PA(M)		CA(C)
District plan	CA(C), FL(N), IL(N), IN(N), KY(E), KY(W), MD	NY(E)	AZ, PA(M)		
District implementation	CA(C), FL(N), IL(N), IN(N), KY(E), KY(W), MD	NY(E)	PA(M) (4%), AZ (1%)		

NOTES: Numbers in parentheses indicate the percentage of cases assigned to the complex track. Pilot districts, IN(N), and NY(E) began implementation in 1/92; the other eight comparison districts began implementation in 12/93.

^aAlthough only GA(N) uses rule-based tracking for all civil cases, other districts use hybrid systems that are partially rule-based and partially judicial-officer-selected tracking. For example, AZ uses a rule to assign cases to their expedited, prisoner pro se, and arbitration tracks, but the assignment of cases to the complex track is done by a judicial officer.

Attorney-selected tracking usually requires the filing attorney to opt for a particular track—expedited, standard, or complex—after which the opposing attorney has an opportunity to dissent. The judge then decides.

Early Judicial Case Management

Early judicial case management as defined in the act includes early and ongoing judicial control of pretrial processes as well as having counsel jointly present a discovery/case management plan at the initial pretrial conference. Related CJRA techniques include parties being represented at pretrial conferences by an attorney with authority to bind them; requiring the signature of the attorney and the party on all requests for discovery extensions or postponements of trial; and requiring party rep-

representatives with authority to bind to be present or available by telephone at settlement conferences.

All advisory group reports favored the principle of early judicial management of general civil cases, and all of the courts' plans accepted the principle of early and ongoing judicial control of the pretrial process. However, case management styles varied considerably between districts and between judges in a given district.

Before CJRA, only one district in our study required that counsel jointly present a discovery/case management plan at the initial pretrial conference, although at least one other district required attorneys to confer before the first pretrial conference to attempt to agree on a scheduling order.⁵ Four of the ten pilot districts adopted this technique in their plan in 1991, and nine of the other pilot and comparison districts later adopted it when the federal rules were changed in December 1993.⁶

Both before and after CJRA, all 20 districts required, or allowed judges to require, that each party be represented at each pretrial conference by an attorney with authority to bind that party.

In contrast, none of the 20 districts required the signature of the attorney and the party on all requests for discovery extensions or postponements of trial either before or after CJRA.

Finally, before CJRA, eight of the 20 districts required, upon notice by the court, that party representatives with authority to bind be present or available by telephone at settlement conferences. Five additional districts adopted this technique as part of their CJRA plan. Note that this technique is not an automatic requirement; rather, it depends on judges' decisions on individual cases.

Discovery Management

Issues in managing discovery include how much the court, rather than lawyers, should control volume and timing of discovery, and what types of information should be voluntarily or mandatorily exchanged without formal discovery requests. The CJRA discovery policies include early and ongoing judicial control of pretrial processes, requiring good-faith efforts to resolve discovery disputes before filing motions, and voluntary exchanges of information.

Before CJRA, most districts left court control of the volume and timing of discovery to the judge in each case; CJRA had little effect on this arrangement. However, as summarized in Table 3.2, a few districts implemented various blanket limitations on the amount and length of certain types of discovery.

⁵See Form 35 of F.R.Civ.P. 26(f) for an example of a possible discovery/case management plan. We consider a discovery/case management plan to include more than the typical scheduling order, although in some districts they may be functionally equivalent.

⁶F.R.Civ.P. 26(f).

Table 3.2
Pilot and Comparison Districts with Various Types of Discovery Limitations
Before and After CJRA Plan Implementation

Type of Discovery Limitation	Pilot Districts		Comparison Districts	
	Before 12/91	After 12/91	Before 12/93 (12/91 for IN(N) and NY(E))	After 12/93 (12/91 for IN(N) and NY(E))
Judicial discretion, with no prospecified limits	2 (NY(S), UT)	2 (NY(S), UT)	2 (AZ, NY(E))	0
Limitation on number of interrogatories and requests for admission (other than 12/93 F.R.Civ.P. 33 limit)	7 (CA(S), DE, GA(N), OK(W), TN(W), TX(S), WI(E))	7 (CA(S), DE, GA(N), OK(W), TN(W), TX(S), WI(E))	8 (CA(C), FL(N), IL(N), IN(N), KY(E), KY(W), MD, PA(M))	10
Limitation on number of depositions (other than 12/93 F.R.Civ.P. 30 and 31 limit)	0	0	0	2 (AZ, NY(E))
Limitation on length of depositions	1 (GA(N))	2 (GA(N), WI(E))	0	1 (PA(M))
Limitation on discovery cutoff time for certain types of cases	2 (GA(N), PA(E))	2 (GA(N), PA(E))	0	1 (MD)

NOTES: Districts with limitations may have more than one type. Pilot districts, IN(N), and NY(E) began implementation in 1/92; the other eight comparison districts began implementation in 12/93.

However, CJRA and the December 1993 changes in the federal rules brought about substantial change in early disclosure. Only one district required it before CJRA;⁷ since CJRA, all pilot and comparison districts have adopted one of five approaches providing either voluntary or mandatory exchange of information by lawyers, sometimes only for specified types of cases.

Table 3.3 summarizes the changes in early disclosure in pilot and comparison districts.

Four pilot districts later switched from their initial disclosure procedure to follow the mandatory disclosure required by the December 1993 revised F.R.Civ.P. 26(a)(1), and six comparison districts are following the revised Rule 26(a)(1). The ten other pilot and comparison districts have exercised their right to "opt out" of the revised Rule 26(a)(1). Some districts opted out to retain their pilot program disclosure rules, some of which were more stringent in their disclosure requirements than the revised Rule 26(a)(1).

The requirement that lawyers certify good-faith efforts to resolve discovery disputes before filing motions has undergone little change. All but one district had rules governing this area before CJRA; these have been continued or strengthened.

⁷At least one other district required attorneys to confer before the first pretrial conference to attempt to agree on a scheduling order.

Table 3.3
Early Disclosure of Information: From Advice to Implementation

Stage in Process	Voluntary	Mandatory for Some Cases	Mandatory, Info on Your Side	Mandatory, Info on Both Sides	Follow 12/93 Rule 26(a)(1)
Pilot Districts					
12/91 before CJRA	9 districts	PA(E)			
Advisory group recommendation	CA(S), TN(W), UT	DE, NY(S), TX(S)	GA(N), WI(E)	OK(W), PA(E)	
District plan	CA(S), TN(W), UT	DE, NY(S), TX(S)	GA(N), WI(E)	OK(W), PA(E)	
District implementation	CA(S) + 2 before 12/93 (TN(W), UT)	NY(S) + 2 before 12/93 (DE, TX(S))	GA(N), WI(E)	OK(W), PA(E)	DE, TN(W), TX(S), UT after 12/93
Comparison Districts					
12/91 before CJRA	8 districts	AZ	CA(C)		
Advisory group recommendation	IL(N), MD, FL(N), KY(W), AZ	IN(N)	CA(C)	NY(E)	KY(E), PA(M)
District plan	IL(N), MD, AZ	IN(N)	CA(C)	NY(E)	KY(E), PA(M), FL(N), KY(W)
District implementation	IL(N), MD	+1 before 12/93 (IN(N))	CA(C)	NY(E) after 12/93	KY(E), PA(M), FL(N), KY(W), AZ, IN(N) after 12/93

NOTES: Mandatory, info on your side, requires mandatory exchange of information bearing significantly on *your* claim or defense, plus other items. Mandatory, info on both sides, requires mandatory exchange of information bearing significantly on *any* claim or defense, plus other items. Rule 26(a)(1) requires mandatory exchange of information relevant to disputed facts alleged with particularity in the pleadings, plus other items, and was revised in 12/93 after cases in our sample were usually beyond the age when the rule would have been applied. Pilot districts, IN(N), and NY(E) began implementation in 1/92; the other eight comparison districts began implementation in 12/93.

Alternative Dispute Resolution

The CJRA's ADR policies include diverting cases, when appropriate, to ADR programs and offering an early neutral evaluation program.

The plans from all 20 districts permit the use of ADR techniques. In implementation, however, two types of programs have emerged, both of which meet the loosely defined requirements of the CJRA. About half the districts have formally structured programs involving between 2 and 19 percent of all their civil case filings. And one district uses magistrate judge early neutral evaluation on 50 percent of its cases. The other districts have unstructured programs that involve less than 2 percent participation.

Table 3.4 summarizes the distribution of various types of ADR programs across the pilot and comparison districts. Districts not listed in the table all permitted ADR of various types but referred less than 1 percent of their civil case filings to ADR programs.

Table 3.4
ADR: From Advice to Implementation

Stage in Process	Mandatory Arbitration	Mandatory Mediation	Mandatory Early		Voluntary Mediation	Voluntary Early Neutral Evaluation
			Neutral Evaluation	Arbitration		
Pilot Districts						
12/91 before CJRA	OK(W) (10); PA(E) (15)	PA(E) (10)				
Advisory group recommendation	OK(W), PA(E), GA(N)	PA(E), NY(S)	CA(S)	CA(S), UT	OK(W), TX(S)	TN(W)
District plan	OK(W), PA(E), GA(N)	PA(E), NY(S)	CA(S)	CA(S), UT	OK(W), TX(S)	TN(W)
District implementation	OK(W) (8); PA(E) (13)	PA(E) (6); NY(S) (5)	CA(S) (50)	UT (4)	OK(W) (6); TX(S) (5)	
Comparison Districts						
12/91 before CJRA	NY(E) (9)				FL(N) (4)	IN(N) (6)
Advisory group recommendation	NY(E)			AZ	FL(N), PA(M), KY(E), KY(W), MD, NY(E)	IN(N), NY(E), CA(C), FL(N), KY(W), PA(M)
District plan	NY(E)			AZ	FL(N), PA(M), KY(E), KY(W), MD, NY(E)	IN(N), NY(E)
District implementation	NY(E) (10)			AZ (4)	FL(N) (4); PA(M) (2)	IN(N) (6); NY(E) (2)

NOTES: Numbers in parentheses indicate referrals to ADR type indicated, as a percentage of all case filings in the district during the year. Pilot districts, IN(N), and NY(E) began implementation in 1/92; the other eight comparison districts began implementation in 12/93.

ASSESSMENT OF IMPLEMENTATION

All pilot districts complied with the statutory language in the act, which provides loosely defined principles but leaves operational interpretation of them to the discretion of individual districts and judicial officers. Many pilot and comparison districts interpreted some or all of their current and past practices to be consistent with the language of the act and continued those practices unchanged. However, if the spirit of the act is interpreted to mean experimentation and change focusing on the six CJRA principles, then the pilot districts met that spirit to varying degrees. Comparison districts, which were required to consider but were not required to adopt the six CJRA principles in their plans, generally made fewer changes than pilot districts.⁸

⁸A detailed discussion appears in Kakalik et al. (1996a).

Even in pilot districts whose plans suggested major changes, implementation often fell short. For example, six of the ten pilot districts adopted a plan with a track model of differential case management, but only one assigned the majority of its general civil cases to tracks and had more than 2 percent of the cases in both the standard and the complex tracks; in the other districts with track models, the assignment of cases to tracks was either not often made or was almost universally made to the standard track. All ten of the pilot districts adopted plans that provided for alternative dispute resolution, but four referred less than 2 percent of their cases to ADR.

Thus, for various reasons, in practice there was much less change in case management after CJRA than one might have expected from reading the plans. This is evident both from observations at the district court level of how the major elements of the plans were implemented and from surveys of the judges in the 1992–93 sample of 5,000+ cases. In 85 percent of the cases surveyed after CJRA, for example, the pilot district judges said that the surveyed case was managed no differently than it would have been before CJRA.⁹

Districts and judges vary widely in how they approach case management. Some have been relatively aggressive, and others have continued with low-key approaches. For example, one district uses differential management tracks, uses early judicial management on all general civil cases, mandates early disclosure of information bearing significantly on both sides of the case, and assigns a substantial number of cases to mandatory ADR programs. This profile contrasts sharply with a district that uses individualized case management, permits voluntary early disclosure, and allows but does not require ADR.

These large differences between districts and judges in case management policies provide the opportunity to evaluate very different policies, even though the districts and judges that use them did not change substantially as a result of CJRA.

Overall, implicit policy changes may be as important as explicit ones. Many judges and lawyers have commented in interviews that the process of implementing the pilot plans has raised the consciousness of judges and lawyers and has brought about some important shifts in attitude and approach to case management on the part of the bench and bar. For example, our interviews suggested, and the case-level data we collected also indicated, that there has been an increase in the fraction of cases managed early and a shortening of time to discovery cutoff.¹⁰

Several of the CJRA advisory group assessments contended that certain factors that are beyond the courts' direct control also influence civil litigation cost and delay. Three factors predominated. First is the pressure generated by the criminal docket. Legislation creating new federal crimes, adoption of the Speedy Trial Act, and the ad-

⁹Our sample was drawn well before eight of the comparison districts implemented their plans, and the comparable percentage for comparison districts was 92 percent "no difference."

¹⁰There are some technical problems with comparing empirical data from a filing sample and a termination sample, but the consistency of the interview information with the empirical information is encouraging. For a discussion of the statistical issue, see Appendix D.

vent of mandatory sentencing guidelines all were said to increase the burden on the federal court and provide less time for the orderly movement of civil cases. Second is the fact that judicial vacancies were being left unfilled for substantial periods of time. The third factor is the need for better assessment of the effect of proposed legislation on the courts' workload.

IMPEDIMENTS TO CHANGE

The experiences surrounding the implementation of the CJRA pilot program exemplify the challenge of implementing change in the federal courts. The pilot program has taken very different paths in different pilot districts. In this section we explore some of the possible reasons for this variation, including the process and definition of the desired change, organizational factors, implementation factors, and local legal culture. We also suggest several concepts that should be taken into account if future changes are contemplated.

The Process and Definition of Policy Change

The process and definition of the intended changes in policy may be responsible for much of the variance in the way the principles and techniques of the CJRA were implemented by the courts. With the exception of the ten pilot districts, the act asked the courts only to *consider* these innovations when formulating the court's cost and delay reduction plan. Even for pilot districts, which were required to adopt the CJRA principles, the *less than precise wording* of most of the act's principles greatly enhanced the opportunities for complying with the statutory language by *retaining*, rather than changing, existing case management policies.

Changing court procedure with the involvement of Congress is by no means unique to the Civil Justice Reform Act. For example, Congress has long been involved in the process for changing the Federal Rules of Civil Procedure and other rules under which the court operates. What makes the CJRA different is that it gives each district a set of broadly defined "marching orders" to develop and implement a cost and delay reduction plan with the aid of a separate district-level advisory group. After receiving its advisory group's report, each court was free to determine the content of its CJRA plan.

The intentionally vague wording of some of the act's case management principles and techniques played a major role in the limited degree of change. The loosely defined principles in the act permitted experimentation but also often allowed status quo case management policies to satisfy the statutory language of the act. Policy analysts who study implementation theory indicate that the lack of clear-cut directives makes the process of change "a more complex task that may fail because of unwillingness to comply or, more likely, some failure of capacity to do so."¹¹ Had the act been less ambiguous, there might have been more change.

¹¹ NDRI (1993), p. 369.

Organizational Factors

Some of the same dynamics that frustrate transformation in corporations, universities, the armed services, religious bodies, and other large social groups exist in the courts as well. However, the federal courts also present a special situation with respect to implementing change. Judges have broad powers, granted by the Constitution, by the Federal Rules of Civil Procedure, and by legal precedent, to manage their caseloads. These powers were not superseded implicitly or explicitly by the Civil Justice Reform Act. In addition, although the judges are scattered across the country in almost 100 separate districts, they share some long- and commonly held beliefs about the special nature of their judicial work and the independence accorded their office under Article III of the Constitution. Each judge is independent and has life-long tenure in the position at a salary that cannot be reduced. Finally, our interviews indicate that some judges and lawyers believe that the act's emphasis on speed and efficiency may be detrimental to the primary "justice" mission of the courts and hence believe they have good reasons for resisting change.

Although all organizations place a premium on stability, it is especially true of the courts. Indeed, part of the judicial system's legitimacy flows from the fact that the courts have operated at times seemingly independent of the changes occurring in the world around them. This independent operation is integral to the constitutional concept of intergovernmental checks and balances in a system of separated powers. The sense that justice is being dispensed according to long-standing principles of right and wrong helps to validate a court's ruling (that may deprive someone of property or liberty). Judicial officers are loath to disturb the perception that they are the conduits of a code of justice handed down over many decades. Little wonder, then, that some judges might look askance at perceived attempts to alter the methods by which they dispense justice.

In addition to a general reluctance to modify existing judicial procedures, sources of change external to the courts may stimulate additional resentment and resistance. The relationship between the three branches of the federal government is complex, and each attempts to guard against any diminution of its independence and authority. The Civil Justice Reform Act's principles and techniques were imposed upon the judges not by the usual rule-making process but by a law enacted by the legislative branch. Indeed, at the time of the passage of the act, very few other court systems in the country had a legislatively mandated delay reduction program.¹²

Notwithstanding the fact that some widely respected and influential jurists participated late in the process that shaped the CJRA, no active federal judge was a member of the Brookings task force that preceded the CJRA, and some judges see the legislation as an unwarranted intrusion into their constitutionally protected territory.

¹²For example, in 1986, California's legislature passed a Trial Court Delay Reduction Act (Gov. Code §68600 et seq.) which, similar to CJRA, required a set of experimental courts to increase the management of cases. For a discussion of that state's experiences with its externally imposed program, see Judicial Council of California (1991).

The cost and delay reduction goals of the act may also fuel some judges' reluctance to embrace the spirit of the act. Few phrases trigger as negative a reaction in members of the bench as do "efficiency" and "assembly-line justice." Certainly the process by which civil disputes are resolved in the courts cannot be likened to the production of widgets. However, some judges and attorneys believe that an undue emphasis on reducing cost and delay in effect makes this analogy. Some judges see attempts to speed up pretrial procedures, limit discovery, force the exchange of information, divert cases into alternative dispute resolution programs, apply rigid procedural rules to particular case types, and encourage settlements as potentially interfering with an orderly and deliberate search for the truth.¹³ Focusing on a judge's output and speed measures, without taking into consideration the individual needs of each of his or her cases, is felt to be yet another step towards the industrialization of the courts.

In sum, implementing the principles and techniques recommended in the CJRA involves more than simply writing a plan and making a few modifications to court rules. To the extent that judges and lawyers feel procedural innovations violate valuable traditions and come from an external and perhaps antagonistic source, these innovations will be more difficult to implement. As with the military and any other large organizations that value predictability and stability, courts are "implicitly adverse to change and explicitly adverse to change dictated from outside the organization."¹⁴

Implementation Factors

Even when a court's plan clearly stated an intent to modify existing management procedures, the actual implementation sometimes differed markedly from the plan. There are a number of reasons for this divergence.

First, the detailed design of the new procedure (such as a tracking system of differential case management, or a mediation program) had to be done by each district, or by each judge within the district. Districts were not provided with fully developed and detailed alternative methods of implementing the procedure. Some districts left implementation to each judge; so, for example, each judge would have to develop his or her own ADR referral forms and procedures and list of neutral ADR providers.

Second, although the district court is required to make an annual self-assessment, there exists no effective mechanism for ensuring that the policies contained in its plan are carried out year after year. The act does not explicitly require a written annual assessment, and six of the 20 districts had no written documentation of the results of any annual assessment when we asked in January 1996.

¹³For an interesting discussion of the "changing nature of adjudication, its transformation into new forms of administering justice, and the problems and conflicts this transformation expresses and generates" see Heydebrand and Seron (1990).

¹⁴For an excellent discussion of the problems inherent in social changes that are externally imposed upon a large organization, see NDRI (1993), p. 370. We have drawn liberally from this book's review of the literature dealing with policy implementation.

Finally, regardless of the intent of the legislators, the conclusions of the advisory groups, or the orders contained in the CJRA plans adopted by the districts, the real responsibility for implementing the innovations contained in the act falls upon the individual judges and lawyers in a district. These judges are the ones who actually have to tailor the level of case management, effectuate early and ongoing control, apply special techniques to complex cases, refer appropriate cases to ADR programs, and perform any of the other litigation management practices envisioned by the act. Lawyers then may implement the innovations in their practices in various ways. Most judges we interviewed were supportive of the “managerial judge” case management policies embodied in CJRA; however, some expressed strong concerns about Congressional efforts to impose these management policies on the judiciary, feeling that each judicial officer has, and must have, discretion in deciding how to manage each case. If some judges do not agree with a policy in the CJRA plan and believe that existing methods of case management are better than the new ones, they may exercise that discretion by not applying the policy to cases.

Local Legal Culture

The research literature suggests two general themes regarding the causes of, and solutions to, litigation delay.¹⁵

The proceduralist theme relies on procedures to establish early and firm judicial control over pretrial activities and thereby reduce delay. This is the approach implicit in the CJRA.

The culturalist theme focuses on the local legal culture—lawyers’ and judges’ customary ways of conducting litigation—as a major influence. This theory suggests that unless the expectations, practices, and attitudes of the major participants in civil litigation are changed, simply adjusting local rules on civil case management will have little lasting effect. Any long-term improvement in cost and delay can come about only after a court system has effected changes in the way lawyers and judges approach case processing.

For example, traditional and fairly stable notions of a lawsuit’s “normal pace” in a district certainly influence time to disposition. If attorneys generally believe that trials are highly unlikely to begin on the date scheduled, then they will not prepare adequately and can reasonably expect a continuance to be granted. If there is a general tradition in a jurisdiction that discovery should not begin until some months after the date of filing an answer, then the average time to disposition will be longer than in a locale where attorneys start discovery earlier. If judges feel that their predominant role is conducting trials and ruling on matters raised by the attorneys, then they will not be much involved with a case until the lawyers indicate they are ready. In contrast, a district may have a tradition of judges managing cases early and actively.

¹⁵See Appendix I for an annotated bibliography of recent major studies and their findings.

Judges learn these traditions before coming to the bench. Once appointed, they pass these expectations along to new additions to the bench. Lawyers do the same to new members of the local bar. Court systems adapt to a certain pace of litigation, which in turn is associated with a backlog of cases in both the judge's chambers and in the attorneys' offices. Each jurisdiction has a relatively unique set of these beliefs, and changing them is not an easy task.¹⁶

The culturalist and the proceduralist themes need not conflict. Even if the effect of local legal culture is a significant one, it can be modified by making the actors conform to different rules that limit discretion. At the level of the individual case, consistent and appropriate application of early active judicial case management techniques can help move cases along at the desired pace, regardless of lawyers' initial attitudes. Thus, although bar practices are important, they can be changed and once changed they may really make a difference. Indeed, they were often the result of district policy to begin with. It is important to note that change in local legal culture does not come easily or rapidly. It takes time for dissemination of knowledge that policies and procedures have changed and that judges are taking the changes seriously.

UNDERSTANDING THE IMPLEMENTATION OF CHANGE UNDER CJRA

Having reviewed a number of specific factors that appeared to impede proposed CJRA changes to the federal judicial system, we now turn to a more general discussion of policy change and methods for facilitating implementation in the courts. We draw on a variety of research studies focused on change in the courts and in large organizations to understand more completely the dynamics that may have hindered change when the CJRA pilot program was adopted.

The Challenge of Changing Organizations

Despite the diversity of theories about change and managing change,¹⁷ there is general consensus that change is more complicated than optimistic implementers—or policymakers—typically think. Other litigation cost and delay reduction studies have also found that “introducing change into existing court systems could be a problem all by itself.”¹⁸

Why is changing an organization, especially courts and the way they manage their caseload, so difficult?

First, *organizations are dynamic and the initiators of change are often not the ones responsible for implementation.* As a result, it is often difficult “to make changes stick.”¹⁹ For example, although federal judges have lifetime tenure, members of

¹⁶Chapper et al. (1984); see especially Chapter 6, “Activating the Process of Change.”

¹⁷Mohrman et al. (1989).

¹⁸Chapper et al. (1984), p. ix.

¹⁹Kanter et al. (1992).

Congress and their staffs do not. Administrations change, political attention shifts, and task forces, committees, and sponsors eventually move on to other issues. In addition, the unique relationship between the initiators of the change in Congress and the implementers in the courts brings with it additional complicating factors. The powers granted to the judiciary and the separation of powers contained in the Constitution mean that the courts necessarily have some latitude in implementing the initiatives of the legislature.

Second, *there are limitations to managerial action in controlling the change process.* In fact, some theorists challenge whether deliberate change to large organizational systems can be effectively managed at all or if forces in the external environment often simply overwhelm such efforts.²⁰ In addition to external constraints, internal constraints also hinder change.²¹ Change often sparks conflicts of interest, ideological debates, and a host of logistical obstacles. In short, change cannot simply be ordered to happen, and no one person, manager, or leader — including a chief judge — is likely to be able to force the process without gaining the support and commitment of others throughout the system.

The courts, given the independence of individual judges, are especially limited in their ability to use managerial actions. It is clear that “one important difficulty faced by change advocates in the justice system which is not faced in hierarchical organizations [is] the absence of the authority and power that emulate from hierarchy as a resource to be wielded in the change process.”²²

The range of interests within and external to a system of justice, often with independent bases of power, is another reason why changes to court operations may be more difficult than those attempted in more hierarchical organizations.²³ Any proposed change in the court system regarding case management policies and procedures should take into account the expectations, attitudes, and practices of the *judges*, the *lawyers*, and the *litigants*.²⁴ Failing to recognize the needs of any one of these participants in the civil justice system may be detrimental to a reform effort.

There also are some real limits to cost and delay reduction that cannot be ignored when case management changes are being considered.²⁵ Lawyers and litigants need a certain amount of time to investigate the factual and legal issues involved in the case and to develop their strategies for litigation; trying to move cases faster than the time needed for adequate preparation raises issues of due process, fairness, and justice. Also, increases in criminal and civil filings may overwhelm any possible improvements in case management.²⁶

²⁰Hannan and Freeman (1989).

²¹Pfeffer (1981).

²²Grau (1981), p. 97.

²³Grau (1981), p. 97.

²⁴Church et al. (1978), p. 5. Consultation with the bar is a minimum requirement for effective caseload management; Solomon and Somerlot (1987), pp. 10–11.

²⁵For a discussion of some limitations affecting criminal case time reduction, see Sipes et al. (1980), p. 31.

²⁶Judicial Council of California (1991), p. 9; Kakalik et al. (1990), p. 115.

Third, *isolated efforts to change policies and procedures often fail because other components of the organization are not aligned simultaneously to support the change.* The federal courts, like other organizations, require consistent and coordinated change across multiple components. Systematic change and complementarity among changes in different parts of the organization strengthen and clarify the goals and direction of the change, align incentives, and provide the requisite tools and information to those who must translate policy into action.²⁷ Without an aligned set of changes to support a new initiative or innovation, the change effort is likely to fall short of its intended objectives.

Finally, *change is often most difficult when the need for change is greatest.* There are two perspectives for explaining this paradox in the context of the courts. Scarce resources or excessive backlogs in a particular court may increase the desirability of change while reducing the court's capacity to achieve it. A large pending caseload per judge may impair a court's ability to implement a new policy of setting firm early trial dates or achieving prompt resolution of motions.²⁸ When resource constraints impede a well intentioned change effort, the results can be particularly detrimental because constituents may begin to question their ability to effect change, as well as the governing body's understanding of and dedication to the overall change program.²⁹ The second perspective explains this paradox in a slightly different way: When conditions become adverse—as backlogs increase or pending motions pile up—courts and judges may become more resistant to change and revert to “well-learned” behavior.³⁰ Indeed, research suggests that under difficult conditions, most individuals will automatically return to familiar ways, and groups will become less flexible and less open to new ideas.

Implementation Strategies and Principles

Given the difficulty and complexity of change, how do organizations such as the courts manage to achieve the changes they desire? The literature is replete with recommended strategies and approaches, many of which are as complex as the changes they address. Despite this abundance, a number of common themes emerge. In the sections that follow, we address several strategic elements of implementing change that have been well documented in the court and other organization change literature. As we do so, we apply these themes to the judicial context.

Traditional theories of implementation maintained that change could be usefully divided into two phases: adoption (formulation of the policy proposal and eventual codification) and implementation.³¹ Such theories depicted change as a discrete process, which, when carried out “correctly,” would lead to a new desired state.

²⁷Galbraith (1995); Pfeffer (1994).

²⁸Goerdts et al. (1991), p. 51.

²⁹Hackman (1990).

³⁰Staw et al. (1981).

³¹NDRI (1993), pp. 372-380.

Contemporary theories, taking a more realistic view of change, recognize that planned change efforts tend to work more erratically than traditional models suggest.³² Rather than viewing implementation as a set of discrete stages, roles, and tasks, change theorists now view the design and implementation of change as a more mutual, emergent, and intertwined process.³³ Exemplifying this newer perspective, some researchers characterize implementation as “action learning,” a process in which change implementers and recipients try out new behaviors, processes, and strategies; assess them; and make modifications necessary to move in a desired direction.³⁴

The distinction between the traditional portrayal of change as planned process and the conception of change as a more iterative process of active learning is significant because it implies a different set of strategies for implementation. Instead of being something that is put into place through a discrete set of procedures and rules, change becomes a process that requires effective direction, communication, and feedback as well as an aligned set of organizational supports to facilitate effective learning. When viewed from this perspective, change is not something that is “done” to members of the organizational system; rather, it is something that they participate in, experience, and shape. As such, members are less resistant to change because they have had a hand in creating it.

From an action learning perspective, the first component of an effective implementation strategy is to *articulate a precise expectation of what the change is to accomplish*.³⁵ This “change vision” may include the mission, goals, and desired results of the intended change. However, the precise details or means by which such objectives are to be met are typically left up to the individuals who have to implement and live with the change.³⁶

The most important aspect of the change vision is that all those involved in the change effort understand and agree on what the vision is. In the case of the CJRA pilot program, the vision was that certain case management principles were to be implemented and evaluated in ten districts. However, the vision was not clearly articulated. We stress that clear articulation of program principles and objectives does not imply micromanagement of the details of program implementation.

Stating expectations at the outset can greatly improve implementation efforts by more directly empowering those responsible for execution.³⁷ Having clearly defined objectives helps to align efforts, provides criteria for evaluating options, and motivates action. For example, clearly defined goals such as time standards are an impor-

³²Kanter et al. (1992).

³³Mankin et al. (1996).

³⁴Mohrman and Cummings (1989).

³⁵Kanter et al. (1992).

³⁶Tawler (1986); Hackman (1990).

³⁷Hackman (1990).

tant part of an effective delay reduction program.³⁸ As such, explicitly stated expectations would have helped individual districts align their efforts with the objectives of the reform, provided judges and judicial staff with criteria to use in selecting performance strategies and assessing progress, and generated energy within and across districts as members sought to reach clearly specified goals.³⁹

In addition to creating clear expectations, it is also important to *create the perceived need for change*. Unless members of an organization perceive a need to do things differently, little will happen. Human factors are critical to a successful litigation cost and delay reduction program and include a commitment to change hopefully among *all* the judges as well as an acceptance of responsibility regarding the cost and pace of litigation.⁴⁰ It is essential to any effort to reduce problems with the civil justice system that the court (and its individual members) consider that the problem to be an institutional and a social one.⁴¹ There are at least three ways to promote the perception that change is necessary: (1) show data that indicate that current practices are ineffective; (2) provide analyses that demonstrate how changing environmental conditions or other factors are likely to exacerbate current problems in the future; and (3) furnish some type of external stimulation in the form of other models and data demonstrating that alternative strategies are available and more effective.⁴²

Certainly in the case of civil justice reform, there has been substantial effort to promote the need for change. The Brookings Institution report,⁴³ as well as reports by the Federal Courts Study Committee⁴⁴ and President Bush's Council on Competitiveness⁴⁵ each concluded that the costs and delays associated with litigation necessitated change in the federal court system. However, as noted above, some judges and lawyers continue to resist the CJRA, believing that the CJRA's emphasis on speed and efficiency is inappropriate. Our interviews suggest that efforts to create the perception among judges and lawyers that change is necessary have not been completely successful.

A third important component of implementing change is to *establish a governance structure and process that coordinates individual initiatives and assigns accountability for results*. A major issue for making change happen is sorting out and assigning responsibility and accountability. Although this may seem obvious, many change

³⁸ABA National Conference of State Trial Judges (1984), p. 11; Mahoney et al. (1988), p. 199; Solomon and Somerlot (1987), p. ix.

³⁹In contrast, California's delay reduction program required the adoption of time standards for timely disposition of filings. See Judicial Council of California (1991).

⁴⁰Sipes et al. (1980), pp. 37-38.

⁴¹Church et al. (1978), p. 5.

⁴²Pfeffer (1994); Hamel and Prahalad (1994). The gathering of good baseline data can have benefit in addition to being useful as a tool to convince individuals of the need to change existing behavior. Case management is made more difficult without good data-gathering systems for getting a complete picture of the current overall court situation, gauging progress in meeting goals, and monitoring of an individual case. Judicial Council of California (1991); Sipes et al. (1980), p. 37.

⁴³The Brookings Institution (1989).

⁴⁴The Federal Courts Study Committee (1990).

⁴⁵President's Council on Competitiveness (1991).

efforts fail because responsibilities are assigned broadly, and there is little guidance or oversight once changes have been mandated. Once the CJRA plans were adopted, there existed no effective mechanism for requiring that proposed changes in each court be implemented fully and widely. Courts with weak governance systems have greater difficulty in taking effective policy actions.⁴⁶ Other reform efforts have found that “the important thing is that the responsibility be lodged in an identifiable body or bodies to keep on top of the problem.”⁴⁷ It is also important to keep the process of change at an even keel among all the individual components. Although individual initiatives are laudable and necessary from the standpoint of developing new techniques, a uniform approach to case management among judges in a court reduces the burden on the court to train attorneys in differing procedures.⁴⁸

Clarifying responsibility and gaining commitment is often achieved by having members closest to the “work” of the system participate in determining how best to improve it. Of course, these members must have a shared understanding of the stated vision and be clear about their roles and responsibilities in achieving it. Many government bureaucracies and other large organizations establish task forces and advisory committees to ensure that implementation strategies reflect the concerns of all constituencies. CJRA’s advisory groups were created to meet this need and to foster the cooperation needed between bench and bar to identify causes of excess cost and delay, and design the cost and delay reduction program.⁴⁹ Participative change strategies allow organizations to use constituent knowledge in developing recommendations for how tasks and responsibilities should be assigned and carried out. Moreover, participative strategies facilitate implementation because they increase the probability that problems will be identified and recognized by all participants, realistic solutions will be generated, and potential obstacles anticipated. They also increase the likelihood that recommendations will be understandable and palatable to those who must adopt them.⁵⁰ However, the freedom handed down to the individual courts to shape their cost and delay reduction plans also provided a mechanism for limiting changes in the way the courts operated.

The CJRA mandate that each pilot district appoint an advisory group is consistent with the notion of participatory management. This group was charged with assessing the condition of its district and developing a set of recommendations for resolving problems in accordance with the principles specified in the act. As mentioned above, these advisory groups generally approached their tasks with care and dedication. Most of them interacted with the court while developing their report, and the judicial representation on the advisory groups should have helped facilitate court acceptance and implementation of the groups’ recommendations.

⁴⁶Handers (1977), p. 7.

⁴⁷Chapper et al. (1984), pp. ix–x.

⁴⁸Handers (1977), p. 9.

⁴⁹The roles of the legal professions, including both lawyers and judges, in reducing delay is discussed in Chapper et al. (1984), pp. 73–76.

⁵⁰Lawler (1986).

Despite the importance of participation, however, we found significant variation in the implementation of CJRA plans. It is likely that the absence of a strong court management system in some districts and the absence of well-defined accountability contributed to this outcome. Although, by necessity, the real responsibility for implementing innovations in the act fell upon individual judges, the judges were usually not monitored and held accountable for their implementation of the plan. Although assigning accountability and monitoring relevant performance measures may impose additional reporting costs, the burden of such costs may be minimized by making key individuals responsible for managing local level performance and by focusing on a small number of critical performance measures.⁵¹ At the minimum, a member of the clerk's office should be tasked with tracking progress toward meeting goals set and monitoring and reporting on whether all aspects of the plan are carried out. Obviously, the subordinate nature of the relationship between the court's clerk and the district judges means that the role of the clerk is limited to data collection, monitoring, and reporting. Correcting any problems found would still remain the responsibility of the judges themselves. As we discuss below, the advantages of increased accountability and open evaluation typically outweigh any costs they impose: "Unless a member of the court's staff has specific responsibility for [monitoring and reporting], they may be overlooked or, worse, turn from small problems of interpretation to major problems of policy and implementation."⁵²

Finally, whatever governance structure is established should also be *flexible enough to allow local innovation*. Allowing some local options or local control over the details of the change may stimulate experimentation that can ultimately produce more effective processes. No matter how well-thought-out the initial change program, every implementation effort is also a kind of experiment in which there are opportunities to learn from local variation and generate improvements to the initial plan.⁵³ However, to capitalize on such improvements, the governance structure should also incorporate integration mechanisms (i.e., regular conferences, integration teams, training programs) to disseminate needed information and communicate emerging knowledge.⁵⁴ For example, in some districts all judges regularly meet weekly or monthly, and such a governance structure can facilitate implementation of change.

The success of any implementation effort depends critically on the ability to measure performance and evaluate progress. As such, *meaningful performance measures must be created or modified to support the change program*.⁵⁵ Despite the best intentions of policymakers, however, even the most clearly stated legislation usually fails to translate easily into operational terms that provide useful guides to action at the local level. For members of the organization to act on legislative mandates and a well-stated vision, such legislative statements must be coupled with an integrated set

⁵¹Larkin and Larkin (1994).

⁵²Sipes et al. (1980), p. 28, discusses the need for a court to organize staff resources to achieve the successful implementation of a delay reduction program.

⁵³Kanter et al. (1992).

⁵⁴Mohrman et al. (1995); Mankin et al. (1996).

⁵⁵Kaplan and Norton (1996); Meyer (1994); Kanter et al. (1992).

of objectives and measures that describe the long-term drivers of success. As mentioned above, although the CJRA indicated that advisory groups should examine docket conditions, filing trends, resource demands, and indicators of cost and delay, it did not specify standard measures for tracking and comparison purposes. CJRA annual reassessment guidelines were similarly vague, again minimizing incentives for tracking progress and limiting the extent to which learning could take place within or across districts.

Existing performance metrics are often inadequate for tracking a fundamental change in management strategy.⁵⁶ Evaluating traditional statistics, ratios, and accounting measures may do little more than confuse and overwhelm those trying to understand and change performance.⁵⁷ How then should measurement systems be designed to facilitate change?

Research suggests a few guiding principles. First, the purpose of the measurement system should be to help implementers, not just overseers, gauge progress. The measurement system should be a tool for telling the individuals implementing change when to take corrective action.⁵⁸ Periodic caseload reports, for example, “encourage a judge to control his docket better if it is less under control than that of other judges.”⁵⁹ This means the measures should be timely and should provide information that individuals can act on. Second, to facilitate implementation, at least two types of measures should be used in conjunction: “results measures” to evaluate efforts to achieve a result or improvement, and “process measures” to monitor the tasks and activities that produce a desired result. Last, only a small number of measures should be adopted. Too many measures cause too much time to be expended collecting data and detract from the measures that are most critical for guiding results.⁶⁰ Meaningful measures that accurately reflect or predict desired outcomes should be the focus.

Facilitating Behavior Change

Despite the importance of clearly defined performance measures and accountability, some people will resist their responsibilities. Several factors may facilitate behavioral change under these conditions. We discuss these below and suggest their potential application in the context of the federal judicial system.

First, the importance of leadership and communication cannot be overstated.⁶¹ *Leadership and a commitment to change must be embedded throughout the system.*⁶² For example, communicating the goals and objectives of the CJRA directly to judges

⁵⁶Kaplan and Norton (1996).

⁵⁷Larkin and Larkin (1994).

⁵⁸Meyer (1994).

⁵⁹Handers (1977), p. 13.

⁶⁰Larkin and Larkin (1994); Meyer (1994).

⁶¹Solomon and Somerlot (1987); Mahoney et al. (1988), p. 191.

⁶²Sipes et al. (1980), p. 25–26.

and court staff is not likely to fully change behavior in the desired way without additional support from the Judicial Conference and the districts' chief judges. Face-to-face communication and the chief judge's commitment to the goals of the act—as demonstrated by his or her behavior—serve to direct attention and reinforce legislative mandate. We must also note, however, that the chief judge in each district is *at best* “first among equals” and has limited means beyond leadership and persuasion to induce change. Complete abolition of any skepticism among the judges regarding a new program is neither possible nor desirable. However, the skepticism can be turned to approval if success is seen in the efforts of the other judges and if any pre-existing reservation about the details of the program is accompanied by an agreement that something needs to be done about the problem.⁶³ Courts that have a consensus about the desirability of changes, such as changes in case management, are in the best position to make changes in the external legal culture.⁶⁴

Judges and court staff are likely to look to the chief judge and the clerk of court to determine if proposed changes are “for real.”⁶⁵ Since change will not occur overnight, motivated district court leaders must be willing to take the long-term view and able to resist the normal tendency to undo unpopular or difficult changes.⁶⁶ Ensuring that changes in policies and procedures are applied uniformly by all the judges and staff in a jurisdiction could lessen the learning curve for attorneys and help prevent a Balkanization of a jurisdiction's practices.⁶⁷

A second strategy likely to facilitate behavioral change among judges is communicating relative performance. *Communicating relative performance of the local work area is a powerful mechanism for effecting change.* It directs attention and exposes less than optimum performance, thereby motivating improvement efforts.⁶⁸ Moreover, it indicates who is doing well, creating an avenue for information-sharing and the diffusion of successful practices. Judges are more apt to change existing behavior when it is demonstrated that their standard operating procedures are neither the norm for the court nor as effective as those employed by others on the bench.

Finally, *effective implementation efforts ensure alignment between all components of the existing system and the proposed reform.* Studies of organizational change clearly demonstrate that implementation is most successful when existing strategies, structures, policies, and procedures are modified to support a new direction or initiative.⁶⁹ Changes in any one major element of the system (what judges do, for example) typically necessitate complementary changes in other elements (what the clerk's office does, for example), and it is important to ensure that all elements are consistent and compatible.

⁶³Sipes et al. (1980), p. 26.

⁶⁴Mahoney et al. (1988), p. 191.

⁶⁵Larkin and Larkin (1994).

⁶⁶Mahoney et al. (1988), pp. 198–199.

⁶⁷Flanders (1977), p. 9.

⁶⁸Larkin and Larkin (1994).

⁶⁹Galbraith (1995).

As is the case in private sector change efforts, *increased education and training* could greatly facilitate change in the federal judicial system. For example, education for both the bench and the bar could address not only the details of the new policies but also the soundness of the reasons behind them as well as the seriousness of the court's commitment. The goal is to have the problem and the change in policy understood and taken seriously by the bench and the bar and by others who would be affected by the reforms. Education programs, as well as increased communication between bench and bar, could also provide opportunities to share effective implementation strategies and local innovation.⁷⁰

A final issue to consider in achieving alignment is the *need for additional resources* and funding. In addition to the funds required to support educational programs, some temporary augmentation of resources, in the clerk's office for example, may be needed during the initial period of the implementation effort.

Implementing change in the courts will never be easy. Yet, as will be seen in the following chapters of this report, how judges manage cases matters. Although no one strategy for implementing change provides a magic bullet for a seamless transition from policy to practice, taken together the principles and strategies discussed above could greatly improve the probability of successful change.

⁷⁰Mahoney et al. (1988), pp. 200-201, 203.

EFFECTS OF DIFFERENTIAL CASE MANAGEMENT

INTRODUCTION

This chapter describes how the CJRA principle of differential case management (DCM), of which special management of complex cases is a subset, affects time to disposition, costs, satisfaction, and views of fairness.

The essence of the DCM concept is that different types of cases need different types and levels of judicial management. One way to implement DCM is to create a number of discrete and well-structured approaches to case scheduling and management, followed by early assignment of cases to these approaches.¹ The assignment may be made in a variety of ways: The court may use objective criteria (such as type of case), the attorneys may choose the track into which their case will fit (subject to judicial review), or the judge may make the track assignment decision after initial case review. Within each track, judges use different case management techniques and schedules that are at least partially predetermined by the track assignment.

Before CJRA, courts were already tailoring their management of certain types of cases. All 20 districts had special management procedures for certain “minimal management” cases such as prisoner petitions,² Social Security appeals, government loan recovery, and bankruptcy appeals. These cases can be a substantial portion of a court’s docket and although they generally require little or no judicial management and so are not a focus of the CJRA, they do impose special requirements. Prisoners often file petitions on a pro se basis, and they are initially processed by a pro se law clerk using standardized procedures and schedules. These cases often involve review of a request for a waiver of court fees and require some correspondence and motions activity, but few need court appearances and judicial case management in the traditional sense. Other minimal-demand cases, such as government loan collection cases and appeals from denials of Social Security benefits, usually do not involve a

¹DCM is a concept that a number of state criminal and civil courts have adopted in recent years—particularly those flooded with cases. For an overview of differential case management, see Allicgro et al. (1993). Also see Bakke and Solomon (1989), pp. 17-21.

²A small fraction of prisoner cases are death penalty cases, which, in contrast to the typical minimal management case, are complicated and time consuming to resolve. Our 1991 sample includes only two such cases; there are none in the 1992-93 sample.

pretrial scheduling conference and go through the entire litigation process without much judicial management of any kind.

After CJRA, these “minimal management” types of cases were usually handled the same way as before. We interviewed judges and other court personnel, and reviewed each of the CJRA advisory groups’ reports and the districts’ plans, to assess the treatment of these cases in the pre- and post-CJRA periods. We found that if the district’s plan explicitly dealt with management of these cases, it usually formalized the preexisting procedures. In a few districts, the types of cases defined as minimal management or the precise program followed were modified slightly.

Minimal management cases are typically disposed of relatively quickly and cheaply. Median time to disposition is about six months, and the median reported number of lawyer work hours per litigant is about ten. In contrast, general civil cases with issue joined have a median time to disposition of about a year, and the median number of lawyer work hours per litigant is about 50. It is the general civil cases to which the CJRA is directed.

Since districts made few changes in their procedures for minimal management types of cases, and since almost none of these cases are managed using the policies and procedures that apply to general civil litigation and that are the focus of the CJRA, they could not inform our evaluation of the procedures of concern in the CJRA. For all of these reasons, we exclude the category of minimal management cases from our statistical analyses.

Minimal management cases aside, virtually all federal judges interviewed as part of the pilot program evaluation stressed that they have always managed their general civil cases individually and differentially. In support of this position, judges note that discovery is often limited, that some cases do not require Rule 16 conferences, and that many lawsuits end with little or no judicial involvement. At the opposite end of the spectrum, complex cases can receive specialized management within a framework enunciated in the federal *Manual for Complex Litigation*.³ In other words, there is a lot of intercase variation in procedure used by judges, and the variation is a manifestation of a tailored approach to case management that, in principle, is not unlike the objectives of the general differential case management concept. As noted previously, we call this approach to DCM, in which judges make case management decisions for general civil cases case by case, the “judicial discretion” model of case management.

After passage of CJRA, six of the ten pilot district plans replaced the judicial discretion model with a track model of differential case management. Using the act’s flexible definition of differential case management, four of the pilot districts interpreted the CJRA’s requirement as being fulfilled by a continuation of the judicial discretion model. Comparison districts, which had the option of not doing DCM, were even less likely to revise their prior judicial discretion model.

³Federal Judicial Center (1995).

Implementing a track model implies having separate tracks for different types of general civil cases, setting guidelines for managing the cases in each track, and assigning a nontrivial number of cases to each track. Of the six pilot districts that planned for a track model of DCM, three either did not assign any general civil cases to tracks, or assigned virtually all of them to the same standard track, within which they were managed individually. Two other pilot districts assigned only 1 or 2 percent of their cases to the complex track. In contrast, Pennsylvania (E) assigned 7 percent of its general civil cases to the complex track.⁴

No matter what tracks are established, every DCM tracking system must overcome the hurdle of deciding how to assign particular cases to particular tracks—a decision needed early in the life of a case if cost and time to disposition are to be affected as much as possible. Cases at either extreme of the complexity spectrum are relatively easy to fit into a track. For example, contrast a simple personal injury suit involving an automobile accident, few questions of liability, and well-defined damage claims, with a complex patent infringement suit, involving multiple parties, many documents, and a high potential for interrogatories and depositions. For such cases, the tracking decision—expedited and complex, respectively—can be made without much confusion or risk of error. But the majority of cases are less clear-cut, and tend to be grouped together in a middle or “standard” track. Since some of these cases will or should actually be handled in expedited fashion and others in complex fashion, some of the potential benefits of DCM tracking may well be lost if they are placed into the standard track and subject to a management approach that is not suited to them.

ASSESSMENT

Differential case management using the track model proved difficult to implement for general civil litigation cases. From the perspective of evaluation, the pilot program looked promising at the plan stage because six of the ten pilot districts planned to adopt a track model for their general civil cases instead of retaining the judicial discretion model. However, because only the Pennsylvania (E) pilot district implemented its tracks for all general civil cases and had over 2 percent of the cases assigned to the complex track, we really have only one district in which to evaluate tracking. That district also implemented other changes, and we cannot reliably separate the effects of the track system from the effects of the other changes. Consequently, we have no basis for evaluating how the track method of DCM affected time, cost, satisfaction, and views of fairness.

Interviews with judges and lawyers suggest some reasons for the lack of experimentation with and successful implementation of a tracking system of DCM for general civil litigation. They include (1) the difficulty in determining the correct track assignment for most civil litigation cases using data available at case filing or soon after; and (2) judges' desire to tailor case management to the needs of the case and to

⁴For details of each district's CJRA plan and its implementation, see Kakalik et al. (1996a).

their style of management rather than having the track assignment provide the management structure for a category of cases.

With respect to the difficulty in determining the correct track assignment for a case, our statistical analysis indicates that the objective data available at the time of filing (such as nature of suit category, origin, jurisdiction, and number of parties) are not particularly good predictors of either time to disposition or cost of litigation.⁵ They apparently do not capture the real complexity of the case very well. This does not mean that a track system is not viable; rather, it suggests that, if a track model is to be implemented, decisions about track assignments should be supplemented with subjective information from the lawyers or judge.

⁵See Appendices D and E for details.

EFFECTS OF EARLY JUDICIAL CASE MANAGEMENT

INTRODUCTION

Early judicial case management includes the CJRA principle of early and ongoing judicial control of pretrial processes, as well as the CJRA optional technique of requiring that counsel jointly present a discovery/case management plan at the initial pretrial conference.

All the courts' plans accepted the principle of early and ongoing judicial control of the pretrial process. However, case management styles varied considerably between districts, and between judges in a given district.¹

Before CJRA, only one district in our study required that counsel jointly present a discovery/case management plan at the initial pretrial conference, although at least one other district required attorneys to confer before the first pretrial conference to attempt to agree on a scheduling order.² Four of the ten pilot districts adopted this technique in their CJRA plan, and nine of the other pilot and comparison districts later adopted it when the December 1993 F.R.Civ.P. 26(f) changes were made.

To conduct our evaluation, we needed information on what type of case management was actually applied to each case. To get that information, we used more than 10,000 court dockets to learn what events took place on what dates for each case. From the dockets, we could learn when a schedule was set for the case, when a trial date was set, when a judicial conference was held (status, scheduling, case management, or "Rule 16"), when a status report or joint discovery/case management plan was filed, and usually when any referral occurred for alternative dispute resolution.³ We note that the specific management style used by the judge is not shown on the docket (for example, what topics are covered in conferences, in what depth, with what degree of managerial activeness).

¹For details of each district's CJRA plan and its implementation, see Kakalik et al. (1996a).

²See Form 35 of F.R.Civ.P. 26(f) for an example of a possible discovery/case management plan. We consider a discovery/case management plan to include more than the typical scheduling order, although in some districts they may be functionally equivalent.

³Some districts maintain information on ADR referrals in a separate data system, to which we had access.

In our statistical analyses, we defined early judicial management as any schedule, conference, status report, joint plan, or referral to ADR management procedures within 180 days of case filing. This definition gives time for nearly all cases to have service and answer or other appearance of the defendants (which legally can take up to six months), so issue is joined and it is appropriate to begin management if the judge wants to do so. We also explored alternative definitions of “early” using time periods other than six months (four to eight months), with results similar to those reported in this chapter.

As discussed in Chapter Two, we base our assessment of early judicial management on data from general civil litigation cases with issue joined. We analyzed all such cases and also the subset of these cases that took longer than nine months to disposition.

ESTIMATED EFFECTS OF EARLY CASE MANAGEMENT

Before CJRA, 58 percent of the general civil litigation cases in the pilot and comparison districts received early judicial management; that number rose to 65 percent in 1992–93. How did early management affect the time to disposition, litigation costs, satisfaction, and views of fairness?

Time to Disposition

Early management is statistically significant in predicting shorter time to disposition. For cases that survive at least nine months, the estimated reduction in the median time to disposition is 1.5 to two months if any judicial case management took place within six months of filing.⁴

We explored the component procedures of early management separately. Setting a schedule for trial early was the most important component of early management in terms of significantly reducing time to disposition. Cases in which a trial schedule was set before day 180 had statistically significantly shorter times to disposition.⁵ We estimate that if a trial schedule is set as part of the early management package, the median time to disposition is reduced by an *additional* 1.5 to two months. We found no statistically significant effects for pretrial conferences or for mandatory arbitration as part of the early management package, and we had mixed results for schedules in general and joint discovery/case management plans when we used 1992–93 data.

⁴For details of our statistical analyses of the effects of early management on time to disposition see Appendix D, especially Table D.9. The 95 percent confidence interval on the two-month estimate is approximately one to three months.

⁵Note that for setting the trial schedule early, our estimated effect is the difference in time to disposition between (1) using early management that includes setting the trial schedule early and (2) using early management that does not involve setting the trial schedule early.

Our results for early management using our 1991 data are similar to the results from the 1992–93 data, although there is some difference in the actual magnitude of the estimates.

Figure 5.1 graphically displays the distribution of time to disposition for cases with and without early judicial management within six months after filing. The effect of early management is seen in the percentage of cases closed from about seven to 18 months time to disposition. Note that the “not managed early” line is higher for cases that closed within six months because the “not managed early” category includes at least 15 percent of the cases that close almost immediately after issue is joined, and before the judge has a chance to manage them.

Figure 5.2, which shows the distribution of time to disposition for cases that receive early management with and without setting a schedule for trial early, demonstrates the statistical finding that setting a trial schedule early further shortens time to disposition.

One of the CJRA’s specific techniques is requiring a joint discovery/case management plan. We note no consistently significant change in the predicted time to disposition as a result of early requirement for a joint plan.

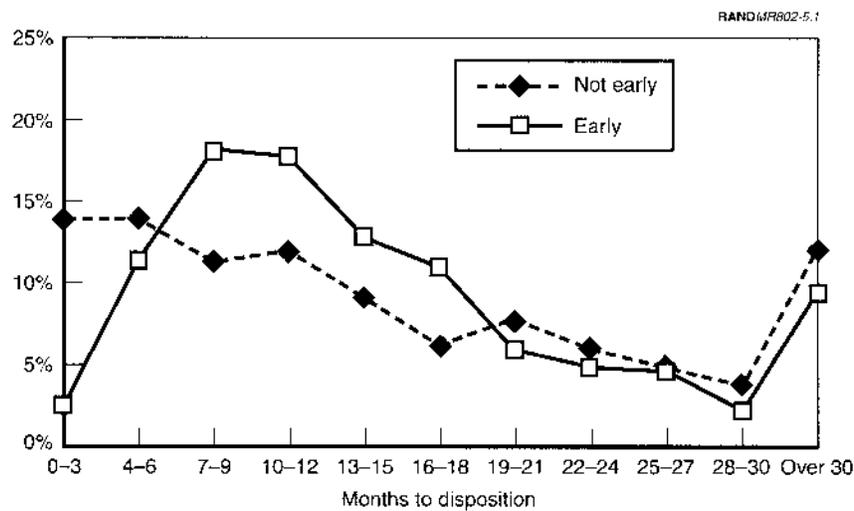


Figure 5.1—1992–93 Time to Disposition with and Without Early Judicial Management: General Civil Cases with Issue Joined

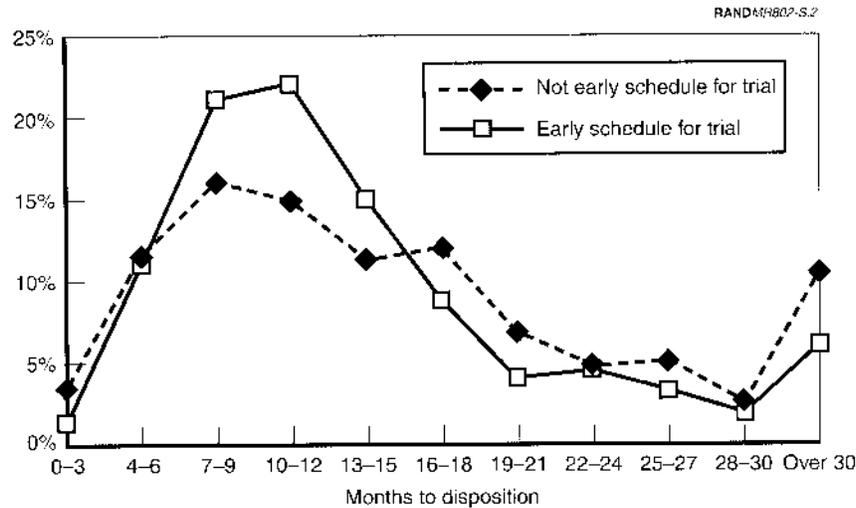


Figure 5.2—1992–93 Time to Disposition with and Without Early Schedule for Trial: General Civil Cases with Issue Joined and Early Management

Litigation Costs

Early management is associated with reduced time to disposition, but at a cost. Early management is significantly related to increased lawyer work hours per litigant. For cases that survive at least nine months and receive early management, we estimate approximately a 20-hour increase in the lawyer work hours. That estimate is based on our statistical analysis, controlling for differences in case characteristics and other factors.⁶

The costs to litigants were also higher in dollar terms, and in litigant hours spent, when cases were managed early (see Tables 5.1 and 5.2).

We again explored the component procedures of early management separately. If a trial schedule is set as part of the early management package, lawyer work hours per case fall by seven, but this is not a statistically significant difference. There also are no statistically significant differences for any of the other components of early management.

The CJRA technique of requiring a joint discovery/case management plan had no significant effect on predicted lawyer work hours. These findings hold true even if we consider only complex cases.

⁶For details of our statistical analyses of the effects of early management on lawyer work hours, see Appendix E, especially Table E.8.

Satisfaction

In our explorations of attorney satisfaction, we found that early management and setting a trial schedule early in the case—the early management policies that had the greatest effects on time to disposition and lawyer work hours—had no statistically significant effect on lawyer satisfaction.⁷ Litigant data showed mixed results for satisfaction with early management, higher in the pre-CJRA sample and lower in the post-CJRA sample (see Tables 5.1 and 5.2).⁸

Furthermore, we find no statistically significant differences in attorney satisfaction for cases receiving any of the components of early management compared to cases not receiving the component. However, we did find that a higher degree of case management, which may or may not have been early, is associated with significantly higher lawyer satisfaction.

Views of Fairness

We found no consistent statistically significant effects of early judicial case management on attorney views of fairness.⁹ A very high percentage of attorneys (93 percent in 1992–93 and 91 percent in 1991) report that case management was fair. Given the lack of variability in the data, the lack of significant effects is not surprising.

Tables 5.1 and 5.2 summarize information on various measures for the 1991 and the 1992–93 samples, respectively, for cases with and without early judicial management. Tables 5.3 and 5.4 present the same information for cases that receive early management with and without setting a schedule for trial early. The numbers in the tables, which reflect actual survey responses from the sample cases, corroborate the results of the statistical analysis.

ASSESSMENT

Early judicial case management is associated with both significantly reduced time to disposition and significantly increased lawyer work hours. Our sample data show that the costs to litigants were also higher in dollar terms, and in litigant hours spent, when cases were managed early. These results debunk the myth that reducing time to disposition will necessarily reduce litigation costs. There is no statistically significant difference in lawyer satisfaction or views on fairness as a result of early management. Litigant data showed mixed results for satisfaction with early management, higher in one sample and lower in the other sample.

⁷For details of our statistical analyses of the effects of early management on satisfaction, see Appendix F.

⁸Because of the low litigant response rate and possibly biased responses, as discussed in Appendix B, we did not conduct inferential statistical analyses using litigant data.

⁹For details of our statistical analyses of the effects of early management on views of fairness, see Appendix G.

Table 5.1
Cases with and Without Early Judicial Management: 1991 Closed General Civil Cases with Issue Joined

Variable	All Closed		Closed After 9 Months		Comments
	Early	Not Early	Early	Not Early	
% with procedure	58	42	57	43	7% less managed early than in 1992-93
Median days to disposition	379	495	487	654	Early is significantly faster
% satisfied with management					
Lawyers	73	69	74	68	Not a significant difference
Litigants	53	50	54	46	
% view management as fair					
Lawyers	91	90	89	88	Not a significant difference
Litigants	77	72	76	70	
Median lawyer work hours per litigant	60	50	86	66	Early is significantly higher
Median cost per litigant (\$1,000)	9	7	13	10	
Median litigant hours spent	40	30	50	40	

NOTES: Early management includes any schedule, conference, status report, plan, or ADR referral within six months of filing. All closed cases include some that close before early management is feasible; those that close after nine months provide a better comparison.

Lawyer work hours may increase as a result of early management because lawyers need to respond to the court's management—for example, talking to the litigant and to the other lawyers in advance of a conference with the judge, traveling, spending time waiting at the courthouse, meeting with the judge, and updating the file after the conference. In addition, once judicial case management has begun, a discovery cutoff date has usually been established, and attorneys may feel an obligation to begin discovery. Doing so could shorten time to disposition, but it may also increase lawyer work hours on cases that were about to settle when the judge began early management.

In terms of predicting reduced time to disposition, setting a schedule for trial early was the most important component of early management. Including early setting of trial date as part of the early management package provides an additional reduction in time to disposition, but no further significant change in lawyer work hours. About three-fourths of both judges and lawyers responding to our surveys said that setting an initial trial date at or around the time of the initial pretrial conference was generally desirable; the other one-fourth said it was rarely desirable.

Our assessment of the effects of early judicial management was based on a six-month definition of "early." We also explored other shorter definitions of early, with similar

Table 5.2
Cases with and Without Early Judicial Management: 1992–93 Closed General Civil Cases with Issue Joined

Variable	All Closed		Closed After 9 Months		Comments
	Early	Not Early	Early	Not Early	
% with procedure	65	35	68	32	7% more managed earlier than in 1991
Median days to disposition	349	328	455	530	Early is significantly faster
% satisfied with management					
Lawyers	77	71	76	77	Not a significant difference
Litigants	51	59	50	60	
% view management as fair					
Lawyers	93	93	92	93	Not a significant difference
Litigants	74	78	74	78	
Median lawyer work hours per litigant	71	40	95	60	Early is significantly higher
Median cost per litigant (\$1,000)	9	5	12	9	
Median litigant hours spent	44	40	50	40	

NOTES: Early management includes any schedule, conference, status report, plan, or ADR referral within six months of filing. The time to disposition on these closed cases (93 percent of the sample) is less than it would be if 100 percent were closed. All closed cases include some that close before early management is feasible; those that close after nine months provide a better comparison.

statistical results. This finding suggests that the *fact* of management adds to the lawyer work hours, not the “earliness” of the management. However, starting earlier than six months means that more cases would be managed because more cases are still open, so more cases would incur the predicted increase in lawyer work hours.

Early management involves a tradeoff between shortened time to disposition and increased lawyer work hours. However, to preview findings presented later in this report, we note that if early management is packaged with shortened discovery cutoff, the increase in lawyer work hours predicted by early management is offset by the decrease in lawyer work hours predicted by judicial control of discovery. We estimate that under these circumstances, litigants pay no significant cost penalty for reduced time to disposition.

Our analysis suggests consideration of the following approach to early management of general civil litigation cases by courts and judges not currently using this approach, and reemphasis by courts and judges that are currently using it. The powers already exist under the Federal Rules of Civil Procedure to use this approach, and they are currently being used by many judges on a discretionary basis.

Table 5.3

Cases with and Without Early Schedule for Trial Date: 1991 Closed General Civil Cases with Issue Joined and Early Judicial Management

Variable	All Closed		Closed After 9 Months		Comments
	Early Management with Schedule for Trial	Early Management Without Schedule for Trial	Early Management with Schedule for Trial	Early Management Without Schedule for Trial	
% with procedure	39	61	39	61	Same percentage of early managed cases as in 1993
Median days to disposition	366	391	434	524	Early management with trial date is significantly faster
% satisfied with management					
Lawyers	76	72	75	73	Not a significant difference
Litigants	51	55	51	56	
% view management as fair					
Lawyers	90	91	89	89	Not a significant difference
Litigants	74	79	71	79	
Median lawyer work hours per litigant	70	60	86	86	Not a significant difference
Median cost per litigant (\$1,000)	11	8	15	12	
Median litigant hours spent	36	40	40	50	

NOTES: Early management includes any schedule, conference, status report, plan, or ADR referral within six months of filing. Early schedule for trial date means the date on which a schedule was set for trial was within six months of filing. All closed cases include some that close before early management is feasible; those that close after nine months provide a better comparison.

- Have a clerk monitor cases that do not yet have issue joined to ensure that deadlines for service and answer are met; if those deadlines are missed, judicial action could be initiated to dispose of the case.
- For cases that have issue joined, wait a short time after the joinder date, perhaps a month, to see if the case terminates, then begin judicial case management.
- Include setting of a trial date as part of the early management package, and adhere to that date as much as possible.

For nearly all general civil cases, these guidelines should result in judicial case management beginning within six months or less after case filing.

Table 5.4

**Cases with and Without Early Schedule for Trial Date: 1992–93 Closed General Civil Cases
with Issue Joined and Early Judicial Management**

Variable	All Closed		Closed After 9 Months		Comments
	Early Management with Schedule for Trial	Early Management Without Schedule for Trial	Early Management with Schedule for Trial	Early Management Without Schedule for Trial	
% with procedure	39	61	39	61	Same percentage of early managed cases as in 1991
Median days to disposition	333	363	407	484	Early management with trial date is significantly faster
% satisfied with management					
Lawyers	78	76	76	76	Not a significant difference
Litigants	47	53	43	54	
% view management as fair					
Lawyers	93	93	92	92	Not a significant difference
Litigants	75	73	71	75	
Median lawyer work hours per litigant	76	60	97	95	Not a significant difference
Median cost per litigant (\$1,000)	12	8	14	11	
Median litigant hours spent	50	40	50	50	

NOTES: Early management includes any schedule, conference, status report, plan, or ADR referral within six months of filing. Early schedule for trial date means the date on which a schedule was set for trial was within six months of filing. The time to disposition on these closed cases (93 percent of the sample) is less than it would be if 100 percent were closed. All closed cases include some that close before early management is feasible; those that close after nine months provide a better comparison.

EFFECTS OF DISCOVERY MANAGEMENT

INTRODUCTION

Discovery management policies include the CJRA principles of early and ongoing judicial control of pretrial processes, exchanging information early without formal discovery, and requiring good-faith efforts to resolve discovery disputes before filing motions.¹ The policy of requiring lawyers to jointly prepare a discovery/case management plan early in the case was assessed in Chapter Five.

Before CJRA, most districts left court control of the volume and timing of discovery to the judge in each case; CJRA had little effect on this arrangement. Before CJRA, most pilot and comparison districts had a local rule that limited the number of interrogatories and requests for admission, but none limited the number of depositions and only one limited the time per deposition. After CJRA, one pilot and one comparison district adopted a new limit on deposition length, and two comparison districts adopted new limits on the number of depositions.

CJRA brought about substantial change in early disclosure. Only one district required it before CJRA;² after CJRA, all pilot and comparison districts have adopted one of five approaches providing either voluntary or mandatory exchange of information by lawyers, sometimes only for specified types of cases. Three pilot and two comparison districts adopted the voluntary exchange model, which encourages lawyers to cooperate in exchanging information. Three pilot districts and one comparison district followed a mandatory exchange model for a limited subset of cases and a voluntary model on other cases. Two pilot districts and one comparison district required lawyers to mandatorily disclose certain information, including anything bearing significantly on their sides' claims or defenses. Two other pilot districts and one other comparison district have a similar mandatory requirement, but they apply it to all information bearing significantly on both sides' claims or defenses.

After our sample cases were selected, four pilot districts switched from their initial early disclosure procedure to follow the December 1993 revised F.R.Civ.P. 26(a)(1),

¹For details of each district's CJRA plan and its implementation, see Kakalik et al. (1996a).

²At least one other district required attorneys to confer before the first pretrial conference to attempt to agree on a scheduling order.

which requires the mandatory exchange of information relevant to disputed facts alleged with particularity in the pleadings, plus information on damages and insurance. Six comparison districts also are following the revised Rule 26(a)(1). The ten other pilot and comparison districts have decided to “opt out” and are not following the revised Rule 26(a)(1). Some districts opted out to retain their pilot program disclosure rules, some of which were more stringent in their disclosure requirements than the revised Rule 26(a)(1).

When one party does not comply with mandatory early disclosure, the other side's lawyer may ignore the problem, make a formal discovery request, or file a motion requesting the court to force compliance. According to our analysis of dockets on over 5,000 cases, and according to judges we have interviewed in pilot and comparison districts that implemented their plans in December 1991, such compliance motions are extremely rare. Despite the dire warnings of critics of early mandatory disclosure, we did not find any explosion of ancillary litigation and motion practice related to disclosure in any of the pilot or comparison districts using mandatory disclosure.

The requirement that lawyers certify good-faith efforts to resolve discovery disputes before filing motions has undergone little change. All but one district had rules governing this before CJRA; these have been continued or strengthened.

To conduct the evaluation, discovery management information was obtained from over 10,000 court dockets. For each case, we learned if a discovery schedule had been set, and if so when and how much time was allowed between the date the schedule was set and the date of discovery cutoff. From the dockets, we also learned if any discovery motions had been filed. However, details of discovery management, such as limitations on depositions or requirements for sequencing of discovery, are usually not recorded on the docket and so could not be analyzed. We also surveyed the lawyers on each case to see if early disclosure of information was made without a formal discovery request.

For reasons discussed in Chapter Two, we assess the effects of discovery management using data from general civil litigation cases with issue joined.

ESTIMATED EFFECTS OF DISCOVERY MANAGEMENT

Discovery was subject to more management in 1992–93 after CJRA was passed. In addition to the new mandatory early disclosure requirements in some districts, the median district times to discovery cutoff were shortened in some districts. For example, in 1991 the fastest and slowest districts' median days from schedule to discovery cutoff were 100 and 274 days, respectively. In 1992–93, these medians had fallen to 83 and 217 days, respectively.

Time to Disposition³

Discovery Control: Too few districts limited depositions to support meaningful statistical analyses of this aspect of discovery control.

Our data show almost no statistical evidence that limiting interrogatories affects time to disposition.

A district's median days to discovery cutoff is a statistically significant predictor of time to disposition. Analysis of our data predicts that the longer the time from the setting of a discovery schedule to the discovery cutoff date, the longer the time to disposition of the case. We estimate approximately a 1.5-month reduction in the median time to disposition for cases that survive at least nine months if the district median discovery cutoff is reduced from 180 days to 120 days.⁴

Figure 6.1 shows the distribution of time to disposition for cases in the 1992–93 sample with shorter and longer times from discovery scheduling to cutoff. “Shorter” means the cases in the ten study districts with the shortest median discovery time to cutoff. Cases with shorter discovery cutoff are more likely to close early.

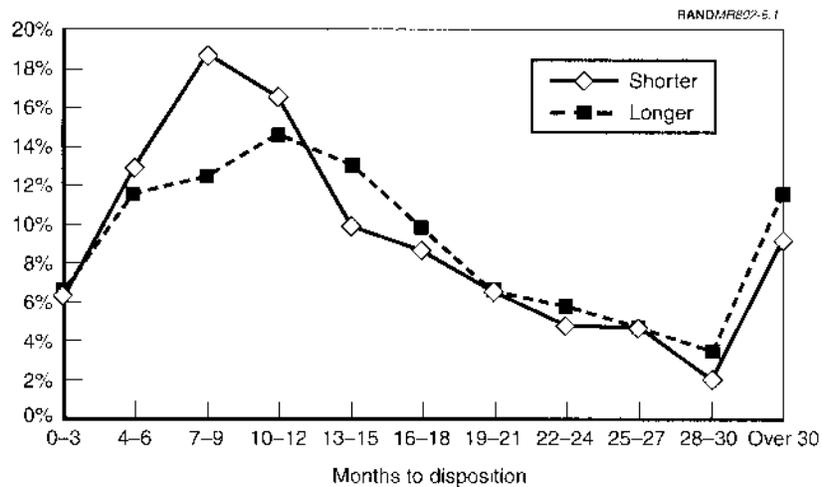


Figure 6.1—1992–93 Time to Disposition with Shorter and Longer Days from Discovery Schedule to Cutoff: General Civil Cases with Issue Joined

³For details of our statistical analyses of the effects of discovery management on time to disposition see Appendix D.

⁴See Appendix D, Table D.9, and related text.

Early Disclosure: We find no statistically significant difference in time to disposition between cases from districts that have a policy of mandatory disclosure and those that do not. Furthermore, we found that cases from districts with a policy of mandatory disclosure of information bearing on *both* sides of the case did not differ significantly in terms of time to disposition from other cases. We also found that a district policy encouraging voluntary early disclosure had no statistically significant effect on time to disposition compared to cases from districts with no general policy on early disclosure.⁵

Using 1992–93 data, we find that cases where the attorneys reported an early disclosure of relevant information were not statistically significantly different from other cases in terms of time to disposition. In 1991, however, individual cases where parties disclosed relevant information tended to be shorter than other cases.

Very few districts had mandatory early disclosure policies in 1991, but between 1991 and 1992–93 many districts implemented such policies. Thus early disclosure cases in the 1991 sample primarily reflect voluntary early disclosure, but early disclosure cases in the 1992–93 sample also include mandatory disclosure. The difference between the two samples makes interpreting findings difficult. For example, cases where the parties volunteer early disclosure could differ from other cases because the parties might be more motivated to settle or the lawyers might have different approaches to litigation.

Good-Faith Efforts in Resolving Discovery Disputes: Good-faith efforts to resolve discovery disputes have no significant effect on time to disposition.

Litigation Costs⁶

Discovery Control: A district policy on limiting interrogatories does not predict significantly different lawyer work hours.

Of all the policy variables we investigated as possible predictors of reduced lawyer work hours, only judicial management of discovery seemed to produce the desired effect. Reported lawyer work hours significantly decrease as the district median time to discovery cutoff gets shorter, using the 1992–93 data. We estimate approximately a 17-hour reduction in lawyer work hours for cases that survive at least nine months if the district median discovery cutoff is reduced from 180 days to 120 days.⁷ When we use the 1991 data, we also see a reduction in lawyer work hours, but the reduction is not significant. The data on costs to litigants in dollar terms, and in litigant hours spent, appear consistent with the data on lawyer work hours.

⁵Some districts had policies on early disclosure for a very limited number of cases (a small minority of their general civil litigation cases). We considered these districts to have no general policy of early disclosure and included them in our comparison group for studying the effects of voluntary and mandatory early disclosure.

⁶For details of our statistical analyses of the effects of discovery management on lawyer work hours, see Appendix E.

⁷See Appendix E, Table E.8, and related text.

Early Disclosure: Our 1992–93 sample data indicate that attorneys representing cases in districts with a *mandatory disclosure policy of any type* have work hours that are not statistically significantly different from hours worked by attorneys in other districts.⁸

Nor are work hours significantly affected by a district policy encouraging voluntary early disclosure.

We also find no statistically significant difference in lawyer work hours between cases where the attorneys reported disclosure of relevant information and cases where there was no early disclosure, using the 1992–93 data. However, in the 1991 data, early disclosure of information at the case level predicts fewer attorney work hours. As discussed above, early disclosure was almost always voluntary for cases in the 1991 sample, but it was mandatory for some districts' cases in the 1992–93 sample. Thus, in 1991, attorneys who voluntarily chose to disclose information may have been more willing to settle the case or less contentious in their litigation behavior. We believe that the 1992–93 data provide a more accurate picture of the effects of a mandatory disclosure policy.

Good-Faith Efforts in Resolving Discovery Disputes: We find no statistically significant effects of good-faith efforts on attorney work hours.

Satisfaction⁹

Discovery Control: We find no statistically significant relationship between the district median days to discovery cutoff and attorney satisfaction. Litigant data also show little difference in satisfaction between shorter and longer discovery cutoff (see Tables 6.1 and 6.2 below).¹⁰

Early Disclosure: A district policy of mandatory early disclosure corresponded to statistically significantly *lower* reported attorney satisfaction. A district policy of voluntary early disclosure is also associated with fewer satisfied attorneys, but our estimated effects are small and not statistically significant. However, for cases in which the attorneys report the actual early disclosure of information, they also report significantly *higher* satisfaction than attorneys from other cases.

Good-Faith Efforts in Resolving Discovery Disputes: Good-faith efforts to resolve discovery disputes had no statistically significant effect on lawyer satisfaction.

⁸However, we found that attorney work hours are significantly lower for the three districts that have a policy of *early mandatory disclosure of information bearing on both sides of the dispute*. The effect is significant in the sample of cases that survive at least 270 days. However, with only three districts using this particular policy, it is difficult to generalize from this statistical finding.

⁹For details of our statistical analyses of the effects of discovery management on satisfaction, see Appendix F.

¹⁰Because of the low litigant response rate and possibly biased responses, as discussed in Appendix B, we did not conduct inferential statistical analyses using litigant data.

Table 6.1
Cases in Districts with Shorter and Longer Time from Discovery Schedule to Cutoff:
1991 Closed General Civil Cases with Issue Joined

Variable	All Closed		Closed After 9 Months		Comments
	Districts with Shorter Time to Discovery Cutoff	Districts with Longer Time to Discovery Cutoff	Districts with Shorter Time to Discovery Cutoff	Districts with Longer Time to Discovery Cutoff	
Median days to discovery cutoff in 10 districts	100-175	176-274	NA	NA	Longer than in 1993
Median days to disposition	364	482	306	594	Shorter cutoff time is significantly faster
% satisfied with management					
Lawyers	69	74	69	73	Not a significant difference
Litigants	51	53	49	51	
% view management as fair					
Lawyers	90	91	88	90	Not a significant difference
Litigants	74	76	72	75	
Median lawyer work hours per litigant	53	60	80	75	Not a significant difference
Median cost per litigant (\$1,000)	8	8	12	12	
Median litigant hours spent	32	35	50	40	

NOTES: Days to discovery cutoff in district means days from first schedule to first discovery cutoff, without consideration of continuances. All closed cases include some that close before early management is feasible; those that close after nine months provide a better comparison.

Views of Fairness¹¹

Discovery Control: We find no statistically significant effects between district median days to discovery cutoff and views of fairness.

Early Disclosure: We find no statistically significant relationships between early disclosure and reported views on the fairness of case management.

Good-Faith Efforts in Resolving Discovery Disputes: We find no significant effects on fairness due to good-faith efforts.

Tables 6.1 and 6.2 summarize information on various measures for the 1991 and the 1992-93 samples, respectively, for cases from districts with shorter and longer discovery time to cutoff. The numbers in the tables, which reflect actual survey

¹¹For details of our statistical analyses of the effects of discovery management on views of fairness, see Appendix G.

Table 6.2
Cases in Districts with Shorter and Longer Time from Discovery Schedule to Cutoff:
1992–93 Closed General Civil Cases with Issue Joined

Variable	All Closed		Closed After 9 Months		Comments
	Districts with Shorter Time to Discovery Cutoff	Districts with Longer Time to Discovery Cutoff	Districts with Shorter Time to Discovery Cutoff	Districts with Longer Time to Discovery Cutoff	
Median days to discovery cutoff in 10 districts	83–177	178–217	NA	NA	Shorter than in 1991
Median days to disposition	319	368	468	470	Shorter cutoff is significantly faster. Not able to see in bivariate table.
% satisfied with management					
Lawyers	72	78	72	80	Not a significant difference
Litigants	51	57	53	55	
% view management as fair					
Lawyers	93	93	91	93	Not a significant difference
Litigants	73	78	74	76	
Median lawyer work hours per litigant	60	56	82	75	Shorter cutoff has significantly fewer hours. Not able to see in bivariate table.
Median cost per litigant (\$1,000)	9	8	11	11	
Median litigant hours spent	47	40	50	40	

NOTES: Days to discovery cutoff in district means days from first schedule to first discovery cutoff, without consideration of continuances. The time to disposition on these closed cases (93 percent of the sample) is less than it would be if 100 percent were closed. All closed cases include some that close before early management is feasible; those that close after nine months provide a better comparison.

responses from the sample cases, generally corroborate the results of the statistical analysis. However, one is not able to see the reduction in lawyer work hours predicted by shorter discovery cutoff in the bivariate tables, because the districts differ on factors other than discovery cutoff time. Our multivariate analyses adjust for those other factors, and the bivariate table does not.

ASSESSMENT

Discovery is a major factor influencing both the length and the cost of litigation.

Shorter time from setting a discovery schedule to discovery cutoff is associated with both significantly reduced time to disposition and significantly reduced lawyer work hours. If a district's median time to discovery cutoff is reduced from 180 days to 120 days, the estimated median time to disposition falls by one month. In addition, lawyer work hours fall by about 17 hours, about 25 percent of their median work hours. These benefits are achieved without any significant change in attorney

satisfaction or views of fairness. The data on costs to litigants in dollar terms, and in litigant hours spent, appear consistent with the data on lawyer work hours. Litigant data also show little difference in satisfaction between shorter and longer time to discovery cutoff.

Early disclosure of information without a formal discovery request had little effect. Neither voluntary nor mandatory early disclosure significantly affects time to disposition or lawyer work hours. The findings on satisfaction and fairness suggest that lawyers do not like a district policy of mandatory early disclosure. They were both significantly less satisfied and a little less prone to call management fair when this district policy existed. However, they also tended to be significantly more satisfied and a little more prone to call management fair when they actually participated in early disclosure on their case.

Findings from a recent survey of about 1,000 attorneys by the American Bar Association's Litigation Section were similar to ours: "Analysis of the survey results suggests that Rule 26(a)(1) disclosure has not had a significant impact on federal civil litigation. To the extent that it has had any measurable effects, most are negative. The survey provided no evidence that, at the one year mark, disclosure had reduced discovery costs or delays. Nor do the responses suggest that disclosure has reduced conflict between adversaries during the discovery process. Consequently, during its first year of implementation, disclosure has not resulted in the systemic improvements for which its proponents had hoped."¹²

RAND's lawyer surveys indicate that when early disclosure was made for cases in the 1992-93 sample, it was "full disclosure" 57 percent of the time, and "pro forma" disclosure 43 percent of the time.¹³ For general civil cases with issue joined, lawyers report more disclosure when it is mandatory (60 percent of the cases in mandatory disclosure districts, versus 45 percent in voluntary disclosure districts and 40 percent in districts with no disclosure policy). Both our interviews and our docket analysis indicate that motions related to mandatory disclosure are extremely rare.

When judges were asked their opinions about discovery management on the cases in our 1992-93 sample,¹⁴ the vast majority responded that such management was generally desirable (96 percent in favor of setting discovery limits; 89 percent in favor of requiring early disclosure; and 98 percent in favor of good-faith efforts before filing discovery motions).

When lawyers were asked their opinions on discovery management on those same cases,¹⁵ a majority responded that such management was generally desirable (86 percent in favor of setting discovery limits; 71 percent in favor of requiring early dis-

¹²Blaner et al. (1996), p. 1.

¹³See Appendix I, item 5.

¹⁴See Appendix I, item 11.

¹⁵See Appendix I, item 6.

closure;¹⁶ and 96 percent in favor of good-faith efforts before filing discovery motions).

¹⁶The PA(E) advisory group also conducted a survey of about 4,000 lawyers regarding the early mandatory disclosure procedures in that district, with results that were very similar to ours. This district's procedures stay discovery until both sides have completed mandatory disclosure of information likely to "bear significantly on the claims and defenses," plus other items such as names of individuals with information and any insurance. Of the 1,000 plus attorneys responding, over 60 percent felt that some rule mandating self-executing disclosure should remain in effect. Judges were 85 percent in favor of such disclosure. When asked about compliance, over 90 percent of lawyers said they themselves had complied more than minimally, and that over two-thirds of their opponents had complied more than minimally (Landis et al., 1996).

The NY(E) advisory group also surveyed lawyers regarding early mandatory disclosure for cases filed after the plan was adopted (Wesely et al., 1994, pp. 5-6.) Their annual report indicated: "Survey results at this stage are neither a ringing endorsement, nor a condemnation, of mandatory disclosure. About half the respondents said that mandatory disclosure improved pretrial discovery, and about half said that there was no change. A majority also said that mandatory disclosure had made either no contribution or a slight contribution to easing the problems of undue cost and unnecessary delay. On the other hand, an overwhelming majority said that mandatory disclosure had no negative effects on pretrial discovery." "A majority (55 percent) would make mandatory disclosure a permanent part of the local rules, and an additional 23 percent would make mandatory disclosure a permanent part of the local rules if modifications were made." "It appears from these data that the parade of horrors predicted by some critics of mandatory disclosure has not come to pass. On the other hand, it is not clear the extent to which mandatory disclosure has improved the operation of pretrial discovery, if at all. The vast majority of respondents have had little experience with mandatory disclosure."

EFFECTS OF ALTERNATIVE DISPUTE RESOLUTION

INTRODUCTION

One of the CJRA principles requires pilot districts to authorize, and other districts to consider authorizing, referral of appropriate cases to some type of alternative dispute resolution program. And one of the CJRA techniques requires courts to consider a neutral evaluation program. Because neutral evaluation is generally considered to be a form of ADR, we include that technique here.

The act fails to define the term "alternative dispute resolution" with specificity, but mentions a number of approaches such as neutral evaluation, mediation, mini-trials, and summary jury trials. Each of these ADR approaches can be designed in many different ways.¹

Some judges and attorneys consider settlement discussions with a judicial officer and requirements that lawyers certify that they have conducted private settlement efforts to be forms of ADR, although the CJRA does not include settlement conferences in the ADR language. In this chapter, we focus on ADR other than settlement conferences conducted by judicial officers.

The rationale for all ADR programs is, of course, the hope that they are faster, cheaper, and more satisfactory than formal court adjudication. Although past research has not confirmed all of these putative benefits, it does seem to suggest that litigants are more satisfied when ADR has taken place, even if they do not settle their case at that time.² Perhaps this is because they feel they have had their "day in ADR court" without the expense of a formal court trial. However, because most ADR is nonbinding and because the vast majority of cases do not go to trial anyway, ADR primarily offers an alternative mode of settlement, not an alternative to trial.

¹For discussions of ADR programs and their design features, see, for example, Plapinger and Shaw (1992); Sander (1991); Plapinger et al. (1993); Wilkinson (1993); Hensler (1986, 1990, 1994); Resnik (1995); Lind et al. (1989); Rolph (1984); and Rolph and Moller (1995).

²For a review of several recent empirical studies on arbitration, for example, see Keilitz (1994). She concluded that "the evidence is mixed on arbitration's effects on the pace of litigation," "the small amount of data gathered has not shown cost reductions," "attorneys view arbitration as fair and satisfactory, but traditional litigation also fares well in their ratings," and "litigants who participated in an adjudicative hearing were more satisfied with the litigation experience than were those whose cases settled without an arbitration hearing or trial" (pp. 41, 42).

A major issue is what type of ADR to use, since several types are mentioned in the act.³ Another major issue raised by the act's ADR referral language is whether courts should move from the current prevalent procedure of purely "voluntary" ADR to a practice of compelling parties to participate in "mandatory," nonbinding ADR techniques before moving to trial.

Implementation of ADR in the pilot program was complicated by an earlier effort to test mandatory ADR programs in federal court in which Congress authorized ten district courts to use mandatory arbitration methods, and ten other district courts to use voluntary arbitration methods.⁴ These districts have implemented their arbitration programs in various alternative ways. The Federal Judicial Center evaluated the mandatory arbitration program, and after "generally favorable findings" recommended authorizing arbitration in all federal district courts, to be mandatory or voluntary at the discretion of the court.⁵ The Federal Judicial Center also studied the voluntary arbitration program and found the caseloads to be lower than for mandatory programs (programs allowing parties to opt out had caseloads comparable to the smallest mandatory arbitration programs, whereas programs in which parties had to opt in to arbitration had "almost no cases").⁶ In 1994, Congress extended the court-annexed arbitration program in only these 20 district courts but did not expand it to others.⁷ This means that pilot districts, like GA(N), that wanted arbitration in their CJRA plan were prohibited from implementing ADR involving arbitration.⁸

Both before and after CJRA, all pilot and comparison districts allowed ADR of one kind or another. Table 7.1 summarizes the percentage of all civil cases filed that were referred by each district to mandatory and voluntary ADR programs of various types.⁹ Pilot and comparison districts not listed in the table all permitted ADR of various types but had less than 1 percent of their cases referred to ADR. Types of ADR not listed in the table all involved less than 1 percent of the cases.

Before CJRA, two pilot districts and one comparison district had formally structured arbitration programs involving 9 to 15 percent of their civil case filings, requiring mandatory nonbinding early arbitration for certain cases involving only monetary damages less than \$100,000. One arbitration pilot district also had a formally structured mediation program involving 10 percent of its civil cases and requiring

³For a discussion of the pros and cons of various types of ADR in the federal courts, see Stienstra and Willging (1995).

⁴28 U.S.C. §651-658.

⁵Meierhofer (1990), p. 12.

⁶Rauma and Krafska (1994), p. 3.

⁷Judicial Amendments Act of 1994, Public Law No. 103-420.

⁸It was concluded on advice of the general counsel of the Administrative Office of the U.S. Courts that the arbitration statutes (28 U.S.C. § 651 et seq.), when read together with the F.R.Civ.P. and the CJRA, prevented the GA(N) district from implementing such an arbitration program without further authorization because the district is not one that was approved by statute to use arbitration.

⁹For details of each district's CJRA plan and its implementation, see Kakalik et al. (1996a).

Table 7.1
Volume of ADR Referrals by District, Before and After CJRA

Before or After CJRA	Mandatory Arbitration	Mandatory Mediation	Mandatory Early Neutral Evaluation	Voluntary Arbitration	Voluntary Mediation	Voluntary Early Neutral Evaluation
Pilot Districts						
1991 before CJRA	OK(W) (10); PA(F) (15)	PA(F) (10)				
District plan implementation after CJRA	OK(W) (8); PA(F) (13)	PA(F) (6); NY(S) (5)	CA(S) (50)	UT (4)	OK(W) (6); TX(S) (5)	
Comparison Districts						
1991 before CJRA	NY(E) (9)				FL(N) (4)	IN(N) (6)
District plan implementation after CJRA	NY(E) (10)			AZ (4)	FL(N) (4); PA(M) (2)	IN(N) (6); NY(E) (2)

NOTE: The number in parentheses indicates referrals to ADR type indicated, as a percentage of all case filings in the district during the year.

mandatory pro bono mediation for certain types of cases by a court-appointed mediator early in the life of the case. One comparison district had a voluntary mediation program involving about 4 percent of its cases, and another comparison district had one judge with a structured early neutral evaluation program involving 6 percent of the cases in the district. As would be expected, the mandatory programs had much higher volume than the voluntary programs.

Following implementation of their CJRA plans, two of the pilot districts continued mandatory arbitration involving 8 to 13 percent of their cases. Two have early mandatory pro bono mediation involving 5 to 6 percent of their cases. Two have voluntary paid mediation involving 5 to 6 percent of their cases. One has voluntary arbitration for 4 percent. And one requires mandatory neutral evaluation efforts by a magistrate judge early in a case, coupled with early pretrial management by the same magistrate judge, involving 50 percent of all civil filings.

Following implementation of its CJRA plan, one of the comparison districts continued its mandatory arbitration program involving 10 percent of civil filings and supplemented it with a voluntary early neutral evaluation program involving 2 percent of filings. One comparison district continued its voluntary mediation program involving 4 percent of filings. One continued its early neutral evaluation program involving 6 percent of filings, and one continued a voluntary arbitration program that was implemented in 1992 involving 4 percent of civil filings. One comparison district began a structured voluntary mediation program involving about 2 percent of all civil case filings.

ESTIMATED EFFECTS OF ALTERNATIVE DISPUTE RESOLUTION REFERRAL

Estimated Effects of Mandatory Arbitration Referral¹⁰

Our pilot program evaluation includes a random sample of 261 cases referred to arbitration: 119 in the 1991 sample and 142 in the 1992–93 sample. Since 97 percent of the arbitration referrals were mandatory, we refer to our estimates as the predicted effect of mandatory arbitration referral.

As we discussed in Chapter Two, we estimate the effects of ADR referrals using data from general civil litigation cases with issue joined.

We explored arbitration in two modes: using all of our data and using only the cases that meet the eligibility requirements of mandatory arbitration in these three federal court districts (only monetary stakes and not over \$100,000).

We find that cases in our sample with an early mandatory arbitration referral do not have statistically significantly different times to disposition from cases with other forms of early management.

We also find that early mandatory arbitration referral did not significantly reduce work hours compared to other forms of early management for cases in our sample. There is some evidence of a beneficial effect in the sample of all cases with issue joined, but not in the sample of cases closed after nine months. Hence it could be that arbitration referrals have a beneficial effect on cases that close before nine months. The results are not statistically significant, but because we have only a small sample of arbitration referrals in our data we cannot determine if this is a real effect or not.

We find moderate but not statistically significant improvement in attorney satisfaction between cases with an early mandatory arbitration referral when compared to other forms of early management. Again, because we have a small sample of arbitration referrals in our data, we cannot determine if this is a real effect or not.

We find inconsistent results in our estimates of the effect of arbitration referral on fairness (sometimes positive effects, sometimes negative effects). It seems doubtful that true causal effects would change direction from more fair to less fair between the two time periods before and after CJRA, since the arbitration referral policy did not change and the same districts used the policy. We suspect that the inconsistent findings are the result of our small sample of arbitration cases. Also, one should remember that even for the arbitration referral cases, over 90 percent of responding attorneys report that case management was fair.

¹⁰For details of our statistical analyses of the effects of arbitration referral on time to disposition, lawyer work hours, satisfaction, and views of fairness, see Appendices D through G, respectively.

Effects of Other Types of ADR Referral

In addition to the arbitration referral cases, our primary pilot program study includes 53 cases referred to other types of ADR in our 1991 sample (eight neutral evaluation, 38 mediation, five summary jury trial, and two mini-trial referrals). These numbers are low because, other than arbitration, ADR referrals were permitted but were not extensively used in the pilot and comparison districts before CJRA.

When we drew our 1992–93 sample, the pilot districts had implemented their ADR referral programs as required, but eight of the ten comparison districts had not yet implemented their plans. The way the pilot district ADR programs were implemented, the percentage of cases referred to ADR other than arbitration was 6 percent or less in each district, usually much less, with the exception of one district. Thus, our random selection of cases captured relatively few ADR referrals in our 1992–93 sample: 86 early neutral evaluation cases in CA(S), 21 early neutral evaluation cases from one judge in IN(N), four neutral evaluation cases in NY(E), 27 voluntary mediation cases in FL(N), 18 mandatory mediation cases in NY(S), 17 voluntary mediation cases in OK(W), 12 mandatory mediation cases in PA(E), 37 voluntary mediation cases in TX(S), ten voluntary mediation cases in UT, 16 mediation referrals in all other districts combined, and no summary jury trial or mini-trial cases.

For each district, the volume of cases referred to ADR was simply too small to generate a large enough number for meaningful statistical analyses when all cases were sampled at random. Furthermore, each of the various mediation and neutral evaluation programs was sufficiently different from the others to make pooling the data to assess the effects problematic.

This CJRA evaluation had a separate supplemental ADR component, in which a sample of cases was purposefully rather than randomly selected to obtain a sample large enough to support statistical analyses. This supplemental evaluation permitted an in-depth examination of the mediation and early neutral evaluation programs in five of the pilot districts that use those ADR techniques for at least 5 percent of their civil filings: CA(S), NY(S), OK(W), PA(E), and TX(S). We also examined a magistrate-judge-administered ADR program in NY(E), a comparison district. The result of this analysis is contained in a companion report.¹¹

ASSESSMENT

All 20 pilot and comparison districts permit the use of ADR techniques in their CJRA plans. The three districts in the study that used mandatory arbitration before CJRA have continued to do so, and two of the three study courts authorized to use voluntary arbitration have started doing so. However, there has been a marked shift in half of the pilot districts toward other formally structured ADR programs—especially mandatory or voluntary mediation and early neutral evaluation.

¹¹Kakalik et al. (1996b).

Some districts with structured and administratively supported programs have only 2 to 4 percent of their cases referred to ADR, so structure and administrative support appears to be a necessary but not sufficient feature for a volume ADR program. However, districts that permit ADR of some kind without a formally structured and administratively supported program have attracted few cases.

These findings send the strong message that the volume of cases in an ADR program depends greatly on the details of how it is designed and implemented. Programs that permit ADR, but are not structured and administratively supported, generate very little volume and have very few costs and effects. And even structured and supported programs where participation is voluntary, rather than mandatory, generate a relatively low volume of ADR.

Voluntary ADR that requires lawyer/party approval for participation has not attracted extensive usage when compared with mandatory ADR, probably due in part to some lawyers' unfamiliarity with ADR and in part to some lawyers feeling that agreement to an ADR process might be viewed as a "sign of weakness" in their cases. Neither lawyers nor judges have used ADR extensively when its use is voluntary. Not all district courts feel that they can or should order unwilling parties to ADR because ADR costs the parties money. Even if the ADR provider works for free, the parties must still spend their own time and pay their own lawyers to prepare for and participate in the ADR. Nevertheless, advocates hope that ADR can reduce litigation costs by inducing early settlements or, at least, by leading to more focused (and thus more cost-efficient) discovery.

Our statistical analyses of cases referred to mandatory arbitration in our primary sample of cases detected no major effect of arbitration. Early mandatory arbitration referral did not significantly affect time to disposition, lawyer work hours, or lawyer satisfaction. The findings for views of fairness were inconclusive.

Nonetheless, there is some evidence that mandatory arbitration referral had a beneficial effect on time, work hours, and satisfaction, but the results are not statistically significant. The small sample of arbitration referrals allows us to detect only major effects, not more modest ones. We suggest that a more thorough study of mandatory arbitration be conducted on a larger sample of cases before any final conclusions are drawn about the effect of this policy.

**USE OF MAGISTRATE JUDGES AND OTHER CASE MANAGEMENT
TECHNIQUES**

INTRODUCTION

The CJRA required districts to consider using six case management techniques.¹

The first technique, having counsel jointly presenting a discovery/case management plan at the initial pretrial conference, was discussed along with early judicial management in Chapter Five.

CJRA technique 2 requires that each party be represented at conferences by an attorney with authority to bind that party. All 20 districts in the study required this both before and after CJRA. Since there was no variation in policy between districts, we could not evaluate the effects of this technique.

CJRA technique 3 requires the signature of the attorney and the party on all requests for discovery extensions or postponements of trial. This technique does not generate enthusiasm from most lawyers, and none of the 20 districts in our study required it for all cases before or after CJRA. Although most plans were silent on why this technique was not adopted, some lawyers we interviewed felt it was unnecessary and increased costs; others resented the implication that some clients are kept in the dark about continuance requests and that they might not approve if requested to sign. Since there was no variation in policy between districts, we could not evaluate the effects of this technique.

The fourth technique, requiring cases to have an *early neutral evaluation*, was discussed along with other forms of alternative dispute resolution in Chapter Seven. This CJRA evaluation had a separate supplemental ADR component, in which we examined early neutral evaluation programs in CA(S) and NY(E). The result of this analysis is contained in a companion report.²

CJRA technique 5 requires, upon notice of the court, that party representatives with authority to bind be present or available by telephone at settlement conferences. Before CJRA, eight of the 20 districts used this technique, and five additional districts

¹ For details of each district's CJRA plan and its implementation, see Kakalik et al. (1996a).

² Kakalik et al. (1996b).

adopted it as part of their CJRA plan. We discuss our evaluation of this technique in this chapter.

The last CJRA technique, “other,” allows the districts some latitude in their CJRA plans. One case management approach included here that varies considerably among districts is the use of magistrate judges in the civil pretrial process. In this chapter we assess the effects of the different intensities of use of magistrate judges in the civil pretrial process across all 20 study districts. In our companion report on ADR, we examine CA(S) and NY(E) in detail. These are the two study districts that made the most intensive use of magistrate judges in the civil pretrial management and ADR processes.

As we discussed in Chapter Two, we base our evaluation on data from general civil litigation cases with issue joined.

ESTIMATED EFFECTS OF HAVING LITIGANTS AVAILABLE FOR SETTLEMENT CONFERENCES

We obtained information on whether the litigants were at or available on the telephone for settlement conferences from surveys of lawyers and judges on our sample of cases.

We find that having litigants at or available for settlement conferences is a statistically significant predictor of reduced time to disposition. However, it is not a statistically significant predictor of lawyer work hours. We also find no statistically significant effects on attorney satisfaction, and no consistent statistically significant effects on attorney views of fairness.³

ESTIMATED EFFECTS OF INCREASED USE OF MAGISTRATE JUDGES IN THE CIVIL PRETRIAL PROCESS

Districts vary in the roles assigned to magistrate judges on civil cases.⁴ Virtually all districts’ magistrate judges conduct felony preliminary proceedings and try misdemeanor and petty offense cases. In some districts, magistrate judges are also given felony pretrial duties, including non-case-dispositive and case-dispositive motions, pretrial conferences, and evidentiary hearings. Prisoner cases are routinely referred to magistrate judges in many districts for pretrial management and the preparation of reports and recommendations. With respect to other civil cases, magistrate judges conduct almost all civil pretrial proceedings in some courts, preparing the case for trial before the assigned district judge. In other courts, they are assigned duties in non-prisoner civil cases on a selected basis in accordance with the preferences of the

³For details of our statistical analyses of the effects of litigant availability for settlement conferences on time to disposition, lawyer work hours, satisfaction, and views of fairness, see Appendices D through G, respectively.

⁴For a historic perspective on the shifting roles of magistrate judges, see Seron (1983); Smith (1992); and Hagopian (1992).

assigning district judge. In addition, magistrate judges conduct jury and nonjury trials and dispose of civil cases with the consent of the litigants.

In two of our study districts—CA(S) and NY(E)—magistrate judges actively manage all aspects of the pretrial process and usually make early attempts to settle cases. This style of case management differs markedly from the traditional approach used in most other districts before CJRA.

Our survey of judges on cases in the 1992–93 sample illustrates the types of civil case activities magistrate judges perform.⁵ The most frequent were scheduling (23 percent of cases), discovery management (22 percent), non-dispositive motions (18 percent), and settlement discussions (17 percent). Magistrate judges were least likely to be involved with dispositive motions (5 percent of cases) and trial (less than 2 percent).

We measured magistrate judge activity in general as the number of civil proceedings (e.g. motions, conferences, hearings) performed by magistrate judges per civil termination in the district. We obtained the number of magistrate judge civil proceedings from forms submitted monthly for each magistrate judge. The average number of magistrate judge proceedings per terminated civil case ranged among the study districts from a high of more than one per case to a low of less than one-tenth per case. Exploratory analyses showed that proceedings per civil termination appears to be the most accurate available measure of magistrate judge activity; however, we found qualitatively similar results if we included in our analysis both proceedings and civil case terminations by magistrate judges.⁶

Regardless of which measure of activity we use, *increased* magistrate judge activity on civil cases had no significant effect on time to disposition or on lawyer work hours, and no consistently significant effect on views of fairness associated with changing the level of magistrate judge activity. This *does not* mean that what magistrate judges *do* to manage cases has no significant effect. We believe that districts with higher levels of magistrate judge activity on civil cases usually are using them to conduct pretrial processing that would otherwise be conducted by a district judge. Hence, we believe our statistical findings mean that using magistrate judges instead of district judges to conduct pretrial civil case processing does not significantly affect time to disposition, lawyer work hours, or attorney views of fairness.

Using the post-CJRA 1992–93 data, we find that increased magistrate judge activity on civil cases is a strong and statistically significant predictor of greater attorney satisfaction. The increased use of magistrate judges on civil cases also predicted greater satisfaction in our 1991 data, but this was not a statistically significant effect. There were, however, changes in some districts in the use of magistrate judges between civil cases that closed in 1991 and cases that were filed 1992–93. For example, CA(S) greatly increased the role of magistrate judges in civil pretrial

⁵ See Appendix I for details.

⁶ For details of our statistical analyses of the effects of magistrate judge activity on time to disposition, lawyer work hours, satisfaction, and views of fairness, see Appendices D through G, respectively.

management in 1992–93. This shift over time in the role of magistrate judges could explain the difference in the significance in our findings.⁷

ASSESSMENT

Having litigants available in person or on the telephone for settlement conferences is associated with significantly shorter time to disposition, but has no statistically significant effect on lawyer work hours, satisfaction, or fairness. This policy appears worth implementing more widely because it has benefits without any significant offsetting disadvantages.

The increased use of magistrate judges in civil pretrial management is associated with significantly increased attorney satisfaction. Our interviews with lawyers suggest that they may be more satisfied with magistrate judges because they see them as more accessible or “user friendly.” However, whether magistrate judges or district judges conduct pretrial management activities does not significantly affect time to disposition, lawyer work hours, or attorney views of fairness. These findings suggest that some magistrate judges may be substituted for district judges on non-dispositive pretrial activities without drawbacks and with an increase in lawyer satisfaction. Of course, this will vary depending on the personality and management style of the particular magistrate judge or district judge.

⁷ We also note an increase in the full-time-equivalent number of magistrate judges per authorized judgeship in our study districts, from 0.50 in 1987 to 0.60 in 1995. See Appendix C, Table C.1 for details.

EFFECTS OF THE PILOT PROGRAM AS A PACKAGE

INTRODUCTION

What are the effects of the CJRA pilot program as a package? Did requiring pilot districts to adopt the package of broadly defined case management principles make a difference in time to disposition, litigation costs, satisfaction, and/or views of fairness for civil cases?

We offer three types of evidence: statistical analysis of our sample of cases in pilot and comparison districts; judicial time study results before and after adoption of the CJRA; and our survey of judges about how they managed cases before and after the pilot program was implemented.

STATISTICAL ANALYSIS OF DIFFERENCE BETWEEN PILOT AND COMPARISON DISTRICTS

In 1991, before the pilot program was implemented, we found no statistically significant difference between pilot and comparison district cases in time to disposition, lawyer work hours, satisfaction, or views of fairness.¹ This means that, despite the limited information available at the time the comparison districts were chosen, the selected pilot and comparison districts were “comparable” before CJRA was passed.

In 1992–93, after the pilot program was implemented but before eight of the comparison districts had implemented their CJRA plans, we still found no significant difference between pilot and comparison district cases in time to disposition, lawyer work hours, satisfaction, or views of fairness. This finding does not mean that all of the pilot program case management principles have no significant relationship to litigation cost and time to disposition. Quite the contrary—the analyses discussed earlier in this report identify some case management policies and procedures contained in CJRA that are statistically significant in predicting time to disposition and litigation costs. We discuss the policies and their implications further in Chapter Ten.

We believe there are at least four reasons why we did not see a significant difference between pilot and comparison districts after implementation of the pilot program.

¹Our detailed statistical analyses are presented in Appendices D through G.

- Some pilot districts' plans, as implemented, did not result in any major change in case management.
- Some pilot districts' plans that resulted in major change in management at the case level did not apply that change to a large percentage of cases within the district.
- Some changes that were more widely implemented (such as early mandatory disclosure of information) did not significantly affect time, cost, satisfaction, or views of fairness.
- Some case management practices that we have identified as significant predictors are used at the case level by judges, not at the district level, and there is much inter-judge variation in case management within both pilot and comparison districts.

Tables 9.1 and 9.2 summarize information on various measures for the 1991 and the 1992–93 samples, respectively. The numbers in the tables are results from the sample cases, not estimates. None of the differences between pilot and comparison districts shown in these tables is statistically significant.

Table 9.1
Cases in Pilot and Comparison District Samples: 1991 Closed General Civil Cases with Issue Joined

Variable	All Closed		Closed After 9 Months		Comments
	Pilot	Comparison	Pilot	Comparison	
Median days to disposition	422	399	584	530	Difference not significant (controlled for case characteristics)
% satisfied with management					
Lawyers	70	73	70	73	Difference not significant (controlled for case characteristics)
Litigants	50	54	49	53	Difference not significant (controlled for case characteristics)
% view management as fair					
Lawyers	90	91	88	90	Difference not significant (controlled for case characteristics)
Litigants	76	74	74	72	Difference not significant (controlled for case characteristics)
Median lawyer work hours per litigant	55	57	75	80	Difference not significant (controlled for case characteristics)
Median cost per litigant (\$1,000)	8	8	12	11	
Median litigant hours spent	30	35	50	40	

NOTES: Litigant satisfaction with the case management process was 25 percent negative and 23 percent neutral in entire sample. Lawyer satisfaction with the case management process was 11 percent negative and 18 percent neutral in entire sample.

Table 9.2
Cases in Pilot and Comparison District Samples: 1992–93 Closed General Civil Cases
with Issue Joined

Variable	All Closed		Closed After 9 Months		Comments
	Pilot	Comparison	Pilot	Comparison	
Median days to disposition	322	358	470	469	Difference not significant (controlled for case characteristics)
% satisfied with management					
Lawyers	76	75	76	77	Difference not significant (controlled for case characteristics)
Litigants	52	57	51	57	
% view management as fair					
Lawyers	94	91	94	91	Difference not significant (controlled for case characteristics)
Litigants	75	76	73	78	
Median lawyer work hours per litigant	60	55	86	75	Difference not significant (controlled for case characteristics)
Median cost per litigant (\$1,000)	9	7	12	10	
Median litigant hours spent	40	40	50	40	

NOTES: The comparison district sample selection began in mid-1992 and was completed in 1993, before eight of the comparison districts had implemented their plans. The time to disposition on these closed cases (93 percent of the sample) is less than it would be if 100 percent were closed. Litigant satisfaction with the case management process was 23 percent negative and 23 percent neutral in entire sample. Lawyer satisfaction with the case management process was 8 percent negative and 17 percent neutral in entire sample.

JUDICIAL TIME STUDY RESULTS

One concern that has been raised about implementing new policies for judicial case management is that benefits such as faster time to disposition may come at the cost of increased time spent by judicial officers. For example, some assert, if judges are involved in managing cases early, referring cases to alternative dispute resolution programs, and presiding over discovery limitation arguments, their total time devoted to the case will rise. In response, proponents of judicial case management argue that extra time spent managing the case early in its life can be offset by time not spent later because the case closes earlier.

To see if the judicial case management principles and techniques of the Civil Justice Reform Act increased the amount of judicial time spent on cases, we conducted a “judicial time study” on the cases in our samples of 1992–93 civil and criminal filings and compared the results with the judicial time study conducted by the Federal Judicial Center (FJC) in the late 1980s. In such surveys, judicial officers are asked to fill out “time sheets” that indicate the time they spent, in or out of the courtroom, on each case. The CJRA survey used the FJC’s questionnaire; thus we have comparable

reported judicial work time data both before and after implementation of the pilot program.²

There was almost no difference in the time spent by judicial officers per civil case in 1992–93 when compared to 1989. The difference in the median time reported per civil case was only one minute and the difference in the mean was only six minutes (Table 9.3). Since 7 percent of the 1992–93 cases were still open in December 1995 when we stopped data collection, the six-minute difference in the mean would decrease and might vanish if the as-yet-unreported time on those open civil cases were included.³

The 1989 FJC and 1992–93 CJRA medians and 75th percentiles are about the same in pilot and comparison districts, but the reported mean time rose slightly for pilot districts (by 12 minutes) and dropped in comparison districts (by 17 minutes). Given the stability of the medians and 75th percentiles in both pilot and comparison districts, we feel that the changes in the means are due to random differences in the number of cases requiring large amounts of judicial time and do not indicate a meaningful trend.

On the criminal side, there has been an increase in the average time required to process a criminal defendant through the system, on the order of 30 to 45 minutes per criminal defendant.

Table 9.3
Average Judicial Time Reported Spent per Civil and Criminal Filing

Time Study	Minutes per Civil Case	Minutes per Felony Criminal Defendant
1989 FJC	191	252
1992–93 CJRA	185	287

SURVEY OF JUDGES ABOUT DIFFERENCE IN CASE MANAGEMENT

In the 1992–93 sample of cases, selected after the pilot program was implemented but before eight of the comparison districts had implemented their CJRA plans, we surveyed the judge on each case after case closure and received over 3,000 responses. One question concerned the difference in case management before and after CJRA: “Was there a difference in how you and any other judicial officer managed this case, compared to how you would have managed it if it had been disposed of *prior* to January 1, 1992?”

The vast majority of the judges (85 percent in pilot districts, 92 percent in comparison districts) answered “no difference” from before CJRA.

²Our detailed analysis of judicial time study data is presented in Appendix II.

³Note that we have partial time reports on the 7 percent open cases through December 1995, so only the remaining judge time spent on those cases after December 1995 is still unreported.

Of the 15 percent of pilot judges who did report a difference, about half said the new case management policies and procedures were better than those before CJRA and about half said they were about the same. None said the new policies were worse than before CJRA. As to workload, of the judges who did report a difference, about two-thirds said the new policies required about the same amount of judicial officer work time, about one-quarter said they required more work time, and less than 10 percent said the new policies required less work time.

ASSESSMENT

We considered the following factors:

- The lack of any statistically significant difference between pilot and comparison districts on measures of time, cost, satisfaction, and fairness;
- The lack of change in reported judicial work time per civil case before and after CJRA; and
- Judges' overwhelming response that they are not managing cases differently after CJRA.

We then concluded that the CJRA pilot program, as the package was implemented, had little effect on time to disposition, costs, or participants' satisfaction or views of fairness.

Although the pilot program has had little effect, there is some evidence that another part of the CJRA may have affected the number of cases pending for over three years in both pilot and comparison districts. Between 1990 and 1995, the number of pending civil cases nationwide increased, but the number of cases pending over three years has been declining. The CJRA requires that "The Director of the Administrative Office of the United States Courts shall prepare a semiannual report, available to the public, that discloses for each judicial officer . . . the number and names of cases that have not been terminated within three years after filing." The number of these older cases has been dropping since the CJRA required public reports on each judge. From September of 1991 to September of 1995, all districts taken together reported a decrease of about 26 percent, comparison districts had a 23 percent decrease, and pilot districts had about one-third fewer three-year-old pending cases.⁴

⁴Refer to Appendix C for details.

We have just seen that the CJRA *pilot program, as the package was implemented*, had little effect on time to disposition, costs, satisfaction, or views of fairness. But this finding does not imply that *judicial case management* has no effect. Because case management varies across judges and districts, we were able to assess the effects of specific policies and procedures. In this chapter, we review this assessment, considering both its implications for a promising case management package, and the prospects for its implementation.

THE CJRA PRINCIPLES AND TECHNIQUES

The CJRA required the pilot districts to adopt six case management principles and recommended the adoption of other techniques.

The principles:

1. Differential case management;
2. Early judicial management and discovery management;
3. Monitoring and control of complex cases;
4. Encouragement of cost-effective discovery through voluntary disclosure and cooperative discovery devices;
5. Good-faith efforts to resolve discovery disputes before filing motions; and
6. Referral of appropriate cases to alternative dispute resolution programs.¹

The techniques:

1. Joint discovery/case management plan;
2. Party representation at each pretrial conference by an attorney with authority to bind that party regarding all matters previously identified by the court for discussion at the conference and all reasonably related matters;

¹Our evaluation of mediation and early neutral evaluation, based on a supplemental study, is the subject of a companion report. See Kakalik et al. (1996b).

3. Required signature of attorney and party on all requests for discovery extensions or trial postponements;
4. Early neutral evaluation;
5. Party representatives with authority to bind to be present or available by telephone at settlement conferences; and
6. Other features that the court considers appropriate.

PRINCIPLES AND TECHNIQUES WE COULD NOT EVALUATE

In some instances, and for a variety of reasons, we were not able to evaluate the effects of a principle or technique.

- **Differential case management (DCM) using a track model** could not be evaluated because comparisons between tracks are needed to do so, and only one district actually implemented this policy with sufficient volume in each track; that district made several other case management changes at the same time, and their assignment of cases to tracks was not based on an experimental design.
- **Limiting depositions** using district-wide restrictions on either the number or the length of depositions could not be evaluated because too few districts implemented this policy.
- **Special management of complex cases** was not implemented in a consistent enough way or sufficiently well documented to permit evaluation. Management of these cases is generally a matter of judicial discretion for each individual case, and decisions about procedures used are not discernible in court dockets.
- **Signature of both the attorney and the party on requests for discovery extensions or postponements of trial** was not required in any of our study districts.
- Both before and after CJRA, all districts required that **parties be represented at pretrial conferences by an attorney with authority to bind**. Since there was no variation in policy between districts, we could not evaluate the policy's effect.

RESULTS OF EVALUATION OF JUDICIAL CASE MANAGEMENT

Table 10.1 summarizes the estimated separate effects of those case management principles and techniques for which the data permitted evaluation. The findings shown in the table indicate that how judges manage cases matters. In particular, some types of case management are associated with less time to disposition. Case management has less effect on costs and lawyers' satisfaction, and almost no effect on views of fairness. Those CJRA case management principles that we could not evaluate, because of the way in which the CJRA was implemented, may or may not have an effect on cost and delay reduction.

Effects on Time to Disposition

Four case management procedures showed consistent statistically significant effects on time to disposition: (1) early judicial management, (2) setting the trial schedule early, (3) reducing time to discovery cutoff, and (4) having litigants at or available on the telephone for settlement conferences. For general civil cases with issue joined that do not close within the first nine months, we estimate that these procedures

Table 10.1
Summary of Statistical Evaluation of CJRA Principles and Techniques

Principle or Technique	Time to Disposition	Cost (Lawyer Work Hours)	Lawyer Satisfaction	Lawyer Perception of Fairness
Early judicial management of any type	S -	S +	0	0
Effect of including trial schedule set early as part of early management	S -	0	0	0
Effect of including pretrial conference as part of early management	0	0	0	0
Effect of including joint discovery/case management plan or status report as part of early management	0	0	0	0
Effect of including referral to mandatory arbitration as part of early management	0	0	0	0
Discovery: limiting interrogatories	0	0	0	0
Discovery: limiting depositions	NE	NE	NE	NE
Discovery: shortening time to cutoff	S -	S -	0	0
Mandatory early disclosure	0	0	S - district S + case	0
Voluntary early disclosure	0	0	0	0
Good-faith efforts before filing discovery motion	0	0	0	0
Litigants available at settlement conferences	S -	0	0	0
Increase use of magistrate judges to conduct civil pretrial case processing	0	0	S +	0
Track model of DCM	NE	NE	NE	NE
Complex case management	NE	NE	NE	NE
Party and lawyer sign continuance requests	NE	NE	NE	NE
Person with authority to bind at conferences	NE	NE	NE	NE

S + = significant increase; 0 = no significant effect; S - = significant decrease; NE = not evaluated (see the text for reasons).

have the *combined effect* of reducing median time to disposition by about four to five months in our post-CJRA sample.²

Case management procedures have a substantial effect on predicted time to disposition. Of the total variance explained in our time to disposition analyses, only about half was explained by the case characteristics and other control variables; case management variables accounted for the rest.

Effects on Costs (Lawyer Work Hours)

Of all the case management procedures we investigated as possible predictors of reduced lawyer work hours, only judicial management of discovery was significantly associated with the desired effect. Cases from districts with shorter median discovery cutoff tend to require fewer lawyer hours; in contrast, cases with early management tend to require more.

Several attorney and case characteristics—especially case stakes and case complexity—explain much more of the variance in lawyer work hours than do the case management variables. This finding suggests that lawyer work hours are driven predominantly by factors other than case management. When time to disposition is cut, lawyers seem to do much the same work but in a shorter time period.

We estimate that the combined effects of early management, setting the trial schedule early, and reducing time to discovery cutoff tend to offset their respective effects on lawyer work hours; consequently, we estimate that cases with all three policies will not differ much from cases that receive none of the policies—a reduction of only one hour in lawyer work hours.³

Our findings that lawyer work hours appear to be driven predominantly by factors other than judicial case management policy and that only judicial management of discovery was significantly associated with reduced lawyer work hours suggest that procedural reform of the type specified in the CJRA may have a limited role to play in reducing litigation costs.

Effects on Satisfaction

In our explorations of attorney satisfaction, we find that early management, median days to discovery cutoff, and setting a trial schedule early in the case—the policies that had the greatest effects on predicted time to disposition and lawyer work hours—have no statistically significant effect on lawyer satisfaction. However, a higher degree of case management is associated with higher lawyer satisfaction.

²See Appendix D for details of our estimate of the combined effect. The 95 percent confidence interval on this estimate is three to six months.

³See Appendix E for details of our estimate of the combined effect. The 95 percent confidence interval on this estimate is –19 to 19 hours.

The increased use of magistrate judges in civil pretrial management is associated with significantly increased attorney satisfaction. However, whether magistrate judges or district judges conduct pretrial management activities does not significantly affect time to disposition, lawyer work hours, or attorney views of fairness. These findings suggest that some magistrate judges may be substituted for district judges on non-dispositive pretrial activities without significant drawbacks and with an increase in lawyer satisfaction.

Attorneys from cases where early disclosure occurs report greater satisfaction. However, attorneys from districts with a policy of requiring early disclosure for all cases were less likely to report satisfaction with case management. Lawyers apparently do not like blanket disclosure policies that apply to all cases, but like the results when they participate in early disclosure on *their* case.

Effects on Views of Fairness

We found no consistent statistically significant effects of case management on attorney views of fairness. Over 90 percent of attorneys report that case management was fair.

A PROMISING CASE MANAGEMENT PACKAGE

These findings suggest a package of case management policies with the potential to reduce time to disposition without changing costs, attorney satisfaction, and views of fairness. The package includes discovery control, the only CJRA case management practice that seemed to be effective in reducing costs.

If early case management and early setting of the trial schedule are combined with shortened discovery cutoff, the increase in lawyer work hours predicted by early management can be offset by the decrease in lawyer work hours predicted by judicial control of discovery. We estimate that under these circumstances, litigants in general civil cases that do not close within the first nine months would pay no significant cost penalty for a reduced time to disposition of approximately four to five months—about 30 percent of their current median time to disposition. And as we have seen, none of these policies has any significant effect on lawyers' satisfaction or perceptions of fairness.

Our analysis suggests that the following approach to early management of general civil litigation cases should be considered by courts and judges not currently using this approach and reemphasized by courts and judges that are using it. The powers to use this approach already exist under the Federal Rules of Civil Procedure.

- For cases that do not yet have issue joined, have a clerk monitor them to be sure deadlines for service and answer are met, and begin judicial action to dispose of the case if those deadlines are missed.
- For cases that have issue joined, wait a short time after the joinder date, perhaps a month, to see if the case terminates and then begin judicial case management.

- Include setting of a firm trial date as part of the early management package, and adhere to that date as much as possible.
- Include setting of a reasonably short discovery cutoff time tailored to the case as part of the early management package.

For nearly all general civil cases, this policy should cause judicial case management to begin within six months or less after case filing.

IMPLEMENTING CHANGE

Given our understanding of how the civil justice system operates, we believe that this package of case management policies has a high probability of reducing time to disposition if implemented, without negatively affecting litigation costs or views of satisfaction and fairness. However, our estimated effect should be treated as an upper bound to the effects that could be anticipated if the policies were implemented more widely.

Our estimate is an upper bound rather than a precise estimate because our quantitative analyses exploited observational data on the naturally occurring variation in judges' management practices rather than data resulting from an experimental random assignment of management practices to cases. We believe we have accurately estimated the effect of a given management practice among districts and judges who currently use it. However, any effects we observe must be interpreted in light of the constraints imposed by observational data.

In particular, judges and districts *choose* to use certain case management policies and practices, and we must assume that these judges and districts may differ from other judges and districts who choose *not* to use the same policies and practices. For example, judges who currently use early management may use it with greater intensity or effectiveness than other judges who may be asked to start using it in the future. Judges who use early management now might be using it in combination with other practices for which we do not have data (such as settlement discussion during the initial case management conference) and which other judges may not choose to use. Also, judges who do not use a particular case management practice now may continue not using it even if they are asked to start using it in the future.

Thus, successful use of a case management procedure by some judges in some districts does not necessarily mean it will be equally effective if all judges are asked to use the procedure in all districts. However, limitations of observational data notwithstanding, practices that we have identified as effective among judges who currently use them are good candidates for practices that could be beneficial if more widely implemented.

The judiciary's ability to ensure widespread implementation of these promising practices is the key to achieving the positive effects we observe. Effective implementation of new policies can be enhanced by examining why the CJRA pilot program had little effect and by learning from prior court and organization research on implementation of change.

Implementation factors that may have contributed to the pilot program's having little effect include:

- *The process and definition of the desired change:* Except for the 10 pilot districts, the act asked the courts only to *consider* the CJRA's principles and techniques. Even in the pilot districts, the vague wording of the act's principles enhanced opportunities for complying with the statutory language by retaining existing case management policies.
- *Organizational factors:* Implementing the principles and techniques recommended in the CJRA involves more than simply writing a plan and making a few rule modifications. To the extent that judges and lawyers view the procedural innovations imposed by Congress as unduly emphasizing speed and efficiency and curtailing judicial independence, they will be less likely to support change.
- *Implementation factors:* Several aspects of the implementation influenced the CJRA's outcomes. The detailed design of new procedures had to be undertaken by individual districts or judges. And there was no effective mechanism for ensuring that the policies contained in district plans were carried out on an ongoing basis.

Prior research on implementation indicates that change is not something "done" to members of an organization; rather, it is something they participate in, experience, and shape. As we detailed in Chapter Three, studies of change in the courts and in other organizations provide some guidelines for improving implementation. They include: clearly articulating what the change is to accomplish and generating a perceived need for it, a governance structure and process that coordinates individuals' activities and assigns accountability for results, and meaningful performance measures to help both implementers and overseers gauge progress.

Studies of change also document that members of organizations are more likely to change their behavior when leadership and commitment to change are embedded in the system, appropriate education is provided about what the change entails, relative performance is communicated across parts of the organization, all supporting elements in the organization also make desired changes, and sufficient resources are available.

Future efforts to change the federal civil justice system could be substantially enhanced by incorporating such guidelines.

CASE SAMPLE DESIGN AND WEIGHTING

GENERAL PRINCIPLES OF CIVIL CASE SAMPLE SELECTION

The fundamental sampling objective was to select a set of cases that would allow several comparisons, including the following four:

1. Cases in the pilot districts vs. comparison districts, before January 1992.
2. Cases in the pilot districts vs. comparison districts, after January 1992, when the pilot program was implemented.
3. Cases grouped according to different case management approaches and procedures used within each district.
4. Cases grouped by broad “average judicial work” case categories, where the categories are defined in the same way for every district.¹

The first two comparisons would help us to evaluate the overall effects of the pilot program. The other two would help us to evaluate the effects of different case management approaches on different types of cases, thereby allowing recommendations for improvements in court case management policies.

The sampling problem was how to draw a sample of cases that would permit these objectives to be realized. It was a difficult problem because of the interdistrict differences that exist with respect to the factors that are most significant for the evaluation—in particular, the cost and delay reduction plans and the methods of implementing them.

A simple random sample of cases (that is, a sample in which each case was selected with the same probability) would represent districts, case management approaches, and case categories in about the same proportions as they are in the universe of cases. However, analytic difficulties would result from such a random approach due to the fact that some types of cases with lower incidence in the universe would have unacceptably low numbers in the resulting sample (e.g., cases from small districts and cases processed with management approaches that are not widely used).

¹The different case management approaches and the nature of suit case categories are discussed in more detail below.

Preliminary analysis in 1991 of federal court and other data sources containing outcome measures similar to the ones we are studying suggested that the relative variation in these measures (the coefficient of variation) was similar in important subsets of cases. Therefore, to enhance our ability to make necessary comparisons, we decided to draw a stratified random sample of cases, with the objective of obtaining roughly equal numbers of cases on both sides of any comparison we wished to make. In particular, this meant our sample should contain the following:

1. About the same number of cases before and after January 1992.
2. About the same number of cases in the pilot districts as in the comparison districts.
3. About the same number of cases processed with each different major case management approach.
4. About the same number of cases in each average judicial work case category.

To accomplish these objectives, we first split the sample by two time periods—before and after January 1, 1992. We also split the sample by the 20 study districts. In each district, we sampled 250 cases in each time period, resulting in a total of 10,000 cases for the entire sample.

Within each district, the sample was stratified according to three “average judicial work” case categories—high, medium, and low—that were largely based on the case weights developed by the Federal Judicial Center’s judicial work time study.² These case categories were uniformly defined for all districts, as discussed later in this appendix.

Within each district, the sample also was stratified according to “minimal management” cases that are typically processed using special procedures that involve little or no judicial management, and “general civil” cases that usually are subject to judicial management after issue is joined. These “minimal” and “general” categories were uniformly defined for all districts, as discussed later in this appendix. Chapter Four of this report discusses the districts’ different approaches to processing minimal management and general civil cases.

In principle, if the district’s CJRA plan included the “track model” of differential case management (as defined in Chapter Four of this report), then the general civil cases were also stratified within each district by the case management “tracks.” However, as documented in Chapter Four of this report and below, the subsequent assignment of cases to tracks by those districts exhibited a good deal less comprehensiveness and variation than expected from reading the plans. For example, one district assigned only 15 percent of its cases to any track, and another district assigned only two cases to its complex track in a year. Consequently, because of the way the track models were implemented by the districts, the sampling plan could not and did not produce the anticipated distributions of cases across the different managerial tracks.

²See Appendix H for information on that FJC study.

However, because the average judicial work case categories, the minimal management category, and the general civil category were defined uniformly for every district and based on available nature of suit data, our sampling plan did produce the desired number of cases in each of those categories.

Following sample selection, appropriate weights were assigned to estimates derived from each sampling stratum to obtain unbiased estimates of overall effects, and those weights are discussed later in this appendix.

Grouping Cases by Average Judicial Work Case Categories

Cases were assigned to three categories based on the nature of suit (NOS)³ codes. These reflect cases that have typically required low, medium, and high average levels of judicial work, based on preliminary case weights derived by the Federal Judicial Center from its judicial work time study.⁴ To expedite sample selection and facilitate analysis, we sought to have three large categories with approximately 20 to 40 percent of the overall judicial work time in each category.

Although 90 percent of the civil cases in that study were closed in the 1991 preliminary data we analyzed, about 10 percent were still open when we created our category definitions. We considered average time spent on closed cases, and average time spent on both closed and open cases to date. Whether we used only closed cases or both open and closed cases, similar categorizations resulted. If and only if the number of cases in a particular NOS was less than 50 in the FJC judicial time study, then we also considered the 1980 FJC judicial time study data before assigning the NOS to a category.

Finally, for certain types of cases that we wanted to sample together, we made sure they were in the same category. This applied to the NOS codes for government collection/recovery cases (which all fell into the low category), to the NOS codes for forfeiture and penalty cases (which all fell into the low category), to the NOS codes for prisoner petitions (all of which were placed into the low category), and to the NOS codes for Social Security cases (all of which were placed into the low category). Because these case types are not the focus of the CJRA, and because they require relatively low judicial effort on average, we wanted to undersample them. Grouping them together in the low category facilitated this relative undersampling.

Table A.1 shows which types of cases were assigned to the high, medium, and low categories for sample selection purposes. Note that asbestos cases do not appear in the table since they are not part of this study.

³The Administrative Office of the U.S. Courts categorizes each civil case using a nature of suit code. Examples include Motor Vehicle Product Liability, Copyright, and Habeas Corpus. For details see Administrative Office of the U.S. Courts (1995), Chapter 5.

⁴We could not use both nature of suit and jurisdiction due to the small sample sizes in the time study data for many of these more detailed cells.

Table A.1

High, Medium, and Low Categories of Cases by Nature of Suit for Sample Selection Purposes

High		Medium		Low	
Contract product liab.	195	Insurance	110	Marine contract	120
Tort product liab.	245	Stockholders	160	Miller Act	130
Airplane prod. liab.	315	Other contract	190	Negotiable instrument	140
Fed. Employ liab.	330	Rent lease	230	Recovery	150-153
Marine prod. liab.	345	Torts to land	240	Land condemn	210
Motor veh. prod. liab.	355	Other real property	290	Foreclosure	220
Pers. Inj. med. mal.	362	Assault, libel, slander	320	Airplane	310
Pers. Inj. prod. liab.	365	Marine pers. inj.	340	Bankruptcy	420-423
Other fraud	370	Motor veh. pers. inj.	350	Deportation	460
Other pers. prop. dam	380	Other pers. inj.	360	Prisoner petitions	510-550
Prop. Dam. prod. liab.	385	Truth in lending	371	Forfeiture and pen	610-690
Antitrust	410	State reapportion	400	Other labor	790
Banks and banking	430	Interstate commerce	450	Social Security	860-865
Civil rights	440-444	Labor Man. Rel. Act	720	Tax suits	870
RICO	470	Railway Labor Act	740		
Fair Labor Stan.	710	ERISA	791		
Labor Man. Reporting	730	Selective service	810		
Patent	830	Copyright	820		
Secur. Commod. Exch.	850	Trademark	840		
Environmental	893	IRS third party	871		
Freedom of Inf.	895	Customer challenge	875		
Constit. State Statutes	950	Other statutory	890		
		Agricultural	891		
		Econ. Stabilization	892		
		Energy Allocation	894		
		Appeal of Fee EAJA	900		
		Local quest.	910-940, 990		

For all pilot and comparison districts combined, Table A.2 shows the percentage of all civil cases terminated in 1991 that were in each of the high, medium, and low categories. The table also shows an estimate of the percentage of judicial officer work time spent on each of the categories.⁵

Grouping Cases by Management Approaches

As noted above, every district has at least two different major case management approaches that we sampled: minimal management and general civil case management. The district's plan sometimes defined formal management tracks within general civil litigation that we also attempted to sample. We implemented this stratified sampling by management approach as follows:

1. *Minimal case management approach:* There is a great deal of commonality among districts in the definition of the types of cases that receive little or no management in practice. For consistency in our analyses, we grouped the cases

⁵The estimate was made for each nature of suit code separately by multiplying the number of terminations in 1991 times the 1991 preliminary average judicial officer work time per case (based on the Federal Judicial Center judicial officer time survey).

Table A.2
Percentage of Cases and Judicial Officer Work Time
in High, Medium, and Low Categories of Cases

Category	Percentage of 1991 Terminations	Percentage of Judicial Officer Work Time
High	19	35
Medium	36	38
Low	41	26
Asbestos (not in study)	4	1

with the following "nature of suit" codes together and sampled from them as a group in every district: government collection/recovery cases (codes 150-153); foreclosure cases (code 220); bankruptcy cases (codes 420-423); prisoner petitions (codes 510-550); forfeiture and penalty cases (codes 610-690) and Social Security cases (codes 860-865). These cases usually do not involve pretrial conferences and are minimally managed using routine procedures.

2. *General civil case management approach*: This is defined as the set of all civil cases other than those defined above as minimal management cases. We always sampled these separately in every district.
3. *Complex case management approach*: Within the general civil case category, we always attempted to sample this approach separately if the district used a track model of differential case management. Sometimes the complex track had a different name, such as "special." Of the 20 districts, we sampled complex cases separately in four pilot districts: GA(N), NY(S), OK(W), and PA(E).
4. *Other case management "tracks"*: Within the general civil case category, if the district had other formal case management tracks that were defined near the time of case filing (so we could sample on them early enough to begin the judicial time survey early in the life of the case), we used them for sample selection. These included a separate mandatory arbitration case management track in two districts, NY(E) and PA(E).

SPECIFIC METHOD OF CIVIL CASE SAMPLE SELECTION

Selection of the sample was supervised by a RAND survey research specialist who visited each of the pilot and comparison districts. To select the sample, the following steps were required.

1. *Management approaches* were defined in each district that reflected the district's case management approach before and after January 1992.
2. Three *case categories* were defined that were the same for all districts that reflected cases that in the past had typically required low, medium, and high average levels of judicial work, respectively.
3. A sampling window was opened for cases filed in each district beginning soon after we made our first evaluation visit to the district (as soon as possible after

January 1, 1992) and a district clerk screened the first 700 cases. The first 50 new filings in this district in each *management approach* were selected in the order the cases were filed (the actual selection quota used for each management approach was set by RAND so that the total number selected was at least 250 and each approach had at least 50 cases). The screening of 700 cases was sufficient to fill the sample quota for each management approach, unless the district defined a management approach that contained less than about 7 percent of the filings on average (in which situation we purposefully sought additional filings subject to that management approach to fill the sample quota).

4. In the same sampling window of 700 cases, the clerks were to select the first 50 cases in the low *case category*, the first 75 in the medium category, the first 75 in the high category, and at least 250 in total, thereby guaranteeing more emphasis on the medium and high categories where the new case management policies may show the most effect). Since there was overlap between cases selected in categories and management approaches, we ended up with at least 249 but no more than 320 selected cases in each district.⁶
5. Steps (3) and (4) were also to be followed for a sampling window of 700 cases terminated in each district in 1991 before the CJRA pilot program was implemented. This was done at RAND by working backwards in time through a list of cases terminated on or before December 15, 1991.⁷ Information used came from the federal court integrated database and, if necessary, case docket information on "track" assignment.

Cases Filed in Batches

A batch of similar cases was defined as more than five cases filed or closed⁸ on the same day with the same NOS code and either the same lawyer or the same party. For example, 10 foreclosure cases closed the same day involving the same bank or 20 cases filed by the federal government on the same day for recovery of defaulted student loans were considered "batches."

Although the case mix observed by the court clerk accepting filings or closing cases was generally random, the problem was that batches of similar cases clearly were not random, and accepting all cases in a given batch would have biased the sample. Excluding every case in the batch would result in loss of important information. We decided to have the clerks screen only five cases from each batch for possible inclusion in the sample (using the normal sample selection procedures). Batch case numbers six and up were *skipped* by the clerks. For the 1991 termination sample, the

⁶The actual sample in one district dropped from the original 250 to 249 because the same case was filed, transferred to another office with a new docket number, and inadvertently selected twice by the court clerk for our sample.

⁷In our preliminary exploration of sample selection, we discovered that the mix of cases terminated the last two weeks of the year may not be representative of the rest of the year (for example, the mix of cases may be biased in favor of the non-complex cases).

⁸"Closed" refers to the 1991 sample selection, and "filed" refers to the 1992-93 sample selection.

same rule applied, except we considered cases terminated on the same day in a batch.

We encountered 67 batches in the 14,000 cases screened for the 1991 sample. This was an average of only three batches per district in the 700 cases screened per district and an average of 14 cases skipped per batch (an average of five skipped per batch if we do not average in the more than 600 veterans' benefits cases closed in four batches in one district).

The effect of the batch partial exclusion rule for the 1991 sample was as follows. Of the 67 batches, 45 had no effect on the sample since they were screened after the sample cell for that type of case had been filled. Nineteen of the batches improved the sample. Improvement here is defined in terms of the mix of case closures—moving the percentage of that nature of suit in the sample close or closer to the district's annual percentage closed for that nature of suit. And the sample was degraded because of exclusion of 26 veterans' benefits cases from two batches in one district and eight foreclosure cases from one batch in another district, thereby moving the percentage of that nature of suit in the sample farther from the annual percentage closed for that nature of suit.

We encountered 77 batches in the 14,000 cases screened for the 1992–93 sample. This was an average of only four batches per district in the 700 cases screened per district and an average of five cases were skipped per batch.

The impact of the batch partial exclusion rule for the 1992–93 sample was similar to what we found for the 1991 sample. Of the 77 batches, 69 had no effect on the sample or improved the sample. Improvement here is defined in terms of the mix of case filings—moving the percentage of that nature of suit in the sample close or closer to the district's annual percentage filed for that nature of suit. The sample was degraded by exclusion of five other batches, but the percentage of that nature of suit in the sample was still within 15 percentage points of the annual percentage filed for that nature of suit. Finally, the sample was degraded by exclusion of six prisoner civil rights cases in two batches in different districts, and by the exclusion of 96 product liability batch cases. This large batch of 96 product liability cases was all breast implant cases that were filed in one district and then immediately transferred to the Alabama judge who was handling those cases, so they were not typical of product liability cases filed in our sample district. Hence, while exclusion of this batch of product liability cases meant that product liability cases were underrepresented in our sample in this district compared to the number filed in FY1993, the exclusion also meant that product liability cases were more accurately represented in our sample in this district compared to the number filed in a more typical year.

Sample Size Determined by Desired Precision of the Estimates in Our Evaluation

We designed our sample to give us at least 250 cases per district per time period. A sample size of at least 200 cases is the number required to offer a "good chance" of detecting a 10 percent change in the average cost or time to disposition of cases be-

fore and after January 1992, or a 10 percent difference between two different districts.⁹

Using data available in 1991, we based this calculation on the following assumptions: (1) case time to disposition and costs have a coefficient of variation ranging between 0.8 and 1.2;¹⁰ and (2) differences in case mix and other characteristics which we will be able to control for in multivariate analyses account for between 0 percent and 50 percent of the variation. Both of these assumptions appear reasonable in light of previous analyses done at RAND.¹¹

The stratification of the sample by case management approaches and case categories determines the statistical power to detect other differences among management approaches, categories, districts, time periods, and various combinations of those factors.

On one hand, our ability to detect differences between individual tracks in a particular district is somewhat limited. For example, we estimated in 1991 that 50 cases would allow us to detect a 14 percent difference in the best of circumstances (low coefficient of variation, large reduction in variation by controlling for case mix and other factors). For this purpose, our sample size is minimally acceptable.

On the other hand, a principal purpose of the study is to evaluate the effects of different case management policies. We can combine data from several districts to do this evaluation. And, when we combine information across a number of districts, our ability to detect relatively small differences is substantial.

For example, suppose a particular management approach was adopted for standard cases in 10 different districts. We would have at least 500 cases in which this approach was attempted, and we would be able to select at least 500 suitable comparison cases in other districts where the approach was not attempted. We estimated that we would have adequate statistical power to detect differences as small as 5 percent in this situation.

For the simplest but perhaps most important of questions—how much effect did the pilot program case management reforms adopted in January 1992 have across all the districts in the study combined—we have about 5,000 cases before and 5,000 after January 1992. This sample size was estimated to be adequate to detect differences as small as 2 percent.

⁹By the phrase "detecting a 10 percent change," we mean that the null hypothesis of no difference will be rejected at a significance level of 5 percent in those comparisons where the true difference is at least 10 percent. By a "good chance," we mean the power of this test is 80 percent—that is, we will detect at least four out of every five comparisons where the relative difference exceeds 10 percent. These levels of significance and power are used for all of the discussions in this appendix.

¹⁰This estimate of the coefficient of variation is based on analysis of time to disposition data for 1990 dispositions for several districts and for several different groupings of cases. We did not have case level information on costs sufficient to estimate the coefficient of variation of litigation costs.

¹¹These analyses were conducted during various previous RAND studies of all federal cases, auto accident cases, asbestos cases, and aviation accident cases.

Asbestos Cases Excluded from the CJRA Sample

Asbestos cases were excluded from the detailed surveys with Judicial Conference approval. The principal reason for this decision is that after the consolidation of the asbestos caseload in the Eastern District of Pennsylvania in 1991, the future management of such cases was unique and not within the mainstream of Civil Justice Reform Act policies that this study is evaluating. Inter-district comparisons under CJRA will be impossible, and no generalizations about case management as a whole can be made.

CIVIL SAMPLE SIZE

For 1991, a total of 5,149 closed civil cases was selected, with the number in each district ranging from 250 to 275. Table A.3 gives a breakdown of the sample in each district by case category and management approach. By case category, the sample has a total of 1,392 low, 2,210 medium, and 1,547 high average judicial effort cases.

Table A.3
1991 Terminated Civil Cases in CJRA Sample

District	Type of Case Category and Management Approach							Total
	Minimal	General (No Track)			General Arb. Refer			
		Low	Low	Medium	High	Low	Medium	
AZ	50	21	122	75				268
CA(C)	50	30	100	73				253
CA(S)	50	21	106	74				251
DE	50	10	122	75				257
FL(N)	50	23	88	89				250
GA(N)	50	10	108	82				250
IL(N)	50	8	126	75				259
IN(N)	50	10	112	78				250
KY(E)	50	25	102	75				252
KY(W)	50	12	103	88				253
MD	50	22	118	75				265
NY(E)	50	9	94	72	6	41	3	275
NY(S)	50	44	106	75				275
OK(W)	50	29	95	75	2	2	0	253
PA(E)	50	9	93	55	2	33	20	262
PA(M)	50	7	115	78				250
TN(W)	50	11	116	75				252
TX(S)	50	44	105	75				274
UT	50	25	95	80				250
WI(E)	50	12	108	80				250
Pilot	500	215	1,054	746	4	35	20	2,574
Comparison	500	167	1,080	778	6	41	3	2,575
Total	1,000	382	2,134	1,524	10	76	23	5,149

By management approach, the sample has 1,000 minimal management, and 4,040 general civil cases not assigned to tracks, and 109 general civil cases referred to mandatory arbitration. Three of the districts had mandatory arbitration referral for certain cases, and two of the three had sufficiently computerized their dockets by the time we selected the sample so that we could consider arbitration referral as a selection factor.

For 1992–93, a total of 5,222 filed civil cases were selected, with the number in each district ranging from 249 to 320. Table A.4 provides details by district, by case category, and by management approach. By case category, the sample has a total of 1,329 low, 2,117 medium, and 1,776 high average judicial effort cases. By management approach, the sample has a total of 1,033 minimal management, 3,420 general civil cases without tracks, 142 mandatory-arbitration-referral cases, 114 complex track cases (sometimes called special track cases by the district), 509 standard track cases, and four expedited track cases that were not part of our definition of “minimal management” cases.

Some further explanation of the relatively low number of cases selected in management tracks is in order. None of the comparison district were using differential case management tracks at the time the sample was selected. This sample was selected months before 8 of the 10 comparison districts had implemented their CJRA plans (the exceptions that implemented early were IN(N) and NY(E) and neither opted for differential case management tracks in their plan). Of the 10 pilot districts, 6 adopted differential case management tracks as part of their CJRA plan: DE, GA(N), NY(S), OK(W), PA(E), and TX(S). DE did not assign cases to tracks near the time of filing, and so we could not use the track as a selection factor. About a year after the sample was selected, analysis of the DE sample case dockets revealed that none of the sample cases had ever been assigned a track designation by the district. GA(N) assigns a track designation at time of filing based on the nature of suit, but very few cases have the three “long discovery” (complex) nature of suit codes, so only seven were filed during the entire sample selection period. NY(S) does not assign cases to tracks near the time of case filing, and inspection of the sample case dockets revealed that after a year, only a small fraction of the sample cases had ever been assigned a track designation by the district. OK(W) also does not assign cases to tracks near the time of case filing, and very few cases receive the complex track designation—two in the entire year we used for sample selection of complex cases in OK(W). PA(E) has a system of track selection by lawyers near the time of filing (with judicial officers reassigning the track if necessary), and about 7 percent of cases are in their “special” (complex) track. TX(S) did not implement its planned track system because it felt that it lacked sufficient staff to do so and because it had difficulty operationalizing the track assignment process.

Table A.4
1992-93 Filed Civil Cases in CJRA Sample

District	Type of Case Category and Management Approach															Total
	Minimal			General (No. Tracks)			General			General			General			
	Low	Med.	High	Low	Med.	High	Low	Med.	High	Low	Med.	High	Low	Med.	High	
AZ	51	21	103	69	0	3	5									252
CA(C)	50	29	127	78												284
CA(S)	49	15	84	102												250
DE	51	11	103	86												251
FL(N)	50	9	74	134												267
GA(N)	68															277
IL(N)	50	6	111	83												250
IN(N)	52	8	80	113												253
KY(E)	54	13	103	79												249
KY(W)	50	16	107	78												251
MD	51	25	118	72												266
NY(E)	50	4	81	68	5	44	6									258
NY(S)	49	25	106	65				5	22	23	3	12	6	1	2	320
OK(W)	51				2	19	6	1	0	1	8	95	69			252
PA(E)	48				1	41	8	1	11	43	4	61	49			267
PA(M)	51	13	98	88												250
TN(W)	53	10	97	92												252
TX(S)	57	36	107	72												272
UT	49	13	99	88	0	1	1									251
WI(E)	49	5	112	84												250
Pilot	524	115	708	589	3	61	15	7	33	74	21	264	224	1	2	2,642
Comparison	509	144	1,002	862	5	47	11	0	0	0	0	0	0	0	0	2,580
Total	1,033	259	1,710	1,451	8	108	26	7	33	74	21	264	224	1	2	5,232

CIVIL SAMPLE WEIGHTS

Since the sample was selected at random from various strata or cells, cases in different sampling cells had different probabilities of being selected. Therefore, we cannot simply average the survey results, giving each case equal weight, because each case in the sample does not represent the same number of cases in the universe being studied. And while we always must weight the sample cases, we must weight them in different ways depending on the question being asked.

If the question concerns the average case in all pilot or all comparison districts combined, for example, then we should use “district size” sample weights that recognize differences in the volume of cases processed in different districts. For example, we chose 75 high case category, general civil cases in both the AZ and the IL(N) districts. Yet the number of 1991 terminations of that group of cases in AZ was 330 and in IL(N) was 1,739. So each such sample case in AZ represented 4.40 cases while each such sample case in IL(N) represented 23.19 cases. Tables A.5 and A.7 display “district size” sample weights for the 1991 and 1992–93 samples, respectively.

If the question concerns comparison of different policies for managing cases at the district level, where the district is the unit of analysis, then we should use “equal districts” sample weights that give equal weight to the sample from each of the districts rather than letting data from the larger district overwhelm data from the smaller district. We have chosen to standardize these “equal districts” weights so that the average case in our sample has a weight of 1.00. This standardization is done by assuming every district has case volume equal to 1/20 of our sample and by prorating the “district size” weights proportionately. Note that every sample cell within the district does not have an equal weight, because the sample selection probabilities vary within a district by sample cell and their relative weights must reflect this to calculate district averages correctly. Tables A.6 and A.8 display “equal districts” sample weights for the 1991 and 1992–93 samples, respectively.

Cases that reopen after being closed in the same district present a special sample weighting issue. Our study is concerned with the entire life of a case in a district, not just the first or last portion of a case’s life. We count each case only once, not matter how many times it reopens in a district. Our sample weights therefore must avoid double counting reopened cases.

About 6.5 percent of the 1991 pre-CJRA sample were cases that had been opened and closed more than once in the same district (only 0.3 percent of the 6.5 percent were closed more than twice). We selected the 1991 sample based on any closure, not just the final closure for a case—whether or not it was a “final” closure was unknown to us at the time of sample selection. Thus, cases with more than one closure had a higher probability of being selected. We adjusted the weights for the 1991 sample shown in the above tables for the specific cases that were closed more than once and then rescaled the “equal districts” weights so the average case had a weight of 1.00 in each district. The actual adjustment depended on how many reclosed cases were in

Table A.5
"District Size" Sample Weights: CY1991 Terminated Civil Cases

District	Type of Case Category and Management Approach							Total
	Minimal	General (No Track)			General Arb. Refer			
		Low	Low	Medium	High	Low	Medium	
AZ	24.68	8.00	5.86	4.40				9.13
CA(C)	74.44	24.00	35.51	24.37				38.62
CA(S)	13.20	8.90	6.91	7.16				8.40
DE	8.44	2.10	1.89	1.61				3.09
FL(N)	14.90	2.09	2.16	1.80				4.57
GA(N)	18.02	8.70	10.68	10.04				11.86
IL(N)	59.66	38.13	26.41	23.19				32.26
IN(N)	16.10	4.00	3.90	4.85				6.64
KY(E)	21.08	3.04	4.05	3.81				7.26
KY(W)	16.00	4.67	3.50	2.92				5.82
MD	22.98	10.68	9.75	7.41				11.66
NY(E)	24.38	26.11	16.61	10.50	14.33	6.17	8.33	15.04
NY(S)	20.22	16.93	25.63	14.59				20.24
OK(W)	17.44	4.00	6.00	5.08	4.00	6.00	5.08	7.74
PA(E)	30.98	18.11	28.95	27.38	19.50	25.15	14.35	26.97
PA(M)	17.12	5.29	4.71	4.27				7.07
TN(W)	13.32	4.00	3.68	3.43				5.53
TX(S)	40.60	19.64	21.52	15.41				23.03
UT	7.36	7.00	4.13	4.80				5.28
WI(E)	11.54	4.50	3.87	3.95				5.46
Pilot								11.98
Comparison								13.85
Total								12.92

the 1991 sample in each district but averaged only about 3 percent. That is, the 6 percent of the cases that closed twice had their weights cut in half because they were twice as likely to be selected and hence overrepresented in the 1991 sample. The post-CJRA 1992–93 sample did not have this issue, because we selected only cases having their first opening in this district. Reopenings of an old case did not trigger selection for the 1992–93 sample. When we calculated the weights for the 1992–93 sample, we used the number of new filings in the district, excluding cases that were reinstated or reopened.

Finally, if a particular lawyer on a case represented more than 20 litigants, then we sampled only 20 for our litigant surveys, and litigant responses for those lawyers-cases were weighted to reflect the number of total litigants represented by the 20 selected.

Table A.6
"Equal Districts" Sample Weights: CY1991 Terminated Civil Cases
Average Sample Case Weight = 1.00

District	Type of Case Category and Management Approach							Total
	Minimal	General (No Track)			General Arb. Refer			
		Low	Low	Medium	High	Low	Medium	
AZ	2.60	0.84	0.62	0.46				0.96
CA(C)	1.96	0.63	0.94	0.64				1.02
CA(S)	1.61	1.09	0.84	0.87				1.03
DE	2.73	0.68	0.61	0.52				1.00
FL(N)	3.36	0.47	0.49	0.40				1.03
GA(N)	1.57	0.76	0.93	0.87				1.03
IL(N)	1.84	1.17	0.81	0.71				0.99
IN(N)	2.50	0.62	0.61	0.75				1.03
KY(E)	2.97	0.43	0.57	0.54				1.02
KY(W)	2.80	0.82	0.61	0.51				1.02
MD	1.91	0.89	0.81	0.62				0.97
NY(E)	1.52	1.63	1.03	0.65	0.89	0.38	0.52	0.94
NY(S)	0.94	0.78	1.19	0.67				0.94
OK(W)	2.29	0.53	0.79	0.67	0.53	0.79	0.67	1.02
PA(E)	1.13	0.66	1.05	1.00	0.71	0.92	0.52	0.98
PA(M)	2.49	0.77	0.69	0.62				1.03
TN(W)	2.46	0.74	0.68	0.63				1.02
TX(S)	1.66	0.80	0.88	0.63				0.94
UT	1.44	1.37	0.81	0.94				1.03
WI(E)	2.18	0.85	0.73	0.75				1.03
Pilot								1.00
Comparison								1.00
Total								1.00

CRIMINAL CASE SAMPLE SELECTION

While criminal cases are not the prime focus of this CJRA evaluation, we are concerned about the impact the criminal case workload has on civil case processing. Consequently, we need to know how much judicial work time criminal cases require, and a sample of criminal cases is needed. We therefore conducted a new judicial time study on about 1000 criminal cases (about 50 cases and all defendants on those cases in each of the 20 pilot and comparison districts).

We stratified cases according to a taxonomy of offenses that identified each case as being one of the following types (given in descending order of seriousness): crime

against a person; drug or narcotic charge; property offense; and other.¹² Because some cases involved multiple defendants and multiple charges, our classification scheme assigned cases to strata according to the most serious offense committed by any of the defendants. For example, if a case involved two defendants, one charged with murder and the other with narcotics sales, then the case is categorized as a crime against a person. Similarly, if the case involved one defendant charged with being an illegal immigrant and the sale of narcotics, then the case is categorized as involving a drug crime.

Our primary interest in gathering criminal case data is in estimating the time spent on criminal cases. Because previous studies found that the time spent on criminal defendants and hence criminal cases depends on the nature of the offense, we chose to use a stratified sampling plan to improve the precision of our estimate and avoid being swamped with all one type of case in a district (such as might happen if 50 immigration cases were all filed the same day in the district). We chose these four particular offense-related strata because of the varied nature of the crimes and possible sentences create different requirements on the court, and we expected this would influence the average time spent on each defendant.

For each district and each of the four strata, we determined a sampling quota within the total of 50 cases based on the number of cases of that type filed in the district during the 1990 calendar year. We used 1990 to establish our quota because it was the last year for which we had the necessary counts of cases for determining the quota. We set the quota for each strata proportional to that strata's share of the total number of cases for the district. For example, suppose in District X that 50 percent of the cases in 1990 were drug cases; then our quota for drug cases in District X was 25 cases.

Clerks in each district selected the sample cases, using the quotas and explicit instructions for selecting sample cases provided by RAND. Beginning in 1993, district clerks classified each criminal case according to the scheme discussed above and selected cases from each strata until the strata quotas were met. In most districts, it took from two weeks to four months to fill all quotas.¹³ However, because of a limited number of criminal cases filed during the selection year in one district, only 43 cases had been selected after 18 months when we stopped the sample selection.

¹²The Administrative Office offense codes used for the category of crime against a person were assault (1500-1603); escape and prison offenses (7310-7331); extortion, racketeering and threats (7400-7401, 9926); homicide (0100-0311, 0680); kidnapping (7600-7611); robbery (1100-1400); sex offenses (6100-6300); and violations of civil rights (9901-9902). The offense codes categorized as drug or narcotic charges were drugs and Drug Prevention and Control Act (6500-6910). Property offense codes were burglary, breaking and entering (2100-2400); larceny and theft (3100-3800); embezzlement (4100-4390); fraud (4510-4999); auto theft (5100-5200); forgery and counterfeiting (5500-5800); arson (7910); and malicious destruction of property (7940). All other offenses were classified as "other."

¹³The classification of each case with multiple defendants and/or multiple filing offenses into one of the four categories by the clerks was sometimes difficult. The final number in each category in a district's sample was sometimes slightly different than the quota. However, the sample strata weights we developed were based on the actual sample in comparison to the actual filings, as discussed in the next section.

CRIMINAL SAMPLE SIZE

Table A.9 gives the number of sample cases by district and offense strata. The total is 1033 cases. The number selected per district ranged from 43 (in one district with very few criminal filings per year) up to 60 (in a district that inadvertently selected several more cases than we requested). Some criminal cases have more than one defendant, and we selected all 1486 defendants on the 1033 cases in the sample.

CRIMINAL SAMPLE WEIGHTS

Since we randomly selected cases within four categories in each district, and since the number of cases filed in each of the four categories in each district varies markedly from year to year, we need to develop sample weights to combine the data appropriately. To ensure that our estimates reflect the criminal caseload mix during the time period that most of our civil sample cases were being processed, we developed criminal sample weights using criminal filings data from October 1992 through September 1994. The "district size" weight is annual number of cases filed in the strata divided by the number of cases we selected from the strata. That is, if half of the annual number of cases in a strata were selected then the weight will equal two

Table A.9
1993–94 Filed Criminal Cases in CJRA Sample

District	Sample Size by Offense				Total
	Personal	Drug	Property	Other	
AZ	9	21	14	8	52
CA(C)	14	7	25	7	53
CA(S)	5	30	4	12	51
DE	2	28	12	1	43
FL(N)	8	17	24	4	53
GA(N)	5	10	23	12	50
IL(N)	5	9	31	9	54
IN(N)	2	11	28	11	52
KY(E)	7	19	14	10	50
KY(W)	5	14	25	16	60
MD	7	17	13	14	51
NY(E)	2	25	14	9	50
NY(S)	2	18	24	6	50
OK(W)	4	15	18	13	50
PA(E)	3	13	28	8	52
PA(M)	12	12	17	10	51
TN(W)	5	22	19	3	49
TX(S)	2	25	13	15	55
UT	12	9	21	14	56
WI(E)	11	14	17	9	51
Total	122	336	384	191	1,033

because each selected case represents two cases filed annually. This weight is analogous to the "district size" weight used in the sample of civil cases.¹⁴ The weights are given in Table A.10.¹⁵

Table A.10
Sample Weights: RY1993–94 Filed Criminal Cases

District	"District Size" Sample Weights			
	Personal	Drug	Property	Other
AZ	23.56	11.94	20.35	47.00
CA(C)	17.92	29.12	18.50	47.93
CA(S)	32.80	34.04	26.80	31.03
DE	4.50	0.64	4.29	17.50
FL(N)	4.94	11.39	4.95	15.00
GA(N)	13.50	18.17	10.23	21.83
IL(N)	12.33	18.34	10.78	11.82
IN(N)	13.25	8.64	4.36	7.05
KY(E)	5.06	5.98	6.38	6.92
KY(W)	4.79	1.39	9.07	5.19
MD	13.29	7.20	18.54	15.82
NY(E)	20.56	19.99	37.97	28.67
NY(S)	97.75	18.54	24.85	58.67
OK(W)	8.88	7.29	11.12	9.58
PA(E)	22.75	19.26	8.45	9.96
PA(M)	4.65	3.24	9.28	9.55
TN(W)	6.08	4.56	7.16	18.38
TX(S)	76.25	27.22	17.07	23.75
UT	4.46	9.07	5.43	5.18
WI(E)	2.88	3.38	4.56	4.83

¹⁴We used the two-year annual average rather than a single year because no single year clearly represents the primary time frame in which the sample of civil cases was being processed. In addition, there is a large year-to-year fluctuation in the distribution of criminal cases, so we felt using the two-year annual average will stabilize the estimates, somewhat, and hopefully provide weights that better reflect the distribution of cases during the entire period of interest.

¹⁵In some districts, the mix of criminal cases filed in October 1992 through September 1994 differed markedly from the corresponding mix of cases from 1990 that we used to set our sample quotas. These differences reflect the large year-to-year variation in the distribution of criminal offenses and cases processed by different federal district courts. This variation arises in part because of the volatility of the criminal offenses and in part because of the relatively small number of criminal cases per year in some districts.

SURVEYS CONDUCTED AND NONRESPONSE WEIGHTING

SURVEYS CONDUCTED

For the main sample of 10,000+ civil cases, we attempted to survey all 19,000+ lawyers¹ and all 40,000+ litigants. We also surveyed the judges on the 5,000+ cases in the 1992-93 sample if the case closed before January 1996, when our last surveys were mailed. Each of the 20 chief judges provided us with cover letters for the surveys encouraging lawyers and litigants to cooperate (these were personalized to each lawyer and litigant by RAND using master form letters from each chief judge). By the end of February 1996, we mailed the third and final reminders to all survey recipients who had not yet responded. The last date for acceptance of survey responses for analysis was April 2, 1996. Complete responses to our surveys were received from about two-thirds of the judges, about one-half of the lawyers, and about one-eighth of the litigants (about one-quarter of the litigants for whom we had addresses).

Table B.1 shows the number of people surveyed in each sample, and subsequent tables in this appendix will provide details of the completion rates for each type of survey. Following the discussion on completion rates, we discuss issues of nonresponse and weighting to adjust for known differences between respondents and nonrespondents.

Table B.1
Sample Sizes for Survey Data Collection

Survey Type	1991 Sample Before CJRA	1992-93 Sample After CJRA Pilot Plan	Total
Cases, main evaluation	5,149	5,222	10,371
Judicial officers (cases)	0	281 (5,222)	281 (5,222)
Lawyers, main evaluation	9,777	9,423	19,200
Litigants, main evaluation	19,949	20,272	40,221

¹If there was more than one lawyer for a litigant, we surveyed the lead counsel. If the lawyer surveyed indicated we should have surveyed some other lawyer for this litigant, then we also surveyed that other lawyer.

Judge Survey

After closure of cases in the 1992–93 sample, judges were surveyed. Only 7 percent of the cases were still open in January 1996 when we mailed our last judge surveys, and a total of 281 judicial officers have been surveyed regarding 4,872 closed cases. Completed survey responses have been received from 251 judicial officers regarding 3,280 closed cases. The completion rate is 89 percent of the judicial officers, and we received completed surveys for 67 percent of the closed cases.

Our procedure was to mail a questionnaire to the judicial officer along with a copy of the docket for the closed case, then send a reminder one month later. If the judicial officer did not complete the survey within two months, then a RAND letter was sent along with another copy of the questionnaire.

Table B.2 shows the number of closed cases by district for which the judge was surveyed and by the number of completed responses.

Table B.2
Judge Survey Response Data for 1992–93 Civil Case Sample

District	Total Cases	Closed Cases	Number Complete	Complete as Percent of Closed Cases
AZ	252	229	112	49%
CA(C)	284	272	157	58%
CA(S)	250	245	141	58%
DE	251	241	172	71%
FL(N)	267	240	176	73%
GA(N)	277	270	192	71%
IL(N)	250	230	131	57%
IN(N)	283	240	150	63%
KY(E)	249	228	159	70%
KY(W)	251	229	107	47%
MD	266	257	217	84%
NY(E)	258	225	184	82%
NY(S)	320	265	197	74%
OK(W)	252	249	220	88%
PA(E)	267	266	227	85%
PA(M)	250	240	193	80%
TN(W)	252	243	124	51%
TX(S)	272	265	128	48%
UT	251	205	170	83%
WI(E)	250	233	123	53%
Total	5,222	4,872	3,280	67%

Lawyer Survey

In addition to surveying lawyers on closed cases in both the 1991 and the 1992–93 civil case samples, we also surveyed lawyers on the 7 percent of the 1992–93 sample cases that were still open in January 1996 when we did our final surveys. The January 1996 mailings for open cases used a RAND cover letter rather than the chief judge cover letter. For these cases that were still open, the lawyers were asked to indicate if the case had closed since December 1995, and if not, then the lawyers were asked to fill out as much of the questionnaire as possible (recognizing that some information is not sensitive and can be provided, some information such as the total legal fees and expenses are not available yet, and some information such as their views on monetary stakes may be considered too sensitive to provide because of the ongoing litigation.)

For the 1991 sample, we have received 4,870 completed lawyer questionnaires, which is 50 percent of the lawyers named on the dockets for the sample cases. The response rate from lawyers who potentially could have completed the survey was 58 percent. By “potentially could have completed the survey” we mean we mailed a survey to the lawyers, they received it, and they were able to respond. We did not consider the lawyer a potential respondent if the case was reported to be reopened and still open or on appeal, if the case was transferred to another court outside the district, or if the lawyer was unable to respond (because, for example, the lawyer was deceased or unavailable, the survey was undeliverable at the last known address, information on the case was no longer available to the lawyer because it was in cold storage, or the lawyer said he or she had little or nothing to do with the case.)

For the 1992–93 sample, we have received 4,061 completed lawyer questionnaires for closed cases and 252 for open cases. The completion rate for all lawyers on closed cases was 47 percent, and the response rate from the lawyers who potentially could have completed the survey was 60 percent.

Tables B.3 and B.4 show the final counts of the number of lawyers surveyed and the number of completed responses, by district for the 1991 and the 1992–93 samples, respectively.

Litigant Survey

For the 1991 sample, we have 2,824 completed responses, about 14 percent of the litigants in the sample. One major factor keeping the response rate down is the fact that we have addresses for only 61 percent of the litigants in the 1991 sample.² The response rate from all litigants with addresses was 23 percent, while the response

²We received litigant addresses from three sources: (1) from the docket as we were preparing the surveys (typically a few dozen addresses in each district, including the pro se litigants whose addresses are almost always on the court docket); (2) from the lawyers we surveyed (although less than half of those who completed our survey also gave us their litigant's address); and (3) from the district case files or a form lawyers are supposed to fill out at filing in some districts. (After we have exhausted the potential of the first two sources, we asked the district to look up addresses in the case files.)

Table B.3
Lawyer Survey Response Data for 1991 Civil Case Sample

District	Total Lawyers	Number Complete	Complete as Percent of Total
AZ	555	222	40%
CA(C)	428	169	39%
CA(S)	477	218	46%
DE	547	323	59%
FL(N)	485	254	52%
GA(N)	500	273	55%
IL(N)	454	208	46%
IN(N)	482	251	52%
KY(E)	518	230	44%
KY(W)	535	274	51%
MD	453	230	51%
NY(E)	429	188	44%
NY(S)	390	172	44%
OK(W)	528	337	64%
PA(E)	491	217	44%
PA(M)	651	292	45%
TN(W)	429	215	50%
TX(S)	515	281	55%
UT	508	301	59%
WI(E)	402	215	53%
Total	9,777	4,870	50%

rate from litigants who potentially could have completed the survey was 35 percent.³ This 35 percent is substantially less than the comparable 58 percent for lawyers, in part because of the difficulty in finding the right person in an organization to send the survey to and in part because of the existence of litigants who were named on the case but had very little involvement because they were not one of the principal litigants.

For the 1992-93 sample, we have 2,264 completed responses from about 13 percent of the litigants on the closed sample cases. For the 57 percent of the litigants whose addresses were obtained, the response rate was 24 percent, while the response rate from litigants who potentially could have completed the survey was 32 percent. Litigants were not surveyed about open cases.

³By "litigants who potentially could have completed the survey," we mean we had an address and mailed a survey to the litigant, they received it, and they were able to respond. We did not consider the litigant a potential respondent if the case was reported to be still open or on appeal, transferred to another court, or the litigant was unable to respond (because, for example, the litigant was deceased or unavailable, the survey was undeliverable at the last known address, information on the case was no longer available to the litigant, or the litigant said he or she had little or nothing to do with the case).

Table B.4
Lawyer Survey Response Data for 1992–93 Civil Case Sample

District	Total Lawyers	Lawyers on Closed Cases	Number Complete	Number Complete on Closed Cases	Complete as Percent of Closed Cases
AZ	442	374	216	194	52%
CA(C)	478	454	183	178	39%
CA(S)	406	397	160	155	39%
DE	480	466	275	270	58%
FL(N)	403	359	190	169	47%
GA(N)	487	474	226	221	47%
IL(N)	445	399	215	196	50%
IN(N)	446	407	203	191	47%
KY(E)	535	465	249	224	48%
KY(W)	489	441	232	216	49%
MD	444	423	246	238	56%
NY(E)	425	365	159	142	39%
NY(S)	516	418	178	154	37%
OK(W)	605	599	304	302	50%
PA(E)	494	492	198	198	40%
PA(M)	454	437	204	199	46%
TN(W)	446	420	190	181	43%
TX(S)	545	530	198	186	35%
UT	457	385	248	218	57%
WI(E)	426	396	239	227	57%
Total	9,423	8,701	4,313	4,061	47%

The problem in getting addresses was a known difficulty from prior litigant surveys on other studies and was discussed with the Judicial Conference Court Administration and Case Management Committee before we began this study. Both we and the committee felt that the litigants' inputs were so important that we should seek them even if the response rate was low because lack of addresses.

Details of the number of litigants surveyed and the number completed by district are shown in Tables B.5 and B.6 for the 1991 and the 1992–93 samples, respectively.

ISSUES IN NONRESPONSE TO SURVEYS

Uncollected survey data, whether due to nonresponse to a mailed survey or due to lack of a correct mailing address for a litigant, has two potential negative effects on any analysis. It increases the variability of the estimates, and it may introduce bias in the estimates. The variability of estimates increases because the sample size is reduced from the full sample to the responding subsample. For example in the 1991

Table B.5
Litigant Survey Response Data for 1991 Civil Case Sample

District	Total Litigants	Total with Address	Number Complete	Complete as Percent of Total	Complete as Percent of Total with Address
AZ	1,475	922	174	12%	19%
CA(C)	945	403	71	8%	18%
CA(S)	1,068	371	93	9%	25%
DE	1,119	749	213	19%	28%
FL(N)	806	362	93	12%	26%
GA(N)	931	774	191	21%	25%
IL(N)	1,018	477	91	9%	19%
IN(N)	860	599	140	16%	23%
KY(E)	968	314	72	7%	23%
KY(W)	977	692	173	18%	25%
MD	890	752	179	20%	24%
NY(E)	868	470	85	10%	18%
NY(S)	1,063	759	114	11%	15%
OK(W)	1,024	558	162	16%	29%
PA(E)	853	757	164	19%	22%
PA(M)	1,275	716	193	15%	27%
TN(W)	786	473	101	13%	21%
TX(S)	984	603	122	12%	20%
UT	1,088	771	226	21%	29%
WI(E)	951	566	167	18%	30%
Total	19,949	12,088	2,824	14%	23%

attorney survey, where we have approximately a 50 percent response rate, the standard error for estimated means will increase by a factor of about 1.4 over what it would have been had we observed all the data. Given a very large sample size and reasonably little nonresponse, loss in precision (i.e., increase in variability) is not too problematic.

However, nonresponse bias may be problematic no matter how large the sample. Nonresponse may introduce bias unless respondents constitute a random subsample of the total sample. There are two ways that respondents can deviate from a random subsample. First, there can be differential nonresponse across identifiable subgroups, with random nonresponse within each subgroup. For example, we found that lawyers are less likely to respond if their cases were over 30 months long than they are for shorter cases. However, conditioning on length of the case, it may be true that lawyers are equally likely to respond. Second, deviation from a random subsample occurs when, even after controlling for variables that are known for all cases, nonresponse depends on variables that are only known for respondents. For example, respondents who are more satisfied with the case outcome and case processing might tend to be more likely to respond, regardless of case length or any other predictor variable known for all cases.

Table B.6
Litigant Survey Response Data for 1992–93 Civil Case Sample

District	Total Litigants	Litigants on Closed Cases	Total with Address	Number Complete	Complete as Percent of Total Closed	Complete as Percent of Total Closed with Address
AZ	1,255	807	767	148	18%	30%
CA(C)	1,170	943	468	51	5%	14%
CA(S)	932	772	312	55	7%	21%
DE	1,006	912	571	143	16%	28%
FL(N)	924	759	552	145	19%	32%
GA(N)	1,085	866	708	130	15%	23%
IL(N)	840	757	300	48	6%	18%
IN(N)	890	767	564	152	20%	31%
KY(E)	887	674	563	116	17%	27%
KY(W)	1,014	880	534	117	13%	25%
MD	904	823	812	146	18%	20%
NY(E)	890	725	547	58	8%	13%
NY(S)	1,596	1,087	1,166	128	12%	16%
OK(W)	1,054	862	339	86	10%	31%
PA(B)	1,070	1,012	688	139	14%	21%
PA(M)	877	808	536	107	13%	22%
TN(W)	889	821	212	54	7%	28%
TX(S)	1,016	885	596	92	10%	18%
UT	1,018	830	653	166	20%	31%
WI(E)	955	899	647	183	20%	30%
Total	20,272	15,889	11,535	2,264	13%	24%

We used nonresponse weights to reduce the potential bias caused by differential nonresponse rates that were correlated with characteristics known for all people who were surveyed, as discussed below. To the extent that response depends only on these observable factors that are known for all people surveyed, these weights should remove most of the bias due to nonresponse. However, these weights cannot remove bias associated with response that depends on factors known only to the attorney or other surveyed person (e.g., satisfaction). Because we had many characteristics to use for modeling nonresponse among attorneys and judges, and because we have responses from about half or more of them, we feel that for attorneys and judges, response is well modeled. Hence, we feel that after controlling for factors known for all people surveyed, respondents represent a nearly random subsample. We cannot verify the accuracy of this claim, and small biases could exist even when we use our nonresponse weights.

For litigants we had an equally rich set of predictors known for each person to be surveyed, but with only 11 percent and 14 percent survey completion response rates for our 1992–93 and 1991 samples respectively, it is hard not to be suspicious of non-response bias. Thus, while we have completed surveys from over 5,000 litigants and we have no clear evidence that nonresponse bias exists after controlling for the factors included in our nonresponse models, we prefer to be cautious and believe that

our litigant survey data should not be used for inferential statistical analyses. We provide nonresponse weights and adjust the information in our descriptive tables to account for known differential response among litigants, but we are not confident that this will adequately remove all possible biases from these data. Hence, we will confine our analyses of these litigant data to descriptive tables, and the information in those tables should be viewed as suggestive rather than statistically definitive.

Another method that we sometimes also used to attempt to correct for any bias that may exist is imputing values for the each item of missing data for nonrespondents. That method is discussed in subsequent appendices where we discuss our analyses of factors predicting outcomes such as time to disposition and litigation cost.

We stress that for most variables known for the entire sample, there is no statistically significant nonresponse bias. We also stress that we have multiple sources of data on some respondent variables, so that in the aggregate dataset, we know a lot more than we know from a single litigant or lawyer.

Finally, if there is residual bias that we cannot adequately adjust for, we can still draw valid policy conclusions if we believe that any bias is consistent between the two sets of data being compared. For example, if responding litigants in both pilot and comparison districts are more likely to be satisfied than nonrespondents, and the "satisfaction bias" is the same in both pilot and comparison districts, then the comparisons of the *differences* between respondents from pilot and comparison districts should be valid.

FACTORS THAT SIGNIFICANTLY AFFECTED RESPONSE RATES

We conducted analyses to investigate differential nonresponse rates. In these analyses, we used contingency tables to look at response rates for different groups with various characteristics. The characteristics for which we had information on all or nearly all persons surveyed were numerous, including: time to disposition of case, nature of suit (e.g., type of tort, contract, prisoner, or other case), origin of case, case procedural progress at termination, method of disposition, district, year filed, mailed to named individual or organization address, referral to arbitration, amount demanded, class action allegation, whether the case was transferred or consolidated, average amount of judge work time for the type of case, which side won judgment, basis for federal court jurisdiction, management approach, closure status, district pilot or comparison, year of termination, type of litigant, type of lawyer, and whether litigant had an attorney. For judges and lawyers, we also considered the number of surveys each person received.

Overall we found that, for most characteristics known for all people surveyed, there were not substantial significant differences among either attorney or litigant response rates. For example, no appreciable difference existed in the response rates (for mailed surveys) for plaintiff litigants (24 percent) or defendants (20 percent). Similarly there was no difference in the proportion of litigants for whom we had addresses to mail surveys in pilot districts (54 percent) and comparison districts (54 percent). And the average amount of judicial work required for the type of case had

little effect on attorney response rates (46, 49, and 50 percent for high, medium, and low judicial work cases, respectively). As illustrated in the examples, both the rates of mailing of surveys to litigants and the response to mailed surveys by lawyers and litigants were invariant to many characteristics.

There are some notable exceptions to this generalization, however, and they are discussed below for litigants, lawyers, and judges separately.

Factors That Significantly Affected Lawyer Response Rates

There are seven variables that were associated with statistically significant differences in attorney response rates:

1. Time to disposition,
2. District,
3. Number of litigants represented by attorney,
4. Nature of suit,
5. Number of sample cases involving the attorney,
6. Transferred cases, and
7. Consolidated cases.

Furthermore, cases from our 1992–93 sample that were open at the time of our final surveys had significantly lower response. All 1991 cases had been closed at least once at the time of our study (although 14 of the 5,149 were still in a reopened or appeal status at the time of our final surveys).

Excluding the open cases from our 1992–93 sample, cases that closed within 12 to 18 months from opening yielded the highest lawyer response rates (50 percent versus 45 percent for other cases that closed earlier or later than that time period). Response rates varied by district from a high of 64 percent to a low of 39 percent in 1991 and from 58 percent to 35 percent in 1992–93. Cases with one or two litigants in 1991 had the highest response rates, about 51 percent, and those with greater than 10 had the lowest rates, 42 percent. The patterns were similar but less pronounced in 1992–93. Lawyers from personal injury tort cases tended to have lower than average response rates in both 1991 and 1992–93, and attorneys on contract–recovery cases had higher than average response rates in both samples. Attorneys with one or two cases in our sample had the highest responses rates, and rates declined as the numbers of cases the attorney had in our sample grew. Attorneys for both transferred and consolidated cases had lower response rates, 33 percent and 38 percent, respectively, for 1991 and 21 percent and 44 percent for 1992–93. This compares to 50 percent for other cases in 1991 and 48 percent for 1992–93.

To avoid bias in our estimates resulting from this differential nonresponse, we developed nonresponse weights for responding attorneys. We used the variables listed above to construct logistic regression models for an attorney's propensity to respond.

For each responding attorney, the model yields an estimated propensity to respond, p . Our nonresponse weight is $1/p$. The motivation for this weight is as follows: Few attorneys with a low propensity to respond actually respond; hence those that do respond must represent a larger number of nonrespondents and receive a higher weight. Alternatively, most attorneys with a high propensity to respond do actually respond and so those that respond represent fewer nonrespondents and receive a lower weight.⁴

We created our final analysis weights as the product of our nonresponse weights and our sampling weights (see Appendix A for a discussion of the sampling weights). The weights were truncated at the 95th percentile of this product in 1991 (90th percentile in 1992–93) to avoid extreme variation in weights that can result from a combination of low response and undersampling for some types of cases and persons surveyed.

Factors That Significantly Affected Litigant Response Rates

We analyzed the response process for litigants as a two-stage process. First, we needed an address and a closed case before a litigant survey could be mailed. We had the address for about 60 percent of litigants in the 1991 sample and about 57 percent of the litigants in the 1992–93 sample. Second, given that we had an address and the case was closed no later than December 1995 so that we could mail a survey, a litigant could either respond or not. Of those litigants to whom we mailed a survey, about 23 percent responded in both samples. Thus, we have completed surveys from a combined total of 14 percent of the litigants in our 1991 sample and 11 percent in the 1992–93 sample. We modeled this as a two-stage address/response process because the factors that contributed to our ability to mail a survey differed from those that contributed to a litigant's decision to respond. Also, we were concerned that some factors contributed to both address availability and responses but in different ways. For example, the PA(E) district required litigants' addresses at the time of case filing, so we had a very high mailing rate for this district. However, large urban areas typically had lower response rates to our mailed surveys.

Model for Address Availability for the Litigant Survey: The following 11 factors were found to contribute significantly to our ability to mail a survey to litigants:⁵

1. Attorney status: pro se; no attorney identified in court files, but not pro se; attorney identified but no attorney response to our survey; and attorney identified and attorney responded to our survey,

⁴For a discussion of nonresponse weights, see Little and Rubin (1987).

⁵In 1993 we did not mail surveys to litigants from open cases. However when modeling nonresponse, we treated these cases as eligible respondents. We specifically did not include an open flag in our model so that our response weights would apply to litigants from both open and closed cases, even though we know that litigants from open cases have no probability of receiving a survey. Our response weights are appropriate for correcting nonresponse bias provided that, given the predictors used in our model, the responses of litigants from open cases would not differ systematically from the responses of litigants in similar but closed cases. For example, if litigant costs are higher in long cases but not substantially higher in open cases than in the long but closed cases, then our weights would provide unbiased analyses of litigant costs.

2. District,
3. Jurisdiction: U.S. defendant, or other basis of jurisdiction,
4. Number of litigants involved in the case,
5. Length of case: 13 to 24 months, or other,
6. Nature of suit: PI tort, contract, prisoner, bankruptcy or intellectual property, or other,
7. Transferred case,
8. Consolidated case,
9. Litigant type: government, private organization, or individual,
10. Name of individual is known (i.e., not just government or private organization name), and
11. Management approach: complex track, minimal management track, or arbitration.

Because addresses were readily available for pro se litigants who were proceeding without an attorney, the mailing rate was very high (95 percent in 1991 and 90 percent in 1992–93) for these litigants. This result was invariant to other characteristics of the case and hence we modeled these litigants' address availability separately from other litigants. For the remaining litigants who were not pro se, litigants with no attorneys identified in the court files had the lowest address availability rate (43 percent in 1991 and 39 percent in 1992–93). Not having an attorney named in the court file may happen if the litigant either did not hire an attorney or if the litigant had an attorney but the case terminated before the attorney made his or her existence known to the court. Litigants with nonresponding attorneys had nearly as low of a rate (42 percent in 1991 and 39 percent in 1992–93), and litigants with responding attorneys had the highest rate of address availability (68 percent and 57 percent in 1991 and 1992–93, respectively). This was due to the fact that we asked the attorney for the litigant address, in addition to checking in the court file.

There was considerable variation in the address availability rate by district. In 1991, the highest rate was 87 percent for litigants who were not pro se in PA(E), which requires the address of litigants when the case is filed, and the lowest was 25 percent. In 1992–93, MD had the highest address availability with 81 percent, while the lowest rate from any district was 15 percent. In 1991, we had a 13 percentage point lower chance of mailing to U.S. defendants than to other litigants because we often lacked an individual's name and could not send the litigant survey to a current government employee who knew something about the case. This problem did not exist in 1992–93 and we found little difference among address availability between government and private parties.

Cases that terminated quickly or lasted a long time had about a 10 percentage point lower rate of address availability than cases that closed after 13 to 24 months (about 58 percent as compared to 68 percent in 1991, and 46 percent as compared to 56 percent in 1992–93). This probably is due to short cases having less information in the

court file, and hence less likelihood of finding an address there, and due to a lower response rate from lawyers on longer cases.

In 1991, personal injury tort cases had the lowest address availability rate (50 percent), followed by bankruptcy and intellectual property cases. Contract cases had a higher mailing rate than either of these previous types of cases, but it was lower than for prisoners or others. In 1992–93, the address availability was again lowest for personal injury tort, product liability cases, and bankruptcy cases, but miscellaneous other cases, prisoner cases, and civil rights cases also had relatively low address availability. In 1992–93, contract cases tended have the highest levels of address availability (above 50 percent).

In both years, consolidated and transferred cases had lower address availability. Also, in both years, the mailing rate went down as the number of litigants on the case went up. This is most likely the result of a greater number of peripheral litigants. Finally, in 1992–93 addresses were more likely to be available for cases sent to arbitration (71 percent compared to 45 percent).

Model for Response Given That a Litigant Survey Was Mailed: The following factors contributed significantly to response to mailed litigant surveys:

1. Attorney status: pro se; no attorney identified in court files, but not pro se; attorney identified but no attorney response to our survey; and attorney identified and attorney responded to our survey,
2. District,
3. Number of litigants involved in the case,
4. Number of lawyers responding on entire case,
5. Nature of suit: personal injury product liability; contract (recovery, other, and miscellaneous); prisoner; civil rights, not prisoners; or other,
6. Transferred cases, and
7. Type of litigant: government, organization, or individual.

Litigants with no attorneys or with nonresponding attorneys tended to have the lowest response rates (12 and 16 percent, respectively, for 1991 and 14 and 21 percent for 1992–93) and litigants with responding attorneys had the highest rates (27 percent for 1991 and 1992–93). In 1991, pro se litigants fell between these two groups with a 25 percent response rate. The response rate for pro se litigants was 28 percent in 1992–93. The differential response rate for mailed litigant surveys is consistent with our expectation that litigants without named attorneys, and not pro se, would have limited involvement in the cases and be less likely to respond.

There was considerable variation in response rates across districts. The range was from 7 to 36 percent in 1991 and 13 to 31 percent in 1992–93.

The probability of a litigant responding increased slightly with the number of attorneys responding on the case but decreased with the number of litigants on the case.

Given that the number of litigants on a case tends to correlate positively with the number of attorneys, it may appear surprising that a greater number of litigants indicates lower response but a larger number of responding lawyers indicates a higher response. However, the number of attorneys responding is not highly correlated with the number of litigants ($r=0.26$). Hence, given the number of litigants, there is variation in the number of attorneys who respond, and greater attorney response corresponds to greater litigant response. This probably occurs because some litigants will consult their lawyers before responding, and lawyers who respond are more likely to approve of their litigants responding.

Litigants involved in personal injury product liability cases had the highest response rate (29 percent in 1991 and 28 percent in 1992–93). In 1991, litigants in miscellaneous or other contract cases (i.e., not in insurance or recovery contract cases) had the lowest response rate (18 percent), while litigants in contract-recovery cases had the lowest response rate in 1992–93 (3 percent). In 1991, 22 percent of litigants on prisoner cases responded (26 percent in 1992–93), and about 24 percent of litigants in civil rights cases not involving prisoners responded (27 percent in 1992–93). Also in 1992–93, government and individual litigants responded less frequently than private organization litigants (21 percent compared to 28 percent).

Our nonresponse weights for litigants are conceptually similar to those for attorneys. For each responding litigant, the weight equals $1/p$ where p denotes the propensity to respond. For litigant respondents, our estimate of the propensity to respond equals the product of our estimated address availability for the litigant and our estimated propensity to respond given that we have an address. That is $p = p(A)*p(R)$, where $p(A)$ denotes the probability that we find an address and $p(R)$ denotes the propensity to respond given that we have an address. The final weight for a litigant is the product of the nonresponse weight and the sampling weight. As discussed above for lawyers, we truncated the largest weights to reduce variability in our final estimates for litigants.

Factors That Significantly Affected Judge Response

In 1992–93, we surveyed the judge assigned to each case in our sample. Overall we had a 67 percent completion rate for judges for closed cases (they were not surveyed for cases that were still open in January 1996). We found that the response rate for judges differed across five factors:

1. Transferred cases (in or out of the district),
2. Consolidated cases,
3. Time to disposition,
4. District, and
5. Number of surveys the judge received.

Judges responded less often for transferred cases than for other cases (49 percent compared to 64 percent). Judges also responded less often to consolidated cases (43

percent compared to 63 percent). Judge response rates declined as time to disposition increased (72 percent response for cases lasting less than 6 months and only about 30 percent for cases lasting over 24 months). There was considerable variation among districts in the judge response rates. The high was 87 percent in OK(W) and the low was 43 percent in KY(W). Judges who received a large number of surveys also tended to respond to fewer of those surveys (since the sample size was about the same in each district, smaller districts had a much larger number of sample cases per judge).

Using these factors, we estimated a propensity to respond for each case and used these estimates to create a nonresponse weight using the procedure described above for attorneys.

RECENT TRENDS IN FEDERAL DISTRICT COURTS

INTRODUCTION

The situation in the federal district courts has not been static in recent years. Significant changes have occurred over time in the type and number of cases filed, and in when and how they have been terminated.

The purpose of this appendix is twofold: first, to set out the conditions all courts were operating under prior to and after the implementation of CJRA; and second, to examine our two initial groupings of districts (“pilot” and “comparison”)¹ to see if any critical dissimilarities appear to exist.

To understand what the effects of the Civil Justice Reform Act may have been upon case processing, we need to understand the composition of the business of the federal district courts. While we have an extensive sample of over 10,000 cases from which to explore the effects of varying management techniques, litigation is influenced not only by the actions of the judges, lawyers, and litigants involved with each sample case but also by the demands upon a district’s resources by other cases, criminal as well as civil.

Though the trend information presented here is useful for understanding changes in the district court environment, one should not make judgments about the impact of the CJRA pilot program solely using this information. Factors other than the aggregated data presented here must be considered in evaluating the pilot program and various case management policies. Our multivariate analyses, presented elsewhere in this report, consider the variables discussed in this appendix.

DATA SOURCES AND METHODOLOGY

Our primary source of information is the Federal Judicial Center’s Integrated Federal Courts Data Base (IDB), which we modify so that we have a single record for each case regardless of the number of reopenings of that case. We concentrate on cases

¹The charts and tables below generally present aggregate district-level data in the groupings of 10 pilot, 10 comparison, and all 94 districts combined. The residual category of “other” (i.e., neither pilot nor comparison) is not shown.

filed or terminated on or after October 1, 1986, and generally present our data by "reporting year" (RY) ending September 30.

Each year, the Administrative Office of the United States Courts (AO) produces a number of useful compilations of tables and charts that document civil filings and terminations (as well as the year-end pending caseload).² Much of the data contained in those reports comes from forms submitted by the various districts for each case's opening (the "JS-5" Filing Report as well as the "JS-44" Civil Cover Sheet) and closing (the "JS-6" Termination Report). Because a civil case can be closed and then reopened a number of times for varying reasons, more than one JS-5 and JS-6 report may be submitted to the AO by a district during a case's "life" (i.e., the time from the first opening to the latest closure). These reports contain summary information for a case that include such items as the dates of opening and closing, basis of federal court jurisdiction over the parties and the subject matter of the dispute (e.g., diversity of citizenship/amount in controversy, the United States as a party, federal question jurisdiction), case type using a set of about 100 codes (e.g., 110 is for actions involving insurance contracts, 355 is for personal injury tort actions involving motor vehicle products liability issues, and 442 is for actions involving employment civil rights), the amount requested from the defendant in the complaint, the point in the litigation process where the case terminated (e.g., during a jury trial), the method of disposition (e.g., a transfer to another district court), and other data.

As part of its Integrated Data Base Project, the Federal Judicial Center's Research Division (FJC) obtains computer files containing the Administrative Office's opening and closing data and creates files that unify the case-level forms into a single record for each termination in a case's life (if the case is pending at the end of the last year of complete data, a similarly structured pending record is created except that data fields relating to the case's termination, such as method of disposition, are set to a missing value). After the data are converted, standardized, and checked for obvious errors, the Integrated Federal Courts Data Base (IDB) is then released by the FJC into the public domain and is available from the Inter-University Consortium for Political and Social Research in Ann Arbor, Michigan.

Besides the data for the civil business of the federal district courts, the FJC also produces a set of parallel IDB files for criminal cases. The steps outlined above are repeated for each criminal matter filed or terminated (except that the forms used are the "JS-2" Criminal Case Opening Report and "JS-3" Criminal Case Termination Report).

Generally, tables and charts published by the AO and the FJC are done so by reporting year (RY) ending on September 30.³ Because of the special needs of this study, we were provided access to the latest available version of the civil case IDB that had all filings and terminations from July 1, 1970, through December 31, 1995.

²See, for example, Administrative Office of the United States Courts (1994a, 1994b).

³Prior to RY1992, the AO and FJC used a year-end date of June 30 for IDB data. Reports published prior to that time often refer to a "fiscal" or "statistical" year, and care should be exercised when comparing data aggregated by reporting year in this document with earlier studies.

In this appendix, we use the IDB to give us the best possible picture of the demands upon and outputs from the federal district courts before and after the inception of the CJRA of 1990. This includes all cases either filed (even if still pending) or terminated (regardless of when filed) from RY1987 through RY1995. An earlier RAND report presented an analysis of civil litigation in federal district courts for cases filed between 1971 and 1986.⁴

As mentioned earlier, a civil case may have more than one opening or closing report form sent to the AO by the district. While the vast majority of cases are opened only once, about 4 percent are opened two or more times. For example, after a case is initially filed in a federal district court, the judge may rule for the defendant on a motion for summary judgment. A JS-5 is generated at the time of first filing and a JS-6 is sent to the AO after the case "closure." Subsequently, the plaintiff appeals the ruling to the Court of Appeals and is successful in having the judgment overturned. The remanded case continues at the district court level, with the original docket number, and another JS-5 is generated. When the case is closed again later, yet another JS-6 is created. Similar results can occur if the defendant defaults but later successfully reopens the case for good cause shown, if an anticipated settlement resulting in a voluntary dismissal falls through and the plaintiff restarts the prosecution of the matter, or a host of other common scenarios.

The problem for the researcher in the situations outlined above is that the FJC's IDB file would then contain two or more records with the same docket number. Each of these records would accurately describe the history of each portion of a case's life where the matter was actively being litigated in the district court but each separately would not give a complete picture of the case or the actual beginning and end of the district court's involvement with the case. For these reasons, we decided to create a single record for each actual case (i.e., same filing year, office, and docket number) regardless of the number of records found in the IDB.

This means that our tables and figures of filings and terminations will differ from those found in reports published by the AO and the FJC by about four percent.⁵ Additionally, with Judicial Conference approval, this study excludes asbestos cases, and they are not included in our analysis of systemwide time trends here because this case type is unique and receives special Multi-District Litigation (MDL) management.⁶

We have also performed a similar process to collapse multiple criminal case openings for the same case and defendant into a single record. Again, our counts will differ from those found in AO and FJC reports that rely upon unmodified IDB data only.

⁴Dunworth and Pace (1990).

⁵Since the percentage of non-initial openings (e.g., cases that are reopened, reinstated, or remanded from appellate courts) varies from year to year, the degree of difference from the tables and figures presented here with published numbers from the AO or FJC will also vary.

⁶Almost all asbestos cases were transferred to the Eastern District of Pennsylvania under MDL Number 875. The AO takes a similar tack by dropping the PA(E) asbestos cases from pending counts for that district.

TRENDS IN FULL-TIME-EQUIVALENT JUDICIAL OFFICERS

Caseloads in districts are traditionally compared by using “actions per judgeship.” This attempts to measure the caseload burden relative to the authorized judgeships possessed by a district. However, presenting actions per authorized judgeship can be misleading in the short term because the actual number of judges working at any one time in a district is often very different from the authorized figure as a result of vacancies. There is often a considerable lag in time between an increase in the authorized number of judgeships, or the retirement of a judge, and the actual appointment of a new judge. Moreover, judges can be absent from active duties because of illness or other reasons, and the impact of this downtime on a smaller district can be dramatic.

On the other side of this equation, a district’s resources can be augmented by the help provided by judges on senior status who continue to work at least part time. Senior judges can be of extraordinary benefit to a district, and the availability of senior judges varies widely by district and by year. Since many of them work part time, we computed the full-time-equivalent (FTE) number of senior judges in each district in each year using information from our interviews and information from court data bases on the case activity of the senior judges.

Finally, the issues outlined above also apply to both the number and usage of magistrate-judges. Because many magistrate judges are part time, we had to calculate the FTE number. Also, districts vary in the tasks assigned to these judicial officers. In some districts, magistrate judges handle almost all pretrial civil matters, while in others they concentrate on criminal arraignments and a limited number of specific civil case tasks referred to them by the assigned Article III judge.

Our calculations of full-time-equivalent judicial officers takes the number of authorized Article III judgeships, subtracts the proportion of a year the judgeship was unfilled or vacant due to illness or some other reason, adds the full-time-equivalent number of senior judges in the year, adds the number of full-time magistrate judges, and finally adds the full-time-equivalent number of part-time magistrate judges. This is the best possible estimate of the total available judicial resources.

We found that, in the aggregate, comparison districts had about the same number of FTE judicial officers as pilot districts had from RY1987 to RY1991 (the last year prior to implementation of CJRA), and in RY1995. However, between RY1992 and RY1994 the pilot districts had 10 to 15 more FTE judicial officers than the comparison districts.

The pilot and comparison districts combined had 218 district judgeships in RY1995, which was about a third of the 645 authorized nationally. Between RY1987 and RY1995, the authorized judgeships went up 13 percent in pilot and comparison districts combined, while the FTE number of judicial officers rose 23 percent because FTE senior judges and magistrate judges grew faster than district judgeships. Time trends are presented in Figure C.1 and Table C.1.

Figure C.1
Full Time Equivalent Judicial Officers: RY1987 - RY1995

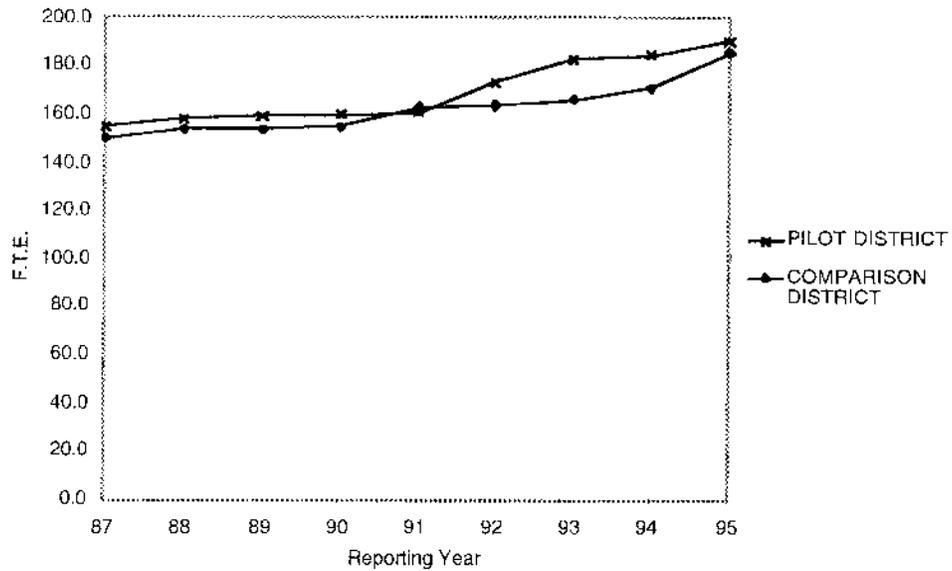


Table C.1
Full-Time-Equivalent Judicial Officers: RY1987-RY1995

District Type	Reporting Year								
	87	88	89	90	91	92	93	94	95
PILOT DISTRICT									
District Judgeships	98.3	98.3	98.3	98.0	112.0	112.0	112.0	112.0	112.0
- FTE Vacancies	-7.4	-9.0	-7.4	-10.5	-25.6	-21.7	-13.8	-16.8	-8.3
+ FTE Sr. Judges	16.4	21.0	20.7	23.1	23.3	29.2	27.2	28.6	22.4
SUBTOTAL: JUDGES	107.3	110.2	111.5	110.6	109.7	119.4	125.4	123.8	126.1
+ Full Time Mag. Judges	41.0	41.0	41.0	43.0	46.0	49.0	54.0	57.0	61.0
+ FTE Part Time Mag. Judges	6.8	6.8	6.8	6.1	5.4	5.0	3.6	3.6	3.2
SUBTOTAL: MAG. JUDGES	47.8	47.8	47.8	49.1	51.4	54.0	57.6	60.6	64.2
TOTAL FTE JUD. OFFICERS	155.2	158.0	159.4	159.7	161.1	173.5	183.0	184.4	190.3
COMPARISON DISTRICT									
District Judgeships	95.0	95.0	95.0	95.0	106.0	106.0	106.0	106.0	106.0
- FTE Vacancies	-6.3	-6.0	-6.1	-6.3	-12.9	-16.0	-15.8	-14.7	-7.9
+ FTE Sr. Judges	12.7	13.7	13.7	12.8	13.5	15.0	15.2	19.0	21.5
SUBTOTAL: JUDGES	101.4	102.7	102.5	101.5	106.6	105.0	105.3	110.4	119.6
+ Full Time Mag. Judges	37.0	40.0	40.0	43.0	48.0	52.0	54.0	55.0	61.0
+ FTE Part Time Mag. Judges	12.2	11.5	11.5	10.4	9.0	6.8	6.8	5.8	5.4
SUBTOTAL: MAG. JUDGES	49.2	51.5	51.5	53.4	57.0	58.8	60.8	60.8	66.4
TOTAL FTE JUD. OFFICERS	150.7	154.2	154.0	155.0	163.6	163.8	166.2	171.1	186.0

TRENDS IN CIVIL LITIGATION IN FEDERAL DISTRICT COURTS

Civil Case Filings and Terminations

Systemwide, the district courts do not have a constant level of total filings and terminations, and did not have a steady trend in filings or terminations, either. For example, there were 12 percent fewer filings in RY1990 than in RY1987, but then civil filings rose 23 percent from RY1990 to RY1995, as displayed in Figure C.2. Similarly, civil case terminations declined from RY1987 to RY1991, and then rose from RY1991 to RY1995 as shown in Figure C.3. The CJRA pilot and comparison districts as groups have experienced similar declines and then increases in both filings and terminations between RY1987 and RY1995, as shown in the same figures, and in Tables C.2 and C.3. These variations in filing and termination counts make simple comparisons among districts over time problematic.

Pending Civil Cases

The relationship between the number of filings and terminations determines the change in the backlog of pending cases from year to year. Prior to RY1991, districts—

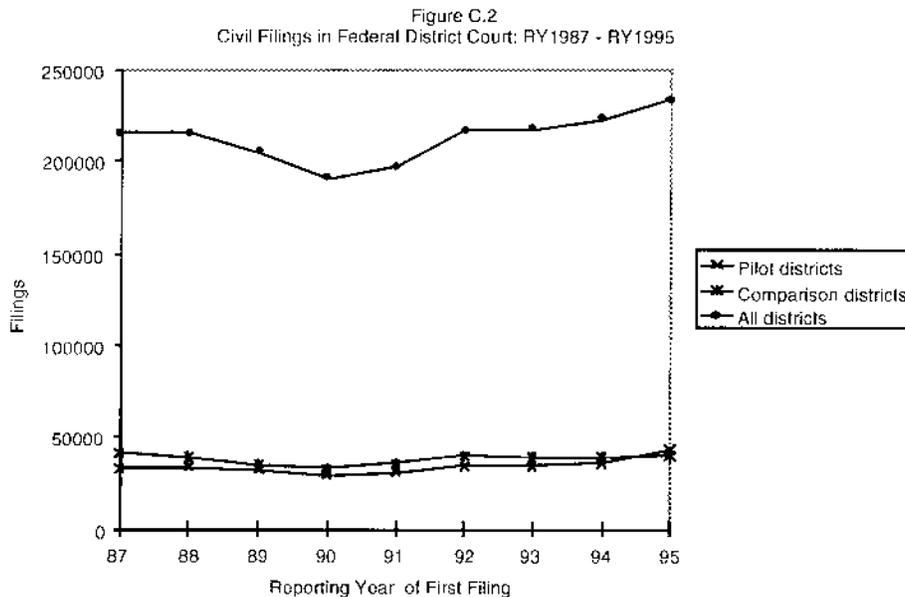


Figure C.3
Civil Terminations in Federal District Court: RY1987 - RY1995

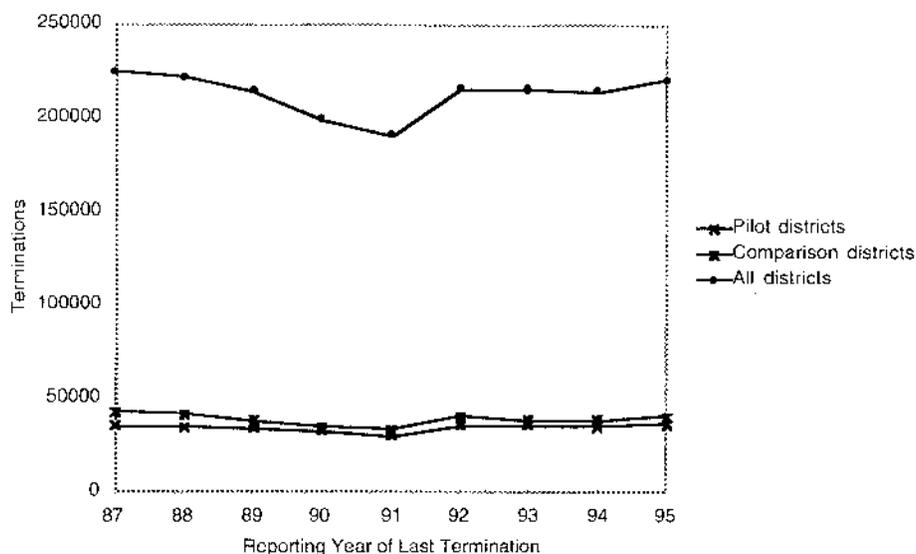


Table C.2

Civil Filings in Federal District Court: RY1987-RY1995

District Type	Reporting Year									Total All Years
	87	88	89	90	91	92	93	94	95	
Other districts	141777	142778	136745	127493	130328	142407	144180	147957	150982	1264647
Pilot districts	32984	33938	32646	29605	30740	34674	34539	36211	43340	308677
Comparison districts	40850	38850	35487	33125	36043	39982	38924	39229	39390	341880
All districts	215611	215566	204878	190223	197111	217063	217643	223397	233712	1915204

NOTES: Asbestos cases not included. Year of filing is for first reported filing, irrespective of number of re-openings.

Table C.3

Terminations in Federal District Court: RY1987-RY1995

District Type	Reporting Year									Total All Years
	87	88	89	90	91	92	93	94	95	
Other districts	146157	146029	142739	131840	128520	140462	141575	141058	144583	1262963
Pilot districts	35414	34350	33838	32602	29180	35201	34967	34248	36073	305873
Comparison districts	42681	41158	37354	34419	32916	39468	37804	38561	39460	343821
All districts	224252	221537	213931	198861	190616	215131	214346	213867	220116	1912657

NOTES: Asbestos cases not included. Year of termination is for last known closing irrespective of number of re-openings.

whether pilots, comparisons, or all combined—generally had fewer filings than terminations so the backlog was declining in the years immediately preceding CJRA. However, at about the time of the adoption of the CJRA, that situation changed. End-of-year pending civil case counts have been increasing in recent years, as shown in Figure C.4 and Table C.4. The changes in pending civil case counts relative to what they were in RY1987 are shown in Figure C.5, where it can be seen that the drop in pending cases prior to about 1990 and then the rise after 1990 exist for both pilot and comparison districts just as they do for the nation as a whole.

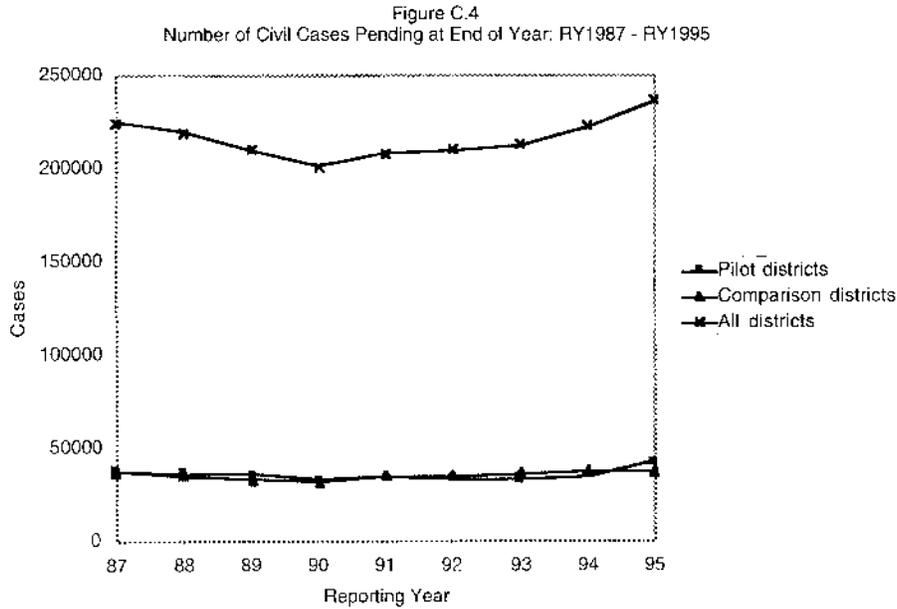
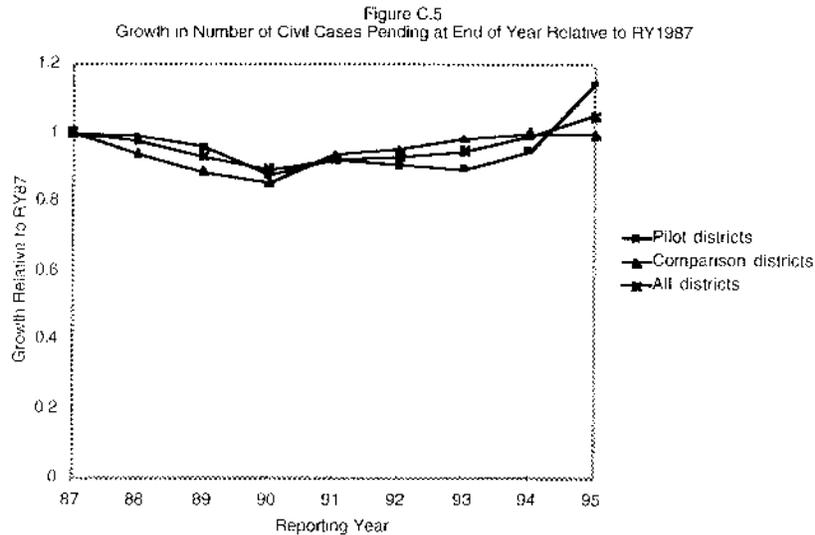


Table C.4
Number of Civil Cases Pending at End of Year: RY1987–RY1995

District Type	Reporting Year								
	87	88	89	90	91	92	93	94	95
Other districts	149782	146531	140537	136190	137998	139943	142548	149447	155846
Pilot districts	37353	36941	35749	32752	34312	33785	33357	35320	42587
Comparison districts	37345	35037	33170	31876	35003	35517	36637	37305	37235
All districts	224480	218509	209456	200818	207313	209245	212542	222072	235668

NOTES: Asbestos cases not included. Year-end pending is calculated by taking end of RY1995 pending counts and adding terminations and subtracting filings for each prior year.

While the number of pending civil cases has been increasing in recent years, we note that the number of civil cases pending over three years has been declining. Data on cases pending over three years for each judge have been collected since 1991. This is required by the CJRA, which indicates that “The Director of the Administrative Office of the United States Courts shall prepare a semiannual report, available to the public,



that discloses for each judicial officer . . . the number and names of cases that have not been terminated within three years after filing.”⁷ As Figure C.6 and Table C.5 show, the number of these older cases has been steadily dropping. From September of 1991 to September of 1995, all districts taken together reported a decrease of about 26 percent, comparison districts had a similar 23 percent decrease, and pilot districts had about a third less three-year-old pending cases.

Types of Cases

There has been a significant shift in the mix of case types in recent years. In RY1987, 30 percent of all filings were contract cases, while in RY1995 that proportion dropped to 13 percent. This drop was complemented by growths in prisoner cases (17 percent to 27 percent) and personal injury tort cases (13 to 18 percent). Contract cases dropped markedly in both pilot and comparison districts, while tort and prisoner cases rose, although not to the same degree in both categories of districts. Details are shown in Figure C.7 and Table C.6.

Some cases of a particular type, recovery of student loans for example, are generally thought to require very little of a judicial officer’s time as they tend to involve a minimum of court appearances and almost never reach the trial stage.⁸ Others, such as RICO cases, may involve really substantial consumption of judge time and other court resources.

⁷These counts do not come from our JDB analysis file but rather from the districts themselves as required every six months by the CJRA §476. Thus, they represent time from the latest filings for each case rather than for the total time the case has existed in the district.

⁸This is not to claim that the impact of these sorts of cases on a district as a whole is also minimal. Even if each of these cases requires little time, by sheer volume alone they may require a significant proportion of the judges’ and clerks’ total work time.

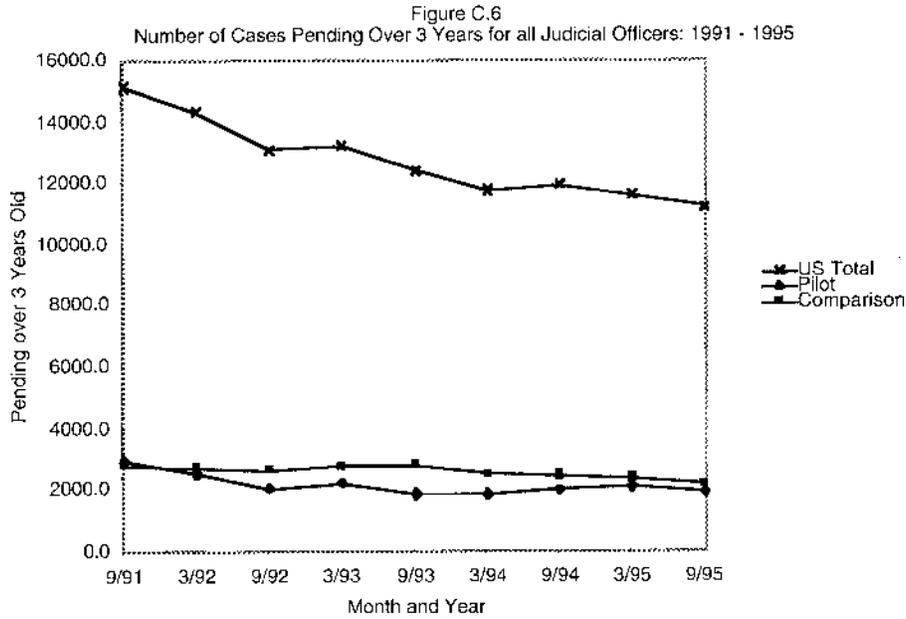


Table C.5
Number of Cases Pending Over 3 Years, All Judicial Officers: 1991–1995

District Type	Month and Year								
	9/91	3/92	9/92	3/93	9/93	3/94	9/94	3/95	9/95
US Total	15109	14291	13052	13224	12406	11752	11938	11605	11179
Pilot	2943	2531	2012	2175	1828	1816	1971	2054	1890
Comparison	2768	2697	2622	2750	2771	2489	2438	2328	2141

We developed three case categories in Appendix A, those that had high, medium, and low average judicial workload per case based on the 1989 judicial time study data. In recent years, there has been a shift toward a case mix with a greater proportion of cases in the high judicial workload category, while cases in the medium and low categories have declined as shown in Table C.7. High judicial workload filings have increased from 19 percent in RY1987 to 30 percent in RY1995, with most of that increase occurring in the last three years.

Origin of Case

When looking only at the initial filings of cases in the district courts, one can see a steady rise in the proportion of those cases that began as a state court matter. In RY1987, 88 percent of cases started as an original proceeding while in RY1995 that

Figure C.7
Types of Civil Cases Filed: RY1987 - RY1995

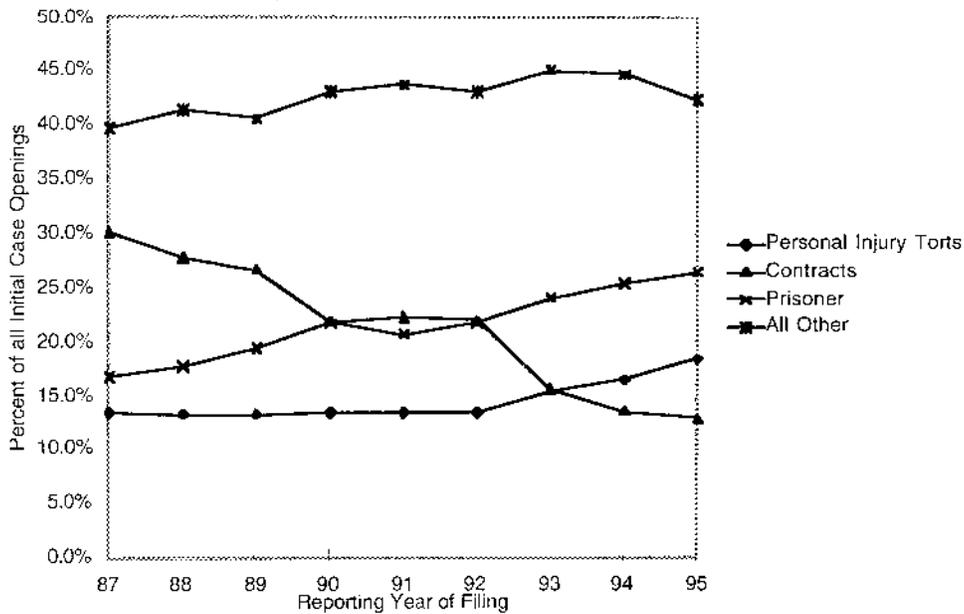


Table C.6

Types of Civil Cases Filed, All Districts: RY1987-RY1995

Case Type	Reporting Year									
	87	88	89	90	91	92	93	94	95	Total
Frequency										
Personal Injury Torts	28748	28448	27194	25352	26249	28842	33450	36823	42733	277839
Contracts	64972	59844	54715	41525	43740	47627	33811	30015	29972	406221
Prisoner	36028	37938	39627	41238	40774	46961	52143	56705	61848	413262
All Other	85863	89336	83342	82108	86348	93633	98239	99854	99159	817882
Total	215611	215566	204878	190223	197111	217063	217643	223397	233712	1915204
Percentage										
Personal Injury Torts	13.3%	13.2%	13.3%	13.3%	13.3%	13.3%	15.4%	16.5%	18.3%	14.5%
Contracts	30.1%	27.8%	26.7%	21.8%	22.2%	21.9%	15.5%	13.4%	12.8%	21.2%
Prisoner	16.7%	17.6%	19.3%	21.7%	20.7%	21.6%	24.0%	25.4%	26.5%	21.6%
All Other	39.8%	41.4%	40.7%	43.2%	43.8%	43.1%	45.1%	44.7%	42.4%	42.7%
Total	100.0%	100.0%	100.0%	100.0%	100.0%	100.0%	100.0%	100.0%	100.0%	100.0%

NOTES: Asbestos cases not included. Year of filing is for initial case opening, irrespective of number of re-openings.

Table C.7
Filings by High, Medium, and Low Judicial Workload Category: RY1987–RY1995

Workload Category	Reporting Year									Total All Years
	87	88	89	90	91	92	93	94	95	
	Frequency									
High	39798	39032	38778	38016	39630	47485	55567	62468	70936	431710
Medium	75854	77148	74076	66725	68494	69757	68204	68057	68958	637273
Low	99959	99386	92024	85480	88987	99821	93872	92872	93818	846219
Total	215611	215566	204878	190221	197111	217063	217643	223397	233712	1915202
	Percentage									
High	18.5%	18.1%	18.9%	20.0%	20.1%	21.9%	25.5%	28.0%	30.4%	22.5%
Medium	35.2%	35.8%	36.2%	35.1%	34.7%	32.1%	31.3%	30.5%	29.5%	33.3%
Low	46.4%	46.1%	44.9%	44.9%	45.1%	46.0%	43.1%	41.6%	40.1%	44.2%
Total	100.0%	100.0%	100.0%	100.0%	100.0%	100.0%	100.0%	100.0%	100.0%	100.0%

NOTES: Asbestos cases not included. Year of filing is for initial case opening, irrespective of number of re-openings.

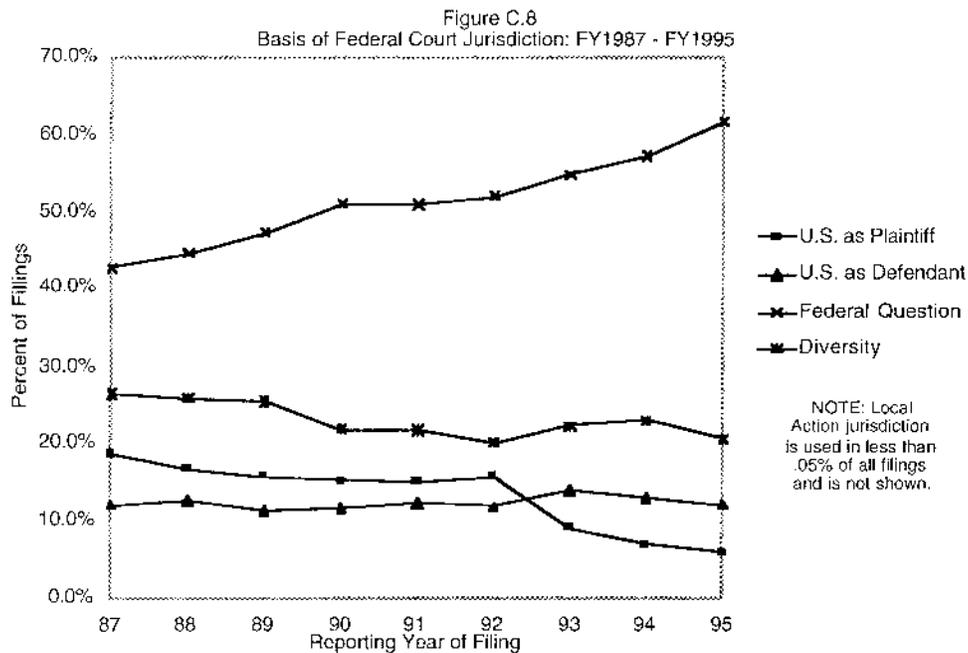
number dropped to 79 percent. During the same period, removals from state court went from 10 percent of all commenced cases to 17 percent.⁹

Basis of Jurisdiction

While the origin of a case tells us the source of the litigation, the basis of jurisdiction describes the legal authority for bringing the action in the federal courts. Over the years, about half of the cases commenced in district courts involve questions of federal law, about a quarter would be tried in state court except for the fact that either a plaintiff or a defendant in the action is the United States government, and the balance meet the tests for “diversity” jurisdiction (i.e., plaintiffs and defendants are from different states and the amount in controversy exceeds some preset floor).¹⁰ These proportions are based upon totals for the nine-year period; the actual year-to-year percentages can vary considerably. There has been a steady rise in the proportion constituted by federal question cases (43 percent to 62 percent) and a decline in the impact of U.S. plaintiff cases (19 percent to 6 percent). The impact of the Judicial Improvements and Access to Justice Act of 1988 can also be seen in Figure C.8. That act raised the amount in controversy required to bring a diversity action from

⁹If this trend was due to an increasing tendency to “kick the case upstairs” to avoid the burden on a crowded state court docket, one might expect to see a commensurate increase on the number of cases therein remanded back to state court because they were lacking the proper basis for federal jurisdiction. In fact, just the opposite seems to be true. In RY1987, 20,380 cases were commenced in federal court as state court removals, while 7,351 cases were disposed of by remand to the state, a ratio of 2.8 to 1 (we note, however, that these are not necessarily the same cases). In RY1995 there were 39,382 removals but only 10,161 remands, a ratio of 3.9 to 1.

¹⁰A small percentage of federal district court civil filings are “local actions,” e.g., litigation brought in courts seated in U.S. overseas territories.



\$10,000 to \$50,000, and, as one would expect, there was a 20 percent drop in the number of diversity cases initiated in RY1990 from the previous year.¹¹

Point of Disposition

Over half of the cases in federal district courts terminated either prior to answer by the defendant(s) or without any action (such as ruling on a motion) by a judicial officer. These are cases that never really had a chance to be affected by management techniques, discovery limits, early trial settings, and the like. About 4 percent of all terminations were disposed of during or after trial. A gradual shift has occurred since RY1987, with trials taking place in fewer cases (4.9 percent in RY1987 vs. 3.3 percent in RY1995) and the number of no-answer or no-activity cases decreasing as well (60.5 percent in RY1987 vs. 54.9 percent nationwide). Details are provided in Table C.8 and Figure C.9.

Just two disposition methods have shifted their importance over the years: default judgments and verdicts. From RY1987 to RY1991, default judgments constituted about 7.5 to 10 percent of all terminations, while in RY1995 that figure was 3.8 percent. There has also been a slight drop in the number of cases disposed of by verdict (jury, court, or directed) from 3.8 percent in RY1987 to 2.9 percent in RY1995. There were about 2100 fewer verdicts reached in RY1995. Pilot and comparison districts followed a similar pattern.

¹¹The increase in the diversity floor was effective for all cases commenced after May 18, 1989.

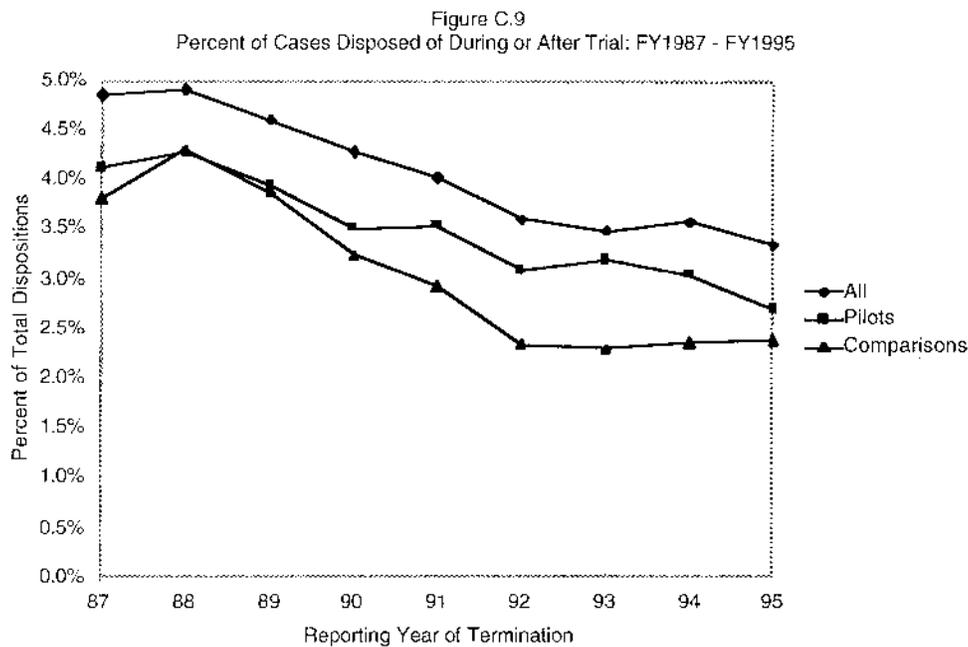
Table C.8
Civil Case Point of Disposition: RY1987-RY1995

Disposition Point	Reporting Year									Total All Years
	87	88	89	90	91	92	93	94	95	
ALL DISTRICTS										
Frequency										
Before Issue										
Joined or No										
Ct. Action	135653	131589	124991	116547	108280	123917	120333	117685	120861	1099856
Pre-Trial or										
Other	77692	79044	79063	73785	74656	83447	86536	88530	91895	734648
During or										
After Trial	10907	10904	9877	8529	7680	7767	7477	7652	7360	78153
Total	224252	221537	213931	198861	190616	215131	214346	213867	220116	1912657
Percentage										
Before Issue										
Joined or No										
Ct. Action	60.5%	59.4%	58.4%	58.6%	56.8%	57.6%	56.1%	55.0%	54.9%	57.5%
Pre-Trial or										
Other	34.6%	35.7%	37.0%	37.1%	39.2%	38.8%	40.4%	41.4%	41.7%	38.4%
During or										
After Trial	4.9%	4.9%	4.6%	4.3%	4.0%	3.6%	3.5%	3.6%	3.3%	4.1%
Total	100.0%	100.0%	100.0%	100.0%	100.0%	100.0%	100.0%	100.0%	100.0%	100.0%
CJRA STUDY PILOT DISTRICTS ONLY										
Frequency										
Before Issue										
Joined or No										
Ct. Action	22297	22478	22254	21017	17272	22064	22070	22171	23000	194623
Pre-Trial or										
Other	11654	10395	10242	10439	10873	12046	11780	11033	12100	100562
During or										
After Trial	1463	1477	1342	1146	1035	1091	1117	1044	973	10688
Total	35414	34350	33838	32602	29180	35201	34967	34248	36073	305873
Percentage										
Before Issue										
Joined or No										
Ct. Action	63.0%	65.4%	65.8%	64.5%	59.2%	62.7%	63.1%	64.7%	63.8%	63.6%
Pre-Trial or										
Other	32.9%	30.3%	30.3%	32.0%	37.3%	34.2%	33.7%	32.2%	33.5%	32.9%
During or										
After Trial	4.1%	4.3%	4.0%	3.5%	3.5%	3.1%	3.2%	3.0%	2.7%	3.5%
Total	100.0%	100.0%	100.0%	100.0%	100.0%	100.0%	100.0%	100.0%	100.0%	100.0%
CJRA STUDY COMPARISON DISTRICTS ONLY										
Frequency										
Before Issue										
Joined or No										
Ct. Action	27301	24646	21440	21510	21376	27067	24462	24213	24571	216986
Pre-Trial or										
Other	13747	14739	14062	11796	10577	11477	12473	13440	13948	116259
During or										
After Trial	1633	1773	1452	1113	963	924	869	908	941	10576
Total	42681	41158	37354	34419	32916	39468	37804	38561	39460	343821

Table C.8 (continued)

	Percentage									
Before Issue Joined or No Cr. Action	64.0%	59.9%	58.5%	62.5%	64.9%	68.6%	64.7%	62.8%	62.3%	63.1%
Pre-Trial or Other	32.2%	35.8%	37.6%	34.3%	32.1%	29.1%	33.0%	34.9%	35.3%	33.8%
During or After Trial	3.8%	4.3%	3.9%	3.2%	2.9%	2.3%	2.3%	2.4%	2.4%	3.1%
Total	100.0%	100.0%	100.0%	100.0%	100.0%	100.0%	100.0%	100.0%	100.0%	100.0%

NOTES: Asbestos cases not included. Year of termination is for last known disposition of case, irrespective of number of re-openings.



While not one of our primary cost, time to disposition, satisfaction, or fairness measures of concern in this CJRA evaluation, the method of disposition of cases is important, and the data collected in this CJRA evaluation could be used in future research on this topic.

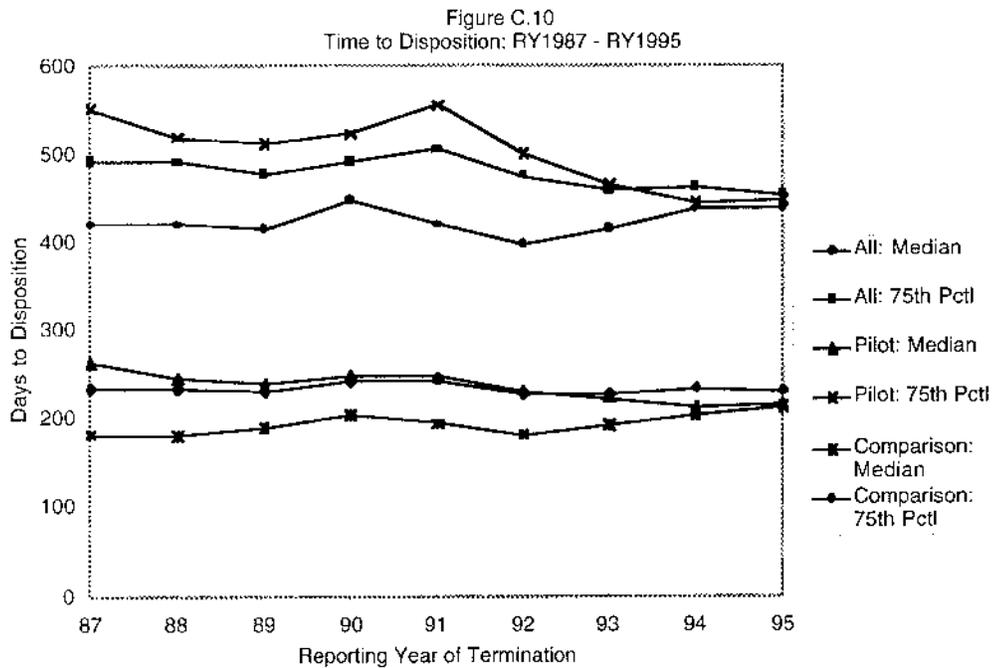
Time to Disposition

Federal district courts, in contrast to many state-level urban trial courts, are comparatively fast in terms of moving cases to disposition. The mean number of days from filing to disposition is about a year, and the median is about eight months for civil cases. Notwithstanding the fact that this is better than the time experienced in many state courts, it may well be that even a year is too long for relatively straightforward litigation, and of course a small percentage of cases take over three years to resolve.

Over the RY1987–RY1995 period nationally, about 42 percent of all final terminations were of cases that had been filed within the previous 6 months, and another 24 percent were filed 7 to 12 months earlier. These national figures of about two-thirds closed within a year have remained fairly stable. About 6 percent of all terminations nationally were over three years old,¹² although the percentage terminated over three years old has been drifting downward from 6.8 percent in RY1990 to 5.2 percent in RY1995, since the passage of the CJRA.

We see a similar pattern of stability for the medians over time in all districts combined. Indeed, the median time has not substantially changed since RY1987 when it was 235 days, to RY1995, when it was 233 days. Details of the distribution of time to disposition are shown in Figures C.10 and C.11 and in Tables C.9 and C.10.

Since we are ignoring any intermediate reopenings a case may experience during the litigation process, our measures of the period from initial filing to last reported termination should present a fairly accurate picture of the total calendar time it takes to finally conclude a case in district court. As would be expected, our calculations of elapsed time resulted in averages and percentiles that are a little higher than would be obtained if one counted every reopening of a case separately.



¹²Data presented here do not include asbestos cases. However, for readers who may be comparing our numbers to nationally published numbers including asbestos, we offer the following caveat. Especially in 1991, there was a massive transfer of asbestos cases to the Eastern District of Pennsylvania for MDL pre-trial processing. Most of these asbestos cases were older than three years, so the transfer (accompanied by a case "closure" in the district from which the transfer was being made) brought about an artificial surge in three-year-old closures if asbestos cases were included.

Figure C.11
Percent of Cases Terminated Within 12 Months and Over 36 Months: RY1987 - RY1995

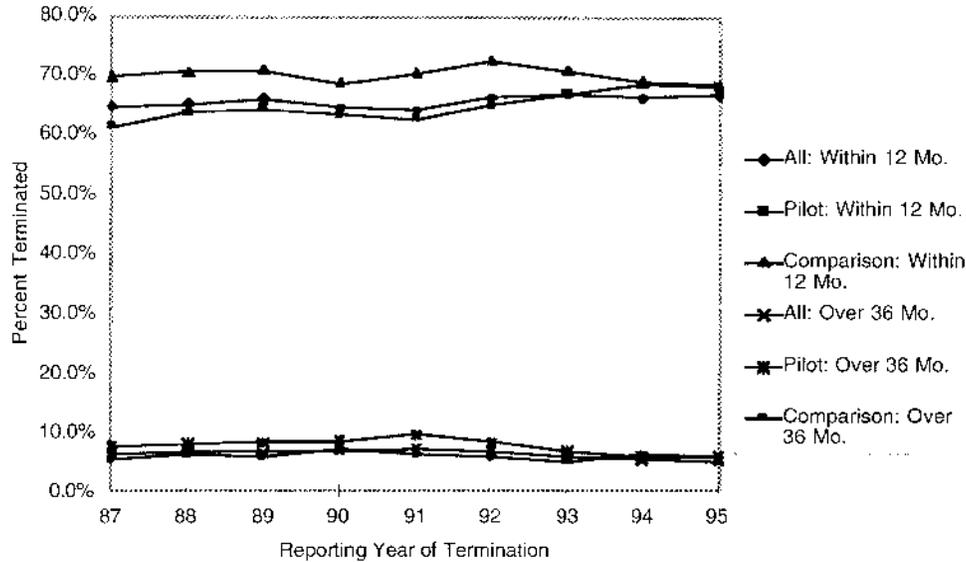


Table C.9
Mean and Percentiles for Time to Disposition: RY1987–RY1995

Termination Report ing Year	All Districts			Pilot Districts			Comparison Districts					
	Mean	Me-dian	75th Pctl	90th Pctl	Mean	Me-dian	75th Pctl	90th Pctl	Mean	Me-dian	75th Pctl	90th Pctl
87	376.2	235	493	891	409.1	263	553	993	333.2	183	421	794
88	378.3	234	492	909	397.9	246	520	976.5	338.1	182	421	829
89	371.5	231	478	879	400.1	241	513	988	339.0	190.5	417	805
90	385.2	243	493	903	411.4	250	523	1007	366.0	205	448	898
91	395.0	245	506	928	424.1	248	556.5	1068	350.7	195	422	857
92	378.1	229	476	892	400.3	233	502	990	329.2	183	399	797
93	359.9	230	460	823	370.2	224	468	878	332.9	194	416	773
94	361.7	236	464	821	348.9	215	444.5	814	355.1	206	439	825
95	353.2	233	456	804	352.0	217	448	839	349.2	215	441	808

NOTES: Asbestos cases not included. Elapsed time is from first filing to last termination; irrespective of number of re-openings.

Table C.10
Distribution of Months from Filing to Disposition: RY1987–RY1995

Time to Disposition	Reporting Year									Average All Years
	87	88	89	90	91	92	93	94	95	
All: 0–6 mos	42.2%	42.1%	42.2%	40.6%	40.3%	42.6%	42.5%	41.8%	42.0%	41.9%
All: 7–12 mos	22.8%	23.1%	23.8%	24.1%	23.9%	23.8%	24.5%	24.4%	24.8%	23.9%
All: 13–18 mos	13.1%	12.9%	13.0%	13.5%	13.3%	12.7%	13.5%	14.1%	14.1%	13.4%
All: 19–24 mos	7.6%	7.3%	7.3%	7.7%	7.5%	6.9%	7.1%	7.2%	7.2%	7.3%
All: 25–30 mos	4.7%	4.6%	4.3%	4.3%	4.7%	4.3%	4.1%	4.3%	4.1%	4.4%
All: 31–36 mos	3.2%	3.1%	2.8%	3.0%	3.2%	2.8%	2.6%	2.6%	2.6%	2.9%
All: over 36 mos	6.4%	6.8%	6.5%	6.8%	7.1%	6.8%	5.6%	5.5%	5.2%	6.3%
Pilot: 0–6 mos	38.4%	40.4%	40.9%	39.7%	40.3%	42.1%	43.1%	44.6%	44.1%	41.5%
Pilot: 7–12 mos	22.9%	23.1%	23.4%	23.6%	22.3%	23.1%	23.9%	23.8%	24.1%	23.4%
Pilot: 13–18 mos	13.4%	13.1%	12.6%	13.0%	11.9%	12.2%	12.7%	13.0%	12.7%	12.7%
Pilot: 19–24 mos	8.2%	7.3%	7.1%	7.6%	7.7%	6.8%	6.9%	6.6%	6.6%	7.2%
Pilot: 25–30 mos	5.4%	4.8%	4.6%	4.4%	4.6%	4.4%	4.1%	3.8%	4.0%	4.5%
Pilot: 31–36 mos	4.1%	3.2%	3.0%	3.1%	3.6%	3.0%	2.6%	2.4%	2.7%	3.1%
Pilot: > 36 mos	7.6%	8.0%	8.4%	8.6%	9.5%	8.3%	6.8%	5.8%	6.0%	7.6%
Comparison: 0–6 mos	50.0%	50.2%	48.6%	46.3%	48.0%	50.0%	48.1%	46.4%	45.0%	48.1%
Comparison: 7–12 mos	20.0%	20.6%	22.3%	22.4%	22.6%	22.6%	22.8%	22.6%	23.6%	22.1%
Comparison: 13–18 mos	12.4%	10.9%	11.4%	11.6%	10.9%	10.5%	11.9%	12.6%	13.0%	11.7%
Comparison: 19–24 mos	6.1%	6.1%	6.1%	6.3%	5.9%	5.5%	6.2%	6.3%	6.6%	6.1%
Comparison: 25–30 mos	3.8%	3.6%	3.5%	3.7%	3.7%	3.4%	3.5%	3.6%	3.8%	3.6%
Comparison: 31–36 mos	2.4%	2.6%	2.3%	2.7%	2.7%	2.3%	2.3%	2.3%	2.4%	2.4%
Comparison: over 36 mos	5.3%	6.0%	5.8%	7.0%	6.2%	5.7%	5.2%	6.1%	5.7%	5.9%

NOTES: Asbestos cases not included. Elapsed time is from first filing to last termination; irrespective of number of re-openings.

One thing that stands out is that nationwide, there really hasn't been much change in aggregate time to disposition between RY1987 and RY1995. Whether one looks at medians (235 days in RY1987 vs. 233 in RY1995), percentage of cases terminated within six months (42.2 vs. 42.0 percent) or one year (65.0 vs. 66.8 percent), or percentage of cases terminating over three years from filing (6.4 vs. 5.2 percent), there are no grounds for asserting that cases on the average are being processed much faster today than they were a decade ago.

Another major point is that there is some change in time to disposition for single districts and groups of districts. However, one must not jump to the conclusion that it was or was not caused by the CJRA solely based on looking at tables in this appendix.

Many factors affect time to disposition, and many of those factors have been changing over time, so a multivariate analysis is required rather than simple inspection of tables. Some of the factors that we know have changed over time are the mix of cases filed; the mix of different types of dispositions such as trials; the size of the pending caseload over three years old; the case management policies used in the districts; how early judges begin to manage cases; how much time judges allow for discovery; the economic environment in which lawyers compete and practice; the number of judicial officers in federal court; and other factors undoubtedly affect the environment within which the cases were processed.

Our multivariate analysis considered many of the factors cited above and shows no statistically significant difference in time to disposition between pilot and comparison districts in either our 1991 sample or our 1992–93 sample, after controlling for differences in case and district characteristics. Refer to Appendix D for details.

TRENDS IN CRIMINAL CASES IN FEDERAL DISTRICT COURTS

The judicial resources that can be devoted to disposing civil cases is influenced by fluctuations in the number and type of criminal prosecutions. Although the total number of criminal defendants are about 20 percent of the number of civil cases commenced each year, these cases have a disproportionate impact on the business of the federal trial courts.¹³ This occurs for two reasons: a defendant's constitutional and legislative right to a speedy trial and the relatively higher demands per case upon judicial officer time associated with criminal prosecutions. Priority in the allocation of judicial time must be given to trials of misdemeanors and felonies; to do so otherwise would risk setting a possibly guilty defendant free.¹⁴

It is thus important to see if there have been any changes in the number and type of the criminal caseload over the years of interest that might affect the implementation of the changes proposed by the Civil Justice Reform Act. Increases in criminal prosecutions, if unmatched by corresponding increases in judicial resources, could result in civil case processing being slowed down.

A distinction needs to be made here between criminal "offenses," "cases" and "defendants." Technically, a criminal case in the federal district courts (identified by the unique combination of the district, the filing office, the filing year, and docket number) can involve multiple defendants, each of whom may be charged with multiple criminal offenses. While we use the case as the primary unit of measurement when discussing civil litigation, it is more useful to look at criminal prosecutions as

¹³In FY1993, there were about 80 criminal defendant and 336 civil filings per judgeship nationwide. However, using the latest case weights developed by the HC (see the discussion of time study data in Appendix B), the average weighted number of criminal filings per judgeship was 133, while the average weighted number of civil filings per judgeship was 285. See Administrative Office of the United States Courts (1993), p. 18. It might be argued that criminal cases take even a larger share of the business of the district courts when the direct and indirect impact of the large number of prisoner civil rights and habeas corpus cases, classified as part of the civil caseload, are figured in.

¹⁴The Speedy Trial Act of 1974 and its amendments generally require that, absent certain circumstances, defendants be indicted within 20 days of arrest and brought to trial within 70 days.

involving prosecution against individual defendants, and each defendant/case combination has a unique identification number in the federal court system.

Accordingly, we present criminal district court data here always on a defendant-level basis, and when we speak of criminal *filings*, or *matters*, we are referring to each defendant separately. In only a few instances, primarily in regards to the selection process for our criminal sample in Appendix A, is the traditional meaning of case used. If a defendant has more than one charge against him or her, we use only the most severe charge (in terms of potential length of sentence) in our tables.

As with civil cases, we have collapsed any multiple filings seen in the IDB for each defendant/case into a single record.

The remainder of this appendix will present information on trends in criminal filings, the mix of offense types, and the mix of offense levels. In sum, while there has been some variation by type of offense, generally the aggregate impact of the criminal caseload upon district resources has stayed at relatively constant levels since the early 1990s. No obvious increase in criminal filings that require higher judicial time can be seen since RY1992 (though there certainly was growth in such cases from RY1987 to RY1992).

Criminal Case Filings

Given the concern among some observers in regard to an expanding criminal caseload and to a perceived trend toward the “federalization” of crimes that were heretofore prosecutable only at the state level, it is somewhat surprising to see that for all districts, as well as for pilots and comparison districts separately, there has not been a major increase in the number of criminal filings during the eight years from RY1987 to RY1995, as shown in Figure C.12 and Table C.11.

While there have been variations in filing levels from year to year, notably an upward swing of 10 to 15 percent during the early 1990s, there was an offsetting decline in 1993 to 1995. This fluctuation is understandable in light of the fact that unlike civil litigation, there is one major “plaintiff” in the criminal side of the federal district courts. The U.S. Attorney exercises some control over the number and type of criminal prosecutions. The number of filings is not only influenced by the current definition of criminal activity and the number of crimes reported, but also by the threshold for prosecution (e.g., in drug-related cases), the workload and staffing levels in the U.S. Attorney’s office, and the workload in the courts.

Unlike for civil litigation, we do not present detailed information on the number of criminal terminations, the number of criminal cases pending for each year, the methods of case termination, and the average time to disposition. The demands of the Speedy Trial Act provide an incentive for prosecutors and judges to proceed with all due haste in moving the defendant’s case along. In fact, very few defendants each year have their cases dismissed because of the failure of the U.S. Attorney to meet the time constraints of the Speedy Trial Act. About 9 out of 10 criminal defendant cases are disposed of within 12 months, and the lion’s share of times that exceed the Speedy Trial Act’s time intervals are usually at the request of the defendant. The

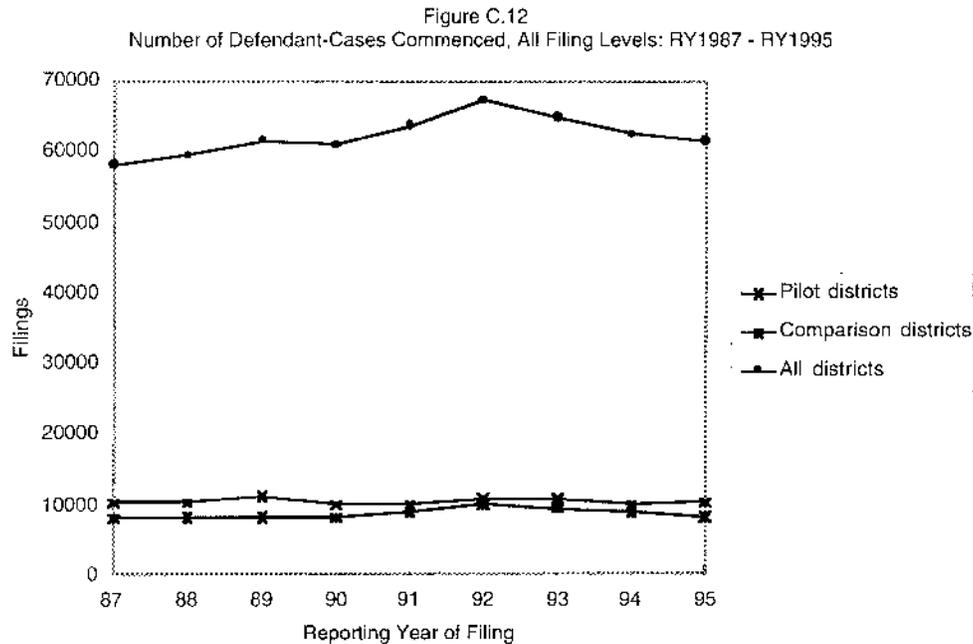


Table C.11
Number of Defendant/Cases Commenced, All Filing Levels: RY1987-RY1995

District Type	Reporting Year									Total All Years
	87	88	89	90	91	92	93	94	95	
Other districts	39802	41082	42484	43155	44957	46289	44502	43616	42884	388771
Pilot districts	10274	10237	11076	9943	9891	10923	10676	9939	10393	93352
Comparison districts	8120	8280	8226	8016	8863	10108	9562	8953	8283	78411
All districts	58196	59599	61786	61114	63711	67320	64740	62508	61560	560534

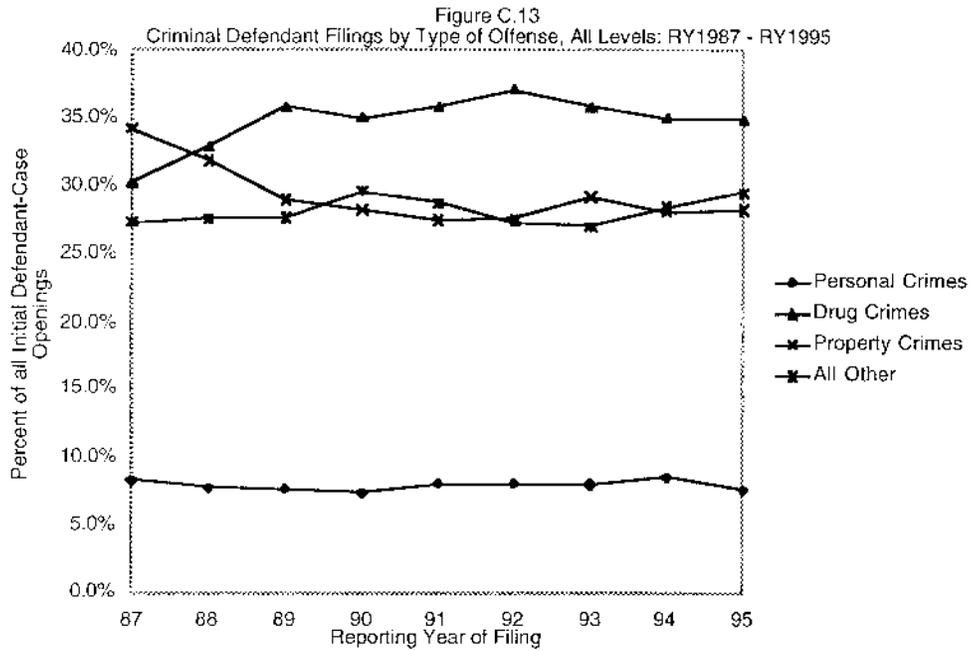
NOTE: Year of filing is for first reported filing, irrespective of number of re-openings.

median time to disposition fluctuates from year to year but is usually between four to six months. The primary concern, as it impacts the civil caseload, is how many defendant/cases are filed and how much judicial officer time is expended in handling them.¹⁵

Types of Offenses

As might be expected from a national focus on drugs and drug-related crimes, there has been an increase in the proportion of prosecutions dealing with such matters since RY1987, as shown in Figure C.13 and Table C.12. At the pilot and comparison

¹⁵See discussion of time study data in Appendix H for information on judicial time expended per criminal defendant/case.



district level, a similar rise is seen. However, all districts taken together have had a small decline in the number of drug case prosecutions since the early 1990s.

Felony and Misdemeanor Filings

Not all filings, even within a single case type, have the same potential severity in sentences. However, the resources of the district court are focused more toward dealing with defendants charged with felonies. Felony cases have had some increase since RY1987 but generally constitute about three-quarters of the total caseload, as shown in Figure C.14 and Table C.13.

Table C.12
Criminal Defendant Filings by Type of Offense, All Levels: FY1987–FY1995

Case Type	Reporting Year									Total All Years
	87	88	89	90	91	92	93	94	95	
ALL DISTRICTS										
Frequency										
Personal Crimes	4818	4599	4677	4452	5118	5395	5160	5299	4614	44132
Drug Crimes	17598	19600	22144	21339	22796	24962	23197	21878	21458	194972
Property Crimes	19897	18997	17876	17254	17462	18589	18852	17540	17324	163791
All Other	15883	16402	17089	18067	18333	18373	17527	17777	18133	157584
Total	58196	59598	61786	61112	63709	67319	64736	62494	61529	560479
Percentage										
Personal Crimes	8.3%	7.7%	7.6%	7.3%	8.0%	8.0%	8.0%	8.5%	7.5%	7.9%
Drug Crimes	30.2%	32.9%	35.8%	34.9%	35.8%	37.1%	35.8%	35.0%	34.9%	34.8%
Property Crimes	34.2%	31.9%	28.9%	28.2%	27.4%	27.6%	29.1%	28.1%	28.2%	29.2%
All Other	27.3%	27.5%	27.7%	29.6%	28.8%	27.3%	27.1%	28.4%	29.5%	28.1%
Total	100.0%	100.0%	100.0%	100.0%	100.0%	100.0%	100.0%	100.0%	100.0%	100.0%
PILOT DISTRICTS ONLY										
Frequency										
Personal Crimes	700	678	676	667	754	765	888	806	773	6707
Drug Crimes	4315	4550	5499	4303	4556	5320	4467	4219	3772	41001
Property Crimes	2922	2844	2772	2503	2419	2611	2977	2666	2721	24435
All Other	2337	2165	2129	2470	2162	2227	2344	2247	3125	21206
Total	10274	10237	11076	9943	9891	10923	10676	9938	10391	93349
Percentage										
Personal Crimes	6.8%	6.6%	6.1%	6.7%	7.6%	7.0%	8.3%	8.1%	7.4%	7.2%
Drug Crimes	42.0%	44.4%	49.6%	43.3%	46.1%	48.7%	41.8%	42.5%	36.3%	43.9%
Property Crimes	28.4%	27.8%	25.0%	25.2%	24.5%	23.9%	27.9%	26.8%	26.2%	26.2%
All Other	22.7%	21.1%	19.2%	24.8%	21.9%	20.4%	22.0%	22.6%	30.1%	22.7%
Total	100.0%	100.0%	100.0%	100.0%	100.0%	100.0%	100.0%	100.0%	100.0%	100.0%
COMPARISON DISTRICTS ONLY										
Frequency										
Personal Crimes	960	840	818	828	912	982	1064	1079	781	8264
Drug Crimes	2662	3054	3248	3069	3490	4074	3217	3020	2673	28507
Property Crimes	3145	3162	2773	2707	2831	3214	3442	3150	2938	27362
All Other	1353	1224	1387	1411	1630	1838	1839	1703	1890	14275
Total	8120	8280	8226	8015	8863	10108	9562	8952	8282	78408
Percentage										
Personal Crimes	11.8%	10.1%	9.9%	10.3%	10.3%	9.7%	11.1%	12.1%	9.4%	10.5%
Drug Crimes	32.8%	36.9%	39.5%	38.3%	39.4%	40.3%	33.6%	33.7%	32.3%	36.4%
Property Crimes	38.7%	38.2%	33.7%	33.8%	31.9%	31.8%	36.0%	35.2%	35.5%	34.9%
All Other	16.7%	14.8%	16.9%	17.6%	18.4%	18.2%	19.2%	19.0%	22.8%	18.2%
Total	100.0%	100.0%	100.0%	100.0%	100.0%	100.0%	100.0%	100.0%	100.0%	100.0%

NOTE: Year of filing is for initial case opening, irrespective of number of re-openings.

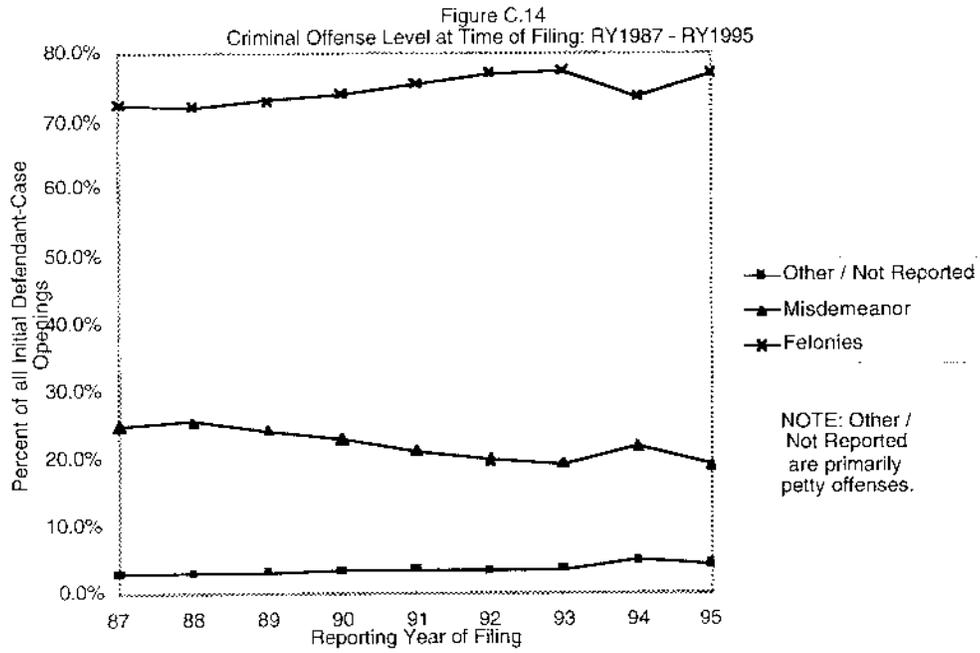


Table C.13
Criminal Offense Level at Time of Filing: RY1987-RY1995

Level	Reporting Year									Total All Years
	87	88	89	90	91	92	93	94	95	
ALL DISTRICTS										
Frequency										
Other / Not Reported	1571	1617	1895	1930	2206	2211	2237	2905	2568	19140
Misdemeanor	14442	15080	14865	13952	13433	13251	12341	13582	11589	122535
Felonies	42183	42902	45026	45232	48072	51858	50162	46021	47403	418859
Total	58196	59599	61786	61114	63711	67320	64740	62508	61560	560534
Percentage										
Other / Not Reported	2.7%	2.7%	3.1%	3.2%	3.5%	3.3%	3.5%	4.6%	4.2%	3.4%
Misdemeanor	24.8%	25.3%	24.1%	22.8%	21.1%	19.7%	19.1%	21.7%	18.8%	21.9%
Felonies	72.5%	72.0%	72.9%	74.0%	75.5%	77.0%	77.5%	73.6%	77.0%	74.7%
Total	100.0%	100.0%	100.0%	100.0%	100.0%	100.0%	100.0%	100.0%	100.0%	100.0%
PILOT DISTRICTS ONLY										
Frequency										
Other / Not Reported	472	436	543	484	478	560	540	496	387	4396
Misdemeanor	1606	1527	1724	1274	980	1040	1494	1824	1584	13053
Felonies	8196	8274	8809	8185	8433	9323	8642	7619	8422	75903
Total	10274	10237	11076	9943	9891	10923	10676	9939	10393	93352
Percentage										
Other / Not Reported	4.6%	4.3%	4.9%	4.9%	4.8%	5.1%	5.1%	5.0%	3.7%	4.7%
Misdemeanor	15.6%	14.9%	15.6%	12.8%	9.9%	9.5%	14.0%	18.4%	15.2%	14.0%
Felonies	79.8%	80.8%	79.5%	82.3%	85.3%	85.4%	80.9%	76.7%	81.0%	81.3%
Total	100.0%	100.0%	100.0%	100.0%	100.0%	100.0%	100.0%	100.0%	100.0%	100.0%
COMPARISON DISTRICTS ONLY										
Frequency										
Other / Not Reported	197	204	270	327	368	381	317	380	400	2844
Misdemeanor	1215	1500	1318	990	1095	1340	1153	1145	1075	10831
Felonies	6708	8576	6638	6699	7400	8387	8092	7428	6808	64736
Total	8120	8280	8226	8016	8863	10108	9562	8953	8283	78411
Percentage										
Other / Not Reported	2.4%	2.5%	3.3%	4.1%	4.2%	3.8%	3.3%	4.2%	4.8%	3.6%
Misdemeanor	15.0%	18.1%	16.0%	12.4%	12.4%	13.3%	12.1%	12.8%	13.0%	13.8%
Felonies	82.6%	79.4%	80.7%	83.6%	83.5%	83.0%	84.6%	83.0%	82.2%	82.6%
Total	100.0%	100.0%	100.0%	100.0%	100.0%	100.0%	100.0%	100.0%	100.0%	100.0%

NOTES: "Other/Not Reported" are primarily petty offenses. Year of filing is for initial case opening, irrespective of number of re-openings.

ANALYSIS OF TIME TO DISPOSITION

INTRODUCTION

Our evaluation of court policies and procedures focuses on estimating their effects on four primary outcomes: (1) time to disposition; (2) lawyer work hours; (3) attorney satisfaction with the case management; and (4) attorney perceptions of fairness of the case management. In this and the following three appendices we provide the details of our analyses of the effects of case management policies and procedures on the four primary outcomes. In this appendix we discuss our models for analyzing time to disposition (TTD).

TTD is defined as the number of days between the first filing of the case and the final disposition of the case in federal district court. If a case reopens in the same district after disposition, the TTD is the time from the first opening to the last closing. TTD is known for all closed cases and is measured uniformly across all districts.

Control and Policy Variables

In modeling the effects of case management policies and procedures on TTD, we considered two types of predictor variables: control variables and policy variables.

Control variables consist of case and district characteristics that could explain differences in case length. For example, we considered case complexity and stakes as controls in our models. District characteristics thought to possibly affect TTD included the number of civil and criminal filings per judicial officer in the district. A more complete listing of control variables that we considered is discussed later in this appendix.

Policy variables refer to those variables that measure the district case management policies and the particular case level management procedures applied to each case. For example, we included variables for whether or not a case received early management (before the 180th day after filing) and whether or not the district enacted a mandatory early disclosure policy. The policy variables also reflect the principles and techniques written into the CJRA and are included in the models so that we can measure the direct effect of the policies espoused by CJRA on case outcomes such as TTD.

Modeling Effects on General Civil Cases with Issue Joined

As discussed in Chapter Two, after reviewing the case management policies and procedures set forth in CJRA, we concluded that the effects of those policies and procedures could be best estimated using data from general civil litigation cases with issue joined.¹

Issue is considered joined after the defendants have answered the complaint in accordance with F.R.Civ.P. Rule 12(a) or as mandated otherwise by the court.² Cases that are not joined usually do not receive judicial case management, and policies and procedures of concern in the CJRA are not relevant to them.

Table D.1 gives the total sample size for each district, the number of general civil cases in our sample, and the number of issue joined cases in our sample of general civil cases that we used for our TTD analyses. Cases that were transferred between districts, consolidated with other cases, or had missing court dockets are excluded. Data are shown for both our 1991 and our 1992–93 samples.

Multivariate Regression Models on Case-Level Data

To estimate the effects of case management policies and procedures, we used multivariate regression models fit to case level data where TTD was our outcome and the policy and control variables were our predictors. Multivariate regression allows us to estimate the unique effects of each policy variable while controlling for the effect of the other policy and control variables. The model can be written as

$$y_{ij} = b_0 + b_1x_{1ij} + b_2x_{2ij} + \dots + b_px_{pij} + e_{ij}$$

where y_{ij} denotes the TTD for the j th case ($j=1, \dots, n_j$) from district i and the x_{kij} denote the policy and control predictor variables. The parameter coefficients of the model (the b 's) denote the effect of policy and are estimated from our data using regression techniques. The number of cases varied from district to district with a mean of about 122 general civil cases with issue joined per district in our 1992–93 sample.

¹In practice, federal district courts split the civil caseload into two categories—those types of cases that usually receive minimal or no management, and those general civil litigation cases to which the district's standard case management policies and procedures apply. Minimal management categories of cases are not subject to the scheduling order requirements of Rule 16 of the Federal Rules of Civil Procedure, for example. The definition of minimal management cases varies from district to district based on local rules and local practice, but minimal management is usually applied to prisoner cases, administrative reviews of Social Security cases, bankruptcy appeals, foreclosure, forfeiture and penalty, and debt recovery cases. For consistency among districts in our analysis, we use a uniform definition of minimal management categories of cases that includes the six categories (see Appendix A for additional details).

Our primary analyses do not include minimal management types of cases because in practice almost none of these cases are managed using the policies and procedures that apply to general civil litigation, and hence they could not inform our evaluation of policies and procedures of most concern in the CJRA.

²Administrative Office of the United States Courts (1995), Chapter 5, p. 15. We obtained the issue joined data primarily using the Federal Courts Integrated Data Base (IDB) which is described in Appendix C. For each case, the IDB includes the district's determination of whether issue was joined. We found a small number of anomalies in the IDB issue joined variable, and we used the docket for the case to correct these errors and provide verification for the majority of the cases.

Table D.1
Sample of General Civil Cases and Issue Joined Cases Used in TTD Analyses,
by District and Sample Year

District	1991 Sample Size			1992-93 Sample Size		
	Total Sample	General Civil	Issue Joined	Total Sample	General Civil	Issue Joined
AZ	268	193	129	252	179	117
CA(C)	253	189	106	284	209	102
CA(S)	251	178	101	250	172	79
DE	257	184	131	251	180	116
FL(N)	250	181	115	267	200	133
GA(N)	250	192	151	277	192	148
IL(N)	259	189	91	250	192	104
IN(N)	250	185	127	253	176	127
KY(F)	252	182	136	249	173	143
KY(W)	253	180	143	251	179	136
MD	265	208	130	266	204	107
NY(E)	275	211	111	258	182	108
NY(S)	275	201	107	320	236	114
OK(W)	253	184	123	252	178	139
PA(E)	262	200	130	267	203	129
PA(M)	250	171	111	250	182	118
TN(W)	252	182	107	252	182	124
TX(S)	274	206	149	272	182	141
UT	250	174	115	251	189	128
WI(L)	250	173	109	250	191	119
Total	5,149	3,763	2,422	5,222	3,781	2,432

NOTE: Cases that were transferred between districts, consolidated with other cases, or had missing court dockers are excluded.

By using case level data we could explore the complex combination of policies and procedures used in managing cases, while at the same time controlling for innate case and district characteristics that could affect TTD. Using only district level analyses would have limited us to exploring only very few policies while controlling for very few differences in district case mix. District level models also ignore the extensive intra-district variability in case management practices.

We analyzed the data from our two sample years separately. Our 1991 sample was a cohort of case closures and our 1992-93 sample was a cohort of case filings. Because there were considerable changes in the federal courts during the period from 1989 to 1993, as shown in Appendix C, a cohort of closures will not necessarily match a cohort of filings. Hence it might be inappropriate to mix our two samples. Instead, we fit models to our 1992-93 sample and looked for confirmation from our 1991 sample data. In general, the 1991 data reaffirm our findings from the 1992-93 data and yield support for our conclusions.

Open Cases

Although we followed our 1992-93 sample of cases for over three years, there remained a small percentage of open cases (8.5 percent) in our sample of general civil litigation cases with issue joined. There were only 12 open cases in the 1991 sample. We included the open cases in our TTD analyses; however, the presence of open

cases required that we use methods other than Ordinary Least Squares (OLS) regression. In particular, we used a censored regression approach that estimated the parameters in model (1) using an iterative approach. At the first iteration, the parameters were estimated using only the closed cases. Using these estimates, we predicted the TTD for open cases. Now, using these predictions and the data from the closed cases, we reestimated the model parameters. The new estimates provided new predictions, and the procedure was repeated until convergence. The predictions were based on assuming that the residual errors in our model (the e_{ij} 's) were Gaussian or normal distributed—this assumption was well justified for the scale of data we fit (see below).³

TIME TO DISPOSITION

Time to disposition is a highly skewed variable. About 50 percent of cases close within 9 months of filing, but some cases survive for many years. Modeling skewed data can be problematic. Typically, predictors of interest do not affect skewed data in a linear and additive manner. Furthermore, the residual variance of skewed data depends on the estimated mean, and so traditional confidence intervals and significance tests are inappropriate. Finally, skewed data would not match the assumptions used in our censored regression models.

To avoid the bias and other problems associated with skewed data, we used a transformation of TTD. We explored various possible transformations in our preliminary analyses and determined that the fourth root transformation proved best—i.e., had the most symmetric, normal residual errors. Other more common transformations (e.g., the natural log or the square root) either over-transformed or under-transformed the data, leaving residual skew. Hence all our models for TTD used the fourth root of TTD as the outcome variable.

The fourth root transformation yielded no obvious lack-of-fit in our regression models, and the residuals showed no systematic structure. Also, plots of the predicted y 's versus the true TTD (fourth root) show no systematic structure.⁴ Sensitivity analyses that used alternative transformation or nonparametric survival analysis approaches (product-limit or Cox proportional hazards regression modeling) demonstrated that our findings were robust with respect to the choice of using linear models on the fourth root scale.

Our sample of cases contained a small percentage of open cases that were used in fitting our models, and this was accounted for by using censored regression methods. Although over 90 percent of our cases were closed at the time we completed gathering our data, we know that a small percentage will reopen at a later date. If our policies have differential effects on such cases that may reopen in the future, these cases could create bias in our estimated effects. To avoid such biases, we incorporated into our model an allowance for future reopened cases. We then explored the

³See Miller (1981), for details on the censored regression model.

⁴See Cook and Weisberg (1994), for a discussion of such plots.

effects of reopened cases and found that our results were insensitive to any percentage of reopened cases we could reasonably expect. Given this result, we fit all models assuming negligible future reopening of cases.

CONTROL VARIABLES

Many characteristics of cases, other than case management practices and policies applied to them, affect the TTD. High stakes cases involving many parties or cases involving complex legal or factual issues will often take considerable time to resolve, while straightforward cases involving small stakes are often closed in much less time. We need to control for such case characteristics when we estimate the effects of policy. Otherwise case differences that are not controlled for might be partially reflected in policy variable coefficients. This might lead us to make inaccurate conclusions about the effect of policy. This might occur if the distribution of a control variable differs between those cases that receive a particular management practice and those that do not. For example, suppose we found a greater percentage of complex cases among those cases receiving early management than we did among other cases in our sample. If this were to happen and complex cases required longer TTD to resolve, then we might underestimate the effects of early management if we left case complexity out of the model. The solution is to put complexity and as many other predictive control variables as possible into the model.

Our multivariate analysis accounted for these control variables by including them in our model so that our regression coefficients represent the effects of policy after controlling for the case and district characteristics represented by the control variables. However, we had many control variables to consider and needed to select the best subset of these variables so as to ensure that we controlled for confounding factors without saturating our model with uninformative predictors. Table D.2 lists the complete set of control variables we explored for our models of TTD. We also considered many other variables related to the variables shown in the table and selected the ones that appeared substantively best for exploration in our models.

In developing our final models for TTD, we followed the following procedures for selecting control variables. Starting with the list of predictors in Table D.2, we used exploratory data analysis to identify the best scale for each predictor variable. For example, we chose to use the natural log of stakes rather than the raw variable. Using these (transformed or raw) predictor variables, we then tested the predictive power of each variable alone for predicting the fourth root of TTD. (Because we used the fourth root of TTD in all models, in the remainder of this appendix we will use TTD to imply the fourth root of observed time-to-disposition.) We then used backward selection to choose the subset of predictor variables that were truly important in predicting TTD.

We considered both case-level and district-level control variables. We explored case-level predictors before looking for district-level predictors. Our goal was to limit the spurious inclusion of district-level variables because of the small district degrees of freedom (we had data from only 20 districts in the sample).

Table D.2
Control Variables Used for Modeling Time to Disposition

Variable	Description
	Case-Level Variables
Class action certification	Class action information from the docket; 0 is no mention of class action, 1 is alleged, 2 is denied, 3 is certified class action.
Nature of suit category	Nature of suit in broad categories; 1 is tort, 2 is contract, 3 is prisoner, 4 is other.
Average judicial work time category (shown as "case type has xxxxx average judicial work" in statistical model tables)	Average judicial work time category for case type; 1 is high, 2 is moderate, 3 is low (See App. A).
Jurisdiction	Jurisdiction in federal court; 1 is US plaintiff, 2 is US defendant, 3 is federal question, 4 is diversity.
Bankruptcy mention	Bankruptcy mentioned in docket for other than bankruptcy nature of suit cases; 1 if there is a mention, 0 otherwise.
Removed case from state court	Case was removed from state court; 1 if removed, 0 otherwise.
Any government parties	Any government (local, state or federal) litigant in the case; 1 is government parties, 0 otherwise. Available only in 1992-93 data.
Any private organizations	Any private organization litigant in the case; 1 is at least one litigant is a private organization, 0 otherwise.
Any pro se litigants	Any pro se litigant on the case; 1 is at least one pro se litigant, 0 otherwise.
Any litigant without an attorney	Any litigant without attorney listed in the docket; 1 is at least one litigant without an attorney (not pro se), 0 otherwise.
Any dispositive motion	Any dispositive motion filed on the docket; 1 if there is a motion, 0 otherwise.
Discovery motion	Any discovery motion filed on the docket; 1 if there is a motion, 0 otherwise.
Any motions	Any motions filed on the docket (discovery, dispositive, other, except for those related to appearance of attorney); 1 if there is at least one motion, 0 otherwise.
Five or more motions	Presence of five or more motions filed on the docket, except appearance of attorney; 1 if there is at least five motions, 0 otherwise.
Number of lawyers	Number of attorneys involved on the case, counting no more than one per litigant; square root was used in model.
Number of litigants	Number of litigants on the case; square root was used in the model.
Maximum stakes (if zero, shown "zero stakes" in statistical model tables)	Natural log of the maximum stakes (best likely or worst likely outcome) reported by any attorney or litigant on the case.
Nonmonetary stakes	Case involved nonmonetary stakes; 1 if at least one attorney or litigant said case involved nonmonetary stakes; 0 otherwise.

Table D.2 (continued)

Variable	Description
Related case	Attorney or litigant responded that additional time or money was spent on the case because of its effects on other cases; 1 if at least one attorney or litigant said yes, 0 otherwise.
State case	Also was state case concerning the same dispute; 1 if at least one attorney or litigant responded that there was a state case, 0 otherwise
Administrative proceeding	Also was a federal or state administrative proceeding prior to filing the case; 1 if at least one attorney or litigant said yes, 0 otherwise.
Any contingent fee attorney	Any attorney working for a contingent fee; 1 if at least one attorney or litigant said yes, 0 otherwise.
Any hourly fee attorney	Any attorney working for an hourly fee; 1 if yes, 0 otherwise.
case complexity	Highest level of case complexity as reported by any attorney or judge; 1 is high complex, 2 is medium complex, 3 is low complex. Only attorney reports were available in 1991.
Dispute began after filing date	For at least one party the attorney responded that the date the dispute began was after the filing date; 1 if yes, 0 otherwise.
Old dispute	For at least one party the attorney responded that the dispute began more than a year before the filing date; 1 if yes, 0 otherwise.
District-Level Variables	
Judges	Number of full-time equivalent judges, including senior judges, in the district.
Judicial officers	Number of full-time equivalent judicial officers, including judges, senior judges, and magistrate judges in the district.
Civil filings	Annual Civil filings per FTE judicial officer.
Criminal filings	Annual Criminal filings per FTE judicial officer.
Total filings	Annual Civil plus Criminal filings per FTE judicial officer.
Offices	Number of geographically different offices in the district.
Days to answer	Median days to answer for all cases in the district.
Percent dispositive motions	Percent of cases in the district with a dispositive motion filed on the docket.
Percent discovery motions	Percent of cases in the district with a discovery motion filed on the docket.
Percent any motions	Percent of cases in the district with at least one motion on the docket, other than attorney appearance-related motions.
Percent five or more motions	Percent of cases in the district with five or more motions on the docket.
Number of motions	Median number of motions per case for all cases in the district.

As shown in Table D.2, our control variables included subjective case-level predictors, such as stakes and complexity as determined by the attorneys or judges. The controls also included objective measures such as the jurisdiction, nature of suit, and presence of a bankruptcy mentioned in the docket. We included variables such as number of litigants, number of attorneys, and the presence of motions as additional measures of case complexity. We used a careful docket analysis to identify each of these measures. Our docket analysis also provided us with class action status. We included the four case-level motion variables (Any Dispositive Motions, Any Discovery Motions, Any Motions, and Five or More Motions) as control variables, even though these variables might be influenced to some degree by management policies. We reviewed these motion variables extensively and determined that motions were largely independent of case management. However, to the extent that motions may be influenced by policy, the inclusion of these controls could introduce some bias. On the other hand, motions were one of our better predictors of case complexity and were important for improving the precision of our estimated effects, and any small bias introduced may be offset by this increase in precision.

Attorney responses (augmented by litigant and judge responses) supplied us with case complexity, stakes (value of monetary, presence of nonmonetary), presence of related cases, fee arrangements, timing of litigant involvement (before filing, after filing), presence of related state cases, and the presence of administrative proceedings. We derived the remaining variables from the Federal Courts Integrated Database (IDB).

Several of the candidate control variables that we explored were highly correlated. These included the various measures related to motions (any motions in case, dispositive, discovery, or five or more motions) and the number of litigants and attorneys. To prevent problems of multi-collinearity we selected the best predictor of TTD from each group and included these in our models. The four motions variables naturally split into two subgroups (many motions and discovery motions were in one group) and one predictor from each group was included.

Because we used attorney survey responses to generate some of our control variables and because we had less than perfect attorney response, we were missing control variables for some cases. In the 1992–93 data, we imputed values from missing controls using the remaining predictors in our model. We used imputed values because the lower response rate on open cases created a possibility of bias from including missing data flags. In our models for the 1991 data, we used missing data flags for cases that were missing predictors. Including records with missing data could bias our results. However, excluding these records leads to greater variability in our estimates. We chose to include these records to increase the precision of our estimates. Sensitivity analyses run using only cases without missing data confirmed that our results are robust to this choice and that any bias is small.

POLICY AND PROCEDURE VARIABLES

Table D.3 lists the policy variables we explored in our models for TTD.

Table D.3
Policy and Procedure Variables Used in Modeling Time to Disposition

Variable	Description
Case-Level Variables	
Early management on case	Judicial case management (set schedule, hold conference, refer to ADR, require status report or joint plan) before the 180th day after filing, as reported in docket; 1 if received early management, 0 otherwise.
Early schedule on case	Provided a discovery, trial or other schedule before the 180th day after filing, as reported in the docket; 1 if schedule, 0 otherwise.
Early setting of trial schedule on case	Provide a trial schedule before the 180th day after filing as reported in the docket; 1 if schedule, 0 otherwise.
Early conference on case	Held a conference (status, scheduling, case management, or Rule 16) before the 180th day after filing, as reported in the docket; 1 if held, 0 otherwise.
Early status report or joint plan on case	Attorneys filed a status report or joint plan before the 180th day after filing, as reported in the docket; 1 if reported filed, 0 otherwise.
Early ADR referral on case	Case referred to arbitration, mediation, or neutral evaluation before the 180th day after filing, as reported in the docket; 1 if referred, 0 otherwise.
Management level	Intensity of case management as reported by the attorney; 1 if high, 0 otherwise (for case level analyses this is the highest level reported by any attorney).
Early disclosure of information (shown as "disclosure on case" in statistical model tables)	Parties made an early disclosure of relevant information without formal discovery request as reported by attorney; 1 if yes for at least one attorney, 0 otherwise.
Good faith effort to resolve discovery dispute before filing motion	Lawyer report of whether good faith effort was made to resolve dispute before filing discovery motion; 1 if yes for at least one attorney, 0 otherwise.
District-Level Variables	
Cases managed (district percent)	Percent of cases in a district that receive a schedule, a conference, file a status report or plan, or are referred to ADR.
Cases with trial schedule set early (district percent)	Percent of cases in a district that receive a trial schedule before the 180th day after filing.
Early disclosure district policy	District policy on early disclosure. For 1991, 1 if district has mandatory early disclosure without formal discovery request for any cases, 0 otherwise. For 1992–93, 1 if district has mandatory disclosure for general civil litigation, 0 otherwise.
Joint plan district policy	District policy on whether attorneys must prepare a joint discovery or case management plan early in the case; 1 if yes; 0 otherwise.
Litigants at settlement conference (district percent)	Percent of cases in a district with litigants present at settlement conferences in person or available on the telephone (as reported by the attorney).
Limits on interrogatories (district)	District policy on limitations on interrogatories; 1 if the district has a policy limiting them, 0 otherwise.
Days to discovery cutoff (district median)	Median days to discovery cutoff for cases with cutoff in a district.
Continuances (district percent)	Percentage of cases in a district that have a continuance, of those cases that have a schedule set.
Magistrate judge activity (district mean)	Number of civil proceedings performed by magistrate judges per civil termination in the district (e.g., motions, conferences, hearings); the square root of the number was used in the model.
Judicial Control Over Trial (District percent firm)	Percentage of trials in a district that had firm active judicial control, of those cases for which lawyers reported judicial control of trial was either firm or minimal.

We selected these policy variables because they explicitly represent the principles and techniques that were mentioned in CJRA (for example, a district policy of mandatory disclosure), or serve as measures of the policies and procedures mentioned in the Act (for example the particular forms of early judicial case management that we explored). Each variable is well measured using our various sources of data. Also each variable pertains to a particular, well formed hypothesis about the effect of policies and procedures on TTD. We did not pick these variables after exploratory analyses. Rather we chose each policy variable because it pertained to a particular policy or procedure of interest that had been hypothesized as a predictor of TTD (and other outcomes).

We were limited in our choice of predictors to those variables that could be identified via the docket analysis, via explicitly known district policies, or via information on variables that could be obtained from the attorney surveys (and judge surveys in 1992–93).

We were further limited because we had only 20 districts and some procedures were not widely implemented across the districts. For example, six of the 10 pilot districts planned to replace the judicial discretion model with a track model of differential case management, but the track model proved difficult to implement. Most districts that included tracking in their plan actually assigned the traditional group of minimal management case types to an expedited track. Five of the six pilot districts whose plans contained a track model assigned 2 percent or less of their cases to the complex track. Pennsylvania (E), which assigned 7 percent of its general civil cases to the complex track, was the sole exception.⁵ The consequence was that most general civil cases to which CJRA procedural principles might be relevant were placed in the standard track if any track assignment was made. This meant that there was little actual “differential” tracking of general civil cases in most districts that adopted a track model in their CJRA plan. Consequently, we have no basis for evaluating how the track method of DCM might have affected time, cost, satisfaction, and views of fairness. If we had used only PA(E) to estimate the effects of tracking, then our estimated effect of tracking would be completely confounded with the effect of the single district’s characteristics and the effects of the several other policies also used in that same district, and we would not have been able to generalize our DCM finding to other districts. We could not evaluate the “judicial discretion” model of DCM because the pre-CJRA existence and nearly universal use of this model negates the possibility of evaluation.

ENDOGENEITY OF TIME TO DISPOSITION AND POLICY VARIABLES

Our evaluation of case-level data is best viewed as an observational study. We did not assign policies and procedures to cases in a random manner as part of an experiment. Pilot and comparison districts were assigned in a quasi-experimental fashion,

⁵PA(E) also implemented other changes, the results of which we cannot reliably separate from the effects of the track system.

but the actual implementation of case-level policies and procedures reflects the discretion of individual judges and districts.

This allows for the confounding of case characteristics and procedures discussed above, and it also allows for the possibility that procedures are driven in part by the unique characteristics of a case. For example, judges may choose to manage complex cases differently than they manage simple cases. If we have not controlled for all characteristics that drive both complexity and the use of policy, then we might mistakenly conclude that the policy makes cases longer (provided complex cases are longer). The model might suggest this conclusion, even if the policy really shortens the time for truly comparable cases.

The problem is that our estimate of the effect of a policy is the difference between the mean TTD for cases receiving a management procedure and those not receiving the procedure. However, for each management procedure, our goal really is not to estimate the difference between these two groups. Rather, we want to know what would be the difference in TTD between a case managed with a particular procedure and an identical case managed without the procedure. If the procedure was applied at random or was somehow applied in a way completely unrelated to any other case characteristics, then a comparison of cases with and without the procedure would yield an unbiased estimate of our desired effect. If on the other hand, the procedure is not applied at random and is related to other cases characteristics that predict TTD, then we cannot accurately estimate the procedural effect unless we fully control for characteristics that affect both the use of policy and TTD. If we can control for all such confounding characteristics, then we can provide an unbiased estimate of effect using the adjusted difference (adjusted for the control variables) between the cases receiving the management and the remaining cases.

TTD Affects Use of Early Management

Some cases close very quickly after being filed. Almost all these cases receive no management—they close before the court has any opportunity to manage the case. In these cases, the very short time to disposition limits the chosen management policies. However, it is not fair to say that the lack of management hastened the TTD either. In fact, the case closed very early, was not managed, and provides little information about the effect of management. If we leave these cases in the group of cases without management being studied, then we will tend to underestimate effects of management in reducing TTD.

For example, let's hypothesize that early management reduces TTD—if we could experimentally treat pairs of identical cases, managing one set and not managing the other, we would find that, on average, cases with management are shorter. If we include in our observed data, however, these very short cases that would close early with or without management, they would tend to make average TTD in the “not managed” group small, and, in extreme situations, this will make the average TTD in the “not managed” group shorter than in the “managed” group. This could happen even though management really reduces TTD.

To avoid this bias in estimating the effects of management procedures, we ran our analyses using only cases that lasted longer than nine months (270 days). We also ran analyses on all cases that had issue joined to explore the robustness of our findings and to demonstrate the bias resulting from such analyses. We chose the nine month cutoff because we defined early management as occurring within the first 180 days and wanted a window between the periods to avoid over-interpreting transitional effects following management. We found in a sensitivity analysis, in which we varied the nine-month period, that our main results were robust with respect to the nine-month cutoff period, and hence we report the findings using these data.

One should note that truncating the data at nine months introduces bias because we underestimate the effects of early management on cases lasting between six and nine months. That is, among cases that could have been managed early, we do not measure difference in the TTD for cases closing between six and nine months when we only look at cases lasting at least nine months. If early management would tend to bring cases into this window below nine months (by moving a case that would close in perhaps ten months without management), we underestimate the effect of early management. If early management tends to move cases above this nine-month window, we could overestimate the management effect. Our analysis, discussed later, indicates that early management appears to be making cases shorter, or pushing some cases from above to below nine months. In general, the truncation bias introduced by using the nine-month cutoff appears to be moderate. On the other hand, bias from including all the very short cases in the analysis and treating them as “not managed” creates a large bias and even changes the sign of the coefficient for early management in the 1992–93 data analysis.⁶

Finally, it is reasonable to argue that although many cases close within nine months (about 50 percent of all cases), it is really cases that last beyond this point in time (or maybe even longer) that are truly of concern. In other words, if all federal civil cases lasted no longer than nine months, then there would be little interest in finding management procedures to reduce TTD.

Other Case Characteristics Affect Case Management

Although the biasing effects of very short cases on early management discussed above provide the most obvious example of an instance where management practices are determined by cases characteristics other than the control variables in our model (in this example management is directly determined by TTD), there are other policies that we suspect could be susceptible to similar biases due to case characteristics for which we have not been able to adequately control. For example, we expect that, if we count the number of docketed management events for each case, we will find that, in our observed data, longer cases receive more management. This does

⁶We considered other approaches rather than using only the cases that survived 270 days or more. However, because the time to disposition of very short cases is the actual cause for not receiving management, standard econometric solutions are not applicable and any solution would be very sensitive to model specification. Hence, we chose to model cases surviving over 270 days because we could fit robust models and readily explore possible biases.

not necessarily imply that if we could find pairs of identical cases, where one set was actively managed and one was not, that the actively managed cases would last longer. Rather, we expect that longer cases receive more management because there is more time for management, and because such cases are more contested and require more management.

In such examples, there are case characteristics that are not controlled for in the models that may effect the use of a management practice and TTD. This again may create bias in our estimates of the effects of management practice. This possible bias is often referred to as selection bias, and it may exist if our model does not adequately correct for influence of case characteristics on the selection of case management procedures for a case.

For some procedures, we used district averages (or medians) to measure the effects of the typical use of procedures in the district, thereby avoiding selection bias that would occur at the case level. For example, we used the district median days to discovery cutoff to measure the effects of discovery cutoff. We do not use the case-level measure because we expect that for individual cases discovery cutoff will be tailored to the needs of the case. After reviewing the cases, a judge will assign a discovery cutoff that he or she feels is appropriate for the needs of the case. Difficult cases may receive more discovery time and take longer to settle; straightforward cases may receive less discovery time and require less time to settle. However, this difference in TTD may be more a characteristic of the case than the management. By using the district median, we hoped to average across the case characteristics that may influence the specific discovery cutoff and identify districts that typically choose shorter or longer cutoff times regardless of case characteristics.⁷

Even though we have taken many steps to avoid selection bias—including those discussed above, as well as exploring a rich set of control variables, we are still concerned that some of our parameter estimates might be biased. We consider such biases in interpreting our parameter estimates and advise that caution be used in interpreting effect size. For example, if there are substantial differences in the case mix of general civil litigation across districts that are not adequately captured by our control variables, and this difference led to differences in management policies, then the effects associated with our district average variables may overestimate or underestimate the effects of the policy.

WEIGHTS, CLUSTERING, AND STANDARD ERRORS

Sampling Weights

Because we selected a stratified random sample, as discussed in Appendix A, we had unequal selection probabilities for cases within each district. Although there are

⁷Because use of early management depends on TTD, we could not use a district average variable to overcome selection bias in that variable. Furthermore, we could use an objective standard to avoid selection bias and fit a model with a case-level early management variable. Because case-level variables are preferable to district-level variables, we chose to use the over 270 day sample for estimating the effects on TTD.

conflicting opinions on the use of sampling weights when fitting data to statistical models,⁸ we determined that it would be most appropriate to use sampling weights when fitting our regression models. The use of sampling weights guards against model misspecification that corresponds to missing predictors that are related to the sampling strata. Although we had a rich set of control variables, we remained concerned about misspecification and used sampling weights as a means of providing robust estimates.

Our outcome TTD is a case-level outcome with considerable intra-district variation. Some of our key predictors (early management, disclosure) are also case level predictors again with considerable intra-district variation. Furthermore, we believe that individual cases in small districts provide as much information about the effects of policy on outcomes as do cases in big districts. Hence, we did not feel it appropriate to over-weight data from big districts, while under-weighting data from small districts. We did not want data from the handful of very large districts to drive our entire model. For this reason, we used weights that accounted for within-district stratified sampling but did not account for variations in the sampling rate between districts. We used a “district equal” weight (see Appendix A) that gives all districts equal weight in our analysis. By using this weight, we are robust against misspecification that is correlated with intra-district oversampling but avoid the excessive loss in precision that would result if we were to allow the data from four very large districts to drive our analysis.

Estimated Standard Errors

The traditional estimates of standard errors for regression (or censored regression) coefficients are not appropriate for analyses with sample weighted data. These standard error estimates tend to underestimate the variability of the coefficients because they assume that sampling weights are actually precision weights. To provide unbiased (more precisely consistent) estimates of the standard errors, a common approach is to use a Huber or White correction.⁹ Using the analytic algebraic formula for the variance of the estimated regression coefficient and nonparametric estimates of the variance matrix of the outcomes, one creates an estimate of the variance-covariance matrix for the regression coefficients. The estimate is of the form ABA' and is thus called a “sandwich” estimator. The only assumption of this estimator is independence between case outcomes. The sandwich error is used to correct standard errors and does not try to adjust the mean structure or estimated effects. It attempts to allow for the fact that there can be some unexplained differences at the district level and that this could result in unexplained variability in our estimates.

The sandwich estimator has been shown to be valid for ordinary linear regression under various conditions¹⁰ and is also valid as an adjustment for the variance of

⁸See, for example, DuMouchel and Duncan (1983); Little (1991); Nathan and Holt (1980); and Koit (1991), for discussion of these issues.

⁹Huber (1967); and White (1980).

¹⁰White (1980).

maximum likelihood estimates (MLEs) under certain types of misspecification.¹¹ Although we have not formally proved that the sandwich is appropriate for censored regression, our censored regression estimates are MLEs, and by analogy to Huber's work and to the linear regression case this proof should be possible. Note that only closed cases are used in estimating the standard error. This may not be the most efficient estimate, but it will be consistent.

One possible shortcoming of this approach is that the standard error estimator is derived under the assumption of the independence of cases. Most likely there is unexplained heterogeneity among districts (see the subsection on inter- and intra-district effects, below), and this results in correlation among the residual errors from cases within a district. The sandwich estimator can be adjusted to allow for correlation among cases within a district by using an alternative estimator for the variance matrix of the outcomes (TTDs). This estimator is again commonly referred to as the Huber correction or a "robust" estimator of standard error because it is appropriate even if the model is misspecified.

The precision of this correlation-adjusted standard error estimator is determined by the number of districts. With only 20 districts, our estimate is relatively imprecise. Thus, we expect that the unadjusted standard errors (unadjusted for intra-district correlation) will tend to underestimate the standard error of our coefficients, and we also expect that correlation-adjusted estimates might be rather imprecise and lead to variable test statistics. As a compromise, we provide the unadjusted standard errors in the tables given below. However, we note when the significance of our variables changes when we use the correlation adjusted standard errors. For case-level predictors, the bias will tend to be small and the unadjusted standard errors will probably be most appropriate. For district-level variables, the bias could be larger, and more attention should be paid to the adjusted standard errors, even though these could be imprecise.

MODELS FOR TIME TO DISPOSITION

Tables D.4 to D.7 give the results of our primary models for TTD for our 1992–93 sample of filings and our 1991 sample of closures. For each year, we have a model for all general civil cases with issue joined, and a model for such cases with time to disposition greater than 270 days.

ESTIMATING TTD USING 1992–93 SAMPLE DATA

Tables D.4 and D.5 give the results of our primary models for TTD for our 1992–93 sample of filings.

¹¹Huber (1967).

Control Variables

For 1992–93, our model selection procedure identified the following as important control variables, without accounting for the effects of policy variables:

- Class action certification,
- Average judicial work level (moderate),
- Bankruptcy mention,
- Removed,
- Nature of suit category (tort, contract),
- Any government parties,
- Discovery motions,
- Any motions,
- Number of lawyers,
- Case complexity (high or moderate), and
- Maximum stakes.

Table D.4

Model for Time to Disposition: 1992–93 Sample, All General Civil Cases with Issue Joined

Variable	Coefficient	T-Statistic	P Value
Constant	3.297	14.530	0.000
Policy Variables			
Early management on case	0.112	3.194	0.001
Cases managed (district %)	0.006	-2.248	0.025
Cases with trial schedule set early (district %)	-0.008	-6.648	0.000
Early disclosure district policy	-0.020	-0.429	0.668
Joint plan district policy	0.113	1.863	0.062
Litigants at settlement conf. (district %)	-0.016	-5.183	0.000
Limits on interrogatories (district)	-0.106	-2.460	0.014
Days to discovery cutoff (district median)	0.004	7.011	0.000
Continuances (district %)	-0.002	-0.928	0.353
Magistrate judge activity (district mean)	0.150	1.825	0.068
Judicial control over trial (district % firm)	0.001	1.007	0.314
Disclosure on case	0.015	0.496	0.620
Control Variables			
Class action certified	0.328	1.895	0.058
Case type has moderate avg. judicial work	0.099	2.646	0.008
Bankruptcy mention	0.438	4.816	0.000
Removed case	-0.305	-7.373	0.000
Nature of suit (tort)	0.150	3.598	0.000
Nature of suit (contract)	0.037	0.821	0.412
Any government parties	0.132	3.728	0.000
Discovery motion	0.397	11.934	0.000
Any motion	0.310	7.032	0.000
Number of lawyers (square root)	0.010	0.178	0.859
Case complexity (high)	0.233	4.196	0.000
Case complexity (moderate)	0.047	1.258	0.208
Zero stakes	0.064	-0.902	0.367
Maximum stakes (log)	0.055	5.231	0.000
N=2,432			

In our all issue joined model, only the following variables remained statistically significant (at the $p=0.05$ level) after including our policy variables:

- Average judicial work level (moderate),
- Bankruptcy mention,
- Removed,
- Nature of suit category (tort),
- Any government parties,
- Discovery motions,
- Any motions,
- Case complexity (high), and
- Maximum stakes.

Table D.5

Model for Time to Disposition: 1992–93 Sample, General Civil Cases with Time to Disposition over 270 Days

Variable	Coefficient	T-Statistic	P-Value
Constant	4.007	21.615	0.000
Policy Variables			
Early management on case	0.131	-4.856	0.000
Cases managed (district %)	0.001	0.358	0.721
Cases with trial schedule set early (district %)	0.006	-6.010	0.000
Early disclosure district policy	0.036	1.019	0.308
Joint plan district policy	0.121	2.580	0.010
Litigants at settlement conf. (district %)	0.009	-3.732	0.000
Limits on interrogatories (district)	-0.028	-0.837	0.403
Days to discovery cutoff (district median)	0.002	4.164	0.000
Continuances (district %)	0.000	-0.157	0.875
Magistrate judge activity (district mean)	0.110	1.710	0.088
Judicial control over trial (district % firm)	0.002	1.628	0.104
Disclosure on case	-0.014	-0.607	0.544
Control Variables			
Class action certified	0.316	2.427	0.015
Case type has moderate avg. judicial work	-0.023	-0.819	0.413
Bankruptcy mention	0.254	3.123	0.002
Removed case	0.006	-0.188	0.851
Nature of suit (tort)	-0.025	-0.854	0.393
Nature of suit (contract)	-0.062	-1.702	0.089
Any government parties	0.053	1.922	0.055
Discovery motion	0.132	4.973	0.000
Any motion	0.090	2.395	0.017
Number of lawyers (square root)	0.042	0.893	0.372
Case complexity (high)	0.154	3.458	0.001
Case complexity (moderate)	0.007	0.226	0.821
Zero stakes	-0.053	-0.952	0.341
Maximum stakes (log)	0.038	4.680	0.000
N=1,624			

Bankruptcy mentions, tort cases, the presence of any motions and discovery motions in particular, the involvement of government parties, highly complex cases, and increasing the stakes all predicted longer TTD. Types of cases classified as moderate judicial work level and removed cases tended to have shorter TTD than other cases.

In our over 270 days model, only the following six variables remained statistically significant after including our policy variables. Each of these predictors is positively correlated with TTD.

- Class action certification,
- Bankruptcy mention,
- Discovery motions,
- Any motions,
- Case complexity (high), and
- Maximum stakes.

Table D.6

Model for Time to Disposition: 1991 Sample, All General Civil Cases with Issue Joined

Variable	Coefficient	T-Statistic	P-Value
Constant	3.586	13.587	0.000
Policy Variables			
Early management on case	-0.127	-3.408	0.001
Cases managed (district %)	-0.007	-2.262	0.024
Cases with trial schedule set early (district %)	-0.005	-3.841	0.000
Early disclosure district policy	-0.161	-2.189	0.029
Litigants at settlement conf. (district %)	-0.009	-3.292	0.001
Limits on interrogatories (district)	-0.235	-4.153	0.000
Days to discovery cutoff (district median)	0.003	5.057	0.000
Continuances (district %)	-0.003	-1.879	0.060
Magistrate judge activity (district mean)	0.256	2.588	0.010
Judicial control over trial (district % firm)	-0.002	-1.504	0.133
Missing data flag, management level on case	0.174	0.826	0.409
Management level on case	0.087	2.200	0.028
Missing data flag, disclosure on case	-0.052	-0.408	0.683
Disclosure on case	0.100	2.614	0.009
Control Variables			
Class action certified	1.000	2.555	0.011
Case type has moderate avg. judicial work	0.185	-5.002	0.000
Bankruptcy mention	0.576	4.951	0.000
Nature of suit (tort)	0.129	2.894	0.004
Nature of suit (contract)	0.094	2.083	0.037
Any government parties	0.133	2.639	0.008
Discovery motion	0.492	12.662	0.000
Any motion	0.265	5.740	0.000
Number of lawyers (square root)	0.315	5.044	0.000
Missing data flag, case complexity	0.161	0.730	0.465
Case complexity (high)	0.330	4.507	0.000
Case complexity (moderate)	0.131	2.970	0.003
Zero or missing stakes	-0.067	-1.434	0.152
Maximum stakes (log)	0.038	3.018	0.003
N=2,422			

For our 1992–93 models, we imputed values for stakes and complexity for cases where we had no attorney response. We used the other predictors in our model to predict these control variables when they were missing. We imputed the values because lower attorney response rates correlated missing data with longer cases. The use of missing data flags could have introduced bias into our estimates, so we used imputed data. For 1991 data, we used missing data flags because the correlation between time-to-disposition and nonresponse was much lower. In our 1992–93 model, we included a flag for zero stakes cases. This allowed us to use the natural log transformation of maximum reported stakes (a transformation that is essential for avoiding lack-of-fit).

Early Case Management

Using all the general civil cases with issue joined from our 1992–93 data, we found that early management predicted longer TTD. However, using only cases with time-

Table D.7
Model for Time to Disposition: 1991 Sample, General Civil Cases
with Time to Disposition over 270 Days

Variable	Coefficient	T-Statistic	P-Value
Constant	4.017	16.431	0.000
Policy Variables			
Early management on case	-0.296	-8.865	0.000
Cases managed (district %)	-0.003	-0.930	0.352
Cases with trial schedule set early (district %)	-0.007	-6.159	0.000
Early disclosure district policy	0.034	0.514	0.608
Litigants at settlement conf. (district %)	-0.008	-3.263	0.001
Limits on interrogatories (district)	0.020	0.406	0.684
Days to discovery cutoff (district median)	0.002	3.776	0.000
Continuances (district %)	-0.003	-2.250	0.024
Magistrate judge activity (district mean)	0.248	2.648	0.008
Judicial control over trial (district % firm)	0.003	1.951	0.051
Missing data flag, management level on case	0.206	0.600	0.548
Management level on case	-0.002	-0.058	0.954
Missing data flag, disclosure on case	-0.048	-0.331	0.740
Disclosure on case	-0.096	-2.732	0.006
Control Variables			
Class action certified	0.732	2.011	0.044
Case type has moderate avg. judicial work	0.076	-2.205	0.027
Bankruptcy mention	0.443	4.189	0.000
Nature of suit (tort)	0.044	1.063	0.288
Nature of suit (contract)	0.049	1.154	0.249
Any government parties	0.026	0.556	0.578
Discovery motion	0.199	5.660	0.000
Any motion	0.113	2.423	0.015
Number of lawyers (square root)	0.233	4.253	0.000
Missing data flag, case complexity	-0.013	-0.038	0.970
Case complexity (high)	0.252	3.799	0.000
Case complexity (moderate)	0.106	2.659	0.008
Zero or missing stakes	0.032	0.753	0.451
Maximum stakes (log)	0.035	3.001	0.003
N=1,732			

to-disposition over 270 days, we found that early management predicted significantly shorter TTD. Explorations of the data clearly demonstrate that the very short cases that close before receiving management are the source of this discrepancy between the two samples. In other words, the models demonstrate the bias we expected to find when using all the issue joined cases. For this reason, we conclude that early management predicts shorter TTD.

We explored the component procedures of early management separately. For this exploration, we fit a separate model for each component. This model includes both a flag for early management as well as a flag for the particular early management procedure. For example, to explore the specific effect of setting a trial schedule prior to the 180th day of the case, we fit a model that includes our early management flag and a flag that is 1 if the case received a trial schedule (before day 180) and 0 otherwise. The estimated coefficient for the trial schedule flag estimates the difference between cases that receive early management that includes setting the trial schedule and those that receive early management but do not include setting the trial schedule early.

Using this approach we found that cases where a trial schedule was set before day 180 tended to have statistically significantly shorter TTD than did cases receiving other types of early management. We found no statistically significant differences for conferences or mandatory arbitration, and we had mixed results for schedules in general and status reports or joint plans using 1992–93 data. Table D.8 summarizes these findings.

We also found a contextual district effect for setting trial schedules early. In both our 1992–93 models, we found a statistically significant negative correlation between the percentage of cases in a district with a trial schedule set before day 180 and TTD, even after controlling for case-level use of early management.

We did not find similar district-level effects for the percentage of cases managed, percentage with continuances, or percentage with firm judicial control of trials. All these predictors were not statistically significantly related to TTD in our 1992–93 models.

Discovery Control

A district policy on limiting interrogatories is a statistically significant predictor of shorter TTD for all issue joined cases, but not for over 270 day cases. However, the coefficient is not significant in either model when we use standard errors that adjust for intra-district correlation. Thus we conclude that our data provides almost no evidence of an effect of district policies of limiting interrogatories on TTD.

In both our 1992–93 models, we find that the district's median days to discovery cutoff is a statistically significant predictor of TTD. Longer cutoff predicts longer TTD. The statistical significance holds even if we use adjusted standard errors. The actual coefficient appears small but that is due to the scale in days—reduction in expected TTD for a change in discovery cutoff of a single day is small, but the effect of reducing cutoff by a month is considerable.

Table D.8
Estimated Effects of the Components of Early Management on General Civil Litigation Cases

Variable	All with Issue Joined			TTD over 270 Days		
	Coeff.	T-Stat.	P-Value	Coeff.	T-Stat.	P-Value
1992-93 Sample						
Set schedule	0.111	2.105	0.035	-0.110	-2.823	0.005
Set trial schedule	-0.002	-0.057	0.954	-0.120	-4.032	0.000
Held conference	0.044	1.264	0.206	-0.023	-0.787	0.431
Report or plan	0.036	0.870	0.384	-0.065	-2.092	0.036
Mand. arb. referral	-0.065	-0.899	0.368	-0.116	-1.808	0.071
1991 Sample						
Set schedule	0.015	0.251	0.801	-0.237	3.578	0.000
Set trial schedule	-0.095	-2.307	0.021	-0.159	-3.997	0.000
Held conference	0.041	1.038	0.290	0.080	2.061	0.039
Report or plan	-0.069	-1.415	0.157	-0.067	-1.518	0.129
Mand. arb. referral	0.250	-2.635	0.008	0.101	-0.971	0.331

Early Disclosure

Similarly, in both our 1992–1993 models we find no statistically significant difference in TTD between cases from districts that have a policy of mandatory disclosure and those that do not. Furthermore, in separate model runs we found that cases from districts with a policy of mandatory disclosure of information bearing on both sides of the case did not differ significantly in terms of TTD from other cases ($b=0.02$, std. err.=0.06, $p=0.76$ for all issue joined; $b=0.04$, std. err.=0.05, $p=0.37$ for over 270 day cases). Also, we found that cases where the attorneys reported an early disclosure of relevant information were not statistically significantly different than other cases in terms of TTD.¹²

We also found that a district policy encouraging voluntary early disclosure had no statistically significant effect on TTD. For both our 1992–93 samples, we found small positive and not statistically significant differences in TTD between cases from districts with a voluntary early disclosure policy compared to cases from districts with no general policy on early disclosure¹³ ($b=0.089$, std. err.=0.072, $p=0.215$ for all issue joined cases; $b=0.045$, std. err.=0.055, $p=0.416$ for over 270 day cases).

Good Faith Efforts in Resolving Discovery Disputes

Our 1992–93 data provide no evidence of effects on TTD for good faith efforts to resolve discovery disputes before filing motions. Looking at cases with at least one discovery motion, we found no statistically significant difference between cases where the attorney reported good faith efforts and other cases ($b=0.05$, std. err.=0.06, $p=0.47$ for all issue joined cases, $b=-0.01$, std. err.=0.5, $p=0.81$ for over 270 day cases). We re-

¹²We imputed the missing values of our early disclosure variable and found our result was not sensitive to the particular imputed values.

¹³Some districts had policies on early disclosure for a limited number of cases. We considered these districts to have no general policy of early disclosure and included them in our comparison group for studying the effects of voluntary and mandatory early disclosure.

stricted our attention to cases with discovery motions because we suspected that for cases without motions, attorneys would respond “No/Not Applicable” to our survey item—even if good faith efforts were made (see Item 5H, Part A in the Attorney Questionnaire in Appendix J). We made this judgment because we found when exploring all cases that both discovery motions and good faith efforts were associated with longer TTD and good faith efforts rarely occurred for cases without motions.

Mandatory Arbitration Referral

We also explored the effects of mandatory arbitration referral on TTD.¹⁴ We found that cases with an early mandatory arbitration referral did not differ significantly from cases with other forms of early management. This is true for all cases with issue joined and the over 270 day cases. We explored this using all our data and using only those cases that meet the eligibility requirements of mandatory arbitration (only monetary stakes and not over \$100,000). The results from those models are $b=-0.15$, $\text{std. err.}=0.11$, and $p=0.18$ for all issue joined cases and $b=0.08$, $\text{std. err.}=0.12$, and $p=0.51$ for over 270 day cases. The coefficient measures the difference between cases with an early arbitration referral compared to cases with other forms of early management.

Joint Discovery/Case Management Plan Requirement

We found that TTD for cases from districts that required lawyers to produce a joint discovery—case management plan was not statistically significantly different from TTD for cases from other districts, when looking at all cases with issue joined. When looking only at over 270 day cases, we found that TTD for cases from districts with a joint plan requirement was statistically significantly longer than in other districts. However, this finding does not hold when we use the adjusted standard error, which for this district level policy is probably more appropriate. Hence we conclude that there is not strong evidence that this policy is an important predictor of TTD.

We also explored the use of status reports or other joint plans as a management practice at the case level. When compared to cases receiving other forms of early management we found that cases that submitted plans were not statistically significantly different in terms of TTD, when using all issue joined cases. When looking only at over 270 day cases, we found that cases where a status report or plan was filed tend to have a somewhat shorter TTD than do other cases.

Litigants at Settlement Conferences

For both our 1992–93 models, we find that increasing the district percentage of cases with litigants at settlement conferences (either in person or available on the telephone) decreases TTD for cases within the district. In both models, this difference is

¹⁴In our 1992–93 sample, 95 percent of our 142 arbitration referrals were mandatory referrals. In our 1991 sample, all but one of our 119 referrals was mandatory. Hence we refer to our estimate as the predicted effect of mandatory arbitration referral.

significant using the unadjusted standard errors, but it is not significant in the over 270 day model when we used the adjusted standard errors. We fit our original model using attorney responses. In an alternative model, we used data from the judicial surveys and found similar results—increasing the percentage of cases with litigants at settlement conferences decreased the TTD for cases in the district ($b=-0.008$, std. err.=0.003, $p=0.01$ for all issue joined; $b=-0.006$, std. err.=0.003, $p=0.02$ for over 270 day cases).

Use of Magistrate Judges

We measured magistrate judge activity as the number of proceedings handled by magistrate judges in the district per the total number of civil terminations in the district.¹⁵ We obtained the number of magistrate proceedings from forms submitted monthly for each magistrate judge. Exploratory analyses showed that proceedings per civil termination appears to be the most accurate available measure of magistrate judge activity; however, we found qualitatively similar results if we included both proceedings and case terminations by magistrate judges in the numerator of our measure. Regardless of which measure of activity we used, we found that increased magistrate judge activity on civil cases did not predict either reduced or increased TTD for cases in a district.

Pilot vs. Comparison Districts

We also explored a model that included only our control variables and a flag for pilot district (1 if the case is in a pilot district, 0 otherwise). Using all cases with issue joined, we found that cases in pilot districts tended to be shorter ($b=-0.13$); however, this difference is not statistically significant when we use the adjusted standard error ($p=0.19$). For the over 270 day cases, we found only a negligible difference between the TTD for cases from pilot and comparison districts and the difference is not significant.

ESTIMATING TTD USING 1991 SAMPLE DATA

Tables D.6 and D.7 give the results of our primary models for TTD for the 1991 sample of closures. We present a model for all general civil cases with issue joined, and a model for such cases with time-to-disposition greater than 270 days.

Control Variables

For 1991, our model selection procedure identified the following as important control variables without accounting for the effects of policy variables. The model also includes a missing data flag for cases missing the case complexity variable and a flag for missing or zero stakes.

¹⁵In our models, we use the square root of the ratio of the number of annual procedures by magistrate judges per total civil terminations in the district.

- Class action certification,
- Average judicial work level (moderate),
- Bankruptcy mention,
- Nature of suit category (tort, contract),
- Any government parties,
- Discovery motions,
- Any motions,
- Number of lawyers,
- Case complexity (high or moderate), and
- Maximum stakes.

In our all issue joined model, all the control variables remained statistically significant (at the $p=0.05$ level) after including our policy variables. Cases classified as moderate judicial work level tended to be shorter, while all the other control variables predicted longer TTD. For the over 270 day case model, nature of suit category (tort or contract) and government parties were not significant after controlling for the other control and the policy variables. Again for over 270 day cases, all the control variables except case types with moderate judicial work level predicted longer TTD, while moderate judicial work level cases tended to have reduced TTD when compared to the other general civil litigation cases with primarily high work level.

Early Case Management

Our results for the policy variables in our 1991 models are similar to the results from the 1992–93 models, although there is often some difference in the actual magnitude of the coefficient estimates.

In both our 1991 models, we found that early management was a significant predictor of shorter TTD. Thus, the endogeneity bias that results from very short cases that close before management can be applied is not as apparent in the issue joined sample for 1991 as it was for 1992–93.¹⁶ Again we explored the components of early management by comparing cases with each component against the remaining cases with early management. We found that for both models, setting the trial schedule before the 180th day of a case predicted statistically significantly shorter TTD than did other forms of early management. We found no significant effects from requiring a joint plan or report, and mixed results for our other component practices of early management (setting any schedule, holding a conference, or referring to arbitration). Table D.8 gives the full details.

¹⁶This endogeneity effect was there in the 1991 sample, but differences in the early management of cases older than three years in the 1991 sample vs. the 1992–93 sample partially masked the effect.

We also found no strong effects for the percentage of cases managed in the district, continuances, or firm judicial control of trials. Although the percentage of cases managed in the district is associated with statistically significantly shorter cases in the all issue joined model, the finding does not hold when we use our adjusted standard error. Similarly, fewer continuances appears to predict longer cases in the over 270 day sample for 1991, but this effect is not statistically significant when we use the adjusted standard error.

In the all issue joined sample, we found that a higher level of management is a statistically significant predictor of longer TTD. We suspect that this reflects selection bias. Almost by definition, cases that last longer will tend to be more difficult to resolve. Because the cases are more difficult to resolve and last longer, over their entire longer length these cases receive higher management than less complicated shorter cases. This results in an association of longer TTD for cases with a high level of judicial management over the entire case. Because of this bias, we do not believe we can make any valid conclusions on the actual effect of this predictor. We did not include the variable in the 1992-93 models for this reason and it is provided here only to demonstrate the possible bias.

Discovery Control

The effects of controlling discovery are also similar across the two years of data. Cases from districts with an explicit policy limiting interrogatories tended to be shorter than cases from other district in the all issue joined sample, but the effect was much smaller and not significant for the over 270 day cases. On the other hand, longer district median days to discovery cutoff was a statistically significant predictor of longer TTD in both samples. This result is identical to that found with our 1992-93 sample.

Early Disclosure

The effects of early disclosure were quite different in the 1991 sample than in the 1992-93 sample, probably because the policies were very different in the two samples. In our 1992-93 sample, we found no statistically significant effects for early disclosure—neither district policies nor practice on individual cases had much of an effect. In the 1991 sample, however, we found that individual cases where parties disclosed relevant information tended to be shorter than other cases. This case effect is statistically significant in both our 1991 models. Also, the few districts with any disclosure policy tended to have shorter TTD in our all issue joined sample of cases, although the effect was not significant when we used the adjusted standard errors and the effect changed sign and was not significant with the over 270 day cases.

It is important to note that between 1991 and 1992-93 many districts implemented policies of mandatory early disclosure, while very few districts had such policies in 1991. Thus, early disclosure cases in our 1991 sample are primarily voluntary early disclosure, but cases with early disclosure in our 1992-93 sample also include mandatory disclosure. Cases where the parties volunteer early disclosure could dif-

fer from other cases; for example, the parties might be more motivated for settlement or the lawyers might have different approaches to litigation. These differences might explain the discrepancy between the two sample years without requiring any change in the hypothesized underlying effect of early disclosure on similar cases.

Good Faith Efforts in Resolving Discovery Disputes

With our 1991 sample, we again used a sample of cases with discovery motions to explore the effects of good faith efforts to resolve discovery disputes. We again found that the effects of good faith efforts were not statistically significant in either of our models ($b=0.06$, $\text{std. err.}=0.07$, $p=0.37$ for all issue joined; $b=-0.09$, $\text{std. err.}=0.06$, $p=0.18$ for over 270 days).

Mandatory Arbitration Referral

The 1991 over 270 day sample confirms our findings on arbitration in the 1992–93 sample: Early referral to arbitration does not lead to shorter TTD than do other forms of early management. As shown in Table D.8, we estimate the difference in TTD between cases with early arbitration referral and other cases with early management to be moderate and not statistically significant. For all issue joined cases, however, we find a larger effect that is statistically significant.

One limitation of the above arbitration analyses is that they consider all cases, but only select cases (with relatively small monetary stakes) are eligible for arbitration. Hence the observed difference between cases with an early arbitration referral and other cases with early management could reflect the differences between the arbitration-eligible cases (even without a referral) and other cases. To explore this issue, we fit models using only cases that would meet arbitration eligibility requirements. In these models, we found that the effect for the all issue joined sample was $b=-0.25$ ($p=0.07$) and for the over 270 day sample it was $b=-0.04$ ($p=0.79$). Hence, the effect for the all issue joined sample is still larger although neither model produces a statistically significant estimate. Given the small sample size in this limited subsample, the larger effect should not be over-interpreted.

Joint Discovery/Case Management Plan Requirement

Table D.8 also shows that in the 1991 sample we found that cases with early status reports or joint plans did not differ significantly from cases with other forms of early management. This is consistent with the mixed finding we observed in the 1992–93 samples.

Litigants at Settlement Conferences

The 1991 data also support our finding from the 1992–93 data on the effects of litigants at settlement conferences: The expected TTD decreases as the percentage of cases with litigants at settlement conference increases in the district. The effect is

statistically significant (with both the adjusted and unadjusted standard errors) in both our 1991 models as well as in both our 1992–93 models.

Use of Magistrate Judges

In the 1991 data, we observe a statistically significant positive coefficient for magistrate judge activity in both our models of TTD. However, the coefficient in neither model is significant when we use the adjusted standard errors ($p=.69$ for all issue joined cases, and $p=.59$ for over 270 day cases). We are especially cautious about over interpreting the 1991 model effects because the effects in our 1992–93 models are much smaller and not statistically significant. Hence, we do not feel that there is any strong evidence of effects on TTD that result from the use of magistrate judges.

Pilot vs. Comparison Districts

Finally, with our 1991 data we also fit separate models (all issue joined, and over 270 days) that contained only the control variables and a pilot district flag. For both models, we found small and not statistically significant positive differences between the TTD for cases in pilot districts and cases in comparison districts ($b=0.04$, std. err.=0.03, $p=0.24$ for all issue joined cases; $b=0.06$, std. err.=0.03, $p=0.05$ ¹⁷ for over 270 days cases). This is consistent with our findings from the 1992–93 sample where we found only small and statistically insignificant pilot effects.

MODELS FOR PERCENTAGE OF CASES WITH ISSUE JOINED AND PERCENTAGE OF CASES WITH TIME-TO-DISPOSITION OVER 270 DAYS

If we were to ignore the effects of case management on the percentage of cases with issue joined and the percentage of cases with time to disposition over 270 days, we would risk biasing our estimated effects of policy and making incorrect inferences. For example, if early management encouraged cases to settle prior to issue being joined or prior to 270 days, then we might underestimate the policy effects by looking only at cases with issue joined or only cases lasting over 270 days.

To test for such biases, we ran models for each sample to determine if district-level policies affect the percentage of cases with issue joined or the percentage of over 270 day cases. Our procedures were similar for studying effects on both issue joined and over 270 day cases. Using the case-level model control variables, we identified a set of related aggregate district-level control variables that we expected to predict TTD and issue joined. We also identified a set of aggregate district-level policy variables that we expected to predict TTD and issue joined. We then fit a model using these district controls and one of the district policy variables.¹⁸ We fit separate models for

¹⁷ $p=0.44$ when we use the adjusted standard error.

¹⁸Because we were modeling proportions, we used logistic regression models for testing these effects. See Appendix E for a description of logistic regression models.

each policy variable, and we repeated the process for both issue joined and over 270 day cases. The unit of analysis was the district so that we avoided any selection bias that might enter our models from using individual cases.

Our goal was to identify any particular policies that had a significant effect on the percentage of cases with issue joined or the percentage of cases with TTD over 270 days. By looking at one policy variable at a time, we estimated each policy variable's total effect. If we were to find little predictive power for our policy variables in these district models, then we could conclude that there is only limited evidence of selection bias. This is an admittedly weak test but it provides a means of identifying cases with large selection bias.

For predicting issue joined, the above process resulted in use of average number of lawyers per case and percentage of cases that were highly or moderately complex as controls in our 1992–93 models. We used percentage of cases with government parties, percentage of cases that were highly or moderately complex, and average log stakes as controls in our 1991 models. In the 1992–93 models, we found no statistically significant relationship between our aggregate district-level policy variables and percentage of cases at issue. Using our 1991 sample, we found that early management and management level were both statistically significant predictors of fewer cases at issue. Thus, we can conclude that our estimates of the effect of early management in 1991 might be conservative, and the actual effect of early management on reduction of TTD may be larger than we predicted.

For predicting the percentage of cases in our over 270 day sample, we used the average number of lawyers per case and percentage of cases that were highly or moderately complex as controls in our 1992–93 models. For our 1991 models, we used the percentage of cases with a bankruptcy mention, percentage of cases with government parties, percentage of cases that were highly or moderately complex, and average log stakes as controls. We found for both samples (1992–93 and 1991) that districts with disclosure policies had a statistically significantly lower percentage of cases in the over 270 day sample. We also found that increasing the percentage of cases with an early arbitration referral significantly reduced the percentage of cases in the over 270 day sample for the district. For both these policies, our case-level models again might be producing conservative estimates of their effects on TTD. Both our models found that median days to discovery cutoff was positively (and statistically significantly) correlated to the percentage of cases in the over 270 day sample for the district. Because we estimate that median days to discovery cutoff is positively related to TTD in our case models, we again tend to underestimate that effect in those case-level models.

INTER- AND INTRA-DISTRICT EFFECTS

In federal district courts, there is large inter-district variation in median time to disposition. The medians vary from less than six months to over a year. Hence one might be concerned that our findings on policies and procedures are actually a reflection of innate district variations that happen to correlate with the use of policy. To explore this issue, we fit fixed district effect models. In these models, we included

a flag variable for each district and removed the other district-level policy variables. By fitting fixed district effect models, we are using only intra-district variation to model our case-level policies.

We then compared the results for case-level policies to our previous results. We found qualitatively similar results using fixed district effect models as we found using our primary models with district-level policy variables. Thus, our estimated policy effects are not being driven by district to district variation but represent both intra- and inter-district effects. This implies that intra-district variation is important in determining our estimates.

We also noted that the models with fixed district effect flags explained about the same amount of variation in the TTD outcome as did models with district-level policy variables. This implies that our district-level policy variables are about as good at predicting TTD as district flags. And district policy variables are much more meaningful in interpreting the effects of case management.

ESTIMATING EFFECTS IN DAYS

We fit our models for TTD on the “fourth root of days” scale for statistical reasons. Although this was appropriate for avoiding model misspecification and making proper inference, it makes the estimated effect difficult to interpret. To aid in the interpretation of these effects, we convert our results from the “fourth root of days” scale to the “days” scale. We provide standardized differences on the “days” scale for three of our most important policy variables: early management, setting a trial schedule early, and median days to discovery cutoff.

Our standardized effect in days was estimated using the following procedure. First, for every case in the target sample (described below) we predicted TTD using the case with the policy to obtain an estimate y_1 and without the policy to obtain an estimate y_2 . For district median days to discovery cutoff, “with the policy” refers to a low median—120 days, which is about the lower quartile—and “without the policy” refers to a high median—180 days, which is about the upper quartile in our data. We then reverse transform the predicted y_1 and y_2 to the real “days” scale (raise each fourth root estimate to the fourth power) to obtain the estimated z_1 and z_2 on the real days scale.

For each case in our sample, we now have two estimates on the real “days” scale: z_1 and z_2 . In our statistical model, we assume that each observed case in our sample is a random draw from a distribution of possible cases. The distribution of possible cases is determined by the our case’s control and policy variables. The estimate z_1 represents our best estimate of the median time-to-disposition for the unobserved distribution of cases with our case’s characteristics with the policy. And the estimate z_2 represents our best estimate of the median time-to-disposition for the unobserved distribution of cases with our case’s characteristics without the policy.

We now take the difference of z_1 and z_2 and call this d . We now have a sample of d ’s which represent our best estimate of the effect of policy on each case in our sample. We summarize this sample of d ’s by taking the median. This median of the differ-

ences is our estimate of the effect size in real days. We take the median because the distribution of d 's tends to be skewed. Hence our estimate of the effect size might be described as the "typical effect we would expect to see on the typical case." Table D.9 contains our estimated effects and confidence intervals for these effects. We used bootstrap methods to calculate these confidence intervals.¹⁹

We repeated this process for the policies of early management, setting a trial schedule early and discovery cutoff. We also estimated a combined effect by estimating TTD for each case in the target sample assuming that the case received all three policies and then again assuming the case received none of the policies.

For estimating the effect of early management and discovery cutoff, the target sample was our over 270 days sample. Thus, we estimated the difference, d , for every case in our over 270 days sample and report the median for this sample. For our combined effect, we also used the entire over 270 days sample. For the effect of setting a trial schedule early, compared to other early management practices, our target sample was all cases in our over 270 days sample that received early management.

Table D.9
Estimated Effect Sizes in Days for Selected Policy Variables
for the Over 270 Day Sample

Variable	1992-93	1991
Primary Model		
Early management	-61 ^a (-85, -37)	-148 (-187, -111)
Set trial schedule early	-50 ^b (-75, -25)	-72 ^b (-114, -29)
Median days to discovery cutoff	-49 ^c (-80, -14)	-62 ^c (-118, -3)
Combined effects	-142 ^d (-183, -99)	-252 ^d (-317, -183)
Adjusting for Days to Answer		
Early management	-39 (-62, -14)	-138 (-176, -102)
Set trial schedule early	-46 ^b (-71, -21)	-70 ^b (-111, -30)
Median days to discovery cutoff	-53 ^c (-84, -18)	-64 ^c (-118, -10)
Combined effects	-122 ^d (-164, -77)	-243 ^d (-307, -176)

NOTE: Numbers shown in parentheses are 95 percent confidence intervals.

^aThe number shown is for any type of early management. For early management that does not include getting a trial schedule early, the number is -43 days.

^bCompared to other forms of early management.

^cDistrict median discovery cutoff of 120 days compared to a median cutoff of 180 days.

^dEffects of early management including setting a trial schedule and reducing district median discovery cutoff from 180 to 120 days from the date the schedule is set.

¹⁹Efron and Tibshirani (1993).

Note that for setting the trial schedule early, our estimated effect is the difference in time to disposition between (1) using early management that includes setting the trial schedule early and (2) using early management that does not involve setting the trial schedule early. We do not estimate effect sizes for the all issue joined sample because the bias from very short cases that close before management can be applied would make any estimate for early management from that sample somewhat suspicious. However, if we were to ignore these very short cases in the all issue joined sample, then we expect that the case management practices would reduce time to disposition for all remaining cases, although we have not estimated this effect.

Table D.9 contains a second estimate of the combined effect of policy, made by adjusting for days from filing to answer by defendants on the case. We found that early management was correlated with the number of days from filing to the first answer by a defendant. We also know that with the number of days from filing to the first answer by a defendant is correlated with TTD—in fact, it is a component of TTD. Thus, because our primary model does not account for this factor, we may be overstating the effect of early management. Cases that are answered quickly may close quickly and along the way be more likely to get early management.

To explore this effect, we ran a model that included days to answer as a predictor as well as our usual control and policy variables. We then used this model to estimate the effect of early management, setting the trial schedule early, and the median days to discovery cutoff. The analysis shown in Table D.9 indicates that if we control for days to first answer, we estimate smaller effects for early management and for the other policies. However, because days to first answer is truly a component of TTD, including this variable as a predictor in the model might cause us to underestimate the effects of policy. Hence, we should consider the effect from this model as a lower bound and the effects from our primary model as an upper bound to an accurate estimate of the effect of policy for cases included in our sample. We do not include days to answer in our primary models because of the possible bias, even though it appears to have negligible effects on any policy variable prediction other than the early management prediction.

Overall, the predicted policy effects are larger in our 1991 sample than in our 1992–93 sample. This is due in part to the fact that, relative to our 1992–93 sample, our 1991 sample is a cohort of closed cases that includes more old cases where policies were not applied. Any changing trends over time in the use of policy make our estimates from the 1991 sample likely to be different because policies used at the case level have changed since then. We know, for example, that more cases are managed early in the 1992–93 sample than in 1991 sample, and that discovery cutoff times were shorter in 1992–93 than in 1991. Hence we believe that our 1992–93 sample provides a better estimate of the effect of policy applied in the most recent time period. We also note that our estimate of the combined effect relies heavily on the assumed additivity of our model. Although we did not find any strong indication of lack-of-fit in our models, some nonadditivity could exist. We advise that caution be used when interpreting this combined effect.

INTERPRETING EFFECTS AND GENERALIZING TO OTHER CASES, JUDGES, AND DISTRICTS

Although these predicted effects serve as a useful gauge of our model estimates, they might present the temptation to interpret them as the exact size of a causal effect. That is, one might incorrectly treat these estimates as if expanding the use of early management will reduce TTD on cases by 61 days for each and every new case that receives early management. This almost assuredly will not happen in exactly the same way. So, rather than interpret these standardized effects as the exact size of a causal effect, one should interpret them as a guide to the observed effects in our data. Because we have observational data rather than randomized experimental data, it is difficult to determine exactly why these differences occur, but our standardized effect provides a measure of the size of the observed effect.

There are reasons why our observed effect might not generalize in exactly the same way to other cases, judges, or districts.

One reason is that the cases in our data that receive the policies might be different from those that do not receive the policies in some way not accurately measured by our control variables. Despite the fact that we have more and better data than have been available to previous studies, and we have been as careful as possible in constructing our many models, one must still interpret the results carefully.

We believe that we have provided a reasonable estimate of the effects of policy for the cases, judges, and districts we observed in our data. It is much more difficult to determine the effect of the policy if implemented on different cases or by different judges in the same or different districts.

Judges who choose to implement policies and management procedures often do so at their discretion. These judges may differ from other judges in their basic approach to case management or because of personality. These differences between judges could affect the implementation of policy, and this could change the policy's effect. For example, if enthusiastic managerial judges currently set trial schedules early and also work hard on settlement and this leads to early closure, then having less enthusiastic non-managerial judges setting trial schedules early may not have the same effect that we observed.

Similarly, districts that choose to implement policies and procedures do so because of the characteristics of the district and the judicial officers. Because policies were not assigned to cases at random in our data, we cannot fully untangle the relationship between district characteristics and the use of policies. Hence, it is hard to determine exactly how the policies will affect TTD if implemented on a wider scale.

We stress that statistical models do not show cause and effect. Causation must be interpreted in light of understanding how the underlying civil justice system that generated the data operates.

Given our understanding of how the civil justice system operates, we believe that the policies we identified as important predictors of shorter time to disposition are likely to reduce time if implemented, but that our estimated effect should be treated as an

upper bound to the effects that would occur if the policies were implemented in all districts by all judges for all cases.

SUMMARY OF EFFECTS ON TIME TO DISPOSITION

We found four policies that showed consistent statistically significant effects on time to disposition: (1) early judicial management; (2) setting the trial schedule early; (3) reducing discovery cutoff (median days to discovery cutoff in a district); and (4) having litigants at or available on the telephone for settlement conferences. For general civil cases with issue joined that do not close within the first nine months, we estimate that early management, setting the trial schedule early, and reducing discovery cutoff time have the combined effect of reducing median time to disposition by about four to five months in our 1992–93 sample.

Other policies and procedures we studied were either not statistically significant or not consistently significant.

Finally, in both our 1991 and our 1992–93 samples we find no significant differences in time to disposition between the cases from the pilot districts and comparison districts.

ANALYSIS OF LAWYER WORK HOURS

INTRODUCTION

Lawyer work hours are our most informative measure of litigation costs, as discussed in Chapter Two. In this appendix we discuss our analysis of lawyer work hours on general civil litigation cases and the effects of case management policies and procedures on those hours.

We use total lawyer work hours in our analyses as a measure of the total litigation costs. In addition to the total hours, we collected the components of lawyer work hours, as a method of helping improve the accuracy of the lawyers responses (see Appendix J, especially question 9). Hence, we have data on lawyer time spent for each sample case on trial, ADR, discovery, motion practice, conferences, and other time spent both before and after filing. Those components of total lawyer work hours are a rich source of information for possible future research into the components of the cost of litigation. We did not collect data on when in the life of the case the lawyer hours were expended.

Control and Policy Variables

Our methodology for modeling the effects of policy and procedures on lawyer work hours are analogous to those that we use for analyzing TTD. We again considered both control variables and policy variables. Control variables consisted of case, district, and attorney characteristics that could explain differences in work hours and policy variables measured the district and case level management policies and procedures. We added some attorney characteristics to the list of control variables used for TTD. The policy variables used in our analysis of work hours are the same as those used for modeling TTD.

Modeling Effects on General Civil Cases with Issue Joined

We used the same sample of general civil litigation cases with issue joined for our analysis of lawyer work hours as we did for analyzing TTD.

Multivariate Regression Models on Attorney-Level Data

To estimate the effects of policy on lawyer work hours, we used multivariate regression models fit to attorney-level data. That is, we used data with one record per each responding attorney from our sample of general litigation cases with issue joined. Work hours can differ within cases and we use attorney-level characteristics to explain some of this variability in our model.

Our model is analogous to the TTD model in Appendix D, except that we now have responses from attorneys within cases, within districts. We assume that the same additive linear model and the estimated coefficients from our model represent the estimated policy (or control) effects. Again we analyzed the data from our two sample years separately, using the 1991 sample data as confirmation for our findings from the 1992–93 sample data.

Open Cases and Missing Data

As discussed in Appendix D, 8.5 percent of our cases from our 1992–93 general civil litigation sample remained open at the end of our study. Although we had lawyer responses from a fraction of these open cases, we do not feel that we should use data from these open cases in exploring the effects of policy on lawyer work hours. The partial data provided by these open cases would not be comparable to complete data on closed cases because work hours tend to be unevenly spent with a potentially major portion of work time coming near settlement or trial. Also, few of the attorney respondents from open cases provided data on work hours. Thus, our models are fit only to data from respondents from closed cases. As discussed below, this is likely to introduce some bias into our estimated coefficients, but because we are dealing with only 8.5 percent open cases, we expect this bias to be limited.

Because lawyer work hours were reported by lawyer survey responses, we do not have data for all attorneys in the sample. About 50 percent of attorneys did not respond to our survey. Of responding attorneys, about 75 to 80 percent provided data on lawyer work hours. Attorneys without reported work hours were excluded from our sample for the purposes of fitting these models. We used nonresponse weights in our analysis to offset the effects of differential nonresponse, as discussed in Appendix B. However, missing information is problematic and could introduce some bias into our estimates of policy effects. Table E.1 gives the number of attorney responses with hours used for fitting our models by district.

LAWYER WORK HOURS

Since lawyers may represent more than one litigant on a case, and since some litigants may have more than one lawyer working on the case, it is necessary to sort out the method of handling these multiple relationships before describing the analysis.

Table E.1
Attorneys Responding with Work Hours on General Civil Cases
by District and Sample Year: All Issue Joined and
Time to Disposition Over 270 Day Samples

District	1992-93 Sample		1991 Sample	
	All Issue Joined	Over 270 Days	All Issue Joined	Over 270 Days
AZ	89	68	89	62
CA(C)	71	44	73	48
CA(S)	46	30	78	51
DE	98	60	123	102
FL(N)	79	50	112	95
GA(N)	109	65	139	96
IL(N)	79	47	85	50
IN(N)	96	59	117	83
KY(E)	107	84	90	71
KY(W)	85	55	116	94
MD	97	63	105	67
NY(E)	58	45	88	57
NY(S)	55	35	68	54
OK(W)	162	66	152	83
PA(E)	100	40	112	56
PA(M)	78	58	116	87
TN(W)	86	55	81	63
TX(S)	93	62	141	109
UT	88	61	134	111
WI(E)	115	75	101	77
Total	1,791	1,122	2,120	1,516

For each litigant on a case, we initially surveyed only one attorney and asked that one attorney for the number of hours “worked by you and ALL attorneys for your party or parties on this case” (see questions 8 and 9 in Appendix J). If the first attorney could not supply information for all attorneys for their party or parties, then we surveyed a second attorney. If we ended up surveying more than one attorney for a party, then we combined the two work hour responses into one total work hours variable before analysis. If we had only partial lawyer work hours for a party, then we did not include them because we knew they were incomplete.

Some attorneys represented multiple clients on the same case, and we found that work hours grew as a function of the number of litigants the attorney represented. Hence we modeled attorney work hours per party represented. We explored both total work hours by the lawyer and hours per party represented by the lawyer and found qualitatively similar results. However, we had the best fitting model (no obvious residual lack-of-fit) when we used hours per party represented and included the square root of the number of parties represented as a predictor variable to allow for economies of scale.

We include lawyer work hours for attorneys with all types, no matter how they are paid (e.g., contingent fee, hourly fee, government attorney, and in-house private organization attorney).¹

Our sample distribution of lawyer work hours per party represented was highly skewed. As discussed in Appendix D, it is not appropriate to model highly skewed data using additive linear regression models. Using exploratory analyses we determined that by using the natural log of lawyer work hours per party represented we removed the skew from our data and could fit linear additive models without obvious lack-of-fit. It is important to note that no attorneys should report zero hours spent on a case so we could use the natural log transformation on all data without worrying about transforming zero.

In the remainder of this appendix will refer to the natural log of lawyer work hours per party represented as lawyer work hours.

CONTROL AND POLICY VARIABLES

In our search for control variables we considered all the case- and district-level controls that we explored in our models for TTD (see Appendix D, Table D.2 for details). We also considered additional attorney-level characteristics that we expected may help explain variation in work hours. The additional control variables we considered are given in Table E.2. The variables are particular characteristics of the attorney (years in practice) or the attorney-client relationship (fee structure) that cannot be considered characteristics of the case as a whole.

Even though TTD and work hours are moderately correlated, we did not include TTD as a control variable in our model for lawyer work hours. As we demonstrate in Appendix D, many of our policy variables are correlated with TTD. Had we included TTD in our model, then we would not fully capture the effects of case management policy on work hours. With TTD in the model, indirect effects, such as the effect of policy on TTD that results in effects on lawyer work hours, would not be captured in our model. We decided to estimate the “total effect” (both the indirect effect through TTD and the direct effect) of case management policy on lawyer work hours and did not include TTD in our model.

We explored the same policy variables in our models of lawyer work time as we did in our analysis of TTD. These policy variables represent the important policies and procedures endorsed by the CJRA and are appropriate for use with all our outcomes.

¹ Even if an in-house or government attorney does not “bill” for services, there is still a cost to the litigant incurred for salary, fringe benefits, and overhead and hence the hours worked by those attorneys on litigation are not “free” over the long term, even if some attorneys may currently have slack time.

Table E.2
Additional Control Variables Used for Modeling Lawyer Work Hours

Attorney Level Variables	Description
Tenure	Years practicing law
Percent practice federal	Percentage of practice devoted to federal district court litigation in past five years
Firm size	Square root of the number of lawyers in law office or legal department
Contingent fee	Primary fee arrangement was a contingent fee; 1 if contingent fee, 0 otherwise
Government attorney	Attorney was a government attorney; 1 if yes, 0 otherwise
Defense attorney	Attorney represented a defendant; 1 if yes, 0 otherwise
Number of parties	Square root of the number of parties represented by the attorney in the dispute

ENDOGENEITY OF LAWYER WORK HOURS AND POLICY VARIABLES

As discussed in Appendix D, because of the observational nature of our study, we must consider the effect of selection bias on our estimates of the effect of policy and procedures on case outcomes such as TTD and lawyer work hours. Of particular concern is the bias that results from very short cases that close before they can receive any management and do not accurately reflect the effects of policy. We used the same approach to avoiding this bias in our models for lawyer work hours as we did for analyzing TTD. We fit models to a subsample of data from cases with time to disposition of greater than 270 days, and we refer to this as our over 270 day sample. For policy variables that could be driven by case characteristics, we again used the approach taken with our TTD analysis and used district-level variables for the policy rather than the case-level variables. The limitations of these approaches to modeling cited in the TTD modeling Appendix D are again applicable to our models for lawyer work hours and the appropriate caution should be used in interpreting our estimated effects.

WEIGHTS, CLUSTERING, AND STANDARD ERRORS

Sampling and Nonresponse Weights

We followed the same procedures when fitting our models for lawyer work hours as we followed for TTD and used both sampling and nonresponse weights to provide a robust alternative to fitting unweighted data. See Appendix B for a discussion of lawyer nonresponse weights.

Estimated Standard Errors

The discussion on standard error estimation given in Appendix D pertains to our models for lawyer work hours as well as for our models of TTD. The traditional ordi-

nary least squares standard error estimates are not appropriate for weighted data. Again we used a sandwich estimator to provide the appropriate estimated standard errors for our analysis of lawyer work hours.

We might again expect that there is residual correlation among cases from the same district in our models for lawyer work hours. Again the sandwich estimate provides a consistent (if possibly imprecise) estimate of the standard errors that accounts for such correlation. Using the same argument given in Appendix D, we report the unadjusted standard errors and note when our results are not robust when accounting for intra-district correlation.

In cases with multiple attorneys, we might expect to find residual correlation among those attorneys. This could again bias our standard errors if we assume that the responses from different attorneys are independent. Using methods analogous to those we used for estimating the standard errors in the presence of intra-district correlation, we can estimate standard errors that account for intra-case correlation. We used this approach to estimate standard errors and found little difference between these standard errors and the unadjusted standard errors. This similarity is the result of having few attorney responses per case, typically only one or two, and having only modest intra-case correlation. Furthermore, the sandwich estimator standard errors that account for intra-district correlation also accurately account for intra-case correlation. Hence for simplicity of presentation, we present the unadjusted standard error (or t-statistics based on them) and note when the standard errors adjusted for intra-district correlation would change our inference.

Models for Lawyer Work Hours

Tables E.3 to E.6 give the results of our primary models for lawyer work hours. Tables E.3 and E.4 give our models for our 1992–93 sample of filings and Tables E.5 and E.6 give our results for the 1991 sample of closures. For each year we have a model for all general civil cases with issue joined and a model for cases with time to disposition greater than 270 days.

ESTIMATING LAWYER WORK HOURS USING 1992–93 SAMPLE DATA

Control Variables

For the 1992–93 sample, our model selection procedure identified the following as important control variables before accounting for the effects of policy variables (see Table D.2 for a discussion of these control variables).

- No mention of class action on the case docket,
- Average judicial work level (high),
- Jurisdiction (diversity)
- Discovery motion,
- Any motion,

Table E.3
Model for Lawyer Work Hours: 1992–93 Sample, All General Civil Cases
with Issue Joined

Variable	Coefficient	T-Statistic	P-Value
Constant	1.229	2.652	0.008
Policy Variables			
Early management on case	0.278	4.761	0.000
Cases managed (district %)	-0.000	-0.111	0.912
Cases with trial schedule set early (district %)	0.002	0.798	0.425
Early disclosure district policy	0.095	1.214	0.225
Joint plan district policy	-0.247	-2.238	0.025
Litigants at settlement conf. (district %)	0.002	0.434	0.664
Limits on interrogatories (district)	-0.108	-1.468	0.142
Days to discovery cutoff (district median)	0.003	3.967	0.000
Continuances (district %)	-0.004	-1.317	0.188
Magistrate judge activity (district mean)	0.059	0.417	0.677
Judicial control over trial (district % firm)	-0.000	-0.004	0.996
Disclosure on case	-0.058	-1.124	0.261
Control Variables			
Number of litigants (square root)	-1.040	-15.038	0.000
Class action: no mention	-0.469	-2.290	0.022
Case type has high avg. judicial work	0.082	1.445	0.149
Diversity jurisdiction	0.130	2.110	0.035
Discovery motion	0.598	9.751	0.000
Any motion	0.324	4.557	0.000
Any government party	0.002	0.022	0.982
Any litigant without an attorney	-0.110	-1.422	0.155
Any pro se litigant	-0.126	-0.959	0.338
Case complexity (high)	0.622	6.026	0.000
Case complexity (moderate)	0.265	3.382	0.001
Zero stakes	0.284	-3.576	0.000
Maximum stakes (log)	0.259	12.959	0.000
Missing fee	-0.210	-1.784	0.074
Contingent fee	-0.131	-0.844	0.399
Government attorney	0.679	0.991	0.322
Missing percent practice federal	0.311	0.939	0.348
Percent practice federal	0.005	4.603	0.000
Missing firm size	0.150	0.488	0.626
Firm size (square root)	0.040	6.078	0.000
Defense attorney	-0.194	-2.894	0.004
State case	-0.233	-4.155	0.000
Contingent fee x case complexity (high)	0.266	1.375	0.169
Contingent fee x case complexity (mod.)	-0.008	-0.052	0.958
Contingent fee x firm size	0.005	0.190	0.850
Gov't atty x case complexity (high)	0.540	1.781	0.075
Gov't atty x case complexity (mod.)	0.110	0.562	0.574
Gov't atty x case maximum stakes	-0.102	-1.754	0.079
N=1,791			

Table E.4
Model for Lawyer Work Hours: 1992–93 Sample, All General Civil Cases
with Time to Disposition over 270 Days

Variable	Coefficient	T-Statistic	P-Value
Constant	0.956	1.654	0.098
Policy Variables			
Early management on case	0.254	3.591	0.000
Cases managed (district %)	0.008	1.617	0.106
Cases with trial schedule set early (district %)	0.003	1.200	0.230
Early disclosure district policy	0.134	1.356	0.175
Joint plan district policy	-0.378	-2.633	0.008
Litigants at settlement conf. (district %)	0.004	0.624	0.533
Limits on interrogatories (district)	-0.170	-1.799	0.072
Days to discovery cutoff (district median)	0.004	3.330	0.001
Continuances (district %)	-0.008	-2.236	0.025
Magistrate judge activity (district mean)	-0.083	-0.467	0.641
Judicial control over trial (district % firm)	0.001	0.292	0.770
Disclosure on case	-0.104	-1.626	0.104
Control Variables			
Number of litigants (square root)	-0.967	-13.108	0.000
Class action: no mention	-0.213	0.879	0.379
Case type has high avg. judicial work	0.131	1.897	0.058
Diversity jurisdiction	0.151	1.965	0.049
Discovery motion	0.538	7.482	0.000
Any motion	0.234	2.277	0.023
Any government party	-0.125	-1.469	0.142
Any litigant without an attorney	-0.136	-1.469	0.142
Any pro se litigant	-0.181	-1.170	0.242
Case complexity (high)	0.588	4.478	0.000
Case complexity (moderate)	0.387	3.718	0.000
Zero stakes	-0.305	-2.849	0.004
Maximum stakes (log)	0.257	9.800	0.000
Missing fee	-0.379	-2.591	0.010
Contingent fee	-0.153	-0.738	0.460
Government attorney	0.373	0.418	0.676
Missing percent practice federal	0.484	1.100	0.271
Percent practice federal	0.006	4.674	0.000
Missing firm size	-0.168	-0.407	0.684
Firm size (square root)	0.042	5.676	0.000
Defense attorney	-0.249	-2.857	0.004
State case	-0.283	3.768	0.000
Contingent fee x case complexity (high)	0.374	1.561	0.118
Contingent fee x case complexity (mod.)	-0.065	-0.329	0.742
Contingent fee x firm size	0.011	0.435	0.664
Gov't atty x case complexity (high)	0.341	0.973	0.330
Gov't atty x case complexity (mod.)	-0.052	-0.212	0.832
Gov't atty x case maximum stakes	-0.066	-0.870	0.384
N=1,122			

Table E.5
Model for Lawyer Work Hours: 1991 Sample, All General Civil Cases
with Issue Joined

Variable	Coefficient	T-Statistic	P-Value
Constant	2.880	7.319	0.000
Policy Variables			
Early management on case	0.152	2.988	0.003
Cases managed (district %)	-0.008	-1.781	0.075
Cases with trial schedule set early (district %)	-0.001	-0.323	0.747
Early disclosure district policy	0.030	0.275	0.783
Litigants at settlement conf. (district %)	-0.001	-0.250	0.802
Limits on interrogatories (district)	-0.066	-0.787	0.431
Days to discovery cutoff (district median)	0.001	1.554	0.120
Continuances (district %)	0.001	0.509	0.610
Magistrate judge activity (district mean)	0.063	0.442	0.659
Judicial control over trial (district % firm)	-0.004	-2.137	0.033
Management level on case	0.097	1.938	0.053
Disclosure on case	-0.237	-5.008	0.000
Control Variables			
Number of litigants (square root)	-0.997	-16.102	0.000
Class action: no mention	-0.864	-5.525	0.000
Case type has high avg. judicial work	0.183	3.588	0.000
Discovery motion	0.441	8.082	0.000
Any motion	0.384	5.963	0.000
Diversity jurisdiction	0.131	2.482	0.013
Any litigant without an attorney	-0.236	-3.406	0.001
Any pro se litigant	-0.315	-3.076	0.002
Case complexity (high)	1.029	10.551	0.000
Case complexity (moderate)	0.526	8.445	0.000
Zero stakes	-0.238	-3.068	0.002
Maximum stakes (log)	0.185	9.954	0.000
Missing fee	-0.012	-0.101	0.919
Contingent fee	0.123	0.942	0.346
Government attorney	-0.567	-6.176	0.000
Missing percent practice federal	0.232	1.112	0.266
Percent practice federal	0.009	8.431	0.000
Missing firm size	-0.117	-0.557	0.578
Firm size (square root)	0.031	4.384	0.000
Defense attorney	-0.130	-2.195	0.028
Contingent fee x case complexity (high)	-0.403	-2.362	0.018
Contingent fee x case complexity (mod.)	-0.178	-1.398	0.162
Contingent fee x percent practice federal	-0.003	-1.466	0.143
Contingent fee x firm size	0.060	2.563	0.010

N=2,120

Table E.6
Model for Lawyer Work Hours: 1991 Sample, All General Civil Cases
with Time to Disposition over 270 Days

Variable	Coefficient	T-Statistic	P-Value
Constant	3.110	6.786	0.000
Policy Variables			
Early management on case	0.076	1.328	0.184
Cases managed (district %)	-0.009	-1.664	0.096
Cases with trial schedule set early (district %)	0.000	0.228	0.820
Early disclosure district policy	0.095	0.730	0.466
Litigants at settlement conf. (district %)	0.001	0.278	0.781
Limits on interrogatories (district)	0.017	-0.178	0.859
Days to discovery cutoff (district median)	0.001	1.013	0.311
Continuances (district %)	0.005	1.890	0.059
Magistrate judge activity (district mean)	-0.023	-0.133	0.894
Judicial control over trial (district % firm)	-0.002	-0.982	0.326
Management level on case	-0.007	-0.116	0.908
Disclosure on case	-0.316	-5.703	0.000
Control Variables			
Number of litigants (square root)	0.979	-13.816	0.000
Class action: no mention	-0.772	-4.131	0.000
Case type has high avg. judicial work	0.163	2.793	0.005
Discovery motion	0.310	5.218	0.000
Any motion	0.189	2.177	0.030
Diversity jurisdiction	0.148	2.393	0.017
Any litigant without an attorney	-0.264	-3.115	0.002
Any pro se litigant	-0.354	-2.810	0.005
Case complexity (high)	0.977	9.606	0.000
Case complexity (moderate)	0.454	6.244	0.000
Zero stakes	-0.165	-1.824	0.068
Maximum stakes (log)	0.184	8.769	0.000
Missing fee	-0.115	-0.812	0.417
Contingent fee	-0.024	-0.154	0.877
Government attorney	0.523	5.186	0.000
Missing percent practice federal	0.415	2.073	0.038
Percent practice federal	0.009	7.896	0.000
Missing firm size	-0.107	-0.518	0.605
Firm size (square root)	0.031	3.804	0.000
Defense attorney	-0.243	-3.462	0.001
Contingent fee x case complexity (high)	-0.309	-1.635	0.102
Contingent fee x case complexity (mod.)	0.026	-0.175	0.861
Contingent fee x percent practice federal	-0.004	-1.483	0.138
Contingent fee x firm size	0.054	1.891	0.059
N=1,516			

- Related state case,
- Any government parties,
- Any litigant without an attorney,
- Any pro se litigants,
- Case complexity (high or moderate),
- Maximum stakes,
- Contingent fee,
- Government attorney,
- Percent practice federal,
- Firm size,
- Defense attorney, and
- Number of parties.

We also found that fee structure interacted with some of the other important predictors of lawyer work hours. Specifically the model includes an interaction between contingent fee and case complexity and contingent fee and firm size. It also contains an interaction between government attorney and case complexity and government attorney and stakes.² We explored the interaction of fee structure with all other variables and found that only the interactions just noted were important. The model also contain missing data flags for fee arrangement, percent federal court practice, firm size, and stakes. Note that there is no flag for missing case complexity because we had no missing data in this sample of lawyers (if they told us about work hours, they always told us about case complexity).

In the model for the all issue joined case sample, the following control variables remain statistically significant after controlling for both policy variables and the other controls:

- No mention of class action,
- Jurisdiction (diversity),
- Discovery motion,
- Any motion,
- State case,
- Case complexity (high or moderate),
- Maximum stakes,
- Percent practice federal,

²The implication of including these fee structure interactions in the model is that it is not necessary to build separate models for each different type of lawyer fee structure.

- Firm size,
- Defense attorney, and
- Number of parties.

The interpretation of the significant case complexity effects is that when the attorney is not a contingent fee or government attorney (e.g., is an hourly fee lawyer), then complex cases (high or moderate) tend to predict greater reported lawyer work hours. The interaction terms imply that for contingent fee attorneys, the effect of highly complex cases is even greater than for hourly fee attorneys but the effect of moderately complex cases is about the same as for hourly fee attorneys. The effect of complex cases is even greater for government attorneys. None of the interactions is statistically significant, but the estimated effects are large for some of them. Similarly, because there are interactions in the model, we should not interpret the lack of significance for the plain variables for contingent fee and government attorneys to mean that fee structure has no effect. Given the presence of large interactions, it is clear that there are some differences in work hours associated with fee structure. We note that since contingent fee lawyers do not keep work hour data for billing purposes, their responses may be less accurate than those of hourly fee lawyers and this may affect the findings discussed above.

For stakes we see that increasing stakes predicts increasing work hours. The increase is somewhat smaller for government attorneys, but the difference is not statistically significant. Attorneys from larger firms tend to put in more hours on their cases. There is a very small increase in these effects for contingent fee attorneys, but it is not statistically significant.

There appear to be some economies of scale, i.e., lawyer work hours per litigant decrease as the number of litigants increases. Similarly, cases that have no mention of class action require fewer hours and cases that have a related case in state court lead to fewer work hours. Also in our data we find that defense attorneys report fewer work hours per litigant than do plaintiff attorneys.

The model fit to our over 270 day sample yields very similar results for our control variables. In the over 270 day sample, we estimate similar effects for all significant control predictors and all remain statistically significant, except the no mention of class action variable, which is not significant in the over 270 day model.

Early Case Management

In both the 1992-93 models for lawyer work hours, we estimate statistically significant positive effects from early management. This implies that early management predicts more time spent by attorneys. The size of this effect in lawyer work hours is discussed later in this appendix. There are no consistent statistically significant differences for any of the components of early management, as shown in Table E.7. We also explored whether the status report or joint plan requirement had a significant effect for moderate or high complexity cases and found no statistically significant difference.

Table E.7
Estimated Effects of the Components of Early Management
on General Civil Litigation Cases

Variable	All with Issue Joined			TTD Over 270 Days		
	Coeff.	T-Stat.	P-Value	Coeff.	T-Stat.	P-Value
1992-93 Sample						
Set schedule	0.056	0.645	0.518	-0.187	-1.883	0.060
Set trial sched.	0.103	1.536	0.125	-0.073	-0.885	0.376
Held conference	0.057	0.902	0.367	-0.041	-0.515	0.607
Report or plan	0.016	0.219	0.826	-0.036	-0.443	0.658
Mand. arb. referral	-0.252	-2.030	0.042	-0.143	-0.660	0.509
1991 Sample						
Set schedule	0.235	2.909	0.004	0.022	0.225	0.822
Set trial sched.	-0.002	-0.035	0.972	-0.078	-1.099	0.272
Held conference	-0.036	-0.527	0.531	0.017	0.255	0.798
Report or plan	-0.036	-0.447	0.655	0.040	0.444	0.657
Mand. arb. referral	-0.265	-1.763	0.078	-0.379	-1.656	0.098

We find no statistically significant effect for the percentage of cases in the district that receive an early trial schedule, any management, or firm control over trial. We do, however, find a statistically significant negative effect for continuances in the over 270 day sample, where we find that attorneys from districts that allow more continuances per case with a schedule tend to have fewer work hours.

Discovery Control

Reported lawyer work hours increase as the number of district median days to discovery cutoff gets larger. This effect is statistically significant in both the all issue joined and the over 270 day models. The coefficient seems small, about 0.0035 for both models, but this is the effect of a change of a single day. The effect on lawyer work hours of decreasing median days to discovery cutoff is discussed later in this appendix.

A district policy on limiting interrogatories predicts shorter lawyer work hours; however, this difference is not statistically significant in either model for our 1992-93 sample.

Early Disclosure

Our 1992-93 models show no statistically significant difference in lawyer work hours between cases where the attorneys reported disclosure of relevant information and cases where there was no early disclosure. We also found that attorneys representing cases in districts with a *mandatory disclosure policy of any type* had work hours that were not statistically significantly different from hours worked by attorneys in other districts.

However, we found that attorney work hours are significantly lower for the three districts that have a policy of *early mandatory disclosure of information bearing on both sides of the dispute*. For our all issue joined sample, when we use the correlation ad-

justed standard errors we find that the t-statistic is -1.85 , which yields a p-value of 0.06 . This is a large effect but not significant at the $p=0.05$ level in the all issue joined sample. The effect is significant when using either standard error in the over 270 day sample. However, with only three districts using this particular policy, it is difficult to generalize this statistical finding.

We also found that a district policy encouraging voluntary early disclosure had no statistically significant effect on attorney work hours. For both our 1992–93 samples, we found small and not statistically significant differences in work hours between lawyers on cases from districts with a voluntary early disclosure policy compared to lawyers from districts with no general policy on early disclosure ($b=-0.006$, std. err. $=0.123$, $p=0.959$ for all issue joined cases; $b=0.236$, std. err. $=0.155$, $p=0.129$ for over 270 day case).

Good Faith Efforts in Resolving Discovery Disputes

As discussed in Appendix D, we explored the effects of good faith efforts in resolving discovery disputes before filing motions using only cases with at least one motion. We found no statistically significant effects of good faith efforts on work hours among attorneys from these cases ($b=0.20$, std. err. $=0.12$, $p=0.12$ for all issue joined cases, $b=0.27$, std. err. $=0.14$, $p=0.06$ for over 270 day cases). It could be that by restricting our attention to only cases with motions we miss the helpful effect of good faith effort avoiding motions; however, the positive effects we observe (i.e., good faith effort increases work hours, but not significantly) do not suggest any reduction in work hours from good faith motions.

Mandatory Arbitration Referral

In our over 270 day model for lawyer work hours, we found that early mandatory arbitration referral did not significantly reduce work hours compared to other forms of early management. This held true whether we considered all cases (see Table E.7) or only those cases that met arbitration eligibility requirements ($b=-0.04$, std. err. $=0.28$, $p=0.90$). For our all issue joined sample, we found that early mandatory arbitration referral tended to be associated with substantially fewer lawyer work hours than other forms of early management. The difference was statistically significant when we considered all cases (see Table E.7) but not when we used only arbitration eligible cases where we have a much smaller but more relevant subsample ($b=-0.27$, std. err. $=0.19$, $p=0.17$).

The story for work hours is similar to that for TID. There is some evidence of an effect in the all issue joined sample but not in the over 270 day sample. Hence, it could be that arbitration referrals have an effect on cases that close before 9 months. Because we only have a small sample of arbitration referrals in our data, we cannot determine if this is a real effect or not.

Joint Discovery/Case Management Plan Requirement

Our 1992–93 models show that attorneys who filed status reports or joint discovery/case management plans before day 180 in the case did not have significantly different work hours on the cases than attorneys on cases with other forms of early management. On the other hand, we found that attorneys from districts with a policy that required early status reports or joint plans did report statistically significantly fewer work hours than attorneys from other districts. This result holds when we use either the correlation adjusted or unadjusted standard errors. Only four districts required such plans or reports and our district level finding could be sensitive to the particular characteristics of these districts. Since the case-level variable was not significant, one should be cautious in interpreting the significant district-level variable.

Litigants at Settlement Conferences

We found no evidence in our 1992–93 data that the presence of litigants at settlement conferences has any effect on lawyer work hours. For both our models we estimated very small effects for the percentage of cases with litigants at settlement, and these effects were not statistically significant. Using judicial responses we found similar results.

Use of Magistrate Judges

We found that increased magistrate judge activity in the district does not affect the lawyer work hours for lawyers from the district. In both our 1992–93 models, the estimated coefficient for magistrate activity was small and not statistically significant.

Pilot vs. Comparison Districts

We again explored a model that included only our control variables and a flag for pilot districts. This model allows us to explore the full effects of the pilot program on lawyer work hours. For both the all issue joined sample and the over 270 day sample, we estimate very small statistically insignificant differences in lawyer work hour between the pilot and comparison districts ($b=-0.01$, $\text{std. err.}=0.05$, $p=0.84$ for all issue joined; $b=0.04$, $\text{std. err.}=0.07$, $p=0.57$ for over 270 days).

ESTIMATING LAWYER WORK HOURS USING 1991 SAMPLE DATA

Control Variables

For the 1991 sample, our model selection procedure identified the following as important control variables before accounting for the effects of policy variables:

- No mention of class action,
- Average judicial work level (high only),

- Jurisdiction (diversity),
- Discovery motion,
- Any motion,
- Any litigant without an attorney,
- Any pro se litigants,
- Case complexity (high or moderate),
- Maximum stakes,
- Contingent fee,
- Government attorney,
- Percent practice federal,
- Firm size,
- Defense attorney, and
- Number of parties.

We also found that fee structure interacted significantly with some of the other important predictors of lawyer work hours. Specifically, the effects of case complexity, percent practice federal, and firm size are different for contingent fee attorneys than for other attorneys. The model also contained missing data flags for fee arrangement, percent practice federal, firm size, and stakes. Note that there is no flag for missing case complexity because we had no missing data in this sample.

In the model for the all issue joined sample, all of our control variables remain statistically significant after controlling for both policy variables and the other controls. Even though the p-value for contingent fee is greater than 0.05, the presence of significant interactions between other variables and contingent fee implies that the contingent fee should be considered a significant predictor of lawyer work hours. The interpretation of the significant case complexity effect is that when the attorney is not working on a contingent fee, then complex cases (high or moderate) tend to require more lawyer work hours than other cases. The interaction terms imply that for contingent fee attorneys complex cases require more work hours, but the effect is smaller than for other attorneys. Similarly, for contingent fee attorneys, an increase in percent practice federal predicts less increase in log attorney work hours than for other attorneys. For contingent fee attorneys, the coefficient for the square root of firm size is larger than for other attorneys.

Increasing stakes predicts increasing work hours.

We find the same economies of scale that we found in the 1992–93 data, i.e., lawyer work hours per litigant decrease as the number of litigants increases. Similarly, the effect of class action, average judicial work level, jurisdiction, the presence of any motions or discovery motions, the presence of pro se or other unrepresented litigants and defense attorney all have the similar effects in our 1991 model as in our 1992–93

model. The actual magnitudes of the coefficients vary somewhat and are typically larger in the 1991 model, but the signs are the same.

The effect of government attorneys differs across the two sample years. This may be because the 1991 model does not include the government party flag, which could capture a portion of the government attorney effect in the 1992–93 model. (The government party flag was available only in our 1992–93 data set.)

The model fit to our over 270 day sample yields very similar results for our control variables. In the over 270 day sample, we estimate similar effects for the significant control variables and all remain statistically significant. The largest difference (relative to standard error) is for the coefficient for defense attorney. The magnitude of the estimated coefficient for the interaction terms is very similar across the models; however, because of the smaller sample size in the over 270 day sample, these terms are not significant in this model.

Early Case Management

The estimated effects of early management for our 1991 all issue joined model are similar to our estimates from the 1992–93 model. We estimate that early management significantly increases lawyer work hours and that there is little difference among the components of early management, although cases without schedules tend to add less work hours than do cases with schedules as shown in Table E.7. With our 1991 over 270 day sample, our estimate of the effect of early management is again that it increases lawyer work hours, but the magnitude of this effect is smaller than in the other models and the effect is not statistically significant. Again we find no statistically significant differences among the components of early management in Table E.7.

Our district-level predictors of management (percent of cases managed, percent of cases with a trial schedule, continuances and firm judicial control of trials) were not statistically significant predictors of lawyer work hours in our 1991 data. The one exception is that in the all issue joined sample, we found a significant effect for firm judicial control; a greater percentage of cases receiving firm control corresponded to fewer lawyer work hours. However, this effect is not significant if we use the correlation adjusted standard errors and the effect is not significant in the over 270 day sample.

Discovery Control

We did not find any significant effects of discovery control on lawyer work hours with our 1991 data. The effect of increasing district median days to discovery cutoff was positive for both the 1991 models but was not statistically significant. This is in contrast to our 1992–93 data, where we found statistically significant positive effects for discovery cutoff.

We also found no statistically significant effect for a district policy limiting interrogatories. The effect of this flag was not significant in either of our 1991 models, nor was it significant in our 1992–93 models.

Early Disclosure

We found that a district policy of early disclosure of relevant information was not a statistically significant predictor of lawyer work hours. This was also true with our 1992–93 models.

However, in both our 1991 models we find that early disclosure of information at the case level did predict fewer attorney work hours. That is, when at least one attorney on the case reported early disclosure, the number of work hours for attorneys on the cases was statistically significantly smaller than for other cases. This finding differs from that found using our 1992–93 data. As discussed in Appendix D, early disclosure was almost always voluntary for cases in our 1991 sample but was mandatory for some districts' cases in our 1992–93 sample. Thus, we might be observing the effects of selection in the 1991 sample. That is, in 1991 attorneys who truly voluntarily choose to disclose information (without being told that they must disclose or that they should volunteer to disclose) may be more willing to settle the case or less contentious in their litigation behavior, and it is this interest in settlement or less contentious behavior that reduces the work hours rather than the disclosure itself. We believe that the 1992–93 data provide a more accurate picture of the effects of a mandatory policy and do not believe one should generalize the results from the 1991 data.

Good Faith Efforts to Resolve Discovery Disputes

Using only cases with at least one discovery motion, we explored the effects of good faith efforts to resolve discovery disputes. We found no statistically significant effects for either of our 1991 samples. The limitations of this analysis, discussed for the 1992–93 analysis, should be considered when interpreting this result.

Mandatory Arbitration Referral

When compared to other forms of early management, we find no significant effects of early mandatory arbitration referral. For both our 1991 samples (all with issue joined and over 270 day cases), the effects are moderately large but are not statistically significant. As noted above, arbitration eligibility requirements could contribute to the observed difference and we estimated the effects of arbitration using a subsample of arbitration eligible cases. The estimated effects are small and not statistically significant ($b=-0.16$, $\text{std. err.}=0.19$, $p=0.60$ for all issue joined cases; $b=-0.02$, $\text{std. err.}=0.31$, $p=0.95$ for over 270 day cases). These results are similar, but weaker than our 1992–93 results. Together, the two years of data suggest that arbitration referrals might be less costly than other forms of early management, but our sample size is small for arbitration referrals and this needs to be investigated further using better data.

Joint Discovery/Case Management Plan Requirement

When compared to other forms of early management, cases that had a status report or joint plan submitted before day 180 did not have a significantly different effect on lawyer work hours.

Litigants at Settlement Conferences

Our results on litigants at settlement conferences are the same in our 1991 sample as in our 1992–93 sample. The percentage of cases in a district that have litigants at the settlement conferences is not a statistically significant predictor of lawyer work hours. This holds in both our all issue joined sample and our over 270 day sample.

Use of Magistrate Judges

Again our findings on magistrate judges from the 1991 sample are similar to our findings from the 1992–93 sample. There is no significant effect on lawyer work hours associated with changing the level of magistrate judge activity. Our estimated effects are similar across the two sample years and are small in both samples.

Pilot vs. Comparison Districts

Our analysis of lawyer hours with our 1991 sample also found no significant differences between pilot and comparison districts. Lawyer work hours were slightly lower in pilot districts in both samples but the effect was small and not statistically significant ($b=-0.07$, $\text{std. err.}=0.05$, $p=0.15$ for all issue joined; $b=-0.05$, $\text{std. err.}=0.05$, $p=0.35$ for over 270 days). This is consistent with our findings on TTD. There did not appear to be large differences between pilot and comparison districts before CJRA.

INTER- AND INTRA-DISTRICT EFFECTS

As discussed in Appendix D, because of large inter-district variations in the management of civil cases, one might hypothesize that our results overemphasize inter-district differences and yield less information about the within-district effects of policy. To explore this issue, we again fit models with district flags and no other district-level predictors. These models yield qualitatively similar estimates for all the case-level policy variables in our model. For example, the estimated effect for early management in our 1992–93 over 270 day model is 0.254 (as shown in Table E.4), our estimate using only intra-district effects is 0.252. Similarly small differences exist for all our parameter estimates. Hence, we can again conclude that our estimates reflect intra-district variations as well as differences between districts.

ESTIMATING EFFECTS IN HOURS

We fit our models for lawyer work hours on the natural log scale. Although this was appropriate for avoiding model misspecification and making proper statistical infer-

ence, it provides the estimated effect on the log scale rather than directly on the hours scale. To aid in the interpretation of these effects, we provide standardized differences in hours for three of our most important policy predictors: early management, setting a trial schedule early, and district median days to discovery cutoff.

Our standardized effect was constructed using the procedure discussed in Appendix D. For every attorney in the target sample we predicted the lawyer work hours under the assumption that the attorney's case received the management policy and we also estimated the lawyer work hours assuming that the case did not receive the policy—other district, case and attorney characteristics were held constant at their observed values. We then exponentiated these predictions to convert to hours, and calculated the difference between the two exponentiated predictions of hours. This provided us with a sample of differences and our standardized effect is the median of these differences in hours.

We repeated this process for the policies of early management, setting a trial schedule early and discovery cutoff. As with TTD, we also estimated a combined effect by estimating lawyer work hours for each attorney in the target sample assuming that the attorney's case received all three policies and then again assuming the case received none of the policies.

For estimating the effect of early management, median discovery cutoff, and our combined effect, the target sample was our entire over 270 day sample. For the effect of setting a trial schedule early, compared to other early management procedures, our target sample was all cases in our over 270 day sample that received early management. We do not estimate effect sizes for the all issue joined sample because the bias from very short cases that close before management can be applied makes this estimate for early management somewhat suspicious.

As shown in Table E.8, using this approach we estimate that 1992–93 cases that received early management required about 21 additional lawyer works hours than other cases. We also estimate that cases where a trial schedule was set early in the cases, required about seven less attorney work hours than did other cases with early management. Similarly we find that attorneys would be expected to work about 17 hours less in districts with a median of 120 days for discovery cutoff than in district with a median of 180 days of discovery cutoff. The effect of the three policies in combination tend to offset each other and we predict that cases with all three policies will not differ much from cases that receive none of the policies (a reduction of only one hour in lawyer work hours, with a 95 percent confidence interval on the combined estimate from –19 hours to +19 hours.) Table E.8 contains our estimated effects and confidence intervals for each of these effects. We used bootstrap methods to calculate these confidence intervals.³

Overall, the effects for 1991 tend to be smaller (closer to zero) and this might again be explained by the differences between using a closing cohort as compared to a filing

³Efron and Tibshirani (1993).

Table E.8
**Estimated Effect for Selected Policy Variables Measured in
 Lawyer Work Hours for the Over 270 Day Sample**

Variable	1992-93	1991
Primary Model		
Early management	21 (9,31)	6 (-3,15)
Set trial schedule early	-7 ^a (-21,8)	-7 ^a (-20,6)
Median days to discovery cutoff	-17 ^b (-31,-1)	4 ^b (-12,5)
Combined effects	-1 ^c (-19,19)	-2 ^c (-16,12)
Adjusting for Days to Answer		
Early management	20 (9,31)	7 (-3,15)
Set trial schedule early	-7 ^a (-21,7)	-6 ^a (-19,6)
Median days to discovery cutoff	-16 ^a (-32,-0)	-5 ^b (-13,4)
Combined effects	-1 ^d (-19,19)	-2 ^c (-16,13)

NOTE: Numbers shown in parentheses are 95 percent confidence intervals.

^aCompared to other forms of early management.

^bDistrict median discovery cutoff of 120 days compared a median cutoff of 180 days.

^cEffects of early management including setting a trial schedule and reducing district median discovery cutoff from 180 to 120 days from date schedule is set.

cohort. It may also be that changes in how these procedures are actually used, and changes in cases to which they are applied, has changed the effects somewhat. Also, some of the difference could be due to sampling variation. The caveats about interpreting the combined effect given in Appendix D apply here as well. The estimate of the combined effect relies on the assumed additivity of our model. However, it should be noted that our tests showed no significant statistical interactions among the three policies of early management, setting a trial schedule early, and median days to discovery cutoff. If we did include such interactions in our model we would estimate an even greater savings in lawyer work hours from the combination of the three policies. Hence, our estimates based on an additive model are conservative.

INTERPRETING EFFECTS AND GENERALIZING TO OTHER CASES, JUDGES, AND DISTRICTS

As discussed in Appendix D, because of the observational nature of our data, one should be cautious in interpreting our estimated effects as causal and one should not assume that changing management procedures will bring about precisely the estimated changes in lawyer work hours. We know that there are unobserved possible predictors of lawyer work hours and they may correlate with the use of management.

For example, one predictor that we know helps explain the use of early management is days to answer. We did not include it in our reported models for reasons discussed in Appendix D. However, to see if controlling for this predictor has a large effect on our estimated effect on lawyer work hours, we fit a model that included days to answer as a predictor of work hours. As shown in Table E.8, including days to answer had almost no effect on our estimated effects. Our estimates are invariant to including days to answer in the model, because unlike TTD that is highly correlated with days to answer, lawyer work hours and days to answer are nearly uncorrelated. This does not imply that there are no other unobserved predictors that we have not accounted for, but it does indicate that one predictor that was troublesome for TTD has almost no effect on lawyer work hours.

As discussed in Appendix D, judges who choose to implement policies and management procedures often do so at their discretion. These judges may differ from other judges in their basic approach to case management or because of personality. These differences between judges could affect the implementation of policy elsewhere and this could change the policy's effect. Similarly, districts that chose to implement policies and procedures do so because of the characteristics of the district and its judicial officers. Because policies were not assigned to cases at random in our data, we cannot fully untangle the relationship between district and judge characteristics and the use of policies. Hence, it is hard to determine exactly how the policies will affect lawyer work hours if implemented on a wider scale. We will probably see some change if the use of early management and reduced discovery cutoff is expanded to other districts and judges, but it is not clear if the magnitude of that change will match the observed effects in our data.

Finally, we stress that there is a difference between adopting a policy at the district level, and implementation in practice at the case level. For policy to have an effect on lawyer work hours, it must not just be adopted "on paper," but it must also be implemented in practice at the case level. Using our attorney-level data, we have estimated the effect conditional on a policy or procedure actually being implemented.

SUMMARY OF EFFECTS ON LAWYER WORK HOURS

Our models for lawyer work hours show that cases with early management tended to require greater work hours and cases from districts with shorter median discovery cutoff tend to require fewer hours. There were no other clearly consistent policy variable effects on lawyer work hours per party represented. Thus, of all the policy variables we investigated as possible predictors of reduced lawyer work hours, only judicial management of discovery seemed to produce the desired effect.

For general civil cases with issue joined that do not close within the first 9 months, we estimated in Appendix D that early management, setting the trial schedule early, and reducing discovery cutoff time have the combined effect of reducing median time to disposition by about four to five months in our 1992–93 sample. For lawyer work hours, we estimated that the effect of the three policies in combination tend to offset each other, and we predict that cases with all three policies will not differ

much from cases that receive none of the policies (a reduction of only one hour in lawyer work hours).

We found that several attorney and case characteristics were important predictors of lawyer work hours. These control variables tended to be far better at explaining variance in lawyer work hours than did the policy variables. For example, of the total variance explained by our model, about 95 percent was explained by the control variables. This means that lawyer work hours seem to be driven primarily by factors other than case management policy. Case stakes and case complexity are the most important predictors of lawyer work hours, and these two case characteristics alone explained about half of the variance in our models. In contrast, of the total variance in our time to disposition models, only about half was explained by the control variables and the other half was explained by the policy variables.

We found no significant difference between the pilot and the comparison districts in terms of lawyer work hours. The differences between attorney responses from the two sets of districts were very small in both years of data and were never statistically significant.

ANALYSIS OF SATISFACTION

INTRODUCTION

It is important to understand whether case management policies and procedures affect the participants' perceived satisfaction with case management and their sense of fairness or justice. Policies and procedures that have little effect on objective outcomes such as time to disposition or lawyer work hours might substantially improve subjective outcomes such as perceived satisfaction and sense of fairness. Hence one might wish to use these policies even if they do not affect litigation time and costs. Conversely, a procedure that reduces time to disposition or litigation costs might have such adverse effects on perceptions of fairness and satisfaction that one might not want to use the procedure.

To investigate such issues, we explore the effects of management policies and procedures on attorneys' satisfaction with case management and their opinions on the fairness of case management. As the professionals who have repeated contacts with the court system, and who guide disputing parties through the system, attorneys' views are important. And attorneys are in a good position to see any beneficial or adverse effects of changes in case management.

It also would be useful to determine the effects of policy on litigant satisfaction and views of fairness. However, because of the low response rate to our litigant survey as discussed in Appendix B, our litigant data cannot be assumed to provide accurate unbiased statistical estimates. We prefer to be cautious and believe that our litigant survey data should not be used for inferential statistical analyses. Hence, we will confine our presentation of these litigant data to descriptive tables in the main text of this report, and the information in those tables should be viewed as suggestive rather than statistically definitive.

Control and Policy Variables

Our study of satisfaction with case management used methods analogous to those used in our studies of time to disposition (TTD) and lawyer work hours in Appendices D and E. We explored the effects of specific case management policies and procedures by comparing the responses of attorneys from cases that were managed using a particular policy or procedure to the responses of attorneys from other

cases. We used attorney, case, and district variables in the analyses to control for variation other than case management. The policy variables considered in our models are the same policy variables considered for both TTD and lawyer work hours.

Multivariate Logistic Regression Analysis on Attorney-Level Data

To estimate the effects of case management policies and procedures on lawyer satisfaction, we used multivariate logistic regression models fit to attorney-level data. That is, we used data with one record per each responding attorney from our sample of general civil litigation cases with issue joined. As with lawyer work hours, using attorney-level data allowed us to control for possible attorney characteristics that could affect satisfaction and help explain the variability in the responses, yielding more precise estimates of policy effects. Using only case- or district-level data would not allow for such exact lawyer control variables and would provide less appealing estimates of the policy effects.

Because we modeled satisfaction as dichotomous response (1 or 0, where 1 is satisfied and 0 is not), it would have been inappropriate to use linear regression models, such as the models used for TTD and lawyer work hours, to model satisfaction. The appropriate model is a logistic regression model. Logistic regression models are analogous to linear regression models, but they account for the dichotomous nature of the outcome being studied. The logistic regression holds that the average response (proportion or probability of satisfied respondents) for a given set of predictors is described by the following model:

$$\Pr(Y_{ijk} = 1 | x_{ijk1}, x_{ijk2}, \dots, x_{ijkp}) = \frac{\exp(\beta_0 + \beta_1 x_{ijk1} + \beta_2 x_{ijk2} + \dots + \beta_p x_{ijkp})}{1 + \exp(\beta_0 + \beta_1 x_{ijk1} + \beta_2 x_{ijk2} + \dots + \beta_p x_{ijkp})} \quad (1)$$

where Y_{ijk} denotes the satisfaction of the k th attorney, from the j th case, from district i . x_{ijk} represents, for this attorney, the control and policy variables included in the models and β denotes the parameters of interest, i.e., the effects of the control and policy variables. This model can be transformed to a linear model

$$\ln p_{ijk} = \beta_0 + \beta_1 x_{ijk1} + \beta_2 x_{ijk2} + \dots + \beta_p x_{ijkp} \quad (2)$$

where $\ln p_{ijk} = \log\{\Pr(Y_{ijk} = 1)/(1 - \Pr(Y_{ijk} = 1))\}$. This transformed model can be fit using simple techniques analogous to fitting linear regression models.¹

As discussed in Appendices D and E, we fit separate models to the 1991 and 1992–93 samples to allow for difference between cohorts of terminated cases and cohorts of filed cases.

¹See Hosmer and Lemeshow (1989) for details on logistic regression.

The coefficients from our logistic regression model have a straightforward interpretation. The coefficient represents the log odds-ratio between cases with a policy and cases without, or the log odds-ratio of a single unit change for a predictor (e.g., a change of one day in the median days to discovery cutoff).

The odds are the ratio of the probability of attorney reporting satisfaction to the probability of attorney reporting dissatisfaction. That is, for a given set of predictor characteristics the odds are

$$\text{Odds} = \frac{\text{Pr}(\text{Satisfied} \mid \text{Characteristics})}{\text{Pr}(\text{Dissatisfied} \mid \text{Characteristics})}$$

The odds-ratio is the ratio of the odds for a case with a policy to the odds of a case without a policy,

$$\text{Odds-Ratio} = \frac{\frac{\text{Pr}(\text{Satisfied} \mid \text{Char. with Policy})}{\text{Pr}(\text{Dissatisfied} \mid \text{Char. with Policy})}}{\frac{\text{Pr}(\text{Satisfied} \mid \text{Char. without Policy})}{\text{Pr}(\text{Dissatisfied} \mid \text{Char. without Policy})}}$$

The odds are a standard measure of the relative probability. If the odds are 10-to-1 then an attorney on a case with the given set of characteristics is 10 times more likely to be satisfied than dissatisfied with management. The odds ratio tells us how much of an increase (or decrease) in the odds is associated with a given policy.

An odds-ratio of one implies that the policy has no effect; an odds-ratio of greater than one implies that the policy increases the odds (and the probability of satisfaction); and an odds-ratio of less than one implies that the policy decreases the odds (and the probability of satisfaction). We will interpret an odds-ratio that is close to one as having a small effect. Odds-ratios between 1.5 and 3 (or 2/3 and 1/3) we will consider moderate, and those greater than 3 (or less than 1/3) we will consider large.

The log odds-ratio is the natural logarithm of the odds-ratio. A log odds-ratio of 0 corresponds to an odds-ratio of one and implies no effect. Log odds-ratios around zero will be considered small. We will consider log odds-ratios in the range of about 0.41 to 1.10 (–0.41 to –1.10) as moderate and log odds-ratios of greater than 1.10 (less than –1.10) as large.

We report the log odds-ratio in our tables and the odds-ratio can be recovered by exponentiating the reported coefficient.²

Open Cases and Missing Data

As noted in Appendix D, about 8.5 percent of the general civil cases in our 1992–93 sample remained open at the conclusion of our data collection. From these cases we cannot obtain a measure of lawyer satisfaction that is comparable to our measure

²See Hosmer and Lemeshow (1989) for a full discussion on the log odds-ratio.

from closed cases. An open case has not, necessarily, received the full array of management procedures that it will receive before closure. For example, if the case is headed for trial, then any intermediate measures of satisfaction will not include the lawyers views on satisfaction with the trial. On the other hand, attorney responses from closed cases will reflect their assessment of all management policies and procedures applied to the case. For this reason we do not include data from open cases in our sample when fitting our satisfaction models. This places some limitations on interpreting our models, but it does provide us with a comparable measure for all sample cases being analyzed.

Because we are using attorney responses for this analysis, we are again missing reported satisfaction from about half the surveyed attorneys because the attorney did not respond to our survey. However, over 90 percent of responding attorneys provided us with their views on satisfaction. Table F.1 gives the number of responding attorneys who provided us with satisfaction data by district. Missing data could be problematic and as discussed previously in Appendices B and E, we use nonresponse weights to offset the possible biasing effects of nonresponse.

ATTORNEY SATISFACTION

We measured attorney satisfaction using Item 21 from our Attorney Questionnaire which is shown in Appendix J. The attorneys were asked to report their satisfaction with the "court management and procedures for this case" for their party or parties.

Table F.1
Attorneys Responding on General Civil Cases by District and Sample Year:
All Issue Joined and Time to Disposition Over 270 Day Samples

District	1992-93 Sample		1991 Sample	
	All Issue Joined	Over 270 Days	All Issue Joined	Over 270 Days
AZ	115	88	120	85
CA(C)	86	54	86	55
CA(S)	61	41	106	74
DE	148	93	194	157
FL(N)	93	60	141	120
GA(N)	131	82	188	126
IL(N)	91	58	101	64
IN(N)	120	77	147	110
KY(E)	145	117	123	97
KY(W)	119	85	160	130
MD	120	81	140	97
NY(E)	75	57	109	72
NY(S)	73	49	86	70
OK(W)	194	76	198	115
PA(E)	120	48	131	70
PA(M)	101	77	136	103
TN(W)	111	74	114	90
TX(S)	116	74	180	140
UT	124	88	163	138
WI(E)	137	94	130	102
Total	2,280	1,473	2,753	2,015

The survey item gave the attorney five response categories: (1) very satisfied; (2) somewhat satisfied; (3) neutral; (4) somewhat dissatisfied; and (5) very dissatisfied. Because we did not feel we could clearly interpret differences between the very satisfied and the somewhat satisfied attorneys we dichotomized the responses into reported satisfied (very or somewhat) or not reported satisfied (neutral or somewhat or very dissatisfied). To ensure ourselves that we did not lose too much information by using only the dichotomous outcome we ran some models using all five outcomes. The results from using all five outcomes were qualitatively very similar to our models with a dichotomous outcome and thus we report only the results from the latter models.

CONTROL AND POLICY VARIABLES

We used the same variable selection strategy for fitting our models for satisfaction that we used for modeling TTD and lawyer work hours. We started with a large pool of possibly relevant control variables and used backwards selection to select those included in our final model. We then fit a model with these control variables and our selected policy variables. We selected our policy variables using substantive justifications based on the case management tenets of CJRA.

Our pool of control variables included all the controls discussed in both Appendices D and E. We explored no additional controls. Also the policy variables we explored were the same as those explored in our models of TTD and lawyer work hours. The effects of each policy or procedure on satisfaction were explored.

For the same reasons that we discussed in Appendix E for not including TTD in the lawyer work hours models, we did not include TTD or lawyer work hours or other costs in our satisfaction models. That is, we excluded TTD and lawyer work hours because we wanted to estimate the total effect of policy on satisfaction. One of our primary motivations for studying satisfaction was to discover any negative effects on satisfaction of policies that had positive effects on other outcomes. Hence we wanted to find any effect that management had on satisfaction, even if the effect was indirect through effects on TTD or lawyer work hours.

ENDOGENEITY OF SATISFACTION AND POLICY VARIABLES

As discussed in Appendices D and E, case characteristics can affect the use of policy. Here again this might result in inaccurate estimates of the effects of policy on satisfaction. For example, if management has no effect on satisfaction, but attorneys tend to be neutral about the case management on very short cases, and if such short cases tend not to receive early management, then we might incorrectly conclude that early management has a positive effect on satisfaction.

In other words, as discussed previously in Appendices D and E, our goal is to estimate the effect on satisfaction of policy when applied or not applied to identical cases. Including very short cases in our sample, cases that did not receive case management because they closed before they could be managed, ensures that the cases

that did not receive management do not look like the cases that did receive management.

Hence we continued with our practice of modeling two subsamples of data from each sample year. We modeled all general litigation cases with issue joined and the subset of these cases with TTD greater than 270 days.

As discussed previously, TTD is not the only possible confounding factor between the outcome (satisfaction) and use of policy. We tried to limit the effect of such selection bias by including relevant control variables in our model and using district-level variables when the case-level variable is too sensitive to case characteristics (for example, discovery cutoff). These methods will limit the effect of selection bias but will probably not completely remove such bias from our result. Hence when interpreting our results one should use the caution appropriate for an observational study.

WEIGHTS, CLUSTERING, AND STANDARD ERRORS

Sampling and Nonresponse Weights

On the basis of the arguments presented in Appendices D and E, our analysis weights for fitting our models of attorney satisfaction included both the sampling weight and the nonresponse weight. As with our previous analyses, we used sampling weights that accounted for intra-but not inter-district over-sampling of cases.

Estimated Standard Errors

In Appendices D and E we noted that traditional estimates of the standard errors of the regression coefficients might be inappropriate for our models because we used sampling weights to fit the models and because of the presence of intra-case and intra-district correlation. This is also true for the estimated coefficients from our logistic regression models. We again used sandwich estimators of standard errors to account for weighting and adjust for correlation. Again for simplicity we report only the unadjusted standard error in our table and note in the text when statistically significant findings are sensitive to choice of standard error estimator.

MODELS FOR ATTORNEY SATISFACTION

Tables F.2 to F.5 give the results of our primary models for attorney satisfaction. Tables F.2 and F.3 give our models for our 1992–93 sample of filings and Tables F.4 and F.5 give our results for the 1991 sample of closures. For each year we have a model for all issue joined general civil cases and a model for cases with time to disposition greater than 270 days.

Table F.2
Model for Attorney Satisfaction: 1992–93 Sample,
All General Civil Cases with Issue Joined

Variable	Coefficient	T-Statistic	P-Value
Constant	2.037	2.695	0.007
Policy Variables			
Early management on case	0.238	2.004	0.045
Cases managed (district %)	-0.010	-1.021	0.307
Early disclosure district policy	-0.453	-2.902	0.004
Joint plan district policy	0.229	1.272	0.203
Litigants at settlement conf. (district %)	0.010	0.890	0.373
Limits on interrogatories (district)	0.086	0.558	0.577
Days to discovery cutoff (district median)	-0.001	-0.502	0.616
Continuances (district %)	-0.001	0.176	0.860
Magistrate judge activity (district mean)	1.141	3.615	0.000
Judicial control over trial (district % firm)	0.002	0.447	0.655
Disclosure	0.297	2.723	0.006
Control Variables			
Nature of suit (tort)	0.342	2.442	0.015
Nature of suit (contract)	0.109	0.841	0.401
Five or more motions	-0.168	-1.491	0.136
Any pro se litigants	0.827	3.252	0.001
Dispositive motions (district %)	0.004	0.650	0.516
Total filings per FTE judicial officer	-0.004	-4.047	0.000
N=2,280			

Table F.3
Model for Attorney Satisfaction: 1992–93 Sample, General Civil
Cases with Time to Disposition over 270 Days

Variable	Coefficient	T-Statistic	P-Value
Constant	2.094	2.206	0.027
Policy Variables			
Early management on case	-0.252	-1.590	0.112
Cases managed (district %)	-0.024	-1.961	0.050
Early disclosure district policy	-0.504	-2.479	0.013
Joint plan district policy	0.459	2.038	0.042
Litigants at settlement conf. (district %)	-0.018	-1.201	0.230
Limits on interrogatories (district)	0.487	2.509	0.012
Days to discovery cutoff (district median)	0.000	0.016	0.987
Continuances (district %)	0.001	0.078	0.938
Magistrate judge activity (district mean)	1.203	2.961	0.003
Judicial control over trial (district % firm)	0.007	1.329	0.184
Disclosure	0.493	3.554	0.000
Control Variables			
Nature of suit (tort)	0.364	2.110	0.035
Nature of suit (contract)	0.102	0.605	0.545
Five or more motions	-0.340	-2.292	0.022
Any pro se litigants	0.765	2.312	0.021
Dispositive motions (district %)	-0.005	-0.690	0.490
Total filings per FTE judicial officer	0.004	-2.573	0.010
N=1,473			

Table F.4

**Model for Attorney Satisfaction: 1991 Sample,
All General Civil Cases with Issue Joined**

Variable	Coefficient	T-Statistic	P-Value
Constant	1.529	2.225	0.026
Policy Variable			
Early management on case	0.062	0.627	0.531
Cases managed (district %)	0.006	0.659	0.510
Cases with trial schedule set early (district %)	0.010	2.466	0.014
Early disclosure district policy	0.553	-2.239	0.025
Litigants at settlement conf. (district %)	0.004	0.480	0.632
Limits on interrogatories (district)	-0.158	-0.960	0.337
Days to discovery cutoff (district median)	0.001	0.618	0.537
Continuances (district %)	0.001	0.135	0.892
Magistrate judge activity (district mean)	0.175	0.603	0.546
Judicial control over trial (district % firm)	-0.003	-0.690	0.490
Management level on case	0.867	8.953	0.000
Disclosure on case	0.227	2.398	0.016
Control Variable			
Nature of suit (tort)	0.463	3.832	0.000
Nature of suit (contract)	0.194	1.708	0.088
Any pro se litigants	0.600	2.841	0.005
Zero or missing stakes	0.286	1.995	0.046
Maximum stakes (log)	-0.091	-3.366	0.001
Total filings per FTE judicial officer	0.003	-2.275	0.023
N=2,753			

Table F.5

**Model for Attorney Satisfaction: 1991 Sample, General Civil Cases
with Time to Disposition over 270 Days**

Variable	Coefficient	T-Statistic	P-Value
Constant	1.309	1.634	0.102
Policy Variable			
Early management on case	0.095	0.825	0.410
Cases managed (district %)	0.019	1.721	0.085
Cases with trial schedule set early (district %)	0.005	1.190	0.234
Early disclosure district policy	-0.414	-1.381	0.167
Litigants at settlement conf. (district %)	0.006	0.622	0.534
Limits on interrogatories (district)	-0.000	-0.001	0.999
Days to discovery cutoff (district median)	-0.000	-0.193	0.847
Continuances (district %)	-0.003	-0.514	0.608
Magistrate judge activity (district mean)	0.265	0.788	0.431
Judicial control over trial (district % firm)	0.003	-0.651	0.515
Management level on case	0.803	6.976	0.000
Disclosure on case	0.292	2.609	0.009
Control Variable			
Nature of suit (tort)	0.442	3.150	0.002
Nature of suit (contract)	0.199	1.487	0.137
Any pro se litigants	0.408	1.719	0.086
Zero or missing stakes	0.331	1.917	0.055
Maximum stakes (log)	-0.075	-2.449	0.014
Total filings per FTE judicial officer	-0.004	-2.527	0.012
N=2,015			

ESTIMATING SATISFACTION USING 1992–93 SAMPLE DATA

Control Variables

For the 1992–93 sample, our model selection procedure identified the following as important control variables before accounting for the effects of policy variables:

- Nature of suit category (tort, contract),
- Five or more motions,
- Any pro se litigants,
- Percentage of cases within district with dispositive motions, and
- Total filings per FTE judicial officer.

See Appendix D, Table D.2, for a discussion of these control variables. This is a smaller number of controls than identified for our previous models for TTD and work hours and we suspect that there are fewer important control predictors because of a general lack of variability in satisfaction—overall 74 percent of responding attorneys on this set of cases report being satisfied with case management. We also suspect that there are fewer controls because we specifically asked the lawyers to evaluate satisfaction with respect to case management, not with respect to other factors.

For our all issue joined model, we find that only three variables (nature of suit category (tort), any pro se litigants, and total filings per FTE judicial officer) remain statistically significant after controlling for policy variables. We find that districts with a high number of filings per FTE judicial officer tend to have lower reported satisfaction than do other districts. We also find that attorneys report being more satisfied with tort cases and if at least one litigant was pro se. In our over 270 day model, we find that each of these same three predictors remains statistically significant after controlling for policy variables and that with longer cases five or more motions in the case becomes a statistically significant predictor of decreased attorney satisfaction.

Early Case Management

In our 1992–93 models for attorney satisfaction, we found no statistically significant effects for early case management in our model with over 270 day data. The estimated effect was negative and small. We did find a statistically significant positive effect for early management in our model for all cases with issue joined. However, as noted above, this could be sensitive to the inclusion of very short cases which receive little or no management and tend to report “neutral” rather than “satisfied” because of their limited exposure to case management. Hence we feel that there is no solid evidence of an effect of early management on attorney satisfaction.

Furthermore we find no statistically significant differences in attorney satisfaction for cases receiving any of the components of early management compared to cases not receiving the component. As shown in Table B.6, the estimated effects are all small except for early referral to arbitration in the over 270 day model, which is moderate but not significant. However, because we have only a small sample of arbitration re-

ferrals, this estimate is not precise and we cannot be sure that there is any real effect. When we explored the effect of arbitration referrals among a subsample of arbitration-eligible cases, we found similar results for both our 1992–93 samples but again we did not have a sufficiently large sample to conclude that the moderate effect for the over 270 day model is not a result of sampling variability ($b=0.162$, std. err.=0.464 $p=0.727$ for all issue joined; $b=0.504$, std. err.=0.755, $p=0.0504$ for over 270 days).

We also find no statistically significant effects for granting continuances or firm control over trials. We find that districts with a greater percentage of cases managed tend to have lower satisfaction. The effect is small in both models but statistically significant in our over 270 day model. However, in a separate model we explored the effect of management level as reported by the attorney and found that high management corresponded to significantly higher satisfaction in both our models ($b=1.03$, std. err.=0.11, $p=0.00$ for all issue joined; $b=0.95$, std. err.=0.14, $p=0.00$ for over 270 day). Because the percentage of cases managed in the district is a much less precise measure of management for a case, we believe that management level on this case as reported by the attorney is a better measure. Hence, we think that a higher management level for a case predicts increased attorney satisfaction. This is consistent with the findings from our 1991 models, discussed later in this appendix.

Discovery Control

We found no statistically significant relationship between the district median days to discovery cutoff and attorney satisfaction. We also found no statistically significant effect for a district policy limiting interrogatories for our all issue joined model. On the other hand, in our over 270 day data, attorneys from districts with a policy of limiting interrogatories report being more satisfied with case management.

Table F.6
Estimated Effects of the Components of Early Management
on General Civil Litigation Cases

Variable	All with Issue Joined			TTD Over 270 Days		
	Coeff.	T-Stat.	P-Value	Coeff.	T-Stat.	P-Value
1992–93 Sample						
Set schedule	0.294	1.644	0.100	0.064	0.276	0.782
Set trial schedule	0.121	0.818	0.413	-0.050	-0.276	0.782
Held conference	0.126	0.889	0.374	0.085	0.481	0.630
Report or plan	-0.085	-0.554	0.579	-0.082	-0.451	0.652
Mand. arb. referral	0.231	0.669	0.503	0.419	0.770	0.441
1991 Sample						
Set schedule	0.255	1.535	0.125	0.163	0.812	0.417
Set trial schedule	0.033	0.236	0.813	0.047	-0.291	0.771
Held conference	-0.049	-0.383	0.701	-0.090	-0.600	0.549
Report or plan	0.213	1.289	0.197	0.147	0.801	0.423
Mand. arb. referral	0.412	1.152	0.249	1.054	1.802	0.071

Early Disclosure

A district policy of mandatory early disclosure corresponded to statistically significantly lower reported attorney satisfaction. This result held for both models in 1992–93 and the effect was moderately large—an odds-ratio of about 0.60. However, for cases in which the attorneys report the actual early disclosure of information, they also report higher satisfaction than attorneys from other cases. Again this result is statistically significant in both models and moderately large in the over 270 day sample.

A district policy of voluntary early disclosure is associated with fewer satisfied attorneys, but our estimated effects are small and not statistically significant ($b=-0.271$, $\text{std. err.}=0.263$, $p=0.302$ for all issue joined cases; $b=-0.070$, $\text{std. err.}=0.339$, $p=0.835$ for over 270 day cases). Our model compared attorney responses from districts with a policy of voluntary early disclosure to the responses from attorneys from districts with no general policy on early disclosure.

Good Faith Efforts in Resolving Discovery Disputes

As discussed in Appendix D, we explored the effects of good faith efforts in resolving discovery disputes using only cases with at least one motion. We found that case-level reported good faith effort in resolving discovery disputes had no statistically significant effects on lawyer satisfaction in either of our samples. As noted in both Appendix D and E, using only cases with a discovery motion could bias our results, and this result should be interpreted cautiously.

Mandatory Arbitration Referral

We found moderate but not statistically significant differences in attorney satisfaction between cases with and cases without an early mandatory arbitration referral when compared to other forms of early management. This is true when we consider all cases or when we consider only arbitration-eligible cases. Because we have a small sample of arbitration referrals in our data, we cannot determine if this is a real effect or not.

Joint Discovery/Case Management Plan Requirement

Using our 1992–93 data, we found no differences, in terms of attorney satisfaction, between cases that required the attorneys to file an early status report or a joint plan and cases that received an alternative form of early management. Similarly, in our all issue joined model, we estimate only a small and insignificant effect of a district policy requiring the submission of joint plans or status reports. However, using our over 270 day sample, we estimated a moderate and statistically significant positive effect for such a district policy. That is, in cases lasting over 270 days, attorneys from districts with a policy of requiring joint plans tended to be more satisfied than attorneys from other districts. These findings are difficult to interpret because of the

inconsistency of results, but it does give another small indication that management of cases has, if anything, a positive effect on attorney satisfaction.

Litigants at Settlement Conferences

Both our 1992–93 models showed no statistically significant effect on satisfaction of increasing the percentage of cases in the district with litigants available in person or on the phone for settlement conferences.

Use of Magistrate Judges

In both our models for our 1992–93 data, we find that increased use of magistrate judges is statistically significantly associated with greater attorney satisfaction with case management.

Pilot vs. Comparison Districts

We find no statistically significant difference in reported attorney satisfaction between pilot and comparison districts in our 1992–93 data. For both our samples we fit a model with only our control variables and a pilot flag. In both models the estimated effect was small and not statistically significant ($b=-0.02$, $\text{std. err.}=0.11$, $p=0.85$ —all issue joined; $b=-0.04$, $\text{std. err.}=0.14$, $p=0.76$ for over 270 days).

ESTIMATING SATISFACTION USING 1991 SAMPLE DATA

Control Variables

For the 1991 sample, our model selection procedure identified the following as important control variables before accounting for the effects of policy variables:

- Nature of suit category (tort, contract),
- Any pro se litigants,
- Maximum stakes, and
- Total filings per FTE judicial officer.

See Appendix D, Table D.2, for a discussion of these control variables. Our model also contains a flag for missing or zero stakes. As we found with the 1992–93 data, this is a smaller number of controls than identified for our previous models for TTD and lawyer work hours, and we suspect that there are fewer important predictor control variables because of a general lack of variability in satisfaction—overall 72 percent of responding attorneys on this set of cases report being satisfied with case management. We also suspect that there are fewer controls because we specifically asked the lawyers to evaluate satisfaction with respect to case management, not with respect to other factors.

For our all issue joined model, we found that all the control variables except nature of suit category for contract cases remain statistically significant after controlling for policy variables. Our results are similar to the 1992–93 data. Attorneys from tort cases tend to be more likely to report satisfaction with case management than attorneys from other types of cases. We find that districts with a high number of filings per FTE judicial officer tend to have lower reported lawyer satisfaction than do other districts. Attorneys from cases with a pro se litigant tend to report greater satisfaction. Additionally, we find that in our 1991 sample, higher stakes are associated with lower satisfaction.

Early Case Management

Our 1991 data provide confirmation on our findings on early management from our 1992–93 data. We find no statistically significant effects of early case management on attorney satisfaction for either our all issue joined or our over 270 day samples from 1991. The estimated effects are very small and so are the effects for all the components of early management (see Table F.6), except early arbitration referral. We estimate moderate (all issue joined cases) or large differences in attorney satisfaction (over 270 day cases) between cases with an early mandatory arbitration referral and cases receiving other forms of early management, although these results are not statistically significant. However, because we have only a small sample of arbitration referrals, this estimate is not precise and we cannot be sure that there is any real effect.

We also find no statistically significant effects for the percentage of cases managed in the district or the percentage of cases with continuances in either of our models. We did find a contextual effect of percentage of cases with a trial schedule set before day 180. In the all issue joined model, we find that a greater percentage of cases with trial schedule set early in the case corresponds to greater reported lawyer satisfaction, even after controlling for early management at the case level. Although the effect remains positive for the over 270 day model, it is substantially smaller and no longer statistically significant. The effect was not significant in our 1992–93 data and was excluded from that model to improve model stability. Hence, overall our data provide little evidence of any large effects on satisfaction from setting trial schedules early in the case.

On the other hand, we found consistently strong, positive, and statistically significant effects for high or intense case management on lawyer satisfaction. Lawyers from cases with a reported high level of management report greater satisfaction in both our 1991 samples. This is consistent with our findings from 1992–93.

Discovery Control

We found in our 1991 sample that the median district's time to discovery cutoff had no significant effects on attorney satisfaction. Also, attorneys from districts with a policy on limiting interrogatories did not report statistically different levels of satisfaction than attorneys from other districts. These results are mostly consistent with

our findings from the 1992–93 data. However, in the 1992–93 over 270 day sample, we found that a district policy limiting interrogatories corresponded to greater reported satisfaction; given that that 1992–93 result on interrogatories did not replicate in any of our other samples, we feel it is best not to overinterpret the significance of the 1992–93 over 270 day sample finding.

Early Disclosure

Our findings on early disclosure in the 1991 data are also consistent with our findings from 1992–93. Lawyers from districts with a policy encouraging early disclosure are statistically significantly less likely to be satisfied with case management. This is true for both our 1991 models. However, attorneys from cases where they report early disclosure tend to report greater satisfaction with management and these effects are also statistically significant for both our 1991 models. These same results occurred with our 1992–93 data. The lower satisfaction with a district disclosure policy for all cases, but more positive satisfaction when disclosure occurs on their cases, is consistent with lawyers not liking blanket policy orders that apply to all cases, but liking the results of the policy on the cases for which early disclosure actually is made.

Good Faith Efforts to Resolve Discovery Disputes

As discussed previously, we used a subsample of cases with at least one discovery motion to estimate the effects on attorney satisfaction of good faith efforts to resolve discovery disputes. The estimated effects were small and not statistically significant ($b=0.226$, $\text{std. err.}=0.184$, $p=0.219$ for all issue joined; $b=0.167$, $\text{std. err.}=0.200$, $p=0.404$ for over 270 day cases). This is consistent with our findings for the 1992–93 data and the limitations of the model discussed there apply here also.

Mandatory Arbitration Referral

We estimated moderate to large positive differences in satisfaction between attorneys from cases with early mandatory arbitration referral and attorneys from other cases, although these results are not statistically significant. Recall that we found consistent results using our 1992–93 over 270 day data. Again, because we have a small sample of arbitration referrals in our data, we cannot determine if this is a real effect or not.

Joint Discovery/Case Management Plan Requirement

We found no statistically significant differences between reported attorney satisfaction from cases that required an early status report or joint plan than from other cases (see Table F.6). This is true in both our 1991 models and is consistent with our finding from the 1992–93 data.

Litigants at Settlement Conferences

Both our 1991 models showed no statistically significant effect on satisfaction of increasing the percentage of cases in the district with litigants available in person or on the phone for settlement conferences.

Use of Magistrate Judges

The use of magistrate judges at the district level predicted greater satisfaction in our 1991 data, but this was not statistically significant. This is in contrast to our 1992–93 data where magistrate judge activity was statistically significant and a strong predictor of greater satisfaction in both our models. There were, however, changes in some districts in the use of magistrate judges on civil cases between cases that closed in 1991 and cases that were filed 1992–93. For example, CA(S) greatly increased the role of magistrate judges in civil pretrial management in 1992–93. This shift over time in the role of magistrate judges could explain the difference in the significance in our findings of a positive effect from magistrate judge involvement.

Pilots vs. Comparison districts

We explored differences between reported attorney satisfaction between pilot and comparison districts using a model that contained only our control variables and a pilot flag. All other policy variables were dropped from the model. This is the same approach taken with our 1992–93 data and our explorations of TTD and work hours. Again we found no statistically significant differences between attorneys from pilot and comparison districts. The effects were small and insignificant in both our models ($b=-0.073$, $\text{std. err.}=0.093$, $p=0.434$ for all issue joined; $b=-0.087$, $\text{std. err.}=0.110$, $p=0.432$ for over 270 day cases).

INTER- AND INTRA-DISTRICT EFFECTS

As we did with our explorations of TTD and lawyer work hours, we fit a fixed district effect model to remove the inter-district differences from our estimated effects. We found that this had almost no effect on our estimated effects for the case-level policy variables. Hence we conclude that our estimates provide a meaningful measure of intra-district variation in attorney satisfaction and are not driven by inter-district differences in reported satisfaction.

INTERPRETING EFFECTS AND GENERALIZING TO OTHER CASES, JUDGES, AND DISTRICTS

As discussed previously in Appendices D and E, generalizing from our observational data to other cases, judges, or districts requires sensitivity to the difficulties of making causal inference from observational data. This study did not conduct an experiment that randomly assigned management policies and procedures to districts,

judges, or cases. Districts use discretion in choosing policies, and judges within districts use discretion to tailor the case management procedures they actually use.

We have attempted to use appropriate control variables to account for variation in attorney satisfaction and limit the possibilities of model misspecification through omitted predictor variables. For reasons explained in Appendix D, one predictor we left out of our reported models, which is known to affect the use of early management, is days to answer. However, to explore any bias that might be caused by ignoring the effect of days to answer on the use of early management, we did fit another model that included days to answer. We found only very minor differences between this other model and our reported models, and hence we are comfortable that leaving days to answer out of our reported models does not have a major effect. This due in part to the limited relationship between days to answer and satisfaction.

We did not identify other obvious missing predictors and conclude that among the cases and judges that used the management policies and procedures we explored, we have provided a reasonable estimate of the effects on satisfaction. This is not a guarantee that there are no missing predictors or selection bias, but we are confident that our results are meaningful.

However because the judges who used the policies did so at least in part at their own discretion, we must be concerned that other judges asked to use a policy or procedure might do so in a different manner. This could cause a different size effect than the one we have observed. To the extent that satisfaction is invariant to characteristics that differ between this study's judges and districts and other judges and districts, one can conclude that future results should be similar to those we have observed.

SUMMARY OF EFFECTS ON SATISFACTION

In our explorations of attorney satisfaction, we found that the policies that had the greatest effects on TTD and lawyer work hours—i.e., early management, median days to discovery cutoff, and setting a trial schedule early in the case—had no statistically significant effect on lawyer satisfaction.

We found that a higher degree of case management is associated with higher lawyer satisfaction. Similarly, a higher degree of involvement of magistrate judges in civil pretrial management is associated with higher lawyer satisfaction.

Attorneys from cases where early disclosure occurs report greater satisfaction. However, attorneys from districts with a policy of requiring early disclosure for all cases were less likely to report satisfaction with case management.

Also we again found no statistically significant differences between attorney satisfaction responses from the pilot and comparison districts. This is true for both samples from both sample years.

INTRODUCTION

In this appendix we discuss the effects of court management policies and procedures on attorney views of fairness for general civil cases with issue joined. Attorney views on fairness serve as a measure of perceived justice in the federal courts.

As the professionals who have repeated contacts with the court system, and who guide disputing parties through the system, attorneys' views are important. And attorneys are in a good position to see any beneficial or adverse effects of changes in case management on the fairness of the civil justice system.

It also would be useful to determine the effects of policy on litigant views of fairness. However, because of the low response rate to our litigant survey as discussed in Appendix B, our litigant data cannot be assumed to provide accurate unbiased statistical estimates. We prefer to be cautious and believe that our litigant survey data should not be used for inferential statistical analyses. Hence, we will confine our presentation of these litigant data to descriptive tables in the main text of this report, and the information in those tables should be viewed as suggestive rather than statistically definitive.

Control and Policy Variables

We used analogous methods to study views on the fairness of case management as we did in our study of satisfaction with case management in Appendix F. We explored the effects of specific case management policies and procedures by measuring their effect on the responses of attorneys from cases that received the management procedure compared to the responses of attorneys from other cases. We included attorney, case, and district level characteristics as control variables in our model so as to control for variation due to factors other than case management.

The policy variables considered in our fairness models are the same policy variables considered in all our previously discussed models in Appendices D through F.

Multivariate Logistic Regression Analysis on Attorney Level Data

We measured views on fairness as a dichotomous (0–1) outcome variable. As discussed later in this appendix, either the lawyer responded that the management was somewhat fair or very fair (outcome=1) or the lawyer viewed the management as somewhat unfair or very unfair (outcome=0). To explore the effects of policy and control variables on views of fairness, we fit multivariate logistic regression models using attorney level data. As discussed in Appendix F, multivariate logistic regression is the analog of linear regression for models of data with dichotomous outcomes. Also, as discussed in Appendix F, the use of attorney level data allows us to control for attorney-level characteristics and improves the precision of our estimates of the effects of policy on views of fairness.

Open Cases and Missing Data

As noted in Appendix D, about 8.5 percent of the cases in our 1992–93 sample of general litigation cases remained open at the end of our data collection. For these cases we cannot obtain a measure of fairness that is comparable to our measure from closed cases. For this reason we do not include data from such cases in our analysis of views on fairness. Excluding open cases could lead to bias in our resulting estimates of policy effects. However, because we have relatively few open cases we expect any bias to be small. Hence we feel it better to run our analysis on a set of comparable data than to mix measures from open and closed cases.

Not only are we missing data from the small fraction of the cases that are still open, but we are missing data from the approximately 50 percent of nonresponding attorneys on closed cases. Because nonrespondents and respondents who skipped our fairness item (approximately 10 percent of the respondents) provide no data on this outcome, these attorneys are also excluded from our analysis. Table G.1 gives the sample size for the analytic data set used in fairness analyses, by districts. As discussed in Appendix E, we use nonresponse weights to offset possible bias introduced by nonresponse.

ATTORNEY VIEWS ON FAIRNESS

We measured attorneys' views on fairness using Item 22 from our Attorney Questionnaire. The attorneys were asked how fair they thought the "court management and procedures" were for their cases for their parties. The survey item provided the attorney with four response categories: (1) very fair; (2) somewhat fair; (3) somewhat unfair; and (4) very unfair. Because we did not feel that we could clearly interpret differences between a response of very fair or somewhat fair (or very unfair and somewhat unfair), we dichotomized the response into fair (very or somewhat) and unfair (very or somewhat). Overall we found that over 90 percent of attorneys report that they viewed the case management as fair on general civil cases with issue joined.

Table G.1
Sample Size by District for Modeling Attorney Response, All Issue Joined
and Time to Disposition Over 270 Day Samples

District	1992-93 Sample		1991 Sample	
	All Issue Joined	Over 270 Days	All Issue Joined	Over 270 Days
AZ	113	88	113	79
CA(C)	82	52	84	55
CA(S)	58	39	99	71
DE	145	93	190	155
FL(N)	93	60	134	115
GA(N)	130	81	185	126
IL(N)	86	55	98	63
IN(N)	119	76	146	110
KY(E)	145	117	121	95
KY(W)	116	82	158	128
MD	111	76	137	95
NY(E)	75	57	109	73
NY(S)	73	49	86	70
OK(W)	193	76	195	112
PA(E)	116	45	125	68
PA(M)	99	75	134	101
TN(W)	109	74	110	89
TX(S)	113	73	180	140
UT	119	86	159	137
WI(E)	130	89	127	101
Total	2,225	1,443	2,690	1,983

CONTROL AND POLICY VARIABLES

We used same the model selection strategy for selecting control variables for our models of fairness that we previously discussed for our analyses of TTD, lawyer work hours and satisfaction. We started with the large pool of district, case and attorney control variables given in Tables D.2 and E.2 and used backwards selection to chose the controls for our final model of fairness. Our final model included the identified control variables and the set of policy variables given in Table D.3.

ENDOGENEITY OF VIEWS OF FAIRNESS AND POLICY VARIABLES

The problems associated with the case characteristics affecting the use of policy exist in this analysis, as they did for our analyses of TTD, lawyer work hours and satisfaction. We used the same methods of modeling cases with TTD over 270 days and using district level predictors when the case level predictor appeared overly sensitive to case characteristics. As noted before, these methods will reduce the effect of some forms of selection bias but will probably not remove all bias from our estimates and thus caution should be used when interpreting our results.

WEIGHTS, CLUSTERING AND STANDARD ERRORS

Sampling and Nonresponse Weights

As discussed previously in Appendices D through F, our analysis weights for fitting our models of attorney views on fairness included both the sampling weight and the nonresponse weight. As with our previous analyses, we used sampling weights that accounted for intra-but not inter-district over-sampling of cases.

Estimated Standard Errors

As discussed in Appendix F, because we are using sampling weights for fitting our models, it would be incorrect to use the traditional standard error estimates for the estimated coefficients of our logistic regression model. The traditional standard errors are derived under the incorrect assumption that our sampling weights are precision weights and therefore these traditional standard errors do not properly account for all the variance in our data. We again used sandwich estimators of standard errors to account for weighting and adjust for correlation. Again for simplicity we report only the unadjusted standard error in our table and note in the text when statistically significant findings are sensitive to choice of standard error estimator.

MODELS FOR ATTORNEY VIEWS OF FAIRNESS

Tables G.2 to G.5 contain our primary models for views on fairness. Tables G.2 and G.3 give our results for the 1992–93 data and Tables G.4 and G.5 contain the results for our 1991 data. For both years we fit a model using the data from all general litigation cases with issue joined (the all issue joined sample) and we fit another model using the subset of cases with time to disposition over 270 days.

ESTIMATING VIEWS ON FAIRNESS USING 1992–93 SAMPLE DATA

Control Variables

For our 1992–93 sample, our model selection procedure identified the following as important control variables before accounting for the effects of policy variables:

- Any motion,
- Discovery motion,
- Case complexity (high or moderate),
- Maximum stakes,
- Dispute began after filing date,
- Nonmonetary stakes, and
- Total filings per FTE judicial officer.

Table G.2
Model for Attorney Views on Fairness: 1992–93 Sample, All General Civil Cases with Issue Joined

Variable	Coefficient	T-Statistic	P-Value
Constant	6.543	4.598	0.000
Policy Variables			
Early management on case	-0.047	-0.220	0.826
Cases managed (district %)	-0.041	-2.828	0.005
Early disclosure district policy	-0.073	-0.316	0.752
Joint plan district policy	0.780	2.583	0.010
Litigants at settlement conf. (district %)	-0.017	-0.871	0.384
Limits on interrogatories (district)	0.055	0.211	0.833
Days to discovery cutoff (district median)	-0.001	-0.362	0.717
Continuances (district %)	0.021	2.116	0.034
Magistrate judge activity (district mean)	0.395	0.840	0.401
Judicial control over trial (district % firm)	0.004	0.566	0.572
Disclosure on case	0.143	0.785	0.433
Control Variables			
Any motion on case	-1.114	-3.020	0.003
Discovery motion on case	-0.251	-1.244	0.213
Case complexity (high)	-0.192	-0.613	0.540
Case complexity (moderate)	-0.092	-0.354	0.723
Zero or missing stakes on case	-0.011	-0.041	0.967
Maximum stakes on case (log)	-0.078	-1.354	0.176
Missing dispute began after filing	-0.424	-1.798	0.072
Dispute began after filing date	0.658	2.811	0.005
Nonmonetary stakes on case	-0.437	-2.390	0.017
Total filings per FTE judicial officer	-0.004	-2.430	0.015
N=2,225			

We also include a flag for missing or zero stakes and a flag for missing data for when the dispute started for this attorney's party or parties.

After controlling for policy variables we find that the presence of a motion, dispute began after filing date, presence of nonmonetary stakes and total filings per FTE judicial officer are statistically significant predictors of reported views of fairness. Of these only the "dispute began after filing date" variable predicts a greater probability of viewing management as fair. All the other significant control variables predict a decreased probability of viewing management as fair. The results are nearly identical for both our 1992–93 models.

Early Case Management

We find no statistically significant effects for early management in general on attorney views of fairness. This is true for both our 1992–93 models. We also find almost no statistically significant differences among the components of early management, as shown in Table G.6. The exception is early mandatory arbitration referral in only one of our models. We find that in our over 270 day model, an early referral to mandatory arbitration is associated with statistically significantly fewer attorneys reporting that case management was fair. The coefficient for arbitration is moderate in magnitude and negative for our all issue joined model, but it is not statistically significant.

Table G.3
Model for Attorney Views on Fairness: 1992–93 Sample, General Civil Cases
with Time to Disposition over 270 Days

Variable	Coefficient	T-Statistic	P-Value
Constant	5.573	3.061	0.002
Policy Variables			
Early management on case	-0.203	-0.812	0.417
Cases managed (district %)	-0.051	-2.629	0.009
Early disclosure district policy	-0.054	-0.188	0.851
Joint plan district policy	0.879	2.360	0.018
Litigants at settlement conf. (district %)	-0.005	-0.172	0.863
Limits on interrogatories (district)	0.249	0.797	0.425
Days to discovery cutoff (district median)	-0.001	-0.188	0.851
Continuances (district %)	0.027	2.236	0.025
Magistrate judge activity (district mean)	0.435	0.711	0.477
Judicial control over trial (district % firm)	0.008	0.853	0.394
Disclosure on case	0.318	1.463	0.144
Control Variables			
Any motion on case	-1.197	-2.341	0.019
Discovery motion on case	-0.306	-1.351	0.177
Case complexity (high)	-0.057	-0.160	0.873
Case complexity (moderate)	-0.033	-0.106	0.915
Zero or missing stakes on case	-0.048	-0.149	0.881
Maximum stakes on case (log)	-0.043	-0.629	0.530
Missing dispute began after filing	-0.509	-1.766	0.077
Dispute began after filing date	0.656	2.341	0.019
Nonmonetary stakes on case	-0.425	-1.911	0.056
Total filings per FTE judicial officer	-0.004	-1.903	0.057
N=1,443			

Table G.4
Model for Attorney Views on Fairness: 1991 Sample, All General Civil Cases
with Issue Joined

Variable	Coefficient	T-Statistic	P-Value
Constant	5.777	5.215	0.000
Policy Variables			
Early management on case	0.076	0.479	0.632
Cases managed (district %)	0.003	0.188	0.851
Cases with trial schedule set early (district %)	0.007	1.163	0.245
Early disclosure district policy	-0.453	-1.065	0.287
Litigants at settlement conf. (district %)	0.026	2.010	0.044
Limits on interrogatories (district)	0.014	0.053	0.958
Days to discovery cutoff (district median)	-0.001	-0.243	0.808
Continuances (district %)	0.001	0.221	0.825
Magistrate judge activity (district mean)	-0.956	-2.209	0.027
Judicial control over trial (district % firm)	-0.010	-1.298	0.194
Management level on case	0.404	2.655	0.008
Disclosure on case	0.275	1.859	0.063
Control Variables			
Any motion on case	-1.024	-3.199	0.001
Discovery motion on case	-0.613	-3.813	0.000
Any pro se litigants on case	0.929	2.312	0.021
Zero or missing stakes on case	-0.084	-0.380	0.704
Maximum stakes on case (log)	-0.109	-2.666	0.008
Total filings per FTE judicial officer	-0.004	-1.414	0.157
N=2,690			

Table G.5
Model for Attorney Views on Fairness: 1991 Sample, General Civil Cases
with Time to Disposition over 270 Days

Variable	Coefficient	T-Statistic	P-Value
Constant	5.681	4.148	0.000
Policy Variables			
Early management on case	0.011	0.062	0.950
Cases managed (district %)	0.005	0.294	0.769
Cases with trial schedule set early (district %)	0.005	0.690	0.490
Early disclosure district policy	-0.129	-0.275	0.783
Litigants at settlement conf. (district %)	0.018	1.288	0.198
Limits on interrogatories (district)	0.141	0.504	0.614
Days to discovery cutoff (district median)	0.001	0.459	0.646
Continuances (district %)	-0.003	-0.368	0.713
Magistrate judge activity (district mean)	-0.539	-1.129	0.259
Judicial control over trial (district % firm)	-0.004	-0.529	0.597
Management level on case	0.494	2.963	0.003
Disclosure on case	0.254	1.558	0.119
Control Variables			
Any motion on case	-0.813	-2.141	0.032
Discovery motion on case	-0.556	-3.154	0.002
Any pro se litigants on case	0.849	1.925	0.054
Zero or missing stakes on case	0.150	0.576	0.565
Maximum stakes on case (log)	-0.069	-1.561	0.119
Total filings per FTE judicial officer	-0.007	-2.511	0.012

N=1,983

We also found that the attorneys from districts with a higher percentage of cases with a continuance are statistically significantly more likely to report that case management was fair in the 1992–93 sample. This variable was not statistically significant in the 1991 sample models.

At the district level, the larger the percentage of cases in a district that received some management in 1992–93, the smaller the fraction of attorneys who reported that management of their case was fair. This district level variable was not statistically significant in the 1991 sample models, and the coefficients were positive rather than negative. At the case level in 1992–93, we find that higher levels of management (estimated in a separate model: $b=0.29$, $\text{std. err.}=0.22$, $p=0.18$ for all issue joined cases; $b=0.39$, 0.25 , $p=0.12$ for over 270 day cases) are both positively correlated with reports of fairer management. However, neither of these case level effects are statistically significant in 1992–93. At the case level in 1991, higher levels of management are significantly associated with higher views of fairness.

Given that we consider case level variables to be better measures than district level variables, and given the weak and inconsistent evidence about whether attorneys are more or less likely to report that management is fair when the case is managed (some positive, some negative), we do not believe that these findings should be interpreted that more case management is either more fair or less fair.

Table G.6
Estimated Effects of the Components of Early Management
on General Civil Litigation Cases

Variable	All with Issue Joined			TTD over 270 Days		
	Coeff.	T-Stat.	P-Value	Coeff.	T-Stat.	P-Value
1992-93 Sample						
Set schedule	0.227	0.771	0.441	0.397	1.193	0.233
Set trial schedule	-0.259	-1.035	0.300	-0.228	-0.798	0.425
Held conference	-0.153	-0.631	0.528	-0.001	-0.003	0.997
Report or plan	-0.190	-0.725	0.468	-0.410	-1.457	0.145
Mand. arb. referral	-0.812	-1.586	0.113	-1.493	-2.232	0.026
1991 Sample						
Set schedule	0.067	0.250	0.803	0.136	0.450	0.652
Set trial schedule	-0.165	-0.762	0.446	-0.089	-0.374	0.708
Held conference	-0.196	-0.996	0.319	-0.235	-1.064	0.287
Report or plan	0.523	1.967	0.049	0.535	1.860	0.063
Mand. arb. referral	1.253	1.939	0.052	2.197	2.030	0.042

Discovery Control

We found no statistically significant effects on reported fairness for a district policy of limiting interrogatories. The estimated effect was small and positive but not statistically significant in either of our 1992-93 models. Likewise, we estimated only very small and not statistically significant effects for district median days to discovery cutoff in both our 1992-93 models.

Early Disclosure

We found no statistically significant relationships between early disclosure and reported views on the fairness of case management. We found a small negative and not statistically significant effect for a district policy requiring mandatory early disclosure. However, we found a somewhat large, but still statistically insignificant, positive effect for attorneys from cases where early disclosure actually took place.

We found no statistically significant effects for a district policy encouraging voluntary early disclosure when we compared the views of attorneys from such districts to attorneys from districts with no general policy on early disclosure ($b=-0.507$, std. err.=0.444, $p=0.253$ for all issue joined cases; $b=-0.917$, std. err.=0.561, $p=0.091$ for over 270 day cases).

Good Faith Efforts in Resolving Discovery Disputes

We estimated the effects of good faith efforts in resolving discovery disputes using a subsample of cases that had at least one discovery motion. Using this sample we estimated a moderate but statistically insignificant effect for cases with one or more discovery motions ($b=0.47$, std. err.=0.33, $p=0.15$ for all issue joined cases; $b=0.45$, std. err.=0.38, $p=0.23$ for over 270 day cases).

Mandatory Arbitration Referral

As mentioned above, in our over 270 day sample we found negative statistically significant differences in reported views on fairness between cases with an early referral to mandatory arbitration and those receiving some other form of early case management. This result holds among all cases as well as only the arbitration eligible cases ($b=-1.36$, $\text{std. err.}=0.66$, $p=0.04$). We found negative but not statistically significant effects for our all issue joined sample in 1992–93. These effects are contradictory to the effects we observe in our 1991 data, and in both years we have only a small sample of cases referred to arbitration. Hence, we suggest that a more thorough study of arbitration be conducted before any final conclusions are drawn about the effects of arbitration on views of fairness. Also, one should remember that even for the arbitration referral cases, over 90 percent of responding attorneys report that case management was fair.

Joint Discovery/Case Management Plan Requirement

We found no statistically significant differences in terms of reported views of fairness between attorneys from cases that required any early status report or joint plan and cases that received other forms of early management. For both our models we estimated small negative effects. However, we found that attorneys from districts with a policy of requiring joint plans were more likely to report that they viewed the case management procedures as fair than were attorneys from other districts. The effects were large and statistically significant in both our all issue joined and over 270 day models. Because we have few districts and only four districts had a specific district policy requiring joint plans, we are somewhat skeptical about the generalizability of our finding on this district policy, especially given that we find no significant effect from using the policy at the case level.

Litigants at Settlement Conferences

Using our 1992–93 data we found no statistically significant effects on attorneys views of fairness from having litigants present at or available on the telephone for settlement conferences.

Use of Magistrate Judges

As described in Appendix D, we measured magistrate judge activity using the number of management procedures conducted in the district by magistrate judges per closed case in the district. We found no statistically significant relationship between this measure of magistrate judge activity and reported views of fairness. The coefficient for magistrate judge activity is small and not statistically significant in both our models for our 1992–93 data.

Pilot vs. Comparison Districts

We found that attorneys from our pilot districts were more likely to report that case management was fair than were attorneys from other districts ($b=0.373$, $\text{std. err.}=0.188$, $p=0.048$ for all issue joined cases; $b=0.342$, $\text{std. err.}=0.225$, $p=0.129$ for over 270 day cases). This finding is statistically significant for our all issue joined sample if we use an unadjusted standard error. However, if we adjust our standard error to account for intra-district correlation then the p -value is 0.068, which is not significant using a rigorous 0.05 level test. Moreover, this is a small effect (an odds-ratio of about 1.45), which corresponds to a difference of about 3 percentage points across the two sets of attorneys (93 percent fair for pilot vs. 90 percent for comparison districts). Given that we have observed only very small effects for management policies and procedures actually used, it is difficult to ascribe this small difference between pilot and comparison districts to the effects of the CJRA.

ESTIMATING VIEWS ON FAIRNESS USING 1991 SAMPLE DATA

Control Variables

For our 1992–93 sample, our model selection procedure identified the following as important control variables before accounting for the effects of policy variables:

- Any motion,
- Discovery motion,
- Any pro se litigants,
- Maximum stakes, and
- Total filings per FTE judicial officer.

We also include a flag for missing or zero stakes. After controlling for policy variables we find that the presence of a motion, and in particular a discovery motion, pro se litigants and maximum reported stakes are statistically significant predictors of reported views of fairness. Of these only the presence of pro se litigants predicts a greater probability of viewing management as fair. All the other significant control variables predict a decreased probability of viewing management as fair. The results are similar for both our 1992–93 models, except that the estimated effects tend to be smaller in magnitude (closer to zero) for our over 270 day model than for our all issue joined model.

Early Case Management

In both our 1991 models we estimated very small positive and statistically insignificant coefficients for early management. Also, except for early mandatory arbitration referral and early filing of a status report or joint plan, we find only small and not statistically significant differences among the various components of early management. However, attorneys from cases that were required to file a status report or joint plan prior to day 180 of the case were more likely to report that the case man-

agement was fair. This joint plan effect was moderate in both our models, although it was statistically significant only in the all issue joined sample in 1991. In our 1992–93 models, filing a status report or plan was not statistically significant and the coefficient had the opposite sign.

We estimated large increases in the probability of reporting fair case management for cases receiving an early referral to mandatory arbitration compared to cases receiving other forms of early management. The effect was statistically significant only in our model for our over 270 sample. The effects were also large but not significant when we considered only an arbitration eligible subsample of cases ($b=1.664$, $\text{std. err.}=1.200$, $p=0.166$ for all issue joined cases; $b=2.686$, $\text{std. err.}=1.585$, $p=0.090$ for over 270 day cases).

These 1991 findings on the effect of early management and its components on views of fairness are consistent with our 1992–93 findings in the sense that the early management variable is not statistically significant, and the components of early management are either not significant or not consistently significant in the different models. In contrast to our 1992–93 sample where we found insignificant negative effects for early management and usually negative effects for its components, for our 1991 sample we found insignificant positive effects for early management and mixed effects for its components. It seems doubtful that true causal effects would change direction from more fair to less fair between the two time periods. Hence, we conclude that our estimates for early management are truly noisy estimates of small differences that probably are not symptomatic of causal effects.

In contrast to our 1992–93 results, the estimated coefficient for percentage of cases managed by the district is small, positive and not statistically significant in either of our 1991 models. This provides confirmation for our previous conclusion that we have no strong evidence that more active case management has negative effects on views of fairness. Consistent with this finding, we observed no statistically significant effects for continuances or firm trial control in our 1991 models and we found that our case level predictor of intense or high management is a statistically significant predictor of more lawyers viewing case management as fair in 1991.

Discovery Control

We found no statistically significant effects for discovery control on views of fairness. The district median days to discovery cutoff did not affect reported views on fairness and neither did a district policy limiting interrogatories. These results are consistent with our findings from the 1992–93 data.

Early Disclosure

We also found no statistically significant effects for early disclosure on views of fairness using our 1991 data. We found that attorneys from cases where early disclosure occurred were more likely to report that management was fair. However, the difference was small and not significant in both our models. On the other hand, we found that attorneys from districts with a policy of requiring or encouraging early disclo-

sure were less likely to report fair management. Again these district policy effects were small and not statistically significant.

Considering both the satisfaction findings in Appendix E and the fairness findings here, it does appear that lawyers tend not to like a district policy of mandatory early disclosure. They were both less satisfied and less prone to call management fair when this district policy exists. However, they also tend to be positive, both in terms of satisfaction and fairness, about early disclosure when they actually participate in such disclosure on their case.

Good Faith Efforts to Resolve Discovery Disputes

We again explored the effects of good faith efforts to resolve discovery disputes using only cases with at least one discovery motion. Using this sample we found no significant effects on fairness due to good faith efforts ($b=0.202$, $\text{std. err.}=0.185$, $p=0.275$ for all issue joined cases; $b=0.203$, $\text{std. err.}=0.202$, $p=0.311$ for over 270 day cases). This is consistent with our findings from our 1992–93 sample data.

Mandatory Arbitration Referral

As discussed above, we found large and statistically significant positive effects for early mandatory arbitration referral when compared to other forms of early management. These effects are contradictory to the effects we observe in our 1992–93 data and in both years we have only a small sample of cases referred to arbitration. It seems doubtful that true causal effects would change direction from more fair to less fair between the two time periods, since the arbitration referral policy did not change and the same districts used the policy. Hence, we suggest that a more thorough study of arbitration be conducted before any final conclusions are drawn about the effects of arbitration on views of fairness.

Joint Discovery/Case Management Plan Requirement

As discussed above, we found moderate difference in the proportion of attorneys reporting that case management was fair between cases that required an early status report or joint plan and cases receiving other forms of early management. In both our 1991 samples more attorneys report fair management when the cases required early status reports or joint plans. The difference is significant for our all issue joined model. This is in contrast to our 1992–93 models, where the effects were small, negative, and not statistically significant.

Litigants at Settlement Conferences

We found a statistically significant positive effect on views of fairness for the district percentage of cases with litigants at, or available on the telephone for, settlement conferences. The greater the percentage of cases in the district with litigants available for the settlement conference, the more likely attorneys were to report fair management. This effect was statistically significant for the all issue joined model, but

was small and not significant for our over 270 day model. In our 1992–93 sample we found no significant effects from having litigants available for settlement conferences. Hence, we do not believe that there is strong evidence that requiring litigants at the settlement conference leads to improved views on fairness.

Use of Magistrate Judges

We found that in our 1991 sample a greater use of magistrate judges by the district was associated with a smaller likelihood that a responding attorney would say that management was fair. The effect was statistically significant for our all issue joined model but not significant for our over 270 day model. This is in contrast to our findings from the 1992–93 models, where we found positive but not significant effects for the use of magistrate judges. There were, however, changes in some districts in the use of magistrate judges on civil cases between cases that closed in 1991 and cases that were filed 1992–93. For example, CA(S) greatly increased the role of magistrate judges in civil pretrial management in 1992–93. This shift over time in the role of magistrate judges could explain the difference in the findings of a positive effect from magistrate judge involvement.

Pilot vs. Comparison Districts

In our 1991 sample we found only very small and not significant differences between pilot and comparison districts in terms of attorney views on fairness. The direction of these differences was not consistent across our two samples ($b=0.030$, $\text{std. err.}=0.148$, $p=0.837$ for all issue joined cases; $b=-0.058$, $\text{std. err.}=0.165$, $p=0.723$ for over 270 day cases).

INTER- AND INTRA-DISTRICT EFFECTS

Again we explored models with fixed district effects to remove the effects of inter-district variability from our estimated effects for early management and other case management procedures. We found that these models provided very similar estimates for the effects of case management procedures for our 1992–93 sample data. However, we found that our estimated effects for early management in our 1991 models were smaller when we included fixed district effects in the model. For the 1991 all at issue primary model, we estimated the effect of early management to be 0.076, while in our model with fixed district effects we estimate the effect to be 0.001. For our 1991 over 270 day primary model, we found an effect of early management of 0.011, while in our fixed district effects model we estimate the effect to be -0.50 . None of these effects are statistically significant. Hence the changes that result from including fixed district effects in our model should probably be interpreted as confirmation that the effects on fairness of early management is small and not significant in our 1991 sample and should not be considered important.

INTERPRETING EFFECTS AND GENERALIZING TO OTHER CASES, JUDGES, AND DISTRICTS

As we have thoroughly discussed in Appendices D, E, and F, generalizing from our observational data to other cases, judges, or districts requires sensitivity to the difficulties of making causal inference from observational data. The same caveats discussed there apply here also.

Most importantly, because the judges who used the policies did so at least in part at their own discretion, we must be concerned that other judges asked to use a policy or procedure might do so in a different manner. This could cause a different effect than we have observed.

SUMMARY OF EFFECTS ON VIEWS OF FAIRNESS

We found no consistent statistically significant effects of case management on attorney views of fairness. A very high percentage of attorneys report that case management was fair; 93 percent in 1992–93 and 91 percent in 1991. Hence there is little variability in our data and it is not surprising that we cannot find strong or consistent significant effects of case management on attorney views of fairness.

We found small but not statistically significant differences between pilot and comparison districts in terms of attorney views on fairness.

ANALYSIS OF TIME EXPENDED BY JUDICIAL OFFICERS

INTRODUCTION

One concern regarding the implementation of new policies and procedures for judicial case management is that while lawyers and litigants may see benefits such as decreases in total case processing time, those benefits may come with the cost of an increase in the time spent by judicial officers. With judicial time being spent on early active case management, referring cases to alternative dispute resolution programs, and presiding over discovery limitation arguments, for example, it is presumed by some that there would be a commensurate rise in the amount of total time devoted to the case by the judicial officer. On the other hand, proponents of judicial case management argue that any extra time spent managing the case early in its life can be offset by less time required later because the case closes earlier.

To see if the judicial case management principles and techniques of the Civil Justice Reform Act increased the amount of judicial time spent on cases, we conducted a “judicial time study” on the cases in our samples of 1992–93 civil and criminal filings, and compared the results with the judicial time study conducted by the Federal Judicial Center in the late 1980s. Essentially, this is a type of survey in which judicial officers are asked to fill out “time sheets” that indicate the time spent on each case by each judicial officer, whether in or out of a courtroom. The civil and criminal time study forms are attached at the end of this appendix.

This research tool has long been used by the Federal Judicial Center to estimate relative “case weights.” By using the average judicial time reported by case type, the raw number of cases of each type filed in each district can be adjusted to a “weighted” number of cases to account for differences in the mix of types of cases between districts. Then this total “weighted” judicial caseload is used to help make decisions on the allocation of authorized judgeships to each district.

In this CJRA study, we use the same questionnaire forms as the FJC used, and thus have comparable judicial work time data before and after implementation of the pilot program.

DATA COLLECTION METHODS

The 1992–93 CJRA judicial time study was identical to that conducted by the FJC in the late 1980s (hereinafter referred to as the 1989 FJC study), with the exception of how the sample of cases was selected. The questionnaire forms were the same for both time studies; the instructions to the judicial officers were the same; the completed forms were mailed back to the same person at the FJC; the data were cleaned and entered into a computer file in the same way by the FJC; and then the computer files for both the 1989 FJC and the 1992–93 CJRA judicial time studies were sent to RAND for analysis.¹

The 1992–93 CJRA sample of cases was selected as described in Appendix A, with an approximately equal number of cases in each district. The 1989 FJC sample of cases was primarily selected by taking all cases filed within a window of time, and hence the number of cases selected in each district was approximately proportional to the total volume of cases filed in each district. We adjust for differences in how the samples were selected in our analyses in this appendix, as discussed later.

All time spent by a judge or magistrate judge on each case in the 1992–93 sample was to be recorded, regardless of whether it took place on the bench, in chambers, or in any other location. Support staff time (such as law clerks, courtroom deputies, or secretaries), though an invaluable component of the total available resources a district uses to process cases, was not recorded. Neither was time spent by special masters, mediators, arbitrators and the like if they were not a judge or magistrate judge. Time spent by senior or visiting judges was to be included; however, some senior judges asked to be exempted from the reporting requirements since their participation was not mandatory in the prior FJC judicial time study. Both the 1989 and the 1992–93 time studies were conducted in the same way.

When a case was selected as part of the samples of 1992–93 filings, it was “flagged” so that judicial officers and their staff would be reminded to record all time spent on these cases. The reminders included marking each document, brief, order, or pleading—including the initial complaint—with a special stamp or a sticker (regardless of whether filed in the courtroom or at the counter or produced in chambers), prominently marking the case files held in the Clerk’s Office and in chambers (or using a different colored folder for them), and setting a “flag” in the district’s electronic database of cases (ICMS) that would alert those making a computer inquiry.

Each time study form provided space for recording either the beginning and end or total time expended for particular events in a case and for using a set of 16 codes that would indicate a broadly defined subject matter category for the task (such as continuances, discovery matters, or jury trials). Often, a supply of the forms used to record the time spent was placed in the case file for easy access by the judicial officer or the personnel he or she designated to perform the task (sometimes a staff person

¹To preserve the confidentiality guarantees made in the 1989 FJC time study, the version of those data provided to RAND had the case characteristics and judicial time reported, but did not have the docket numbers or judicial officer codes attached.

was given the responsibility to insure that an accurate report of the judicial officer's time was being made). Again, the intent was to have the 1992–93 time study conducted in the same way as the 1989 FJC time study.

ANALYSIS ISSUES

Before and After Comparison

We needed to be able to compare time spent per case by judicial officers for both civil and criminal cases before and after implementation of the CJRA pilot program. Since a judicial time study cannot be conducted accurately after a case is terminated, and since we did not start this study until just before the pilot program was implemented, we could not do a separate time study on a sample of cases managed before CJRA. Fortunately, the FJC had just done a judicial time study in the late 1980s (they do one about once a decade) and it could be used for comparison if we adjusted for differences in the sample selection methods. This was done by using the same districts for the before and after comparison; by adjusting for differences in the mix of case types between the 1989 and the 1992–93 samples; and by adjusting for differences in the volume of cases in each district in each of the two samples.

We were fortunate to have the original data, minus case and judge identifiers, from the FJC's 1989 Time Study for comparison with our own. This was necessary since the FJC's published results (the case weights) were at too aggregate a level for us to make the needed adjustments to account for differences in the samples. Additionally, the FJC uses only the time reports from Article III judges for their weights, while we wished to include the contribution of magistrate judges in our analysis.

Nonresponse to Time Study

As with any survey, the problem of nonresponse exists. This is true for both the 1989 FJC and the 1992–93 CJRA time studies.

We attempted to minimize underreporting and nonresponse by sending a representative to each of the study districts to meet with personnel from the Clerk's Office and with interested judicial officers to explain both the procedures involved and the importance of accurate time keeping. The FJC had done a similar visit in their 1989 time study.

Nevertheless, comparisons of time study data with events reported in the dockets and with trial time reports (from a separate form submitted on each trial) led us to the conclusion that in a number of cases, the judicial officers had spent time but a time sheet was not submitted. This is not surprising in light of the fact that some judges and magistrate judges view this sort of survey (or any survey for that matter) as an intrusion into their already busy work day. Also, there may have been an inadvertent breakdown in the procedures for identifying some of the cases being currently worked upon as part of the sample (e.g., the outside of the case folder was not stamped, no flag was set in the computer). And finally, in criminal cases judges often

spend time reviewing pre-sentencing reports. Those pre-sentencing reports do not necessarily flow through the clerk and hence the time spent on them may not all be reported.

Ideally, cases on which judicial officers spent time but did not report it could be identified and analyzed separately. However, interviews with district officials indicated that there was no definitive way to know from each docket that no judicial time had been spent. Examinations of the dockets are not always conclusive as some "orders" are actually generated by courtroom or chambers staff (without the involvement of any judicial officer) and cases with no events docketed that appear to involve a judge or magistrate judge may well have had a significant amount of out-of-courtroom time expended to review papers or pleadings. Even if these problem cases could be identified, we would still not be able to get accurate time records since it would require the judicial officer and/or his or her staff to estimate the time spent on an event that happened many weeks or even months earlier.

The 1989 FJC time study had time reports on 81 percent of the cases in their sample, while the 1992–93 CJRA time study had time reports on 67 percent of the cases in the sample. Some cases clearly did not require and did not receive judicial time, but some other cases that did receive judicial time had no time reported on the "time sheets." So the number of cases without time reports in both studies is a mixture of true zeros and nonresponses. Given that we know that more cases were judicially managed earlier in 1992–93 than in 1991, we strongly suspect that the number of true zeros in the 1992–93 CJRA time study should be less than the number of true zeros in the 1989 FJC time study. Since the opposite is true in the responses received, one is led to suspect that judicial officers were more likely to respond to the FJC study (perhaps because they knew it was being used to help determine the number of authorized judgeships) than to the CJRA time study (perhaps because they perceived it as having less effect or importance than the FJC study).

Given that our primary purpose here was to determine whether the amount of judicial officer time per case had changed since the implementation of the CJRA, we felt that the problem of nonresponse, and differential nonresponse between the two different time studies, could be minimized by focusing on those cases with at least one time study form submitted. If the judicial officer cooperated by sending in at least one form for a case, then the procedures for time study data submission were in place for the case, and the judicial officer and his or her staff were cooperating, at least in the beginning. There could certainly be an increase in indifference to filling out time records as additional calendar time elapses from the start of the sampling selection or the proper flagging procedures might fall by the wayside. Nevertheless, using only cases with at least one record submitted is a good way to avoid cases where no time records were kept at all. The explicit assumption we make is that if a judicial officer started to cooperate on a case by sending in at least one report, then any future underreporting on the same case would be the same in both the FJC and the CJRA time studies. As evidence that this assumption is reasonable, we note that the FJC study averaged 4.7 time study reports per case with any reports, which the CJRA study averaged 4.3 such reports up through December 1995 when data collection stopped and 7 percent of the cases were still open. If we were to follow the CJRA

cases until they were 100 percent closed, we expect the 4.3 CJRA reports per case with any reports would approach the 4.7 reports per case in the FJC study.

Exclusion of Certain Districts

Unfortunately, we were unable to use all 20 districts in our sample for our comparisons with the 1989 FJC study.

For the civil cases, we used 19 districts and excluded CA(S) because it was a “pre-test” district with very few cases in 1989 and was not a part of the main FJC data collection effort. Hence, we had essentially no 1989 data from CA(S) for comparison.

For criminal cases, we used 16 districts, and excluded CA(S) for the same reason as noted above. We also saw severe problems with the data from CA(C), NY(S), and TN(W) and deleted them from the analysis due to problems with implementation of the criminal time study in those three districts.

We discovered the problem with the criminal time study in those three districts when we reviewed the ratio of cases with any time study reports submitted to the number of cases in our sample on a district-by-district basis. The ratio for the 1992–93 study was then compared to that for the 1989 study. On the civil side of the sample, all the districts had a 1992–93 ratio similar to that observed in the 1989 FJC data. In the criminal samples, however, the three districts noted above had a large drop of more than 50 percent in the ratios of criminal defendants with judicial time data to all defendants in the sample from 1989 to 1992–93. Further examination revealed that these same three districts had less than half as many criminal time reports per case with any reports in 1992–93 than they had in 1989. This lead us to surmise that, for the criminal time study only, there was a much greater-than-average difficulty in the implementation of the time study in those districts either in the flagging of the cases, in the recording of the time expended, or in the degree of cooperation by the judicial officers involved. We note that the criminal time study did not begin in most districts until many months had passed after the initial visits by RAND personnel, and that criminal cases often have their own filing and docketing clerks and procedures, separate from those for civil cases. Whatever the reason, the criminal data from these three districts were clearly very incomplete and we elected to not include these three districts in our calculations for criminal cases.

Adjusting for Differences in Case Mix Between the Two Judicial Time Studies

The 1989 FJC sample of cases was primarily selected by taking all cases filed within a window of time, and hence the number of cases selected in each district was approximately proportional to the total volume of cases and the mix of cases filed in each district during a time period in the late 1980s (the actual window of time used varied from district to district in the FJC study). The 1992–93 CJRA sample of cases was selected as described in Appendix A, with an approximately equal number of cases in each district. The CJRA sample was a stratified random sample, so the stratified

sample weights discussed in Appendix A were used to adjust the CJRA data to be representative of the volume and mix of cases filed in each district in 1992–93. This adjustment for the difference in how the samples were drawn makes the data as comparable as possible, except for differences that exist in the mix of cases that were filed in the late 1980s vs. the 1992–93 time period. Differences in the mix of cases can then be adjusted for by using the reported time for each type of case in the two different time studies, and the same volume of each type of case. Either the 1989 case mix or the 1992–93 case mix can be used, and we discuss the results later in this appendix.

Time by Personnel Other Than Judicial Officers

Any time study taking into consideration only activity by judges and magistrate judges provides an incomplete picture of how cases consume district resources. Given that law clerks, courtroom deputies, and others are handling some aspects of case management, a better method might be to track the time expended by *all* staff on a particular case or case type. A good example of this would be pro se prisoner litigation. A substantial amount of district pro se law clerk time is devoted to minimizing the total effort needed from judges and magistrate judges. Focusing only on judicial officer time for these and similar cases provides a skewed picture of the total drain on district resources, staff, as well as judges and magistrate judges. Since neither the FJC nor the CJRA study considered time spent by staff other than judicial officers, we cannot draw any conclusions about changes in the workload on court staff.

A Caution on Proper Use of Reported Time per Case

It is critical to remember that reported judicial officer times per case are shown here only for the purpose of comparing reported time spent on cases filed in 1989 to reported time spent on cases filed in 1992–93. This is a valid use of the data to the extent that any underreporting of data on cases with time reports is similar in the two time studies, as discussed above.

However, it is important to remember that these reported time data do *not* represent the actual total time spent on *all* cases by *all* judicial officers because (1) some cases with judicial time spent did not have any time reports submitted; and (2) some cases with time reports submitted did not have *all* time reported. Our interviews indicate that there was some flagging of interest in filing out time sheets as both time studies progressed, and analysis of trial time reports from separate sources indicates some underreporting. While we and the FJC both adjusted for known underreporting of trial time, we are confident that the final time study numbers still are underestimates from underreporting.

Thus, it would be misleading to use the figures displayed below to estimate total judicial officer time on all cases combined. It would also be misleading to use case-related work time as a measure of total judicial officer work time, since some judicial work is not related to specific cases (e.g., time spent managing the court as a whole, and time spent keeping abreast of developments in the legal field).

Likewise, it would be misleading to view these numbers as representative of the average civil case which has reached the stage of issue joinder. Many cases close without issue being joined, and our numbers include those cases if the judicial officer spent even a few minutes on the case.

JUDICIAL OFFICER REPORTED TIME PER CIVIL CASE

There was almost no difference in the time spent by judicial officers per civil case in 1992–93 when compared to 1989. As Table II.1 shows, the difference in the median time per civil case was only one minute and the difference in the mean was only six minutes (191 minutes per case in 1989, and 185 minutes per case in 1992–93). Since 7 percent of the 1992–93 cases were still open in December 1995 when we stopped data collection, the six-minute difference in the mean could be even less if we knew the remaining unreported time on those open civil cases.²

The above comparisons use the actual mix of cases in each sample. If we use the reported average times for each sample and the *same* mix of cases in the comparison between 1989 and 1992–93, the findings are essentially the same. Using the 1989 mix of cases results in a one-minute difference instead of a six-minute difference in the mean. Using the 1992–93 mix of cases results in a ten-minute difference instead of a six-minute difference in the mean. In each comparison the CJRA number is slightly smaller than the FJC number, and the CJRA number still has some unreported time from the 7 percent of the cases that were still open at the end of the data collection.

While the 1989 FJC and 1992–93 CJRA medians and 75th percentiles are about the same in pilot and comparison districts, it does appear that the reported mean time rose slightly for pilot districts (by 12 minutes) and dropped in comparison districts (by 17 minutes). Given the stability of the medians and 75th percentiles, we feel that the changes in the means are due to random differences in the number of large judicial time cases, and probably do not indicate a meaningful trend.

Table II.1
1989 FJC and 1992–93 CJRA Civil Time Studies: Total Minutes Reported
in Cases with One or More Time Study Reports—All Judicial Officers,
All Origins, 19 Reporting Districts

District Type	Number of Cases		Proportion of Cases with Time Reports				Median Reported		75th Percentile Reported	
	1992-93	1989	1992-93	1989	1992-93	1989	1992-93	1989	1992-93	1989
Pilot	1,481	1,099	0.62	0.74	212	200	36	33	125	135
Comparison	1,863	1,251	0.72	0.87	165	182	36	35	124	135
Total	3,344	2,350	0.67	0.81	185	191	36	35	124	135

NOTES: CJRA time study data weighted to BY 1993 filing levels. 1992-93 weighting based on case type distribution, by district. 1989 FJC time study data are unweighted. GMSJ excluded because it was a pre-test district in the 1989 study.

²Note that we have time reports on the 7 percent open cases through December 1995, so only the remaining judge time spent on those cases after December 1995 is still unreported.

JUDICIAL OFFICER REPORTED TIME PER CRIMINAL DEFENDANT¹

While the CJRA did not address the criminal caseload directly, changes in the criminal workload in districts might well affect the amount of judicial resources available for handling civil matters. As a result, it is important to examine the available time study data to see if there have been any changes in judicial time expended per criminal defendant since 1989.

Many examinations of federal district court criminal case processing focus primarily on felony matters. This is partly because such filings tend to dominate the total criminal caseload (about three-quarters) and because the potential sentences involved are more severe, the issues and procedures more complex, and the amount of judicial time greater than for misdemeanors and other lesser offenses. We present information for all defendants, and for felony defendants only.

We compared the FJC 1989 data with the data from the CJRA sample weighted to the average annual volume of criminal defendants in RY1993–94, as shown in Table H.2. The average for felony defendants is 35 minutes higher in the CJRA 1993–94 data than in the FJC 1989 data. For all defendants, the CJRA average is 46 minutes higher. However the typical defendant, represented by the median, consumes about the same amount of judicial time in both sets of data.

The above comparisons use the actual mix of defendants in the year(s) of each sample. If we use the reported average times for each sample and the *same* mix of defendants in the comparison between 1989 and 1993–94, the findings are similar. Using the 1989 mix of defendants results in a 31-minute difference instead of a 46-minute difference in the mean. Using the 1993–94 mix of defendants results in a 53-minute difference instead of a 46-minute difference in the mean. In each comparison the CJRA number is larger than the FJC number, and the CJRA number lacks some unreported time from the defendants' cases that were still open at the end of the data collection.³

SUMMARY OF JUDICIAL TIME STUDY FINDINGS

There was almost no difference in the time spent by judicial officers per civil case in 1992–93 when compared to 1989. On the criminal side, there does appear to be an overall rise in the average time required to process a criminal defendant through the system, on the order of 30 to 45 minutes per criminal defendant.

³About 13 percent of defendants in the criminal sample had their cases still pending in September 1996, judging by information from the latest file we had from the Administrative Office.

Table II.2
1989 FIC and 1993–94 CJRA Criminal Time Studies: Total Minutes Reported
for Defendants with One or More Time Study Reports—
All Judicial Officers, 16 Reporting Districts

District Type	Proportion of Defendants with Time Reports		Mean Reported		Median Reported		75th Percentile Reported	
	CJRA	FIC	CJRA	FIC	CJRA	FIC	CJRA	FIC
All Filing Levels								
Pilot	0.79	0.88	264	200	75	65	220	162
Comparison	0.80	0.83	284	254	80	90	167	179
Total	0.79	0.85	276	230	79	75	184	174
	+open		+open		+open		+open	
Felonies Only								
Pilot	0.78	0.88	277	219	79	70	225	188
Comparison	0.80	0.81	295	280	81	93	170	205
Total	0.79	0.84	287	252	81	85	194	195
	+open		+open		+open		+open	

NOTES: CJRA time study data weighted to averaged RY 1993–94 filing levels. CJRA weighting based on district and offense type distribution. 1989 FIC criminal time study data are unweighted. CA(S) excluded because it was a pre-test district in the 1989 study. NY(S), TN(W), and CA(C) excluded because of difficulties with implementation of criminal study. Some 1989 criminal time study records dropped because of lack of offense information.

Federal Judicial Center

**CIVIL
CASE TIME REPORT**

District Court Case Time Study

CASE IDENTIFICATION

DOCKET NUMBER: _____	DISTRICT CODE OR ABBREV: _____	OFFICE: _____
PTF: _____		v DEF: _____

JUDGE OR MAGISTRATE IDENTIFICATION

INITIALS: _____	AO ID CODE (if known): _____
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INSTRUCTIONS

Report EVERY OCCASION When a Judge or Magistrate Judge Does ANY Work On A Time Study Case

1 **USE THIS FORM TO RECORD AS MANY OR AS FEW EVENTS AS YOU WISH.** But please do not retain the form more than one week after the date of the first entry.

2 See separate instructions or telephone for help regarding consolidated cases, hearings involving multiple cases, etc.

3 **SUBJECT MATTER OF TASK.** Using the codes described below, indicate the **matter** on which the judge worked (for example: discovery motion, pretrial conference). Use the "other" category (code 12) when needed, and describe the subject matter in the comment space below. **DO NOT** describe the *character* of the work; **DO** describe the *matter* to which the work related. (For instance, research and review of a magistrate judge's recommendation on a motion for attorney fees should be described simply as "motion for attorney fees," NOT as "research and review of magistrate's recommendation.") Use more than one subject matter code if needed, listing the most time-consuming matter first.

DATE (MO /DAY YR)	TIME		OR Total (HR :MIN)	SUBJECT MATTER CODE(S)	----->	CODES FOR SUBJECT MATTER OF TASK
	Begin (HR :MIN)	End (HR :MIN)				
a / /	:	:	:	_____		Work pertaining to: 1 - Any non-contested disposition (settlement, voluntary dismissal, consent judgment, etc.) 2 - Extension of time, leave to file 3 - Remand to state court 4 - Transfer to another division or judge 5 - Default judgment 6 - Discovery 7 - Motion to dismiss for failure to state a claim 8 - Motion to dismiss for lack of subject matter jurisdiction 9 - Motion for summary judgment 10 - Jury trial, including preparation 11 - Bench trial, including preparation and findings 12 - Other matters. Please describe below (see instruction 3, above)
b / /	:	:	:	_____		Work Not Pertaining to a Specific Matter 13 - Final pretrial conference 14 - Settlement conference 15 - Scheduling conference 16 - Other. Please describe below
c / /	:	:	:	_____		
d / /	:	:	:	_____		
e / /	:	:	:	_____		
f / /	:	:	:	_____		
g / /	:	:	:	_____		
h / /	:	:	:	_____		
i / /	:	:	:	_____		

COMMENTS OR FURTHER EXPLANATIONS (please refer to time entries by line letter): _____

Please return form to Research Division, Federal Judicial Center, One Columbus Circle, N.E., Washington DC 20002
 Thank you for your contribution to the study.

QUESTIONS? - (202) 273-4070

Federal Judicial Center

**CRIMINAL
CASE TIME REPORT**

District Court Case Time Study

CASE IDENTIFICATION

DOCKET NUMBER: _____ DISTRICT CODE OR ABBREV: _____ OFFICE: _____
 US v _____

JUDGE OR MAGISTRATE IDENTIFICATION

INITIALS: _____ AO ID CODE (if known): _____

INSTRUCTIONS

Report EVERY OCCASION When a Judge or Magistrate Judge Does ANY Work On A Time Study Case

- 1 USE THIS FORM TO RECORD AS MANY OR AS FEW EVENTS AS YOU WISH. But please do not retain the form more than one week after the date of the first entry.
- 2 IN MULTIPLE DEFENDANT CASES, show the defendant numbers to which each entry applies. If defendant numbers aren't known, use names or initials. Use comment space if needed.
- 3 See separate instructions or phone for help regarding consolidated cases, hearings involving multiple cases, etc.
- 4 **SUBJECT MATTER OF TASK.** Using the codes described below, indicate the matter on which the judge worked (for example: discovery motion, pretrial conference). Use the "other" category (code 12) when needed, and describe the subject matter in the comment space below. **DO NOT** describe the character of the work; **DO** describe the matter to which the work related. (For instance, research on a motion for a new trial should be described simply as "motion for new trial," NOT as "research.") Use more than one subject matter code if needed, listing the most time-consuming matter first.

DATE (MO /DAY YR)	TIME		DEFEN- DANT NUMS)	SUBJECT MATTER CODE(S)	CODES FOR SUBJECT MATTER OF TASK
	Begin (HR:MIN)	End (HR:MIN)			
a. / /	: :	: :			Work pertaining to: 1 - Arraignment, initial: bail setting, appointment of counsel, pleas, and/or scheduling
b. / /	: :	: :			2 - Change pleas to guilty or not, Rule 11 hearing
c. / /	: :	: :			3 - Pretrial Detention §3142(e)
d. / /	: :	: :			4 - Other issues re: pretrial release
e. / /	: :	: :			5 - Withdrawal of counsel, appointment of new counsel
f. / /	: :	: :			6 - Admission or exclusion of evidence
g. / /	: :	: :			7 - Discovery matters
h. / /	: :	: :			8 - Motion to dismiss
i. / /	: :	: :			9 - Extension of time, continuance
j. / /	: :	: :			10 - Jury trial, including preparation
k. / /	: :	: :			11 - Bench trial, including preparation and findings
l. / /	: :	: :			12 - Sentencing under Sentencing Guidelines
m. / /	: :	: :			13 - Sentencing: Pre-guideline
n. / /	: :	: :			14 - Correction or reduction of sentence (Rule 35)
o. / /	: :	: :			15 - Probation revocation
p. / /	: :	: :			16 - Other. Please describe below, see instruction #4, above.

COMMENTS OR FURTHER EXPLANATIONS (please refer to time entries by line letter):

Please return form to Research Division, Federal Judicial Center, One Columbus Circle, NE, Washington DC 20002.
 Thank you for your contribution to the study.

QUESTIONS? - (202) 273-4070

JUDGE SURVEY RESPONSES

This appendix contains the judge survey questionnaire and the judges' weighted responses for general civil litigation cases with issue joined from our 1992-93 sample.¹ We did not survey judges from our 1991 sample.

By general civil litigation, we mean cases other than those that usually have minimal judicial management (Social Security, prisoner, bankruptcy, foreclosure, forfeiture, and/or government collection cases) as discussed in Appendix A. Cases that are filed and closed before the defendants appear and issue is joined are excluded from the data presented in this appendix because they usually are closed before they need judicial case management. Issue is considered joined "after defendant has answered the complaint in accordance with Rule 12(a), F.R.Civ.P. or as mandated otherwise by the court."² The date issue was joined is defined as the date on which the last answer or reply of the defendant(s) was filed before the first proceeding in the case began. To avoid having survey responses for only part of the processing of a case in federal court, cases terminated in one district by transferring them to another district are also excluded from the data presented in this appendix. Similarly, to avoid having survey responses that pertain to more than one case, consolidated cases are excluded from the data presented in this appendix.

We surveyed judges on the 4,872 cases in the 1992-93 sample that closed before January 1996 when our last surveys were mailed. Complete responses to our surveys were received from the judges on about two-thirds of those cases (3,280 responses). Appendices A and B provide information on the sample design and weighting.

The numbers shown on the questionnaire are the percentages giving each different response, unless the questions concern dates, hours, and dollars. The labels "Med" and "75th %" refer to the median and 75th percentile of responses, respectively.

¹As discussed in Appendices A and B, these responses have been weighted to account for the stratified random sampling probability and differential response rates.

²Administrative Office of the United States Courts (1995).

RAND

Date

name
court
address
city state zip

Dear Judge _____ :

Enclosed is a batch of questionnaires to collect information on your closed cases that are a part of the CJRA evaluation. Attached to each questionnaire is a cover sheet that identifies the case and a copy of the docket sheet for the case. We would appreciate it if you would complete the questionnaires at your earliest convenience and return them to us in the enclosed envelope(s).

If you have any questions please feel free to call me at (310) 393-0411, extension 7621. We appreciate your assistance with this project.

Sincerely,

James S. Kakalik
Director
CJRA Evaluation

cc: clerk, Clerk of Court

Enclosures as noted

1700 Main Street, PO Box 2138
Santa Monica, CA 90407-2138
TEL: 310.393.0411
FAX: 310.393.4618

RESPONSES FOR GENERAL CIVIL CASES WITH ISSUE JOINED
EVALUATION OF CJRA COURT MANAGEMENT POLICIES

JUDGE QUESTIONNAIRE

CASE ID:

FORM:

J 1

BATCH: i i i i

A. MANAGEMENT OF THIS CASE

1. Did any Judge or Magistrate Judge spend time working on this case?

(Circle One) 8.2

NO 1 → **STOP AND RETURN THIS QUESTIONNAIRE**

YES 2 → **CONTINUE WITH QUESTION 2**

91.8

2. What type of judicial officer spent time on each of the following types of activities?

(Circle One Number for Each Type of Activity)

Type Of Activity	Judge	Magistrate Judge	Both	Neither	missing
a. Pretrial scheduling	1 56.1	2 15.3	3 7.7	4 16.2	4.7
b. Dispositive Motions	1 45.4	2 2.5	3 2.0	4 39.1	11.1
c. Other Motions	1 45.5	2 7.9	3 9.6	4 27.3	9.8
d. Discovery Management	1 37.3	2 15.4	3 6.4	4 29.4	11.4
e. Settlement Discussion	1 17.4	2 15.1	3 2.2	4 51.0	14.4
(including mediation or neutral evaluation)					
f. Other Pre-Trial	1 31.6	2 6.2	3 5.3	4 42.9	14.0
g. Trial	1 6.8	2 1.7	3 0.0	4 71.5	19.9
h. Post-Trial	1 4.6	2 1.1	3 0.1	4 73.1	21.1

Statement of Confidentiality

All information that would permit identification of the case, judge, magistrate judge, lawyers, or parties will be regarded as strictly confidential, will be used only for the purpose of the study, and will not be released for any other purpose without your consent, except as may be required by law. All identification information will be destroyed following the completion of our analyses.

3. When you first became involved with this case, how would you have rated it in terms of each of the listed factors?

(Circle One Number On Each Line)

	High	Medium	Low	No Opinion, or Don't Know	missing
a. Overall complexity	1 5.2	2 41.3	3 33.5	4 9.1	10.9
b. Difficulty of discovery	1 3.3	2 28.5	3 39.8	4 16.4	12.0
c. Complexity of legal issues.....	1 4.8	2 38.6	3 35.1	4 9.9	11.5
d. Difficulty in relations between parties and/or attorneys	1 6.2	2 17.7	3 36.3	4 27.6	12.3

4. Some civil cases have intensive judicial management. Some cases may be largely unmanaged, with the pace and course of litigation left to lawyers and with court involvement only when requested.

How would you characterize the level of judicial management in this case?

(Circle One)

Intensive or high.....	1	11.4
Moderate	2	41.4
Low or minimal.....	3	34.2
None	4	10.8
I'm not sure	9	0.9
missing		1.4

5. In hindsight, do you feel the level of judicial management in this case should have been different than it actually was?

(Circle One)

Should have been more intensive.....	1	1.8
Level was correct.....	2	85.0
Should have been less intensive	3	1.0
No opinion.....	4	2.6
missing		9.7

6. Was this case formally assigned to a specific case management category or "track"?

(Circle One)

Yes	1	24.0	→ ANSWER QUESTION 7
No, or District does not have "tracks".....	2	60.6	→ SKIP TO QUESTION 8
Don't Know	3		
missing		10.3	

7. Did this specific "track" assignment influence how you managed this case?

(Circle One)

Yes, made it more intensive.....	1	1.3
Yes, made it less intensive	2	1.3
Yes, different but not more or less intensive.....	3	4.7
No, made no difference	4	15.9
No opinion	5	0.7
	missing	76.1

8. Was any kind of court-related Alternative Dispute Resolution (ADR) method used in this case? IF YES, indicate which.

NO ADR USED.....	0	→ SKIP TO QUESTION 9	missing
	66.8		33.2

OR

(Circle All That Apply)

Arbitration	1	3.5	96.5
Mediation (with neutral lawyer)	2	5.3	94.7
Mini or Summary Jury Trial	3	0.0	100.0
Early Neutral Evaluation (with neutral lawyer).....	4	0.4	99.6
Settlement Conference with Judicial Officer	5	13.1	86.9
Special Master	6	0.1	99.9
Certification that Lawyers Discussed Settlement.....	7	1.1	98.9

9. In hindsight, what kind of ADR should have been used in this case, if any?

NO ADR SHOULD HAVE BEEN USED.....	0	60.2	
		→ SKIP TO QUESTION 10	19.5
APPROPRIATE ADR WAS USED.....	9	20.3	

OR

SHOULD HAVE USED THE FOLLOWING ADR:

(Circle All That Apply)

Arbitration	1	1.2	98.8
Mediation (with neutral lawyer)	2	2.0	98.0
Mini or Summary Jury Trial	3	0.1	99.9
Early Neutral Evaluation (with neutral lawyer).....	4	0.4	99.6
Settlement Conference with Judicial Officer	5	2.7	97.3
Special Master	6	0.1	99.9
Certification that Lawyers Discussed Settlement.....	7	0.3	99.7

10. For each of the following, please indicate what happened in this case.

(Circle One Number For Each Item)

<p>a. Pretrial schedule set by court</p> <p>No, or not applicable 1 17.9</p> <p>Yes, but it was not followed 2 7.6</p> <p>Yes, and it was generally followed 3 64.9</p> <p style="padding-left: 100px;">missing 9.6</p>	<p>g. Parties made a good faith effort to resolve discovery disputes before filing motions</p> <p>No, or not applicable 1 28.9</p> <p>Yes 2 29.3</p> <p>Don't Know 3 32.2</p> <p style="padding-left: 100px;">missing 9.6</p>
<p>b. Rule 16 Scheduling Conference held</p> <p>No, or not applicable 1 39.4</p> <p>Yes, but not with Judicial Officer 2 8.6</p> <p>Yes, with Judicial Officer 3 50.7</p> <p style="padding-left: 100px;">missing 1.3</p>	<p>h. Specific limits (time, scope or quantity) were set on discovery</p> <p>No, or not applicable 1 28.2</p> <p>Yes, but not adhered to 2 10.2</p> <p>Yes, and were adhered to 3 51.0</p> <p style="padding-left: 100px;">missing 10.6</p>
<p>c. Additional pretrial conference(s) held to manage the case</p> <p>No, or not applicable 1 67.4</p> <p>Yes 2 30.6</p> <p style="padding-left: 100px;">missing 2.1</p>	<p>i. Trial date was set on court calendar</p> <p>No, or not applicable 1 44.3</p> <p>Yes, at or around the Rule 16 Conference 2 34.1</p> <p>Yes, later in the case 3 11.5</p> <p style="padding-left: 100px;">missing 10.1</p>
<p>d. Settlement Discussion(s) and/or Conference(s) held</p> <p>No, or not applicable 1 58.8</p> <p>Yes, but never with Judicial Officer 2 10.9</p> <p>Yes, with Judicial Officer 3 28.9</p> <p style="padding-left: 100px;">missing 1.4</p>	<p>j. Trial date was reset</p> <p>No, or not applicable 1 72.8</p> <p>Yes, primarily to meet parties needs 2 13.5</p> <p>Yes, primarily to meet court needs 3 3.2</p> <p style="padding-left: 100px;">missing 10.6</p>
<p>e. Litigants were present at settlement conference</p> <p>No, or not applicable 1 74.5</p> <p>Sometimes 2 5.4</p> <p>Always 3 14.8</p> <p style="padding-left: 100px;">missing 5.2</p>	<p>k. Attorneys submitted a written trial plan prior to trial</p> <p>No, or not applicable 1 83.5</p> <p>Yes, but only in general terms 2 5.8</p> <p>Yes, detailed plan 3 8.9</p> <p style="padding-left: 100px;">missing 1.8</p>
<p>f. Parties made an early disclosure of relevant information without formal discovery requests</p> <p>No, or not applicable 1 28.6</p> <p>Yes 2 24.3</p> <p>Don't Know 3 37.5</p> <p style="padding-left: 100px;">missing 9.5</p>	<p>l. Judicial control was exercised over trial</p> <p>No, or not applicable 1 84.8</p> <p>Yes, minimal control 2 4.4</p> <p>Yes, active firm control 3 8.5</p> <p style="padding-left: 100px;">missing 2.3</p>

11. What would be the ideal way for the court to manage this case and other cases of this type?

(Circle One Number on Each Line)

	<u>Generally Desirable...</u>		<u>Rarely Desirable...</u>					
	but not needed in this case	and needed in this case	and not needed in this case	but needed in this case				
a. Establishing and generally following a pretrial schedule	1 23.6	2 63.8	3 0.8	4 0.1	missing			11.7
b. Holding a Rule 16 scheduling conference with judicial officer	1 25.2	2 50.0	3 12.0	4 0.6				12.3
c. Holding additional pretrial conference(s)	1 40.9	2 27.2	3 16.6	4 2.2				13.1
d. Requiring settlement discussion(s) and/or conference(s) with judicial officer	1 43.5	2 29.3	3 14.5	4 0.7				12.1
e. Requiring litigants to be present at settlement conferences	1 49.3	2 22.3	3 15.4	4 0.5				12.5
f. Requiring parties to make an early disclosure of relevant information without a formal discovery request	1 41.4	2 36.8	3 9.5	4 0.3				12.0
g. Requiring parties to make a good faith effort to resolve discovery disputes before filing motions	1 48.9	2 36.6	3 2.0	4 0.2				12.3
h. Setting and adhering to specific limits (time, scope or quantity) on discovery	1 35.8	2 48.3	3 3.6	4 0.2				12.1
i. Setting an initial trial date at or around the time of the Rule 16 conference	1 30.4	2 34.4	3 21.4	4 0.5				13.3
j. Granting continuances of trial	1 15.8	2 5.3	3 55.7	4 10.2				13.0
k. Requiring attorneys to submit a detailed written trial plan prior to trial, such as witness lists, summary of expert testimony, statement of damages, arguments to be made, etc.	1 61.6	2 16.3	3 9.5	4 0.4				12.2
l. Exercising firm judicial control over trial	1 70.1	2 14.2	3 2.8	4 0.3				12.5

B. EVALUATION OF NEW CASE MANAGEMENT POLICY COMPARED TO OLD POLICY

12. Was there a difference in how you and any other judicial officer managed this case, compared to how you would have managed it if it had been disposed of prior to January 1, 1992?

(Circle One)

Yes, more intensive case management now	1	6.5	} ANSWER QUESTION 13
Yes, less intensive case management now	2	0.4	
Yes, different but not more or less intensive	3	5.6	
No difference	4	63.1	} → SKIP TO COMMENTS SECTION, LAST PAGE
Don't Know / Not Applicable	5	12.8	
	missing	11.6	

13. From your point of view, were the case management policies and procedures used in this case better or worse than those in effect for the same kind of case prior to January 1, 1992?

(Circle One)

Much better.....	1	1.5
Better.....	2	5.5
About the same	3	5.4
Worse	4	0.0
Much worse	5	0.0
Don't know.....	9	0.2
	missing	87.5

14. How did the overall judicial officer work time required for this case compare to the work time that would have been needed under the case management policies in effect for the same kind of case prior to January 1, 1992?

(Circle One)

Much greater	1	0.2
Greater	2	2.9
About the same	3	7.9
Less	4	0.9
Much less	5	0.1
Don't know.....	9	0.2
	missing	87.7

Please use this page for any comments you would like to make about management of this case in particular or about management of litigation by the federal courts in general.

COMMENTS:

POSITIVE COMMENT ABOUT THE COURTS

LITTLE MANAGEMENT NECESSARY

POSITIVE COMMENT ABOUT ADR

If you have any questions, please call RAND collect at
(310) 393-0411 and ask for Jim Kakalik (extension 7621).

When you are finished, please return the questionnaire in the enclosed envelope.

THANK YOU FOR PROVIDING YOUR INPUT TO THIS STUDY

RAND
1700 Main Street
PO Box 2138
Santa Monica CA 90407-2138

LAWYER SURVEY RESPONSES

We surveyed 19,200 lawyers on the 10,371 cases in the 1991 and 1992–93 sample cases. Complete responses to our surveys were received from the lawyers on a little less than half of the cases (4,870 responses out of 9,777 lawyers on the 1991 sample cases and 4313 responses out of 9,423 lawyers on the 1992–93 sample cases). Appendices A and B provide information on the sample design and weighting.

This appendix provides the survey questionnaire used and the lawyers' weighted responses for general civil litigation cases with issue joined.¹ To avoid having survey responses for only part of the processing of a case in federal court, cases terminated in one district by transferring them to another district are excluded from the data presented here. Similarly, to avoid having survey responses that pertain to more than one case, consolidated cases are also excluded.

The numbers shown on the questionnaire are the percentages giving each different response, unless the questions concern dates, hours, and dollars. The labels "Med" and "75th %" refer to the median and 75th percentile of responses, respectively.

¹As discussed in Appendices A and B, these responses have been weighted to account for the stratified random sampling probability and differential response rates.

June 18, 1996

Name
Firm
Street
City, State Zip

Re: Caption & Docket

Dear Name:

I am writing to ask for your assistance in meeting the requirements of the Civil Justice Reform Act of 1990. This act requires all federal district courts to implement plans to reduce cost and delay in civil litigation, and establishes a Pilot program in 10 pilot and 10 comparison districts. The District of *** has been selected by the Judicial Conference of the United States to participate in this program. The legislation mandates that an independent research organization evaluate the Pilot program and its implementation. The Judicial Conference has selected RAND, a non-profit research organization with nationally known expertise in civil justice research, to conduct the study, and has approved RAND's evaluation plan.

One component of the plan calls for each judge, attorney, and party in a randomly selected set of cases to fill out a questionnaire focusing on issues of cost and delay. The case identified in the attached document has been selected for inclusion in the study. Since you are listed as one of the attorneys in this case we would appreciate it if you would fill out the enclosed questionnaire. Information obtained from the questionnaires will be combined with information from other cases before being reported as averages and totals. No case, judge, attorney, or party will be identified in any RAND report and the district court will not have access to the completed questionnaires.

Your cooperation is greatly appreciated. Because the findings of the study are likely to influence future federal court case management policies, it is critical that the study have the contribution of lawyers' experience and opinions concerning these policies. We ask that you promptly return the requested information in the enclosed envelope.

Thank you for your help.

Chief Judge

RESPONSES FOR GENERAL CIVIL CASES WITH ISSUE JOINED

EVALUATION OF FEDERAL COURT MANAGEMENT POLICIES

ATTORNEY QUESTIONNAIRE

INSTRUCTIONS:

- 1. Purpose: This questionnaire seeks information about the court management of the case identified in the cover letter, the timeliness with which it was resolved, the costs of litigation, and your satisfaction with the litigation process and outcome.
- 2. Case and Court: Most questions refer to "this case", which is the case identified in the cover letter. Some questions also refer to "this court", which is the federal district court in which the case was litigated. Please answer all questions with reference to this case and this court only.
- 3. Please answer questions by circling the appropriate number, 1 2, or by filling in the answer as requested.
- 4. If you have any questions, please call the Survey Coordinator, Laural Hill, collect at (310) 393-0411, extension 5107.
- 5. When you are finished, please return the questionnaire in the enclosed, postage-paid envelope.

THANK YOU FOR TAKING PART IN THIS STUDY

Statement of Confidentiality

All information that would permit identification of the case, judge, lawyers or parties will be regarded as strictly confidential, will be used only for the purpose of the study, and will not be released for any other purpose without your consent, except as may be required by law. All identification information will be destroyed following the completion of our analyses.

CASE ID:

1-10/

FORM:

13-14/

BATCH:

15-18/
19-30/5K

SECTION 1: COURT MANAGEMENT OF THIS CASE

1. When this litigation began, how would you have rated this case in terms of each of the listed factors?

(Circle One Number On Each Line)

		High		Medium		Low	
		<u>1991</u>	<u>1993</u>	<u>1991</u>	<u>1993</u>	<u>1991</u>	<u>1993</u>
a. Overall complexity	1	13.3	13.4	49.6	52.9	36.5	33.0
b. Difficulty of discovery	1	13.2	12.4	39.1	41.9	46.4	44.1
c. Complexity of legal issues.....	1	13.3	15.0	47.8	51.1	37.8	32.8
d. Difficulty in relations between parties and attorneys	1	15.5	14.7	29.1	31.1	54.0	53.1

2. Some civil cases are intensively managed by a judge or magistrate judge through actions such as detailed scheduling orders, frequent monitoring of discovery, substantial effort to settle the case, or by requiring rapid progress to trial. Some cases may be largely unmanaged, with the pace and course of litigation left to lawyers and with court intervention only when requested.

How would you characterize the level of court management in this case?

(Circle One)

		<u>1991</u>	<u>1993</u>
Intensive	1	1.9	2.4
High	2	11.9	14.5
Moderate	3	35.0	35.7
Low	4	19.5	19.3
Minimal	5	21.2	18.5
None	6	6.6	5.4
I'm not sure.....	9	2.5	2.6
		1.4	<i>missing</i> 1.6

3A. Did the court assign this case to a special case management category or "track"?

	<u>1991</u>	<u>1993</u>
YES	1	→ SKIP TO QUESTION 3C
	4.3	10.3
NO	2	} 62.1
	67.7	
DON'T KNOW.....	3	→ → →
	22.4	22.2
missing	5.6	5.4

3B. Do you think this case should have been assigned to a special case management category?			
<i>(Circle One)</i>			
	<u>1991</u>	<u>1993</u>	
YES	1	9.3	6.7
NO	2	78.1	74.3
missing		12.6	19.0
SKIP TO QUESTION 4A, PAGE 3			

3C. What effect did this track assignment have on:

	INCREASED		DECREASED		NO EFFECT		DON'T KNOW	
	<u>1991</u>	<u>1993</u>	<u>1991</u>	<u>1993</u>	<u>1991</u>	<u>1993</u>	<u>1991</u>	<u>1993</u>
costs (legal fees and expenses) to your party/parties?	1		2		3		4	
	0.5	1.1	1.7	3.2	1.8	4.8	0.2	0.7
time to disposition?	1		2		3		4	
	0.7	1.8	2.1	4.1	0.9	3.3	0.4	0.5
missing -- costs: 1991=95.8 1993=90.2 time - 1991=95.9 1993 = 90.3								

3D. What do you think of the decision to assign this case to a track?

		<u>1991</u>	<u>1993</u>
<i>(Circle One)</i>			
Case was assigned to the correct track.....	1	3.7	7.9
Case should have been assigned to a track with more intensive management.....	2	0.2	0.7
Case should have been assigned to a track with less intensive management.....	3	0.1	0.5
Case should <u>not</u> have been assigned to a track	4	0.1	0.8
		95.8	missing 90.1

4A. Alternative Dispute Resolution (ADR) methods may include arbitration, mediation, mini or summary jury trial, early neutral evaluation, settlement conferences, special masters or other settlement techniques. Was there any kind of ADR used in this case? IF YES, indicate which ones were used.

		<u>1991</u>	<u>1993</u>	<u>1991</u>	<u>1993</u>
NO ADR USED	0 → SKIP TO QUESTION 4C			missing	
		77.4	72.4	22.6	27.6

OR

(Circle All That Apply)

Arbitration.....	1	1.5	2.4	98.5	97.6
Mediation	2	2.9	9.2	97.1	90.8
Mini or Summary Jury Trial.....	3	0.2	0.2	99.8	99.8
Early Neutral Evaluation.....	4	0.6	1.4	99.4	98.6
Settlement Conference with Judicial Officer ..	5	11.8	10.9	88.2	89.1
Special Master	6	0.4	0.4	99.6	99.6
Other ADR	7	0.4	0.4	99.6	99.6

4B. What effect did ADR have on:

	INCREASED		DECREASED		NO EFFECT		DONT KNOW	
	<u>1991</u>	<u>1993</u>	<u>1991</u>	<u>1993</u>	<u>1991</u>	<u>1993</u>	<u>1991</u>	<u>1993</u>
costs (legal fees and expenses) to your party/parties?.....	1		2		3		4	
	2.0	3.9	8.6	10.9	4.9	6.6	0.6	0.9
time to disposition?	1		2		3		4	
	1.6	2.4	9.3	11.6	4.5	6.9	0.6	1.1
missing -- costs: 1991=83.9 1993=77.6			time: - 1991=84.0; 1993=78.0					

4C. Given what you now know about this case, what do you think the court should have done regarding ADR for this case?

		<u>1991</u>	<u>1993</u>	<u>1991</u>	<u>1993</u>
SHOULD NOT HAVE USED ADR AT ALL.....	0	54.1	52.8	missing	47.2
				45.9	

OR

SHOULD HAVE USED THE FOLLOWING
(Circle All That Apply)

Arbitration.....	1	5.1	5.7	94.9	94.3
Mediation	2	13.5	17.0	86.5	83.0
Mini or Summary Jury Trial.....	3	3.9	3.1	96.1	96.9
Early Neutral Evaluation.....	4	14.3	12.0	85.7	88.0
Settlement Conference with Judicial Officer ..	5	22.7	20.4	77.3	79.6
Special Master	6	2.1	1.5	97.9	98.5
Other ADR	7	1.0	0.8	99.0	99.2

A. What happened in this case?

(Circle One on Each Line)

	No, or Not Applicable	Yes, primarily to meet parties needs	Yes, primarily to meet court needs
K. Trial date was reset	1	2	3
Yes = 0.2	1991 74.0	3.5	5.7
(1991 only)	1992 78.2	11.2	3.7
missing -- 1991	10.7	1992 6.5	

	No, or Not Applicable	Yes, but only in general terms	Yes, detailed plan required
I. Attorneys submitted a written trial plan prior to trial, such as witness lists, summary of expert testimony, statement of damages, arguments to be made, etc.	1	2	3
Yes = 0.4	1991 71.3	7.7	10.5
(1991 only)	1992 75.0	8.1	11.0
missing -- 1991	10.5	1992 5.9	

	No, or Not Applicable	Yes, minimal control	Yes, active firm control
(1991 only) Yes = 0.9			
m. Judicial control was exercised over trial	1	2	3
	1991 73.3	6.5	9.7
	1992 75.6	6.3	10.6
missing -- 1991	7.6	1992 7.4	

B. What effect did this have on costs to your party/parties?

(Circle One on Each Line)

	Increased	Decreased	No Effect or Don't Know
	1	2	3
	1991 6.0	5.7	56.3
	1992 5.5	5.7	60.2
missing -- 1991	32.0	1992 28.7	

	Increased	Decreased	No Effect or Don't Know
	1	2	3
	1991 6.2	8.9	54.3
	1992 7.3	8.1	57.0
missing -- 1991	30.6	1992 27.6	

C. What effect did this have on time to disposition?

(Circle One on Each Line)

	Increased	Decreased	No Eff or Don't Know
	1	2	3
	1991 8.3	5.1	54.6
	1992 7.8	5.4	58.0
missing -- 1991	32.0	1992 28.8	

	Increased	Decreased	No Eff or Don't Know
	1	2	3
	1991 3.6	8.0	57.7
	1992 3.8	7.8	60.8
missing -- 1991	30.7	1992 27.7	

6. What would be the ideal way for the court to manage this case and other cases of this type?

(Circle One on Each Line)

	NO OPINION		NOT NEEDED IN THIS CASE, BUT GENERALLY DESIRABLE		NEEDED IN THIS CASE AND GENERALLY DESIRABLE		NOT NEEDED IN THIS CASE AND RARELY DESIRABLE		NEEDED IN THIS CASE BUT RARELY DESIRABLE	
	1991	1993	1991	1993	1991	1993	1991	1993	1991	1993
a. Establishing and generally following a pretrial schedule..... 1 missing - 1991=8.6; 1993=3.9	3.7	3.8	32.1	30.5	50.6	57.2	4.7	4.0	0.3	0.6
b. Holding a Rule 16 scheduling conference with a judicial officer..... 1 missing - 1991=8.9 1993=3.9	7.4	7.1	30.8	30.8	42.9	47.3	9.7	10.3	0.3	0.7
c. Holding additional pretrial conference(s) to manage the case 1 missing - 1991=9.1 1993=4.4	6.9	7.0	39.1	39.6	26.7	29.3	17.6	18.3	0.6	1.4
d. Requiring settlement discussion(s) and/or conference(s) with a judicial officer 1 missing - 1991=8.8 1993=3.9	4.5	4.7	34.6	37.9	39.9	39.2	11.4	13.1	0.7	1.2
e. Requiring litigants to be present at settlement conferences 1 missing - 1991=9.1 1993=3.8	5.5	5.4	33.4	35.1	28.9	32.9	22.2	21.3	1.0	1.5
f. Ruling promptly on motions 1 missing - 1991=8.9 1993=3.9	3.6	3.4	34.3	35.7	52.3	56.4	0.7	0.5	0.1	0.1
g. Requiring parties to make an early disclosure of relevant information without a formal discovery request 1 missing - 1991=8.9 1993=3.6	5.6	5.6	28.6	28.1	29.4	36.9	26.5	24.6	1.0	1.3

(Circle One on Each Line)

	NO OPINION		NOT NEEDED IN THIS CASE, BUT GENERALLY DESIRABLE		NEEDED IN THIS CASE AND GENERALLY DESIRABLE		NOT NEEDED IN THIS CASE AND RARELY DESIRABLE		NEEDED IN THIS CASE BUT RARELY DESIRABLE	
	1991	1993	1991	1993	1991	1993	1991	1993	1991	1993
h. Requiring parties to make a good faith effort to resolve discovery disputes before filing motions with the court..... 1			2		3		4		5	
missing - 1991=8.8 1993=3.5	3.4	2.8	47.6	48.0	37.7	42.3	2.3	3.2	0.3	0.2
i. Setting and adhering to specific limits (time, scope or quantity) on discovery..... 1			2		3		4		5	
missing - 1991=8.8 1993=3.9	4.2	3.9	38.9	39.4	35.6	39.4	11.9	12.7	0.7	0.7
j. Setting an initial trial date at or around the time of the Rule 16 conference 1			2		3		4		5	
missing - 1991=9.1 1993=3.8	8.4	7.7	33.2	34.3	33.8	37.2	15.1	16.6	0.3	0.4
k. Granting continuances of trial 1			2		3		4		5	
missing - 1991=9.2 1993=4.4	13.6	15.3	34.7	37.5	9.3	13.1	30.5	27.2	2.6	2.5
l. Requiring attorneys to submit a detailed written trial plan prior to trial, such as witness lists, summary of expert testimony, statement of damages, arguments to be made, etc... 1			2		3		4		5	
missing - 1991=9.0 1993=3.7	6.5	6.6	38.7	40.1	23.2	25.8	22.0	23.2	0.5	0.5
m. Exercising firm judicial control over trial 1			2		3		4		5	
missing - 1991=9.3 1993=4.2	8.0	8.0	48.0	50.1	26.3	30.4	8.1	6.9	0.2	0.4

7. Which of the following are true for this case?
(Circle All That Apply)

		<u>1991</u>	<u>1993</u>
There was a state court case concerning the same dispute.....	1	23.3	23.3
There was a federal or state administrative proceeding prior to filing this federal case.....	2	18.1	21.0
There was an appeal filed in this case	3	<i>used docket info</i>	
None of the above.....	4		

SECTION 2: ATTORNEY WORKLOAD ON THIS CASE

We are surveying attorneys in various districts with different court management policies so we can estimate the impact of court management on attorney workload.

8. We would like to know the approximate number of hours worked by you and ALL attorneys for your party or parties on this case. Please include all in-house attorneys, government agency counsel, attorneys employed by party's insurers, U.S. Attorneys, and outside attorneys who worked for your party/parties.

Can you provide estimates of the total hours spent by ALL attorneys for your party or parties?

(Circle One)

		<u>1991</u>	<u>1993</u>
YES, I can provide estimates of the work time spent by all attorneys combined for this party or these parties	1	71.5	72.5
NO, other attorneys outside my firm or organization also represented this party or these parties, and my estimates do not include their work time	2	11.0	missing 11.1

(Please provide name(s) and address(es) below)

↓
 WRITE IN OTHER ATTORNEYS' NAME(S) & ADDRESS(ES)
 ONLY IF YOUR ESTIMATES DO NOT INCLUDE THEIR WORK TIME.

PLEASE PRINT CLEARLY

NAME _____

FIRM _____

ADDRESS _____

CITY _____

STATE _____ ZIP _____

9A. Not counting time spent by those attorneys named in Question 8, what is the approximate total number of hours worked by you and all other attorneys for your party or parties on this federal case? Do not include activity related to state court, any government administrative proceeding, or appellate litigation.

Total Hours _____		<u>1991</u>	<u>1993</u>
	Med.	70	74
	75th %	185	160

9B. Approximately how many of the total number of hours worked for your party or parties were spent on each of the activities listed below? Again do not include activity related to state court, any government administrative proceeding, or appellate litigation.

Type of Activity for Federal Court Case		Number of Lawyer Work Hours	
		1991	1993
ALL ACTIVITY AFTER FILING FEDERAL CASE	a. Trials (include direct preparation for trial here)	Med. 0 75th % 0	0 0
	b. Alternative dispute resolution such as arbitration or mediation after filing (include preparation time)	Med. 0 75th % 0	0 0
	c. Discovery after filing, including motions	Med. 15 75th % 50	15 50
	d. Motion practice, excluding discovery	Med. 5 75th % 25	9 30
	e. Other pretrial conferences or talks with judicial officer	Med. 2 75th % 5	2 5
	f. Other time worked AFTER filing federal case: on research, investigation, writing, talking with parties and lawyers outside court, or anything else related to the litigation	Med. 20 75th % 50	18 45
	PREPARATION FOR FILING FEDERAL CASE	g. All time worked BEFORE filing federal case, in preparation for filing case.	Med. 1 75th % 10

SECTION 3: TIMELINESS OF LITIGATION OF THIS CASE

10. Please indicate the approximate dates for the following events:

				<u>1991</u>	<u>1993</u>
a. Date the dispute started			Med.	104	133
DAYS: Dispute Began to Filing (± 15)	<u>MO</u>	<u>YR</u>	75th %	389	429
b. Date you began work on this case			Med.	5	6
DAYS: Hire Attorney to Filing (± 15)	<u>MO</u>	<u>YR</u>	75th %	107	113
c. Date the case ended for your party or parties. (This may or may not be the same as the court disposition date)			Med.	5	3
DAYS: Term to Dispute End (± 15)	<u>MO</u>	<u>YR</u>	75th %	68	46

11. In your opinion was the amount of time it took from filing this case in federal court to the end of this case, too long or too short for the interests of justice to be served?

			<u>1991</u>	<u>1993</u>
<i>(Circle One)</i>				
Much too long?..... 1	}	missing - 1991=5.6 1993=6.3	6.8	3.9
Too long? 2		ANSWER Q12 BELOW	12.0	11.6
Reasonable? 3		→ SKIP TO Q14, PAGE 10	70.1	71.7
Too short?..... 4		→ SKIP TO Q13, PAGE 10	1.3	1.9
Don't know..... 9		→ SKIP TO Q14, PAGE 10	4.2	4.7

12. Which of the following were significant causes of the time being too long?

		<u>1991</u>	<u>1993</u>
<i>(Circle All That Apply)</i>			
Too many civil cases; backlog of civil cases in the court	1	5.1	3.1
Too many criminal cases; demands of the court's criminal caseload.....	2	6.0	3.1
Court management, procedures, and schedules.....	3	8.1	7.6
Nature or complexity of the case.....	4	4.8	3.8
Actions or failure to act by parties or attorneys.....	5	10.1	8.7
Other (please explain).....	6	0.4	0.2

SKIP TO QUESTION 14, PAGE 10

13. **IF AMOUNT OF TIME WAS TOO SHORT:** Which of the following were significant causes for the time being too short for the interests of justice to be served?

		<u>1991</u>	<u>1993</u>
	<i>(Circle All That Apply)</i>		
Court management, procedures, and schedules	1	1.1	1.5
Actions or failure to act by parties or attorneys	2	0.1	0.4
Other (please explain)	3	0.1	0.0

SECTION 4: STAKES AND OUTCOME OF THIS CASE

14. Was more time or money spent on this case because of its possible effects on other current or future cases?

		<u>1991</u>	<u>1993</u>
	<i>(Circle One)</i>		
Yes	1	35.6	31.5
No	2	59.1	62.3
I'm not sure	3	5.3	6.2

15A. Did this case have monetary stakes?

		<u>1991</u>	<u>1993</u>
	<i>(Circle One)</i>		
YES	1	82.0	83.0
NO	2 → SKIP TO QUESTION 17, PAGE 11	13.0	12.0

missing - 1991=6.0
1993=5.0

15B. What was the combined total dollar amount of the final settlement and/or verdicts for or against your party or parties in this case?

(If your party received money in settlement or verdict, do not subtract what your party paid in legal fees and expenses. If your party or party's insurer paid money in settlement or verdict, do not add what your party or party's insurer paid in legal fees and expenses).

			<u>1991</u>	<u>1993</u>
Party/Parties to receive	\$ _____ .00	Med.	25,437	19,528
		75th %	110,594	93,194

OR

Party/Parties (and/or insurer) to pay	\$ _____ .00	Med.	11,732	10,454
		75th %	66,356	51,034

16A. Think about the worst likely monetary outcome for your party or parties combined that might have occurred at trial in this case. What did you think the verdict amount might have been?

			<u>1991</u>	<u>1993</u>
Party/Parties to receive	\$ _____ .00	Med.	- 0 -	- 0 -
		75th %	44,238	39,249

(OR)

Party/Parties (and/or insurer) to pay	\$ _____ .00	Med.	111,105	150,314
		75th %	444,421	459,995

16B. Now think about the best likely monetary outcome for your party or parties combined that might have occurred at trial in this case. What did you think the verdict amount might have been?

			<u>1991</u>	<u>1993</u>
Party/Parties to receive	\$ _____ .00	Med.	110,675	105,730
		75th %	499,488	315,801

(OR)

Party/Parties (and/or insurer) to pay	\$ _____ .00	Med.	- 0 -	- 0 -
		75th %	- 0 -	- 0 -

17. Were there any nonmonetary stakes involved in this case, for example your party or another party being asked to do something or stop doing something that didn't involve money?

			<u>1991</u>	<u>1993</u>
(Circle One)				
YES.....	1		35.9	37.7
NO.....	2		59.8	57.9
		missing	4.3	4.4

18. Was there any nonmonetary outcome that resulted from this case, for example an order from the court, or a nonmonetary substantive agreement between parties?

			<u>1991</u>	<u>1993</u>
(Circle One)				
Yes, a nonmonetary order from court...	1		9.3	8.7
Yes, a nonmonetary substantive agreement between parties.....	2		10.5	11.5
No.....	3		69.3	65.0
		missing	10.9	14.7

SECTION 5: SATISFACTION AND FAIRNESS

19. Now we would like you to think about the **outcome** of this case, as you previously described it. Overall, how **satisfied** were you with the **outcome** of this case for your party or parties?

(Circle One)

		<u>1991</u>	<u>1993</u>
Very satisfied	1	42.2	40.4
Somewhat satisfied.....	2	25.0	23.5
Neutral	3	12.6	10.4
Somewhat dissatisfied	4	7.2	5.8
Very dissatisfied.....	5	6.1	6.9
	missing	6.9	13.1

20. And, overall, how **fair** do you think the **outcome** of this case was for your party or parties?

(Circle One)

		<u>1991</u>	<u>1993</u>
Very fair	1	45.7	45.1
Somewhat fair.....	2	31.3	27.1
Somewhat unfair.....	3	10.5	8.9
Very unfair	4	4.7	4.8
	missing	7.9	14.1

21. Next think about the court management and procedures for this case. How **satisfied** were you with the **court management and procedures** for this case for your party or parties?

(Circle One)

		<u>1991</u>	<u>1993</u>
Very satisfied	1	44.4	43.7
Somewhat satisfied.....	2	22.0	23.2
Neutral	3	16.7	14.9
Somewhat dissatisfied	4	7.1	6.2
Very dissatisfied.....	5	2.8	2.7
	missing	7.1	9.3

22. And how **fair** do you think the **court management and procedures** were for this case for your party or parties?

(Circle One)

		<u>1991</u>	<u>1993</u>
Very fair	1	57.2	55.6
Somewhat fair.....	2	24.9	26.3
Somewhat unfair.....	3	6.5	5.0
Very unfair	4	2.1	2.2
	missing	9.2	10.9

SECTION 6: COSTS OF LITIGATING THIS CASE

Information about the costs of litigating this case will allow us to analyze the effect of federal court case management on attorney fees and other litigation costs, both before and after federal case filing. Include all activity that was in preparation for or occurred subsequent to filing the case in U.S. district court, up until the time of final disposition of the district court proceedings. Do not include activity related to state court, or any government administrative proceeding, or appellate litigation.

23. Do you think that attorney fees and/or attorney salaries and other costs of this lawsuit to your party or parties were:

(Circle One)

		<u>1991</u>	<u>1993</u>
Much too high	1	3.3	2.3
Too high	2	12.2	12.0
About right.....	3	66.6	66.1
No opinion	9	10.4	11.1
		missing 7.5	8.4

} ANSWER Q24

} SKIP TO Q25, PAGE 14

24. Which of the following do you think were significant causes of the unreasonable costs to your party or parties?

(Circle All That Apply)

		<u>1991</u>	<u>1993</u>
Court management, procedures, and schedules	1	5.3	5.3
Delays caused by backlog of civil cases or demands of the courts criminal caseload increased costs by causing extra lawyer work, such as repeated review of case	2	3.1	1.9
Nature or complexity of case	3	6.8	6.7
Attorney fees were unreasonable.....	4	1.1	0.7
Legal expenses other than attorney fees were unreasonable.....	5	1.2	0.9
Actions or failure to act by lawyers or parties....	6	8.9	8.3
Other (please explain)	7	0.4	0.3

25. What was the primary attorney fee arrangement with your party or parties?
(Circle One)

			Percentage of Monetary Outcome Paid	
			%	
			1991	1993
Contingent fee or Sliding contingent fee.....	1	→ → →	18.0	16.8
			1991	1993
			59.6	56.8
Hourly fee	2	→ → →		
Fixed fee	3	1.1 0.8		
Prepaid legal insurance plan	4	0.3 0.3		
Government attorney who was an employee of a party or parties	5	8.7 8.6		
Private attorney who was a full time employee of a party or parties	6	0.7 0.7		
Lawyer charged no fee	7	1.3 1.0		
Other fee arrangement (for example, a mixture of the above)	8	3.4 4.2		
	missing	7.0 10.8		

			Average Rate Per Hour	
			(Circle One Number)	
			'91	'93
1	\$ 75 or less		2.8	2.3
2	\$ 76 - \$125		27.2	23.0
3	\$126 - \$175		17.7	18.2
4	\$176 - \$250		10.1	11.4
5	More than \$250	0.8 1.8		
	missing	41.5 43.3		

26. We need to estimate the total attorney fees and other litigation costs for ALL attorneys for your party or parties, excluding costs associated with any state case, government administrative proceeding, or appeal. Will you provide these estimates?

(Circle One)

		1991	1993
YES, I will provide estimates of attorney fees and costs for <u>all</u> attorneys combined for this party/ these parties	1	62.9	61.7
NO, other attorneys outside my firm or organization also represented this party/these parties, and my estimates do not include their charges	2	20.7	19.0
	missing	16.3	19.3

26A. Were there any government attorneys, or private attorneys who were salaried employees of your party or parties, or prepaid legal plan attorneys working for your party or parties on this case?

(Circle One)

		1991	1993
YES	1	19.2	17.6
		72.5	72.2
NO	2	8.4	10.2
	missing		

→ SKIP TO Q27, PAGE 15

26B. What was the approximate total number of hours that government attorneys, or private attorneys who were salaried employees of your party or parties, or prepaid legal plan attorneys worked on this case?

_____ Hours		<u>1991</u>	<u>1993</u>
	Med.	25	35
	75th %	80	100

26C. Please estimate the number of hours worked by government or other salaried or prepaid legal plan lawyers in each of the salary categories listed.

YEARLY SALARY	NUMBER OF HOURS	1991		1993	
		Med.	75th %	Med.	75th %
Less than \$50,000		-- 0 --	15	-- 0 --	6
\$50,000 - \$75,000		5	50	-- 0 --	50
\$76,000 - \$100,000		-- 0 --	1	-- 0 --	20
\$101,000 - \$125,000		-- 0 --	-- 0 --	-- 0 --	-- 0 --
Greater than \$125,000		-- 0 --	-- 0 --	-- 0 --	-- 0 --

26D. For these government or other salaried or prepaid legal plan attorneys, please estimate the expenses such as expert witness fees, travel cost, transcript fees, the cost of paralegals or investigators paid by your party or parties.

\$ _____ .00		<u>1991</u>	<u>1993</u>
	Med.	\$ 135	\$ 250
	75th %	1,738	2,091

27. Not including the cost and expenses reported in Q.26A-D for government or other salaried or prepaid legal plan attorneys, please estimate the legal fees and expenses paid by your party or parties. If you cannot separate fees from expenses you may just enter the total in row C.

		<u>1991</u>	<u>1993</u>
	Med.	\$ 9,961	\$ 10,355
	75th %	33,202	27,641
A. Legal fees paid by your party or parties (not including expenses).....\$ _____			
	Med.	1,107	1021
	75th %	4,427	4,093
B. Expenses such as expert witness fees, travel costs, transcript fees, the costs of paralegals or investigators.....\$ _____			
	Med.	10,514	10,348
	75th %	33,299	28,547
C. Legal fees plus expenses (A + B above)			
\$ _____			

SECTION 7: BACKGROUND INFORMATION
 Finally, a few questions about your legal practice.

28. How many years have you been practicing law?

		<u>1991</u>	<u>1993</u>
_____ years	Med.	15	16
	75th %	21	22

29. What percentage of your practice has been devoted to federal district court litigation during the past five years (or, if less than five years, during the time you have been in practice)?

		<u>1991</u>	<u>1993</u>
_____ %	Med.	25	25
	75th %	60	60

30. Approximately how many lawyers work in your law office or legal department?

		<u>1991</u>	<u>1993</u>
_____ lawyers	Med.	12	12
	75th %	36	40

Please use this space for any comments you would like to make about management of this case in particular or about management of litigation by the federal courts in general.

Thank You

COMMENTS:

1991: POSITIVE COMMENTS ABOUT COURT
 NEGATIVE COMMENTS ABOUT COURT
 RULINGS ON MOTIONS NOT PROMPT
 LITTLE MANAGEMENT NECESSARY

1993: POSITIVE COMMENTS ABOUT COURT
 NEGATIVE COMMENTS ABOUT COURT
 RULINGS ON MOTIONS NOT PROMPT

RAND
 1700 Main Street
 PO Box 2138
 Santa Monica CA 90407-2138

LITIGANT SURVEY RESPONSES

Of the 40,221 litigants on the 10,371 cases in the 1991 and 1992–93 sample cases, we had addresses only for 23,623. Complete responses to our surveys were received from about 13 percent of the litigants on closed cases, or about 23 percent of the litigants on closed cases for whom we had addresses (2,824 responses on the 1991 sample cases and 2,264 responses on the 1992–93 sample cases). Appendices A and B provide information on the sample design, completion rates, and weighting.

This appendix provides the survey questionnaire used, and the litigants' weighted responses for general civil litigation cases with issue joined.¹ To avoid having survey responses for only part of the processing of a case in federal court, cases terminated in one district by transferring them to another district are excluded from the data presented here. Similarly, to avoid having survey responses that pertain to more than one case, consolidated cases are also excluded.

The numbers shown on the questionnaire are the percentages giving each different response, unless the questions concern dates, hours, and dollars. The labels “Med” and “75th %” refer to the median and 75th percentile of responses, respectively.

¹As discussed in Appendices A and B, these responses have been weighted to account for the stratified random sampling probability and differential response rates.

June 18, 1996

Name
Firm
Street
City, State Zip

Re: Caption & Docket

Dear Name:

I am writing to ask for your cooperation in a national study of costs and delay in the federal district court. This study was ordered by Congress as part of the Civil Justice Reform Act of 1990. RAND, an independent, non-profit research organization is conducting the study.

We have selected at random a number of civil cases in 20 federal courts to be a part of the study and the case named above is one of these. As a party in this case we would like you to give us your opinions about the way the case was handled, what it cost, and how long it took.

Please take a few minutes to fill out the attached questionnaire and return it in the enclosed postage paid envelope. The questionnaires are mailed directly to RAND and will not be seen by attorneys, the judge, or the federal district court. No case, person, or organization will be identified in our report. Information about your case will be combined with information about other cases and reported as averages and totals. Please do not send this questionnaire to your attorney to fill out because they will be contacted separately to get their opinions.

Your cooperation is important. This study will help Congress and the courts decide whether, and in what way, the management of the civil justice system in the United States should be changed.

Thank you for your help.

Chief Judge

RESPONSES FOR GENERAL CIVIL CASES
WITH ISSUE JOINED

CARD 01

11-127

EVALUATION OF FEDERAL COURT MANAGEMENT POLICIES
LITIGANT QUESTIONNAIRE

This questionnaire seeks information about the court management of the case identified in the cover letter, the timeliness with which it was resolved, the costs of litigation, and your satisfaction with the litigation process and outcome. All questions refer only to this case and to this federal district court.

Fill Out the Questionnaire if:

- 1) You are one of the parties named in this case, or
- 2) You are the person in the organization named in this case who knows the most about it. If you are not the person in the organization who knows the most about this case, please give it to that person to complete. Or you may write the name and address of that person on the back of the questionnaire and return it to RAND.

Please answer questions by circling the appropriate number, 1 (2), or by filling in the answer as requested.

If you have any questions, please call the Survey Coordinator, Laural Hill, collect at (310) 393-0411, extension 6107.

When you are finished, please return the questionnaire in the enclosed, postage-paid envelope.

THANK YOU FOR TAKING PART IN THIS STUDY

Statement of Confidentiality

All information that would permit identification of the case, judge, lawyers or parties will be regarded as strictly confidential, will be used only for the purpose of the study, and will not be released for any other purpose without your consent, except as may be required by law. All identification information will be destroyed following the completion of our analyses.

CASE ID: 1-10/

FORM: 13-14/

BATCH: 15-18/
19-30/EX

SECTION 1: BACKGROUND

1. Counting the current case, approximately how many federal cases have you (or your organization) been a party to in the last FIVE years?

	<i>(Circle One)</i>	<u>1991</u>	<u>1993</u>
One	1	41.1	45.6
2 to 10	2	25.6	25.0
More than 10	3	30.4	25.1
		2.9 missing	4.2

2. Which of the following are true for this case?

	<i>(Circle All That Apply)</i>	<u>1991</u>	<u>1993</u>
There was a state court case concerning the same dispute	1	26.4	25.3
There was a federal or state administrative proceeding prior to filing this federal case	2	26.9	31.4
There was an appeal filed in this case	3	<i>used docket info</i>	
None of the above	4		

3. Are you answering these questions:

	<i>(Circle One)</i>	<u>1991</u>	<u>1993</u>
As an individual party in this case?	1	→ ANSWER Q4A, PAGE 2	
		40.6	41.1
For a government organization?	2	14.4	13.3
For a private organization with fewer than 50 employees?	3	9.5	9.2
For a private organization with 50 or more employees?	4	SKIP TO Q5A, PAGE 3	
		31.3	29.0
		4.2 missing	7.5

SECTION 2: HOURS SPENT ON CASE

INDIVIDUAL PARTIES SHOULD ANSWER QUESTIONS 4A AND 4B

4A. Altogether, about how many hours did you spend on the legal aspects of this case? Include time spent talking with lawyers, going to court, collecting information, and filling out forms, but do not include time discussing the case with family and friends. Do not count any time related to state court, government administrative proceeding, or appellate litigation.

ENTER TOTAL HOURS _____

	<u>1991</u>	<u>1993</u>
Med.	35	40
75th %	100	100

4B. How many of the total hours above were spent on each of the following activities:

		<u>1991</u>	<u>1993</u>
Meeting and/or talking with the judge or magistrate judge	Med.	0	0
	75th %	1	0
Meeting and/or talking with an arbitrator or a mediator	Med.	0	0
	75th %	0	0
Meeting and/or talking with lawyers	Med.	15	10
	75th %	40	32
All other legal aspects of this case; for example, filling out paper work, collecting information, and attending meetings regarding this litigation	Med.	10	16
	75th %	50	50

SKIP TO QUESTION 6, PAGE 4

SECTION 3: TIMELINESS OF LITIGATION OF THIS CASE

6. Please indicate the approximate dates for the following events:

			<u>1991</u>	<u>1993</u>
a. Date the dispute started Minus Filing Date (days)	___ / ___ MO YR	Med. 75th %	145 497	169 509
b. Date your lawyer began work on this case Minus Filing Date (days)	___ / ___ MO YR	Med. 75th %	20 283	34 265
c. Date this case actually ended for you or your organization. After Disposition Date (days) (This may or may not be the same as the court disposition date)	___ / ___ MO YR	Med. 75th %	10 186	2 62

7. In your opinion was the amount of time it took from filing this case in federal court to the end of this case too long, or too short for the interests of justice to be served?

		<u>1991</u>	<u>1993</u>
<i>(Circle One)</i>			
Much too long?	1	20.5	22.3
Too long?	2	14.5	18.2
Reasonable?	3	46.5	43.3
Too short?	4	1.5	1.7
Don't know	9	8.3	7.5
		<i>missing</i> 8.7	7.1

ANSWER Q8A BELOW

→ SKIP TO Q9, PAGE 5

→ SKIP TO Q8B, PAGE 5

→ SKIP TO Q9, PAGE 5

8A. Which of the following were significant causes of the time being too long?

		<u>1991</u>	<u>1993</u>
<i>(Circle All That Apply)</i>			
Too many civil cases; backlog of civil cases in the court..	1	10.0	12.5
Too many criminal cases; demands of the court's criminal caseload.....	2	4.9	3.7
Court management, procedures, and schedules.....	3	14.4	13.3
Nature or complexity of the case	4	10.0	10.9
Actions or failure to act by parties or attorneys.....	5	21.0	23.6
Other (please explain)	6	0.9	0.3

SKIP TO QUESTION 9, PAGE 5

8B. **IF AMOUNT OF TIME WAS TOO SHORT:** Which of the following were significant causes for the time being too short for the interests of justice to be served?

<i>(Circle All That Apply)</i>	<u>1991</u>	<u>1993</u>
Court management, procedures, and schedules 1	0.6	0.6
Actions or failure to act by parties or attorneys 2	0.9	0.6
Other (please explain)..... 3	0.2	0.1

SECTION 4: STAKES AND OUTCOME OF THIS CASE

9. Was more time or money spent on this case because of its possible effects on other current or future cases?

<i>(Circle One)</i>	<u>1991</u>	<u>1993</u>
Yes..... 1	23.9	23.6
No 2	48.7	47.8
I'm not sure 3	27.4	28.5

10A. Was any money at stake in this case?

<i>(Circle One)</i>	<u>1991</u>	<u>1993</u>
YES..... 1	93.5	92.2
NO 2 → SKIP TO QUESTION 12, PAGE 6	6.5	7.8

10B. What was the total dollar amount of the final settlement and/or verdict for or against you or your organization in this case?

(If you or your organization received money in settlement or verdict, do not subtract what you paid in legal fees and expenses. If you or your insurer paid money in settlement or verdict, do not add what you paid in legal fees and expenses).

RECEIVE	\$ _____ .00	Med.	<u>1991</u> \$ 15,483	<u>1993</u> \$12,224
		75th %	110,594	77,609

Ⓞ

PAY (OR INSURER TO PAY)\$ _____ .00	Med.	11,067	5,254
	75th %	77,472	47,579

11A. Think about the worst likely monetary outcome for you or your organization that might have occurred at trial in this case. What did you think the verdict amount might have been?

RECEIVE	\$ _____ .00	Med.	<u>1991</u> \$- 0 -	<u>1993</u> \$- 0 -
	(OR)	75th	55,337	36,391
PAY (OR INSURER TO PAY)\$ _____ .00		Med.	110,675	113,374
		75th	665,984	517,392

11B. Now think about the best likely monetary outcome for you or your organization that might have occurred at trial in this case. What did you think the verdict amount might have been?

RECEIVE	\$ _____ .00	Med.	<u>1991</u> \$110,997	<u>1993</u> \$105,073
	(OR)	75th	554,987	521,647
PAY (OR INSURER TO PAY)\$ _____ .00		Med.	-- 0 --	-- 0 --
		75th	-- 0 --	-- 0 --

12. Were there any nonmonetary stakes in this case (for example you or another party being asked to do something or stop doing something that didn't involve money)?

(Circle One)

		<u>1991</u>	<u>1993</u>
YES.....	1	40.9	41.0
NO.....	2	59.1	59.0

13. Was there any nonmonetary outcome that resulted from this case (for example, an order from the court, or a nonmonetary substantive agreement between parties)?

(Circle One)

		<u>1991</u>	<u>1993</u>
Yes, a nonmonetary order from court ..	1	8.5	11.2
Yes, a nonmonetary substantive agreement between parties	2	12.2	12.8
No	3	65.5	64.8
		13.8 missing	11.2

SECTION 5: SATISFACTION AND FAIRNESS OF OUTCOME
--

14A. Now we would like you to think about the outcome of this case, as you previously described it. Do you think you or your organization won or lost?

(Circle One)

		<u>1991</u>	<u>1993</u>
Won.....	1	38.1	37.9
Lost	2	21.0	22.2
Mixed result.....	3	26.9	26.4
Don't know	9	4.4	5.1
		9.6 missing	8.4

14B. How satisfied were you with the **outcome** of this case?

(Circle One)

		<u>1991</u>	<u>1993</u>
Very satisfied.....	1	24.3	25.4
Somewhat satisfied	2	21.4	18.5
Neither satisfied nor dissatisfied	3	12.7	13.6
Somewhat dissatisfied	4	11.0	13.0
Very dissatisfied	5	20.0	21.1
		10.6 missing	8.4

14C. And how fair do you think the **outcome** of this case was for you?

(Circle One)

		<u>1991</u>	<u>1993</u>
Very fair.....	1	29.6	29.9
Somewhat fair	2	24.1	22.6
Somewhat unfair	3	15.0	15.4
Very unfair.....	4	20.2	22.5
		11.1 missing	9.5

SECTION 6: SATISFACTION AND FAIRNESS OF COURT PROCESS

15A. Next think about the court management and procedures for this case. How satisfied were you with the **court management and procedures** for this case?

(Circle One)

	<u>1991</u>	<u>1993</u>
Very satisfied..... 1	25.5	25.6
Somewhat satisfied..... 2	20.0	22.1
Neither satisfied nor dissatisfied..... 3	20.0	19.9
Somewhat dissatisfied..... 4	10.1	8.6
Very dissatisfied..... 5	12.1	11.8
	12.3 missing	11.9

15B. And how fair do you think the **court management and procedures** were for this case?

(Circle One)

	<u>1991</u>	<u>1993</u>
Very fair..... 1	33.6	34.1
Somewhat fair..... 2	30.2	30.6
Somewhat unfair..... 3	10.4	8.3
Very unfair..... 4	11.0	12.6
	14.9 missing	14.4

SECTION 7: COSTS OF LITIGATING THIS CASE

16. Do you think your attorney fees and/or attorney salaries and other costs of this lawsuit were.

(Circle One)

	<u>1991</u>	<u>1993</u>
Much too high..... 1	15.8	15.2
Too high..... 2	20.1	19.0
About right..... 3	37.4	36.0
No opinion..... 4	16.2	21.0
	10.5 missing	8.8

ANSWER Q 17, PAGE 9

SKIP TO Q 18, PAGE 10

17. Which of the following do you think were significant causes of the unreasonable costs?

(Circle All That Apply)

		<u>1991</u>	<u>1993</u>
Court management, procedures, and schedules	1	10.5	10.6
Delays caused by backlog of civil cases or demands of the court's criminal caseload increased costs by causing extra lawyer work, such as repeated review of case	2	7.6	7.8
Nature or complexity of case	3	13.4	12.2
Attorney fees were unreasonable	4	15.4	13.2
Legal expenses other than attorney fees were unreasonable	5	6.7	6.1
Actions or failure to act by lawyers or parties	6	19.1	17.0
Other (please explain)	7	1.2	0.3

18. What was the primary fee agreement with your lawyer? If you used more than one lawyer or law firm, circle a code for the type of agreement you had with each lawyer.

(Circle All That Apply)

Contingent fee or Sliding contingent fee.....	1	→	→	→			
					<u>1991</u>	<u>1993</u>	
					10.5	9.4	

Hourly fee	2	→	→	→	<u>1991</u>	<u>1993</u>	
					46.6	42.5	
Fixed fee.....	3				3.9	4.7	
Prepaid legal insurance plan	4				1.4	1.0	
Government attorney who was an employee of the litigant	5				11.7	9.2	
Private attorney who was a full time employee of the litigant	6				4.3	3.9	
Lawyer charged no fee.....	7				2.1	3.1	
Other fee arrangement (for example, a mixture of the above).....	8				4.0	2.6	
I don't know because my insurance company paid lawyer.....	9				7.1	9.5	
Did not use a lawyer.....	10	→	SKIP TO QUESTION 22, PAGE 13		<u>1991</u>	<u>1993</u>	
					3.4	4.9	

Percentage of Monetary Outcome Paid		
	%	
	<u>1991</u>	<u>1993</u>
Med.	33	33
75th	33	33

Average Rate Per Hour		
	<i>(Circle One Number)</i>	
	'91	'93
1 Less than \$75	2.4	2.1
2 \$76 - \$125	16.2	12.5
3 \$126 - \$175	14.4	13.1
4 \$176 - \$250	12.0	10.4
5 More than \$250	2.3	3.1
	<i>missing</i>	52.7
		58.8

**PEOPLE WHO ARE ANSWERING FOR ORGANIZATIONS
SHOULD SKIP TO QUESTION 20A**

**PEOPLE WHO ARE ANSWERING AS INDIVIDUALS
SHOULD ANSWER QUESTIONS 19A AND 19B:**

19A. **INDIVIDUALS ONLY:** What were the TOTAL legal fees and expenses you paid in this case, including lawyers' fees, expert witness fees, transcript fees, and fees for legal assistants or paralegals or investigators. Do not include the costs of medical treatment or lost earnings while injured, or the premiums paid for prepaid legal insurance.

		<u>1991</u>	<u>1993</u>
\$ _____,00	Med.	\$ 5,368	\$ 3,196
	75th	33,332	23,685

19B. **INDIVIDUALS ONLY:** Approximately how much of the total above was for lawyers' fees?

		<u>1991</u>	<u>1993</u>
\$ _____,00	Med.	\$ 5,534	\$ 3,196
	75th	35,519	18,500

SKIP TO QUESTION 22, PAGE 13

**PEOPLE WHO ARE ANSWERING FOR ORGANIZATIONS
SHOULD ANSWER QUESTIONS 20A - 21**

20A. **ORGANIZATIONS ONLY:** Were there any government attorneys, or private attorneys who were salaried employees of the organization, or prepaid legal plan attorneys working for the organization on this case?

	<i>(Circle One)</i>	<u>1991</u>	<u>1993</u>
YES.....	1	31.2	29.4
NO	2 → SKIP TO QUESTION 21, PAGE 12	23.9	22.5
		44.9 <i>missing</i>	48.1

20B. **ORGANIZATIONS ONLY:** What was the approximate total number of hours that government attorneys, or private attorneys who were salaried employees of the organization or prepaid legal plan attorneys worked on this case?

		<u>1991</u>	<u>1993</u>
_____ Hours	Med.	35	45
	75th	100	120

20C. **ORGANIZATIONS ONLY:** Please estimate the number of hours worked by government or other salaried or prepaid legal plan lawyers in each of the salary categories listed.

YEARLY SALARY	NUMBER OF HOURS		<u>1991</u>	<u>1993</u>
Less than \$50,000		Med.	-- 0 --	-- 0 --
		75th	-- 0 --	3
\$50,000 - \$75,000		Med.	-- 0 --	-- 0 --
		75th	25	10
\$76,000 - \$100,000		Med.	-- 0 --	-- 0 --
		75th	10	30
\$101,000 - \$125,000		Med.	-- 0 --	-- 0 --
		75th	-- 0 --	-- 0 --
Greater than \$125,000		Med.	-- 0 --	-- 0 --
		75th	-- 0 --	-- 0 --

20D. **ORGANIZATIONS ONLY:** For these government or other salaried or prepaid legal plan attorneys please estimate the expenses such as expert witness fees, travel costs, transcript fees, the costs of paralegals and investigators that were paid by your organization.

		<u>1991</u>	<u>1993</u>
\$ _____ .00	Med.	\$ 846	\$2,056
	75th	5,621	5,263

21. **ORGANIZATIONS ONLY:** Not including the cost and expenses reported in Question 20A-D for government or other salaried or prepaid legal plan attorneys, please estimate the legal fees and expenses paid by your organization. If you cannot separate fees from expenses you may just enter the total in row C.

		<u>1991</u>	<u>1993</u>
A. Legal fees paid by your organization (not including expenses) \$ _____	Med.	\$ 9,315	\$10,303
	75th	32,564	36,242
B. Expenses such as expert witness fees, travel costs, transcript fees, the costs of paralegals or investigators \$ _____	Med.	664	997
	75th	5,091	5,168
C. Legal fees plus expenses (A + B above)	Med	12,351	14,855
	75th	49,767	55,491

\$ _____

22. Do you have any other comments about management of this case in particular or about management of litigation in general by the federal courts?

COMMENTS:

1991: NEGATIVE COMMENTS ABOUT THE COURT

 POSITIVE COMMENTS ABOUT THE COURT

 CASE WAS FRIVOLOUS: NO MERIT

1993: NEGATIVE COMMENTS ABOUT THE COURT

 POSITIVE COMMENTS ABOUT THE COURT

 NEGATIVE COMMENTS ABOUT THE LAWYER

Thank You

RAND
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PO Box 2138
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**ANNOTATED BIBLIOGRAPHY OF RECENT RESEARCH LITERATURE
ON ISSUES OF DELAY AND COST OF LITIGATION**

INTRODUCTION

There has been a long history of research regarding the magnitude, causes, and possible solutions to the perceived problem of delay in civil litigation.¹ While much has been said about the perceived high costs of litigation, the limited available data has impeded research in that area.

Early in this century, Roscoe Pound railed against unnecessary delays and the inefficiencies of the civil justice system,² while increases in filings were of concern to nineteenth century lawyers and legal reformers. In the 1930s, researchers began to turn their attention towards performing empirical analyses on the size and makeup of civil litigation and the time to case disposition. Even then, it was noted that the “congestion of court calendars has been, and still is, a serious problem in judicial administration.”³ During the 1950s—highlighted by Zeisel, Kalven, and Buchholz’s study⁴—and continuing through the present day, numerous scholars and legal commentators have examined more closely the causes for pre-trial delay and have suggested a number of possible solutions. Organizations such as the American Bar Association, the Federal Judicial Center, the National Center for State Courts, and RAND’s Institute for Civil Justice have made the study of the pace and costs of litigation a priority.

For our CJRA research, it was useful to review the findings from some of the more recent major research studies. Several of these recent reports have identified factors possibly affecting both the time to disposition and the costs associated with litigation, and we wanted to make sure that we considered those factors in our study to the extent possible. Earlier RAND reports have set forth the major findings in the relevant research literature and have detailed delay reduction efforts through the early

¹For a discussion of the history of civil delay research, see Selvin and Ebener (1984); and Kakalik et al. (1990).

²Roscoe Pound, address to the American Bar Association in St. Paul, 1906. Reprinted in Pound (1964).

³Clark and Shulman (1937), p. 168.

⁴Zeisel et al. (1958).

1980s and need not be repeated here.⁵ Accordingly, with two exceptions, we focus on some of the more important studies published from the 1980s onward.⁶

There was by no means a single, monolithic set of findings reached in these studies. Although some common themes occur in past research, as noted in Chapter Three of this report, there are some differences regarding the causes of delay and costs of litigation and how best to go about reducing them. What does become clear is that all felt the problems were solvable, but not without some serious effort. All also appeared to operate under the assumption that reducing delay and costs did not automatically result in a reduction of “justice.” What follows in this appendix is an annotated bibliography of some of the major findings from recent studies regarding civil litigation cost and/or delay.⁷

1. Flanders, Steven. *Case Management and Court Management in U.S. District Courts*, Federal Judicial Center, Washington, D.C., 1977
2. Church, Thomas W. Jr., Carlson, Alan, Lee, Jo-Lynce, and Tan, Teresa. *Justice Delayed: The Pace of Litigation in Urban Trial Courts*, National Center for State Courts, Williamsburg, Virginia, 1978
3. Sipes, Larry L., Carlson, Alan, Tan, Teresa, Aikman, Alexander B., and Page, Robert W., Jr., *Managing to Reduce Delay*, National Center for State Courts, Williamsburg, Virginia, 1980.
4. Trubeck, David M., Sarat, Austin, Felstiner, William L. F., Kritzer, Herbert M., and Grossman, Joel B., “The Costs of Ordinary Litigation,” 31 *UCLA Law Review* 72, October 1983b. See also: Trubeck, David M., Grossman, Joel B., Felstiner, William L. F., Kritzer, Herbert M., and Sarat, Austin, *Civil Litigation Research Project: Final Report—Summary of Principal Findings*, University of Wisconsin Law School, Madison, Wisconsin, March 1983a.
5. Chapper, Joy A., et al, *Attacking Litigation Costs and Delay*, American Bar Association, Chicago, Illinois, 1984.
6. Solomon, Maurcen, and Somerlot, Douglas, *Caseflow Management in the Trial Court: Now and for the Future*, American Bar Association, Chicago, Illinois, 1987.
7. Mahoney, Barry, et al, *Changing Times in Trial Courts*, National Center for State Courts, Williamsburg, Virginia, 1988

⁵See, for example, Selvin and Ebener (1984), Ebener (1981).

⁶One of the documents included here whose publication date is somewhat earlier than our cutoff point is Thomas Church's 1978 study of delay in state trial courts suggesting that “local legal culture” was a key factor in a court's backlog and time to disposition. Besides articulating an interesting perspective on why changing processing rates is sometimes difficult, the study's methodology was partially replicated in some later projects.

We have also included Steven Flander's 1977 report on the Federal Judicial Center's large-scale District Court Studies Project, which focused on the federal courts. That effort parallels our own in some respects and was a significant addition to the research literature.

⁷Many of these studies also dealt with criminal case filings; however, since the major thrust of our research concerns civil litigation, we concentrate this review on their findings in reference to the civil caseload.

8. Goerdt, John A., Lomvardias, Chris, Gallas, Geoff, and Mahoney, Barry, *Examining Court Delay: The Pace of Litigation in 26 Urban Trial Courts, 1987*, National Center for State Courts, Williamsburg, Virginia, 1989
9. Dunworth, Terence, and Pace, Nicholas M., *Statistical Overview of Civil Litigation in the Federal Courts*, RAND, Santa Monica, California, R-3885-ICJ, 1990.
10. Kakalik, James S., Selvin, Molly, and Pace, Nicholas M., *Averting Gridlock: Strategies for Reducing Civil Delay in the Los Angeles Superior Court*, RAND, R-3762-ICJ, Santa Monica, California, 1990.
11. Judicial Council of California, *Prompt and Fair Justice in the Trial Courts: Report to the Legislature on Delay Reduction in the Trial Courts*, Administrative Office of the Courts, State of California, July 1991.
12. Goerdt, John A., Lomvardias, Chris, and Gallas, Geoff, *Reexamining the Pace of Litigation in 39 Urban Trial Courts*, National Center for State Courts, NCSC R-128, Williamsburg, Virginia, 1991
13. Goerdt, John A., Ostrom, Brian J., Rottman, David B., LaFountain, Robert C., and Kauder, Neal B., "Litigation Dimensions: Torts and Contracts in Large Urban Courts," *State Court Journal*, Vol. 19, No. 1, 1995 (special issue).

ANNOTATED BIBLIOGRAPHY OF MAJOR PRIOR STUDIES

Case Management and Court Management in U.S. District Courts

- Flanders, Steven, *Case Management and Court Management in U.S. District Courts*, Federal Judicial Center, Washington, D.C., 1977

Study Overview

This study was an outgrowth of the Federal Judicial Center's District Court Studies Project and was based on visits to 10 courts and docket information on approximately 500 randomly selected civil cases terminating in fiscal year 1975 from 6 courts (MD, PA(E), LA(E), CA(C), FL(S), and MA were the primary districts; AL(N), NM, KY(E), and WI(E) were the other four). Visits included discussions with judges, clerk's staff, public defenders, U.S. Attorneys, members and representatives of the local bar, and personal observations of proceedings. The goal was to identify the differences between, and reasons for, fast and slow courts.

Conclusions Regarding Cost and Delay

- The federal court system and the practice of law are highly localized, with limited interaction between judges in different districts or even within some districts.
- Great differences exist between courts in the nature and extent of case management and these differences appear to have a major effect on disposition time.

- Although most courts have procedures meant to achieve early and effective pretrial control, most fall short of attaining that goal.
- In slow courts, little activity occurs during much of the time a typical case is pending and many cases violate F.R.Civ.P. time limits.
- Fast and/or highly productive districts tend to have:
 - Procedures to monitor pleadings, begin discovery quickly, finish discovery within a reasonable time, and promptly initiate a trial if needed.
 - Procedures to minimize judge's investment of time until discovery is complete. Only when the judge is needed to resolve preliminary matters, handle dispositive motions, or plan complex cases, is he or she involved with the case.
 - Docket control, attorney contacts, and many conferences are often delegated to a courtroom deputy or magistrate judge.
 - Judges play a minimal role in settlements until the case is nearly ready for trial (though some judicial officer raises the issue early in the case).
 - Relatively fewer written opinions are prepared for publication.
- Courts with weak internal governance systems (as well as poor communication among judges) have greater difficulty in taking effective policy actions. In courts with serious problems, a weak governance system can be crippling.
- A uniform approach to case management among judges reduces the burden on judges to train attorneys in the case they oversee as to procedural requirements (and reduces the burden on attorneys to learn the different practices of individual judges).
- While bar practices (such as the time interval between incident and filing, choice of forum questions, variations in pleading practice, settlement behavior, contentiousness, and trial practice) are important, they can be changed and influenced by district policy.
- The backlog of cases awaiting court action is not the primary source of delay.
- Concentrations of cases that are traditionally considered to be fast or slow do not cause differences in time to disposition.
- The degree of control by the courts over preparation of civil cases, particularly for discovery, is closely associated with the time required for each stage of a case.
- Not all answers are filed in a timely fashion. This is important since a timely filing is a precondition to subsequent judicial management.
- Though delay in completing service has only a marginal effect on the delay in filing answers to complaints, reducing this time would help.
- Some courts could save months by asserting earlier control of civil cases (by shortening the time from the answer to the first pretrial date).
- Delay in beginning discovery is controllable if management of the case is asserted early. Some months elapse before discovery begins in the typical case.

Overall time for discovery, and not the amount of discovery activity, is greatly affected by discovery controls.

- Judicial participation in settlement produces mixed results. Though a limited role may be valuable, a large expenditure of judicial time may not be cost-effective.
- Courts should protect their trial time carefully by assuring that a trial is actually underway for six hours or more for each trial day.
- The use of magistrate judges varies between and within districts from minimal employment to using them for nearly all pre-trial matters. The magistrate judge system has been of great benefit to some courts. Magistrate judges should not hear motions that are likely to be appealed (such as dispositive motions or motions to suppress).
- There should be an effective system to train and supervise courtroom deputy clerks in case management.
- Little evidence was found to support the idea that speed and quality are in conflict (other than a few instances of failing to grant a trial continuance for good cause); in fact, delay can hurt the quality of justice.
- If all other steps are taken to accelerate the pace of litigation, then continuances for good cause shown could be granted without changing the overall speed of a district.
- Statistics used as measures of a district's or a judge's performance may be misleading in some ways but are necessary for judge/personnel/resource allocation and for discovering wide and obvious differences between judges. Monthly caseload reports for each judge provide valuable information.

Justice Delayed: The Pace of Litigation in Urban Trial Courts

- Church, Thomas W. Jr., Carlson, Alan, Lee, Jo-Lynce, and Tan, Teresa, *Justice Delayed: The Pace of Litigation in Urban Trial Courts*, National Center for State Courts, Williamsburg, Virginia, 1978

Study Overview

This National Center for State Court report constituted the final report of the Pretrial Delay Project. The Project collected data on more than 20,000 cases in 21 general jurisdiction courts and intended to uncover the extent, causes of and remedies for delay. Attention was restricted to the processing of felony and civil cases (exclusive of domestic relations, probate, and juvenile matters) and did not focus on trial conduct or alternatives to adjudication. The sample included 500 civil and 500 criminal cases disposed of in 1976 in each of the 21 courts. Ten courts had a more intensive evaluation (including informal interviews of key players).

Conclusions Regarding Cost and Delay

- Traditional explanations, such as court size, judicial caseload, seriousness of the cases, or jury trial rate, are not good predictors of delay.
- However, the calendaring of civil cases, the charging process used in criminal matters, and the amount of management exercised by the court during pretrial do bear some relationship to the pace of litigation.
- Informal expectations, attitudes, and practices of judges and lawyers have a great deal to do with court delay.
- Concern by the court with delay as an institutional and social problem is an essential element to any efforts to reduce pretrial delay.
- For civil cases, individual calendar courts are relatively faster than master calendar systems. Individual calendar systems may increase accountability on the part of the judges and perhaps productivity as well.
- “Local legal culture” is a very important determinant of case processing speed. Court systems adapt to a certain pace of litigation which in turn is associated with a backlog of cases in both the judge’s chambers and in the attorney’s offices. These established expectations and practices make changes in processing speed difficult.
- Major strategy for reducing delay is management by the court to control the progress of cases (as opposed to letting the attorneys dictate the pace).
- Trials should be firmly set and continuances granted rarely in order to create an environment where all players expect the trial to begin on the date scheduled.

Managing to Reduce Delay

- Sipes, Larry L., Carlson, Alan, Tan, Teresa, Aikman, Alexander B., and Page, Robert W., Jr., *Managing to Reduce Delay*, National Center for State Courts, Williamsburg, Virginia, 1980.

Study Overview

Eight jurisdictions cooperated with the National Center for State Courts (NCSC) in order to test various types of case management and their effects upon delay. Experiments were set up in two courts to test the total case management concept, in three to test firm trial dates and continuance policies, and in one to test an expedited disposition program for older cases.

Conclusions Regarding Cost and Delay

- Case disposition pace is largely determined by local legal culture (judges’ and attorneys’ established expectations, practices, and informal rules of behavior). Courts need to understand this in order to improve processing rates.

- Court size, caseload, and trial rate do not determine disposition speed.
- Total case management requires the court to move the pending caseload on pre-set time standard. This is accomplished by controlling caseflow, specifying the number of months within which cases should be concluded, and the maximum permissible period for each major step within the case. It requires new procedures for scheduling appearances, granting continuances, restricting trials, and identifying cases failing to meet deadlines.
- Steps needed to implement a management system include: monitoring receipt of the answer; identifying cases appropriate for ADR programs (and diverting as early as possible); checking trial-readiness document filings, developing realistic trial-setting practices; a firm continuance policy with few granted; emphasizing older cases; and developing a useful information system that accurately measures the pace of litigation.
- Court control (as evidenced by firm dates and limited continuances), rather than attorney control, is needed after a trial date has been set.
- Human factors are critical; there must be a commitment to change among all the judges as well as an acceptance of responsibility regarding the pace of litigation. There must also be a commitment of the clerk's office resources. The bar and other affected entities such as government and businesses must be consulted, both to reach a consensus and to get a handle on the dimensions of the local legal culture.

The Costs of Ordinary Litigation

- Trubeck, David M., Sarat, Austin, Felstiner, William L. F., Kritzer, Herbert M., and Grossman, Joel B., "The Costs of Ordinary Litigation," 31 *UCLA Law Review* 72, October 1983b. See also: Trubeck, David M., Grossman, Joel B., Felstiner, William L. F., Kritzer, Herbert M., and Sarat, Austin, *Civil Litigation Research Project: Final Report - Summary of Principal Findings*, University of Wisconsin Law School, Madison, Wisconsin, March 1983a.

Study Overview

This is the largest previous empirical study of litigation costs. The reports use data from the Civil Litigation Research Project, a nationwide study of civil cases terminating in 1978 in five federal districts (focusing on the federal court and at least one state court of general jurisdiction in each of the study districts) and a survey of over 5,000 households. Estimates are made of the frequency of, costs and outcomes related to, and activities involved in civil litigation, used interviews with over 2,000 lawyers from across the country and data from over 1,600 cases. The sample excluded disputes involving less than \$1,000 and some mega-cases.

Conclusions Regarding Cost and Delay

- For each dispute in court records there are nine that failed to reach the filing stage.
- Parties usually fight over less than \$10,000 worth of money. The typical case is usually procedurally simple and will be settled voluntarily after some pretrial activity (but no trial). The typical case also involves the expenditure of about 30 hours of lawyer time on each side. The average plaintiff will recover some portion of the amount claimed and that will significantly exceed the money and time spent on the case; defendants will have litigation expenses that are less than the amount the plaintiff's original claim was reduced.
- Relatively little discovery occurs in the ordinary lawsuit and none in over half of the cases. It is rare to have more than five separate discovery events in court records. Seventeen percent of lawyer time was spent on discovery.
- About half of lawyer time was spent in conferences with clients, non-discovery-related factual investigation, and settlement negotiation. Only 9 percent went to trials and hearings.
- The great majority of clients' litigation costs were to cover legal fees and in-house legal counsel. Only a small percentage of the expenditures were for expenses other than legal fees and in-house counsel.
- Primary drivers for the expenditure of lawyer time are: events in the case (motions, discovery, pleadings); federal vs. state court; client goals; lawyer goals (professional visibility, public service); case characteristics (stakes, complexity); case management (standard operating procedure for pretrial events, plan for discovery, plan for settlement); craftsmanship of the lawyers; and type of client.
- Differences between hourly and non-hourly lawyers were included in model to explain lawyer investment of time.

Attacking Litigation Costs and Delay

- Chapper, Joy A., et al, *Attacking Litigation Costs and Delay*, American Bar Association, Chicago, Illinois, 1984.

Study Overview

This study chronicles five years of work done by the ABA's Action Commission to Reduce Court Costs and Delay. It focuses on simplifying the pre-trial phase, shortening the time consumed by appeals, and using telephone conferencing. A primary test site was Kentucky but they also looked at programs in Vermont, Colorado, and California. The Kentucky experiment involved three courts, took a random sample of cases disposed, and conducted interviews with judges, attorneys, and staff members.

Conclusions Regarding Cost and Delay

- Average time to disposition went from 16 to 5 months, less discovery was conducted, and less lawyer time was spent on cases in a pilot program in Kentucky. The program used judicial control and case simplification.
- The Kentucky program included: enforced deadline on pleadings, presumptive fast schedules, discovery conferences, discovery limits, mandatory information exchange, motions day, final pretrial conference, and a strict continuance policy.
- Without both judicial control *and* case simplification, other programs did not achieve such dramatic results; in fact using only one may increase pretrial activity.
- Lawyers and judges did not think that the quality of the litigation was affected by the programs' limitations and deadlines.
- Keys are cooperation between bench and bar to locate points of delay and design reduction programs, require new procedures be followed in particular kinds of cases, and monitor the results.
- There is potential for great savings of travel time and courthouse waiting time in courts by the use of telephone conferencing. A wide range of matters can be handled by the telephone. Attorneys and judges found it equally satisfying to use telephonic conferences, but this needs careful attention to detail in allocating responsibility for initiating the conference.
- Time savings for attorneys do not always translate into cost savings for clients (e.g., contingency fees). Litigants must be assured of receiving economic benefits of lower litigation costs.
- When time savings yield cost savings, the amount can be substantial.
- Introduction of change can cause problems. Bench and bar might not acknowledge delay and cost problems.
- Responsibility must be established for setting time standards and court needs to monitor operations.
- Lends credence to hypothesis that local legal culture is important factor in delay.

Caseflow Management in the Trial Court: Now and for the Future

- Solomon, Maureen, and Somerlot, Douglas, *Caseflow Management in the Trial Court: Now and for the Future*, American Bar Association, Chicago, Illinois, 1987.

Study Overview

This study updates a 1973 ABA monograph that outlined 11 essential elements of effective caseflow management. It reviews empirical research since mid-70s that dealt with issues and assertions raised in the original report. No original data collection was done; it uses work by others to explain ABA's position on caseflow management.

Conclusions Regarding Cost and Delay

- Court delay can be prevented. Where it exists, it can be reduced.
- Many courts have successfully reduced or prevented excessive litigation delay.
- Successful approaches are characterized by active management of case progress by able, committed trial judges under the leadership of presiding officers selected for their skill and motivation, and who are assisted by trained administrators with input into policy decisions.
- Assignment systems of whatever type depend on leadership of persons in charge (need to motivate judges to get the job done). Individual judges also need commitment and skills. Resources must be sufficient.
- Court attention should be directed to progress of all cases, not only those requiring a trial.
- Prompt disposition of the more than 90 percent of cases that will not go to trial should be emphasized.
- Overscheduling of trials is an acceptable practice unless it gets too heavy. Counsel will have not incentive to prepare if overscheduling is too widespread.
- The key is having the lawyers devote time to filed cases rather than having them wait until the court contacts them to find out why nothing is going on.
- Court should have an active role in decisions affecting the progress of a case.
- The joint responsibility of counsel to assure that all cases are processed promptly should be emphasized.
- Caseload management yields a process where a reliable allocation of time by counsel and clients can be made.
- Instituting a new caseload management system is not simple, requires cooperation of all levels, and requires organization and management.
- Common elements of caseload management include: judicial commitment and leadership; court consultation with the bar; continuous court supervision of case progress; adoption of time standards and goals to compare system performance; establishment of deadlines for completion of specific case events; monitoring each case to assure that scheduled events are concluded on time and to allow tracking of progress; responding to failures to comply with deadlines; scheduling of credible trial calendars; and court control of continuances.

Changing Times in Trial Courts

- Mahoney, Barry, et al, *Changing Times in Trial Courts*, National Center for State Courts, Williamsburg, Virginia, 1988

Study Overview

An outgrowth of a three-year study of case processing times in 18 general jurisdiction courts, this study followed closely the methodology used in the Church et al. (1978) study. The sample included 500 felony and 500 civil terminations in most of the 18 courts studied for the years 1983, 1984, and 1985. The study also used court-level data and interviews with judge, lawyers, and court personnel.

Conclusions Regarding Cost and Delay

- Delay is an important but not all-pervasive, or inevitable, or insoluble problem.
- No single model exists for effective caseload management (although courts utilizing management most effectively share a number of characteristics including strong leadership, clear goals, timely and accurate caseload information, mechanisms for effective communication, and the use of certain simple case management techniques).
- It appears that differences in the attitudes and expectations among practitioners are important aspects of the pace of litigation.
- Major changes in the local legal culture have taken place in jurisdictions with successful delay reduction programs. There is general agreement in these courts of the desirability of caseload management and the need to minimize delay. Courts that do make improvements must be prepared to resist pressure to retreat from case management.
- Pace of litigation is not clearly correlated with court size, jurisdiction population, caseload composition, per-judge caseloads, or jury trial percentage. However, combinations of these factors may be meaningful.
- The presence of an ADR program is not related to speed; however, the way ADR is used (i.e., early referral and continued management of referred cases) might be.
- For civil cases, the earlier a court becomes involved in monitoring and scheduling, the better. In faster courts, continuances are limited and granted only for good cause (mere stipulations are not sufficient grounds). Slower courts either leave the pace of litigation to counsel or become involved only after "trial-readiness" is reached.
- Leadership is important. Courts with rotating chief judges with little management authority may lack a strong central core for delay reduction programs. Involvement of other judges, clerks, and prosecutors is important, as is education and training in the new techniques. Communication with the bar, state-level leaders, key actors, and others is essential.
- Monitoring case progress, flagging slow cases, and acting when a problem is discovered are all important management procedures.

- Schedules can be made in consultation with attorneys but once set, continuances should be granted only infrequently. There should be a high expectation that scheduled events will actually take place.

Examining Court Delay: The Pace of Litigation in 26 Urban Trial Courts, 1987

- Goerdt, John A., Lomvardias, Chris, Gallas, Geoff, and Mahoney, Barry, *Examining Court Delay: The Pace of Litigation in 26 Urban Trial Courts, 1987*, National Center for State Courts, Williamsburg, Virginia, 1989

Study Overview

The study used data on 31,000 cases in 26 large urban trial courts, obtained using sampling and coding procedures developed in the Mahoney et al. (1988) study. That study sampled 500 criminal and 500 civil cases in each district from all felony (initiated in the court of general jurisdiction) and most standard civil (excluding probate and small claims) dispositions during 1987.

Conclusions Regarding Cost and Delay

- Generally, the courts had a better record of meeting ABA time standards with criminal cases than with civil. Several courts are close to achieving the ABA disposition goal of all civil within two years (suggesting that the goal is a realistic one).
- Early control over civil case schedules was the most important predictor of shorter processing times.
- When other factors were controlled for, court size, filings per judge, calendar type, and the jury trial rate were not significant predictors of the civil processing times.
- Judges and administrators rated the increase in drug crimes as a serious problem.
- "Insufficient number of judges" was rated by personnel in faster courts as a minor problem and as a serious problem in slower courts (non-judicial resources were not examined).
- Cases filed per judge were about the same in faster and slower courts.

Statistical Overview of Civil Litigation in the Federal Courts

- Dunworth, Terence, and Pace, Nicholas M., *Statistical Overview of Civil Litigation in the Federal Courts*, RAND, R-3885-ICJ, Santa Monica, California, 1990.

Study Overview

The study dealt with case filing and disposition data in the federal courts between 1971 and 1986, and focused on private civil actions. The Integrated Federal Courts Data Base was analyzed to see how the processing of civil cases in the district courts has changed over time. Principal performance measures included length of time to disposition and means of case termination; these measures were compared to size and growth of districts, type of action taken before termination, case mixture, and level of court resources. The study compared inter-district variation in processing rates and grouped a subset of 30 districts by median time to disposition (into "fast," "slow," and "average" categories) for more detailed analysis.

Conclusions Regarding Cost and Delay

- The focus of the report is private civil cases, since the most rapid increase in filings has been for those cases rather than for criminal cases or civil cases in which the U.S. is a party.
- On the average, private civil cases are being disposed of in about the same length of time in 1986 as they were in 1971, despite the substantial increase in caseload. However, specific case types experienced changes in processing times over this period.
- While the percentage of cases that received no court action over this period has remained fairly stable, there has been a significant drop in the trial rate from 10.9% in 1971 to 6.6% in 1986.
- Considerable diversity existed in performance measures among districts and in rates of filing growth. Some districts have 80 percent of their cases terminated in one year while in others this proportion drops to 40 percent; the proportion of cases taking three years or more to terminate ranges from 1 percent to over 20 percent.
- District categorization by processing speed does not reveal intuitively expected differences when dimensions such as the U.S. civil and criminal caseload, case type mix, court action taken prior to termination, and the number of judges are examined.
- Given that there is little explanation for inter-district variation in processing speed from the above-cited measures of caseload and resource level, the determinants of time to disposition may involve other factors such as the practices of the local bar or differences in judicial case management. Further research and more detailed data collection are needed.

Averting Gridlock: Strategies for Reducing Civil Delay in the Los Angeles Superior Court

- Kakalik, James S., Selvin, Molly, and Pace, Nicholas M., *Averting Gridlock: Strategies for Reducing Civil Delay in the Los Angeles Superior Court*, RAND, R-3762-ICJ, Santa Monica, California, September 1990.

Study Overview

This detailed study of our nation's largest urban trial court used docket coding of 1,000 cases, surveys of lawyers and judges, jury verdict data for 2,500 trials, interviews and other sources.

Conclusions Regarding Cost and Delay

- Causes of delay in Los Angeles primarily include a demand for judicial services that greatly exceeds the supply, a need to improve court and case management, and delay on the part of litigants and lawyers.
- No quick and cheap fix is possible. Two basic steps are needed: significantly increase, and better manage, court resources. Since increasing the number of judges to adequate levels is unlikely, some changes in management would be needed (but alone would not solve the problem).
- Productivity improvement measures would include more active judicial management (especially in terms of controlling trial length), using an individual calendaring system, holding a single settlement conference before a neutral lawyer early and only one judicial settlement conference within a month of trial, endorsing realistic time standards, denying continuances without good cause, tightening the timeline for arbitration, and placing limits on discovery.
- Focus extra pretrial judicial effort on cases most likely to go to trial, use different time standards depending on the case type, prioritize trial by estimated length.
- Strengthen court management, lengthen the presiding judge's term, and improve the management information system.

Prompt and Fair Justice in the Trial Courts: Report to the Legislature on Delay Reduction in the Trial Courts

- Judicial Council of California, *Prompt and Fair Justice in the Trial Courts: Report to the Legislature on Delay Reduction in the Trial Courts*, Administrative Office of the Courts, State of California, July 1991.

Study Overview

The report summarizes the findings of the National Center for State Courts' evaluation of California's Trial Court Delay Reduction Act of 1986. That act required the adoption of time standards for case processing, collection of data, and publishing of

statistics on compliance. Also required was implementation of an experimental delay reduction program for three years in nine superior courts with the highest ratio of pending at-issue civil cases per judicial officer. In the nine pilot courts, judges were to actively monitor and control the movement of study cases from filing to termination. Essentially, the responsibility for reducing delay was placed on the individual judge. There were to be firm policies limiting continuances and for starting trials on the scheduled date. Sanctions were available to enforce their orders. Judges and court staff received instruction in case management and delay reduction techniques. Variations in how each court developed its own program included using a master, individual, or hybrid calendaring system; instituting an intensive civil settlement program; establishing criminal backlog reduction programs; and using status conferences and regular court intervention to manage cases versus a rule-based pre-programmed set of deadlines, with regular monitoring of key steps in the litigation.

Data sources used included about 500 dispositions and 75 jury verdicts occurring in baseline years of 1986 and 1987 in each of the nine pilot courts, at least 500 dispositions and 50 jury verdicts in 1988, 1989, and 1990 in each court, and about 500 cases pending at the end of 1987, and 500 cases pending at mid 1990 in each court. Monthly statistics compiled by the Judicial Council, reports generated by the staff of the courts, judicial and bar questionnaires, personal interviews, and on-site evaluations were also used.

Conclusions Regarding Cost and Delay

- The evaluation generally found that cases in the three-year experimental program had dramatic improvements in processing time, and shortened jury trial length, and that the pending caseload was younger in age.
- Substantial progress can be made in delay and backlog reduction without adding judgeships. However, there are limits to the gains that are possible only through changes in case management, especially if criminal and civil case filings grow.
- Transformation of judges into active case managers was accompanied by two costs: substantial opposition from a significant proportion of the bar, and significant increases in support staff and equipment for the more proactive judiciary. While attorneys generally supported the concepts involved, their assessment of the program's operation was less positive.
- Successful programs had the following critical characteristics: judicial acceptance of responsibility for case progress, a shared goal of disposition within the time standards, and management of the calendar to maximize the certainty of trial dates.
- Leadership is important to a successful reduction of delay.
- State court administration put delay reduction and calendar management into judge and staff education programs.
- A consortium of bar and bench was helpful in improving communications.

- Both pretrial management and trial management are important to program success. Trial-time reductions resulted in additional judicial resources being available for new case-management techniques and for starting other trials on time.
- After three years of program implementation, most judges were supportive of a more intensive case management approach. Almost unanimous agreement existed among program judges that justice did not suffer as a result of increased judicial case management.
- Reductions in backlog generally affected all cases, not just the ones easiest to dispose of.
- Management is made more difficult without good data-gathering systems, for both getting a complete picture of the overall court situation and for the monitoring of an individual case.
- All pilot courts showed significant gains even though there was a mix in the type of calendar systems, the use of early status conferences, the use of mandatory settlement conferences, and the application of sanctions on attorneys. No statistically significant difference was seen regarding the calendaring system's effect on program success. No statistically significant difference existed between courts that employed a status-conference system with scheduling and those that use a pre-defined schedule with periodic checkpoints. No statistically significant relationship existed between time or trial rate and the use of mandatory settlement conferences.
- Arbitration may be a more limited disposition tool than once thought. The effect on cases and courts was not uniform.
- National Center for State Courts' recommendations included:
 - Establishing three tracks (short, standard, or complex) with different time standards, with assignment to each track made by the judge.
 - Not mandating one single calendaring system statewide and not mandating use of status conferences versus rule-based schedules.
 - Identifying criteria for when a case would be better suited for rule-based schedules than for status conferences.
 - Eliminating the statewide requirements for use of the at-issue memorandum (although it may be useful as one tool in a rule-based scheduling court) and the use of mandatory settlement conferences in all cases.
 - Using regular purge programs until dispositions equal filings.
 - Increasing the uniformity of rules of court.

Reexamining the Pace of Litigation in 39 Urban Trial Courts

- Goerdt, John A., Lomvardias, Chris, and Gallas, Geoff, *Reexamining the Pace of Litigation in 39 Urban Trial Courts*, National Center for State Courts, NCSC R-128, Williamsburg, Virginia, 1991

Study Overview

This National Center for State Court report was based on felony and civil case data in 1987 from 39 large urban trial courts and followed closely the methodology used in the studies by Mahoney et al. (1988). The study also looked at trends in processing times from 1983 to 1987 in selected courts.

Conclusions Regarding Cost and Delay

- Most courts have experienced an increase in civil case disposition times between 1976 and 1987. Only the few courts that had implemented a comprehensive delay reduction program had a substantial improvement in the pace of litigation.
- Neither the size of the pending caseload per judge, nor the pace of litigation, could be explained by the number of filings per judge (suggesting that factors other than judicial resources are important in explaining pending caseload size and processing speed).
- When pending caseloads per judge were taken into account, "early court control" over case event scheduling was significantly correlated with shorter processing times (suggesting that effective case management is important).
- In cases that have reached the trial calendar, stricter time goals, lower proportions of tort cases, lower jurisdictional amounts, smaller pending caseloads per judge, and early court scheduling all are moderately correlated with a shorter time to disposition.
- The jury trial rate is not associated with the pace of litigation.
- It is difficult to determine the causal relations between pending caseload per judge, early court scheduling, and the pace of litigation. A large pending caseload per judge may impair a court's ability to set firm trial dates or achieve early resolution of motions. Early control may be more easily established in courts with already more manageable pending caseloads. On the other hand, failure to set firm dates or dispose of motions early in a case's life might increase the pending caseload.

Litigation Dimensions: Torts and Contracts in Large Urban Courts

- Goerdt, John A., Ostrom, Brian J., Rottman, David B., LaFountain, Robert C., and Kauder, Neal B., "Litigation Dimensions: Torts and Contracts in Large Urban Courts," *State Court Journal*, Vol. 19, No. 1, 1995 (special issue).

Study Overview

This special issue of the *State Court Journal* was the outgrowth of a 30-month effort by the National Center for State Courts to collect systematic, broadly based empirical data it felt were necessary for the civil justice reform debate. The collection effort, also known as the Civil Trial Court Network (CTCN), pulled data from 45 of the 75

most populous counties and comprised some 24,155 sample cases (mostly tort, contract, and real property litigation in courts of general jurisdiction). Additional investigation was done on tort cases alone and on all cases disposed of by jury verdict. Although the CTCN data yielded much useful information in which to help answer questions about the nature of tort and contract disputes, the litigants to those disputes, and the role in of the court in dispute resolution, we focus below only on the study's findings that effect cost and delay.

Conclusions Regarding Cost and Delay

- Differences between jurisdictions regarding processing times are multifaceted: court resources vary; the percentage of complex cases differ; some courts are primarily reactive in that they let attorneys dictate when hearing and trial occur while others are more proactive in setting schedules, in denying continuances, and in trial management. Local legal culture (judges' and attorneys' attitudes, values, and practices) plays a part.
- Three large jurisdictions that embarked on extensive civil delay reduction programs in the late 1980s and early 1990s had dramatic reductions in the time to disposition: Wayne County, Michigan, had a median days to jury verdict of 1,150 in 1987 and 584 in 1992; Los Angeles's time went from five years in the mid 80s to just over two years in 1992; and King County, Washington, reduced its median tort case processing time from 449 days (and 31 percent over two years) in 1987 to 304 days (and 8 percent over two years) in 1992. The development of proactive case management programs in Detroit and Seattle, including early scheduling and strict adherence, was responsible for the reduction in median times even though no new judges were added. In Los Angeles, the reduction was achieved by stricter and more effective case management by judges and staff and by using municipal court judges to hear superior court civil trials.

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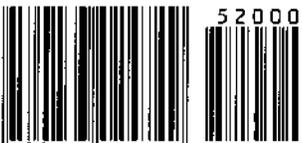
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