DO CASE OUTCOMES REALLY REVEAL ANYTHING ABOUT THE LEGAL SYSTEM? WIN RATES AND REMOVAL JURISDICTION

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ABSTRACT

General Observations on Interpreting Win-Rate Data Properly. Many empirical legal studies use data on plaintiffs' rate of success, because of those data's ready availability and apparent import. Yet these "win rates" are probably the slipperiest of all judicial data. Win rates are inherently ambiguous because of the case-selection effect. The litigants' selection of the cases brought produces a biased sample from the mass of underlying disputes. The settlement process, usually conducted by rational and knowledgeable persons who take into account and thereby neutralize the very factor that one would like to study, produces a residue of litigated cases for which the win rate might indicate nothing more than the percentage of successful plaintiffs in this peculiar and nonrandom sample of cases.

Nevertheless, careful research and theorizing can sometimes tease out an explanation of win-rate data by isolating case-selection effects to reveal meaningful non-case-selection effects, such as the effect of the forum chosen. This artful process of win-rate explanation involves controlling for and otherwise investigating multiple variables to see which of the possible explanations conform to the additional evidence, and then applying a plausibility screen to the surviving explanations.

Specific Study of Removal Jurisdiction. Plaintiffs' win rates in removed cases are very low, compared to cases brought originally in federal court and to state cases. For example, our data reveal that the win rate in original diversity cases is 71%, but in removed diversity cases it is only 34%. In a regression controlling for many case variables, this "removal effect" remains sizable and significant. The explanation for this phenomenon could be the ready one based on the purpose of removal: by defeating the plaintiffs' forum advantage, defendants thereby shift the biases, inconveniences, court quality, and procedural law in their own favor. Alternatively, the

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explanation might lie not in forum impact, but instead in case selection: removed cases may simply be a set of weak cases involving (i) out-of-state defendants who have satisfied or settled all but plaintiffs' weakest cases or (ii) plaintiffs' attorneys who have demonstrated their incompetence by already exposing their clients to removal.

Our analysis indicates that both case selection and forum impact are at work. Thus, forum really does affect outcome, with the removal process taking the defendant to a much more favorable forum.

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Dear Ann Landers: . . . . In order for a father to get custody in this country, the wife has to be physically abusive, a drug addict or a prostitute who has just murdered the mayor of a major city. . . .

—Dallas

Dear Dallas: . . . . Recent studies have found that fathers who fight for custody win sole or at least joint custody in 70 percent of the cases.[1]

INTRODUCTION

Here we go again: Ann uses win-rate data from case outcomes to blow poor Dallas out of the water. Of course, she gives no details of the win-rate studies, thus hiding their weaknesses, limitations, and true meaning from view.[2] Nevertheless, it is obvious that she lumps sole custody with joint custody, thereby mixing clear outcomes with compromises and making any meaningful inference difficult. And

1 Ann Landers, *The Process Is Not Easy for a Single Father or Mother*, ITHACA J., Jan. 21, 1997, at 4C.

2 Not all custody statistics agree with Ann's. *See, e.g.*, WALTER O. WEYRAUCH & SANFORD N. KATZ, AMERICAN FAMILY LAW IN TRANSITION 514 (1983) ("But the publicity connected with isolated cases is not yet reflected in a change of statistics in custody awards, which still favor mothers more than 90 percent of the time.").

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Ann seems to ignore the difference[3] between legal custody, which merely governs the allocation of parental decisionmaking, and the physical custody that concerned Dallas, an aggrieved father and attorney. But most importantly, Ann exhibits total unawareness of case-selection theory.[4] Most fathers might just surrender or settle, so that those fathers who "fight for custody" may be those with cases nearly strong enough to meet one of Dallas's criteria.[5] Thus, Ann's reliance on data is more deceptive than descriptive. And although her unawareness of case-selection theory may not be surprising, her statistic is improbable enough on its face as to suggest a case-selection explanation to almost anyone.

Ann Landers is not at fault alone. Legal commentators are discovering the rich store of empirical studies that legal scholars have begun to create in good number. The commentators skim these for their own empirical "observations," which readily permit unqualified assertions that yield unbending conclusions. For example, not long ago, we reported that medical-malpractice plaintiffs won 50% of judge trials but only 29% of jury trials, along with other like data.[6] We spent fifty-four pages, however, explaining that judges were seeing a different stream of cases than juries were—a stream of cases that were easier for plaintiffs to win. Case selection, not real differences in judge and jury behavior, drove the win-rate data to their suggestive but deceptive extreme. Nevertheless, newspapers reported along these lines:

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3 *See* HARRY D. KRAUSE, FAMILY LAW IN A NUTSHELL 317 (3d ed. 1995); *see also id.* at 296-304 (noting differences in state laws under which custody disputes arise).

4 *See infra* text accompanying notes 21-30. Briefly, one would expect only close cases to survive the settlement process and go to court, with decisions therein being evenly split. Thus, a casebook's concluding note on the role of gender in custody cases is hardly surprising: "A study of all reported custody cases decided by state appellate courts in 1982 found that fathers obtained custody in 51 percent of the cases; mothers in 49 percent." JUDITH AREEN, CASES AND MATERIALS ON FAMILY LAW 522 (3d ed. 1992) (citing Jeff Atkinson, *Criteria for Deciding Child Custody in the Trial and Appellate Courts*, 18 FAM. L.Q. 1, 10 tbl.B (1984)).

5 A scientific study concluded that only 1.5% of custody cases go to judicial decision, and that in settlements physical custody in the mother is almost eight times as frequent as physical custody in the father. *See* ELEANOR E. MACCOBY & ROBERT H. MNOOKIN, DIVIDING THE CHILD: SOCIAL AND LEGAL DILEMMAS OF CUSTODY 137-38 & fig.7.2, 149-51 & tbl.7.6 (1992). As to the closer results in adjudicated cases, the authors observe:

> Even if one assumes that on average mothers in fact have stronger custody claims than fathers, it is certainly plausible to assume that fathers with relatively stronger claims to custody might well constitute a higher proportion of those who continue to fight and reach the top of the conflict pyramid [that is, adjudication]. If fathers without strong claims are more likely to settle earlier in the process, then one would expect a higher proportion of fathers at the top of the conflict pyramid to "win."

Id. at 153.

6 Kevin M. Clermont & Theodore Eisenberg, *Trial by Jury or Judge: Transcending Empiricism*, 77 CORNELL L. REV. 1124, 1137 tbl.3 (1992) [hereinafter Clermont & Eisenberg, *Jury/Judge*].

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"A study published Thursday in the Cornell Law Review concludes that juries are less likely than judges to side with plaintiffs in lawsuits over car accidents, defective products and medical treatment."[7] This view has entered the mainstream of legal writing. The massive and excellent review of the jury system in a very recent issue of the *Harvard Law Review* in part utilized our article thus:

> However, reason exists to question the allegation that juries are responsible for high damages awards. A recent study of federal district court cases adjudicated during fiscal years 1979 through 1989 indicated that plaintiffs won more often before judges than before juries in certain types of frequently litigated tort cases, including product liability cases, medical malpractice cases, and motor vehicle cases.[8]

The ongoing renaissance of empirical studies is catalyzing an explosion of empirical misinformation about the implications of win-rate data. This development is cause for concern. Because win-rate studies form such a prominent part of the recent empirical literature, the misinformation threatens legal reform. Part I of this Article explains the particular genesis of this misinterpretation of win rates. It discusses the role of case selection in producing ambiguity in raw win-rate data. Part II, with new data on removal jurisdiction, illustrates the cure for the widespread misinterpretation of win-rate data. It presents data showing that plaintiffs endure lower win rates in cases removed to federal court than in cases originating in federal court. It then discusses the plausible explanations, analyzes the interpretive pitfall constructed by case selection, and concludes that the most plausible explanation for much of the lowering of win rates in removed cases is independent of case selection. The shift from a favorable forum, chosen by plaintiffs, to a less favorable forum, chosen by defendants, drives down plaintiffs' win rates. Thus, notwithstanding the ubiquitous interpretive problem of case selection, carefully analyzed win-rate data can convey useful information about the legal system.

I
GENERAL OBSERVATIONS ON INTERPRETING WIN-RATE DATA PROPERLY

The popular form of recent empirical studies involves examining the parties' success in federal cases. The reasons for this focus are that data on federal outcomes are readily available and that they appear to be full of meaning. But hidden dangers lurk in these slippery data.

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[7] Reynolds Holding, *Jurors Aren't Pushovers for Plaintiffs, Studies Say*, S.F. CHRON., July 25, 1992, at A6.

[8] *Developments in the Law—The Civil Jury*, 110 HARV. L. REV. 1408, 1492 n.18 (1997).

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A. Nature of the Data

Data gathered by the Administrative Office of the United States Courts, assembled by the Federal Judicial Center, and disseminated by the Inter-university Consortium for Political and Social Research enable study of the outcomes of all cases terminated in federal court. When any civil case terminates in a federal district court, the court clerk transmits to the Administrative Office a form containing information about the case.[9] The form includes data regarding the subject-matter category and the jurisdictional basis of the case; the case's origin in the district as original, removed, or transferred; the amount demanded; the dates of filing and termination in the district; the procedural stage of the case at termination; the procedural method of disposition; and, if the court entered judgment, the identity of the prevailing party and the relief granted. At present, the computerized data set covers fiscal years 1970 through 1995. Thus, it contains all of the millions of federal civil cases litigated over many years from the entire country. In the aggregate, the data appear reliable.[10]

Still, data of such vast coverage, gathered under sometimes confusing instructions, must contain minor *gaps and misclassifications.* Many different people entered the data over an extended period of time, although this dispersion at least would neutralize mistakes and biases. Also, the standards for coding have evolved over time, which necessitates careful attention. Only in fiscal 1979 did the Administrative Office start recording which party prevailed by judgment.[11] In fiscal 1986 it began to indicate meaningfully the citizenship of the two

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9 *See* 11 ADMINISTRATIVE OFFICE OF THE U.S. COURTS, GUIDE TO JUDICIARY POLICIES AND PROCEDURES transmittal 64, at II-18 to -28 (Mar. 1, 1985). For a complete description of Administrative Office data, see INTER-UNIVERSITY CONSORTIUM FOR POLITICAL AND SOCIAL RESEARCH, FEDERAL COURT CASES: INTEGRATED DATA BASE, 1970-1987, ICPSR 8429 (2d ed. Winter 1989 & Supps. 1990, 1992 & 1995) [hereinafter ICPSR]. For easy access to this database, see Theodore Eisenberg & Kevin M. Clermont, *Judicial Statistical Inquiry Form* (last modified Jan. 27, 1997) <http://teddy.law.cornell.edu:8090/questata.htm> [hereinafter Eisenberg & Clermont, *Web Site*], which is discussed in Theodore Eisenberg & Kevin M. Clermont, *Courts in Cyberspace*, 46 J. LEGAL EDUC. 94 (1996).

10 *See* James A. Henderson, Jr. & Theodore Eisenberg, *The Quiet Revolution in Products Liability: An Empirical Study of Legal Change*, 37 UCLA L. REV. 479, 518-20 (1990) (discussing strengths and shortcomings of the Administrative Office data, and concluding that the data produce only minor nonsystematic distortions); Theodore Eisenberg & Stewart Schwab, *The Reality of Constitutional Tort Litigation*, 72 CORNELL L. REV. 641, 677-81 (1987) (comparing the impression conveyed by the Administrative Office data with that conveyed by physical inspection of court records). For a description of possible sources of error in the coding of awards by the Administrative Office, see Theodore Eisenberg et al., *Litigation Outcomes in State and Federal Courts: A Statistical Portrait*, 19 SEATTLE U. L. REV. 433, 439 & n.13 (1996). *Cf.* Jennifer Connors Frasier, *Caught in a Cycle of Neglect: The Accuracy of Bankruptcy Statistics*, 101 COM. L.J. 307 (1996) (discussing inaccuracies in special bankruptcy data).

11 *See* ICPSR, *supra* note 9, at 15.

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principal parties in diversity cases, as well as their corporate or individual status.[12]

Unfortunately, the Administrative Office data do not contain other information one would like to know. They show *no particulars* of each lawsuit. For example, although the Administrative Office form distinguishes among many subject-matter categories, including branches of tort such as medical-malpractice and motor-vehicle cases, it does not distinguish among kinds of claims within the categories. This failing is an important limitation, because outcomes depend heavily on the type of case.[13] One must always control for the case category, but one would always like to control on a finer level.[14]

When working with outcome data, one faces a difficulty in dealing only with *formal wins.* The Administrative Office data set records only formal outcome, as in judgment for plaintiff or defendant. So a formal loss, which may have been worthwhile for the plaintiff because of its deterrent effect or other long-run benefit, counts as a loss. And a formal win, which may have resulted in an unexpectedly small or economically insufficient recovery, still counts as a win. Nevertheless, formal outcomes, especially when averaged over all cases for many years, can tell a researcher quite a bit.

Another difficulty lies in limiting the focus to *technical judgments.* Many grievances are abandoned, claims satisfied, and disputes settled. Most litigated cases settle or terminate short of judgment in some manner that prevents ascertaining the winner. Nevertheless, judgments comprise much more than trial outcomes. For Administrative Office purposes, judgments might be the result of adjudication, consent, or default, although they normally do not include voluntary dismissals or dismissals for lack of prosecution. Again, although the researcher must keep the data's limitations in mind, the study of judgments can yield much information.[15]

Usually, research focuses on outcomes in *trial courts.* The Administrative Office termination data reflect all adjustments of jury verdicts by trial judges, but not changes on appeal. Appeal generally has a

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12 *See id.* at 9.

13 *See* Marc Galanter, *Case Congregations and Their Careers,* 24 L. & SOC'Y REV. 371 (1990); Neil Vidmar, *Making Inferences About Jury Behavior from Jury Verdict Statistics: Cautions About the Lorelei's Lied,* 18 LAW & HUM. BEHAV. 599, 605-08 (1994) (discussing the "apples and oranges problem").

14 *See, e.g.,* PATRICIA M. DANZON, MEDICAL MALPRACTICE: THEORY, EVIDENCE, AND PUBLIC POLICY 38-39 (1985) (reporting that the type of injury in medical-malpractice cases correlates to the plaintiff's chance of winning).

15 *See* Theodore Eisenberg & James A. Henderson, Jr., *Inside the Quiet Revolution in Products Liability,* 39 UCLA L. REV. 731, 744-48 (1992) (analyzing the completeness of judgment data).

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small effect on aggregate trial court outcomes.[16] Yet if necessary, it is possible, albeit unwieldy, to pin down that small effect by linking the trial court data set to the comparable set of appellate court data the Administrative Office gathers.

Furthermore, research in this area tends to consider only cases in *federal courts* because of the richness and availability of this data set. State data are much less accessible. This restriction could introduce a troublesome bias to any study. However, some data from the National Center for State Courts are now available on the Internet.[17] Thus, although the researcher must consider federal and state differences, rough controls are becoming possible.

B. Meaning of the Data

All these concerns aside, the most disturbing difficulty with federal outcome data is that they are inherently ambiguous. For example, win rates differ sharply from data on case duration. One uses case-duration data directly and simply to study how long tried cases last, so that the concern is truly limited to cases actually tried.[18] But one usually uses win-rate data to uncover some underlying factor affecting outcome more generally, such as a substantive or procedural rule or some nonlegal factor favoring one side or the other in the set of all disputes. The classic example is concluding, from differences in win rates among cases tried before judges and juries, something about their decisional behavior that might favor plaintiff or defendant.

Admittedly, win rates can teach some important lessons merely on the descriptive level. For example, in an earlier article, we found that foreign litigants win much more often in federal court than do their domestic counterparts.[19] That result is in itself surprisingly telling. It further permits this statement: "[T]he available data offer no support for the belief that there exists xenophobic bias in American

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16 *See* Marc Galanter, *The Civil Jury as Regulator of the Litigation Process*, 1990 U. CHI. LEGAL F. 201, 224-27 (discussing the fact that "[o]nly a small fraction of jury verdicts are disturbed by appellate courts").

17 *See* Eisenberg & Clermont, *Web Site*, *supra* note 9. Those Web pages utilize a sampling that comprises thousands of state tort, contract, and real property cases terminated during fiscal 1992 in the general jurisdiction court of 45 of the nation's most populous counties. Also, these data are available on CD-ROM from the Bureau of Justice Statistics.

18 *See, e.g.*, Theodore Eisenberg & Kevin M. Clermont, *Trial by Jury or Judge: Which Is Speedier?*, 79 JUDICATURE 176 (1996) (reporting that judge-tried cases last longer on the docket than jury-tried cases). If one uses case-duration data not merely to describe tried cases, but also to explore whether judges or juries actually process cases more quickly, one must worry about whether the effects are attributable to case selection. *See id.* at 179-80.

19 Kevin M. Clermont & Theodore Eisenberg, *Xenophilia in American Courts*, 109 HARV. L. REV. 1120 (1996) [hereinafter Clermont & Eisenberg, *Xenophilia*].

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courts."[20] Direct observations of this sort can dispel widespread misimpressions and more widely open the door to serious inquiry.

However, the observer usually wants to go farther, to understand what is producing the observed win rates and reverberating throughout the universe of all disputes. It is this extra step in dealing with win-rate data—the descent from the descriptive level to the inference level—that leads the observer astray. And it is the case-selection effect—whereby the parties' selection of the cases to litigate produces a biased sample from the mass of underlying disputes—that causes the near-fatal ambiguity.

1. *Inherent Ambiguity*

An established body of literature describes what has come to be called the case-selection effect.[21] In general, this effect refers to the proposition that the parties' selection of litigated or tried cases is not a random sample of the mass of underlying disputes or cases, but instead is a biased sample. More specifically, theorists have tried to describe this biased sample, most often proceeding as follows. Disputes and cases that clearly favor either the plaintiff or the defendant under the law tend to settle readily, because both sides can save costs by settling in light of their knowledge of the applicable law and all other aspects of the case. Difficult cases falling close to the applicable legal criterion tend not to settle, because the parties are more likely to disagree substantially with respect to their predicted outcomes. These unsettled close cases fall more or less equally on either side of the legal criterion, regardless of both the position of that criterion and the underlying distribution of disputes. Thus, even if the legal criterion highly favors plaintiffs, as does strict liability, one should not observe a plaintiff win rate well above 50%. Instead, case selection will leave for adjudication a residue of unsettled cases exhibiting some nonextreme equilibrium win rate.

According to this case-selection effect theory, any distinction between two streams of cases that the parties evaluate without systematic inaccuracy should not lead to a difference in adjudicated win rates. Indeed, under simplifying assumptions, and as a limiting implication, the theory suggests a trial win rate of 50% for both streams.[22]

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20 *Id.* at 1132 (emphasis omitted).

21 *See* Daniel Kessler et al., *Explaining Deviations from the Fifty-Percent Rule: A Multimodal Approach to the Selection of Cases for Litigation*, 25 J. LEGAL STUD. 233, 235-36 (1996); George L. Priest & Benjamin Klein, *The Selection of Disputes for Litigation*, 13 J. LEGAL STUD. 1 (1984). *Compare* Donald Wittman, *Is the Selection of Cases for Trial Biased?*, 14 J. LEGAL STUD. 185 (1985) (criticizing Priest and Klein's model) *with* George L. Priest, *Reexamining the Selection Hypothesis: Learning from Wittman's Mistakes*, 14 J. LEGAL STUD. 215 (1985) (defending the Priest-Klein model).

22 *See* Priest & Klein, *supra* note 21, at 17-20.

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Although refined theoretical work[23] and comprehensive trial data[24] do not support the 50% hypothesis, the underlying insight—that close cases tend to dominate the limited universe of adjudicated cases—is important in analyzing win rates.

In other words, the case-selection effect theory holds that win rates reveal something about the set of adjudged cases, and not much about the underlying mass of disputes and cases. But it does not predict any universal win rate, or that two streams' rates will be the same. What factors might lead to win rates different from 50%? There are three types of such factors.

First, different stakes to the parties is the most common explanation of win rates that depart from the idealized predictions of the case-selection effect theory.[25] The doctor whose reputation might suffer from an unfavorable judgment may have more to lose than the dollars that one plaintiff seeks. The company defending a product-liability action may have more at stake than the money sought from it in the particular case. Such differential stakes may make defendants more willing to settle the stronger cases, and so lead to plaintiffs' win rates that are lower than either 50% or whatever other level one expects absent the differential. Similarly, greater stakes to plaintiffs may raise their win rates.

Many other factors are of this contextual type, in that they all constitute real-world complications that alter the economic model's simplified assumptions and consequently its purified predictions.[26] Therefore, for a stream of cases in which the main dispute concerns damages, so that liability is a given, the formal win rate obviously would increase.[27] Similarly, differences in the two parties' access to

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23 *See* Steven Shavell, *Any Frequency of Plaintiff Victory at Trial Is Possible*, 25 J. LEGAL STUD. 493 (1996).

24 *See* Theodore Eisenberg, *Testing the Selection Effect: A New Theoretical Framework with Empirical Tests*, 19 J. LEGAL STUD. 337 (1990).

25 *See* Theodore Eisenberg, *Litigation Models and Trial Outcomes in Civil Rights and Prisoner Cases*, 77 GEO. L.J. 1567, 1579, 1581-82 (1989); Priest & Klein, *supra* note 21, at 24-29, 40; *cf.* Donald Wittman, *Dispute Resolution, Bargaining, and the Selection of Cases for Trial: A Study of the Generation of Biased and Unbiased Data*, 17 J. LEGAL STUD. 313, 341-45 (1988) (providing a more complete model). *See generally* Kathleen Engelmann & Bradford Cornell, *Measuring the Cost of Corporate Litigation: Five Case Studies*, 17 J. LEGAL STUD. 377 (1988) (studying asymmetrical stakes more generally); Samuel R. Gross & Kent D. Syverud, *Getting to No: A Study of Settlement Negotiations and the Selection of Cases for Trial*, 90 MICH. L. REV. 319 (1991) (broadening consideration to include parties' strategic behavior). The differential stakes analysis also applies to litigants' different estimates of the size of judgment and different attitudes toward risk.

26 *See* Clermont & Eisenberg, *Jury/Judge*, *supra* note 6, at 1130-33; Theodore Eisenberg & Henry S. Farber, *The Litigious Plaintiff Hypothesis: Case Selection and Resolution*, 28 RAND J. ECON. S92, S109-11 (1997) (special issue); Kessler et al., *supra* note 21, at 237-48.

27 *See* Priest, *supra* note 21, at 228-29; J. Mark Ramseyer & Minoru Nakazato, *The Rational Litigant: Settlement Amounts and Verdict Rates in Japan*, 18 J. LEGAL STUD. 263, 284-85 (1989).

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information and competence in forecasting would also affect the win rate.[28] Or, if the two streams of cases under study differ in costs of litigating or size of potential award, win rates would not equalize.[29]

Second, another type of powerful explanation of aberrant win rates is the parties' mutual misperceptions about the prevailing standard of decision.[30] If the parties perceive the adjudicator to be favorable to the plaintiff, but the adjudicator turns out not to be, then the apparently close cases would turn out to be losers and the win rate would drop. Similarly, if the adjudicator appears to be neutral, but turns out to be unfavorable to the plaintiff, then the win rate would drop. Imagined or unperceived biases of the adjudicator therefore affect win rate.

The direction of this effect on win rate is opposite to the misperception. An elevated win rate might not mean that plaintiffs have an advantage, but merely that plaintiffs are not as disadvantaged as the parties think. A change in win rate might therefore mean almost the opposite of what it seems to mean. This complexity adds a cruel twist to win-rate data's inherent ambiguity.

Third, strength of the case is a factor different in kind from both the contextual factors and misperception. This factor draws on the reassuring thought that a stream of stronger claims should have a higher win rate than a stream of weaker claims.[31] The claims' strength could lie in favorable facts, in an easy legal criterion, or in a biased adjudicator. In other words, the case-selection effect is merely a tendency to remove meaning from outcome data, but it may not do so completely. Thus, for example, our earlier work found that transfer of venue out of a forum favorable to the plaintiff results in a lowered win rate.[32]

The case-strength factor tends to diminish as the settlement process progresses, so that in trial data this factor has largely, but not completely, disappeared. Thus, it is normally dangerous to work only with trial data. At earlier termination stages, the case-strength factor enjoys greater influence, allowing one to draw inferences with greater confidence.[33] But generally, the case-strength factor is at work throughout the litigation process. Thus, a strong positive correlation

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28 *See* Kessler et al., *supra* note 21, at 242-43; Wittman, *supra* note 25, at 325-27.

29 *See* Kessler et al., *supra* note 21, at 246, 255; Wittman, *supra* note 25, at 335-37.

30 *See* Clermont & Eisenberg, *Jury/Judge, supra* note 6, at 1131-32, 1156-57, 1170-72 (discussing the role of attorneys' misperception of both jury performance and the "adjudicator's standard of decision").

31 *See* Kessler et al., *supra* note 21, at 244-45; Joel Waldfogel, *The Selection Hypothesis and the Relationship Between Trial and Plaintiff Victory,* 103 J. POL. ECON. 229, 232-35 (1995).

32 Kevin M. Clermont & Theodore Eisenberg, *Exorcising the Evil of Forum-Shopping,* 80 CORNELL L. REV. 1507 (1995) [hereinafter Clermont & Eisenberg, *Transfer*].

33 *See, e.g.,* Eisenberg et al., *supra* note 10, at 445-47 (reporting that the downward time trend in product-liability win rates is observable in earlier stages but not at trial).

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exists between win rates on pretrial motion and at trial.[34] Also, it seems that win rates for settlements correlate with those for judgments.[35] In sum, one sees what may be called a "refraction effect" as the win rate at earlier stages closes in on some nonextreme equilibrium trial win rate. Although the trial win rate may not convey much meaning by itself, its meaning becomes clearer as one tracks back to the mass of underlying cases and disputes.

2. *Residual Meaning*

That last type of factor—case strength—is tantalizing, because it hints that win-rate data can have straightforward implications. Win rates may, after all, retain residual meaning, which case selection with all its qualifications has not obliterated. The challenge is to tease out the residual meaning in win-rate data by isolating the case-selection effects from the case-strength factor, a challenge at which careful research and theorizing can often succeed.

The first step involves regression. Multivariate regression operates to segregate the independent effects of several variables, such as time trend and case category, on win rates.[36] The dependent variable—what we are trying to explain—is whether the judgment is a win or a loss for the plaintiff. The regression should use a broad set of independent variables—factors that may affect the win rate—as controls. This technique helps to ensure that any comparison of win rates rests on kinds of cases that are as similar as possible.

The second step involves formulating the possible explanations of the observed phenomenon and then testing them by investigating additional variables. For example, if a possible explanation of a low success rate is inept counsel, one might compare win rates for corporate and individual plaintiffs to see if the observed effect is more pronounced for individuals with their possibly less effective counsel. This process can eliminate many possible explanations.

The third step involves the application of a plausibility screen to the surviving explanations. Some will simply make more sense than others, fitting better within the framework of accumulated experience

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34 *See* Theodore Eisenberg, *The Relationship Between Plaintiff Success Rates Before Trial and at Trial*, 154 J. ROYAL STAT. SOC'Y ser. A, pt. 1, at 111 (1991).

35 *See* Eisenberg & Henderson, *supra* note 15, at 744-45, 756-58; Theodore Eisenberg, *Negotiation, Lawyering, and Adjudication: Kritzer on Brokers and Deals*, 19 L. & SOC. INQUIRY 275, 292-93 & n.64 (1994) (review essay).

36 Multivariate regression is a statistical technique that quantifies the influence of each of several factors (independent variables) on the phenomenon being studied (dependent variable). *See generally* MICHAEL O. FINKELSTEIN & BRUCE LEVIN, STATISTICS FOR LAWYERS 323-467 (1990) (applying regression analysis to various legal issues). Because the dependent variable in this study is dichotomous (judgment for plaintiff or defendant), we use logistic (rather than ordinary least squares) regression. *See* DAVID W. HOSMER, JR. & STANLEY LEMESHOW, APPLIED LOGISTIC REGRESSION 1 (1989).

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and knowledge. For example, higher medical-malpractice win rates in judge-tried cases than in jury-tried cases probably do not mean that judges are more sympathetic to personal-injury plaintiffs than juries; instead, case selection is the more plausible explanation.[37] Reliance on experience and knowledge may not sound too scientific. But in fact, the prior two steps are less rigorous than they may appear initially.

Our point is that this form of analysis is as much art as science—and it is a difficult and subjective art. For example, one would surely be disposed to accept the first plausible explanation, as one works through the straightforward explanations of case strength and then proceeds to wrestle with the more indirect case-selection contextual explanations or to resort to convoluted explanations based on parties' misperceptions. Caveat emptor accordingly applies to this form of art.

Therefore, the consumer of empirical research should verify that the researchers had no axe to grind, that they truly immersed themselves in the data, and that they explained their investigatory and reasoning processes in detail.[38] All this requires time and effort from both the user and the researchers. But art and science demand no less.

II
SPECIFIC STUDY OF REMOVAL JURISDICTION

To illustrate the interpretive techniques discussed above, this Part empirically analyzes an important source of federal cases—those arriving in federal court via removal jurisdiction.[39] Removal jurisdiction is supposed to affect outcome. For example, in diversity cases, a plaintiff suing in state court typically chooses that court to maximize its advantage. Removal jurisdiction allows the out-of-state defendant to remove the case from the presumably more biased state forum chosen by the plaintiff to a more neutral federal forum.[40] It is hard to imagine that the plaintiff's choice of forum has no effect, and it is equally unlikely that allowing the removing defendant to undo the plaintiff's initially unfettered choice of forum has no effect.[41] Thus, this process should affect outcome. But can we observe the intended removal effect by observing win rates? And if win-rate differences emerge between re-

37 *See* Clermont & Eisenberg, *Jury/Judge*, *supra* note 6, at 1174.

38 *See* CYNTHIA CROSSEN, TAINTED TRUTH: THE MANIPULATION OF FACT IN AMERICA 237-38 (1994); Vidmar, *supra* note 13, at 614-15.

39 *See* 28 U.S.C. § 1441 (1994). *See generally* 14A CHARLES ALAN WRIGHT ET AL., FEDERAL PRACTICE AND PROCEDURE §§ 3721-3740 (2d ed. 1985 & Supp. 1997) (explaining law of removal).

40 *See* JACK H. FRIEDENTHAL ET AL., CIVIL PROCEDURE 55 (2d ed. 1993).

41 *See* Clermont & Eisenberg, *Transfer*, *supra* note 32, at 1514-25.

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moved cases and other cases, how should one interpret them, mindful of the pitfall constructed by case selection?

This Part first presents empirical evidence of a "removal effect," and then works through several indirect explanations for the effect based on case selection. It concludes that the soundest interpretation of the removal effect is the straightforward one: a shift to an unfavorable forum depresses the plaintiffs' win rate, even after accounting for case-selection forces.

A. The Numbers

Removal of civil cases from state to federal court results in a precipitous drop in the plaintiffs' win rate. As we have previously reported, the overall win rate in federal civil cases is 57.97%, but in the subset of those cases that have been removed the win rate is only 36.77%.[42] Apparently, the defendants' ability to choose the forum greatly augments their odds of success.

A new and closer comparison of federal-court win rates in original proceedings and in removed cases tells a similar, but more subtle, story. Using the Administrative Office data for civil cases terminated during fiscal years 1987 to 1994, Table 1 compiles cases terminated only by judgment for plaintiff or for defendant, not by judgment for both or for party unknown, and not by a nonjudgment such as a remand order. "Win rate," to be precise, is the fraction of plaintiff wins among judgments for either plaintiff or defendant. The rows in Table 1 distinguish among the several bases for federal jurisdiction. The columns isolate the stage of procedural progress at the time of termination: "early" means judgment entered before filing of an answer or before any court action; "trial" means judgment entered during or after trial; and "middle" comprises terminations following court action after filing of the answer but before commencement of trial. The pair of columns labeled "Total Win Rate" give the cumulative figures, across all procedural stages, for each jurisdictional basis. For example, 73,131 diversity cases reaching definitive judgment originated in the reporting federal court and 15,576 such diversity cases reached that federal court by removal from state court.

The last pair of columns in Table 1 show that in diversity cases, the win rate drops from 71% in original cases to 34% in removed cases. This highly significant drop is of major importance, as removed cases account for 17% of the diversity judgments in the data set. The removal effect is not just a diversity phenomenon, however. For example, federal question cases, excluding prisoner litigation, show an

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42 *Id.* at 1512 & 1514 n.18 (reporting data from 1979-1991). The newer data in Table 1 indicate a 53% win rate in all original cases and a 33% win rate in all removed cases.

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TABLE 1
PLAINTIFFS' WIN RATES IN ORIGINAL AND REMOVED FEDERAL CIVIL CASES, FY 1987-1994
DISTINGUISHED BY JURISDICTIONAL BASIS AND PROCEDURAL PROGRESS
(NUMBER OF CASES IN PARENTHESES)

Procedural Progress	Early Win Rate		Middle Win Rate		Trial Win Rate		Total Win Rate	
Jurisdictional Basis	Original	Removed	Original	Removed	Original	Removed	Original	Removed
U.S. as plaintiff	.9847 (111,192)	.8988** (336)	.9143 (27,103)	.8711** (745)	.7116 (2,621)	.7808 (73)	.9661 (140,916)	.8735** (1,154)
U.S. as defendant	.1436 (20,307)	.2739** (1,285)	.1956 (42,440)	.3437** (2,083)	.3840 (4,083)	.5176** (284)	.1913 (66,830)	.3327** (3,652)
Federal question (prisoner & nonprisoner)	.3449 (103,117)	.2615** (2,195)	.2173 (97,849)	.2144 (7,769)	.3522 (25,586)	.3916** (1,762)	.2906 (226,552)	.2499** (11,726)
Federal question (nonprisoner only)	.6895 (49,882)	.2756** (2,061)	.3879 (50,993)	.2172** (7,628)	.4316 (19,106)	.3939* (1,749)	.5202 (119,981)	.2548** (11,438)
Diverse citizenship	.8775 (31,932)	.3244** (1,788)	.5991 (26,643)	.2556** (8,280)	.5558 (14,556)	.4710** (5,508)	.7121 (73,131)	.3396** (15,576)
Local question	.8344 (157)	NA (0)	.8108 (185)	.5714* (21)	.7465 (71)	.0000 (1)	.8087 (413)	.5455* (22)

* Significant at <0.05 by two-sided Fisher's exact test; ** Significant at <0.0005.

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analogous drop in win rate from 52% to 25%.[43] Indeed, all the jurisdictional bases show the removal effect, once appropriate controls are introduced.[44] Moreover, the removal effect appears at every stage of procedural progress.[45] Nevertheless, with respect to what a comparison of original and removed win rates really means, a couple of obvious questions present themselves.

Why compare win rates in original and removed cases only in federal court? We do so because the federal data are available. Nevertheless, we recognize that, to some extent, the meaningfulness of the comparison depends on the assumption that the win rate in original federal cases approximates the win rate in comparable state cases. Although state data are less accessible, prior studies have indicated that federal and state win rates are about the same.[46] Also, the state data from the National Center for State Courts, now available on the Internet,[47] permit the compilation of Table 2. It compares, across only those case categories that are roughly comparable, federal and state win rates for original cases terminated by adjudication in fiscal year 1992. The federal cases include judgments by pretrial motion or at trial from July 1, 1991, to September 30, 1992.[48] The state cases include judgments by summary judgment or at trial from July 1, 1991, to June 30, 1992.

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43 The removal effect in the whole set of federal question cases almost fades from view in Table 1 because of the heavy dose (45%) of cases brought by prisoners (case category nos. 510-550). These prisoner cases are virtually all original proceedings in federal court and are seldom successful, thus seriously depressing the original win rate to 29%. Table 3, which reports regressions that control for case category, accordingly reveals the true and strong removal effect for all federal question cases.

44 Note, for example, the jurisdictional basis "U.S. as defendant." Although the raw numbers in Table 1 suggest that removal increases the win rate from 19% to 33%, the regression in Table 3 reveals the usual negative effect of removal on win rate, although it is an insignificant one. The misleading increase seen in the raw numbers is another category artifact, as most prominently shown by foreclosure cases: most federal foreclosure cases against the United States have been removed, so that 18% of removed cases against the United States are foreclosure cases (with an 80% win rate), while 0.5% of original cases against the United States are foreclosure cases (with an 85% win rate).

45 *See infra* Table 4, Panel C.

46 *See* Clermont & Eisenberg, *Xenophilia*, *supra* note 19, at 1122 n.10; Eisenberg et al., *supra* note 10, at 434-38 (reporting that win rates in jury trials in federal and state courts are "strikingly similar").

47 *See supra* note 17 and accompanying text.

48 Beginning in fiscal 1992, the end of the federal fiscal year shifted from June 30 to September 30. Therefore, the Administrative Office data for fiscal 1992 covers 15 months rather than the normal 12 months.

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TABLE 2
PLAINTIFFS' WIN RATES IN ORIGINAL FEDERAL AND STATE ADJUDICATED CIVIL CASES, FY 1992
DISTINGUISHED BY COMPARABLE CASE CATEGORY
(NUMBER OF CASES IN PARENTHESES)

Case Category [federal/state category codes]	Federal	State
Contract actions [110-190/21,22,24,29]	.6357 (4,291)	.7064* (419)
Condemnation [210/31]	.5692 (130)	.5714 (21)
Foreclosure [220/23]	.9447 (760)	.9350 (123)
Other real property actions [290(diversity)/39]	.4528 (53)	.5926 (27)
Intentional torts and defamation [320/6,9]	.2105 (95)	.3182 (44)
Motor vehicle [350/1]	.5027 (547)	.5425 (365)
Medical malpractice [362/7]	.2276 (290)	.1630 (92)
Product liability [195,245,315,345,355,365,385/3]	.2326 (516)	.3095 (42)
Other tort actions [240,310,340,360,368,371,380/2,8,10,15,19]	.3552 (1,740)	.3624 (334)
Fraud [370/20]	.4415 (188)	.5476 (42)

* Significant at <0.05 by two-sided Fisher's exact test; ** Significant at <0.0005.

Table 2 shows that federal and state win rates track each other fairly closely, despite the incompatibilities of data-gathering techniques and the small sample sizes. Only the broad category of contract actions—where, for instance, the subcategory of Miller Act cases in federal court[49] might not match up so well with the state subcategory of so-called seller suits—exhibits significantly different win rates. One cannot reject the hypothesis that state win rates are at least as high as win rates in original federal cases. We therefore can defensibly use exclusively federal data to describe the removal effect in terms of a comparison between original and removed cases.

But does the set of original cases differ from the set of removed cases? Yes, of course. For example, Table 1 shows that, compared to original cases, removed cases include few easy and early plaintiff wins. Defendants who resist by removing cases are less likely than the mass of nonremoving defendants to allow defeat at an initial procedural stage. By itself, this difference creates a removal effect of depressed

49 40 U.S.C. §§ 270a-270f (1994) (requirement of surety bonds for public construction contracts).

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win rates. Also, the case-category mix differs between original and removed dockets. Therefore, we need to employ a statistical technique that controls for these differences between original and removed cases.

Regression analysis allows us to isolate these differences while controlling for several other known factors.[50] But a single regression is inappropriate for our data. Table 1 suggests that the impact of removal varies substantially according to the basis for federal jurisdiction. For example, the drop in win rates is much larger in diversity cases than in cases in which the United States is plaintiff. We therefore conducted separate regressions for each jurisdictional basis. In the interest of space, we report the full regression results for only one jurisdictional basis—diversity. We then report only the size and statistical significance of the removal effect for the other jurisdictional bases.

Panel A of Table 3 thus reports the full regression results for diversity cases. The dependent variable is judgment for plaintiff and the independent variables include dummy variables for case category, procedural progress, disposition method, and judicial circuit, as well as continuous variables for termination year and amount demanded.[51] The independent variable of principal interest is the dummy variable for removal. It equals one for cases that came to the reporting federal court via removal and zero for cases originally filed in that federal court. The coefficient for removal from this basic regression gives a measure of the separate effect of removal on win rate, holding the other variables constant. Panel A shows a sizably negative and statistically significant coefficient (–0.711) for removal.[52]

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50 *See supra* note 36.

51 We included only cases from categories with five or more removed cases. Controlling for amount demanded acts to hold the merits constant to the extent that demand correlates with merit. *See* Clermont & Eisenberg, *Transfer, supra* note 32, at 1518. The amount demanded is missing from the Administrative Office data in many cases, so we repeated the analysis by excluding the demand variable and including those cases with missing demand amounts. The results do not differ substantially from those reported here.

Later, we further control for the additional variables of the parties' corporate or individual status and their citizenship. *See infra* text accompanying notes 67-71.

52 We explored regressions using many subsets of the explanatory variables reported in Panel A of Table 3. In particular, we explored regression models omitting the procedural-progress and disposition-method variables because of concerns about their close correlation with case outcome. All of these regressions lead to a sizable, significant, and negative removal coefficient. The removal effect is not an artifact of any particular choice of independent variables.

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TABLE 3
REMOVAL EFFECT IN FEDERAL CIVIL CASES, FY 1987-1994

A. Logistic Regression Results for Diversity Cases

Independent Variable	Coefficient	Robust Standard Error
Removal	-.711	.032**
Case category dummies	not separately reported	
Procedural progress (early stage = reference)		
Middle stage	.158	.036**
Trial stage	-.199	.075*
Disposition method (default judgment = reference)		
Consent judgment	-1.095	.104**
Pretrial motion	-4.195	.086**
Trial method	-2.816	.110**
Other method	-3.029	.096**
Judicial circuit dummies	not separately reported	
Termination year	-.038	.005**
Amount demanded	-.023	.004**
Constant	79.175	10.374**

$\chi^2(46)$ = 10,299.35; prob. $>\chi^2$ = 0.0000

log likelihood = -20,641.296; pseudo r-squared = 0.4050; number of cases = 55,875

* Significant at <0.05; ** Significant at <0.0005.

B. Regression Coefficients for Removal Variable by Jurisdictional Basis

Jurisdictional Basis	Coefficient	Significance
U.S. as plaintiff	-.207	<.249
U.S. as defendant	-.128	<.293
Federal question (prisoner & nonprisoner)	-.844	<.0005
Federal question (nonprisoner only)	-.832	<.0005
Diverse citizenship	-.711	<.0005
Local question	NA	NA

Panel B of Table 3 presents the coefficients for the removal variable from a series of logistic regressions—one for each jurisdictional basis—similar to that reported in Panel A. Panel B shows that a consistent removal effect survives regression: a negative coefficient signifies that removal lowers plaintiffs' win rate. That is, while the raw numbers in Table 1 suggest the existence of a removal effect, Table 3 tends to prove that the effect is not an illusion, because the effect survives when the various case characteristics are held constant. This removal effect is significant and sizable in the ordinary cases in which jurisdiction is based on diversity of citizenship or a nonprisoner federal question. In fact, the coefficients permit estimation of the magnitude of the removal effect.[53] Compared to an original diversity or nonprisoner federal question case with a 50% chance of a plaintiff victory, an apparently identical but removed

53 For those two jurisdictional bases, the logistic regression coefficients translate into odds multipliers of 0.491 and 0.435, respectively. For an explanation of converting logistic regression coefficients into odds multipliers, and of the meaning of odds multipliers, see Clermont & Eisenberg, *Xenophilia, supra* note 19, at 1132 n.25.

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diversity case has only a 33% chance, while a removed nonprisoner federal question case has a 30% chance.

B. The Explanations

1. *Forum Impact*

The apparent explanation of the removal effect is that forum matters. By removal, the defendant defeats the plaintiff's forum advantage, inducing such changes as dislodging the plaintiff's lawyer from a familiar and favored forum, and more generally reversing the various biases, costs and other kinds of inconveniences, disparities in court quality, and differences in procedural law that led the plaintiff to prefer state court.[54] So, removed cases have lower win rates than those in which the plaintiff chooses the forum, whether the plaintiff elects state or federal court.

This impact of forum should not be a surprising one. After all, removal jurisdiction is intended to favor the removing defendant.[55] "It is quite an anomalous jurisdiction, giving a defendant, sued in a court of competent jurisdiction, the right to elect a forum of its own choosing."[56] Indeed, removed cases comprise those cases in which forum matters most, or at least those in which the parties agree in thinking that forum matters most, because removed cases are those in which both sides have tried to forum-shop.

The considerably increased advantages for the defendants should show up in the data. Under federal law, certain pending state cases are subject to immediate and unilateral removal by the defendants, without any role for the settlement process during the removal process. After removal, however, the parties will settle or litigate subject to the real or perceived differences of the federal forum. Because those differences favor defendants, removal should lower the plaintiffs' win rate. Therefore, because removal temporarily trumps the settlement process, the observed removal effect should be at least partly

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54 These are the principal considerations that might affect the choice between a federal or a state forum, according to empirical studies of attorneys' preferences. *See* Kristin Bumiller, *Choice of Forum in Diversity Cases: Analysis of a Survey and Implications for Reform*, 15 L. & SOC'Y REV. 749 (1981); Victor E. Flango, *Litigant Choice Between State and Federal Courts*, 46 S.C. L. REV. 961 (1995); Jerry Goldman & Kenneth S. Marks, *Diversity Jurisdiction and Local Bias: A Preliminary Empirical Inquiry*, 9 J. LEGAL STUD. 93 (1980); Thomas B. Marvell, *The Rationales for Federal Question Jurisdiction: An Empirical Examination of Student Rights Litigation*, 1984 WIS. L. REV. 1315; Neal Miller, *An Empirical Study of Forum Choices in Removal Cases Under Diversity and Federal Question Jurisdiction*, 41 AM. U. L. REV. 369 (1992); Jolanta Juszkiewicz Perlstein, *Lawyers' Strategies and Diversity Jurisdiction*, 3 LAW & POL'Y Q. 321 (1981); Marvin R. Summers, *Analysis of Factors That Influence Choice of Forum in Diversity Cases*, 47 IOWA L. REV. 933 (1962); Note, *The Choice Between State and Federal Court in Diversity Cases in Virginia*, 51 VA. L. REV. 178 (1965).

55 *See supra* text accompanying notes 40-41.

56 CHARLES ALAN WRIGHT, LAW OF FEDERAL COURTS 223 (5th ed. 1994).

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the result of the change of forum, rather than merely an illusion explainable by case-selection effect theory.

What particular aspects of the forum actually cause the removal effect? The leading candidates are real or perceived differences in bias, inconvenience, court quality, and procedural law. With the data available, it is not possible to pin down the precise causes, but we can make a few observations.

On the one hand, some aspects of forum have less than the perhaps expected impact. For example, the significant change of law upon removal[57] is the shift from state procedure to federal procedure, but only in those states whose post-pleading process differs from the model of the Federal Rules of Civil Procedure.[58] To explore the role of a shift in governing procedure, we ran separate regressions, each similar to that described in Panel A of Table 3, for the Federal Rules states and for the non-Federal Rules states. These regressions reveal no great difference in the removal effect, as Panel A of Table 4 documents. Although the removal effect for nonprisoner federal-question cases is somewhat bigger in non-Federal Rules states than in Federal Rules states, the removal effect shows slightly the opposite trend for diversity cases. This result suggests that forum-shopping for more favorable law is not a major factor in producing the removal effect.[59]

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57 *Cf.* Clermont & Eisenberg, *Transfer*, *supra* note 32, at 1514 & n.17 (explaining the change of law upon transfer).

58 We relied on John B. Oakley & Arthur F. Coon, *The Federal Rules in State Courts: A Survey of State Court Systems of Civil Procedure*, 61 WASH. L. REV. 1367, 1378-1424 (1986), to select California, Connecticut, Florida, Illinois, Iowa, Louisiana, Maryland, Michigan, Missouri, Nebraska, New Hampshire, New Jersey, New York, Oregon, Pennsylvania, Texas, Virginia, and Wisconsin as the 18 non-Federal Rules states; incidentally, we similarly treated the territorial courts of Guam, the Northern Mariana Islands, Puerto Rico, and the Virgin Islands. This fairly small group of jurisdictions is relatively populous, accounting for 62% of the federal-question judgments and 64% of the diversity judgments in the data set.

59 *Cf.* Miller, *supra* note 54, at 391-92 (stating that special federal procedures, such as class-action certification or transfer of venue, are seldom used in removed cases). *But cf. id.* at 418-20 (reporting that the survey indicated that defense counsel prefer the federal summary judgment rule and practice).

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TABLE 4
REMOVAL EFFECT IN FEDERAL CIVIL CASES, FY 1987-1994
DISTINGUISHED BY STATE PROCEDURAL MODEL,
TRIAL MODE, AND PROCEDURAL PROGRESS

Jurisdictional Basis	Logistic Regression Coefficient		
A. State Procedural Model	FRCP		nonFRCP
Federal question (nonprisoner)	-.771**		-.841**
Diverse citizenship	-.699**		-.697**
B. Mode of Trial	Judge		Jury
Federal question (nonprisoner)	-.390*		-.087
Diverse citizenship	-.472**		-.161*
C. Procedural Progress	Early	Middle	Trial
Federal question (nonprisoner)	-1.132**	-.956**	-.233*
Diverse citizenship	-.859**	-1.153**	-.256**

* Significant at <0.05; ** Significant at <0.0005.

On the other hand, some preconceptions seem to hold up quite well, at least at first glance. For example, many plaintiffs' lawyers fear antipathy or other characteristics of federal judges.[60] Separate regressions for judgments after judge trial and those after jury trial reveal a much greater removal effect for judge-tried cases, as Panel B of Table 4 shows. Removed plaintiffs fare relatively worse before judges than before juries.

When we recall that Panel B of Table 4 includes only completed trials, the results appear more startling. By the time of trial, one would expect the settlement process to have largely neutralized the impact of the trier.[61] Could it be that the lawyers are underestimating the magnitude of federal judges' antipathy to plaintiffs, thus leaving a fairly pronounced removal effect in judge-tried cases? The more likely explanation is that the lawyers are overestimating the level of antipathy of federal juries to plaintiffs,[62] thus relatively raising the plaintiffs' win rate in removed jury-tried cases.

From this theorizing flows a renewed observation that the removal effect exists at all stages of procedural progress. Panel C of Table 4 reinforces this point. The ongoing settlement process might reduce, but does not eliminate, the removal effect, even at trial.

60 *See* Flango, *supra* note 54, at 972-74. An anecdote of some celebrity appears in JONATHAN HARR, A CIVIL ACTION (Vintage Books 1996). That book recounts the protracted Woburn toxic-tort litigation, in which the defendants dealt the plaintiffs their critical setback by removing the case. A defendant's lawyer reasoned that the "federal courts . . . had a generally higher caliber of judges whose tolerance for personal injury cases of questionable merit was correspondingly lower." *Id.* at 99.

61 *See* Clermont & Eisenberg, *Jury/Judge*, *supra* note 6, at 1128-30.

62 *See* Miller, *supra* note 54, at 420-23 (explaining that attorney perception of state and federal juries is a big factor in state and federal court preferences of plaintiffs' and defendants' lawyers).

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Therefore, the advantages to the defendants of forum-shopping seem to be real. Moreover, the parties' bargaining in the settlement process might not fully take the defendants' advantages into account. Even in tried cases, defendants fare better in removed cases than the parties apparently expect. Although the parties clearly think that forum matters, in fact, forum—with all its implications of bias and inconvenience shifted in favor of defendants—may matter even more than they believe.

Finally, it bears repeating that this study concerns only judgments, and not the mass of settlements. Judgments include some settlements in the form of consent and default judgments, but most settlements do not produce a judgment. The actual or potential improvement in defendants' position upon removal presumably would also manifest itself in these nonjudgment settlements and other dispute resolutions.[63] Thus, the removal effect seen in judgments has a much broader importance with regard to the universe of all cases and disputes.

2. *Case Selection*

Before embracing the impact of forum as the explanation of the removal effect, however, one must more seriously consider the possibility that the parties' selection of cases for removal yields a set of weak cases compared to original federal proceedings, and thus a lower win rate. That is, maybe the removal effect represents nothing more than the impact of case selection.

The regressions served to avoid the most obvious case-selection explanations, such as the fact that removed cases include fewer easy, early plaintiff wins. But there may be further case-selection processes that could explain away even the purified removal effect left after the regressions. The possibility exists that removable cases have certain characteristics that correlate with weak plaintiffs' cases, or it could be that the defendants' selection of cases to remove among the set of potentially removable cases signals weak plaintiffs. Systematically thinking through the various factors that can cause two streams of

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63 *Cf.* Clermont & Eisenberg, *Xenophilia, supra* note 19, at 1139-40 (discussing the influence of foreigner strength on nonjudgment resolutions). Some proof of this point lies in the observation that, although removed cases constitute a substantial proportion of judgments, they constitute an even higher percentage of the docket. Removed cases account for 17% of the diversity judgments in the data set, *see supra* text accompanying note 43, but they account for 25% of diversity cases if that data set is not limited to judgments. Apparently, many removed cases produce settlements, probably often in the removing defendants' favor.

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cases' win rates to depart from equality[64] narrows the focus to differences in asymmetric aversion to litigation and quality of counsel.

First, and most plausibly, removable cases might involve defendants averse to litigation who therefore defend only their strongest cases. Removed cases often involve out-of-state or foreign defendants, especially in diversity cases.[65] The plaintiffs probably chose the initial forum for geographic advantage. Because these defendants would be averse to litigation away from home, they would satisfy or settle all but their strongest cases. This tendency would elevate their success rate, and hence lower the plaintiffs' win rate.[66] This could be a powerful explanation of the removal effect.

We therefore must separate the impact of this locale aversion from the forum impact in producing the removal effect. We can approach this task by looking at diversity cases exclusively, because the diversity data after 1986 permit distinguishing the parties' citizenship, as well as the parties' corporate or individual status.[67] We should first

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64 *See supra* text accompanying notes 21-30. The other case-selection factors commonly involved, such as differences between streams of cases with respect to the parties' relative stakes in the outcome or access to information, seem not to apply to the original and removed streams of cases.

65 Specific to removal in diversity cases is the bar to removal by in-state defendants. *See* 28 U.S.C. § 1441(b) (1994). We therefore know that the set of removed diversity cases involves out-of-state or foreign defendants.

66 *See* Clermont & Eisenberg, *Xenophilia*, *supra* note 19, at 1133-35, 1142-43 (reporting that foreign and out-of-state plaintiffs and defendants win more than their domestic counterparts).

67 We can use a similar technique to fortify our earlier article's conclusion that a change of forum caused the "transfer effect" of depressed win rates after transfer of venue. Clermont & Eisenberg, *Transfer*, *supra* note 32, at 1524-25. We had to qualify that conclusion in these terms:

> [A] powerful case-selection explanation would be that transfer cases involve defendants particularly averse to litigation in the plaintiffs' chosen forum, and that aversion results in their defending only cases to which they have a strong defense. We explore such a phenomenon with respect to foreigners in Kevin M. Clermont & Theodore Eisenberg, *Xenophilia in American Courts*, 109 HARV. L. REV. 1120 (1996). We believe, however, that aversion cannot fully explain the transfer effect, which appears to be bigger than the foreigner effect despite the facts that domestic defendants would be less averse than foreigners and would discount their aversion by the possibility of transfer.

Id. at 1517 n.23.

In the current data set for diversity jurisdiction, there were also 1,550 transfer cases with a win rate of 0.4535. A basic regression for original, removal, and transfer cases gives a highly significant coefficient for transfer of –0.457, which stays big and highly significant at –0.450 after controlling for the corporate status variables, and still stays big and highly significant at –0.488 after additionally controlling for the citizenship variables. Therefore, the impact of forum was indeed the principal cause of the transfer effect.

The odds multiplier for the transfer coefficient, as finally measured at –0.488, indicates that compared to a nontransferred original diversity case with a 50% chance of the plaintiff's winning, an apparently identical but transferred diversity case has a 38% chance. *Cf. id.* at 1524 n.39 (reporting that the transfer effect reduces the chance of winning to 40%, as measured by a similar regression for all jurisdictional bases).

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account for the parties' status, because corporate defendants heavily populate removed cases[68] and because corporations tend to win in litigation.[69] A regression similar to the basic one of Table 3, except that it now controls for the parties' corporate status, produces for diversity jurisdiction a highly significant removal coefficient of –0.618.[70] Then, a full regression that also controls for the parties' citizenship lowers the highly significant removal coefficient somewhat, to –0.462.[71]

Thus, controlling for locale aversion drops this measure of the removal effect only from –0.618 to –0.462. Even though one would think that removal by out-of-state or foreign defendants would isolate the most averse defendants and thus the weakest cases, controlling for this aversion proves that removal still has a sharp effect on outcome. Forum is a causative factor, probably the major cause. But admittedly, locale aversion does contribute to the removal effect.

It appears, then, that both forum and aversion operate to cause the removal effect. Indeed, aversion might explain the especial persistence of the removal effect in tried diversity cases, despite any theoretical expectation that by then the settlement process would have largely obliterated the removal effect. The locale aversion in removed diversity cases should show up even in tried cases—that is, a more finely tuned case-selection theory would not predict original and removed win rates to equalize—because of differentially higher costs for the out-of-state and foreign defendants and because of the parties' misperception in exaggerating the advantages for in-state plaintiffs.[72]

Second, and less plausibly, one might argue that the removed cases are weaker because they involve plaintiffs' lawyers too unskilled to prevent removal and otherwise inferior to the aggressive and knowledgeable defendants' lawyers. But often, the plaintiff cannot prevent removal. Statutes and doctrines, such as fraudulent joinder, often prevent plaintiffs from freely manipulating jurisdiction.[73] Moreover,

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68 *See* Miller, *supra* note 54, at 391 (reporting that 62% of removing defendants in two recent studies were corporations); *infra* Table 5.

69 In this new regression, the highly significant coefficient for corporate plaintiffs is 0.501 and for corporate defendants is –0.163, where the coefficients show a variable's effect on plaintiffs' win rate and thus indicate that corporate litigants tend to win. *See* Clermont & Eisenberg, *Xenophilia*, *supra* note 19, at 1131 tbl.2; Eisenberg & Farber, *supra* note 26, at S109.

70 The odds multiplier here indicates that compared to an original diversity case with a 50% chance of the plaintiff's winning, an apparently identical but removed diversity case has a 35% chance.

71 The odds multiplier here indicates that compared to an original diversity case with a 50% chance of the plaintiff's winning, an apparently identical but removed diversity case has a 39% chance.

72 *See* Clermont & Eisenberg, *Xenophilia*, *supra* note 19, at 1134, 1136.

73 *See* WRIGHT, *supra* note 56, at 185-89, 228-31, 233-40, 244-45 (discussing, inter alia, the doctrine of fraudulent joinder, the artful-pleading doctrine, joinder of separate and

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the removal effect hits corporate plaintiffs as hard as it hits individual plaintiffs. Table 5 shows this fact by reporting the removal effect among the regressed diversity cases for both corporate and individual parties.[74] These data refute any simplistic explanation based on inept plaintiffs' lawyers, as ineptitude would supposedly correlate with noncorporate clients.[75] Therefore, this alternative case-selection explanation, based on quality of counsel, seems implausible as an explanation of any major drop in win rate for removed cases.[76]

TABLE 5
PLAINTIFFS' WIN RATES IN ORIGINAL AND REMOVED FEDERAL DIVERSITY CASES, FY 1987-1994
DISTINGUISHED BY PARTY PAIRINGS
(NUMBER OF CASES IN PARENTHESES)

Pairing	Original	Removed
Individual P v. individual D	.6538	.4278**
	(15,198)	(2,223)
Corporate P v. individual D	.8721	.6192**
	(17,782)	(386)
Individual P v. corporate D	.5452	.2897**
	(15,727)	(8,777)
Corporate P v. corporate D	.7897	.3929**
	(12,190)	(1,667)

* Significant at <0.05 by two-sided Fisher's exact test; ** Significant at <0.0005.

In any event, the pursuit by the plaintiffs' lawyers of a state-forum advantage in the first place, despite possible removal, might suggest a clever set of lawyers. Moreover, the risk of removal might lead the plaintiffs' lawyers to discount their clients' chances, resulting in

independent claims or causes of action, and the minimum amount-in-controversy requirement).

74 Separate full regressions for cases with individual plaintiffs and for cases with corporate plaintiffs indicate that the removal effect indeed is greater for corporate plaintiffs, as the highly significant removal coefficient is –0.416 for the former set of cases and –0.703 for the latter. It is possible that this difference reflects greater forum-shopping by corporate plaintiffs in the state courts. *Cf.* Clermont & Eisenberg, *Transfer, supra* note 32, at 1516 (presenting similar data suggesting that transfer works to defeat forum-shopping by the strong against the weak).

75 *See* JOHN P. HEINZ & EDWARD O. LAUMANN, CHICAGO LAWYERS: THE SOCIAL STRUCTURE OF THE BAR 127-30, 170-75 (rev. ed. 1994) (reporting that two separate hemispheres of the legal profession serve corporate and individual clients); *cf.* Richard L. Abel, *United States: The Contradictions of Professionalism, in* 1 LAWYERS IN SOCIETY 186, 232-34 (Richard L. Abel & Philip S.C. Lewis eds., 1988) (describing the stratification between "elite" and "ordinary" lawyers).

76 *Cf.* Clermont & Eisenberg, *Xenophilia, supra* note 19, at 1133 (concluding that wealthy foreign litigants' probable tendency to retain elite American law firms is implausible as the sole explanation for the observed "foreigner effect").

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bringing only strong cases, thus leading to a stronger set of removed cases. In other words, arguments exist that the set of removed cases is, in fact, not a weaker set of cases at all.

C. The Implications

With the basic statistical manipulation behind us, removal seemed to work a seriously negative effect on plaintiffs' win rate. At that point, however, we ran into two very different explanations: forum impact and case selection. The forum-impact view holds the data to be more or less straightforwardly meaningful, while the case-selection view implies that output data with uncontrolled input are too uncertain to support any meaningful conclusion. That is, do the data mean something, in which event the drop in win rate rather obviously suggests a loss of favorable forum? Or do these output data prove nothing, because the input of original and removed cases could be completely dissimilar? Because of this conflict, we needed to make an artful but critical choice between the two kinds of explanation.

We had reached this crossroads before. On the one hand, we studied the "foreigner effect," whereby foreign litigants win more often than their domestic counterparts. We found implausible the straightforward explanation that American courts favor foreign litigants, and so opted for the competing explanation that foreigners are averse to litigating here and hence more selective in choosing strong cases to pursue to judgment.[77] On the other hand, we also studied the "transfer effect," whereby the plaintiffs' win rate drops markedly after transfer of venue from one federal district to another. Here, the loss of a favorable forum, resulting in a strongly shifted balance of inconveniences and a shift of local biases, seemed to be the sole explanation, because we were able to discount case-selection explanations.[78]

In removal, we have found a phenomenon in the middle. The especially large removal effect owes its existence to both forum impact and case selection. Statistical analysis indicates a removal effect for diversity cases in the neighborhood of a reduction from even (or 50%) odds for plaintiffs to about 35%.[79] A further regression controlling for the case-selection theory of locale aversion, however, raises plaintiffs' odds to 39%.[80] The residual 11% reduction represents the

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77 *Id.* at 1133-35.

78 Clermont & Eisenberg, *Transfer*, *supra* note 32, at 1514-25; *see supra* note 67.

79 *See supra* note 70.

80 *See supra* note 71.

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impact of forum, and it is roughly the same magnitude as that observed after transfer of venue.[81]

To summarize, direct explanations such as forum effect and indirect explanations of case selection both deserve consideration. Sometimes one, sometimes the other, and sometimes both—as in the case of removal—work to produce the observed pattern in win rates.

CONCLUSION

The difficulties of interpreting win-rate data constitute a serious impediment to describing, evaluating, and reforming the legal system. The inherent ambiguity of win-rate data will often yield, however, to sound analytic techniques. Here, a study of win-rate data on removal jurisdiction leads to the conclusion that forum really does affect outcome, with removal taking the defendant to a forum much more favorable in terms of biases and inconveniences.

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81 *See supra* note 67. This comparison suggests a consistent "forum effect," whereby the plaintiffs' loss of forum advantage by removal or transfer reduces their chance of winning by about one-fifth.

For transfer, we ultimately performed a cost-benefit analysis to conclude that "[g]ood policy calls, at the least, for preserving the transfer mechanism." Clermont & Eisenberg, *Transfer*, *supra* note 32, at 1530; *cf. id.* at 1524 n.36 (supporting removal of diversity cases). Here we conclude only that removal affects outcome. We note, however, that Miller's survey research led him to consider expanding removal "to include cases involving federal defenses or counterclaims." Miller, *supra* note 54, at 445. Certainly, if forum really does affect outcome, forum-access provisions deserve the most careful study and thought as to who gets access under what circumstances. The legal system must strive to equalize while it optimizes access.

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