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Does Frye or Daubert Matter? A Study of Scientific Admissibility Standards

Edward K. Cheng

Albert Yoon

ESSAYS

DOES *FRYE* OR *DAUBERT* MATTER? A STUDY OF SCIENTIFIC ADMISSIBILITY STANDARDS

Edward K. Cheng[*] *and Albert H. Yoon*[**]

SINCE it was announced by the Supreme Court in 1993, *Daubert v. Merrell Dow Pharmaceuticals, Inc.*[1] has become the foundational opinion in the modern law of scientific evidence and arguably one of the most important decisions in the area of tort reform. Over the years, the *Daubert* test for scientific admissibility has spawned countless articles, symposia, and informal discussions about its merits and drawbacks, particularly in contrast to its principal rival, the *Frye* "general acceptance" test.[2] Commentators have extensively debated which test is the stricter standard,[3] and

[*] Assistant Professor of Law, Brooklyn Law School; J.D., Harvard Law School; M.Sc., London School of Economics; B.S.E., Princeton University.

[**] Associate Professor of Law, Northwestern University School of Law; Visiting Professor, NYU School of Law (Fall 2004); Ph.D., Stanford University (Political Science); J.D., Stanford Law School; B.A., Yale University. We would like to thank Margaret Berger, Joe Cecil, Jenny Diamond Cheng, Ted Eisenberg, Jesse Rothstein, Aaron Twerski, Nicole Waters, and the Vanderbilt Dean's Lunch Workshop for helpful comments and discussions. Melissa Ballard, Jackie Debs, and Monica Falcone provided research assistance, and Victoria Szymczak provided library assistance. We would like to give special thanks to Stanley Drosky of the New York State Unified Court System Division of Technology; Linda Dykman and Jacek Aleksandrowicz of the Judicial Information Systems division of the Connecticut Judicial Branch; the National Center for State Courts (through ICPSR Study No. 9266), and the Federal Judicial Center (through ICPSR Study No. 8429) for providing us with the relevant data.

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[1] 509 U.S. 579 (1993).

[2] See D.H. Kaye, Choice and Boundary Problems in *Logerquist*, *Hummert*, and *Kumho Tire*, 33 Ariz. St. L.J. 41, 42 (2001) ("Much has been written about the merits, pedigree, and operation of these standards. Each has its strengths and weaknesses, its friends and foes.").

[3] Erica Beecher-Monas, Blinded by Science: How Judges Avoid the Science in Scientific Evidence, 71 Temp. L. Rev. 55, 75–76 (1998) (describing the two sides of the

whether either standard places decisionmaking power in the proper institution (*Frye* in the scientific community, *Daubert* in the judiciary). In addition, state supreme courts have repeatedly grappled with whether to adopt *Daubert* or maintain *Frye*.[4]

Although the practical effects of *Daubert* were initially ambiguous,[5] the enduring legacy of the *Daubert* decision is now relatively clear.[6] In federal courts, where the decision is legally binding, *Daubert* has become a potent weapon of tort reform by causing judges to scrutinize scientific evidence more closely.[7] Tort reform

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debate but arguing that the issue of whether the *Daubert* standard is more strict than the *Frye* standard is a "red herring").

[4] See, e.g., Logerquist v. McVey, 1 P.3d 113, 125–29 (Ariz. 2000); People v. Leahy, 882 P.2d 321, 327–31 (Cal. 1994); State v. Porter, 698 A.2d 739, 749–52 (Conn. 1997). Numerous articles have also peppered the literature advocating for a particular state to adopt *Daubert* or maintain *Frye*. See, e.g., Mary Gaston, Note, *State v. Gentry*: The Washington Supreme Court Opens the Door for Unreliable Scientific Evidence, 31 Gonz. L. Rev. 475, 498–99 (1995–96) (proposing either modifications to *Frye* or an adoption of *Daubert*); Penelope Harley, Comment, Minnesota Decides: *Goeb v. Tharalson* and the Admissibility of Novel Scientific Evidence, 24 Hamline L. Rev. 460, 463 (2001) (summarizing argument that Minnesota should have switched from *Frye* to *Daubert*); Andrew R. Stolfi, Note, Why Illinois Should Abandon *Frye's* General Acceptance Standard for the Admission of Novel Scientific Evidence, 78 Chi.-Kent L. Rev. 861, 862–63 (2003).

[5] Compare Jeffry D. Cutler, Implications of Strict Scrutiny of Scientific Evidence: Does *Daubert* Deal a Death Blow to Toxic Tort Plaintiffs?, 10 J. Envtl. L. & Litig. 189, 191–92 (1995) (suggesting a "grim overall outlook for toxic tort plaintiffs which could result from strict interpretation of *Daubert*"), with Paul M. Barrett, Justices Rule Against Business in Evidence Case—Restrictive Standard for Use of Scientific Testimony in Trials Is Struck Down, Wall St. J., June 29, 1993, at A3 (characterizing *Daubert* as a pro-plaintiff decision). Some of the commentary immediately following the *Daubert* decision expressed skepticism that the *Daubert* rule would change outcomes very much. See, e.g., Richard D. Friedman, The Death and Transfiguration of *Frye*, 34 Jurimetrics J. 133, 143 (1994) (arguing that little would change under the new standard); Barbara Frederick, Note, *Daubert v. Merrell Dow Pharmaceuticals, Inc.*: Method or Madness?, 27 Conn. L. Rev. 237, 270 (1994) (predicting early on that "while *Daubert* will change the language of admissibility decisions, it will have little impact on their outcome").

[6] Joseph Sanders, Shari S. Diamond & Neil Vidmar, Legal Perceptions of Science and Expert Knowledge, 8 Psychol., Pub. Pol'y & L. 139, 141 n.13 (2002) (noting that early on, both plaintiffs and defendants attempted to spin *Daubert* in their direction, but that ultimately "in practice the *Daubert* test has been more restrictive than *Frye*").

[7] Lloyd Dixon & Brian Gill, Changes in the Standards for Admitting Expert Evidence in Federal Civil Cases Since the *Daubert* Decision xv (2001) (reporting that after *Daubert*, "[federal] judges scrutinized reliability more carefully and applied stricter standards in deciding whether to admit expert evidence"); Carol Krafka et al., Judge and Attorney Experiences, Practices, and Concerns Regarding Expert Testimony in Federal Civil Trials, 8 Psychol., Pub. Pol'y & L. 309, 330–31 (2002) (reporting

efforts often focus on medical malpractice, products liability, and toxic torts—all cases in which scientific evidence is likely to play a decisive or at least highly influential role. The resulting effects of *Daubert* have been decidedly pro-defendant. In the civil context, *Daubert* has empowered defendants to exclude certain types of scientific evidence, substantially improving their chances of obtaining summary judgment and thereby avoiding what are perceived to be unpredictable and often plaintiff-friendly juries.

The big question, however, is how the *Daubert* decision has affected state courts, since state courts provide the fora for the vast majority of tort litigation. As Figure 1 shows, over the years a number of states have formally adopted the *Daubert* standard. In those states, one might expect results similar to those observed in the federal context. As Figure 1 also details, however, many states have expressly rejected *Daubert* and chosen to retain the *Frye* standard. What influence, if any, has the *Daubert* decision had on these states despite being formally rejected? Does a state's adoption of a *Daubert* or *Frye* test make any difference in the way scientific evidence is handled in practice?

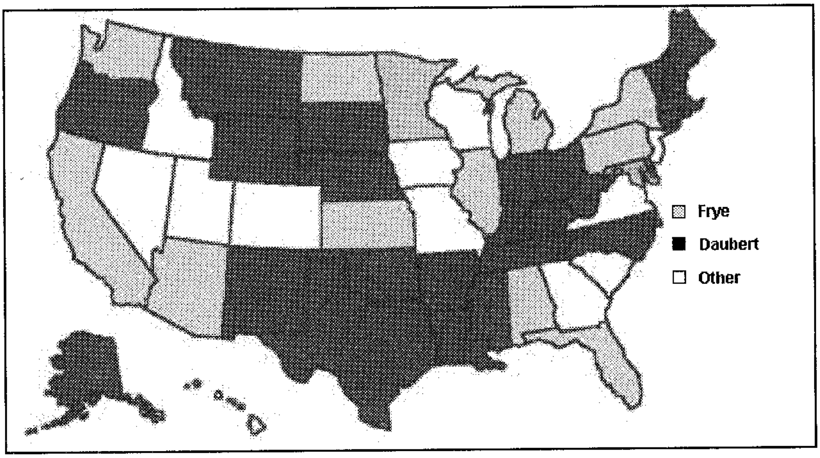

Figure 1: Geographical Map of *Frye* Versus *Daubert* States[8]

results from judge and attorney surveys that suggest greater scrutiny of scientific evidence in the wake of *Daubert*).

[8] *Daubert-Frye* surveys have become rather popular contributions to the scholarly literature. See, e.g., David E. Bernstein & Jeffrey D. Jackson, The *Daubert* Trilogy in the States, 44 Jurimetrics J. 351 (2004); Clifton T. Hutchinson, *Daubert* in State

Among some commentators, there has been growing suspicion that whether a state adopts *Daubert* or *Frye* does not ultimately affect how courts handle scientific evidence. As the authors of the leading treatise on scientific evidence suggest: "Arguably . . . relatively few toxic tort case admissibility rulings actually turn on the difference between *Daubert* and *Frye*. *Daubert*'s shadow now casts itself over state court opinions even in jurisdictions that have not formally adopted the *Daubert* test."[9] Under this view, the real contribution of the *Daubert* decision was not in creating a new doctrinal test, but rather in raising the overall awareness of judges—in all jurisdictions—to the problem of unreliable or "junk" science.[10] Therefore, whether a jurisdiction nominally follows *Frye* or *Daubert*, the practical results are essentially the same.

This theory, if true, could have important ramifications for both the field of scientific evidence and for tort reform more generally. If courts are making scientific admissibility decisions based not on doctrinal tests but rather on other extralegal views, then the traditional focus on the merits of *Frye* versus *Daubert* may be largely misguided. Instead of debating *Frye* versus *Daubert*, perhaps research should concentrate on these "softer" extralegal mechanisms that judges use in their decisionmaking process, as well as on how

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Courts, 9 Kan. J.L. & Pub. Pol'y 15 (1999); Manuel L. Real, *Daubert*—A Judge's View—A Reprise *in* ALI-ABA Course of Study Materials: Civil Practice and Litigation Techniques in Federal and State Courts, 411, 450 (2004). This map is based on those summaries as well as independent research and verification. The diagram is necessarily somewhat of a simplification. First, some states, such as Maine, have adopted *Daubert* in all but name. The map classifies these states as "*Daubert*" since they are doctrinally very similar. Second, some states have adopted *Daubert* but not subsequent, related Supreme Court decisions, such as *Kumho Tire Co. v. Carmichael*, 526 U.S. 137 (1999), and *General Electric Co. v. Joiner*, 522 U.S. 136 (1997); the map does not capture these nuances. For a breakdown among the states along these more complex lines, see Bernstein & Jackson, supra, at 357–61. Nevertheless, Figure 1 hopefully provides a convenient snapshot of the geographic distribution of the different standards.

[9] 4 David Faigman et al., Modern Scientific Evidence § 35-1.3, at 150–51 (2d ed. 2002); see also, e.g., Paul C. Giannelli, Admissibility of Scientific Evidence, 28 Okla. City U. L. Rev. 1, 11 (2003) (noting that the language of *Daubert* has "crept into the *Frye* lexicon").

[10] See David E. Bernstein, *Frye*, *Frye*, Again: The Past, Present, and Future of the General Acceptance Test, 41 Jurimetrics J. 385, 388, 404 (2001) (observing that the "case law under *Frye* is slowly converging with *Daubert* jurisprudence"). The term "junk science" is commonly attributed to Peter Huber. Peter W. Huber, Galileo's Revenge: Junk Science in the Courtroom 2 (1991).

best to educate the judiciary further about scientific methods and the interaction between law and science.

In addition, such a theory would caution against tort reform efforts centered on purely doctrinal changes to procedural (or evidentiary) standards. Although announcing new tests seems to be a straightforward method of changing court behavior, the judicial decisionmaking process in some cases may be too complex to be significantly affected by a vague and indeterminate standard. This is particularly true in the procedural or evidentiary context, in which trial judges tend to have broad discretion and are less subject to appellate scrutiny.

This Essay tests that theory and provides evidence on whether state court adoption of *Frye* or *Daubert* matters. Part I will begin with some background on the *Frye* and *Daubert* standards. We will then review the existing scholarship on scientific admissibility standards and discuss how it informs our research question.

Part II will explain the general difficulties of empirically testing the effect of an evidentiary standard such as *Daubert*, and will conclude that the limitations of traditional methods such as surveys or case analyses make them undesirable tools in this context. Part II will then suggest a new and potentially interesting metric based on the rates at which defendants remove cases from state to federal court. Using a removal metric enables researchers to harness the vast datasets produced and made available by the Federal Judicial Center, the National Center on State Courts, and various state court information systems departments.

Part III will present our research design and interpret the results from an initial, more limited comparison of removal rates between one *Daubert* state (Connecticut) and a geographically similar region of a *Frye* state (New York). Part IV will expand this preliminary study to encompass a much broader swath of the country, limited only by the availability of data and the determinacy of a state's admissibility standard.

Both Parts III and IV will offer strong support for the theory that the choice between a *Frye* and *Daubert* standard does not make any practical difference. Part V will discuss the ramifications and limitations of the results and will touch upon two areas for future study.

I. BACKGROUND

A. Scientific Admissibility Standards

Conceptually, the admissibility requirements for scientific evidence are the same as those imposed on any type of evidence: the evidence must be both reliable and relevant. As a practical matter, however, courts have scrutinized scientific evidence more carefully, revisiting what the appropriate standards should be and who should be making that determination.

For most of the twentieth century, pursuant to *Frye v. United States*,[11] courts evaluated scientific evidence under a "general acceptance" standard.[12] In affirming the trial court's decision to exclude expert testimony regarding a lie detector test based on changes in systolic blood pressure, the D.C. Circuit held that scientific findings must "be sufficiently established to have gained general acceptance in the particular field in which it belongs."[13] The court rejected the testimony because the lie detector test "ha[d] not yet gained such standing and scientific recognition among physiological and psychological authorities."[14] Although *Frye* did have its detractors, who thought it imposed an unreasonably high standard and would serve to exclude information that jurors would find otherwise helpful to deciding cases,[15] *Frye*'s "general acceptance" emerged as the standard at trial for determining the reliability of scientific evidence.

In 1993, the Supreme Court directly addressed the reliability of scientific evidence in *Daubert v. Merrell Dow Pharmaceuticals, Inc.*[16] In reversing the trial court's decision to preclude expert tes-

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[11] 293 F. 1013, 1014 (D.C. Cir. 1923).

[12] See Bernstein, supra note 10, at 388–89 (noting that although there were few citations to *Frye* through the 1960's, it remained influential nonetheless).

[13] *Frye*, 293 F. at 1014.

[14] Id.

[15] For example, Judge Harvey Brown wrote

> [T]he *Frye* test was criticized because the newness of a scientific theory does not necessarily reflect its unreliability, "nose counting" of the scientific community could be difficult and unhelpful, and the standard delays the admissibility of new evidence simply because the scientific community has not had adequate time to accept the new theory.

Harvey Brown, Eight Gates for Expert Witnesses, 36 Hous. L. Rev. 743, 779 (1999).

[16] 509 U.S. 579 (1993). This issue of reliability had, in a sense, been percolating since 1975, when Congress codified the Federal Rules of Evidence. In particular, Congress

timony on the health risks of Bendectin pursuant to *Frye's* "general acceptance" standard, the Court adopted a new framework for evaluating the reliability of scientific evidence, based on four considerations:[17] falsifiability, peer review, error rates, and "acceptability" in the relevant scientific community.[18] While not meant to be exhaustive, these factors were intended to provide guidance to the judge. Perhaps most importantly, *Daubert* established the role of the judge as a "gatekeeper" in the scientific evidence context, requiring trial courts to scrutinize the reliability of any expert evidence offered by the parties.[19]

Since *Daubert*, the Supreme Court has strengthened and broadened the gatekeeping role of the trial judge regarding scientific evidence. In *General Electric Co. v. Joiner*, the Court held that a trial judge's determinations regarding the admissibility of expert testimony were to be reviewed only for abuse of discretion by appellate courts.[20] Most recently, the Court in *Kumho Tire Company v. Carmichael* extended the four-factor test and the court's gatekeeping role to encompass all expert testimony, whether scientific or otherwise.[21]

B. Existing Scholarship

The everyday practice of law suggests that a state's adoption of *Frye* or *Daubert* should make at least some practical difference. Doctrine provides the framework by which judges analyze facts and decide cases, so changing that framework should presumably change outcomes. Nevertheless, a number of recent studies have

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included Federal Rule of Evidence 702 to address the admissibility of expert testimony: "If scientific, technical, or other specialized knowledge will assist the trier of fact to understand the evidence or to determine a fact in issue, a witness qualified as an expert by knowledge, skill, experience, training, or education may testify thereto in the form of an opinion or otherwise." Fed. R. Evid. 702. Neither the rule nor the commentary notes refer to Frye, emphasizing relevance rather than reliability or "general acceptance." Not surprisingly, many federal courts continued to follow Frye in evaluating scientific evidence.

[17] The Court specifically held that Frye was superseded by Rule 702 (notwithstanding the fact that the rule made no mention of Frye or "general acceptance"). See Daubert, 509 U.S. at 587.

[18] Id. at 593–94.

[19] Id. at 592–93.

[20] 522 U.S. 136, 142 (1997).

[21] 526 U.S. 137, 141 (1999).

provided some cause to believe that a state's choice of *Frye* or *Daubert* has no effect in tort cases.

1. Application of the Tests in Practice

A few studies suggest that *Daubert* courts in practice perform what is essentially a *Frye* analysis. In a 2001 study analyzing federal district court decisions, Lloyd Dixon and Brian Gill found that the "general acceptance" prong played a critical role in *Daubert* admissibility determinations in federal court.[22] Similarly, a 2001 survey by Sophia Gatowski and others reported that state court judges not only found general acceptance to be the most useful *Daubert* factor, but that state judges also had a strikingly poor understanding of other *Daubert* factors such as falsifiability and error rate.[23] Thus, while the *Daubert* decision itself may have raised judicial scrutiny of scientific evidence across the board, courts in practice engage in essentially the same analysis regardless of whether their jurisdiction is formally *Frye* or *Daubert*. Courts either do not understand the additional *Daubert* factors or simply do not find them useful. This result is particularly understandable given that many states have adopted deferential standards of appellate review for scientific admissibility determinations. The resulting discretion given to trial courts may undermine any constraints that formal evidentiary doctrine purports to impose.

2. Studies of Criminal Cases

Other evidence that *Frye* and *Daubert* may not matter in tort cases is found in a study conducted by Professor Jennifer Groscup and others in 2002. The Groscup study involved a case analysis of 372 federal and 321 state criminal appellate decisions on scientific admissibility from 1988 to 1999.[24] The study found that in criminal cases, the adoption of the *Daubert* test, whether in state or federal

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[22] Dixon & Gill, supra note 7, at 41. Dixon and Gill analyzed 399 district court opinions from January 1980 to June 1999. Id. at 15–18.

[23] Sophia I. Gatowski et al., Asking the Gatekeepers: A National Survey of Judges on Judging Expert Evidence in a Post-*Daubert* World, 25 Law & Hum. Behav. 433, 444–48 & tbl.1, 452–53 (2001).

[24] Jennifer L. Groscup et al., The Effects of *Daubert* on the Admissibility of Expert Testimony in State and Federal Criminal Cases, 8 Psychol. Pub. Pol'y & L. 339, 342, 344 (2002).

court, had no statistically significant effect on admission rates.[25] A more limited case analysis by Pamela Jensen published in 2003 reported similar results.[26]

Both the Groscup and Jensen studies certainly made important contributions to our understanding of the practical implications of adopting *Frye* over *Daubert*. Both studies, however, were limited to criminal cases,[27] making their results difficult to generalize to the tort context, since courts are motivated by different considerations and biases in criminal cases. In addition, both studies performed case analyses of appellate decisions, which are limited by possible selection effects and other drawbacks that Part II will discuss. Our study therefore seeks to address these limitations, as well as to fill a significant gap in the literature by looking at the effect of *Frye* versus *Daubert* in tort cases.

II. RESEARCH METRIC

A. The Problems of Measurement

How does one determine whether the adoption of a *Frye* or *Daubert* standard makes a difference? Unfortunately, the effect of a scientific admissibility standard can be extremely difficult to measure. Traditional methods such as case analyses, surveys, or various other quantitative measures, while helpful and informative, have significant limitations, and so we ultimately employed a different methodology for this study.

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[25] Id. at 345, 363. Professor Groscup also observed that while *Daubert* courts discussed reliability issues at greater length, discussion about the three new *Daubert* factors was scant, a result consistent with the Dixon and Gatowski studies. Id. at 365 (concluding that while judges understood the import of the *Daubert* decision and cited to it accordingly, they did not apply the criteria in any meaningful way).

[26] Pamela J. Jensen, Note, *Frye* Versus *Daubert*: Practically the Same?, 87 Minn. L. Rev. 1579, 1581 (2003). Jensen considered all relevant state appellate decisions on three forms of scientific evidence used in criminal cases (32 in total) and found no support for "the idea that *Frye* and *Daubert* admissibility standards lead to distinct practical outcomes." Id. at 1611–12 & tbl.1; see also id. at 1619 (commenting that "[a]lthough states vary widely in how they treat certain types of scientific evidence, this variation does not correlate with the adherence to *Frye* or *Daubert* admissibility standards").

[27] Groscup, supra note 24, at 344 (limiting study to criminal cases only); Jensen, supra note 26, at 1585–90 (describing the types of expert evidence studied, which are primarily found in criminal cases).

1. Case Analyses

Case analyses, while often a powerful tool for observing and interpreting the behavior of appellate courts, face significant difficulties in the scientific admissibility context. Scientific admissibility determinations are evidentiary rulings, and so unlike most other important legal decisions, their primary forum is the trial court. In addition, because the vast majority of tort litigation occurs in state courts, research therefore must focus on *state trial courts*. Unfortunately for researchers, however, very few state trial court opinions are published or are available on electronic database services such as Westlaw or Lexis-Nexis.[28]

Case analyses could instead focus on state appellate decisions, but those studies would necessarily suffer from potential selection bias. Furthermore, to the extent that appellate courts focus on establishing bright-line rules regarding the admissibility of broad types of evidence, reading those opinions alone may neglect more subtle influences that admissibility standards can have, such as their effect on the level of scrutiny that trial judges impose on an everyday basis. Case analyses also cannot switch to federal district court decisions because all federal courts operate on the *Daubert* standard, eliminating any basis for comparison.

Case analyses also do not observe instances in which the parties settle early in the litigation process.[29] The vast majority of civil cases never go to trial,[30] and a substantial number never proceed to a stage at which formal opinions are likely to be written, creating more potential bias effects.

Finally, case analyses involve reading published decisions, which are stylized communications that may not necessarily provide an accurate, unadulterated look into the actual judicial decisionmaking process. Courts may—consciously or unconsciously—fail to discuss certain considerations in their opinions, leaving researchers

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[28] See Bernstein, supra note 10, at 389 (recognizing that "most state court opinions, particularly at the trial court level, are unpublished").

[29] See Krafka, supra note 7, at 331 ("To determine how *Daubert* and its associated cases have affected judicial and attorney practices in the majority of cases that never go to trial, further research is needed.").

[30] See, e.g., Theodore Eisenberg et al., Litigation Outcomes in State and Federal Courts: A Statistical Portrait, 19 Seattle U. L. Rev. 433, 442, 443 tbl.4 (1996) (reporting an overall state trial rate of 2.9% for all torts for 1991–1992).

only with a sanitized view. Case analyses also often require the subjective interpretation and coding of decision texts.

2. Surveys

Surveys offer the potential advantage of richer responses and discussions, but are of limited use because they rely heavily on the respondents' ability to recall past experiences truthfully and accurately. Our research question requires assessments of a somewhat vague concept (the scrutiny given to scientific evidence), over the significant period of time before and after a state's adoption of a new scientific standard, in a great variety of cases. It is therefore unclear how much knowledge could be gained from a survey. While a survey could certainly be helpful for ascertaining an impressionistic view of whether attorneys perceive a difference between *Frye* and *Daubert* jurisdictions, its usefulness is limited. Surveys naturally also suffer some selection bias effects based on the willingness of participants to respond.

3. Basic Quantitative Measures

Beyond case analyses or surveys, one could track various quantitative measures to study the effect of a switch from *Frye* to *Daubert*. For example, changes in the number of favorable or unfavorable admissibility rulings in a jurisdiction could suggest a tightening or loosening of scrutiny. The problem, however, is that admissibility rulings are dependent not only on the governing admissibility standard, but also on the perceived validity or strength of the scientific evidence in question. Whether certain types of evidence are found admissible or inadmissible can therefore be significantly time-dependent, because the underlying scientific basis can improve (or decline) over time as new studies are conducted.[31]

Observing changes in final damage awards presents similar problems. Damage award data is subject to the censoring effects of settlements, which are generally sealed.

[31] See Edward K. Cheng, Changing Scientific Evidence, 88 Minn. L. Rev. 315, 333–35 (2003) (discussing the scientific life cycle).

B. The Removal Metric

In an attempt to address some of the above concerns, our study develops what we hope to be a promising new metric for understanding the effect of scientific admissibility standards. Rather than observing case decisions, tallying admissibility determinations, or conducting surveys, our study measures the effect of *Frye* versus *Daubert* by using the rate at which defendants choose to remove cases from state to federal court.[32]

1. Review of How Removal Works

Generally speaking, tort claims are only actionable under state law and therefore must be litigated in state courts. However, when the parties in a lawsuit are citizens of different states, either party has the option of forcing the lawsuit into federal court under diversity jurisdiction.[33] The reasons for litigating in federal court vary—for example, preference for federal procedural rules, concern about out-of-state bias, perceived quality of the federal judiciary, and so forth.[34]

If the plaintiff chooses to file her claim in federal court, the procedural issues are straightforward. However, if the plaintiff files the claim in state court, and the defendant wishes to litigate in federal court, the defendant must remove the case. In order to remove, the defendant must file a motion with the appropriate

[32] We are aware of only one previous study that has used removal rates to measure the effect of a legal change—a 2002 Federal Judicial Center study of the effect of two Supreme Court decisions on federal class actions. Bob Niemic & Tom Willging, Federal Judicial Center, Effects of *Amchem/Ortiz* on the Filing of Federal Class Actions: Report to the Advisory Committee on Civil Rules, Sept. 9, 2002, at 12–13. This study, however, looked only at the number of removals, not the removal rate as defined below. See infra note 44 and accompanying text. Additionally, the Niemic study used different data sources than we used. Niemic & Willging, supra, at 4–5.

[33] 13B Charles Alan Wright et al., Federal Practice & Procedure § 3602 (2d ed. 1984 & Supp. 2004) (noting that a plaintiff may seek federal diversity jurisdiction even when litigating in his or her home state). But see 28 U.S.C. § 1441(b) (2000) (barring removal if any of the defendants is a citizen of the state in which the action was brought).

[34] See, e.g., Neal Miller, An Empirical Study of Forum Choices in Removal Cases Under Diversity and Federal Question Jurisdiction, 41 Am. U. L. Rev. 369, 400–23 (1991) (describing the various factors that attorneys consider in making forum choices).

federal court within thirty days of being served with process.[35] The federal court then transfers the case from the state court and asserts jurisdiction over it.

2. Admissibility Standards and Removal

A change in scientific admissibility standards is likely to affect removal rates considerably. First, scientific admissibility determinations are not sporadic or isolated instances, but rather are implicated in the vast majority of tort cases.[36] Thus, while not all tort cases are removable because of the diversity-of-citizenship requirement, among those that are, scientific evidence is likely to be involved.

Second, in cases that involve scientific evidence, the governing standard is likely to play a major role in defendants' decisions to remain in state court or remove to federal court. Under the Supreme Court's well-established doctrine in *Erie Railroad Co. v. Tompkins*, parties litigating tort claims in federal court are governed by the same substantive tort law as those in state court.[37] The primary potential legal advantages of litigating in federal court are therefore procedural.[38] Scientific admissibility standards, however, while technically procedural, have a significantly substantive cast, since the inability to introduce certain types of scientific evidence can severely undermine a litigant's substantive case and result in an adverse judgment.[39] Consequently, one would expect parties, particularly those in products liability and similar tort litigation—

[35] 28 U.S.C. § 1446(b) (2000); see also 14C Wright et al., supra note 33, at § 3732.

[36] See Samuel R. Gross, Expert Evidence, 1991 Wisc. L. Rev. 1113, 1118–19 (reporting that in a sample of California civil trials from 1985 to 1986, 86% involved expert testimony). Professor Gross further found that experts were involved in 97% of medical malpractice trials (at an average of five experts per trial), and in 100% of products liability trials. Id. Naturally, these rates may suffer from selection bias because they only describe cases that went to trial, but they nonetheless support the general proposition that the use of expert testimony is widespread in tort litigation.

[37] 304 U.S. 64, 78 (1938).

[38] Scholars such as Judge Richard Posner also argue that there is a qualitative difference between state and federal judges due to selection effects and institutional incentives. See Richard Posner, The Federal Courts 142–45 (1985) (discussing factors lawyers consider in choosing between federal and state court).

[39] Margaret A. Berger, Upsetting the Balance Between Adverse Interests: The Impact of the Supreme Court's Trilogy on Expert Testimony in Toxic Tort Litigation, 64 Law & Contemp. Probs. 289, 290 (2001).

where scientific evidence often plays a major role—to care deeply about the governing scientific admissibility standard.

3. Advantages

The removal metric also provides several advantages over other methods. While the removal metric is by no means perfect, it has different attributes that, in combination with previous case and survey studies, will help to produce a more comprehensive picture.

For example, unlike case analyses and other outcome-dependent measures, the removal metric observes cases at a much earlier stage of the litigation process. As mentioned previously, defendants must remove within thirty days of being served. Removal thereby captures a larger and more representative sample of cases. It suffers less from the selection bias of appeal or publication and is also sufficiently early to avoid most of the censoring caused by sealed settlements.

Additionally, the removal metric measures the effect of admissibility standards by what attorneys do, rather than what they say. As a result, it avoids the concerns in surveys about inaccurate recall, and the problem in case analyses of less-than-candid judicial opinions. The removal metric measures the law in action, rather than the law on the books.

Finally, unlike studying actual admissibility decisions, the removal metric minimizes concerns about the effect of changes in the strength of scientific evidence over time. We can expect removal decisions to be made with minimal regard to the underlying facts in a case. Litigants are likely to seek favorable forums regardless of the strength of their specific case.[40]

[40] To the extent that litigants must trade off aspects of litigating in federal court, strength of evidence may affect removal decisions. For example, a state-court-inclined defendant may be more inclined to remove to federal court if the admissibility of plaintiff's scientific evidence is debatable. In those instances, the criticality of having the stricter *Daubert* standard in federal court may outweigh any preferences the defendant would normally have for state court.

III. Preliminary Study: New York and Connecticut

A. Scope

As a preliminary study, we analyzed removal rates in tort cases filed from 1994 to 2000 in the geographical areas defined by the federal district courts for the Eastern District of New York ("EDNY") and the District of Connecticut. The EDNY region is comprised of Kings, Nassau, Queens, Richmond, and Suffolk counties in New York. The District of Connecticut encompasses the entire state of Connecticut. Figure 2 shows both districts.

The EDNY and the District of Connecticut were chosen because they set up somewhat of a "natural experiment" for studying scientific admissibility standards.

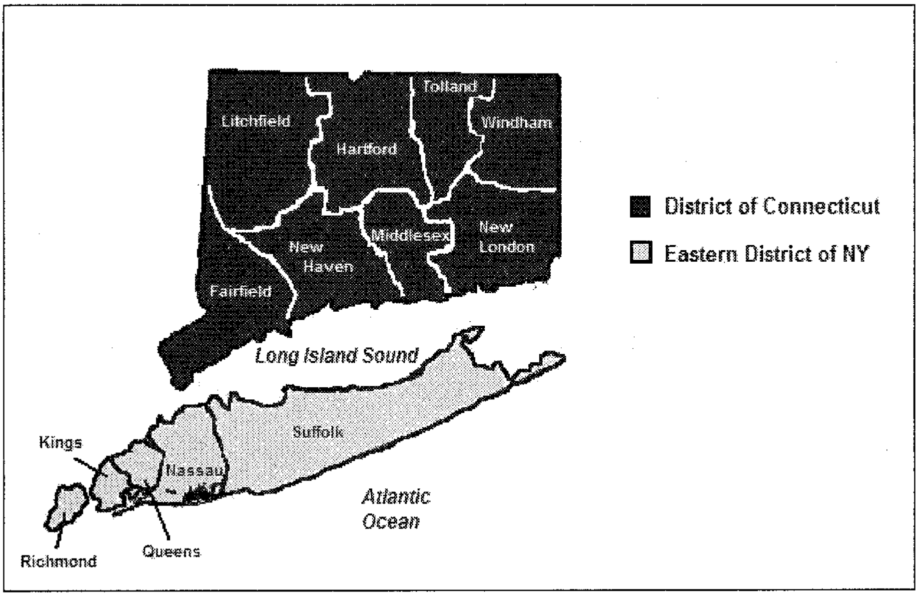

Figure 2: Counties Chosen for Preliminary Study

New York state courts consistently adhered to the *Frye* standard throughout the entire period from 1994 to 2000 (and indeed, continue to adhere to *Frye* today).[41] Connecticut state courts, however, followed *Frye* until May 1997, when they switched to the *Daubert* standard.[42] Federal courts in both states, of course, have applied the *Daubert* standard since 1993. Consequently, the removal rates in Connecticut serve as the treatment group (that is, the group affected by the policy change), while the removal rates in EDNY serve as a convenient control group (that is, a comparable group not affected by the policy change). If a state's adoption of *Frye* and *Daubert* has a practical impact, all else being constant, we would expect removal rates to change in Connecticut after 1997 because defendants would have significantly different incentives to remove.[43] The scientific admissibility standards for the various jurisdictions during the dates studied are shown in Figure 3.

	Period	
Jurisdiction	1994–1997	1997–2000
New York State Courts	*Frye*	*Frye*
Connecticut State Courts	*Frye*	*Daubert*
Federal Courts (both in New York and Connecticut)	*Daubert*	*Daubert*

Figure 3: Scientific Admissibility Standards by Jurisdiction

EDNY and Connecticut actually provide a rather compelling comparison because of the similarities between the regions. For one thing, Connecticut and EDNY are geographically proximate, essentially comprising the northern and southern shores of Long Island Sound. Both are well-connected to New York City, generally considered to be part of the New York metropolitan area, and demographically similar. Thus, exogenous factors, such as political, economic, or social changes affecting one region are likely (or as likely as can be found for any two federal districts) to affect the other. Other states or federal districts encompass much larger geographic regions, creating problems such as multiple metropolitan

[41] See People v. Wernick, 674 N.E.2d 322, 324 (N.Y. 1996); Marsh v. Smyth, 785 N.Y.S.2d 440, 441 (N.Y. App. Div. 2004) (applying *Frye* standard to medical expert's testimony).

[42] See State v. Porter, 698 A.2d 739, 746 (Conn. 1997).

[43] Econometrically, this comparison sets up a "difference-in-differences" approach. See infra notes 55–57 and accompanying text.

areas with different political or economic environments, demographic variations, and so forth.

B. Calculation and Data Collection

1. Definition of Removal Rate

As seen in Figure 4, removal rates were defined to be the ratio of the number of tort cases removed to federal court under diversity jurisdiction in a given year and geographical area to the total number of tort cases filed in the state courts of that area.[44]

$$\text{Removal Rate} = \frac{\text{Number of tort cases removed}}{\text{Total number of tort cases}}$$

Figure 4: Removal Rate Formula

This definition of "removal rate" does not describe the rate at which "removable" cases in fact remove. Many of the cases counted in the denominator are not removable, often because the parties fail the diversity-of-citizenship requirement. What is critical, however, is that the denominator accounts for relative changes in caseloads from year to year or from jurisdiction to jurisdiction, which would otherwise skew a metric based on raw numbers alone.

The definition of "year" also necessarily involves some imprecision. The denominator (total number of tort cases) is determined by the number of cases filed in state court during the given calendar year. The numerator (number of tort cases removed) is determined by the number of cases removed to federal court during the given calendar year. Because there is some delay between state court filing and removal to federal court, some cases filed in state court during one year will be removed to federal court during the next. There appears to be no reason, however, why these numbers would not average out over the long term, or why the method would exert any biasing effect.

[44] This definition of "removal rate" differs from the one used in the Niemic & Willging study, which is the only previous study to have used removal rates to investigate legal changes. See Niemic & Willging, supra note 32, at 12. Niemic & Willging measured removal rates by comparing the ratio of cases originally filed in federal court with cases removed from state court. Id.

2. Data Collection

To calculate the removal rates, we gathered data for each jurisdiction. For removed cases in the two federal district courts, data collection was simplified by using the Federal Court Cases: Integrated Data Base created by the Federal Judicial Center and publicly available through the Inter-University Consortium for Policy and Social Research ("ICPSR").[45] The full ICPSR database provides information on every civil and criminal case filed in federal court between 1970 and 2002. Given our research design, we extracted only tort cases filed in the EDNY and the District of Connecticut for the period of 1994 to 2000.

To determine the total number of cases filed in New York state courts in the EDNY region, we obtained data from the Technology Division of the New York State Unified Court System. Extracting only tort cases filed in the five counties associated with EDNY provided the required information. We acquired similar data from the Judicial Information Systems division of the Connecticut Judicial Branch to determine the relevant Connecticut figures.

C. Results

Figure 5 shows the raw numbers and the calculated removal rates for Connecticut and the EDNY. Figure 6 graphs the two removal rates. The dotted line in Figure 6 represents the year (1997) in which Connecticut switched from the *Frye* to the *Daubert* standard.

If Connecticut's change from *Frye* to *Daubert* had an impact, we would expect a change in Connecticut's removal rate relative to EDNY's removal rate after 1997. (Recall that EDNY is acting as the control group, so all assessments of Connecticut's removal rate must be made relative to it.) Removal rates for both Connecticut and EDNY, however, appear to move in lockstep between 1997 and 1998.

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[45] Federal Judicial Center, Federal Court Cases: Integrated Data Base, 1970–2000, ICPSR Study No. 8429 (2001), at http://www.icpsr.umich.edu/cocoon/ICPSR-STUDY/08429.xml (on file with the Virginia Law Review Association).

	Connecticut			EDNY		
Year	Filed in State Court	Removed to Federal Court	Removal Rate	Filed in State Court	Removed to Federal Court	Removal Rate
1994	16172	56	0.35%	42120	207	0.49%
1995	18417	64	0.35%	46199	237	0.51%
1996	20165	48	0.24%	47711	333	0.70%
1997	20295	49	0.24%	47235	263	0.56%
1998	20054	63	0.31%	46808	288	0.62%
1999	18845	52	0.28%	45838	310	0.68%
2000	18201	56	0.31%	43964	362	0.82%

Figure 5: Removal Rates for D. Conn. and EDNY, 1994–2000

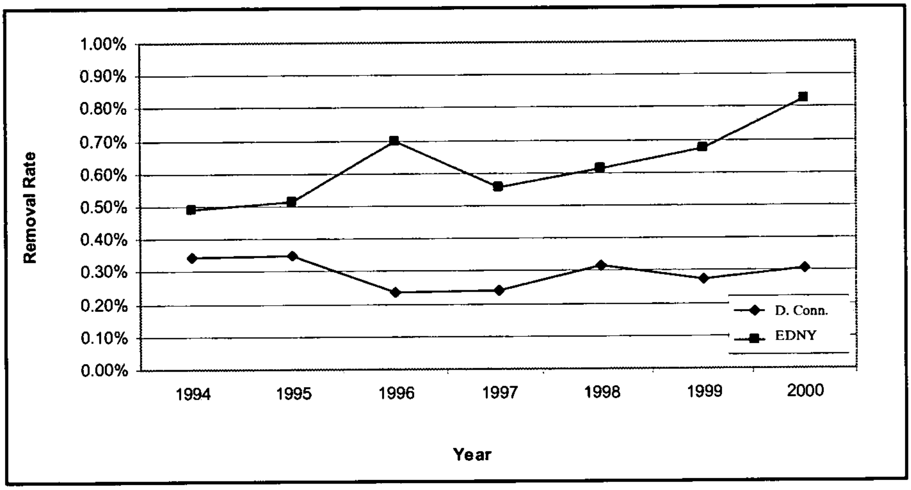

Figure 6: Removal Rates for D. Conn. and EDNY, 1994–2000

Looking more broadly, the removal rates in both states appear relatively stable over the entire period. The removal rate in EDNY does have a slight upward trend, but the difference between Connecticut's removal rates (again, relative to EDNY) in the pre-*Daubert* period (1994–1996) and the post-*Daubert* period (1998–2000) is not statistically significant.[46] This result suggests that the

[46] The change in Connecticut's removal rate between the pre- and post-periods was assessed using a difference-in-difference model. For an explanation of the difference-in-difference approach, see infra notes 55–57 and accompanying text.

change in Connecticut from *Frye* to *Daubert* did not have any significant effect detectable by this model.

D. Possible Refinements

1. Types of Torts

A natural extension of this study would involve breaking down the removal rates into smaller subsets. For example, because one might expect removal rate to vary by type of tort (medical malpractice, automobile accidents, products liability, and so forth), separating the aggregate data could shed further light on the effect of scientific admissibility standards. After all, one would expect expert evidence to be given far greater weight in a products liability case than any other tort case. Products liability litigation may also offer more opportunities for removal, since the defendant is often an out-of-state manufacturer; automobile accidents, in contrast, are ordinarily between two in-state drivers.

Unfortunately, our datasets were ultimately too inconsistently coded at the "tort-type" level to enable further analysis. While the state and federal data all had a products liability category, the jurisdictions appeared to either define "products liability" differently, or use the category irregularly. As a result, attempts to measure removal rate often yielded percentages above 100%, suggesting either coding errors or different approaches in coding by state and federal data compilers.

2. Other States

As previously mentioned, studying two well-matched jurisdictions such as Connecticut and the EDNY has a number of advantages, especially the presumed presence of a control group. One difficulty, however, is that there are relatively few data points. While the number of cases involved in constructing the removal metric is enormous, ultimately there is only one removal rate per year for each jurisdiction. This small number of data points inhibits our ability to control precisely for single-year variations.

Another limitation of a two-state comparison is that removal rates may be affected by unobserved variables that differ between Connecticut and EDNY. For example, perhaps Connecticut's adoption of *Frye* over *Daubert* does make a difference, but Connecticut experienced some political or legal change at approximately the same time that cancelled out any accompanying removal rate effect.[47] We selected Connecticut and the EDNY specifically to minimize these types of asymmetric changes, but unfortunately the study's construction can only do so much.

The best method to remedy both of these deficiencies is to expand the inquiry to include as many states as possible. Such a study would generate more data points for econometric analysis, control for regional variations, and reduce the likelihood that some unique political or other change in a particular jurisdiction distorts the results. The next Part does just that.

IV. NATIONAL STUDY, 1990–2000

In this Part, we expand the scope of our study to look across the country. By increasing the number of states (and therefore the number of data points), we are able to control for variations from year to year as well as from state to state, enabling us to better isolate the effect of the doctrinal admissibility standard. Other unobserved variables average out over the various states located in different geographic regions. This broader approach provides more definitive conclusions on whether a state's choice of admissibility standard has any practical effect.

A. Data Sources and Selection

1. Data Sources

To obtain data on the total number of cases removed to federal court, we once again relied on the Federal Court Cases database created by the Federal Judicial Center and publicly available through ICPSR.[48] Given our research goals in this national study,

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[47] One can argue that a change in scientific admissibility standard is an exogenous determination that is minimally correlated with short-term changes in demographic or other unobserved variables. The length of judicial tenure and the independence of the judiciary make judicial decisions by and large independent of any short-term demographic or political change. Furthermore, given that the precise admissibility standard for scientific evidence is a specialized evidentiary issue unlikely to attract significant public attention, one would expect it to play a sharply limited role, if any, in the judicial appointment process.

[48] See Federal Judicial Center, supra note 45.

however, we retained tort cases from all jurisdictions. We combined figures wherever appropriate—for example, to ascertain the number of removed cases in Pennsylvania, we combined data from the Western and Eastern Districts of Pennsylvania. In addition, because the state court data discussed below was available for 1985 to 2001, we were able to expand the time period to encompass 1990 through 2000 for observational purposes, although, for reasons discussed below, the econometric analysis was still confined to 1994 through 2000.

For state court data, the study relied on the State Court Statistics, 1985 to 2001 dataset created by the National Center for State Courts and also publicly available through ICPSR.[49] This dataset includes summary statistics on all state court systems from 1985 to 2001 whenever such statistics are available. Therefore, unlike the Connecticut, EDNY, and federal datasets, the state court dataset does not have case-level information. For our purposes here, however, the aggregate level data was sufficient.

2. *States Selected*

The states ultimately included in the national study are presented in Figure 7. Not all states were appropriate for use in this national study. While we kept as many states as possible, we had to exclude states on the basis of two criteria: data availability and the existence of a clear *Frye* or *Daubert* standard. Many states did not have statistics on total tort cases filed for the entire period, or the National Center for State Courts reported that the statistics were incomplete or overinclusive. Using a state that had only reported some cases or only had complete data from 1997 to 2000 could undesirably skew results. As a result, we required that any state used in the national study have complete data going back at least as far as the *Daubert* decision in 1993.

Perhaps even more importantly, some states did not have a clearly defined scientific admissibility standard for some portion of the period, while others had a standard that was neither *Frye* nor *Daubert*. Since these states would invariably lead to coding errors if

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[49] National Center for State Courts, State Court Statistics, 1985–2001, ICPSR Study No. 9266 (2002), at http://webapp.lcpsr.umich.edu/cocoon/ICPSR-STUDY/04266.xml (on file with the Virginia Law Review Association).

State	Starting Year of Data	Standard	Year of Change to *Daubert*
Alaska	1990	Daubert	1999
Arizona	1990	Frye	N/A
Arkansas	1990	Daubert	1990
Connecticut	1990	Daubert	1997
Florida	1990	Frye	N/A
Indiana	1990	Daubert	1995
Kansas	1990	Frye	N/A
Michigan	1990	Frye	N/A
Minnesota	1990	Frye	N/A
Missouri	1990	Frye	N/A
New Mexico	1993	Daubert	1993
New York	1990	Frye	N/A
North Carolina	1990	Daubert	1995
Oregon	1991	Daubert	Pre-1990
Tennessee	1990	Daubert	1997
Washington	1990	Frye	N/A

Figure 7: States Included in National Study

characterized as either a "Frye" or "Daubert" state, the study excluded these as well. It was not necessary for a state to specifically adopt Frye or Daubert by name, but the test adopted had to be substantially equivalent. Hence, for the purposes of this study, states such as Arkansas,[50] Indiana,[51] North Carolina,[52] and Ore-

[50] Arkansas followed a multifactor, *Daubert*-like test from 1990 to 2000, and then explicitly adopted *Daubert* in 2000. See Farm Bureau Mut. Ins. Co. v. Foote, 14 S.W.3d 512, 519 (Ark. 2000). Arkansas was considered a "*Daubert*" state for purposes of this study.

[51] Indiana followed *Frye* until 1994, had a somewhat unclear standard from 1994 to 1995, and then began relying on *Daubert* to guide evidentiary expert rulings beginning in 1995. See Steward v. State, 652 N.E.2d 490, 498 (Ind. 1995) ("[A]lthough not binding upon the determination of state evidentiary law issues, the federal evidence law of *Daubert* and its progeny is helpful to the bench and bar in applying Indiana Rule of Evidence 702(b)."). For purposes of this study, we considered Indiana to be a *Daubert* state.

[52] From 1984 to 1995, North Carolina refused to adopt *Frye* but adopted its principles. See State v. Peoples, 319 S.E.2d 177, 187 (N.C. 1984). In 1995, the North Carolina Supreme Court expanded its standard to look at factors beyond general acceptance, in essence adopting a *Daubert* standard. See State v. Goode, 461 S.E.2d 631, 639 (N.C. 1995); see also Taylor v. Abernethy, 560 S.E.2d 233, 273 (N.C. Ct. App.

gon[53] were considered to be *Daubert* states, and Missouri[54] was considered a *Frye* state.

B. Results

1. Removal Rates

Removal rates for all the included states were calculated using the same methodology described in the preliminary study. Removal rate was once again defined to be the total number of cases removed to federal court during the calendar year divided by the total number of tort cases filed during the same year. For reference purposes, Appendix A contains the resulting data.

2. Graphical Trends

As one might imagine, a graph of the removal rates of all sixteen states is noisy and too difficult to interpret without further analysis. Examining smaller geographic regions, however, can illuminate in-

2002) (noting the North Carolina Supreme Court's citation of *Daubert*); Bernstein & Jackson, supra note 8, at 358 & n.41 (classifying North Carolina as a *Daubert* state). The North Carolina Supreme Court later held its standard to be distinct from *Daubert* in 2004, see Howerton v. Arai Helmet, Ltd., 597 S.E.2d. 674, 689 (N.C. 2004), but from 1995 to 2000, appellate courts often thought otherwise. See, e.g., *Taylor*, 560 S.E.2d at 273; State v. Bates, 538 S.E.2d 597, 600 (N.C. Ct. App. 2000) (interpreting *Goode* as adopting *Daubert*).

[53] Oregon adopted a multi-factor standard of admissibility similar to *Daubert* in 1984. See State v. Brown, 687 P.2d 751, 759 (Or. 1984).

[54] At first glance, Missouri's standard during the study period is somewhat ambiguous. Prior to 1993, the Missouri Supreme Court clearly followed the *Frye* standard. See State v. Davis, 814 S.W.2d 593, 600 (Mo. 1991) (stating that *Frye* had "been adopted and regularly applied in a variety of Missouri decisions"). After the *Daubert* decision in 1993, confusion initially arose among Missouri courts because Missouri's statutory rule governing expert evidence, § 490.065 of the Missouri Revised Statutes, was modeled after Federal Rule of Evidence 702. See Long v. Mo. Delta Med. Ctr., 33 S.W.3d 629, 642 (Mo. Ct. App. 2000). A consensus soon formed among the appellate courts, however, that *Frye* should continue to be applied. See id.; M.C. v. Yeargin, 11 S.W.3d 604, 619 (Mo. Ct. App. 1999) ("The Missouri Supreme Court continues to apply the *Frye* test of the admissibility of expert testimony in criminal cases and in civil cases."). But cf. Lasky v. Union Elec. Co., 936 S.W.2d 797, 802 (Mo. 1997) (remanding case for an admissibility determination under the statutory language itself). In light of this apparent consensus, we have classified Missouri as a *Frye* state for the study. See also Bernstein & Jackson, supra note 8, at 355 n.25 (defining Missouri as a *Frye* state). The Missouri Supreme Court subsequently adopted a more *Daubert*-friendly approach in 2003. See State Bd. of Registration for Healing Arts v. McDonagh, 123 S.W.3d 146, 155 (Mo. 2003).

teresting trends. For example, examining Midwestern and Plains states (Indiana, Kansas, Michigan, Minnesota, and Missouri) together yields the graph in Figure 8.

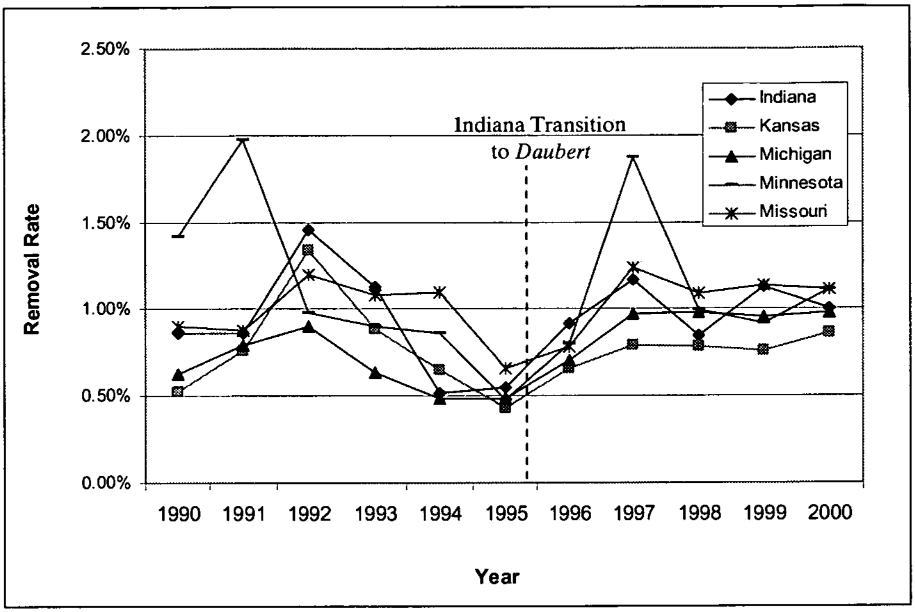

Figure 8: Removal Rates in Midwest and Plains States

All of the states in Figure 8 seem to demonstrate the same general trend, even though Indiana switched from *Frye* to *Daubert* in 1995, and the remainder of the group remained *Frye* throughout. The graph shows a relatively high removal rate until around 1993, then a steady decrease in removal rate until a nadir in 1995, and then a steady return climb in removal rate until 1997.

3. Basic Econometric Model

Ultimately, a regression best controls for year-to-year and state-to-state variations analytically, and ensures that a switch to *Daubert* is not playing some small but heretofore undetected role. For the regression analysis, we use a difference-in-differences approach, a common econometric method of program or policy evaluation.[55] This approach measures the effect of a policy when one group (treatment) is exposed to the policy and another group (control) is not. The central assumption underlying difference-in-differences is that, in the absence of the policy change, the trend in the treatment group relative to the control group would have remained the same over time. Hence, the absence of any relative change in the treatment group on the variable of interest following the policy change is probative to show that the policy did not have an effect. Econometrically, the basic model looks like the following:

$$RR = \alpha + \sum_i \beta_i YEAR_i + \sum_j \gamma_j STATE_j + \delta * DAUBERT + \varepsilon$$

Equation 1

RR represents the rate of removal (in percentage points) and is the dependent variable. The y-intercept is measured by α. $YEAR_i$ represents a series of dummies for the year *i* in which the removal rate is measured, normalizing for year-to-year effects.[56] $STATE_j$ represents a series of dummies for the various states *j* included in the study, normalizing for state effects.[57] DAUBERT is an indicator variable for whether or not the jurisdiction was following the *Daubert* standard at the time. The error term is captured by ε.

For the econometric model, the data was limited to the period from 1994 to 2000. Because *Daubert* was decided in 1993, removal decisions prior to 1994 would be based on federal courts operating under a *Frye* standard, disrupting the analysis. Furthermore, our

[55] See Orley Ashenfelter, Estimating the Effects of Training Programs on Earnings, 84 Rev. Econ. & Stat. 47 (1978); David Card, The Impact of the Mariel Boatlift on the Miami Labor Market, 43 Indus. & Lab. Rel. Rev. 245 (1990); David Card & Alan B. Kreuger, Minimum Wages and Employment: A Case Study of the Fast-Food Industry in New Jersey and Pennsylvania, 84 Amer. Econ. Rev. 772 (1994); Jonathan Gruber, The Incidence of Mandated Maternity Benefits, 84 Amer. Econ. Rev. 622 (1994); Albert Yoon, Damage Caps and Civil Litigation: An Empirical Study of Medical Malpractice Litigation in the South, 3 Amer. L. & Econ. Rev. 199 (2001).

[56] Given the econometric model, all year coefficients are in relation to 1994. The year 1994, which was chosen arbitrarily, is accounted for when all the state variables are zero.

[57] Just as with the year variables, all state coefficients are in relation to Alaska. Alaska, which was chosen arbitrarily, is accounted for when all the state variables are zero.

study only investigates whether *Frye* or *Daubert* makes a difference in state courts, making pre-*Daubert* removal rates unnecessary for the regression analysis.

Running the basic econometric model in Equation 1 yields the results in Figure 9.

Regression with robust standard errors				Number of obs	=	110
				F(22, 87)	=	24.11
				Prob > F	=	0.0000
				R-squared	=	0.8262
				Root MSE	=	.23301

	Coefficient	Robust Std. Err.	t	P>|t|	[95% Conf. Interval]	
DAUBERT	-.0053476	.0863988	-0.06	0.951	-.1770746	.1663793
Year 1995	-.2732086	.096789	-2.82	0.006	-.4655872	-.0808301
Year 1996	-.0237572	.0765122	-0.31	0.757	-.1758335	.1283191
Year 1997	.2221668	.0975977	2.28	0.025	.0281809	.4161528
Year 1998	.1584701	.1106303	1.43	0.156	-.0614196	.3783598
Year 1999	.1317904	.0972652	1.35	0.179	-.0615347	.3251154
Year 2000	.2278502	.0826192	2.76	0.007	.0636357	.3920647
Arizona	-1.554228	.188518	-8.24	0.000	-1.928928	-1.179528
Arkansas	-.4039759	.2148389	-1.88	0.063	-.8309915	.0230398
Connecticut	-1.587598	.1885287	-8.42	0.000	-1.962319	-1.212876
Florida	-1.335077	.1859045	-7.18	0.000	-1.704582	-.9655713
Indiana	-1.012043	.1787954	-5.66	0.000	-1.367418	-.6566681
Kansas	-1.184077	.1828137	-6.48	0.000	-1.547439	-.8207152
Michigan	-1.094621	.1876164	-5.83	0.000	-1.467529	-.721713
Minnesota	-.8849699	.2208067	-4.01	0.000	-1.323847	-.4460927
Missouri	-.8732507	.1858851	-4.70	0.000	-1.242717	-.5037839
New Mexico	-.9864639	.163869	-6.02	0.000	-1.312171	-.6607566
New York	-1.095563	.1863652	-5.88	0.000	-1.465984	-.7251422
N. Carolina	-1.346426	.1681778	-8.01	0.000	-1.680698	-1.012155
Oregon	-1.153108	.214477	-5.38	0.000	-1.579404	-.7268117
Tennessee	-.6373306	.1758412	-3.62	0.000	-.9868339	-.2878272
Washington	-1.588858	.1922537	-8.26	0.000	-1.970983	-1.206733
Constant	1.827034	.1837049	9.95	0.000	1.461901	2.192168

Figure 9: Regression Results from Basic Model (1)[58]

[58] In reading the regression results, variable names are in the leftmost column. The correlation of each variable on the removal rate is measured by the coefficient in the second column. Thus, for example, in Figure 9, the fact that the year is 1995 tends to lower removal rates, whereas the fact that the year is 1997 tends to raise removal

The most important result in Figure 9, of course, is that after year and state effects have been accounted for, the DAUBERT variable—whether a state follows the *Daubert* standard in the year in question—has a vanishingly small effect on removal rate. DAUBERT contributes only five-thousandths of a percentage point to a state's removal rate, and the result is statistically insignificant. This result suggests that, in making removal decisions, defendants place little weight on whether a state follows *Frye* or *Daubert.*

A few other observations can be made about the results using the basic model. First, the various state variables all have large coefficients that are statistically significant. This result makes sense, because each state is likely to have an average baseline rate of removal, depending on how comfortable defendants are with the state's rules of procedure, the perceived quality of the state's judiciary, and so forth. Second, most of the year variables are also statistically significant, suggesting that there are indeed year-to-year variations. Again, this finding is intuitive. Tort litigation can be trendy, focusing on a particular industry or defendant for a time and then moving on to new pastures. Removal rates for a given year may therefore reflect the particular trend in tort litigation for that year.

4. Weighted Econometric Models

One possible concern about the basic model presented in Equation 1 is that it fails to distinguish light-caseload jurisdictions from heavy-caseload jurisdictions. The removal rates observed for smaller jurisdictions will be more sensitive to small, random fluctuations in the number of removed cases (conversely, jurisdictions

rates. A coefficient of zero, or relatively close to zero, suggests that a variable either has no effect on removal rates (in the case of a single variable like DAUBERT), or no effect relative to the omitted baseline dummy variable. See supra notes 57–58.

Statistically, the confidence we have that any coefficient is different from zero is found in the P>|t| (or p-value) column. A small p-value means that there is a very small probability that the coefficient is actually zero (but calculated to be non-zero because of random variations). "Statistical significance" is generally set at the 5% level, or p=0.05, whieh means that there is only a 5% chance that the coefficient is actually zero.

with heavier caseloads experience greater averaging effects). Without some weighting mechanism, swings in light-caseload jurisdictions are thus inappropriately valued equally to swings in heavy-caseload jurisdictions, where such swings are far more difficult to achieve randomly.

Weighting within the econometric model can be achieved via two alternatives. The first method is not to look at removal rate, but rather at the number of cases removed, as seen in Equation 2:

$$C_{removed} = \alpha + \sum_i \beta_i Year_i + \sum_j \gamma_j State_j + \delta * DAUBERT + \varphi * C_{filed} + \varepsilon$$

Equation 2

$C_{removed}$ represents the total number of cases removed to federal court in the jurisdiction for a given year. The remainder of the model is identical to Equation 1, except that the number of tort cases filed in a given year is controlled by using C_{filed}.

Figure 10 shows the results from a regression based on the model in Equation 2. The fit of this model appears to be far better than the basic model in Equation 1 (Equation 2 has an R^2 of 0.96, while Equation 1 has an R^2 of 0.82).[59] As expected, the number of cases filed, represented by the variable TORTFILE in the regression, is a statistically significant predictor of the number of cases removed: the more cases that get filed, the more cases that will get removed.[60] Once again, many of the state and year variables are statistically significant, but, importantly, the DAUBERT variable is once again relatively small and not statistically significant.

[59] R^2 is a statistical measure of "fit"—that is, how well the model and its variables predict removal rate. Generally, the higher the R^2 the better the predictive force of the model.

[60] Compared to the other coefficients, the coefficient for TORTFILE may appear small, and therefore insignificant, but only deceptively so. Unlike the other variables, which have values of only zero or one, the TORTFILE variable generally has values in the thousands, which means that a small TORTFILE coefficient can still suggest a strong relationship between the number of cases filed and the number of cases removed.

Regression with robust standard errors					Number of obs = 110 F(23, 86) = 60.35 Prob > F = 0.0000 R-squared = 0.9617 Root MSE = 34.252	
	Coefficient	Robust Std. Err.	t	P>|t|	[95% Conf. Interval]	
DAUBERT	-9.829792	11.56274	-0.85	0.398	-32.81577	13.15618
TORTFILE	.004749	.001473	3.22	0.002	.0018208	.0076773
Year 1995	-27.52213	15.7583	-1.75	0.084	-58.84859	3.804323
Year 1996	13.71642	11.03207	1.24	0.217	-8.214613	35.64746
Year 1997	39.86707	13.17015	3.03	0.003	13.68567	66.04847
Year 1998	26.14495	10.67405	2.45	0.016	4.925644	47.36425
Year 1999	29.08516	11.40336	2.55	0.013	6.416019	51.75429
Year 2000	44.10236	16.12342	2.74	0.008	12.05006	76.15467
Arizona	-37.98712	24.01074	-1.58	0.117	-85.7189	9.744657
Arkansas	40.64963	15.28405	2.66	0.009	10.26593	71.03332
Connecticut	-44.07844	30.34544	-1.45	0.150	-104.4032	16.2463
Florida	8.941038	67.51588	0.13	0.895	-125.2761	143.1582
Indiana	43.62383	22.26032	1.96	0.053	-.6282314	87.87589
Kansas	-2.18437	12.50121	-0.17	0.862	-27.03595	22.66721
Michigan	69.55586	38.48628	1.81	0.074	-6.952324	146.064
Minnesota	20.03074	14.83085	1.35	0.180	-9.45201	49.51349
Missouri	87.5226	29.69488	2.95	0.004	28.49113	146.5541
New Mexico	15.92776	15.00656	1.06	0.291	-13.90428	45.75981
New York	242.367	125.9804	1.92	0.058	-8.073721	492.8078
N. Carolina	-1.758002	17.80038	-0.10	0.922	-37.14398	33.62798
Oregon	11.06637	20.14878	0.55	0.584	-28.98807	51.12081
Tennessee	89.91687	20.94021	4.29	0.000	48.28911	131.5446
Washington	-36.9327	20.14384	-1.83	0.070	-76.97732	3.111927
Constant	-2.469264	11.47271	-0.22	0.830	-25.27626	20.33773

Figure 10: Regression Results from Weighted Model Two

The second method for weighting is to include analytic weights to the initial regress. These weights are the total number of tort cases filed in the jurisdiction for the year that give rise to that state's respective removal rate, and are inversely proportional to the variance of each observation. Results from this weighting method are shown in Figure 11.

Under this second method, we see that the results largely remain unchanged. As in the basic model, nearly all of the state and year variables are statistically significant. Again, the DAUBERT variable is small, contributing two hundredths of a percentage point, and not statistically significant.

Regression with robust standard errors				Number of obs = 110 F(22, 87) = 33.20 Prob > F = 0.0000 R-squared = 0.8395 Root MSE = .14857		
	Coefficient	Robust Std. Err.	t	P>|t|	[95% Conf. Interval]	
DAUBERT	.0233748	.0684189	0.34	0.733	-.1126151	.1593647
Year 1995	-.1629444	.0543312	-3.00	0.004	-.2709335	-.0549552
Year 1996	.034418	.0542082	0.63	0.527	-.0733267	.1421627
Year 1997	.2023167	.0641396	3.15	0.002	.0748323	.3298011
Year 1998	.1223483	.0503039	2.43	0.017	.0223639	.2223327
Year 1999	.1651497	.0526122	3.14	0.002	.0605772	.2697221
Year 2000	.2663094	.0510324	5.22	0.000	.1648771	.3677418
Arizona	-1.561249	.1997495	-7.82	0.000	-1.958273	-1.164226
Arkansas	-.464657	.2549951	-1.82	0.072	-.9714875	.0421734
Connecticut	-1.615328	.2111751	-7.65	0.000	-2.035061	-1.195594
Florida	-1.338229	.1993651	-6.71	0.000	-1.734489	-.9419693
Indiana	-1.039019	.2095735	-4.96	0.000	-1.455569	-.6224686
Kansas	-1.192793	.1979847	-6.02	0.000	-1.586309	-.7992768
Michigan	-1.125059	.2034454	-5.53	0.000	-1.529429	-.7206891
Minnesota	-.8814852	.2462549	-3.58	0.001	-1.370944	-.3920269
Missouri	-.8827942	.203519	-4.34	0.000	-1.28731	-.4782783
New Mexico	-1.025804	.2094682	-4.90	0.000	-1.442145	-.6094637
New York	-1.103796	.199178	-5.54	0.000	-1.499684	-.7079083
N. Carolina	-1.375954	.2017611	-6.82	0.000	-1.776977	-.9749323
Oregon	-1.209415	.2471505	-4.89	0.000	-1.700653	-.7181765
Tennessee	-.6647566	.2060349	-3.23	0.002	-1.074273	-.2552399
Washington	-1.595988	.2031096	-7.86	0.000	-1.99969	-1.192285
Constant	1.808944	.2004615	9.02	0.000	1.410505	2.207383

Figure 11: Regression Results Using Analytic Weights

5. Summary

Figure 12 summarizes the results for the various econometric models. It also includes the results from a basic regression relating the DAUBERT variable to removal rates in which no state or year effects are controlled. Notably, DAUBERT is a statistically significant predictor of removal rate in this crude model, but any predictive effect it has disappears once year and state effects are controlled for.

	Model			
	No Controls	Equation 1	Equation 1 using Analytic Weights	Equation 2
Location of results	--	Figure 9	Figure 11	Figure 10
DAUBERT	0.1949* (1.99)	-0.0053 (0.06)	0.0234 (0.34)	-9.83 (0.85)
Constant	0.7782** (13.29)	1.8270** (9.95)	1.8089** (9.02)	-2.47 (0.22)
Control for Number of Cases Filed in State in Year	No	No	Yes	Yes
Control for State Effects	No	Yes	Yes	Yes
Control for Year Effects	No	Yes	Yes	Yes
N R^2	110 .0348	110 0.8262	110 0.8395	110 0.9617

* Statistically significant at 5% level
** Statistically significant at 1% level

Figure 12: Summary of National Study Results[61]

[61] Again, in reading Figure 12, the number in the DAUBERT row is the coefficient describing the effect that adoption of the *Daubert* standard has on removal rate (or number of cases removed). The parenthetical number is the t-value, a statistical measure of confidence. N is the number of observations of removal rate considered in the analysis (one observation per state per year). R^2 is a statistical measure of "fit," or how well the model and its variables predict removal rate. Note that without the state

V. Discussion

The results from the national study suggest that a state's choice of scientific admissibility standard does not have a statistically significant effect on removal rates (or number of cases removed). This finding may support the broader theory that a state's adoption of *Frye* or *Daubert* makes no difference in practice. Graphically speaking, as shown in Figure 9, removal rates seem to follow the same trends regardless of whether a state retains the *Frye* standard during the entire period, or as in the case of Indiana, switches to *Daubert* in the middle of the period. More importantly, however, using econometric techniques and controlling for year-to-year and state-to-state variations, the data shows that whether or not a jurisdiction follows the *Daubert* standard has no statistically significant effect on the removal rate.

A. Ramifications

1. Daubert's *Influence*

The results of this study are consistent with the theory that the power of the Supreme Court's *Daubert* decision was not so much in its formal doctrinal test, but rather in its ability to create greater awareness of the problems of junk science. This suggests that courts apply some generalized level of scrutiny when considering the reliability of scientific evidence, regardless of the governing standard. If accepted, this thesis suggests that debates about the practical merits and drawbacks of adopting a *Frye* versus a *Daubert* standard are largely superfluous.[62]

One basic policy recommendation arising out of this result is that state courts should consider uniformly adopting *Daubert* as their scientific admissibility standard.[63] If *Frye* and *Daubert* do not make a difference, then the skirmishing between the champions of *Frye*

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and year controls, R^2=0.03, suggesting that a model based purely on DAUBERT cannot predict removal rate at all.

[62] Of course, theoretical discussions about the differences between *Frye* and *Daubert* remain important, since those debates may ultimately lead to a greater understanding of how to assess science.

[63] Bernstein, supra note 10, at 404–07 (arguing that *Frye* jurisdictions should adopt *Daubert*, not necessarily because *Daubert* is a different or better rule, but merely to eliminate confusion).

and *Daubert* yields few benefits and creates more confusion than anything else. Certainly, states should feel free to experiment with entirely different standards of admissibility—for example, Utah maintains a more rigorous test for scientific admissibility.[64] These alternative standards, however, must be sufficiently different and well understood to have any hope of achieving different results. If the states are to be the laboratories of legal progress, variation and experimentation should be embraced, but having doctrinal differences in name only provides little benefit.[65]

In addition, the findings suggest that future attempts to improve the handling of scientific evidence in the courts could be more effective if advocates for rigorous use of scientific evidence shifted their focus away from tinkering with doctrinal tests and instead toward "softer" solutions that increase the judiciary's understanding of scientific concepts and processes.[66] For example, reformers instead might pay greater attention to judicial education programs and help develop official literature such as the acclaimed *Reference Manual on Scientific Evidence*.[67]

Alternatively, if reformers believe that doctrinal tests are important and would like these tests to have greater traction, they may want to concentrate on advocating for stricter appellate review standards. Although by no means universal, many state courts, perhaps following the lead of the federal courts, review scientific admissibility decisions under an abuse-of-discretion standard.[68] This deferential standard of review empowers trial judges with substantial discretion, making the exact contours of the governing doctrinal test less important than it might otherwise be. Increasing ap-

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[64] See State v. Crosby, 927 P.2d 638, 641–42 (Utah 1996) (suggesting that Utah's test is similar to *Daubert*, but is more rigid).

[65] See New State Ice Co. v. Liebmann, 285 U.S. 262, 311 (1932) (Brandeis, J., dissenting) ("It is one of the happy incidents of the federal system that a single courageous State may, if its citizens choose, serve as a laboratory; and try novel social and economic experiments without risk to the rest of the country.").

[66] See Gatowski et al., supra note 23, at 454–55 (advocating for "more science-based judicial education").

[67] Federal Judicial Center, Reference Manual on Scientific Evidence (2d ed. 2000).

[68] See General Electric Co. v. Joiner, 522 U.S. 136, 142 (1997) (holding that abuse of discretion is the proper standard of review for district court's scientific evidentiary rulings); Bernstein & Jackson, supra note 8, at 352–66 (summarizing state adoption or rejection of *Joiner*).

pellate scrutiny would allow finer variations in admissibility standards to have more bite.

2. Tort Reform Through Procedure

More broadly, our study suggests some caveats when implementing substantive tort reform through changes in procedural rules. Placing procedural limits on tort litigation has been quite popular of late, ranging from new scientific admissibility rules, to mandatory arbitration, to limits on class certification. Studies of these procedural mechanisms, however, suggest that they often have little or no effect on ultimate outcomes. For example, a recent study of mandatory arbitration rules has shown that they have no observable effect on plaintiff awards or on litigation time.[69] Another study on the effect of two recent Supreme Court decisions limiting class settlements has reported that the decisions have had no clear effect on class action filings in federal courts.[70]

Our results shed further light on this issue, though read in light of the existing literature on *Daubert*, our findings are more subtle. In combination with previous studies that have shown that the *Daubert* decision itself had a substantial effect on the treatment of scientific evidence in federal courts and beyond, our study suggests that *Daubert's* influence was not from its doctrinal reform, but from its educative function. Therefore, any subsequent state tort reform effort that focused on doctrinal shifts from *Frye* to *Daubert* was ineffective, because any potential benefits from *Daubert* had already been realized.

3. Removal as a Metric

From a research methodology perspective, our study also suggests that the removal metric holds much promise as a research de-

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[69] Albert Yoon, Mandatory Arbitration and Civil Litigation: An Empirical Study of Medical Malpractice Litigation in the West, 6 Am. L. & Econ. Rev. 95, 118–27 (2004) (finding that the implementation of mandatory arbitration in Nevada did not lead to a statistically significant effect on how much plaintiffs recovered or the duration of their litigation, but did have a small but statistically significant downward effect on the probability that the judicial system would resolve the dispute).

[70] Niemic & Willging, supra note 32, 1–2, 23–24 (studying the effect of *Amchem Products, Inc. v. Windsor*, 521 U.S. 591 (1997) and *Ortiz v. Fibreboard Corp.*, 527 U.S. 815 (1999)).

sign, particularly for those studying the effect of procedural reforms. As with scientific admissibility, many procedural reforms do their work very early in the litigation process and have subtle effects. They are therefore difficult to study through case analyses or other metrics, which, as discussed previously, suffer from censoring effects and are perhaps suited for studying more substantive legal reforms. The removal metric offers an important, useful, and much-needed alternative.

By taking advantage of the dual federal-state system, as well as the *Erie* doctrine, the removal metric also offers a method of tapping the quantitative data readily available from state and federal court systems. That data often consists primarily of basic filing information and lacks descriptive richness, necessitating the use of more complex phenomena such as removal to extract as much information as possible.

B. Limitations

In interpreting the results, one must bear in mind some of our study's limitations. Valid use of the removal metric rests on a number of significant assumptions, which we discuss below.

1. The Effect of Evidentiary Standards on Removal Rates

An assumption necessarily made when using a removal metric is that procedural rule changes will affect defendants' decisions to remove. This assumption seems reasonable. As explained previously, the *Erie* doctrine limits defendants' ability to gain substantive legal advantages by transferring to federal court. The only advantages are therefore either procedural or intangible (judiciary quality, bias, and so forth). Given that the scientific admissibility rulings have substantive effects and may themselves be outcome determinative, one would expect that they would receive serious consideration by defense attorneys.

A serious concern would arise, however, if defendants automatically removed every case they could to federal court. In other words, if the incentives for defendants to litigate in federal court are already enormous, and the percentage of *removable* cases that are being removed is near 100%,[71] then it may be difficult to detect the effect of *Daubert* or any other procedural rule.[72]

This scenario, however, seems highly unlikely. While it is unfortunately very difficult to empirically determine the removal rate for *removable* cases,[73] survey data suggests that removal is not an automatic decision. Most notably, a 1981 survey investigating attorney forum selection strategy showed that if offered a choice, 55% of out-of-state defense attorneys preferred to litigate in federal court and 45% preferred to litigate in state court.[74] Other studies on diversity jurisdiction and forum selection, though not as directly relevant, similarly imply that attorneys consider removal an open question.[75] Furthermore, the fluctuations in removal rates observed in our study in and of themselves may suggest that removal is not automatic. While year-to-year changes in the number of eligible cases may account for some of the variation in removal rates seen, it seems unlikely that those random changes alone could account for all of the increases or decreases in removal observed.[76]

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[71] Note that this rate is quite different from the removal rate used in the study. The study defined "removal rate" as the percentage of all tort cases filed in state court that are ultimately removed to federal court. This figure is far lower than the rate at which removable cases are removed, because many of the state court cases lack diversity of citizenship.

[72] Another concern about using a removal metric is the apparent recent increase in the abuse of removal motions to increase costs and delays. See Theodore Eisenberg & Trevor Morrison, Forum Manipulation by Defendants: The Growth of Wrongful Removal to Federal Court (2004) (unpublished manuscript) (on file with author). This phenomenon, however, should not pose any problems because our study includes *Frye* states as controls, and the model controls for year-to-year variations.

[73] Calculating the removal rate for removable cases would require information on the citizenship of the parties involved in the litigation. Unfortunately, this information is typically not available in most state judicial databases.

[74] Jolanta Juszkiewicz Perlstein, Lawyers' Strategies and Diversity Jurisdiction, 3 Law & Pol'y Q. 321, 328 tbl.1 (1981) (showing baseline forum preferences of out-of-state defense attorneys as a "control group").

[75] Miller, supra note 34, at 400–23 (describing the various factors that attorneys consider in making forum choices); cf. Jerry Goldman & Kenneth S. Marks, Diversity Jurisdiction and Local Bias: A Preliminary Empirical Inquiry, 9 J. Legal Stud. 93, 100 tbl.4 (1980) (ranking various reasons why attorneys in the Chicago area prefer filing in federal court over state court).

[76] This second inference, however, has a weakness arising from the earlier insight that toxic tort litigation follows trends in which certain defendants or industries are targeted in a given year. Thus, even if the rate of removal for removable cases was 100%, there could be significant fluctuations in observed removal rates on the basis of

2. *Attorney Perception*

This study's ability to use changes in removal rates to measure the effect of scientific admissibility standards also critically relies on the ability of defense counsel to accurately judge the practical ramifications of the scientific admissibility standard adopted in his or her jurisdiction. Just as case analyses depend on judicial pronouncements and surveys depend on the perception of respondents, the removal metric depends on defense counsel's judgment.

This information source, however, is not only reasonable, but arguably better than court decisions or survey responses. Courts often have incentives to obscure their decisional processes; survey respondents are impressionistic and have no incentives to be accurate. In contrast, attorneys operating in their professional capacity—i.e., making tactical legal decisions—have huge incentives to make accurate choices. They are paid largely for their ability to ascertain the practical effect of legal rules and to predict future court behavior, and, most importantly, they want to win their case. For instance, a recent study on Supreme Court decision forecasting showed that appellate attorneys had a 92% accuracy rate, whereas academics were right only 53% of the time.[77]

3. *Amount in Controversy Requirement*

Finally, we should note that the removal metric is limited by the "amount in controversy" requirement.[78] As is well known, the requirements for diversity jurisdiction (and hence removal) not only require that the parties be from different states, but also that the amount in controversy be greater than a certain value. This requirement necessarily limits the scope of our inquiry to cases in which the damages claimed are greater than the statutory requirement (currently $75,000).[79] While one would obviously prefer not to be so limited, the limitation should not affect the validity of the

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the number of suits filed against the targeted defendant and whether removal was available to that defendant.

[77] See Theodore W. Ruger et al., The Supreme Court Forecasting Project: Legal and Political Science Approaches to Predicting Supreme Court Decisionmaking, 104 Colum. L. Rev. 1150, 1177–79, 1178 tbl.5 (2004) (cautioning, however, against reading too much into the results given the small sample size and potential selection effects).

[78] 28 U.S.C. § 1332 (2000).

[79] Id.

results. Due to the high price of experts, scientific evidence battles generally surface in cases involving high damage claims.[80] The focus of the *Daubert* decision, products liability cases, also typically have high damage claims. And finally, to the extent that *Daubert* is viewed as an element of tort reform, it is those high-claim cases that are of particular interest to policymakers, practitioners, and scholars.

C. Future Areas of Research

Looking forward, two areas of future study are particularly noteworthy:

1. Removal Rate Trends

The removal rate trends seen in Figure 8 may have broader significance than as a graphical example of the non-effect of Indiana's change to a *Daubert* standard. Although Figure 8 is rather noisy, one might speculatively tell a story about the history of the treatment of scientific evidence in the federal courts. Prior to the *Daubert* decision in 1993, each jurisdiction had some average removal rate based on considerations such as the perceived quality of the state judiciary, concerns about out-of-state bias, and so forth. After federal courts switched to *Daubert*, removal rates plummeted for one of three reasons. First, the uncertainty of the new standard encouraged many defendants to remain in state court, where at least the results were more predictable (the "devil you know" phenomenon). Second, much of the early commentary on the *Daubert* decision hailed it as a defeat for defendant corporations and a victory for plaintiffs,[81] which may have deterred some defendants from litigating in federal court. Third, and relatedly, plaintiffs in diversity cases may have increasingly filed in federal court to start, eliminating the need for defendants to remove.

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[80] See 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, 354 tbl.5 (1991) (showing trends suggesting a correlation between higher settlement offers, higher trial awards, and the greater use of experts).

[81] See, e.g., Paul M. Barrett, Justices Rule Against Business in Evidence Case—Restrictive Standard for Use of Scientific Testimony in Trials Is Struck Down, Wall St. J., June 29, 1993, at A3 (characterizing *Daubert* as a pro-plaintiff decision).

Around 1995, as practitioners gained experience in both state and federal courts, however, the removal calculus began to change. *Daubert*, at least as practiced in the federal courts, turned out to be a defendant-friendly decision, and so those initial disincentives for defendants to litigate in federal court gradually vanished. Why did the removal rates not ultimately end up at levels higher than the baseline pre-1993? One guess is that by 1997 or 1998, the softer effects of *Daubert* emphasized throughout this study had already taken hold in state courts, negating any doctrinal advantage of switching from a *Frye*-governed state court to a *Daubert*-governed federal one.

A future study could explore this hypothesis further. For example, if the story is indeed accurate, then the rate of original diversity filings should increase from 1993 to 1997, corresponding to the general decline in removal rates during the same period. Research on original filings could offer some verification of the tentative hypothesis.

2. Joiner

Some recent scholarship argues that the crucial decision in the *Daubert* trilogy is not the *Daubert* decision itself, but the Supreme Court's decision in *General Electric Company v. Joiner*, which established an abuse-of-discretion standard for reviewing of scientific admissibility determinations.[82] A future study could investigate whether state adoption of a *Joiner*-type standard—and not the difference between *Frye* and *Daubert*—is positively correlated with harsher scrutiny of scientific evidence.

CONCLUSION

The overarching goal of this Essay was to determine whether formal, doctrinal standards have any effect on scientific admissibility determinations. Nearly every discussion of scientific evidence begins with a treatment of the differences between the *Frye* and *Daubert* standards. This Essay asked candidly whether a state's adoption for *Frye* or *Daubert* has any practical impact.

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[82] 522 U.S. 136, 138–39 (1997).

Using both a preliminary study of Connecticut and the EDNY, as well as a national study of all available and relevant states, we found no evidence that *Frye* or *Daubert* makes a difference. In the preliminary study, removal rates in EDNY and Connecticut remained relatively stable from 1994 to 2000, despite Connecticut's change from a *Frye* to *Daubert* standard in 1997. In the national study, the econometric model established that the governing scientific admissibility standard was not a significant factor in determining removal rates after appropriately controlling for year-to-year and state-to-state variations.

The results of this study have both immediate and broader ramifications. For the scientific evidence field, the results suggest that debates about the practical merits and drawbacks of *Daubert* versus *Frye* may be largely superfluous, and that that energy should be refocused. In addition, our findings lend support to those scholars advocating for the uniform adoption of *Daubert* by the states. Perhaps it is time to move away from debating the merits of *Frye* versus *Daubert* and toward a broader focus on how judges actually make decisions about science.

More broadly, this study has made the first steps in developing removal as a method for measuring the effect of changes in procedural or evidentiary rules. The results suggest that doctrinal reforms do not always directly correlate with substantive changes in practice. Sometimes the power of a court decision or even a piece of legislation comes more from its underlying idea than from its technical legal effect.

Appendix A:

Removal Rates in States Used in National Study

Year	Alaska	Arizona	Arkansas	Connecticut	Florida
1990	1.69%	0.30%	1.27%	0.16%	0.58%
1991	2.39%	0.36%	1.12%	0.23%	0.73%
1992	5.15%	0.51%	1.35%	0.29%	0.61%
1993	1.39%	0.61%	1.42%	0.24%	0.54%
1994	1.49%	0.26%	1.28%	0.36%	0.43%
1995	1.17%	0.25%	0.57%	0.36%	0.31%
1996	1.79%	0.31%	1.45%	0.25%	0.46%
1997	2.39%	0.40%	1.35%	0.25%	0.76%
1998	2.83%	0.34%	1.87%	0.31%	0.58%
1999	1.62%	0.31%	2.09%	0.28%	
2000	1.94%	0.48%	1.75%	0.30%	

Removal Rates (A–F)

Year	Indiana	Kansas	Michigan	Minnesota	Missouri	New Mexico
1990	0.86%	0.52%	0.63%	1.42%	0.90%	
1991	0.86%	0.76%	0.79%	1.97%	0.88%	
1992	1.45%	1.34%	0.90%	0.98%	1.20%	
1993	1.13%	0.89%	0.64%	0.90%	1.08%	0.35%
1994	0.51%	0.65%	0.49%	0.86%	1.10%	0.93%
1995	0.55%	0.43%	0.49%	0.48%	0.66%	0.37%
1996	0.91%	0.66%	0.71%	0.80%	0.78%	0.81%
1997	1.17%	0.79%	0.97%	1.87%	1.24%	1.03%
1998	0.85%	0.79%	0.98%	0.99%	1.09%	1.01%
1999	1.13%	0.76%	0.96%	0.92%	1.14%	0.99%
2000	1.01%	0.86%	0.98%	1.12%	1.11%	1.15%

Removal Rates (I–N)

Year	New York	North Carolina	Oregon	Tennessee	Washington
1990	0.46%	0.31%		0.97%	0.61%
1991	0.56%	0.49%	0.88%	0.84%	0.24%
1992	0.55%	0.47%	0.97%	1.15%	0.43%
1993	0.61%	0.56%	1.08%	1.24%	0.38%
1994	0.71%	0.42%	1.46%	1.25%	0.28%
1995	0.57%	0.37%	0.61%	0.76%	0.16%
1996	0.83%	0.44%	0.48%	1.03%	0.38%
1997	0.80%	0.52%	0.86%	1.39%	0.23%
1998	0.80%	0.62%	0.41%	1.28%	0.25%
1999	0.85%	0.64%	0.56%	1.36%	0.32%
2000	1.01%	0.77%	0.74%	1.67%	0.48%

Removal Rates (N–W)

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