Does Anyone Get Stopped at the Gate? An Empirical Assessment of the *Daubert* Trilogy in the States

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The Supreme Court's trilogy of evidence cases, Daubert, Joiner, *and* Kumho Tire, *appear to mark a significant departure in the way scientific and expert evidence is handled in federal court. By focusing on the underlying methods used to generate the experts' conclusions,* Daubert *has the potential to impose a more rigorous standard on experts. Given this potential, some individuals have called for states to adopt the* Daubert *standards to purge "junk science" from state courts. However, there is relatively little empirical support for the notion that* Daubert *affects the quality of expert evidence. Using a large data set of state court litigation, we examine whether state adoption of the* Daubert *standards has a systematic effect on the observable characteristics of experts retained in civil cases. We find very little evidence in support of a significant* Daubert *effect, even when we do a more detailed analysis of experts in products liability cases, an area of particular concern in the expert evidence debate. These results suggest that, at the state level at least, adoption of the* Daubert *standards has not led to increasing rigor in expert testimony.*

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I. INTRODUCTION

Daubert[1] and its progeny, *Joiner*[2] and *Kumho Tire*,[3] appear to change drastically the way federal courts deal with scientific and, ultimately, all expert witness evidence. Moving away from the traditional *Frye*[4] test, which looks at whether the expert's testimony is generally accepted in the relevant scientific community,[5] *Daubert* interprets Federal Rule of Evidence 702's reliability standard as requiring methodological soundness or rigor[6] rather than scientific consensus. By charging federal judges with this inquiry into the methods underlying the expert's claims, *Daubert* places those judges as gatekeepers with wide discretion in determining the admissibility of expert testimony.[7]

Although the Court itself implied that the *Daubert* standard was more liberal than *Frye*'s general acceptance test, noting that the *Frye* test was "rigid" and runs against the general thrust of the Federal Rules of Evidence toward reducing barriers to opinion testimony,[8] most commentators now believe that *Daubert* is a more demanding standard.[9] Given the central role of expert evidence in modern civil litigation, it is not surprising that supporters of the tort reform movement began to push for the adoption of *Daubert* at the state court level,[10] while pro-plaintiff advocacy groups argued against state adoption.[11]

[1] *Daubert v Merrell Dow Pharmaceuticals*, 509 US 579 (1993).

[2] *General Electric Co v Joiner*, 522 US 136 (1997).

[3] *Kumho Tire Co v Carmichael*, 526 US 137 (1999).

[4] *Frye v United States*, 293 F 1013 (DC Cir 1923).

[5] Id at 1014.

[6] *Daubert*, 509 US at 580.

[7] *Joiner*, 522 US at 137 (clarified this point regarding judicial latitude in the gatekeeping function).

[8] *Daubert*, 509 US at 579.

[9] See Edward Cheng and Albert Yoon, *Does* Frye *or* Daubert *Matter? A Study of Scientific Admissibility Standards*, 91 Va L Rev 471, 472 (2005). For empirical evidence consistent with this belief, see Lloyd Dixon and Brian Gill, *Changes in the Standards for Admitting Expert Evidence in Federal Civil Cases since the Daubert Decision* xv (RAND 2001).

[10] See, for example, *American Tort Reform Foundation, Judicial Hellholes* 34 (2007), available at http://www.atra.org/reports/hellholes/report.pdf (visited on Nov 19, 2008).

[11] See, for example, Project on Scientific Knowledge and Public Policy, *Daubert: The Most Influential Supreme Court Ruling You've Never Heard Of* (2003), available at http://www.defendingscience.org/upload/Daubert-The-Most-Influential-Supreme-Court-Decision-You-ve-Never-Heard-Of-2003.pdf (visited on Nov 19, 2008).

Despite conventional legal wisdom and the efforts of advocacy groups, however, there is very little evidence regarding the causal effects of state adoption of the *Daubert* trilogy. Even at the federal level, it is not generally possible to draw causal inferences regarding *Daubert* as distinct from a general trend toward skepticism of expert evidence.[12]

In this Article we present the first systematic analysis of the effects of state adoption of each of the three parts of the *Daubert* trilogy, using a large representative data set of trials across different case types. We look specifically at whether experts from various disciplines are introduced in the disputes as well as at the objective qualifications of the experts who are introduced as they relate to the state evidence rules. We find very little evidence that state adoption matters along either dimension. To provide a more complete picture of the effect of expert evidence rules in the products liability context—an area of particular concern to commentators and activists—we collect more detailed information on the experts offered in these disputes, again finding that adoption of the *Daubert* standards is of little consequence. We are unable to determine whether these non-effects are due to inconsistent application of the *Daubert* standards at the state level or whether *Daubert* itself is inconsequential relative to more general trends toward demanding greater rigor from experts, regardless of the formal rule. However, our results do suggest that claims about the importance of *Daubert* are overblown.

The Article is organized as follows: Section II provides background information regarding the *Daubert* trilogy; Section III discusses the patterns of adoption of the federal standards in state courts; Section IV describes our data source and statistical identification strategy with results, including the products liability specific analysis, presented in Section V. After outlining the shortcomings of our study in Section VI, we conclude.

II. THE *DAUBERT* TRILOGY

Before 1993, federal courts applied a test of "general acceptance" to determine whether to admit scientific evidence as directed by the

[12] Note that concerns over "junk science" predate the 1993 *Daubert* decision. For example, a 1985 Department of Justice report criticizes the trend toward using junk science to demonstrate the causation element in tort suits. *Report of the Tort Policy Working Group on the Causes, Extent and Policy Implications of the Current Crisis in Insurance Availability and Affordability* (1986). ERIC Document Reproduction Service No. ED274437. Also, Peter Huber's *Galileo's Revenge: Junk Science in the Courtroom* (Basic 1991) brought popular attention to the supposed problem.

holding of *Frye v United States*.[13] Under this test, courts simply asked whether the evidence represented the consensus view of the relevant scientific community or literature. However, in *Daubert*, the Court unanimously decided that *Frye* was no longer the standard for admissibility under Rule 702 of the Federal Rules of Evidence.

Noting that *Frye* was not mentioned in the drafting history of Rule 702, the Court declared the general acceptance rule to be too rigid.[14] However, the Court did not interpret Rule 702 as eliminating all constraints on admissibility of expert evidence. Instead, the Court stated that trial judges must ensure that expert evidence is both relevant and reliable according to Rule 702.[15]

In determining reliability, the Court takes a Popperian view that defines science as the generation and testing of falsifiable hypotheses.[16] To guide trial judges, the Court lays out a number of criteria for determining reliability, all of which relate to this underlying view of what constitutes science—namely, rigorous empirical methods. Among the elements a trial court should consider when determining whether to admit scientific evidence are peer review and publication, which, the Court notes, increase the likelihood that methodological flaws will be discovered.[17] The Court also directs trial judges to consider the underlying method's error rate and fidelity to established methodological standards. Last, the Court does suggest that acceptance within the relevant scientific community can be considered in the admissibility decision, although it is not a sufficient condition for admissibility.[18] Above all, the Court stresses that the trial judge's analysis is a flexible one that must focus on methodological rigor and soundness of the underlying evidence to determine its reliability.[19]

Perhaps because of the Court's seemingly inconsistent aspirations—both a preference for removing barriers to the admissibility of expert evidence[20] and a quest for methodological rigor[21]—questions arose as to whether the *Daubert* standard applied symmetrically to

[13] *Frye v United States*, 293 F 1013 (DC Cir 1923).

[14] *Daubert*, 509 US at 588.

[15] Id at 589.

[16] Id at 593.

[17] However, the Court does note that publication and peer review weigh against work that is innovative, although methodologically sound, and it suggests that publication and peer review are informative but not necessary conditions for admitting evidence.

[18] *Daubert*, 509 US at 594.

[19] Id at 595.

[20] Id at 588.

[21] Id at 595.

both the admission and the exclusion of evidence, as well as to what degree of latitude a trial judge has in questioning the connection between an expert's conclusion and the underlying method used to reach that conclusion. These questions were largely answered in *Joiner*, which held that the *Daubert* standard applies symmetrically in decisions both to allow and to exclude expert evidence.[22] Further, the Court stated that the trial judge in *Joiner* operated within his gatekeeper capacity in ruling that the *Joiner* experts were merely speculating when they extrapolated the results of methodologically sound animal studies to support their conclusions regarding the effects of PCBs on the development of cancer in humans.[23] The decision in *Joiner* affirmed that the *Daubert* standard provides trial judges with wide latitude in basing admissibility on their own evaluation of the reliability of the underlying methods used to reach a scientific conclusion and their connection to the facts of the case before them.

The last part of the *Daubert* trilogy, *Kumho Tire*, clarified the domain of the *Daubert* analysis. Namely, in *Kumho Tire* the Court makes it clear that a trial judge's focus on methodological rigor is not restricted to scientific evidence, but includes all expert evidence.[24] The Court also makes it clear that the specific criteria laid out in *Daubert* are only illustrative, not necessary preconditions for admissibility, noting that some may not be relevant in certain fields of expertise.[25] That is, *Daubert* directs trial judges to focus on methodological soundness in their gatekeeping role, but it does not provide a cookbook approach to the admissibility inquiry.

III. ADOPTION OF *DAUBERT*, *JOINER*, AND *KUMHO TIRE* IN THE STATES

Within three months of the Court's adoption of the *Daubert* standards,[26] New Mexico had embraced the them;[27] three other states followed suit by the end of that year.[28] By 2005, nearly half the states had adopted *Daubert* as the framework for determining the admissibility of expert evidence, and many of those went on to adopt the positions taken in *Joiner* and *Kumho Tire*. Using the work of David

[22] *Joiner*, 522 US at 517.
[23] Id at 518.
[24] *Kumho Tire,* 526 US at 147.
[25] Id at 151.
[26] June 28, 1993.
[27] Aug 30, 1993.
[28] Vermont (Nov 29, 1993), Louisiana (Nov 30, 1993), and West Virginia (Dec 13, 1993).

Bernstein and Jeffrey Jackson[29] as our starting point, we code the date of adoption of *Daubert*, *Joiner*, and *Kumho Tire* by each state as well as provide some indication of the state's pre-1993 expert evidence rule. Table 1 provides the details of this analysis.

Table 1. Year of Adoption of *Daubert* Trilogy in the States

State	Pre-1993 Standard	*Daubert*	*Joiner*	*Kumho Tire*	Relevant Cites
Alabama	*Frye*	2002 (limited application)			586 S 2d 242, 247; Ala Code 36-18-30; 842 S 2d 689, 690
Alaska	*Frye*	1999	1999		718 P 2d 129; 974 P 2d 386; 110 P 3d 982
Arizona	*Frye*				780 SW 2d 581
Arkansas	Relevancy approach	2000	2003	2003	20 SW 2d 429; 14 SW 3d 512; 100 SW 3d 715
California	*Frye*				130 Cal Rptr 144;
Colorado	"Sufficiently advanced"				637 P 2d 354; 22 P 3d 68,77
Connecticut	*Frye*	1997	2001		534 A 2d 877; 698 A 2d 739
Delaware	DE Rules of Evidence	1999	1999	1999	510 A 2d 488; 737 A 2d 513
Florida	*Frye*				471 S 2d 9
Georgia	Admissible if jurors couldn't reach conclusion on their own	2005	2005	2005	277 SE 2d 678; 2005 Ga Laws 1 (SB 3)
Hawaii	Modified test including *Frye*		2001	2001	645 P 2d 1330; 19 P 3d 42, 56-57
Idaho	Totality of circumstances				682 P 2d 571
Illinois	*Frye*				88 Ill 2d 225, 241

[29] David Bernstein and Jeffrey Jackson, *The* Daubert *Trilogy in the States*, 44 Jurimet J 351 (2004).

Table 1. (*continued*)

State	Pre-1993 Standard	*Daubert*	*Joiner*	*Kumho Tire*	Relevant Cites
Indiana	*Frye*	Helpful but not binding			833 NE 2d 93; Ind Rule Evid 702
Iowa	Ad hoc		1999		297 NW 2d 80; 590 NW 2d 525
Kansas	*Frye*				895 P 2d 1238
Kentucky	*Frye* equivalent	1995	2000	2000	777 SW 2d 930; 908 S W 2d 100; 11 SW 3d 575
Louisiana	Balancing test	1993	2001	2000	368 S 2d 975; 628 S 2d 1116; 774 S 2d 1022; 793 S 2d 336
Maine	Relevance and qualifications of expert				388 A 2d 500, 503
Maryland	*Frye*				391 A 2d 364
Massachusetts	*Daubert*-like	1994	2000	2000	641 NE 2d 1342; 733 NE 2d 1042
Michigan	Court as gate-keeper; Frye in 1999				443 NW 2d 340; 607 NW 2d 123
Minnesota	*Frye*				459 NW 2d 332
Mississippi	*Frye*	2003	2003	2003	612 S 2d 381; Miss Rule Evid 702
Missouri	*Frye* until 1997				700 SW 2d 823; 936 SW 2d 797
Montana	Trial judge's discretion	1994	1996		Barmmeyer v Mont Power Co; 885 P 2d 457; 909 P 2d 1171
Nebraska	*Frye*	2001	2001	2001	457 NW 2d 405; 631 NW 2d 862
Nevada	Trustworthy				765 P 2d 1147
New Hampshire	Trial judge's discretion	2002		2002	574 A 2d 934; 813 A 2d 409; 814 A 2d 159
New Jersey	*Frye* equivalent				478 A 2d 364

Table 1. (*continued*)

State	Pre-1993 Standard	*Daubert*	*Joiner*	*Kumho Tire*	Relevant Cites
New Mexico	Trial judge's discretion	1993			671 P 2d 640; 861 P 2d 192
New York	*Frye*				552 NY S 2d 883
North Carolina	Reliability				393 SE 2d 847
North Dakota	Trial judge's discretion				460 NW 2d 400
Ohio	Trial judge's discretion	1998	1998	1999	446 NE 2d 444; 694 NE 2d 1332; 714 NE 2d 426
Oklahoma	Reliability	2003	2003	2003	687 P 2d 106, 115; 65 P 3d 591
Oregon	Assistance to the jury				639 P 2d 1264
Pennsylvania	*Frye*				436 A 2d 170
Rhode Island	*Frye*	2001	1999	2001	545 A 2d 1014; 729 A 2d 677; 772 A 2d 1056
South Carolina	Four factor test				392 SE 2d 781
South Dakota	*Frye*	1994	2000	2000	429 NW 2d 26; 512 NW 2d 482; 609 NW 2d 456
Tennessee	Multifactor		2002	2002	955 SW 2d 257,265 78 SW 3d 817
Texas	Trial judge's discretion	1995	1998	1999	923 SW 2d 549; 88 SW 3d 623
Utah	Inherent reliability				775 P 2d 388
Vermont	Reasonable assistance	1993		2004	264 A 2d 779; 643 A 2d 226; 862 A 2d 269
Virginia	Similar to *Daubert*				393 SE 2d 609, 621
Washington	*Frye*				755 P 2d 806
West Virginia	Rule 702	1993			443 SE 2d 196
Wisconsin	Relevance				351 NW 2d 469

Table 1. (*continued*)

State	Pre-1993 Standard	*Daubert*	*Joiner*	*Kumho Tire*	Relevant Cites
Wyoming	Relevance and helpfulness	1999	2002	1999	Wyo Rule Evid 702; 859 P 2d 85; 984 P 2d 467; 60 P 3d 151

Source: David Bernstein and Jeffrey Jackson, *The* Daubert *Trilogy in the States*, 44 Jurimet J 351 (2004); as well as authors' search of Westlaw and Lexis case databases.

As discussed at the outset, both sides of the more general tort reform debate became invested in the question whether or not state courts should follow the federal approach. Edward Cheng and Albert Yoon note that although both sides of the debate initially viewed *Daubert* as favoring their cause, the conventional wisdom quickly converged to the view that *Daubert* is pro-defendant because it induces judges to scrutinize plaintiffs' claims more closely.[30] Because of this shift, each side mounted efforts at the state level either to encourage adoption (pro-defendant groups) or to discourage it (pro-plaintiff groups).[31] Despite the conventional wisdom, however, there is relatively little evidence of the causal effects of adopting *Daubert*, *Joiner*, or *Kumho Tire* at the state level on various litigation metrics.

There is some evidence that judicial scrutiny increased at the federal level after *Daubert*'s adoption. A 2001 RAND study examined how judges handled expert opinions in 399 federal district court cases between January 1980 and June 1999, especially whether the judge addressed the issue of the evidence's reliability and whether the evidence was found to be unreliable conditional on addressing the issue.[32] Although the study does conclude that federal district court judges were more likely to scrutinize the reliability of expert evidence after *Daubert*,[33] there were important fluctuations in that trend. Namely, while the likelihood of a judge's examining the reliability of an expert's testimony rose to 80 percent by the period July 1995–June 1996, compared with just under 70 percent during the two years before *Daubert*, the same figure had previously been at

[30] Cheng and Yoon, 91 Va L Rev at 471 (cited in note 9).

[31] See proviso notes.

[32] Lloyd Dixon and Brian Gill, *Changes in the Standards for Admitting Expert Evidence in Federal Civil Cases since the Daubert Decision*, xiii (RAND 2001), available online at http://www.rand.org/pubs/monograph_reports/2005/MR1439.pdf.

[33] Id at 61.

80 percent for the period January 1980–June 1989.[34] Comparable patterns were found for the likelihood that a judge would find the evidence to fail the reliability criteria.[35] Last, this pattern of increasing scrutiny appears to have peaked by June 1997 (90 percent of evidence subjected to a reliability analysis) and exhibited a decline in the last two years of the RAND data (July 1997–June 1999).[36] In separate analyses by case type, the RAND researchers found that reliability scrutiny actually declined slightly for product liability and toxic tort cases in the two-year period directly following *Daubert* although it increased in subsequent periods.[37]

The RAND study finds slightly stronger support for the proposition that *Daubert* induced judges to scrutinize the relevance of an expert's testimony more closely. In the data set, the RAND researchers find an unbroken upward trend in the likelihood that a judge addresses the issue of relevance with respect to expert evidence during the six years following *Daubert*. However, the likelihood that evidence is found to be unreliable exhibits significant variation with a rate at the end of the sample (July 1997–June 1999) that is virtually identical to that found at the beginning of the sample (January 1980–June 1989).[38]

The variability of these findings is puzzling in the light of the conventional wisdom that *Daubert* led to stricter scrutiny. Perhaps, the very nature of statistical analyses makes it difficult to isolate subtle changes that are observed by participants in the underlying litigation, such as attorneys and judges. Perhaps parties, induced by stricter scrutiny, find better experts, an effect that does not show up in trends regarding how frequently judges scrutinize testimony. That is, even though judges are not explicitly addressing reliability and relevance much more post *Daubert*, the fact that they are addressing them at comparable rates on a set of more qualified experts could imply that the system is more rigorous post *Daubert*. This explanation does not seem to be borne out in the data. The RAND study finds that the likelihood of an expert's being found to be unqualified remains basically constant throughout the sample,[39] as does the likelihood that a given expert exhibits objective indicators of quality.[40]

[34] Id at 28 (Figure 4-1).

[35] Id at 28 (Figure 4-1).

[36] Dixon and Gill, *Changes in the Standards for Admitting Expert Evidence* at 28 (Figure 4-1) (cited in note 32).

[37] Id at 30 (Figure 4-2).

[38] Id at 50 (Figure 6-1).

[39] Id at 51 (Figure 6-2).

[40] Dixon and Gill, *Changes in the Standards for Admitting Expert Evidence* at 72 (Table A-6) (cited in note 32).

In terms of ultimate outcomes, although the RAND study finds that parties more frequently requested summary judgment on the basis of shortcomings in the other side's expert evidence after the adoption of *Daubert*, the likelihood of such a request being granted was lower in every sample period post *Daubert* than it was in the January 1980–June 1989 period.[41]

Evidence of a *Daubert* effect at the state level is elusive as well. In a research approach mirroring the RAND study, researchers from the National Center for State Courts examined a sample of products liability cases[42] from the Delaware court system.[43] Delaware adopted the *Daubert* trilogy in 1999. The researchers found no difference pre and post *Daubert* adoption in the likelihood of a motion to exclude an expert witness and in the likelihood of a summary judgment's being entered,[44] although these results must be viewed with caution since the sample includes only fifty-seven cases.[45] In addition to those case reviews, researchers from the National Center for State Courts interviewed attorneys and judges from Delaware. Although those attorneys and judges claim that Delaware courts scrutinize experts more thoroughly post adoption,[46] the researchers conclude that the impact of the *Daubert* trilogy has been minimal in Delaware courts.[47]

In a more comprehensive study of *Daubert's* effects at the state level, Cheng and Yoon employ a creative strategy whereby they examine the rate at which defendants request removal to federal court in tort cases as a function of whether the state in which the plaintiff files the case has adopted the *Daubert* standard. For a preliminary analysis, they look at the Eastern District of New York[48] and the District of Connecticut. Because Connecticut adopts *Daubert* in May of 1997, Cheng and Yoon are able to exploit the two state comparison as a kind of natural experiment.[49] Cheng and Yoon compare the removal rate in Connecticut before and after the adoption of *Daubert*. Because the expert evidence rule does not change in New

[41] Id at 57 (Table 7-3).

[42] Specifically, the sample was drawn from Sussex, Kent, and New Castle counties.

[43] Nicole L. Waters and Jessica P. Hodge, *The Effects of the* Daubert *Trilogy in Delaware Superior Court*, (Natl Ctr for State Courts 2005),available at http://www.ncsconline.org/D_Research/Res_Daubert_EffDaubDelawareSupCtFinal.pdf.

[44] Id at15.

[45] Id at 14.

[46] Id at 16–18.

[47] Waters and Hodge, *The Effects of the* Daubert *Trilogy in Delaware Superior Court* at 21 (cited in note 43).

[48] New York retains the *Frye* rule throughout their sample period.

[49] Cheng and Yoon, 91 Va L Rev at 485 (cited in note 9).

York, the removal rate in New York serves as a control or comparison group to net out any non-*Daubert* temporal effects in terms of changes in the incentive for defendants to seek removal during the post–May 1997 period.

Effectively, if *Daubert* has an effect on the admissibility of expert evidence that is beneficial to defendants, on average, defendants will attempt to remove their cases to federal court where *Daubert* is in force, unless the state of filing is also governed by the *Daubert* standard. In the Cheng and Yoon framework, if they observe that removal rates for cases in Connecticut state courts decline after May 1997, and there is no contemporaneous decline in New York state court cases, it is plausible that Connecticut's adoption of *Daubert* led to admissibility decisions that were more defendant friendly. However, if no such change is observed, confidence in that hypothesis is diminished.

Examining the period 1994–2000, they find that removal rates increase slightly in the Eastern District of New York after May 1997, while removal rates in Connecticut remain stable. Although this finding is consistent with the hypothesis that *Daubert* changed admissibility standards in Connecticut, making those courts more defendant friendly, the effect is not statistically significant.[50] That is, while it appears that *Daubert* is associated with Connecticut's not following the more general trend (observed in New York) toward more removal, the effect cannot be distinguished from ordinary random variation in the data.

To expand their analysis, Cheng and Yoon examine data from sixteen other state courts, eight of which adopt *Daubert* and eight of which do not, over the period 1994–2000. Examining the same outcome, removal rates for tort cases, the authors attempt to discover whether the Connecticut experience was a more general phenomenon. While they find a very small decline in the removal rate, on average, once states adopt *Daubert*, the effect amounts to a little more than one half of a percentage point and is not statistically different from zero.[51]

Cheng and Yoon's research design is very persuasive, but their identification strategy relies on the assumption that only the defendants' decisions are affected by the change in evidence standards. If a nontrivial fraction of plaintiffs prefer the *Daubert* standard, the case mix between federal and state courts will be different before and after *Daubert* is adopted, causing problems for the natural experiment framework. For example, if plaintiffs systematically prefer

[50] Id at 489.
[51] Id at 497.

the *Daubert* standard and defendants hold no preference, we would expect to observe Cheng and Yoon's results (that is, no change in removal rates) as well, even though the implication would be very different from the conclusion they draw. While such a scenario seems unlikely given conventional views of *Daubert*, the more general point remains. Any deviation from the assumption that defendants prefer *Daubert* more than plaintiffs limits the ability of Cheng and Yoon's test to identify *Daubert*'s effect.

Further, given the aggregate nature of their data, they are not able to observe any heterogeneity effect across different kinds of torts cases or across different kinds of experts. For example, it could be the case that *Daubert* has a large effect in products liability cases alone; but such a result might be interesting given the importance many commentators assign to products liability cases. Cheng and Yoon would not be able to detect such an effect because they cannot distinguish among torts cases, and products liability cases make up a relatively small fraction of total torts cases. Last, Cheng and Yoon are unable to examine whether the other parts of the *Daubert* trilogy have any effect given their sample.

To confront these limitations of the Cheng and Yoon study, we adopt a complementary approach that uses a large-scale data set containing dispute specific information to examine the effect of state adoption of the *Daubert* trilogy. Further, by focusing on the characteristics of the experts offered in these disputes, we avoid having to make assumptions about which side in a dispute is more likely to prefer a given evidence standard.

IV. TRIAL DATA AND STATISTICAL APPROACH

We examine data from the Jury Verdict Research (JVR) Company, which collects data on cases, both tried and settled, from state courts throughout the country.[52] Although the JVR is not a random sample, the biases identified in the JVR do not appear to be related to case attributes that correlate with which expert witnesses are used.[53] From the text file for each case in the database, we extracted information on the experts offered by both sides in the dispute. Of interest for this study, the information includes the expert's field of expertise and whether the expert includes a graduate or professional degree in

[52] The JVR data are described in detail in Eric Helland, Jonathan Klick, and Alexander Tabarrok, *Data Watch: Tort-uring the Data*, 19 J Econ Persp 207, 213–14 (2005). The data are available through the Westlaw database LRP-JV.

[53] For a more complete discussion of the shortcomings in the JVR data, see id.

his or her title. We examine cases between 1990 and 2003. Table 2 provides the breakdown of cases by year in our sample. In total, we examine 9,125 cases that span every state with the distribution shown in Table 3. Given that the average case in our sample has 1.53 experts, we have 14,048 expert-case observations.

Table 2. JVR Cases by Year

Year	Cases
1990	929
1991	914
1992	558
1993	627
1994	737
1995	1,137
1996	1,225
1997	1,429
1998	1,605
1999	1,781
2000	1,366
2001	1,134
2002	525
2003	81

We examine regressions analyzing the relationship between the likelihood that an expert from a given field[54] is put forth in a case and whether the state where the case is filed has adopted *Daubert*, *Joiner*, or *Kumho Tire*, controlling for state and year fixed effects.[55] The state fixed effects net out any state-to-state heterogeneity in the baseline use of various experts, while the year fixed effects capture any universal changes over time. We also examine the likelihood that an expert has a Ph.D. or some other degree listed, in the same

[54] We restrict attention to those fields for which a nontrivial number of experts appeared in the full JVR data set.

[55] We do not examine cases in which no expert testifies. It is unclear whether the absence of a listed expert in the JVR data reflects no expert testimony in the case or an omission in the reported case. In results not shown we find similar results when we estimate the models treating those cases without reported experts as having no expert testimony.

Table 3. JVR Cases by States

State	Cases
Alaska	18
Alabama	167
Arkansas	222
Arizona	27
California	1721
Colorado	31
Connecticut	160
Delaware	16
Florida	938
Georgia	157
Hawaii	6
Iowa	181
Idaho	48
Illinois	710
Indiana	18
Kansas	32
Kentucky	27
Louisiana	53
Massachusetts	67
Maryland	68
Maine	17
Michigan	109
Minnesota	33
Missouri	541
Mississippi	10
Montana	17
North Carolina	250
North Dakota	*1*
Nebraska	241
New Hampshire	47
New Jersey	43
New Mexico	3

Table 3. (*continued*)

State	Cases
Nevada	4
New York	650
Ohio	442
Oklahoma	203
Oregon	200
Pennsylvania	209
Rhode Island	13
South Carolina	31
South Dakota	5
Tennessee	166
Texas	494
Utah	17
Virginia	218
Vermont	*1*
Washington	227
Wisconsin	236
West Virginia	10
Wyoming	20

Note: North Dakota and Vermont eliminated from regressions as they each had only one case.

framework. In addition to analyzing data from the full JVR sample, we examine case type specific data to determine whether there is any effect heterogeneity across different kinds of cases. We estimate probit models, meaning that our dependent variable in each case takes the value of zero (that is, expert not offered) or one (expert offered). These models allow us to interpret the resulting coefficients as the change in probability associated with the adoption of a given evidence standard.[56] We also include control variables for the severity of the injury (major, minor, death, and emotional injuries

[56] We present the marginal effects of each variable estimated at the sample mean. Our results are robust to using a linear probability model or logit model.

without a physical injury). In addition we include controls for the types of cases: sexual assault, sexual harassment, wrongful termination, premises liability, railroad liability, government liability, police liability, employer liability (excluding wrongful termination), medical malpractice, bad faith, auto case, product liability, aircraft liability, and legal malpractice. These case-type controls allow us to account for differences in the baseline likelihood that a given expert is offered in a particular kind of case.

This kind of analysis has the potential to highlight important changes brought about by *Daubert*'s focus on methodological rigor. For example, because methodological rigor is often stressed in fields where the terminal degree is a research doctorate (for example, a Ph.D.) as opposed to a taught degree (such as an M.D.), if adoption of *Daubert* really leads to an emphasis on methodological rigor, we might expect to see a significant decline in the likelihood that a medical doctor is offered in a case as opposed to a toxicologist with a Ph.D.

V. WHAT HAPPENS WHEN STATES ADOPT *DAUBERT*?

Before presenting the regression results, in Table 4, we provide the means and their associated standard errors for the likelihood that each kind of expert appears in cases in state-by-year cells where the Frye (or some other non-*Daubert*) standard is in force (columns i and ii) and where the *Daubert* standard applies (column iii). Column ii restricts attention to those states that eventually adopt *Daubert*. That is, these are the means while *Frye* (or another non-*Daubert* standard) governs in states that later switch to *Daubert*. This column provides some insight into whether states that eventually adopt *Daubert* are somehow different than other states with respect to expert evidence, helping to flag any sample selection or endogeneity problems with treating any differences associated with adopting *Daubert* as being causally related to the use of the *Daubert* standard. In general, these unconditional means provide some evidence about the differences generated by the adoption of *Daubert* and provides context for interpreting the regression results that follow.

Although there are no particularly remarkable findings in Table 4, especially relative to the underlying variation in the data, the specialties seem to fall roughly into three categories. In each case, the likelihood of an expert of a given specialty being offered in a case is lower in states that eventually adopt *Daubert* during the pre-adoption period. Once *Daubert* is adopted, the likelihood stays lower than non-*Daubert* states for: neurologists/neurosurgeons;

Table 4. Mean Likelihood that Expert in Given Field Is Offered in a JVR Case: All Case Types (Standard Errors in Parentheses)

	All States	Eventual *Daubert* States Only	
	Frye or Other	*Frye* or Other	*Daubert*
	i	ii	iii
Surgeon	0.22346	0.15007	0.21452
	(0.00363)	(0.00791)	(0.01362)
Psychiatrist	0.02321	0.01373	0.02200
	(0.00131)	(0.00258)	(0.00487)
Neurologist/neurosurgeon	0.10084	0.05002	0.07921
	(0.00263)	(0.00483)	(0.00896)
Psychologist	0.05320	0.02452	0.03300
	(0.00196)	(0.00343)	(0.00593)
Medical doctor	0.66824	0.49828	0.69857
	(0.00418)	(0.01108)	(0.01523)
Chiropractor	0.06553	0.04365	0.07811
	(0.00216)	(0.00453)	(0.00891)
Doctor of osteopathy	0.32902	0.27612	0.34543
	(0.00410)	(0.00990)	(0.01578)
Podiatrist	0.00350	0.00098	0.00330
	(0.00052)	(0.00069)	(0.00190)
Nurse	0.00632	0.00294	0.00880
	(0.00069)	(0.00120)	(0.00310)
Dentist	0.01096	0.00785	0.00880
	(0.00091)	(0.00195)	(0.00310)
Epidemiologist	0.00464	0.00147	0.00770
	(0.00059)	(0.00085)	(0.00290)
Economist	0.03798	0.01520	0.02530
	(0.00217)	(0.00271)	(0.00521)
Accountant	0.00426	0.00343	0.00770
	(0.00057)	(0.00130)	(0.00290)
Vocational expert	0.00274	0.00098	0.00110
	(0.00046)	(0.00069)	(0.00110)
Engineer	0.07421	0.03580	0.05611
	(0.00229)	(0.00412)	(0.00764)

Table 4. (*continued*)

	All States	Eventual *Daubert* States Only	
	Frye or Other	*Frye* or Other	*Daubert*
	i	ii	iii
Accident reconstruction expert	0.04087	0.02158	0.03080
	(0.00173)	(0.00322)	(0.00573)
Toxicologist	0.00556	0.00343	0.01100
	(0.00065)	(0.00130)	(0.00346)
Attorney	0.00396	0.00098	0.00330
	(0.00055)	(0.00069)	(0.00190)

psychologists; dentists; economists; vocational experts; engineers; and accident reconstruction experts. Among those specialties where eventual *Daubert* states converge to the mean likelihood observed in non-*Daubert* states are: surgeons; psychiatrists; medical doctors; doctors of osteopathy; podiatrists; and attorneys. Lastly, the specialties where the mean likelihoods observed in eventual *Daubert* states increase after adoption to exceed the likelihoods observed in non-*Daubert* states are: chiropractors; nurses; epidemiologists; accountants; and toxicologists.

Similar (though not identical) patterns emerge when we examine the means separately for medical malpractice cases (Table 5), automobile cases (Table 6); products liability cases (Table 7), and premises liability cases (Table 8).

Table 5. Mean Likelihood Expert from a Given Field Is Offered in a JVR Medical Malpractice Case (Standard Errors in Parentheses)

	All States	Eventual *Daubert* States Only	
	Frye or Other	*Frye* or Other	*Daubert*
	i	ii	iii
Surgeon	0.28065	0.30918	0.30159
	(0.00970)	(0.03220)	(0.04105)
Psychiatrist	0.02424	0.00966	0.01587
	(0.00332)	(0.00682)	(0.01118)

Table 5. (*continued*)

	All States	Eventual *Daubert* States Only	
	Frye or Other	*Frye* or Other	Daubert
	i	ii	iii
Neurologist/neurosurgeon	0.09091	0.03865	0.05556
	(0.00621)	(0.01343)	(0.02049)
Psychologist	0.03963	0.00966	0.02381
	(0.00421)	(0.00682)	(0.01364)
Medical doctor	0.78695	0.67150	0.76984
	(0.00884)	(0.03272)	(0.03765)
Chiropractor	0.00699	0.00483	0.00000
	(0.00180)	(0.00483)	(0.00000)
Doctor of osteopathy	0.42657	0.32850	0.38889
	(0.01068)	(0.03272)	(0.04360)
Podiatrist	0.00746	0.00000	0.00794
	(0.00186)	(0.00000)	(0.00794)
Nurse	0.03077	0.02415	0.06349
	(0.00373)	(0.01070)	(0.02181)
Dentist	0.01772	0.02899	0.03175
	(0.00285)	(0.01169)	(0.01568)
Epidemiologist	0.02098	0.00483	0.02381
	(0.00310)	(0.00483)	(0.01364)
Economist	0.04848	0.00966	0.04762
	(0.00464)	(0.00682)	(0.01905)
Accountant	0.00280	0.00000	0.00000
	(0.00114)	(0.00000)	(0.00000)
Vocational expert	0.00186	0.00000	0.00000
	(0.00093)	(0.00000)	(0.00000)
Engineer	0.00280	0.00000	0.00000
	(0.00114)	(0.00000)	(0.00000)
Accident reconstruction expert	0.00047	0.00000	0.00000
	(0.00047)	(0.00000)	(0.00000)
Toxicologist	0.00466	0.00000	0.00000
	(0.00147)	(0.00000)	(0.00000)
Attorney	0.00047	0.00000	0.00000
	(0.00047)	(0.00000)	(0.00000)

Table 6. Mean Likelihood Expert from a Given Field Is Offered in a JVR Automobile Case (Standard Errors in Parentheses)

	All States	Eventual *Daubert* States Only	
	Frye or Other	*Frye* or Other	*Daubert*
	i	ii	iii
Surgeon	0.22381	0.12927	0.21327
	(0.00519)	(0.00957)	(0.01996)
Psychiatrist	0.01273	0.00569	0.01185
	(0.00140)	(0.00215)	(0.00527)
Neurologist/neurosurgeon	0.12013	0.05772	0.09242
	(0.00405)	(0.00665)	(0.01411)
Psychologist	0.03368	0.01545	0.00711
	(0.00225)	(0.00352)	(0.00409)
Medical doctor	0.71256	0.50000	0.80332
	(0.00564)	(0.01426)	(0.01937)
Chiropractor	0.11718	0.06423	0.15166
	(0.00401)	(0.00699)	(0.01748)
Doctor of osteopathy	0.34425	0.28780	0.42654
	(0.00592)	(0.01291)	(0.02410)
Podiatrist	0.00202	0.00163	0.00000
	(0.00056)	(0.00115)	(0.00000)
Nurse	0.00062	0.00000	0.00000
	(0.00031)	(0.00000)	(0.00000)
Dentist	0.01086	0.00407	0.00474
	(0.00129)	(0.00181)	(0.00335)
Epidemiologist	0.00016	0.00000	0.00000
	(0.00016)	(0.00000)	(0.00000)
Economist	0.01769	0.00569	0.00711
	(0.00164)	(0.00215)	(0.00409)
Accountant	0.00124	0.00081	0.00000
	(0.00044)	(0.00081)	(0.00000)
Vocational expert	0.00062	0.00081	0.00000
	(0.00031)	(0.00081)	(0.00000)

Table 6. (*continued*)

	All States	Eventual *Daubert* States Only	
	Frye or Other	*Frye* or Other	*Daubert*
	i	ii	iii
Engineer	0.04842	0.01138	0.01659
	(0.00267)	(0.00303)	(0.00622)
Accident reconstruction expert	0.05634	0.02358	0.02844
	(0.00287)	(0.00433)	(0.00810)
Toxicologist	0.00466	0.00325	0.00474
	(0.00085)	(0.00162)	(0.00335)
Attorney	0.00016	0.00000	0.00000
	(0.00016)	(0.00000)	(0.00000)

Table 7. Mean Likelihood Expert from a Given Field Is Offered in a JVR Products Liability Case (Standard Errors in Parentheses)

	All States	Eventual *Daubert* States Only	
	Frye or Other	*Frye* or Other	*Daubert*
	i	ii	iii
Surgeon	0.10070	0.14815	0.10256
	(0.01030)	(0.03972)	(0.02817)
Psychiatrist	0.01639	0.01235	0.04274
	(0.00435)	(0.01235)	(0.01878)
Neurologist/neurosurgeon	0.04918	0.08642	0.04274
	(0.00740)	(0.03141)	(0.01878)
Psychologist	0.06323	0.03704	0.02564
	(0.00833)	(0.02111)	(0.01468)
Medical doctor	0.40749	0.41975	0.49573
	(0.02083)	(0.05518)	(0.04642)
Chiropractor	0.00468	0.00000	0.00000
	(0.00234)	(0.00000)	(0.00000)

Table 7. (*continued*)

	All States	Eventual *Daubert* States Only	
	Frye or Other	*Frye* or Other	*Daubert*
	i	ii	iii
Doctor of osteopathy	0.22014	0.22222	0.18803
	(0.01419)	(0.04648)	(0.03628)
Podiatrist	0.00351	0.00000	0.00000
	(0.00203)	(0.00000)	(0.00000)
Nurse	0.00468	0.00000	0.00000
	(0.00234)	(0.00000)	(0.00000)
Dentist	0.00820	0.01235	0.00000
	(0.00309)	(0.01235)	(0.00000)
Epidemiologist	0.01171	0.01235	0.00855
	(0.00368)	(0.01235)	(0.00855)
Economist	0.04215	0.02469	0.04274
	(0.00688)	(0.01735)	(0.01878)
Accountant	0.00468	0.00000	0.00000
	(0.00234)	(0.00000)	(0.00000)
Vocational expert	0.00234	0.00000	0.00855
	(0.00166)	(0.00000)	(0.00855)
Engineer	0.35363	0.32099	0.23932
	(0.01637)	(0.05220)	(0.03961)
Accident reconstruction expert	0.09133	0.07407	0.11966
	(0.00986)	(0.02928)	(0.03013)
Toxicologist	0.01054	0.00000	0.05983
	(0.00350)	(0.00000)	(0.02202)
Attorney	0.00000	0.00000	0.00000
	(0.00000)	(0.00000)	(0.00000)

Table 8. Mean Likelihood Expert from a Given Field Is Offered in a JVR Premesis Liability Case (Standard Errors in Parentheses)

	All States	Eventual *Daubert* States Only	
	Frye or Other	*Frye* or Other	*Daubert*
	i	ii	iii
Surgeon	0.24663	0.14328	0.28966
	(0.00899)	(0.01917)	(0.03780)
Psychiatrist	0.02740	0.02388	0.02759
	(0.00341)	(0.00835)	(0.01365)
Neurologist/neurosurgeon	0.08786	0.03284	0.13103
	(0.00591)	(0.00975)	(0.02812)
Psychologist	0.07960	0.04478	0.08276
	(0.00565)	(0.01132)	(0.02296)
Medical doctor	0.61896	0.42090	0.63448
	(0.01013)	(0.02701)	(0.04013)
Chiropractor	0.02827	0.02687	0.04138
	(0.00346)	(0.00885)	(0.01660)
Doctor of osteopathy	0.29404	0.22090	0.24828
	(0.00950)	(0.02270)	(0.03600)
Podiatrist	0.00565	0.00000	0.01379
	(0.00156)	(0.00000)	(0.00972)
Nurse	0.00130	0.00299	0.00000
	(0.00075)	(0.00299)	(0.00000)
Dentist	0.00870	0.00896	0.01379
	(0.00194)	(0.00515)	(0.00972)
Epidemiologist	0.00087	0.00299	0.00000
	(0.00062)	(0.00299)	(0.00000)
Economist	0.04828	0.03284	0.01379
	(0.00639)	(0.00975)	(0.00972)
Accountant	0.00913	0.01493	0.00000
	(0.00198)	(0.00663)	(0.00000)
Vocational expert	0.00522	0.00299	0.00000
	(0.00150)	(0.00299)	(0.00000)

Table 8. (*continued*)

	All States	Eventual *Daubert* States Only	
	Frye or Other	*Frye* or Other	*Daubert*
	i	ii	iii
Engineer	0.09830	0.05970	0.07586
	(0.00621)	(0.01296)	(0.02206)
Accident reconstruction expert	0.02784	0.01194	0.00000
	(0.00343)	(0.00594)	(0.00000)
Toxicologist	0.00652	0.00597	0.00000
	(0.00168)	(0.00422)	(0.00000)
Attorney	0.00261	0.00299	0.01379
	(0.00106)	(0.00299)	(0.00972)

Although the results in Tables 4–8 are descriptively interesting primarily because they do not show any strong *Daubert* effect (at least relative to the underlying variation in the data), the implicit comparison could be misleading if there are general background trends in the data that are obscured by looking only at the unconditional means. To control for these effects, we examine regressions that control for year fixed effects as well as idiosyncratic state fixed effects.

We first present regression results using the entire JVR sample in Table 9. Each row represents a separate regression where we examine the likelihood that each kind of expert is offered in the case as a function of the adoption of the various parts of the *Daubert* trilogy, controlling for state and year fixed effects. The coefficient in each column represents the change in likelihood that each kind of expert is offered associated with state adoption of each of the parts of the trilogy.[57]

While most of the coefficients cannot be distinguished from zero, we find a few potentially interesting associations. For example, the

[57] N/A signifies coefficients that we could not estimate given limitations in the data (i.e., too few observations where the given specialty was observed in states adopting the given standard).

Table 9. Effect of *Daubert*, *Joiner*, and *Kumho Tire* on Which Experts Are Offered (Standard Error in Parentheses)

	Daubert	*Joiner*	*Kumho Tire*
Surgeon	0.046*	−0.021	−0.036
	(0.027)	(0.041)	(0.030)
Psychiatrist	0.007	0.038	0.001
	(0.012)	(0.035)	(0.012)
Neurologist/neurosurgeon	0.014	0.005	−0.002
	(0.020)	(0.036)	(0.026)
Psychologist	−0.023***		0.019
	(0.009)	N/A	(0.025)
Medical doctor	−0.006	0.021	−0.011
	(0.030)	(0.055)	(0.041)
Chiropractor	0.010	−0.002	0.010
	(0.011)	(0.015)	(0.015)
Doctor of Osteopathy	−0.064**	0.051	−0.024
	(0.025)	(0.054)	(0.037)
Podiatrist	0.001	0.995***	−0.002***
	(0.003)	(0.001)	(0.001)
Nurse	−0.001***	0.013	0.013
	(0.001)	(0.017)	(0.030)
Dentist	0.000		0.003
	(0.005)	N/A	(0.009)
Epidemiologist	−0.001		0.002
	(0.000)	N/A	(0.004)
Economist	−0.017***		0.076**
	(0.005)	N/A	(0.038)
Accountant	−0.002		0.060
	(0.002)	N/A	(0.061)
Vocational expert	−0.002***		0.980***
	(0.001)	N/A	(0.004)
Engineer	−0.000		−0.016*
	(0.011)	N/A	(0.008)

Table 9. (*continued*)

	Daubert	*Joiner*	*Kumho Tire*
Accident reconstruction expert	−0.004		−0.006
	(0.005)	N/A	(0.005)
Toxicologist	−0.004**		0.025
	(0.002)	N/A	(0.032)
Attorney	0.001		−0.000
	(0.002)	N/A	(0.000)

Note: Each row represents the marginal effects from a separate regression including state and year fixed effects. Standard errors are clustered by state to allow for dependence of observations across time. Statistical significance is denoted as follows: ***p < 0.01; **p < 0.05; *p < 0.10. All p values are defined relative to the two sided hypothesis that the effect of adopting the given standard = 0.

likelihood of using a psychologist as an expert appears to decline with the adoption of *Daubert*, though much of this effect is undone once *Kumho Tire* is adopted. A similar pattern is observed with economists and toxicologists.

In general, we find that the effects of each part of the *Daubert* trilogy are not all of the same sign for a given specialty. Given this finding, it is useful to examine the net effect of adopting any part of the trilogy. These results are presented in Table 10. Again, although most of the effects are not distinguishable from zero, there are some exceptions. Psychologists are less likely to be offered as experts if a state adopts some part of the trilogy, although this result may be an artifact of selection bias since it disappears if we restrict attention to only those states that eventually adopt *Daubert*. Doctors of osteopathy also appear to fare poorly once some part of the *Daubert* trilogy is adopted, and this result largely survives (though the effect is smaller in magnitude) if we restrict the sample.

To examine objective measures of the quality of experts offered, we also exploit the fact that the JVR includes information regarding whether the expert includes an indication of academic credentials in her title. Given the limitations inherent in the JVR documentation, it is not possible to know how complete this information is, but there is no obvious reason why inclusion of this information should systematically vary with the evidence rules in a state, suggesting that no bias should arise due to incomplete information in this regard.

Table 10. Net Effect of Adopting Any Part of Daubert Trilogy (Standard Error in Parentheses)

	All States	Eventual *Daubert* States Only
Surgeon	0.023	0.009
	(0.022)	(0.025)
Psychiatrist	0.016	0.003
	(0.012)	(0.007)
Neurologist/neurosurgeon	0.014	0.007
	(0.017)	(0.020)
Psychologist	−0.019**	−0.005
	(0.008)	(0.010)
Medical doctor	−0.072	−0.007
	(0.130)	(0.146)
Chiropractor	0.012	−0.010
	(0.010)	(0.021)
Doctor of Osteopathy	−0.064***	−0.037
	(0.021)	(0.030)
Podiatrist	0.002	−0.004
	(0.004)	(0.006)
Nurse	0.000	−0.124
	(0.001)	(0.137)
Dentist	−0.000	−0.169**
	(0.004)	(0.066)
Epidemiologist	0.001	0.002
	(0.002)	(0.002)
Economist	0.000	0.006
	(0.009)	(0.010)
Accountant	0.005	−0.001
	(0.006)	(0.002)
Vocational expert	0.000	0.957***
	(0.005)	(0.034)
Engineer	−0.009	0.016
	(0.007)	(0.013)

Table 10. (*continued*)

	All States	Eventual *Daubert* States Only
Accident reconstruction expert	−0.007**	0.008
	(0.003)	(0.015)
Toxicologist	0.001	0.022
	(0.004)	(0.033)
Attorney	0.000	0.000
	(0.001)	(0.001)

Note: Each row represents the marginal effects from a separate regression including state and year fixed effects. Standard errors are clustered by state to allow for dependence of observations across time. Statistical significance is denoted as follows: ***p < 0.01; **p < 0.05; *p < 0.10. All p values are defined relative to the two sided hypothesis that the effect of adopting the given standard = 0.

Given the focus of *Daubert* on methodological rigor, we might expect that adoption is associated with a higher likelihood of an expert's having a Ph.D., since methodological training is generally a component of a Ph.D. program. We find no systematic effect of state adoption of any part of the trilogy on the likelihood of the expert's listing a Ph.D. This is true whether we examine all states or just those that eventually adopt *Daubert*. We find the same result when we look at the presence of any academic or professional degree in the expert's title.

Because we have limited confidence in the completeness of the JVR's inclusion of academic and professional degrees, we engaged in further data collection regarding the experts' characteristics. While it would be infeasible to track down the thousands of experts who appear in the JVR data set, it is possible to put together this information for a subset of cases. We focus on products liability cases given the special importance commentators have assigned *Daubert* in the products liability context.[58]

[58] See, for example, Project on Scientific Knowledge and Public Policy, *Daubert:* The Most Influential Supreme Court Ruling You've Never Heard Of, 3 (2003), available at http://www.defendingscience.org/upload/Daubert-The-Most-Influential-Supreme-Court-Decision-You-ve-Never-Heard-Of-2003.pdf.

Table 11. Effect of *Daubert* Trilogy on Objective Quality of Experts in JVR Product Liability Cases (Standard Errors in Parens)

	Daubert	*Joiner*	*Kumho Tire*
Top ten education	1.000***	0.288	−0.048
	(0.000)	(0.353)	(0.028)
University affiliation	−0.043	−0.224	0.066
	(0.170)	(0.213)	(0.172)
Top ten affiliation	−0.035***	0.992***	0.070***
	(0.012)	(0.003)	(0.019)
Years of experience	1.257	4.848	1.225
	(5.387)	(8.897)	(4.942)
Publication	0.067	0.013	−0.129
	(0.162)	(0.262)	(0.164)

Note: Each row represents the marginal effects from a separate regression including state and year fixed effects. Standard errors are clustered by state to allow for dependence of observations across time. Statistical significance is denoted as follows: ***p < 0.01; **p < 0.05; *p < 0.10. All p values are defined relative to the two sided hypothesis that the effect of adopting the given standard = 0.

We attempted to collect the following information for all the experts named in products liability suits in the JVR data set: (1) whether the expert's graduate or professional training occurred at a top ten university as defined by US News and World Report or the Gorman Report[59]; (2) whether the expert has a university affiliation; (3) whether that affiliation is at a top ten university as defined by US News and World Report; (4) years of experience in the field; and (5) whether the expert has any academic publications on her curriculum vitae. While none of these pieces of information is a perfect proxy for an expert's qualifications, each likely exhibits a positive correlation with quality. We examine these outcomes in the same regression framework used above. Items 1, 2, and 4 are analyzed as binary outcomes allowing us to interpret the resulting coefficients as the likelihood that there is an expert in the case exhibiting each

[59] US News and World Report began its rankings in 1983. For experts who received their degrees prior to 1983 we utilize the Gourman Report which began its rankings in 1967. For the handful of experts receiving degrees before 1967 we use the ranks from the 1967.

criterion. Years of experience are included as an integer outcome where the regression relates the adoption of the evidence standard to the years of experience the offered expert has. Each regression includes year and state fixed effects, and results are presented in Table 11 with each row representing a separate regression.

Once again, we find relatively little by way of a systematic effect of *Daubert*, *Joiner*, and *Kumho Tire* on experts. *Daubert* itself does appear to make it more likely that an expert has been educated at a top ten university, but its adoption is also associated with a lower likelihood that the expert is currently affiliated with a top ten university relative to the non-*Daubert* standards. This may reflect a combination of an increasing need for methodological rigor as provided by the top graduate programs but a reduced reliance on prestigious affiliations as opposed to the quality of an expert's work in terms of certifying herself as an expert. Both of these results survive if we collapse the adoption variable into a single indicator of whether the state has adopted any part of the trilogy.

VI. CAVEATS

Empirical work with litigation-related data suffers from a number of problems, and this study is no different in this regard. As pointed out in our review of litigation data with Tabarrok, existing civil litigation data sets are deeply flawed.[60] With respect to the JVR data in particular, there is evidence that coverage of cases and settlements differs from area to area, and, most likely, this variation is not random.[61] Although we have no reason to believe that inclusion of cases in the data set is systematically related to state expert evidence rules, we cannot rule out the possibility that our results suffer from sample selection bias. Another source of potential sample selection bias arises from which cases list the parties' experts and which do not. Because of the limited documentation regarding JVR's data collection methods, we cannot be sure that our dataset captured all of the offered experts in the cases included in the JVR dataset. Again, we have no prior indication that experts are missing in a non-random way that could bias our results, but the limited documentation precludes us from investigating this point more completely.

Another source of bias that might arise involves the fact that cases are not randomly distributed across states or across courts within a state. Given this, using pre-*Daubert* cases within a state or contemporaneous cases in non-*Daubert* states as our counterfactual

[60] Helland, Klick, and Tabarrok, 19 J Econ Perspectives at 217–18 (cited in 52).
[61] Id at 214.

or control group may not be justified. If these control cases are systematically different in terms of characteristics that also affect which experts are offered in a case, then the means and regressions we present above cannot be interpreted causally.

Further, even for our more detailed products liability, our quality proxies are very rough. For example, while it may be true that trilogy adoption has no effect on the likelihood the expert has published in an academic journal (as our results suggest), perhaps a better quality-adjusted publication measure would exhibit a systematic relationship with the adoption of *Daubert*.

Given these problems, it is appropriate to ask what value our results provide. As described above, state adoption of *Daubert*, *Joiner*, and *Kumho Tire* has generated significant interest from academic commentators, legal professionals, and activists on both sides of the issue. Empirical evidence is necessary to guide the debate and to craft optimal legal rules. Unfortunately, the relevant data are in short supply. However, it is interesting that multiple research designs, including the Cheng and Yoon removal-focused strategy and the more exhaustive case study approaches of the National Center for State Courts and the RAND Corporation, all generate qualitatively similar conclusions. Although each study is far from perfect, they are not duplicative in their shortcomings, so it is unlikely that they are all being driven by the same kind of bias. Once this point is recognized, it becomes easier to place confidence in the weight of the evidence, even if no individual study provides a decisive answer regarding the effect of adopting *Daubert* in terms of how rigorously experts are scrutinized.

VII. CONCLUSION

The *Daubert* trilogy creates a new standard for determining the admissibility of expert evidence in federal courts. Because of its focus on methodological rigor, many tort reformers trumpet the *Daubert* standards as a way to get rid of junk science in the courtroom. Conventional wisdom holds that *Daubert* led to stronger scrutiny of expert evidence in the federal courts, seemingly supporting the tort reformers' view. This has led to a related effort to encourage state courts to adopt the *Daubert* standard. Despite all these efforts, as well as the efforts of those opposing adoption on the grounds that *Daubert* is overly restrictive, there is virtually no systematic evidence supporting the view that adoption of *Daubert* makes any difference at all.

Because the existing evidence draws from either the federal courts or a very limited range of state courts, we examine this issue using a

large data set that spans almost every state over a wide range of civil case types. In this more comprehensive analysis, we too find very little evidence that adoption of the *Daubert* trilogy has any systematic effect on whom is offered as an expert in state court disputes. This is true even when we examine more detailed data in the area of products liability disputes where *Daubert* is thought to be particularly important. While we cannot determine exactly why *Daubert* seems to have no systematic effect, our results are consistent with other empirical studies on this topic. While none of these studies is perfect, their imperfections are largely orthogonal to each other, making it unlikely that design flaws or data limitations are driving this non-effect. While courts may be scrutinizing expert evidence more carefully, as suggested by the RAND research at the federal level, it seems unlikely that this has anything to do with *Daubert* per se.