Issues related to the plaintiff’s win rate in litigated cases have been discussed, and contested, for more than 30 years. In a seminal paper, Priest and Klein (1984) pointed out that it’s not an accident that some cases are litigated while others aren’t. Consider a simple litigation system that has essentially no procedural law. When disputes materialize, the parties may take them to court for a simple, one-shot adjudication in which one side or the other wins immediately (here, winning means paying nothing, if the defendant wins, or a damages judgment if the plaintiff does). Of course, the parties are also free to settle their dispute on mutually agreeable terms without going to court.

Which cases will go to court? Even the posited one-shot litigation is costly, which gives the parties an incentive to settle. It stands to reason that if the parties agreed about the likelihood that the plaintiff would win in litigation, it would be in their interests to settle, thereby saving the costs of litigation. Therefore, the cases that will be litigated will tend to be ones in which the parties have different beliefs about the plaintiff’s chances of winning.¹

Priest and Klein constructed a specific mathematical model of the simple litigation environment described above. They showed, using computer simulations, that in this particular model, the share of actually litigated cases that plaintiffs win tends to be closer to one-half than the share of cases that plaintiffs would win if all cases were litigated. Further, they showed that as the quality of parties’ information improved, the simulated plaintiff’s win rate becomes closer to one-half; as information becomes very close to perfect, the plaintiff’s win rate became indistinguishable from one-half. While Priest and Klein didn’t mathematically prove that this tendency was a general and necessary one, they did offer some heuristic mathematical arguments suggesting that the tendency toward one-half was likely to hold widely.

Finally, Priest and Klein acknowledged that the parties might regard the possibility of a plaintiff’s win differently—as, for example, when a repeat-player defendant is worried about allowing disadvantageous precedent to develop, when the plaintiff is a one-shot player who stands to gain relatively little in damages. They argued that when the stakes are higher for the defendant, we can expect the plaintiff’s win rate among litigated cases to be lower than one-half. Conversely, when the stakes are higher for the plaintiff, Priest and Klein argued that the plaintiff’s win rate would exceed one-half. As with the case of equal stakes, Priest and Klein argued that these patterns were likely to occur regardless of the share of cases that plaintiffs would win, if all cases were to be litigated.

¹ They might also differ concerning the likely damage award if the plaintiff does win. For simplicity my paper ignores this possibility. Accommodating it would add realism but would not change any of the key qualitative results. For simplicity I therefore adopt the simplifying assumption that the parties agree about what the damage award would be if the plaintiff were to win.
Following Priest and Klein’s paper, an empirical cottage industry sprung up, with papers measuring the plaintiff’s win rate in different types of cases, types of courts, and among different types of parties. Much attention was directed at “testing” the Priest-Klein “hypothesis” concerning a 50% plaintiff’s win rate. Of course, as the discussion above indicates, Priest and Klein never claimed the plaintiff’s win rate would be exactly 50%. They merely suggested a tendency in that direction when stakes are equal and parties are well informed. And they also identified factors that would push the plaintiff’s win rate away from 50%. Recently, Professors Daniel Klerman and Yoon-Ho Alex Lee of USC have formally proved most of Priest and Klein’s conjectures about these tendencies. Still, in the absence of good data on party information or relative stakes—and there is precious little real data on these types of variables—there really wasn’t anything to test about the Priest and Klein paper.

So why were legal scholars so interested in the Priest and Klein hypotheses? One very important reason has to do with the ability to use litigation outcomes to measure the state of the law in different jurisdictions or in different substantive areas. Let’s unpack that statement a bit. Suppose you get data identifying the winner in tort cases litigated in Alabama state courts. Suppose you find plaintiffs win roughly half the time in Alabama. Does that mean that Alabama tort law is equally favorable to defendants and to plaintiffs? Priest and Klein would say no; they predict a plaintiff’s win rate near one-half any time stakes are symmetric and party information about case quality is very good. What if the plaintiff’s win rate were, instead, 30%? It is possible that Alabama tort law is just more favorable to defendants than to plaintiffs. But Priest and Klein’s argument suggests that it is also possible that stakes for defendants in Alabama tort suits are very high compared to the stakes for plaintiffs; that would cause defendants to litigate even some cases that are quite strong for plaintiffs.

The take-home point from this example is that, since litigated cases are not a random sample of all disputes, using litigation outcome data to draw inferences about the state of the law is precarious. While the Priest and Klein model has been controversial, in the wake of their paper no one seriously questions the non-random nature of selection of cases for litigation. As a result, many researchers gave up on using plaintiff’s win rate data to evaluate whether torts are adjudicated more favorably to plaintiffs in Alabama than in New York, or whether Alabama torts are adjudicated more favorably to plaintiffs than, say, Alabama civil rights cases.

A basis for this surrender was aptly supplied in an influential 1996 paper by Steven Shavell, which showed that it is theoretically possible for the plaintiff’s win rate to take on any value between 0% and 100%.

Shavell used a different kind of model from the one that Priest and Klein had used. Priest and Klein assumed that the disagreements that cause parties to litigate occurred because parties with the same information just had different beliefs about who would win given commonly known facts; such a model is sometimes referred to as involving “divergent expectations.” By contrast, in Shavell’s model one party has systematically better information than the other; such models are known as “asymmetric information” models of litigation. As I discuss in section 5 of the paper for this workshop, Shavell argued that this distinction is critical for explaining his “anything goes” plaintiff’s win rate result.

---

At the same time, a number of researchers have continued to use (the functional equivalent of) the plaintiff’s win rate to try to measure changes in certain legal standards. One example is the literature following the 1986 summary judgment trilogy, which sought to evaluate not only whether the trilogy led to more summary judgment motions being filed, but also whether defendants were more likely to win them. Another example is the empirical literature on the *Twombly* and *Iqbal* pleading standard cases. If we take seriously the Priest and Klein point about systematic selection, then these approaches are problematic at best.

In another recent paper, Professors Klerman and Lee have argued otherwise. They point out that even though data on the plaintiff's win rate information might not be helpful to understand the state of the law at any given time—e.g., Is Alabama tort law more favorable to defendants?—it might still be the case that changes in the law could be detected using data on changes in the plaintiff’s win rate. Thus, Klerman and Lee argue, we might be justified in deciding that a change in Alabama tort law doctrine was pro-plaintiff if we found that Alabama plaintiffs won a higher share of tort cases after the doctrinal change. Similarly, if we were willing to assume that Alabama and New York tort cases were otherwise similar, we might be justified in evaluating whether Alabama or New York tort law standards are more pro-plaintiff by comparing the plaintiff’s win rate in tort actions in the two states.

To recap, while glossing over many details:

1. Much attention has focused on conditions under which plaintiffs will win 50% of the time—either exactly or approximately.

2. Priest and Klein suggest that when the stakes are symmetric, the selection of cases for litigation will push the plaintiff’s win rate toward 50%. They also suggest that when the plaintiff’s stakes are relatively greater than the defendant’s, selection will push the plaintiff’s win rate above 50%; when the plaintiff’s stakes are relatively less than the defendant’s, selection will push the

---


5 Daniel Klerman & Yoon-Ho Alex Lee, *Inferences from Litigated Cases* 43 J. Legal Stud. 209 (2014)

6 Of course, the assumption that these cases are otherwise comparable might be dubious. Beyond that, my view is that the conditions under which Klerman and Lee’s results hold might actually be relatively demanding. Consequently, I have doubts about the empirical usefulness of their result. But the basis for those doubts is a topic for a different paper.
plaintiff’s win rate below 50%. Lee and Klerman prove that a number of these suggestions are approximately correct, and become exactly correct in the limit as party information about case quality becomes perfect.

3. Shavell argues that any plaintiff’s win rate between 0 and 100% is possible, but only in a model with asymmetric information.

4. Klerman and Lee argue that there are reasonable conditions, for both the Priest and Klein model and Shavell’s asymmetric information model, under which changes in the plaintiff’s win rate will meaningfully inform us about the direction of change in the law.

That’s the background for the paper I’ll be presenting at this workshop.

My paper uses a different approach to modeling litigation from papers in the literature. Those papers may be thought of as “structural” in the sense that they specify the full structure either of parties’ beliefs or of their bargaining behavior. My paper instead takes both party beliefs and underlying bargaining behavior as given, focusing on what I call the “reduced form” of litigation models. Virtually every structural model in the literature may be expressed in reduced form terms. That means the results I prove using the reduced form approach will hold for structural models as well, provided that the conditions of my results are satisfied for the reduced form of those structural models. Thus my approach is general enough so that earlier structural models may be analyzed on reduced form terms; that allows qualitative comparisons to be made across otherwise very different-looking structural models. I find that very little of the received wisdom about litigation selection is robust in the sense of applying generally across reduced form litigation models.

Finally, please don’t feel any obligation to read the appendix sections. They contain some technical, mathematical derivations. They are there for those who are interested in and comfortable with the details of the arguments in the main paper, but I excluded them from the main text because they aren’t essential for those who are willing to take the math on faith. Also, section 7—especially section 7.2—is a bit mathy; you should feel free to skip it if you wish.