

SYMPOSIUM ON FRONTIERS OF EMPIRICAL PATENT LAW SCHOLARSHIP
University of North Carolina School of Law
Oct. 24, 2008

Keynote Address by the Hon. S. Jay Plager*

The name of this symposium that brings us here is FRONTIERS OF EMPIRICAL PATENT LAW SCHOLARSHIP. I congratulate Professor Andrew Chin and the UNC law review, and especially the two symposium editors, Tram Rattanavong and Michael Martinez, for organizing this most worthwhile endeavor. It is a special pleasure for me to be back at my alma mater.

I do not think it useful for me to attempt to summarize the impressive work represented by the scholars who have been brought together for this very special symposium when we will have the good fortune to hear from them personally. Instead, I want to talk about two aspects of the empirical enterprise not directly addressed in their studies—one, the more general subject of empirical scholarship, especially as a law studies enterprise, including some of the basic issues such scholarship confronts. And two, how empirical studies relate, if they do, to the area I currently work in—the judicial process and how we judges decide our cases.

At the outset, I hope you will forgive a small digression—a bit of a personal note—particularly since I have not yet gotten to the subjects from which I am digressing. I am reminded of a story about Professor Felix Frankfurter after he became a Supreme Court Justice. As an after-dinner speaker he had a propensity for loquaciousness. His wife, who sat through many of his after-dinner talks, noted that as a speaker he had two

* Circuit Judge, United States Court of Appeals for the Federal Circuit. Caveat—the views expressed are solely those of the speaker, and are not necessarily shared by his colleagues on the court. In particular, Judge Plager absolves the UNC Department of Philosophy, from which he graduated, from responsibility for any of the philosophical (or other) misunderstandings he displays in this talk.

problems—one was that he always strayed from the subject, and the other was that he often found his way back.

I discuss this business of empirical legal research with the benefit of some experience. When I was a young member of the Illinois law faculty, back in the days when the earth was boiling and the dinosaurs were roaming, I was teaching among other things estates and trusts. Don't laugh—some of you, before you became patent law savants at the cutting edge of technology and the global economy, probably taught equally exciting subjects, like contracts and torts. At least I was teaching property law, the queen of subjects.

The question of why surviving spouses were given something called a 'forced share,' which enabled them to take a piece of the decedent spouse's estate different from that which the spouse had planned, intrigued me. Its historical roots were in common law dower, but that was a form of social security before there was social security. Why now? Was it true that spouses got even for the slings and arrows of married life by dying angry and disowning their loved ones? I know this is a far cry from patent law, but bear with me.

I spent the better part of a year analyzing data gathered from New York, Wisconsin, and Illinois about estates and testamentary dispositions in the probate courts, extracting what I could from the available data in the little bit of empirical writing there was on the subject. This was, I am sad to say, at a time before public data bases became available on the internet. My piece got published in the University of Chicago Law Review under the title of "The Spouse's Nonbarrable Share: A Solution in Search of a Problem."

I learned something about empirical research. First of all, don't pick cute titles for your work—it gets lost in the indexing, and your colleagues in the field won't be able to find it. Second, it's hard work; you give up the readily-available fodder of non-copyrighted court opinions that you can make into casebooks, and from which you can earn royalties; worse, you may have to leave the comfort of your office and deal with real life; and if you need a tenure article or a thesis piece, there are a lot easier ways to get one than to poke around in data or attempt regression analyses. Even so, I was not particularly discouraged. In later years I participated in other studies, including a National Science Foundation-sponsored study of class-action litigation, which at least had the benefit of sending me to San Francisco for a summer.

In more recent years, having left the academy to work in the Government and later on the court, I did not keep as close tabs on the academic side of legal research as I had as a professor. In preparation for this gathering I looked at some of the academic literature devoted to empirically-oriented studies. The resurgence of interest in that kind of work, and the quantity of it, and indeed the creativity of much of it, is truly impressive. I am personally delighted to see this greater understanding of and appreciation for empirically focused inquiry.

Creative empirical study remains hard work, often slow work, and work not always appreciated by the dean. When I became dean of one of the Big 10 law schools, I tried to be appreciative. I encouraged my new faculty to think in terms of empirical testing of legal propositions, particularly those propositions that were of long standing and self-evident, to see whether there was any reason to continue to believe our received wisdom. I note with due appreciation the goal stated by the editors of the

recently-established Journal of Empirical Legal Studies that, “recognizing that many legal and policy debates hinge on assumptions about the operation of the legal system, the Journal seeks to encourage and promote the careful, dispassionate testing of these assumptions.” As dean I hired a number of double-discipline faculty—law and history; law and sociology; law and econ, of course. I was gratified to see that in a published ranking of the leading empirical legal scholarship schools my old school continues to rank respectably.

Now I recite this personal history not to claim membership in the empirical scholarship club, or even sympathy for my errant youth, but to make clear that none of my comments about empirical research are by way of disparaging or discouraging of the empirical enterprise, but rather are to remind us of a bit of historical background to the enterprise, and to note some of the common issues that history reveals.

An early proponent of experimental science and empirical studies was Sir Francis Bacon, a lawyer and royal court official who lived and wrote in the late 1600s and early 1700s, during the great Elizabethan Age. For reasons not relevant here, Elizabeth was not particularly kind to him, and beheaded his primary sponsor at court, M’Lord Essex, who happened also to be Elizabeth’s lover. High government office to this day is not the most secure way to make a living, and, as before, one’s sexual proclivities may not help; however, for better or worse we have stopped beheading.

You may know Sir Francis Bacon as the author of “The Elements of the Common Law of England,” published in 1597. More importantly, Sir Francis was an early force in the development of the modern scientific world view. He is often coupled with Sir Isaac

Newton as among the first of the advocates of testing and reassessing the ignorances of the antique past.

In 1620 Bacon published his masterpiece, *Novum Organum*, the New Instrument. In this work he argued for the authority of experience over texts; he advocated the need to reject traditional thought if it didn't hold up against what he considered light-shedding experiments—systematic studies of external events. This was at a time when orthodoxy did not yield readily to the secular dimension of knowledge.

Since the ground-breaking work of Bacon in social science and Newton in physical science, the history of science suggests several common concerns with empirical research as a method of inquiry. One is the problem inherent in data collection itself. In the course of time, I spent a year as a research professor at the Wisconsin law school—a law school whose faculty were known for their interest in law and society and for doing empirical work, a faculty who were in the forefront of the legal realism movement, at that time a frontier of its own. The Wisconsin faculty were careful to avoid the error some early empirical researchers fell into—thinking that if you can pile the data high enough, out of the bottom would squeeze some juice of understanding. Data-gathering is only the first step in doing worthwhile empirical studies—sometimes the easier step. More on this later.

A second problem, more a problem for hard science than for law and social sciences, is the theoretical problem of how to test propositions to determine if they are indeed universal truths. That would get us into a discussion of Karl Popper and his falsification thesis.

A more practical problem, one with which we lawyers are very familiar, is what can you really believe, what is factually true? We all know better than to accept at face value what someone tells us, unless it happens to be our mothers. But do we know enough not to believe what we think we know? The ultimate philosophical skeptic in that regard was a Frenchman named Rene Descartes. He questioned everything—do our senses lie to us? Even if they don't, do our minds process the information without distortion? Are we capable of processing scattered bits of sensory data into a cohesive picture—and if we are, can we see it? Can we communicate it to someone else, or do we lose it in the process? T.S. Eliot put it more poetically: “Where is the wisdom we have lost in knowledge? Where is the knowledge we have lost in information?”

Thinking of ways supposedly-empirical data can mislead, I am reminded of the story of the 60-year-old, the richest man in town, who married a beautiful woman 30 years younger. When asked by a friend how he persuaded the young lady to marry him, he said he lied about his age. “You mean you told her you were 45?” “No, I told her I was 85.”

Back to Descartes—as I'm sure you all remember from your sophomore year in college, Descartes even doubted whether he existed. He finally convinced himself that he did with his classic ‘cogito ergo sum’—‘I think therefore I am.’ You may know the joke about Descartes—he went into a bar and had a drink. The bartender then asked him if he would like another. He replied, “I think not” — and disappeared.

Of course the joke in a way misapprehends Descartes' point. He was not making only an ontological point—I think, therefore **I am**. He was also making an

epistemological one—I **think**, therefore I am. It was the method of inquiry, the way to determine what is, that mattered.

Later empiricists argued that if something exists, it exists to some extent; if it exists to some extent, it is measurable; if it is not measurable, then it must not exist. Gottfried Leibnitz responded to the empiricists with a classic rationalist's argument: you could build the largest machine ever, with equipment that gathered and recorded every possible bit of sensory data, and when you got through all you would have is an indigestible mass of detail—you still need that ultimate god-or-nature-given mind that can synthesize data and have new ideas.

Well, much as I know you cannot wait for me to go on like this, let me segue from the historical, and perhaps somewhat esoteric, to the immediate of today—what does all this have to say about how we judges do our work, and in particular the judges of the Federal Circuit? Our court, after all is the arbiter of patent jurisprudence, not, as the saying goes, because we are right but because we are final, at least until the Supremes choose to play in our sandbox. I personally do not mind when they do. Most of the time I agree with them, and typically I find their decisions, at least in the patent area, not necessarily that constraining.

Now, among skeptics there is no group more skeptical than judges. Oliver Wendell Holmes said, “When the ignorant are taught to doubt, they do not know what they safely may believe.” I am sure he was not thinking about lawyers, or professors, and by no means other judges.

Despite the focus on the rules and doctrines that judges elaborate on in our opinions and that constitute much of classroom law, what we often wrestle with in the

decisional process is the truth of, or even a reasonable understanding of, the facts. One reason for this tension in the judging process is the same tension between the received wisdom of the past—precedents to which as a matter of law we are bound, and to which we look for guidance and predictability—as against our own sense of reality when we are confronted with the parties’ conflicting versions of the facts, and even a desire to ascertain where justice resides. This tension between precedent and judicial experience is Bacon’s 17th century struggle between orthodoxy and the secular dimension of knowledge, brought into the 21st century.

How useful is empirical data when studying the work of the courts? One use of empirical data has been to develop pseudo-scientific methods for understanding how different judges go about deciding cases, based on various types of analyses of our prior decisions. Sometimes that effort is focused on the Supreme Court. Not surprisingly, however, because the issues there lend themselves to the interplay of politics and ideology, it is not rocket science, though an academic favorite, to explain statistically how Justice Scalia or Breyer will come down in the next contentious case.

On the other hand, in the courts of appeals where the vast majority of the federal appellate work is performed, the issues are much more likely to turn on more subtle behaviors—how a judge approaches statutory construction, or one’s view of the legitimate range for distinguishing precedent based on somewhat different facts. At bottom, many of our appellate cases invoke relatively little ideological force; put another way, they are not often result-driven. This is certainly the case in the Federal Circuit.

There have been attempts to classify the judges on the Federal Circuit using various empirically-based descriptors derived from the way we vote or write. I always

seem to fall in the middle somewhere, perhaps indicating that my writing is not very clear or my voting is equally confused. Those of us who live and work with the judges might be helpful in these efforts by classifying colleagues, using somewhat less scientific methods—perhaps indicating those who tend to be a bit stubborn, or maybe occasionally wrong-headed, or most often brilliant (those are the ones who agree with us).

Be that as it may, in part because of the centrality in recent years of innovation and invention in the American economy, patent law has departed from its earlier form of backwater possession by a small part of the legal profession, and has become a major subject of broad academic and political attention. As a result there are many frontiers of patent law scholarship, including a number that do not involve the courts, as the papers in this symposium attest. Nevertheless, because of its role in the patent system, the federal courts and in particular the Federal Circuit get their share of plaudits and criticisms. I hasten to add that I am not here to applaud or defend any particular decision we have issued. That would not be appropriate for a judge of the court.

Rather, I want to look briefly at a few of these issues and ask how and in what ways empirical evidence could be useful. Looking at the literature about our court there is no shortage of critically treated topics. I am sorry that time does not permit me to comment here on many of the issues that the scholars are writing about. I will touch only on a couple of the issues I find of particular concern.

Claim construction. It has been rightly said that claims are the basic building blocks of the patent right, so what a claim means lies at the heart of the process. The fundamental problem with claim construction today is that too many claims are no

longer describing hammers or machines or other physical objects—though we have had our share of interpretive trouble even with some simple things like what is a “board.” How much more difficult it is when inventions involve complex software technologies or biochemical processes, or even how to conduct an online auction. Too often the problem is that words, the limitations of language, are such that trying to put the invention into English following the phrase “I claim ...” is difficult, and sometimes virtually impossible, for even an adroit drafter. The problem is complicated by the reality that not all claim drafters are adroit, and by some silly conventions that grew up in simpler times like the use of magic phrases such as “comprising,” or the rule that using simple sentences in a claim is prohibited.

We had a way of dealing with all this—in an infringement suit, let the jury purport to apply its understanding of the claim to its understanding of the allegedly infringing whatever, and just tell us who wins. Worked fine, if you did not mind that the jury’s verdict was a black box, essentially unreviewable on appeal unless winning counsel was totally incompetent about getting some supporting evidence into the case.

So in the *Markman* case we came up with the brilliant idea of taking claim construction from the jury and giving it to the trial judges, whose articulated understandings we would then be able to review. I thought it a great idea, as I say, even brilliant, except there are the unintended consequences. Put aside the creation of a new industry, called *Markman* hearings. It is what happens after the *Markman* hearing that is so troubling. Many critics, including some of us on the court, think the reversal rate is unacceptable, and we need to defer more to the trial judges. But how would that work? On what basis would we prefer one trial judge’s view of an

indeterminate claim's words over our own, and how distinguish a different judge's view that we do not prefer? Other than reciting percentages of reversals, are there any empirically-based tests that would help?

There have been several efforts to test propositions empirically regarding our claim construction practice. One in particular I found intriguing—the question was whether, with more experience at doing claim construction, district judges, and in a companion study, International Trade Commission ALJs, improved their batting average in getting claims right, as measured by Federal Circuit agreement. The notion was that claim construction might be a learnable skill.

The researcher came to the conclusion that there was no empirical support for the proposition that doing more claim constructions improved the ability of a trial judge or an ALJ to get them correct, at least as measured by whether the Federal Circuit agreed with the claim construction. The researcher hypothesized that the explanation is one of three things: 1) trial judges and ALJs cannot master claim construction, especially if they are without a technical background; 2) the Federal Circuit has failed to provide useful rules and other guidance from which trial judges can learn; or 3) claim construction is inherently indeterminate.

I would hazard the guess that it is some of all three, though in my experience it is the last that is at the heart of the problem. If so, where do we go from here? Are there further empirical studies that can help? As something of a sidelight, I was interested to note that one empirical study of the *Markman* process concluded that a consequence of *Markman* was to contribute to the fading away of the doctrine of equivalents—I thought

that interesting since I had been working toward that goal—at least the goal of putting the equivalent genie back in the bottle—well before *Markman* was decided.

Turning to another favorite of mine, business method patents, the data about the enormous increase in patent applications following our *State Street* opinion are disturbing, if not distressing. Should we have not approved the patent in that case, preserving the so-called rule against business method patents, even when we could find no statutory or judicially-supported basis for the rule? I feel some responsibility for the unintended consequences of *State Street*, since I was on the opinion with Judge Rich and at the time thought that was what the law required; a year later I applied the *State Street* rationale to a process claim in *AT&T*. What can empirical studies tell us about this?

Finally, one criticism of the Federal Circuit that is a favorite among some is that we are not actively exercising our policy levers to ensure that patents today are doing the job today's world needs. As one author put it, we are too concerned with whether the law is precise—that is, reproducible and predictable—and not concerned enough with whether it is accurate. The author explains that accurate means correct, which means “responsive to the philosophy of the Patent Act, to national competition policies, and to the needs of researchers and technology users.”

In *State Street* we thought that, by extending patentability to the broad sweep of modern invention except as specifically limited by the Act itself, we were acting in full conformity with the philosophy of the Patent Act and responsively to the needs of technology users. Forgetting for the moment any role that Congress or administrative

agencies might have in these matters, the basic jurisprudential dilemma is that one judge's version of sound patent policy might be someone else's patent law nightmare.

A rather sweeping proposal by those who see the court as not sufficiently policy-oriented is to do away with having a single court of appeals handle patent cases, and return to the bad old days of having multiple appellate courts, with multiple opportunity for forum shopping. The argument is that this would make for a richer and more diverse jurisprudence, one more likely to address the underlying policy issues that the Federal Circuit seems unwilling to attend to.

This is not the time for an extended discussion of the proper role of courts in a democratic society, or of comparative institutional competence as against a legislature or an administrative agency. If you are interested, you can read the article in the recent *Northwestern University Law Review*, co-authored by Lynne Pettigrew and myself, responding to an article on the topic in the same issue by professors Nard and Duffy. More to the point of this discussion, in our reply to professors Nard and Duffy we highlight the absence of any empirical support for their thesis that more courts and more judges will produce better, rather than simply different, results. Is that an empirically testable proposition? Is it fair of us to criticize them for not having the data to support their thesis, or are we left only with the rationalists' argument that right thinking people will know what's right?

I conclude by returning to the point about whether we really know what we think we know. That is a conundrum that can be helped, but not necessarily solved, by empirical evidence. The skeptic might say that where one comes out on many of these issues depends on how one views the known facts, the empirical evidence, based on

the assumptions with which one begins. As an optimist, and a one-time empirical researcher, I believe that imperfect but nevertheless useful information is still better than no useful information. As a judge, I have learned to live with both.

Since I began with a story, I will conclude with one on the point of imperfect information and assumptions. After a long day of dealing with peoples' various ailments, a neurosurgeon friend of mine came out into her waiting room and found the last patient sitting there with a frog stuck to the top of his head. The doctor exclaimed, "good heavens, when did this happen?" The answer came back in a low raspy voice, "when I first noticed this growth on my foot."