CONSUMING THE HANGOVER: LEGAL CHANGE AND LITIGANT CHOICE

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Jurisprudential regimes theory (JRT) has argued that scholars could detect the effects of legal doctrine on Supreme Court decision-making by analyzing major changes in legal doctrine. Critics of JRT have argued that the evidence constitutes a simple statistical artifact and that appropriate analyses find no such results. I investigate whether changes in the legal regime affect judicial decision-making by changing the population of cases available for the courts to rule on. I find evidence that changes in the legal regime have contingent effects on decision-making. A decision that changed potential litigants' eligibility for bringing claims had negative effects on dispute generation and settlement payouts. A decision that made proof of claims easier for plaintiffs to establish increased litigant selection but decreased payouts, suggesting that plaintiffs may have reacted overconfidently. Finally, another decision that was believed to make plaintiffs' proof easier to show, but was widely disregarded, increased payouts in all cases. That decision had an initial positive effect on settlements that later shifted, suggesting that litigants saw the way lower courts were treating the decision and shifted away from their pro-plaintiff positioning. Taken together, these results offer evidence that merely asking if law constrains judicial decision-making is an incomplete

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inquiry. Litigants change their behavior in response to changes in the law, which affects the sample of cases that judges get to decide.

INTRODUCTION

In 2014, the Supreme Court announced a per curiam decision in *Johnson v. City of Shelby*.[1] In that case, the Court reversed a Fifth Circuit decision based on a local rule requiring that claims brought under 42 U.S.C. § 1983 plead that they arise under that statute.[2] The court of appeals justified this rule because of the unique defense of qualified immunity which applies only to § 1983 claims.[3] The Fifth Circuit argued that a complaint without the statutory reference is "fatally defective in that it fails to state a claim upon which relief may be granted . . . ."[4]

The Supreme Court summarily reversed, concluding that the Fifth Circuit's rule unlawfully imposed pleading requirements not required by the Federal Rules of Civil Procedure (FRCP).[5] The Court emphasized that Rule 8, which governs the content of pleadings, only requires "a short and plain statement of the claim showing that the pleader is entitled to relief[.]"[6] Regardless of the policy concerns underlying the Fifth Circuit's rule, dismissing claims because the pleader failed to recite the legal basis for the claim was contrary to the clear commands of the FRCP.[7]

Two years later, the Fifth Circuit changed its behavior. In *Groden v. City of Dallas*, the district court dismissed a *Monell* municipal-liability claim[8] because the plaintiff had not pleaded the identity of the municipal policymaker responsible for the allegedly unconstitutional policy.[9] The court of appeals characterized this pleading question as one of first impression, claiming that the authority the district court relied upon referred to *proof* of a

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1. Johnson v. City of Shelby, 574 U.S. 10 (2014) (per curiam).

2. *Id.* at 11–12.

3. Johnson v. City of Shelby, 743 F.3d 59, 62 (5th Cir. 2013) ("Certain consequences flow from claims under § 1983, such as the unavailability of *respondeat superior* liability, which bears on the qualified immunity analysis.").

4. *Id.* (quoting Hearth, Inc. v. Dep't of Pub. Welfare, 617 F.2d 381, 382–83 (5th Cir. 1980) (internal quotations omitted)).

5. *Johnson*, 574 U.S. at 11 (citing Leatherman v. Tarrant Cnty. Narcotics Intel. & Coordination Unit, 507 U.S. 163, 164 (1993)).

6. *Id.* (citing FED. R. CIV. P. 8(a)(2)).

7. *Id.* ("[The Rules] do not countenance dismissal of a complaint for imperfect statement of the legal theory supporting the claim asserted.").

8. Monell v. Dep't of Soc. Servs., 436 U.S. 658 (1978).

9. Groden v. City of Dallas, 826 F.3d 280, 283–84 (5th Cir. 2016).

policymaker, not *pleading* one.[10] Concluding that the *identity* of the policymaker, despite its *existence* being an element of the claim, was a question of law, the Fifth Circuit held that courts could not require *Monell* plaintiffs to identify the policymaker at the pleading stage.[11]

This story is an example of the behavior predicted by jurisprudential regimes theory, which is a reaction to behavioralist explanations of judicial decision-making that argue law is nothing more than a façade to cover up political considerations that *actually* motivate judicial decisions.[12] Contrariwise, jurisprudential regimes theory argues that essential changes in the legal regime governing claims lead to changes in outcomes[13] and that major doctrinal shifts or clarifications from the Supreme Court can create detectable changes in patterns of case outcomes after the shift.[14] While scholars have found evidence that lower courts have conditional influence over how the Court resolves legal splits among the circuit courts of appeal and have identified effects of jurisprudential regimes in the courts of appeals, work that examines the way that changes in doctrine affect lower court outcomes is rare.[15]

Jurisprudential regimes theory is not without its critics; scholars have criticized both its methodological underpinnings and

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10. *Id.* at 283 n.3 (quoting Piotrowski v. City of Houston, 237 F.3d 567, 578 (5th Cir. 2001)).

11. *Id.* at 284–85 (citing City of St. Louis v. Praprotnik, 485 U.S. 112, 124 (1988)).

12. *See* Mark J. Richards & Herbert M. Kritzer, *Jurisprudential Regimes in Supreme Court Decision Making*, 96 AM. POL. SCI. REV. 305, 305 (2002).

13. Herbert M. Kritzer & Mark J. Richards, *Jurisprudential Regimes and Supreme Court Decisionmaking: The* Lemon *Regime and Establishment Clause Cases*, 37 LAW & SOC'Y REV. 827, 836–37, 839 tbl.1 (2003) (explaining the impact of *Lemon v. Kurtzman* on the Court's Establishment Clause cases).

14. Mark J. Richards, Joseph L. Smith & Herbert M. Kritzer, *Does* Chevron *Matter?*, 28 LAW & POL'Y 444, 455–61 (2006) (providing evidence supporting the hypothesis that "*Chevron* constituted a regime change").

15. *See* Stefanie A. Lindquist & David E. Klein, *The Influence of Jurisprudential Considerations on Supreme Court Decisionmaking: A Study of Conflict Cases*, 40 LAW & SOC'Y REV. 135, 143–50 (2006) (finding that circuit splits are more likely to be resolved in the preferred direction of the majority of circuit courts that have ruled on the issue, with certain prestigious judges having outsized weight); Jennifer K. Luse, Geoffrey McGovern, Wendy L. Martinek & Sara C. Benesh, *"Such Inferior Courts . . ." Compliance by Circuits with Jurisprudential Regimes*, 37 AM. POL. RSCH. 75, 92–96 (2009) (finding evidence that the courts of appeals are likely to comply with the *Lemon* test in Establishment Clause cases).

substantive findings.[16] While its proponents still defend it, it is clear that the theory is more nuanced than initially thought.[17] For example, in *Arizona v. Gant*, the Supreme Court arguably changed the legal regime governing vehicle searches incident to arrest.[18] While an analysis of lower court decisions suggested that *Gant* did render the legal regime more friendly to defendants in suppressing searches incident to arrest, analysis of police search records indicates that law enforcement simply adopted different rationales to support their searches.[19]

One missing piece of the puzzle is the Supreme Court's ability to shape the population of cases that proceed to a decision by changing litigant perceptions of the likelihood of winning. This systematic selection of cases out of the universe of judicial decisions means that it is hard to discern any effect the Supreme Court has in changing the legal regime by looking at outcomes.[20] Any effect is as likely to express itself through the choice to bring a lawsuit or to settle it as it is through lower court decisions.

Litigants make decisions based on their understanding of the judge's predilections, their own preferences and needs, and their understanding of the law. Because judges can only decide cases

16. Jeffrey R. Lax & Kelly T. Rader, *Legal Constraints on Supreme Court Decision Making: Do Jurisprudential Regimes Exist?*, 72 J. POL. 273, 275–82 (2010) (arguing that Kritzer and Richards have used an incorrect statistical test and that using a more appropriate test provides no evidence for jurisprudential regimes); Brandon L. Bartels & Andrew J. O'Geen, *The Nature of Legal Change on the U.S. Supreme Court: Jurisprudential Regimes Theory and Its Alternatives*, 59 AM. J. POL. SCI. 880, 891–92 tbl.1 (2015) (finding that some legal doctrines remain stable, while others exhibit patterns of change consistent with JRT or incremental evolution).

17. *See generally* Herbert M. Kritzer & Mark J. Richards, *Taking and Testing Jurisprudential Regimes Seriously: A Response to Lax and Rader*, 72 J. POL. 285 (2010) (explaining limitations and complexities of the Chow tests); Bartels & O'Geen, *supra* note 16.

18. *See* Arizona v. Gant, 556 U.S. 332, 335–39, 341–346 (2009).

19. Michael C. Gizzi & R. Craig Curtis, *The Impact of* Arizona v. Gant *on Search and Seizure Law as Applied to Vehicle Searches*, 1 U. DENVER CRIM. L. REV. 30, 44–47 (2011) (analyzing lower court decisions in the wake of *Gant* and identifying several exceptions utilized by law enforcement to alleviate Fourth Amendment concerns); Ethan D. Boldt & Michael C. Gizzi, *The Implementation of Supreme Court Precedent: The Impact of* Arizona v. Gant *on Police Searches*, 6 J.L. & CTS. 355, 370–74 (2018) (finding sharp decreases in searches incident to arrest immediately post-*Gant* and sharp increases of other rationales).

20. Daniel M. Klerman & Yoon-Ho Alex Lee, *Inferences from Litigated Cases*, 43 J. LEGAL STUD. 209, 210 (2014).

that litigants submit to them,[21] the policy space in which judges can maneuver is only partially within their control. While it is true that high court judges can signal their interest in certain types of cases,[22] this process is slow, uncertain, and requires litigants to read between the lines.

Prior work on jurisprudential regimes theory has focused on constitutional doctrines and *Chevron* deference.[23] While it is impossible to overstate the importance of these issues, there are many individuals who will go their entire lives never experiencing an Establishment Clause issue, a search incident to arrest, or administrative deference. But everyone works. Workplace rules and dynamics are usually set in relation to governing law and with the expectation that violations may lead to some sort of enforcement action. Unlike most developed democracies, the United States has chosen a regulatory apparatus that is both substantively threadbare and limited in capacity.[24] American employment law covers very little by comparison to other countries in the Organisation for Economic Co-operation and Development (OECD). The regulatory agencies that enforce the limited American employment regulations have much less capacity relative to their scope than in comparator countries. But American employment law provides an additional wrinkle that most countries do not: in addition to administrative enforcement, employees in the United States usually have a private right of action to enforce their statutory rights.[25]

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21. Claire B. Wofford, *Says Who?*: *Case Participants and Legal Doctrine in the U.S. Supreme Court*, 46 J. POL. SCI. 37, 41 (2018).

22. *See* VANESSA A. BAIRD, ANSWERING THE CALL OF THE COURT: HOW JUSTICES AND LITIGANTS SET THE SUPREME COURT AGENDA 54–67 (2007) ("[I]ndications of the justices' policy priorities cause litigation efforts in those areas.").

23. *See generally* Richards & Kritzer, *supra* note 12; Richards, Smith & Kritzer, *supra* note 14; Lindquist & Klein, *supra* note 15.

24. *Compare* Miriam Hartlapp, *Enforcing Social Europe through Labour Inspectorates: Changes in Capacity and Cooperation across Europe*, 37 WEST EURO. POL. 805 (2014) (measuring European employment-law enforcement capacity), *with* Nicholas Pedriana & Robin Stryker, *The Strength of a Weak Agency: Enforcement of Title VII of the 1964 Civil Rights Act and the Expansion of State Capacity, 1965–1971*, 110 AM. J. SOCIO. 709, 725–27 (2004) (arguing that the EEOC used social-movement pressure to increase its enforcement capacity in the late 1960s).

25. Raphaël Gellert, Katja de Vries, Paul de Hert & Serge Gutwirth, *A Comparative Analysis of Anti-Discrimination and Data Protection Legislations*, *in* DISCRIMINATION AND PRIVACY IN THE INFORMATION SOCIETY 61 (Bart Custers, Toon Calders, Bart Schermer & Tal Zersky eds., 2013) (compiling European Union member states' anti-discrimination legislation); *see also* Christopher McCrudden, *Regulating Discrimination: Advice to a Legislator on Problems Regarding the Enforcement of Anti-*

This private right of action means that employment-law cases present additional puzzles for marginalized individuals. While the lion's share of the burden of employment discrimination falls on people who have historically been denied privilege or power, when the legal regime shifts either toward or against them, they frequently find that claims that were viable no longer are, or they may think that their claims have been given new life only to see a court interpret their rights away. Shifts in the legal regime can be especially devastating for unsophisticated litigants like employees who thought they were being given redress only to have it snatched away.

One of the problems that the bench and bar have is that we experience law and its effects very differently from everyone else. For those not steeped in law, they see its operation when it impinges on their life. This means that their perception of law is deeply selected. One of the selectors is salience. When the only time that someone hears about the judiciary is when it is granting them rights or taking them away, it is very easy for an individual to let that lens shape their view of law as a whole.[26] Similarly, if the only lawyers who make the news are the ones who have engaged in egregious misconduct, we can expect outsiders to view the profession as if those lawyers are normal.[27] But the bench and bar do have to grapple with the fact that our view of the system is so different from those around us. What if we've bamboozled ourselves?

In this Article, I use several pre-existing datasets of employment-law cases that include settlement information to examine how changes in jurisprudential regimes change litigant

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Discrimination Law and Strategies to Overcome Them, *in* NON-DISCRIMINATION LAW: COMPARATIVE PERSPECTIVES 296 (Titia Loenen & Peter R. Rodrigues eds., 1999).

26. *See* Michael J. Nelson & James L. Gibson, *Has Trump Trumped the Courts?*, 93 N.Y.U. L. REV. 32, 34–35 (2018) (describing the impact of Donald Trump's public criticism of the Supreme Court); James L. Gibson & Gregory A. Caldeira, *Has Legal Realism Damaged the Legitimacy of the U.S. Supreme Court?*, 45 LAW & SOC'Y REV. 195, 207 (2011) (finding that belief in mechanical jurisprudence is not a necessary condition for Americans to believe that the U.S. Supreme Court is legitimate); James L. Gibson & Michael J. Nelson, *Reconsidering Positivity Theory: What Roles Do Politicization, Ideological Disagreement, and Legal Realism Play in Shaping U.S. Supreme Court Legitimacy?*, 14 J. EMPIRICAL LEGAL STUD. 592, 604–05 (2017) ("[I]nstitutional support is strongly related to perceived judicial politicization . . . .").

27. *See* Malinda L. Seymore, *Specialty Bar Associations and the Marketing of Ethics: The Example of the Academy of Adoption Attorneys*, 35 NOTRE DAME J.L. ETHICS & PUB. POL'Y 49, 92 (2021) ("Maintaining high public esteem can be a valuable marketing position . . . .").

choices. I find substantial evidence that *Sutton v. United Air Lines*[28] created a substantial shift in the legal regime governing claims under the Americans with Disabilities Act of 1990 (ADA); it affected the numbers of disabled employees seeking relief; the share of enforcement resources devoted to ADA claims; and reduced the amounts plaintiffs received in settlements—although its effects on payouts in all cases are more difficult to discern. I find further evidence that *Reeves v. Sanderson Plumbing Products, Inc.*[29] led to an exuberant burst of Equal Employment Opportunity Commission (EEOC) enforcement actions seeking to pursue newly viable claims that judges and juries viewed skeptically. Finally, I find evidence that *Desert Palace, Inc. v. Costa*[30] increased plaintiff payouts in all cases and, as a subset, in settled cases, although the effect on settlements was tempered as lower courts registered their disagreement with the initial assessment of *Desert Palace*'s effects.

This analysis carries multiple implications. It suggests that judges' decisions shape the contours of the legal system in ways that reach far beyond the courthouse doors. It reminds us that disputes have a life beyond litigation and that the people who make decisions about dispute resolution are more than the judges themselves. These findings have disturbing implications for anyone who thinks that the justice system should offer litigants substantial justice—they illustrate the broader pattern of ongoing reification of power and privilege being effected by the judicial system in the form of creating a regime that protects those in power from the consequences of their own actions, even against the command of Congress or the Supreme Court.

To some extent, it may be plausible to see the Court's efforts at changing the jurisprudential regime as a wild party.[31] At the end, everyone is left with a vague understanding of what happened and with the task of integrating their new understanding into a broader social fabric. Much like how a hangover prevents those who drink to excess from understanding the full consequences of their actions until time has passed, my findings suggest that the

28. Sutton v. United Air Lines, Inc., 527 U.S. 471 (1999).

29. Reeves v. Sanderson Plumbing Prods., Inc., 530 U.S. 133 (2000).

30. Desert Palace, Inc. v. Costa, 539 U.S. 90 (2003).

31. As a caution against getting too wedded to this metaphor, I offer Matthew Reid Krell, *Judging as Soccer: Jurisprudence, Legisprudence, and Metaphor*, 10 ELON L. REV. 125, 140–41 (2018) (arguing that a variety of metaphors prevents any one metaphor from reifying itself and limiting our mental capacity to evaluate the phenomenon in question).

Supreme Court's influence is limited because immediate litigation consequences are clouded by jumbled legal actions. Primarily, litigants—and not monolithic Court precedent—exert control over how the legal regime affects which disputes become ripe for decision. These findings deepen our understanding of the relationship between courts and litigants, further support the self-conception of the legal system against the perceptions of critics, and suggest that there is space for litigants to respond to and resist exercises of judicial power. In a world where the Court appears poised to position itself as the final and unaccountable arbiter of all exercises of state power,[32] that space is more important now than ever.

The remainder of this Article proceeds in four sections. Section I begins with an overview of the doctrinal expectations developed from the subject, explains a theoretic concern that limits prior work's ability to make inferences, and introduces jurisprudential regimes theory in detail. Section II derives the hypotheses that I test from the preceding theoretical discussion and discusses the data and methods that I employ. Section III reports the results of the statistical analysis and discusses the implications of these findings on our understanding of the relationship between law and litigant behavior. Finally, I offer concluding thoughts about the import of these findings both to our understanding of litigation and to the relationships between courts and litigants.

I. THE STATE OF KNOWLEDGE

A. THE DOCTRINE: CHANGING EMPLOYMENT LAW IN THE 1990s AND 2000s

The success of employment cases depends on numerous factors, only some of which can be discussed here. Representation by counsel is one important factor.[33] Parker finds that plaintiffs are more likely to win, and more likely to settle, if represented by counsel.[34] She argues that the presence of plaintiff's counsel serves two functions: a screening function (thus weeding out particularly

32. *See generally* STEPHEN VLADECK, THE SHADOW DOCKET: HOW THE SUPREME COURT USES STEALTH RULINGS TO AMASS POWER AND UNDERMINE THE REPUBLIC (Basic Books 2023).

33. Wendy Parker, *Lessons in Losing: Race Discrimination in Employment*, 81 NOTRE DAME L. REV. 889, 915 (2006).

34. *Id.* at 915 n.120.

weak claims and signaling that the claim is strong) and a competence function (ensuring that the plaintiff's case is litigated on the merits and not dismissed due to a procedural fault or because some key piece of evidence is not discovered or incorrectly interpreted).[35] "The decision of an attorney to accept the case, along with [their] efforts to win the case, should indicate not only some merit to the claim but also increased chances that the case will be competently litigated."[36] While this finding is intuitive, Parker cautions against overinterpretation due to the small number of unrepresented plaintiffs in her dataset: "[T]he win rate . . . where plaintiffs have attorney representation is only 30%, as compared to the overall win rate of 27%."[37] Berger, Finkelstein, and Cheung find similar results between all cases and represented cases.[38]

Efforts to determine the relative difficulty of plaintiff victories across areas of law suffer from sharp disagreement over the appropriate methods to analyze the question. Some scholars argue that statistical patterns of outcomes are superior because they avoid concerns about judges masking their decisions through legal rhetoric. Others prefer to analyze the doctrine in legal decisions that apply the law to determine whether judges apply more stringent standards to certain types of plaintiffs. Both approaches are defensible given their own assumptions, but both are to some extent speaking past one another. My goal here is to integrate both camps into a single understanding of how expectations in employment-law claims can be calibrated based on the type of claim presented.

1. QUANTITATIVE ANALYSIS OF CASE OUTCOMES IN EMPLOYMENT LAW

Within the group of statistical approaches, low-powered studies frequently find no difference between areas of law.[39] For

35. *Id.* at 915–16.

36. *Id.* (footnote omitted).

37. *Id.* at 916 (footnote omitted).

38. Vivian Berger, Michael O. Finkelstein & Kenneth Cheung, *Summary Judgment Benchmarks for Settling Employment Discrimination Lawsuits,* 23 HOFSTRA LAB. & EMP. L.J. 45, 56 (2005).

39. Statistical power is the probability that if the hypothesis is true, the analysis will correctly confirm it. Small samples often lack statistical power. *See generally* PAUL D. ELLIS, THE ESSENTIAL GUIDE TO EFFECT SIZES: STATISTICAL POWER, META-ANALYSIS, AND THE INTERPRETATION OF RESEARCH RESULTS 3, 52, 59, 78, 102, 131 (2010).

example, Parker finds no difference between issue areas but cautions that her findings are in some cases predicated on samples as small as eighty-two cases.[40] Berger, Finkelstein, and Cheung reach a similar conclusion using samples as small as twenty-nine cases, with the largest only seventy-two cases, although they do discern a difference in win rates for gender-discrimination plaintiffs.[41] It appears that both of these analyses suffer from insufficient observations to discern the differences between areas of law.

Other empirical scholars, using more data, have identified differences but their conclusions are hard to reconcile with one another. Rutherglen concludes that plaintiffs with claims under the Age Discrimination in Employment Act of 1967 (ADEA) are more likely to prevail than plaintiffs with other claims.[42] His conclusion is based on the finding that ADEA plaintiffs, on average, receive greater monetary relief compared to plaintiffs who bring claims under other employment-discrimination statutes.[43] He hypothesizes that ADEA claims are more valuable because age-discrimination plaintiffs are more likely to be high earners and thus be entitled to larger back-pay awards; they are more likely to get attractive settlement offers for various reasons; and they are more likely to be White men who elicit sympathy from an older, whiter, maler jury pool than currently exists.[44] However, Clermont and Schwab find little difference between types of claims related to plaintiff win rates both prior to and at trial.[45] Ultimately, they conclude that employment-discrimination plaintiffs face "a tough row to hoe."[46]

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40. Parker, *supra* note 33, at 928–31, 949 tbl.A3. There are other methodological critiques of Parker's analysis that are beyond the scope of this Article.

41. Berger, Finkelstein & Cheung, *supra* note 38, at 59–60 tbl.5. I also note that the smallest sample consists of an "other" category amalgamating three types of claims that do not necessarily present similar issues (national-origin, religion, and "reverse" discrimination). *Id.* at 60 tbl.5.

42. George Rutherglen, *From Race to Age: The Expanding Scope of Employment Discrimination Law*, 24 J. LEGAL STUD. 491, 504–09 (1995).

43. *Id.* at 509–16 ("[T]he average recovery for each ADEA action brought by the EEOC . . . was two-and-one-half times the average recovery in each Title VII case and over four times the average recovery in [Equal Pay Act cases].").

44. *See id.* at 516. Rutherglen's analysis is based on data from the mid-1980s. *Id.*

45. Kevin M. Clermont & Stewart J. Schwab, *How Employment Discrimination Plaintiffs Fare in Federal Court*, 1 J. EMPIRICAL LEGAL STUD. 429, 444–46 tbl.2 (2004) ("[D]ifferences in win rates seem driven much more by the type of disposition than the type of discrimination at issue.").

46. *Id.* at 429.

2. DOCTRINAL ANALYSIS OF CASE OUTCOMES IN EMPLOYMENT LAW

Doctrinal analyses present a more complicated question, and the divergence of results between the empirical analyses above and the work of doctrinal scholars raises important questions about the power of law to predict case outcomes. One scholar concluded that age claims were easiest, followed by gender-discrimination claims, disability, and race claims, respectively.[47] Another analysis suggests gender claims appear easier because traditional gender-discrimination claims are lumped with sexual harassment claims, which are frequently regarded as always presenting questions of fact.[48]

The differing difficulty of claims matters because the legal model of judicial decision-making suggests that the law which governs a claim should drive the outcome.[49] Social scientists have insisted for decades that judges decide cases based on their ideological predilections, sometimes tempered by other considerations.[50] Yet, the evidence for this in lower court decision-making is minimal. Epstein, Landes, and Posner insist that there is no evidence of any significant ideological influence on district judges.[51] Zorn and Bowie similarly find that district court judges are less likely to enact their policy preferences than are judges farther up the judicial hierarchy.[52] If these scholars are correct, and district court judges decide cases in accordance with the law, then why is there a divergence between what doctrinal scholars

47. *See* Michael Selmi, *Why Are Employment Discrimination Cases So Hard to Win?*, 61 LA. L. REV. 555, 562–69 (2001).

48. *See* Berger, Finkelstein & Cheung, *supra* note 38, at 60–61.

49. Howard Gillman, *What's Law Got to Do With It? Judicial Behavioralists Test the "Legal Model" of Judicial Decision Making*, 26 LAW & SOC. INQUIRY 465, 468–74 (2001). *See generally* Barry Friedman & Andrew D. Martin, *Looking for Law in All the Wrong Places: Some Suggestions for Modeling Legal Decisionmaking*, *in* WHAT'S LAW GOT TO DO WITH IT? WHAT JUDGES DO AND WHAT TO DO ABOUT IT 1 (Charles Geyh ed., 2011).

50. *See generally* JEFFREY A. SEGAL & HOWARD J. SPAETH, THE SUPREME COURT AND THE ATTITUDINAL MODEL REVISITED 1–3 (2003); Lee Epstein & Jack Knight, *Strategic Accounts of Judging, in* THE ROUTLEDGE HANDBOOK OF JUDICIAL BEHAVIOR 48, 53–54 (Robert M. Howard & Kirk A. Randazzo eds., 2017).

51. LEE EPSTEIN, WILLIAM M. LANDES & RICHARD A. POSNER, THE BEHAVIOR OF FEDERAL JUDGES: A THEORETICAL AND EMPIRICAL STUDY OF RATIONAL CHOICE 217 (2013).

52. Christopher Zorn & Jennifer Barnes Bowie, *Ideological Influences on Decision Making in the Federal Judicial Hierarchy: An Empirical Assessment*, 72 J. POL. 1212, 1218 (2010).

like Selmi expect and what empirical scholars like Clermont and Schwab and Berger, Finkelstein, and Cheung find?

3. RECONCILING PAST FINDINGS

This puzzle suggests that the first step in reconciling these findings should involve an independent doctrinal analysis. For various reasons, I have elected to consider data from various sources, all covering varying time ranges between 1997 and 2021. First, the EEOC enforced four statutes in the time period under consideration: Title VII of the Civil Rights Act of 1964 (as amended by the Civil Rights Act of 1991) (Title VII),[53] the Equal Pay Act (EPA),[54] the ADEA,[55] and the ADA.[56] Second, during the time period in question, the Supreme Court decided *Sutton v. United Air Lines, Inc.*,[57] which severely limited the definition of "disability" under the ADA, and *Reeves v. Sanderson Plumbing Products, Inc.*[58] and *Desert Palace, Inc. v. Costa*,[59] both of which arguably changed the necessary proof required of a plaintiff to defeat summary judgment in an employment-discrimination claim.

During the relevant time period, plaintiffs could prove claims under any of the statutes the EEOC enforced by demonstrating that their protected classification was a "motivating factor" in the adverse employment action.[60] A plaintiff could demonstrate motivating-factor evidence one of two ways: either the plaintiff could produce "direct evidence," usually characterized as a "smoking gun" sort of admission from the defendant;[61] or they could rely on "circumstantial evidence," which courts analyzed through a burden-shifting framework first outlined by the

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53. 42 U.S.C. § 2000e-4.

54. 29 U.S.C. § 206(d).

55. *Id.* § 621.

56. 42 U.S.C. § 12101.

57. *Sutton*, 527 U.S. 471 (1999).

58. *Reeves*, 530 U.S. 133 (2000).

59. *Desert Palace*, 539 U.S. 90 (2003).

60. *Compare* Price Waterhouse v. Hopkins, 490 U.S. 228, 258 (1989) (if plaintiff proves gender played a motivating part in adverse employment action, burden shifts to defendant to prove same action defense), *with* Gross v. FBL Fin. Servs. Inc., 557 U.S. 167, 173–74 (2009) ("Title VII [] explicitly authoriz[es] discrimination claims in which an improper consideration was a 'motivating factor' . . . .") (no burden shifting in ADEA claims).

61. *E.g.,* Holtz v. Rockefeller & Co., 258 F.3d 62, 76–77 (2d Cir. 2001) ("[D]irect evidence of discrimination—a 'smoking gun' . . . attesting to a discriminatory intent . . . is typically unavailable . . . .") (internal quotations and citations omitted).

Supreme Court in *McDonnell Douglas Corp. v. Green*.[62] Under this framework, a plaintiff who made a prima facie case of discrimination—which consisted of a showing that they belonged to a protected class, that they were qualified to do the job, that they suffered an adverse employment action, and that the job went to someone outside their protected category—was entitled to a presumption of discrimination.[63] At that point, the employer had to produce a legitimate, nondiscriminatory motive that would explain their actions.[64] Once they did so, the plaintiff then bore the burden of showing that the employer's proffered motive was pretext for discrimination.[65] The employee could overcome this hurdle either by showing that discrimination was the actual motive, or, after 2000, that the proffered motive was false.[66]

Each of the statutes had its own wrinkles that introduced obstacles or opportunities for plaintiffs. EPA claims required proof of a comparator: an individual of a different gender from the plaintiff who performed similar work for more pay.[67] However, once the necessary comparator evidence was shown, liability was strict.[68] Title VII and ADEA claims required showings of discriminatory animus, which could be shown indirectly, but courts were usually skeptical of plaintiff efforts to infer animus absent some direct statement.[69] ADA claims did not require a comparator or animus, but they required that the plaintiff be disabled, and

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62. *See generally* McDonnell Douglas Corp. v. Green, 411 U.S. 792, 802 (1973).

63. *E.g.*, Tex. Dep't of Cmty. Aff. v. Burdine, 450 U.S. 248, 253 n.6 (1981) (citing *McDonnell Douglas*, 411 U.S. at 802).

64. *E.g.*, *id.* at 254–56.

65. St. Mary's Honor Ctr. v. Hicks, 505 U.S. 502, 515–16 (1993).

66. *Reeves*, 530 U.S. at 147 ("Proof that the defendant's explanation is unworthy of credence is simply one form of circumstantial evidence that is probative of intentional discrimination . . . .").

67. *See, e.g.,* Spencer v. Va. State Univ., 919 F.3d 199, 203–04 (4th Cir. 2019) ("[S]imilar [work] . . . consider[s] 'whether the employees (i) held [] same job description, (ii) were subject to [] same standards, (iii) were subordinate to [] same supervisor, and (iv) had comparable experience, education, and other qualifications—provided the employer considered these latter factors in making the personnel decision.'") (quoting Bio v. Fed. Express Corp., 424 F.3d 593, 597 (7th Cir. 2005)).

68. *See* Strag v. Bd. of Trs., Craven Cmty. Coll., 55 F.3d 943, 948 (4th Cir. 1995); *but see* Lavin-McEleney v. Marist Coll., 239 F.3d 476, 481 (2d Cir. 2001) (allowing statistical comparison after a particular comparator was identified for prima facie purposes).

69. Cary Franklin, *Discriminatory Animus*, *in* A NATION OF WIDENING OPPORTUNITIES: THE CIVIL RIGHTS ACT AT 50, at 35–37 (Ellen Katz & Samuel Bagenstos eds., 2014).

after 1999, plaintiffs whose disabilities could be remediated through treatment were no longer regarded as disabled.[70]

Traditionally, the standard of care element of a tort (such as employment discrimination) travels between four levels of increasing difficulty of proof: strict liability, negligence, gross negligence or recklessness, and intent (or knowledge). Essentially, these boil down to "the defendant never should have let this happen," "the defendant should have known this could happen," "the defendant did not care that this could happen," and "the defendant intended or knew this would happen," respectively.[71] Using these categories, the proof of the requisite mental state to show liability was easiest for EPA claims, then for ADA, and finally most difficult for Title VII and ADEA claims.[72] However, the necessity of showing a strict comparator for EPA claims makes them more difficult to prove than ADA claims, where mere negligence suffices and no comparator evidence is necessary.[73] Thus, I would rank the types of claims as ADA being easiest to prove, then EPA, and finally Title VII and ADEA claims the most difficult.[74]

While the doctrinal analysis offered here may be contested, it can also be tested statistically. Pairwise tests compare the likelihood of plaintiffs' receiving a payout in cases that present certain types of claims against one another, head-to-head. They are performed by regressing the data to predict payouts, first using the categories for both types of claims being compared, and then only with one, and seeing if there is a statistically significant difference in the predictive power of the two models. In other words, first I regress payouts for all types of claims, and then, in

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70. Alexander v. Choate, 469 U.S. 287, 302–03 n.22 (1985); *Sutton*, 527 U.S. at 477–79. *Alexander* was decided under § 504 of the Rehabilitation Act of 1975, which tracks the ADA's prohibitions of discrimination, but limits itself as an exercise of Congress's Spending Power rather than the Commerce Clause (and thus is limited to recipients of federal funds). *Id.* at 287. Section 504 cases are routinely used to interpret the ADA, and vice versa.

71. *See* Pers. Adm'r of Mass. v. Feeney, 442 U.S. 256, 279 (1979).

72. *See Strag*, 55 F.3d at 948 (EPA); *Burdine*, 450 U.S. at 253 n.6 (Title VII); *Holtz*, 258 F.3d at 76 (ADEA).

73. *Compare Strag*, 55 F.3d at 948 ("[EPA] plaintiff bears the burden of showing that she [] receive[d] lower pay than a male co-employee . . . ."), *with Burdine*, 450 U.S. at 253, *and Holtz*, 258 F.3d at 78 ("[E]vidence of disparate treatment is not an essential element of a *prima facie* case of discrimination . . . .").

74. I would also add that, as discussed *supra*, *Sutton* made ADA claims almost impossible to win, because very few employees were "disabled" under the statute.

turn, perform regressions for the same model but excluding one of the types. The method then compares the regression that includes, for example, controlling for Title VII claims with the regression that does not, to determine if controlling for Title VII gives us meaningful results. The likelihood-ratio test gives us our confidence level; the difference in coefficients between the models gives us the substantive effects.[75]

Pairwise tests produce a cacophony of results. The full regression tables and likelihood-ratio test statistics are reproduced in the Appendix,[76] but the key results are these: with 90% confidence, the data suggests that EPA claims are the easiest to prove, followed by ADA claims with 80% confidence. The data is clear that Title VII and ADEA claims are the most difficult to prove, although comparing between them is statistically impossible due to the amount of noise in the data.

I thus disagree with Selmi's conclusion that age claims are easiest for plaintiffs to win. Rutherglen suggests that this relative ease of victory is because most ADEA claims are brought by White men, and thus they have a more receptive audience in both judges and juries.[77] This would be consistent with Parker's conclusion that judges are generally skeptical of race-discrimination claims, particularly those brought by minorities.[78] In other words, the observed success of ADEA claims has less to do with the statute's ability to redress age discrimination and more to do with the particular sympathies that ADEA plaintiffs engender. This analysis seems to harmonize my doctrinal approach with prior findings.

Berger, Finkelstein, and Cheung argue that ADA claims are the most difficult to prove due to the aftereffects of the *Sutton* case.[79] While I agree with their doctrinal analysis, it is unlikely to have any effect on the data. This is because cases filed prior to *Sutton* and pending when it came down would have had to litigate whether they were subject to the decision. Post-*Sutton*, the EEOC can be expected to have become much more selective in the ADA cases it brought. I can analyze the EEOC's charge resolution

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75. *See generally* Adolf Buse, *The Likelihood Ratio, Wald, and Lagrange Multiplier Tests: An Expository Note*, 36 THE AM. STATISTICIAN 153, 153 (1982).

76. *Infra* Tables A1, A2.

77. Rutherglen, *supra* note 42, at 491, 496.

78. Parker, *supra* note 33, at 930–31.

79. *See* Berger, Finkelstein & Cheung, *supra* note 38, at 60.

statistics to test this expectation. Historical data from the same time period as the pairwise analyses discussed above[80] show that the EEOC saw a drastic reduction in disability discrimination charges after *Sutton* was decided.[81] Further confirming the role that *Sutton* played in dispute generation over disability issues is that, after Congress amended the ADA to overrule *Sutton* in 2009,[82] the share of EEOC charges alleging disability discrimination almost immediately resumed their pre-*Sutton* share of the administrative docket.

In considering administrative enforcement actions, the share of the EEOC litigation docket that presented disability claims post-*Sutton* did not significantly change. Pre-*Sutton*, about 15% of cases alleged disability claims; post-*Sutton*, about 10% of EEOC cases brought disability claims. A chi-square test measures whether the difference in share between these two samples can be attributed to the effect of *Sutton*, but does not provide us with confidence that the case is the causal mechanism. The full tables are provided in the Appendix.[83]

Ultimately, it is clear that *Sutton* impacted the set of ADA cases decided between 1999 and 2009; however, the evidence suggests that the effect occurs earlier in the dispute generation process than the point where the agency is selecting cases to litigate. Instead, it appears that *Sutton*'s effect was seen in the "naming, blaming, and claiming" process that Felstiner, Abel, and Sarat identified.[84] This analysis cannot distinguish between litigants who no longer see themselves as legally injured ("naming" in this parlance) and those who no longer see their injuries as redressable ("claiming").[85] It seems implausible that *Sutton*, which decided *who* was entitled to *redress* for disability discrimination,

80. *See supra* note 67 and accompanying text.

81. *See* discussion *infra* Section III.A.1.

82. ADA Amendments Act of 2008, Pub. L. No. 110-325, 122 Stat. 3553 (2008) ("[T]he holdings of the Supreme Court in [*Sutton*] and its companion cases have narrowed the broad scope of protection intended to be afforded by the ADA, thus eliminating protection for many individuals whom Congress intended to protect.").

83. *Infra* Table A3.

84. William L. F. Felstiner, Richard L. Abel & Austin Sarat, *The Emergence and Transformation of Disputes: Naming, Blaming, Claiming . . .*, 15 LAW & SOC'Y REV. 631, 633 (1980).

85. *See id.* at 635–36.

would have affected disputants' ability to identify the guilty party ("blaming").[86]

Taken together, the doctrinal analysis suggests that there is a clear hierarchy of the types of claims that are most likely to succeed. The picture is not pretty for those who value an equitable approach to American law. While racialized plaintiffs experience deep difficulties, and disability and gender claimants have their own hurdles, White men presenting age claims have the easiest time.[87] In short, the most privileged band of protected workers have the best protections—while more marginalized groups are once again left out in the cold.

B. CHOOSING THE SAMPLING FRAME: ENSURING REPRESENTATIVENESS IN THE DATA

Litigants always have the right to settle their case rather than proceed to a decision.[88] Because litigants would prefer settling to losing, they attempt to predict the likelihood of winning based on the available information.[89] If they successfully convince the other side to settle for the expected value of the case, discounted by the expected probability of a plaintiff victory, then the case settles.[90] Because of the uncertainty around both point estimates (the value of the case and the probability of a plaintiff victory), settlement values cover a range of potential outcomes referred to as a "zone of agreement" or a "contract zone."[91] Rational litigants should therefore be expected to litigate two classes of cases to judgment: first, those where the parties have profoundly divergent predictions of either the value of the case or the probability of a plaintiff victory; and, second, those where the parties are unable to predict a victor, i.e., where the probability of a plaintiff victory is 50%.

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86. *See id.* at 635, 638.

87. Whether this is still true post-*Gross* is an open question. *See Gross*, 557 U.S. at 177 ("[P]laintiff retains the burden of persuasion to establish that age was the 'but-for' cause of the employer's adverse action.").

88. *See* Carrie Menkel-Meadow, *Whose Dispute Is It Anyway?: A Philosophical and Democratic Defense of Settlement (In Some Cases)*, 83 GEO. L.J. 2663, 2263–65 (1995) ("[S]ettlement has become the 'norm' for our system.").

89. Klerman & Lee, *supra* note 20, at 211–12.

90. *See generally id.* at 223.

91. *See* Parker, *supra* note 33, at 913, 925 (explaining that asymmetry of information creates a "gray zone"). *See generally* Amy Farmer & Paul Pecorino, *Discovery and Disclosure with Asymmetric Information and Endogenous Expenditure at Trial,* 42 J. LEGAL STUD. 223, 235 (2013).

The classic theoretical approach to selection effects in trial results suggests that no inferences about the effects of the law on legal disputes can be drawn from plaintiff win rates because the parties will choose to settle all but the closest of cases.[92] These effects can lead both practitioners and scholars to think that the law is balanced when the legal regime is actually strongly favoring one side or the other. This means that if the observed plaintiff win rate is 50%, the bench and bar will assume that this holds for all cases, when in fact the *population* win rate among all disputes may be significantly different from 50% but is unobservable because all cases that do not have a 50% likelihood of plaintiff victory settle.[93]

The question that arises thus becomes, to what extent do these selection effects inhibit scholars' ability to make inferences from trial result data? When making these evaluations, the literature begins with the assumption that the *population* consists of all disputes that get litigated. The *sample* consists of that subset of cases which proceed to judgment for one side or the other. Selection effects arise when the process of determining which cases are in the sample is systematically biased.[94] This systematic bias is due to the choices of the parties to settle cases where one side or the other has a particularly weak case.

The scholarly literature offers mixed support for the Priest–Klein hypothesis that litigant choices systematically[95] select cases to litigate by settling cases where the parties share similar expectations about the outcome. Some scholars have found partial selection, while some find complete selection.[96] Partial selection means that the likelihood of plaintiff victory has an effect on whether the case is tried, but that other issues also drive

92. *See* George L. Priest & Benjamin Klein, *The Selection of Disputes for Litigation*, 13 J. LEGAL STUD. 1, 10, 13 (1984).

93. *Id.* at 45–46.

94. Peter Siegelman & John J. Donohue, III, *The Selection of Employment Discrimination Disputes for Litigation: Using Business Cycle Effects to Test the Priest–Klein Hypothesis*, 24 J. LEGAL STUD. 427, 428–29, 442 (1995).

95. A note on nomenclature: "systematically" here should not be read as implying a litigant conspiracy or even a conscious choice. Rather, it refers to the fact that the Priest–Klein hypothesis assumes that litigants are rational and seek to maximize their utility in litigation, and that similar incentives will operate on all rational litigants similarly.

96. Siegelman & Donohue, *supra* note 94, at 431 (partial selection); Oscar Ashenfelter, Ted Eisenberg & Steven J. Schwab, *Politics and the Judiciary: The Influence of Judicial Background on Case Outcomes*, 24 J. LEGAL STUD. 257, 261 (1995) (complete selection).

litigation.[97] Complete selection predicts a 50% win rate for plaintiffs in cases that do not settle.[98] Others find that external characteristics influence trial results without regard to the parties' choices.[99]

Priest and Klein offer the most important early theoretical approach to the effect that settlement has on the power to make inferences from litigation outcomes.[100] They conclude, relying strictly on a formal-theoretic model, that "where the gains or losses from litigation are equal to the parties, the individual maximizing decisions of the parties will create a strong bias toward a rate of success for plaintiffs at trial or appellants at appeal of 50 percent regardless of the substantive standard of law."[101] By "relax[ing] the assumption" that payouts are equal, Priest and Klein's model predicts that variation from the 50% plaintiff-victory rule will be driven almost entirely by the differences in expected payouts.[102] They test the model using their own data from Cook County, Illinois jury verdicts and find some support for the model's predictions; after reviewing prior scholarship, they conclude that most of it supports the model.[103]

Klerman and Lee, thirty years later, note that "Priest and Klein's article is startling in suggesting that selection bias is so strong that a change in the legal standard would result in no observable change in the plaintiff trial win rate."[104] Ultimately, they argue that "under all standard settlement models and under a wide range of reasonable conditions, one may be able to make valid inferences from the percentage of plaintiff trial victories."[105] While they agree that "[t]he characteristics of litigated cases do deviate significantly from those of settled cases[,]" they conclude

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97. Siegelman & Donohue, *supra* note 94, at 433.

98. *Id.* at 428.

99. *See* EPSTEIN, LANDES & POSNER, *supra* note 51, at 220–23; Felstiner, Abel & Sarat, *supra* note 84, at 642; Ted Eisenberg & Steven Johnson, *The Effects of Intent: Do We Know How Legal Standards Work?*, 76 CORNELL L. REV. 1151, 1151 (1991); Carol T. Kulik, Elissa L. Perry & Molly B. Pepper, *Here Comes the Judge: The Influence of Judge Personal Characteristics on Federal Sexual Harassment Cases Outcomes*, 27 LAW & HUM. BEHAV. 69, 72 (2003).

100. Priest & Klein, *supra* note 92, at 4.

101. *Id.* at 5 (footnote omitted).

102. *Id.* at 24–30.

103. *Id.* at 31–44.

104. Klerman & Lee, *supra* note 20, at 210.

105. *Id.*

that, all else being equal, changes in the legal standard should change plaintiff win rates.[106]

Historically, there have been two competing models for explaining the difference between the true plaintiff win rate and the observed plaintiff win rate (and the scope conditions of selection effects): divergent-expectations models and asymmetric-information models. Divergent-expectations models assume that both sides have equal information but draw different conclusions about their likelihood of victory.[107] Asymmetric-information models assume that one side has private information not shared with the other party.[108] However, Jonah Gelbach argues that both models are appropriately seen as limited applications of a broader "reduced-form" model of litigation that applies to all disputes and does not demonstrate systematic selection effects.[109]

This win-rate finding, Klerman and Lee argue, holds regardless of whether plaintiffs make offers in hopes of settling with high-liability defendants, or defendants make offers in hopes of signaling their information regarding their likelihood of liability (referred to as asymmetric-information models).[110] They also argue that Priest and Klein's finding only holds if the parties accurately measure the defendant's level of fault.[111] The only exception is changing damages standards.[112] The Priest–Klein model dictates that an increase in damages moves plaintiff win rates toward the population rate; while under the asymmetric-information models, an increase in damages increases plaintiff win rates.[113]

The degree of selection effects on observed litigation outcomes is significant if notable differences between the set of cases which settle and the set of cases decided at trial are found. If Klerman

106. *Id.* at 214.

107. William M. Landes, *An Economic Analysis of the Courts*, 14 J.L. & ECON. 61, 63–65 (1971); Joel Waldfogel, *Reconciling Asymmetric Information and Divergent Expectations Theories of* Litigation, 41 J.L. & ECON. 451, 457 (1998).

108. I.P.L. P'ng, *Strategic Behavior in Suit, Settlement, and Trial*, 14 BELL J. ECON. 539, 542 (1983); Lucian Arye Bebchuk, *Litigation and Settlement Under Imperfect Information*, 15 RAND J. ECON. 404, 404, 409, 414 (1984).

109. Jonah B. Gelbach, *Maybe There Is No Bias in the Selection of Disputes for Litigation: Comment*, 174 J. INST. & THEORETICAL ECON. 171, 174–76 (2018).

110. Klerman & Lee, *supra* note 20, at 222–23.

111. *Id.* at 228–29.

112. *Id.* at 210.

113. *Id.* at 210, 235.

and Lee are correct, then it becomes vital to determine two things: first, whether litigants use asymmetric-information or divergent-expectations behaviors in settlement negotiations; and, second, if they use divergent-expectations models, whether their errors in predicting outcomes are large enough to support Klerman and Lee's assertions.[114] The evidence from the literature, discussed below, suggests that the answers to these questions are, respectively, "divergent-expectations" and "possibly."

To the first question regarding litigant behavior, Waldfogel tested the predictive power of both models, and found divergent-expectations models more consistent with the evidence.[115] He argued that divergent-expectations models predict that plaintiff win rates converge on 50% as the percentage of cases that go to trial decline, while asymmetric-information models predict that as trial rates decline, plaintiff win rates converge on either zero or one.[116] Examining 65,000 federal district court cases in New York filed between 1979 and 1986, Waldfogel found that raising trial rates in tort and civil rights cases by a standard deviation reduced plaintiff win rates by half—and if he controlled for the amount in controversy, the reduction was three-quarters.[117] His research design predicted a positive relationship between trial rates and plaintiff win rates if asymmetric information applied, and a negative relationship if divergent information applied.[118] This is consistent with Klerman and Lee's argument that both approaches lead to partial selection, such that inferences may be made from trial results.[119]

Divergent-expectations models are not only supported by the evidence, but by the legal regime. In the United States, and particularly in federal courts, the judiciary has methodically moved away from "trial by ambush," meaning the introduction of surprise evidence at trial, since the adoption of the Federal Rules of Civil Procedure in 1938.[120] In cases where one side attempts, knowingly or unknowingly, to ambush the other, the result is a

114. *Id.* at 229–29.
115. Waldfogel, *supra* note 107, at 474.
116. *Id.* at 470–72.
117. *Id.* at 468 tbl.4.
118. *Id.* at 459, 474.
119. Klerman & Lee, *supra* note 20, at 237.
120. *See* Smith v. Ford Motor Co., 626 F.2d 784, 790, 792 (10th Cir. 1980).

mistrial and a substantial sanction against the ambushing party.[121]

In short, while asymmetric information models may have stronger game-theoretic roots and broader scholarly acceptance, they do not represent the litigation process as it exists in the United States.[122] Thus, empirical work on trial results in the American context should assume divergent expectations. This is consistent with most efforts to test these models quantitatively.[123] Under these models, the greater the parties' differences in predicted outcomes, the higher the proportion of cases that are tried, and the more plaintiff win rates will diverge from the 50% predicted by Priest and Klein.[124] This matters because if the parties have large prediction errors, then Klerman and Lee suggest that the outcomes from tried cases will approach the population outcome; but if the parties accurately measure the outcome probability, plaintiff victories will converge on 50%.[125]

With regard to the second question, whether errors in outcome estimation are large enough to justify Klerman and Lee's optimism for inferences from trial outcomes, Boyd and Hoffman suggest that it might be.[126] They argue that non-discovery motion practice serves as an information exchange mechanism.[127] They suggest that if this is true, then cases in which motion practice occurs will be more likely to settle as the variance in estimation of outcomes narrows.[128] Using a random sample of federal litigation, they find such an effect.[129] This suggests that the litigation process may

121. *See e.g.*, Putman v. Mgmt. & Training Corp., A-08-CA-685-SS, 2009 WL 10669155, at *3 (W.D. Tex. Aug. 18, 2009) (mistrial); Furr v. Ford Motor Co., CIV 05-313 MCA/RLP, 2005 WL 8163470, at *2 (D.N.M. Dec. 9, 2005) (sanctions).

122. *See generally* Klerman & Lee, *supra* note 20, at 214; Waldfogel, *supra* note 107, at 474–75.

123. *E.g.*, Henry S. Farber & Michelle J. White, *Medical Malpractice: An Empirical Examination of the Litigation Process*, 22 RAND J. ECON. 199, 210 (1991); Kuo-Chang Huang, *Does Discovery Promote Settlement? An Empirical Answer*, 6 J. EMPIRICAL LEGAL STUD. 241, 251 (2009) (quantitative assessment of litigation in Taiwan). *See generally* Steven Shavell, *The Sharing of Information Prior to Settlement or Litigation*, 20 RAND J. ECON. 183, 189 (1989).

124. Klerman & Lee, *supra* note 20, at 210.

125. *Id.* at 212, 228–29.

126. *See* Christina L. Boyd & David A. Hoffman, *Litigating Toward Settlement*, 29 J.L. ECON. & ORG. 898, 902 (2013).

127. *Id.* at 907.

128. *Id.* at 907–09.

129. *Id.* at 908, 914–15.

facilitate settlement by reducing estimation error in outcome prediction, and that if settlements are included in the dataset, then inferences about the law from a sample of litigated cases could be made.

In addition, Bock identifies cases where after settlement is reached, the parties ask the district court to vacate a previous ruling as part of the settlement.[130] In those cases, vacating a ruling usually eliminates a finding relating to an issue that would recur in other cases and present collateral estoppel issues, meaning that the parties would be unable to relitigate the issue.[131] This is consistent with Boyd and Hoffman's conclusion that motion practice gives the parties a lens on how the court views the case and allows them to reach agreement on the settlement value of the case on that basis.[132] Bock's findings support Boyd and Hoffman because the court in his cases drives settlement when it announces its resolution of an issue; but because one or both parties would want the opportunity to press their arguments on that issue in a later case, they ask the court to vacate the ruling as a condition of settlement.[133] Bock's argument is suggestive rather than conclusive because his sample is quite small (N=79) and the sampling frame is unrepresentative (patent cases where post-settlement motion practice sought to vacate a ruling adverse to a particular side).[134] But this finding provides some confirmation that Boyd and Hoffman's work should provide consistent expectations for the relationship between motion practice and trial.

Boyd and Hoffman and Bock also represent examples of the conversation between judges and litigants being embedded in trial courts research, but not being explicated. Both pieces focus on how the parties update their expectations using information from the court, but neither models the way that the substantive information from the court changes the outcome. Instead, the additional information is treated as either present or absent.

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130. Jeremy Bock, *An Empirical Study of Certain Settlement-Related Motions for Vacatur in Patent Cases*, 88 IND. L.J. 919, 920 (2013).

131. *Id.* at 922.

132. Boyd & Hoffman, *supra* note 126, at 908, 914–15.

133. I note for the sake of completeness that Federal Rule of Civil Procedure 7.1(a)(2) was amended December 1, 2022 to clarify that a court order is required for this type of vacatur to take place; the parties cannot stipulate to it as a condition of settlement anymore.

134. Bock, *supra* note 130, at 934–38.

These analyses, both theoretical and empirical, drive home one key issue: litigant choices are as important in determining case outcomes as judicial decision-making. If the parties choose to take an issue away from the court, whether by stipulating to it or simply reaching an agreed resolution, the court is unable to engage in any decision on that issue.[135] The largest barrier to overcoming the Priest–Klein analysis is the lack of data on settlements. However, this barrier does not exist if we have information on the settlements reached. Thus, government litigation, where one of the parties is prevented from reaching a confidential settlement, offers a subset of cases that allow us to make inferences about the broader litigation population.

C. JURISPRUDENTIAL REGIMES THEORY

My goal in this analysis is to test whether changes in the legal regime affect litigation outcomes. Legal regimes (or jurisprudential regimes; the terms are interchangeable)[136] are broad bodies of doctrine that set the rules and define the scope of many cases over a long period of time. The basic argument of jurisprudential regimes theory is that when the Supreme Court makes a major change in legal doctrine, the change can be traced out in later cases.[137] Jurisprudential regimes either establish case factors which matter to the courts' decision-making, change the way that courts evaluate key case factors, or both.[138] For example, a pre-existing regime may dictate the weight a court should give certain case factors.[139] The justices then shift the regime in the relevant area of law, creating a change in the way that courts should weigh the same case factors and, in turn, creating a new

135. *See generally* Wofford, *supra* note 21, at 37–39.

136. Kritzer writes in criticism of using the terms interchangeably: "In our very first draft of what was to become the APSR paper, we used the label 'legal regime,' but discarded it because 'legal regime' was a widely used term/concept. [I]f you do a [G]oogle scholar search of 'legal regime' you get over 200,000 hits; if you do a similar search for 'jurisprudential regime,' you get only 246 hits and only [sixteen] of those were from prior to 2002 when our article was published." E-mail from Herbert L. Kritzer (May 1, 2023 11:34 CST) (on file with author). I recognize that the theory refers specifically to "jurisprudential regimes" and respect the right of Kritzer and Richards to choose nomenclature that distinguishes their work on doctrinal shifts from work that focuses on discrete bodies of law more generally. However, as the distinction presents nothing normative or empirical to draw on, I use the terms synonymously.

137. *See* Kritzer & Richards, *supra* note 13, at 827–29. *See also* Luse, McGovern, Martinek & Benesh, *supra* note 15, at 75–77.

138. Kritzer & Richards, *supra* note 13, at 827–32.

139. Richards, Smith & Kritzer, *supra* note 14, at 448.

regime.[140] The evidence for jurisprudential regimes theory is mixed, with its proponents seeing it in several areas of the law and its critics arguing that its findings are simply a statistical artifact.[141] Finally, scholars have asked whether jurisprudential regimes theory's findings are simply a case of mistaking evolutionary change for "punctuated equilibri[a]", and found, again, mixed evidence.[142]

1. FOUNDATIONS AND EVIDENCE

Although Richards and Kritzer began developing their theory of jurisprudential regimes in the context of Establishment Clause cases,[143] the most developed version of their theory considered the evidence supporting it from administrative-law cases.[144] In a piece entitled *Does Chevron Matter?*, Richards, Kritzer, and Joseph Smith consider whether the Supreme Court's decision in *Chevron v. Natural Resources Defense Council*[145] led to a significant change in *how* the Supreme Court decided administrative-law cases.[146] In *Chevron,* the Court announced for the first time that administrative agencies were entitled to deference in their statutory interpretations if the statute was ambiguous and if the agency's interpretation was reasonable.[147]

Richards, Smith, and Kritzer argue that this doctrinal shift would change the way that the Court analyzes administrative-law cases involving statutory interpretation going forward.[148] Specifically, they argue that "*Chevron* conditions the influence of the jurisprudential regime variables."[149] They test whether *Chevron* changed the way the Court analyzes agency statutory interpretation by seeing whether the Court is less likely to defer as statute length increases, where statute length is used as a proxy for ambiguity, under the theory that longer statutes are more likely to speak to the question under consideration and, hence,

140. *Id.*
141. *Compare id.* at 464–65, *with* Lax & Rader, *supra* note 16, at 282.
142. *See generally* Bartels & O'Geen, *supra* note 16, at 880–81.
143. Kritzer & Richards, *supra* note 13, at 828.
144. *See generally* Richards, Smith & Kritzer, *supra* note 14, at 444.
145. Chevron v. Nat. Res. Def. Council, 467 U.S. 837 (1984).
146. Richards, Smith & Kritzer, *supra* note 14, at 464.
147. *Chevron*, 467 U.S. at 842–43.
148. Richards, Smith & Kritzer, *supra* note 14, at 464.
149. *Id.* at 451.

contain less ambiguity.[150] They also test whether the Court is less likely to defer in rulemaking cases: those that involve regulations written through rulemaking processes as opposed to adjudicatory processes, under the theory that rulemaking cases present higher stakes and that the Court is therefore more likely to want to impose its own views on the agency.[151]

Their findings support the existence of a jurisprudential regime shift in *Chevron*. "[B]efore *Chevron*, the justices were more likely to defer when there was a longer statute, but after *Chevron*, the justices were less likely to defer when there was a longer statute."[152] They also find "that the increase in deference post-*Chevron* was located primarily in non-rulemaking cases, rather than the rulemaking cases that have higher stakes, affect a broader range of policies, and affect more potential litigants."[153]

Kevin Scott argues that jurisprudential regimes can only be accurately measured in the behavior of justices who dissented from the case establishing the new regime.[154] After all, "[i]t is quite easy for the majority justices to adapt their behavior to comply with the precedent they established."[155] If justices who dissented from the decision that established the regime choose to follow the regime in subsequent cases, then this is important evidence that precedent influences justices rather than policy preferences.[156] Scott replicates Richards and Kritzer's work on search and seizures while specifically singling out dissenting justices for special analysis.[157] He finds that while majority justices immediately comply with the new legal factors the regime imposes, dissenting justices have a delay in their adoption of the regime.[158]

2. METHODOLOGICAL CRITICISMS OF JURISPRUDENTIAL

150. *Id.* at 452–53.

151. *Id.*

152. *Id.* at 464.

153. Richards, Smith & Kritzer, *supra* note 14, at 464 (citation omitted).

154. Kevin M. Scott, *Reconsidering the Impact of Jurisprudential Regimes*, 87 SOC. SCI. Q. 380, 382–84 (2006).

155. *Id.* at 382.

156. *Id.* at 382–83.

157. *Id.* at 383–84.

158. *Id.* at 390–92.

REGIMES THEORY

However, the process of uncovering the evidence of jurisprudential regimes theory in every instance relied on several sophisticated statistical techniques. Specifically, Richards, Smith, and Kritzer (and Richards and Kritzer before them, and Scott after) rely on a Chow test, which measures the impact of a given variable between a treatment and control by analyzing subsets of the data and comparing the results between them.[159] Lax and Rader argue that this test assumes:

> [T]here is no relationship among votes within the same case . . . or among votes cast within the same Court term . . . or among votes cast by the same justice . . . . If this assumption is not met, then we are acting as though we have a much larger number of independent observations than we indeed have.[160]

When Lax and Rader divide their data into "even" and "odd" years, rather than "before" and "after" years, they continue to find evidence of jurisprudential regime change.[161] Thus, "the standard jurisprudential regimes test would conclude that Supreme Court justices use a different legal regime in odd years than they do in even years."[162] Ultimately, Lax and Rader, in performing the analysis that they believe is appropriate, conclude that almost any analysis would have found evidence of regime change in the content-neutrality regime.[163] They further conclude that the evidence of regime change in Establishment Clause cases was caused by personnel shifts.[164] Finally, they argue that there is no evidence of the regime change being anything other than random chance in the criminal-procedure context.[165] While they do not analyze Richards, Smith, and Kritzer, they are (reasonably) skeptical that the evidence would lead to different results.[166]

Richards and Kritzer reply, arguing that criticizing their argument solely on statistical grounds misconceives the strength

159. Richards & Kritzer, *supra* note 12, at 315, 319.

160. Lax & Rader, *supra* note 16, at 279 (footnote omitted).

161. *Id.* at 277.

162. *Id.* at 281.

163. *Id.* at 282–83.

164. *Id.* at 281–83. *See also* Richards & Kritzer, *supra* note 12, at 314; Kritzer & Richards, *supra* note 13, at 837.

165. Lax & Rader, *supra* note 16, at 282, 284.

166. *Id.* at 278 n.13.

of their argument.[167] Rather, they argue that jurisprudential regimes theory requires that statistical results be compared against theoretical expectations.[168] If the statistics for significant results do not agree with the predictions of theory derived from understanding the shifts in legal doctrine, then that is evidence that the case being analyzed does not present a regime shift. While that would certainly weaken the argument in favor of jurisprudential regimes theory, it does not destroy it because there is always the possibility that another case represents the correct regime-forming doctrine.

Kritzer and Richards also dispute Lax and Rader's characterization of jurisprudential regimes theory as thinking of justices assigning cases to categories and deciding them solely on that basis: "[w]e would certainly recoil at any conception of Supreme Court decision making that reduces the justices to robots sorting cases into bins."[169] Instead, they note that the richness of their theory includes its capacity to "[use] interpretive methods to trace the origins of the [legal] regimes and look[] to legal scholars for confirmation of [their] hypotheses about the parameters of the regimes."[170] Ultimately, they conclude, "there is no substitute for reading the cases."[171]

3. SUBSTANTIVE CRITICISMS OF JURISPRUDENTIAL REGIMES THEORY

Pang, et al. take a different tack in criticizing Richards and Kritzer's approach. They argue that "from a legal standpoint, it is hard to believe that opinions are inconsequential."[172] But, they also note that in analyzing Supreme Court decision-making, "there are *two* dependent variables of interest: the judgment and the opinion."[173] Instead of focusing on just the legal factors governing the decision, as legal scholars would, or just on the judgment itself, as attitudinalist social scientists would, they see these choices as "interdependent."[174]

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167. *See generally* Kritzer & Richards, *supra* note 17, at 285.

168. *Id.* at 285, 288.

169. *Id.* at 288.

170. *Id.*

171. *Id.*

172. Xun Pang, Barry Friedman, Andrew Martin & Kevin Quinn, *Endogenous Jurisprudential Regimes*, 20 POL. ANALYSIS 417, 418 (2012) (citation omitted).

173. *Id.*

174. *Id.*

Their skepticism boils down to being uncertain that jurisprudential regime theorists have adequately identified the relevant breaks in jurisprudential regimes.[175] They question whether these breaks are sufficiently sharp to accommodate the empirical tests Richards and Kritzer use.[176] After all, "[s]ince many legal concepts are fuzzy and because several decisions may be necessary to sort out the interstices in existing precedent, transitions may not be quick."[177] In short, Pang and her co-authors are searching to see if jurisprudential regimes can be better identified through a more evolutionary-change approach. While they agree with jurisprudential regime theorists that *Lemon v. Kurtzman* involves a change in Establishment Clause law and that *Chevron* may represent a change in administrative law, they are more skeptical regarding Richards and Kritzer's findings on free speech and search-and-seizure cases.[178] In free speech cases, they found "five regimes over the fifty-year study[,] and in search-and-seizure cases, they found the change point far earlier than hypothesized."[179]

Pang et al.'s empirical strategy relies on letting "the *data* [] speak about the existence, or not, and location of change-points."[180] This approach seems to contradict the authors' insistence that law is interdependent with the case-specific and jurist-specific factors that color decision-making in a particular case.[181] After all, if reading a decision creates an expectation that cases in this area will be decided differently after, then that expectation should color our modeling. Being completely naïve about the role that law plays in judicial decision-making, which is what Pang et al.'s model does, implies that the way that lawyers and litigants evaluate the effects of judicial decisions is completely at odds with the actual effects of those decisions.

Bartels and O'Geen question whether Richards and Kritzer have accurately identified the break point for a free-speech jurisprudential regime.[182] In particular, they note that "the key is

175. *Id.* at 420.

176. *Id.* at 419–20.

177. Pang, Friedman, Martin & Quinn, *supra* note 172, at 420.

178. *Id.* at 427–28, 431–33.

179. *Id.* at 433.

180. *Id.* at 420 (emphasis in original).

181. *Id.* at 418.

182. Bartels & O'Geen, *supra* note 16, at 885–86 (discussing pre-*Grayned v. City of Rockford* cases that apparently follow the *Grayned* content-based standard of review).

to uncover patterns of how the Court treats different legal categories over time . . . ."[183] Thus, just because an alleged shift in the jurisprudential regime does not produce changes in case outcomes, Bartels and O'Geen posit that it "do[es] not necessarily foreclose the influence of legal doctrine."[184] Instead, they argue that legal change can exhibit patterns of stability or of slow, evolutionary change requiring a broader lens than Richards and Kritzer's data capture.[185]

They then proceed to reanalyze Richards and Kritzer's free-speech data, expanding the temporal frame to encompass almost all of the post-War decisions.[186] Bartels and O'Geen make seven key findings about the role of legal change in free-speech doctrines, two of which support the sudden-shift paradigm of jurisprudential regimes theory, while the other five tell a mixed story of evolutionary change, stability, and drastic change inconsistent with the expectations of jurisprudential regimes theory.[187] From this, they conclude that there is a significant difference between claiming that the *Court* is constrained by law, and that the *justices* are.[188]

4. APPLYING JURISPRUDENTIAL REGIMES THEORY BEYOND THE SUPREME COURT

However, all of this work has focused on the Supreme Court's effect on itself. As Canon and Johnson note, the Court is far from the only audience for its decisions.[189] They identify four additional populations that pay attention to the Court's decisions and adjust their behavior. The "interpreting population" consists of lower courts who are tasked with resolving ambiguities in the Court's

183. *Id.* at 882.

184. *Id.* at 892.

185. *Id.* at 883–85.

186. *Id.* at 886 (Bartels and O'Geen analyze free speech decisions from 1946–2004, while Richards and Kritzer studied decisions from 1953–1997).

187. *See* Bartels & O'Geen, *supra* note 16, at 892 tbl.1 (table summarizing results).

188. *See id.* at 892 (emphasis in original) ("This aspect of our work stresses the importance of differentiating *legal change* from the *impact of law on behavior* . . . . Of course, in order to analyze this latter aspect, a justice-level analysis is called for, but it must recognize the dynamic nature underlying the Court's decisions in order to truly understand the nature and degree to which justices are constrained by legal doctrine.").

189. *See generally* BRADLEY CANON & CHARLES JOHNSON, JUDICIAL POLICIES: IMPLEMENTATION AND IMPACT (CQ Press 1999).

decisions.[190] The "implementing population" focuses on those actors who "apply the system's rules to persons subject to their authority"—police, bureaucracies, and legislatures.[191] "Consumer populations" are litigants who use the Court's decision to evaluate their ability to bring claims under the new legal regime.[192] Finally, "secondary populations" are a "residual group," frequently seen as groups and individuals who see the Court as an opportunity to pursue agendas unrelated to law.[193] Because of this, work that expands the scope of jurisprudential regimes theory can sometimes reach beyond the Court itself to consider the way that other audiences receive changes to the legal regime.

a. Jurisprudential Regimes Theory in Lower Courts

Therefore, "[g]iven the Supreme Court's lack of strong enforcement powers and the inability to choose its subordinates in the judicial hierarchy, we might expect there to be high levels of noncompliance with Court decisions."[194] In testing whether the *Lemon* test changed the behavior of the courts of appeals, an interpreting population, Jennifer Luse and her co-authors argue that judges are, in Shapiro's words, "both political and legal entities and that they ought to be understood as such."[195] Because lower courts are at least expected to nod toward compliance with the decisions of supervising courts, jurisprudential regimes are *more* useful for analyzing lower court decision-making than Supreme Court decision-making.[196] However, the authors recognize that there are limits to judicial decision-making; they choose to study the role of the *Lemon* jurisprudential regime because its establishment "motivated litigants to act on their Establishment Clause claims, prompting litigation that may not have been initiated without the Supreme Court's foray into this area."[197] In other words, the Establishment Clause jurisprudential regime is valuable for study because the regime *spurred* litigation, allowing for judges to decide Establishment Clause claims post-

190. *Id.* at 29–31.

191. *Id.* at 19.

192. *Id.* at 92–93.

193. *Id.* at 115.

194. Luse, McGovern, Martinek & Benesh, *supra* note 15, at 78.

195. *Id.* at 79 (citing MARTIN M. SHAPIRO, LAW AND POLITICS IN THE SUPREME COURT: NEW APPROACHES TO POLITICAL JURISPRUDENCE (1964)).

196. *Id.* at 81.

197. *Id.*

Lemon. A shift in jurisprudential regimes that *stifled* litigation would present different challenges.[198]

They ultimately find that there is significant evidence that the circuit courts of appeals were making use of the *Lemon* regime to decide Establishment Clause cases.[199] In particular, they find that the *Lemon* case factors outperform the ideology of the circuit court panel in predicting whether the court will accommodate allegedly religious government practices.[200] They further note that there is no evidence that circuit court compliance comes from a fear of reversal; the ideology of the Supreme Court does not predict the ideological direction of the court of appeals' decisions.[201] From these findings, they conclude that "[lower court] compliance is driven in a meaningful way by legal factors . . . ."[202]

b. Jurisprudential Regimes Theory in Non-Judicial Audiences

However, as noted above, if jurisprudential regimes empower litigants to *avoid* judicial decision-making, it can be more difficult to discern the effects of jurisprudential regimes. Instead of the evidence being seen in judicial decisions, it arises in the way that *other* actors behave. Studying an implementing population, Boldt and Gizzi ask whether police behavior changed after *Arizona v. Gant*.[203] In *Gant*, the Court ruled that police searches of vehicles incident to arrest had to be supported by probable cause to find evidence of an ongoing crime, or in the event that the arrestee could access the vehicle's passenger compartment.[204] Prior to *Gant,* police were authorized to search a vehicle's passenger compartment any time they arrested one of the vehicle's occupants.[205] Boldt and Gizzi ask if this change in the law changed the patterns of vehicle searches incident to arrest.

Boldt and Gizzi describe *Gant* as "sen[ding] shockwaves throughout the law enforcement community."[206] However, they

198. *See* discussion *infra* Section I.C.4.b.
199. Luse, McGovern, Martinek & Benesh, *supra* note 15, at 93–95.
200. *Id.* at 95.
201. *Id.* at 95–96 tbl.4.
202. *Id.* at 97.
203. *See generally* Boldt & Gizzi, *supra* note 19, at 355–56.
204. *Gant*, 556 U.S. at 342–43.
205. *See* New York v. Belton, 453 U.S. 454, 460 (1981).
206. Boldt & Gizzi, *supra* note 19, at 360.

note that many training publications that emphasized the new difficulty of conducting a vehicle search incident to arrest under *Gant* also emphasized the availability of other exceptions to allow for warrantless searches of vehicles.[207] One author argued, "[i]f an officer cannot justify a search of a vehicle incident to arrest under *Gant*, or is uncertain whether a [search] is warranted, other established exceptions to the search warrant requirement remain available to safeguard evidence and protect the safety of officers."[208] In addition, prior research had indicated that police would simply alter their claimed rationales for action; when one door was closed to them, law enforcement would use a different one.[209] From this, Boldt and Gizzi hypothesized that while searches incident to arrest would drop after *Gant*, other rationales for searches would become more common as police simply shifted what they claimed to justify the search.[210]

Boldt and Gizzi find substantial evidence that *Gant* played an important role in shaping the ways that police search vehicles. Vehicle searches incident to arrest decreased by 41% after *Gant* in Illinois, and by 60% in North Carolina.[211] While Boldt and Gizzi claim to find evidence that police use alternative rationales to justify vehicle searches post-*Gant*, their evidence seems speculative.[212] Ultimately, they conclude that "when the [U.S.] Supreme Court changes the law surrounding search and seizure, the police are paying attention and change their behavior to comply with the shift in policy."[213] Noting the extremely quick turnaround for the change in police behavior (almost immediately after the *Gant* decision was announced, the numbers of vehicle searches incident to arrest dropped precipitously), they conclude that "[t]his underscores the interconnectedness of the institutions of courts and law enforcement by which alterations to long-

207. *Id.* at 360–61.

208. Jennifer G. Solari, *The United States Supreme Court's Ruling in* Arizona v. Gant*: Implications for Law Enforcement Officers*, 2009 FED. L. ENF'T INFORMER 4, 8 (May 2009), https://www.fletc.gov/sites/default/files/imported_files/training/programs/legal-division/downloads-articles-and-faqs/research-by-subject/4th-amendment/ArizonaVsGant.pdf.

209. Jeffry Hoffman & Richard Seltzer, *Effect of* Mapp v. Ohio *on Police Search-and-Seizure Practices in Narcotics Cases*, 4 COLUM. J.L. & SOC. PROBS. 87, 95 (1968).

210. Boldt & Gizzi, *supra* note 19, at 370–74.

211. *Id.* at 367–69 figs.1 & 2.

212. *See id.* at 373–74 ("[I]t may incorporate inventory searches, searches of persons under correctional supervision, or searches based on warrants.").

213. *Id.* at 374–75.

standing rules can inspire change before sanctions may even be applied."[214]

My analysis focuses on litigants, identified by Canon and Johnson as a secondary population. The entire edifice of jurisprudential regimes theory rests on a stool with a missing leg. Because litigants have the power to take issues away from judges through settlement, the mixed evidence of jurisprudential regimes theory could be caused instead by litigants simply choosing not to allow decisions under an unfriendly legal regime. Thus, instead of examining the decisions of reviewing courts, as the debate surrounding jurisprudential regimes theory does, it is necessary to consider the cases at the lowest possible level of the hierarchy—the trial court. Implementing populations give us important insight into the effect of law on policymaking, but where regulatory enforcement has been entrusted to litigation rather than administrative action, the relevant actors are better described as a consumer population. It is also necessary to consider the outcomes of settled cases as well as decided ones.[215]

The only study I have identified that systematically investigates whether litigants change their behavior in response to legal change focuses on the change in pleading standards after *Bell Atlantic Corporation v. Twombly*.[216] Morgan Hazelton points out that "[w]e would anticipate that if procedural rules have meaning and Supreme Court decisions are influential, then litigants, especially those represented by attorneys, will adapt their behavior in light of such changes. Such adaptations likely include differences in decisions regarding whether and how to engage the courts."[217] In response to changes in doctrine, "litigants . . . anticipate compliance by lower [] court judges . . . [and] form expectations about the likely outcomes of actions and choose the most advantageous course."[218]

From these arguments, Hazelton considers whether *Twombly* changed pleading practices by analyzing the level of specificity alleged in complaints before and after the change in the pleading

214. *Id.* at 375.

215. *See supra* Section I.B.

216. Bell Atlantic Corp. v. Twombly, 550 U.S. 554 (2007).

217. Morgan L. W. Hazelton, *Judicial Impact and Factual Allegations: How the Supreme Court Changed Civil Procedure Through the Plausibility Standard*, 9 J.L. & CTS. 159, 161 (2021).

218. *Id.* at 166.

standard.[219] She finds strong evidence that in cases where plaintiffs have access to extensive information pre-filing, plaintiff's attorneys have significantly increased the specificity of their factual allegations to ensure that they meet the *Twombly* standard.[220] However, in cases where proof of the claim usually requires discovery of information under the defendants' control, the increase in specificity was not apparent.[221]

Hazelton's findings have disturbing implications for the ability of the legal system to vindicate the rights of those with fewer resources and power. Because "[h]ard-to-plead issue areas include core constitutional issues, . . . these results indicate that governmental abuses may be less likely to be checked" post-*Iqbal*.[222] Similarly, if we only understand the effects of a legal regime through judicial decisions, we will fail to adequately understand the way that it shapes society more broadly.

In particular, the ways that changes in law move and shift the playing field for litigants of differential power and resources will be poorly understood if we do not recognize that the shift actually moves some litigants off the playing field altogether. The final step in the dispute generation process involves "claiming" against someone for the claimant's injury, including, in legal disputes, developing a legal theory to explain why the respondent is responsible.[223] If the regime becomes unfriendly enough to certain types of claims, then it is possible that claimants simply stop bringing them. Thus, I argue here that we should consider the ways that legal changes affect the choices that litigants make in whether they bring cases altogether, in addition to what the case brings.

II. EMPIRICAL STRATEGY

After describing the current understanding of the causes and implications of observable phenomena, empirical scholarship then

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219. *Id.* at 178.

220. *See id.* at 178–79 tbl.2 ("[T]here is evidence that [*Ashcroft v.*] *Iqbal* had an impact on the specificity found in torts complaints.").

221. *Id.* at 178–79 ("No such differences [between pre-*Twombly* and post-*Iqbal*] are apparent in civil rights cases . . . . [T]he difference in specificity in torts pleadings after *Iqbal* . . . [was] significantly larger than those in civil rights cases.").

222. Hazelton, *supra* note 217, at 181.

223. *See* Paul Harpur, *Naming, Blaming, and Claiming Ableism: The Lived Experience of Lawyers and Advocates with Disabilities,* 29 DISABILITY & SOC'Y 1234, 1242–43 (2014).

must ask, "What does this imply about what we should observe elsewhere in the world?" Thus, in order to undertake empirical analyses, we have to derive hypotheses that predict what we will see *if our theories are correct*. I begin this Section by explaining how theory and doctrine interact to generate the hypotheses I will test; I then describe the data that I will use to test these hypotheses; finally, I explain the statistical modeling strategies that I will employ to analyze the data.

A. HYPOTHESES

The theoretical and doctrinal expectations derived from prior work give rise to thirteen hypotheses as to how the three cases analyzed here affect litigation outcomes and litigant choices. Because employment-discrimination litigation requires litigants to begin their process by exhausting their administrative remedies, the EEOC, as the enforcing agency, can choose the sample of cases that proceed to enforcement litigation, diverting the rest of the population of disputes to the private docket.[224] The first group of hypotheses measures whether legal changes affect the way that employees bring charges to the EEOC's attention. The second set focuses on the EEOC's investigatory and enforcement decisions. The third set focuses on substantive outcomes in cases related to the regime shift. The final set focuses on the specific outcomes in settled cases brought by the EEOC.

The purpose of focusing on multiple stages of the process is to allow for the analysis to discern if litigants are selecting out of the process prior to a final judgment. If changes in the law affect the way that cases come before district courts in systematic ways, then we should be skeptical of analyses that focus on the ways that legal changes affect judicial decision-making. If cases that would otherwise come before the court are being selected out, then inferences about the effects of the law are inappropriate if they are coming from judicial decisions.

1. DISPUTE GENERATION

Even before employment litigation begins, employees frequently weigh whether to bring charges of discrimination at all.

224. *See* Gregory v. Ga. Dep't of Hum. Res., 355 F.3d 1277, 1279 (11th Cir. 2004) ("The purpose of this exhaustion requirement is that the EEOC should have the first opportunity to investigate the alleged discriminatory practices to permit it to perform its role . . . .") (internal quotations omitted).

Sometimes they consult with attorneys and sometimes they do not. But when they do, it is probably rational to expect them to receive advice on whether their claims are viable based on the law as it exists at that time. Because *Sutton*, *Reeves,* and *Desert Palace* have different predicted effects on the likelihood of plaintiff success, I expect them to play different roles in the charges filed with the agency. *Sutton* should lead private attorneys to advise clients not to bother to file ADA charges, while *Reeves* and *Desert Palace* should encourage employment-discrimination plaintiffs to file charges. Because *Reeves* itself is an age-discrimination case, even though its logic applied broadly to all employment-discrimination cases, at the charge stage its applicability may have been more limited.

However, because it takes time for knowledge of new legal developments to propagate through the profession, not all plaintiffs will receive up-to-date advice immediately upon announcement of the decision. Thus, while we would expect the new legal regime to affect the disputes that are brought to the EEOC through charges of discrimination, it is difficult to say *when* that shift will occur. We find ourselves in a theoretical space similar to that described by Pang, et al., where we feel confident that the legal regime's shift had observable implications, but we are less confident that the implications arrived exactly contemporaneously with the shift. Thus, the hypothesized effects of the cases are as follows:

H_1: Sutton *will decrease the share of EEOC charges that allege disability discrimination at some point after its ruling.*

H_2: Reeves *will increase the share of EEOC charges that allege age-discrimination causes of action at some point after its ruling.*

H_3: Desert Palace *will increase the numbers of EEOC charges in all causes at some point after its ruling.*

2. EEOC SELECTION

The EEOC's ability to control its docket is nearly unfettered. While the agency has an obligation to offer conciliation and mediation services, which have a fairly high success rate when used, either the charging party or the respondent can refuse to participate. Once these informal processes are complete or become impossible to conduct, the EEOC can investigate the charge as

much or as little as it sees fit.[225] When it chooses to end its investigation, it takes one of three actions. First, it can make a finding of no discrimination, in which case the file is closed.[226] Second, the EEOC can state that the record does not permit making a finding of discrimination, in which case it will issue a "right-to-sue" letter that grants the charging party the right to file a private cause of action.[227] Finally, it can make a finding of probable discrimination, in which case it can either issue a right-to-sue letter or bring an enforcement action.[228]

Because the EEOC exercises complete discretion over the scope and breadth of its investigations, the findings it makes, and the remedy it offers, it can respond to legal shifts by shedding cases that might have been the subject of enforcement actions to either "no discrimination" findings or right-to-sue letters. Shifts in employers' favor may affect total enforcement, as marginal cases are moved to the private docket and not replaced. Legal shifts toward employees are unlikely to affect total enforcement, since the agency may be resource-constrained and already bringing the maximum number of cases possible. However, changes in the legal regime that affect *some* types of claims could change the *distribution* of enforcement actions. Unlike the filing of charges by aggrieved actors, the EEOC can be expected to monitor new legal developments and immediately shift its behavior to meet the new standard. Because *Desert Palace* was a pro-employee decision that affected all EEOC claims, this theory does not provide a falsifiable prediction for it. Thus, the cases are expected to have the following effects:

H_4: Sutton *will reduce the share of EEOC enforcement actions that bring ADA claims.*

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225. For example, age-discrimination charges are an exception to the exhaustion-of-remedies requirement; the EEOC receives sixty days to investigate the claim, after which the claimant may file suit. For Title VII and ADA claims, the EEOC is given 180 days to investigate, and then the claimant may request a right-to-sue letter, but until the letter is issued the claim is not ripe. Equal Pay Act claims have no exhaustion-of-remedies requirements. *After You Have Filed a Charge*, U.S. EQUAL EMP. OPPORTUNITY COMM'N, https://www.eeoc.gov/after-you-have-filed-charge (last visited Mar. 21, 2023).

226. *See* 29 C.F.R. § 1601.18 (2023). The regulations refer to this as "dismissal" of the charge. *Id.*

227. *Id.* § 1601.19(a). The regulations refer to this as a finding of "no reasonable cause" for discrimination. *Id.*

228. *See id.* § 1601.28(b). The regulations refer to this as finding "reasonable cause" for discrimination. *Id.*

H_5: Sutton *will reduce the total number of enforcement actions.*

H_6: Reeves *will increase the share of EEOC enforcement actions that bring age claims.*

3. PLAINTIFF PAYOUTS

The Priest–Klein hypothesis literature argues that in order to understand the effects that law has on case outcomes, it is necessary to be able to see the effects not only of tried cases, but settled ones.[229] Theories of dispute generation complicate this narrative,[230] but certainly if we limit our inferences to being about "the universe of cases that proceed to litigation," and if we have information about settlements then the issue of selection effects does not arise.[231]

Most of this literature focuses on the likelihood of plaintiff victory. When working with that kind of dichotomous outcome, cases that are neither a victory nor a loss, such as settlements, do not fit into the analysis. But if the question becomes what effect does legal change have on the amounts plaintiffs receive, then settlements become useful information, as long as their payouts are observable. While most private settlements are confidential, government parties are much more likely to have their settlements be public due to public records statutes. Thus, administrative enforcement litigation is a unique opportunity to include litigated cases with settled cases to determine the effects of jurisprudential regime shifts on all litigated matters.

When the Supreme Court decided *Sutton*, it subsequently made ADA claims much more difficult to prove. Because being a "disabled person" is an essential element of the claim, defining any person whose disability is remediable as "not disabled" meant that previously viable ADA claims could no longer be proven.[232] Thus, I predict:

H_7: Sutton *will decrease payouts for plaintiffs in EEOC administrative enforcement litigation for cases containing ADA claims.*

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229. *See generally* Klerman & Lee, *supra* note 20. *See also supra* Section I.B.

230. *See generally* Harpur, *supra* note 223, at 1245.

231. Leandra Lederman, *Which Cases Go To Trial: An Empirical Study of Predictors of Failure to Settle*, 49 CASE W. RSRV. L. REV. 315, 327–28 (1999).

232. *See* Allison Duncan, *Defining Disability in the ADA:* Sutton v. United Air Lines, Inc., 60 LA. L. REV. 967, 975 (2000).

However, in *Reeves*, the Supreme Court made it much easier to defeat summary judgment motions in employment cases. In that case, the Court held that plaintiffs could meet the last step of the *McDonnell Douglas* burden-shifting framework either by showing that discrimination was the *actual* motivation behind their claim, or that the explanation the employer proffered was false, without having to show the true motivation.[233] In other words, plaintiffs were no longer required to show the actual mental state of the employer, which was frequently impossible.[234] It was enough for them to show that the employer's claimed mental state was false.[235] While *Reeves* was an ADEA case, its logic was applicable to all the statutes where *McDonnell Douglas* had been applied.[236] Thus, I predict both:

H8: Reeves *will increase payouts for plaintiffs in EEOC administrative enforcement actions for age cases.*

H9: Reeves *will increase payouts for plaintiffs in EEOC administrative enforcement actions for all cases.*

Desert Palace presents an odd situation. Zimmer argues that *Desert Palace* fundamentally changed the proof structure of employment-discrimination cases, because:

> [O]nce the defendant introduces evidence of a nondiscriminatory reason the case ceases to be a single-motive case subject to the *McDonnell Douglas* "determinative influence" standard because, at that stage, it becomes at least potentially a mixed-motive case . . . and must be treated accordingly [by finding a dispute of material fact and giving a mixed-motive instruction].[237]

However, the hopeful expectations of employee-side advocates came to nothing in the run of litigation; because *Desert Palace* did not explicitly overrule the use of *McDonnell Douglas* in summary

233. *Reeves*, 530 U.S. at 146–47.

234. *See id.* at 147.

235. *Id.* at 147–48.

236. *See, e.g.*, Price v. Thompson, 380 F.3d 209, 212 (4th Cir. 2009) (applying *Reeves* in the Title VII retaliation context); Forrester v. Rauland-Borg Corp., 453 F.3d 416, 416 (7th Cir. 2006) (Title VII discrimination).

237. Michael J. Zimmer, *The New Discrimination Law:* Price Waterhouse *Is Dead, Whither* McDonnell Douglas*?*, 53 EMORY L.J. 1887, 1941 (2004). *See also* Jeffrey A. Van Detta, *"Le Roi Est Mort; Vive Le Roi!": An Essay on the Quiet Demise of* McDonnell Douglas *and the Transformation of Every Title VII Case after* Desert Palace, Inc. v. Costa *Into a Mixed Motives Case*, 52 DRAKE L. REV. 71 (2003).

judgment proceedings, many courts have continued to make use of it.[238] From this, I predict that *Desert Palace* did not create a jurisprudential regime at the outcome level, and, therefore, that the analysis will not disclose changes in outcomes. So, I hypothesize:

H_{10}: Desert Palace *will have no effects distinguishable from random outcomes on plaintiff payouts in EEOC administrative enforcement litigation.*

4. SETTLEMENT OUTCOMES

Though including settled cases in the analysis allows us to make inferences about litigated cases broadly, the fundamental argument of the Priest–Klein literature is that settled cases *differ* systematically from cases that proceed to judgment. If this argument is true, then settled cases before each jurisprudential regime shift should differ from settled cases after the shift, as litigants change their willingness to accept settlements based on the new legal landscape.

Because *Sutton* made ADA claims more difficult to prove, plaintiffs should be expected to lower their projected value and accept smaller settlements. Similarly, defendants should be expected to stiffen their negotiating posture and refuse to pay what plaintiffs might have obtained before *Sutton.* Thus, I hypothesize:

H_{11}: Sutton *will reduce payouts in settled cases involving ADA claims.*

Reeves opened the door for plaintiffs to prove their claims in a manner previously thought to be foreclosed under the statute.[239] Thus, after it was handed down, plaintiffs can be expected to have strengthened their negotiating positions, while defendants paid

238. *See* David F. Hamilton, *On* McDonnell Douglas *and Convincing Mosaics: Toward More Flexible Methods of Proof in Employment Discrimination Cases*, 17 EMP. RTS. & EMP. POL'Y J. 195, 196 (2013) ("[I]n terms of practical effects, the last time I checked, a few weeks ago, *McDonnell Douglas* had been cited about 40,000 times and is cited hundreds more times every month."). *See generally* Paul W. Mollica, *What's On The Secret Title VII Menu?: Proving "Motivating Factor" and "Same Action" Under the 1991 Civil Rights Act*, 35 ABA J. LAB. & EMP. L. 53, 53 (2020).

239. *See generally* Charles F. Thompson, Jr., *Juries Will Decide More Discrimination Cases: An Examination of* Reeves v. Sanderson Plumbing Prods., Inc., 26 VERMONT L. REV. 1, 41–42 (2001); *but see* David J. Turek, *Affirming Ambiguity:* Reeves v. Sanderson Plumbing Prods., Inc., *& the Burden-Shifting Framework of Disparate Treatment Cases*, 85 MARQ. L. REV. 283, 303–310 (2001).

more than they would have prior. This gives rise to the following hypothesis:

H_{12}: Reeves *will increase payouts in all settled cases.*

Again, *Desert Palace* presents a difficult situation to model. As discussed above, early commentary hailed it as an incredibly plaintiff-friendly decision, but later analysis demonstrated that its reach had been quite limited.[240] In settlement negotiations, this would suggest that the immediate aftermath of *Desert Palace* would have led to plaintiff windfalls, followed by a reversion to prior patterns as its limited reach became more apparent. This leads me to predict:

H_{13}: Desert Palace *will demonstrate a non-linear relationship with payouts in all settled cases.*

Now that I have developed predictions for the relationship that each case will have on the phases of litigation that I am interested in, in the next section I will describe the data that I will use to test them. This data comes from three sources: the EEOC itself, the Federal Judicial Center (FJC), and a dataset compiled by the EEOC Litigation Project through an exhaustive review of federal court docket sheets.

B. DATA AND METHODS

The EEOC—the first of three data sources—publishes annual data related to the number of charges filed related to each statutory source of enforcement authority.[241] This data does not disclose the ways in which the charges are resolved—whether it is with a "no-discrimination" finding, a right-to-sue letter, or an enforcement action.[242] Thus, this data provides useful information about the way that charging parties (private litigants and their private counsel) understand the law. When they see the legal landscape as less welcoming to certain types of claims, they are less likely to file charges that would lead to those types of claims.

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240. Zimmer, *supra* note 237, at 1941; Hamilton, *supra* note 238, at 196.

241. *Charge Statistics (Charges filed with EEOC) FY 1997 Through FY 2021*, U.S. EQUAL EMP. OPPORTUNITY COMM'N, https://www.eeoc.gov/data/charge-statistics-charges-filed-eeoc-fy-1997-through-fy-2021 (last visited Feb. 5, 2023). *See also Charge Statistics FY 1992 Through FY 1996,* U.S. EQUAL EMP. OPPORTUNITY COMM'N, https://www.eeoc.gov/data/charge-statistics-fy-1992-through-fy-1996 (last visited Feb. 5, 2023).

242. This data is available elsewhere. *See infra* note 244, and accompanying and following text.

However, other factors also play a role in determining whether a given litigant files a charge. Some of these factors can be tested with the relatively bare-bones data the agency provides; others cannot. For example, while the agency's enforcement and ideological priorities can be proxied with an administration fixed-effects variable,[243] the ideological expectations related to jurisdictional effects are impossible to model because the data does not break down charges by jurisdiction.[244] In addition, because the charge data does not provide information on resolutions, the EEOC charge data cannot provide any information about agency actions.[245]

When using this data, the dependent variable is either the number or share of charges filed with relevant allegations related to the case under analysis. The test variable is the date; my priors point to the date of the case being handed down being the relevant break point, but because I do assume that charging parties may become aware of changes in the law over time, I test the results I get depending on different dates.

In order to test whether the date that matters is the date the decision is handed down or a later date, I use Markov chain Monte Carlo methods. Markov chain Monte Carlo methods are methods

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243. Fixed-effects variables hold as constant everything that is true across all instances with a common value. In this case, because the analytical period is 1997–2021, there will be five fixed-effects variables: a "Clinton" variable that takes the value of "true" from 1997–2000 and "false" thereafter; a "Bush" variable that is "true" from 2001–2008; an "Obama" variable that is "true" from 2009–2016; a "Trump" variable that is "true" from 2017–2020; and a "Biden" variable that is "true" for 2021. *See infra* Table A5. This will enable the analysis to ignore differences in litigant perceptions of the agency across administrations. *See generally* JEFFREY M. WOOLDRIDGE, INTRODUCTORY ECONOMETRICS: A MODERN APPROACH 463–469 (7th ed. 2019).

244. The EEOC does publish data on charge receipts by state, including information on the share of national filings on a particular basis filed in that state. However, the EEOC only publishes charge receipt by state going back to 2009, which means that it is impossible at this time to test the legal changes in question at the state level. *FY 2009–2021 EEOC Charge Receipts by State (includes U.S. Territories) and Basis*, U.S. EQUAL EMP. OPPORTUNITY COMM'N, https://www.eeoc.gov/statistics/enforcement/charges-by-state (last visited Feb. 5, 2023).

245. The EEOC publishes extensive data on charge resolution by statute, dating back to 1997. *Enforcement & Litigation Statistics*, U.S. EQUAL EMP. OPPORTUNITY COMM'N, https://www.eeoc.gov/data/enforcement-and-litigation-statistics-0 (last visited Feb. 5, 2023). However, adding this data to the charge data introduces a level of complication in the analysis as it converts a longitudinal data set ("panel" data) into a hierarchical, longitudinal data set ("multi-level" data). Thus, in the interest of simplifying the analysis, I limit my charge discussion to considering charges filed and approach resolutions in a different way, outlined *infra*.

of statistical analyses that "rely on the generation of pseudorandom numbers . . . where each number is dependent on the previous number in the sequence."[246] In other words, the method uses the real data to develop an expectation that is tested by generating pseudo-random data that shares the relevant characteristics of the real data (but may differ in outcome-determinative details). The computer then reanalyzes the new, generated data and uses its results to update its expectations. After thousands of iterations of this process, the analysis can confidently identify when, in a time series sharing the characteristics of the real data, we can expect to see an effect for our test variable. To translate into the particular analysis I am offering, at the end of the analysis, the computer will tell us confidently when (as in, at what point in time) we see an effect in, for example, the numbers of ADA charges filed after *Sutton*.

Second, the charge resolution data the EEOC produces can be used to consider if legal changes are systematically pushing the agency to move claims toward the private docket. I include a dummy variable[247] for *Sutton* that takes a value of "true" from Fiscal Year 2000 (the first one after the handing down of Sutton in June 1999) until Fiscal Year 2009 (when the ADA Amendments Act of 2008, which reversed *Sutton*, went into effect). In addition, including a variable for the share of charges filed under a particular statute in a given year, drawn from the charge filing data, will control for the possibility that charging parties are causing shifts in resolutions by changing the population of charges being resolved.

Because I expect the EEOC to be immediately responsive to jurisprudential regime shifts, it is unnecessary to run the simulated analyses as with the charge distribution hypotheses. For H_4 and H_5, the dependent variable will be the share of charge resolutions for the appropriate sample (ADA claims and all claims) that result in the issuance of a right-to-sue letter. These hypotheses are built around an assumption that after each of the

246. Chuck Huber, *Introduction to Bayesian Statistics, Part 2: MCMC and the Metropolis-Hastings Algorithm*, THE STATA BLOG: NOT ELSEWHERE CLASSIFIED (Nov. 15, 2016), https://blog.stata.com/2016/11/15/introduction-to-bayesian-statistics-part-2-mcmc-and-the-metropolis-hastings-algorithm/.

247. Dummy variables are variables that measure the presence or absence of a factor hypothesized to cause an effect. They take a value of one (or "true") when present, and a value of zero (or "false") when absent.

three decisions, the agency will begin shuffling cases that they otherwise might have pursued onto the private docket.

In order to analyze this data, the appropriate modeling strategy involves panel analysis. Panel data is where the same set of variables are observed for the same units over time.[248] Panel data can be tested using a "fixed-effects" model, where the unobserved variables are held constant within either cross-sectional units or time units or both, or "random-effects" models, where unobserved variables are treated as randomly-assigned.[249] When unobserved variables are believed to be correlated with the cross-sectional units or time units, a random-effects model is superior to a fixed-effects model.[250] In this analysis, because I believe that there may be time-dependent characteristics that are unobserved, or geographic variation correlated with the categories of dispositions, a random-effects model is appropriate.

Third, the FJC's Integrated Database provides information on the complete federal docket dating back to 1988.[251] The only information the database provides on the type of claim being brought is the "nature of suit" (NOS) code listed on the initial filing cover sheet; this data will only be used to test the hypothesis related to the distribution of ADA claims between EEOC enforcement action and the private docket.[252] In this data, the appropriate measure is the share of cases in the NOS that list the EEOC as the plaintiff (and thus are enforcement actions) versus the share of cases in the NOS that list any other plaintiff. Because the ADA NOS code only came into existence in 2005, the test will be the inverse of that done with the EEOC charge data. Instead of

248. WOOLDRIDGE, *supra* note 243, at 427, 484–96, 806.

249. *Id.* at 462–63, 470, 484.

250. *Id.* at 473, 480, 496.

251. The database actually dates back to 1970, but data collection processes differed. Pre-1988 data can only be combined with post-1988 data with great difficulty. FED. JUD. CTR., THE INTEGRATED DATABASE: A RESEARCH GUIDE 1–2 (2019), https://www.fjc.gov/sites/default/files/IDB-Research-Guide.pdf. Because I am only using this data for ADA claims, pre-1988 cases (which predate the ADA) are irrelevant.

252. This is because the NOS codes contain a separate category for "Civil Rights – ADA Employment." All other EEOC claims are brought under "Civil Rights – Other," which also contains a population of claims that are not EEOC claims at all (mostly claims under 42 U.S.C. § 1983). Because of this, it is impossible to differentiate between private claims being brought because the EEOC has declined enforcement, and private claims being brought under statutes that did not involve the EEOC at all. This will skew the results, leading us to conclude that claims are being diverted to the private docket when they were in fact never under the EEOC's umbrella to begin with.

asking "did *Sutton* reduce the share of EEOC enforcement given to ADA claims?" the FJC data will answer "did the ADA Amendments Act of 2009, which reversed *Sutton,* increase the share of EEOC enforcement resources given to ADA claims?" This question is the mirror image of the EEOC question, but it still serves as a valid test of H_4.

Because the dependent variable (whether the action was brought by the EEOC) is binary (it is either true or false), and because the FJC data includes over 32,000 observations of ADA employment claims, the appropriate analytical method is logistic regression. The FJC data is relatively bare-bones; it does not include any information on the judges assigned to the case and very little information on the disposition of the case.[253] I include two controls from the FJC data: a fixed-effects variable for the district court to control for any unobserved characteristics that are present in one court but not another, and a fixed-effects variable for the disposition of the case. I use them as fixed-effects variables because jurisprudential regimes theory does not actually predict that either the jurisdiction or the disposition of the case matter; but in case there are systematic differences in whether the EEOC brings cases in certain jurisdictions or cases that tend toward certain dispositions, using a fixed-effects variable holds those differences constant in the analysis.

Finally, the remaining hypotheses are tested using data from the EEOC Litigation Project. This dataset consists of 2,316 enforcement actions brought by the EEOC between 1996 and 2006.[254] It contains a stratified random sample of all enforcement actions, and captures information on 302 variables, including information about the allegations brought, the relief sought, interventions by private plaintiffs, judges, motion practice, and defendants.[255] Because the data collection process used docket

253. The disposition variable is a categorical variable that captures twenty different potential dispositions of cases. Some are very rare; for example, it is the rare ADA claim that gets disposed of by the denial of an appeal from a magistrate judge's ruling (N=1).

254. Pauline T. Kim, Margo Schlanger, Christina L. Boyd & Andrew D. Martin, *How Should We Study District Court Decision-Making*, 29 WASH. U. J.L. & POL'Y 83, 110 (2010); *EEOC Litigation Project*, WASH. UNIV. (June 13, 2019), https://eeoclitigation.wustl.edu/.

255. Kim, Schlanger, Boyd & Martin, *supra* note 254; *EEOC Litigation Project*, *supra* note 254.

sheets to capture case events, the entire dataset contains multiple observations about each case.

For H_6, H_7, and H_8, the dependent variable was the plaintiff's payout, regardless of whether it comes from settlement, a consent decree, or a final judgment. I use both raw dollars and log dollars to control for the effect of outliers.[256] The major test variable is the date of termination—cases terminated after the relevant legal shift should be expected to exhibit the relationship predicted as compared to cases terminated prior to the shift. I use the date of termination instead of the date of filing because legal shifts *may* affect cases pending when the legal shift occurs. The same analysis applies for H_9, H_{10}, and H_{11}, except their analysis only uses settled cases as the sampling frame. I include controls for a number of variables, as explained in the Appendix.[257]

For data where the dependent variable cannot take certain values because it is logically impossible for those values to occur, the tobit regression model is most appropriate.[258] Because the logarithm of zero is undefined,[259] for log-dollar models, I added a nominal amount to all dependent-variable values to eliminate zeros and set the tobit limit to the minimum value that the logarithm could take. As a robustness check, I also run the models using a zero-inflated Poisson model. Poisson distributions are appropriate when the dependent variable is a count; "dollars received" can be conceived as either a count (each dollar representing a discrete upward step) or a continuous variable. Zero-inflated models are appropriate when the number of zeros in the dataset is driven by reasons other than variables included in the model. In this case, plaintiffs may lose their case (recover zero dollars) both because of variables included in the model and because of presenting insufficient evidence, which is not captured in the model. I offer these three alternative models as robustness checks; I do not have a strong intuition as to which of them is superior given the data structure. If all three report similar

256. Logarithms (log dollars) are a way to compress the variation in a given distribution by using the "power of ten" that equates to the real value. Using logarithmically transformed values analysis can avoid false statistical relationships being observed caused by outlier observations. *Math Review: Useful Math for Everyone, What Is A Logarithm?*, UNIV. MINN. SCH. PUB. HEALTH (2004), http://www.mclph.umn.edu/mathrefresh/logs3.html.

257. *Infra* Table A4.

258. WOOLDRIDGE, *supra* note 243, at 572–73, 596–97.

259. UNIV. MINN. SCH. PUB. HEALTH, *supra* note 256.

relationships, then that would be strong evidence to support the hypothesis. If they move in different directions, it raises the question of why.

At this point, it has become clear what I predict to see in the analysis based on pre-existing theory and reason. Based on the data that is available and what is being tested, I have also described a plan that tests the hypotheses in such a way that permits appropriate causal inferences to be drawn. The next step is to report the results of those tests and determine how well our predictions fit the observations of the real world we experience. The next Section will report the results of the statistical analysis and discuss the implications of those results for jurisprudential regimes theory in litigant behavior.

III. EFFECTS OF THE LEGAL REGIME

A. RESULTS

Interpreting results from sophisticated statistical analyses presents several difficulties. A real danger exists in overclaiming. Thus, while complete regression tables will be provided in the Appendix, this section of the Article will focus on translating the statistical results into an explanation of what they mean for substantive effects and how confident readers should feel in interpreting the results.

1. DISPUTE GENERATION

The change-point analysis is predicated on the assumption that while jurisprudential-regime shifts should be expected to cause charging parties and private counsel to shift their willingness to file charges with the EEOC, it may be unreasonable to expect that shift to happen immediately when the new regime is announced. In other words, the purpose of the analysis is not only to identify whether the regime shift occurs, but *when* it occurs. The Bayesian analysis tests which fiscal year makes the most sense as a break point, depending on the characteristics of the data when treating a given year as the hypothetical break point. The year that best presents a coherent "before and after" pattern is reported as the most likely change point.

While there is *some* evidence of diffusion delays, it is minimal. Complete results are available in the Appendix.[260] For *Sutton*, handed down in June 1999, the evidence points to a break point in FY2001.[261] For *Reeves*, handed down in June of 2000, the evidence points to a break point early in 2000, actually prior to its ruling, suggesting that it was not a jurisprudential regime shift in charge filing.[262] And for *Desert Palace*, handed down in June of 2003, the evidence points to a break point *also* prior to its announcement, again suggesting that it did not represent a regime shift for filing charges.[263]

These findings suggest that, of the proposed dispute generation hypotheses, only H_1 is confirmed. The evidence seems clear that *Sutton* had a chilling effect on the filing of EEOC charges, but that there was some delay. Because the most likely change points for *Reeves* and *Desert Palace* were before their announcement, it is impossible for those cases to have triggered the change in charge filing that the data identifies. Thus, H_2 and H_3 are rejected on this data.[264]

2. EEOC SELECTION

In this analysis, I tested whether the *share* of ADA claims dropped due to *Sutton*, controlling for time-based effects and year-over-year changes in total charges received. Considering *only* merits determinations presents a statistical power issue; with that limitation, only twenty-five observations can be had. Full results are available in the Appendix.[265] While it appears that *Sutton* led to a reduction in merits determinations of over 211, year-over-year, we can only be confident of that result in about 55% of samples. This is a result of which we should be skeptical. When we include total ADA charges filed in order to control for the possibility that charging parties may be removing cases from the docket by filing fewer charges, we see much stronger evidence of legal effect. Including that element shows that *Sutton*'s influence reduces

260. *Infra* Table A5.

261. *Infra* Table A5.

262. *Infra* Table A5.

263. *Infra* Table A5.

264. Rejection of H_2 and H_3 should not be read as a definitive statement on the effect of *Reeves* and *Desert Palace* on private litigants filing charges with the EEOC. Rather, it should simply be read as a claim that *this data* does not demonstrate a relationship between these cases and dispute generation with the agency.

265. *Infra* Table A6.

merit determinations and charges filed by over 2,100, year-over-year. The confidence in this point estimate is over 99%, meaning that we expect less than one in one hundred samples to deviate significantly from this estimate. We can further demonstrate that the causal mechanism is being driven by charging parties rather than the agency itself. This is accomplished by running the same analysis but instead of measuring by merits determinations, we measure by the number of charges given right-to-sue letters. In that case, we see an even greater reduction year-over-year from *Sutton*—over 4,200 fewer right-to-sue letters being granted in ADA charges, year-over-year, when *Sutton* is in effect.

A few interpretive caveats are in order here. First, the phrase "reduction year-over-year" should not be interpreted as a linear relationship—it is not 4,200 fewer right-to-sue letters every year during *Sutton*'s tenure. Instead, the way to interpret this is that if one was to observe the same year in two different universes, one where *Sutton* applied and one where it did not, then they would expect to see 4,200 fewer right-to-sue letters in the universe where *Sutton* applied than in the one where it did not. The strength of the panel analysis is that it leverages the collection of the same data over time and the control for unobserved characteristics to allow us to *infer* what we would have observed in the counterfactual where our observables were different but all else was the same.

Second, the model that directly tests H_4 is skeptical of its results. With a confidence measure around 55%, there is only slightly more confidence in concluding that *Sutton* had an effect on EEOC merit resolutions than in predicting the results of a fair coin flip. Thus, while the evidence offered is *consistent* with H_4, I am not yet comfortable with declaring it *confirmed.*

The FJC data confirms that the hypothesis is correct, although it is skeptical that the effect is worth caring about. Full results are available in the Appendix,[266] but during the four years of data during which *Sutton* was good law, the EEOC was six-tenths of one percent (.6%) less likely to bring an ADA claim than it was after Congress amended the statute to reverse *Sutton.* While the confidence in this estimate is quite high, the tiny effect-size suggests that any effect of the *Sutton* jurisprudential regime on the EEOC's selection of ADA cases for enforcement action was

266. *Infra* Table A7.

minimal, and certainly not consistent with the sweeping claims of jurisprudential-regimes proponents. While H_4 may be confirmed, I would hardly describe these effects as a resounding victory for jurisprudential regimes theory.

Similarly, the panel analysis for the effect of *Sutton* on total charges filed is deeply skeptical. Full results are available in the Appendix.[267] Although the pattern is as predicted—*Sutton* predicts a reduction of slightly less than 1,500 total charges filed per year—the confidence is actually slightly worse than our confidence in the result of a fair coin flip, about 45%. Thus, I am skeptical that the analysis has captured a meaningful result. My guess is that because *Sutton* can only be interpreted as affecting ADA claims, which are only between 15–25% of EEOC charges, and because *Sutton*'s effect on ADA resolutions was so substantively small, that it is impossible to discern an effect from this data that is distinguishable from random fluctuations in the data. Thus, I am not prepared to describe H_5 as confirmed.

The effect of *Reeves* on EEOC resolutions of ADEA claims is stronger. The direction is as predicted—*Reeves* causes an increase in ADEA charges resolved, controlling for total receipts. The substantive effect is smaller than *Sutton*; only about 1,500 more ADEA charges reaching a merits resolution year-over-year. However, the confidence is much stronger, nearly 75%. This means that in three-quarters of samples, we would expect to see *Reeves* lead to an increase in ADEA charges resolved.

One possible explanation for the lack of confidence may be that the model does not incorporate the effect of *Gross v. FBL Financial Services, Inc.*, where a fractured Supreme Court concluded that ADEA claims cannot be proven if the plaintiff cannot prove that age was the sole motivation for the adverse employment action.[268] That decision could reasonably be expected to reduce ADEA charges resolved, as it makes it impossible to prove an ADEA claim. However, rerunning the model with a dummy for *Gross*'s applicability produces a smaller positive effect, with almost zero confidence; in other words, *Gross*, which made age claims harder, led to more resolutions. This seems unlikely, and the very low confidence level leads me to simply conclude that *Gross* had no effect. This seems plausible; because *Gross* was a

267. *Infra* Table A8, col.1.

268. *Gross*, 557 U.S. at 178–80.

plurality decision, its logic was regarded skeptically until it was adopted for retaliation claims in *University of Texas Southwestern Medical Center v. Nassar*.[269]

Another possible explanation for the model's caution in having confidence may be that *Reeves* had a greater impact on the broader EEOC landscape. While *Reeves* itself is an age-discrimination case, its logic applied to any claim using the *McDonnell Douglas* framework, including those evaluating Title VII claims. Rerunning the analysis but looking to the total charges received[270] rather than only ADEA claims provides powerful evidence that *Reeves* was seen as a broader legal shift than only affecting ADEA claims. Looking at total EEOC charges, not only is the substantive effect larger—over 7,300 additional merits resolutions year-over-year compared to 1,500 for ADEA charges alone—but the confidence level reaches 97%, meaning that less than 5% of samples will see a reduction in total charges post-*Reeves*. This is the traditional standard for "statistical significance,"[271] which allows us to confidently state that *Reeves* triggered a shift in the jurisprudential regime that governed EEOC selection of charges for resolution. Thus, while H_6 is not confirmed as phrased, the evidence *does* confirm that *Reeves* changed the way that the EEOC perceived the law for choosing cases to bring into enforcement.

3. PLAINTIFF PAYOUTS

At this point, the evidence for a jurisprudential regime shift is mixed; it appears that *Sutton* had effects on whether aggrieved employees filed ADA charges at all, and *Reeves* appears to have had a broader effect than its own terms, but that effect appears focused on the cases that the EEOC was willing to pursue as enforcement actions. *Desert Palace* does not appear to have had an impact on charge filing, and it is impossible to discern if it had an impact on enforcement decisions.

But there might be effects that remain to be seen. If the change in the law affects the payouts plaintiffs receive, that would be an effect not captured in the jurisprudential regimes literature that focuses on judicial voting. If more cases result in zero payouts after the legal shift, or if cases that would have resulted in nominal

269. Univ. of Tex. Sw. Med. Ctr. v. Nassar, 570 U.S. 338, 356–60 (2013).

270. *Infra* Table A8, col.2.

271. *See generally* Ronald L. Wasserstein & Nicole A. Lazar, *The ASA's Statement on p-Values: Context, Process, and Purpose*, 70 AM. STATISTICIAN 129, 129 (2016).

payments lead to more significant payouts, then this is a major result for the legal model. To test these hypotheses, I use two different model specifications—one which assumes that payouts are a continuous variable, and one which assumes that it is a count variable. The difference between these models amounts to whether the model thinks that the space between $1,000 and $1,001 is a spectrum or a void.

Interpreting these results presents different challenges from the regressions that came before. When reviewing the results in the Appendix, tobit regressions assume that it is impossible for plaintiff outcomes to go below zero and ignore observations with negative values. The raw-dollar tobit regressions should be interpreted as, holding all else constant, a case *before* the alleged jurisprudential regime shift will result a payout that differs from the same case *after* the shift by the coefficient.[272] The log-dollar regressions have the benefit of reducing the influence of outliers (such as the large payouts in pattern-and-practice cases involving national chains), but have the downside that their coefficients are expressed in logarithms that have to be translated into real numbers.[273] The zero-inflated Poisson regressions produce incidence-rate ratios, which in this context can be interpreted as payout ratios before and after the regime shift. Thus, if a legal shift produces an incidence rate ratio of 1.1, then a case that paid out $1,000 before the shift would be expected to pay out $1,100 after the shift.

Interpretation would be easy if all the regressions in the Appendix broadly agreed on the effects they identified, but they do not.[274] For *Sutton*, the tobit regression is highly unconfident; both the raw-dollar and log-dollar regressions are less confident in their point estimates than in a fair coin flip. Of course, the point estimate for both is positive, suggesting that this regression points toward ADA cases having *higher* payouts post-Sutton. This is completely contrary to the predictions of the theory, but consistent with earlier findings that *Sutton* had a major effect on the filing of ADA charges. If *Sutton*'s effects are being felt prior to litigation,

272. The exception to this is when the coefficient would predict that one or both cases would pay the plaintiff less than zero, in which case the result would be zero. This analysis ignores the possibility of counterclaims, or net-negative payouts, because they are empirically very rare in the data.

273. The results reported in the main text will translate logarithms into real dollars; for log coefficients, review the Appendix.

274. *Infra* Table A9.

then only very strong ADA claims are proceeding to enforcement litigation. By comparison, the Poisson regression sees *Sutton* associated with a 4% drop in rate ratios, meaning that payouts for a case post-*Sutton* would be 4% lower than payouts for the same case pre-*Sutton*. It is highly confident in this outcome; less than 1 in 1,000 samples would have different results. This is consistent with theoretical expectations, and because the tobit regression is so uncertain, it is not contradicted by other results. However, the narrative produced by these results is deeply interesting because it suggests that there may be *interactive* effects in regime shifts. On one hand, the effect on charge filing means that the ADA claims that proceed to enforcement litigation are significantly stronger; on the other hand, those stronger claims are still facing a skeptical judiciary and a more stringent legal standard and thus the ultimate payout is still dropping, *but not as much as they would have without the pre-litigation effects*.[275]

For *Reeves*, all three regressions at least produce results in the same direction. Unfortunately, all three are contradicting theoretical expectations. All three regressions, reported in the Appendix,[276] predict that an ADEA case resolved pre-*Reeves* will have higher payouts than the same case post-*Reeves*. The raw-dollar tobit regression predicts a drop of almost $200,000, while the log-dollar tobit regression predicts a drop of almost $40,000. Confidence is similarly scattered; the raw-dollar regression is 69% confident, while the log-dollar regression is 85% confident.[277] With only forty-four observations, it appears from the data that this result is driven by the fact that the lion's share of ADEA cases in the dataset (including eight of nine cases where plaintiffs won nothing) occur after *Reeves*. Using the Poisson model to account for the zero-inflation addresses this, resulting in an 18% drop in incidence rate ratios, predicting that the same case would pay 18% less after *Reeves*. Again, the Poisson model is highly confident, to the point of expecting a different result in only 1 out of 1,000 samples.

The results in *Reeves* demonstrate the way that limiting the analysis of regime effects to judicial outcomes can skew our expectations. Because *Reeves* is an age-discrimination case, the

275. The possibility of this is left for future research.

276. *Infra* Table A10.

277. The difference between the two is almost certainly driven by the reduced variance that log-transformed dependent variables offer.

rarity of these cases could lead us to conclude that *Reeves* either had no effect on case outcomes or that it had a negative effect. But because we have examined the pre-litigation data, we know that what happened was that *Reeves* emboldened the EEOC to pursue more age-discrimination cases, which meant that weaker cases were brought into court, leading to a net result of reducing expected payouts in litigated cases.[278] This is clear evidence of selection effects among the population of litigated cases, but, crucially, it is evidence that is *impossible* to obtain if analysis is limited to *litigated* cases.

However, as discussed above, the logic of *Reeves* reached beyond its applicability to age-discrimination claims.[279] In fact, *Reeves* was applied to all employment-discrimination claims using the *McDonnell Douglas* framework. As we saw in the selection of cases for enforcement, *Reeves* had a much more measurable effect beyond the age-discrimination context. However, that effect did not carry over to plaintiff payouts. When I reran the analysis for Title VII cases, the Appendix[280] reports that once again *Reeves* had a negative effect on plaintiff payouts. While point estimates and confidence intervals varied across model specifications, in all three models the data suggests that *Reeves* led to EEOC overconfidence and that the agency brought cases post-*Reeves* that courts and juries viewed skeptically, leading to reduced plaintiff payouts. Once again, the evidence of regime shifts is impossible to obtain from case outcomes.

The prediction for *Desert Palace* is that it produced no discernible pattern; thus, we should expect regression results to be scattered and point in both directions. However, that expectation is not borne out; as reported in the Appendix,[281] all three regressions associate *Desert Palace* with higher plaintiff payouts. Confidence levels vary; the raw-dollar regression is confident of its results in less than one in three samples, while the log-dollar regression is 88% confident and the Poisson regression is confident to one in one thousand samples. This suggests that even if *Desert Palace* was not the sea change in employment law that worker advocates and scholars had hoped, it *did* successfully make federal courts friendlier to employment-discrimination claims. Because

278. *See supra* Section III.A.2.

279. *Supra* Section III.A.2.

280. *Infra* Table A11.

281. *Infra* Table A12.

there is no evidence for *Desert Palace* playing a role in pre-litigation outcomes, the evidence supports an inference that *Desert Palace* did lead to an increase in plaintiff payouts.

Thus, the evidence for jurisprudential regimes theory in payouts across all cases is mixed. H_7 offers a confused set of results, some of which confirm it and others which do not; H_8 and H_9 are contradicted by the evidence; and H_{10} is contradicted in a way that suggests the existence of a regime shift that theory did not predict.

4. PAYOUTS IN SETTLED CASES

The Priest–Klein hypothesis asks whether settled cases are significantly different from litigated cases, and whether that means we can make inferences about the former from the latter. Having both types of cases in the dataset means that I can investigate this hypothesis directly by testing "all cases" against "settled cases" and determining whether those two populations systematically differ. Except as discussed below, these models replicate the approaches of the last subsection but eliminate cases that proceeded to judgment. Because there are no zeroes among the data at all, a zero-inflated approach is unnecessary, and a regular Poisson regression is appropriate.[282]

In the case of *Sutton*, both of the regressions in the Appendix point toward a massive reduction in settlement payouts.[283] While confidence levels vary from extremely low for the raw-dollar regression to extremely high for the Poisson regression, all three models agree: *Sutton* caused defendants to become more aggressive in settlement negotiations, and led the EEOC to be willing to accept lower settlements. The Poisson regression reports an incidence rate ratio of about 55%, suggesting that post-*Sutton* ADA claimants would nearly halve their recovery in settlement.

Reeves presents no significant deviations between settled and litigated cases. As with litigated cases, the Appendix reports that settlements in age discrimination cases dropped significantly after *Reeves*, although the confidence levels vary.[284] For Title VII, all

282. The only difference between a zero-inflated Poisson regression and a regular Poisson is that the zero-inflated model asks for an equation that explains the unexpected zeroes.

283. *Infra* Table A13.

284. *Infra* Table A14.

three regressions associate reductions in settlements with the legal shifts.[285] Confidence levels are fairly high for the raw-dollar and Poisson regressions—90% and 99.9%, respectively—but quite low for the log-dollar regression. Again, we have evidence that *Reeves* was perceived as a larger change than it actually effected.

Desert Palace presents a slightly different approach; because the specific theory underlying H_{12} predicts a non-linear relationship, where *Desert Palace* engenders confidence in cases that erodes over time, settlements are expected to be non-linear. We account for this by using a variable that represents the case termination date. If the coefficient for this variable is negative, but the coefficient for being post-*Desert Palace* is positive, then we can treat the hypothesis as confirmed.

The regressions in the Appendix[286] all agree: when *Desert Palace* was handed down, the EEOC and defendants read it as shifting the legal regime toward plaintiffs and thus began increasing settlements. However, as time went on and litigants gained experience with how courts were treating *Desert Palace*, that perceived shift eroded, and settlements reverted toward where they had been. Confidence levels are relatively high on the raw-dollar and Poisson regressions, and less so in the log-dollar regressions. The raw-dollar regression is 90% confident on both the first-order and second-order effects, while the log-dollar regression is 90% confident on the first-order only. The Poisson regression is 99.9% confident.

Returning to our hypotheses, H_{11} and H_{13} are strongly confirmed by the evidence; H_{12} is contradicted by it. Again, we see that the evidence of a regime shift is deeply contingent and conditional.

B. CONSEQUENCES

The results of the tests of the thirteen hypotheses tested here are summarized in the table below.

285. *Infra* Table A15.

286. *Infra* Table A16.

Type of Hypothesis	Hypothesis	Result
Dispute Generation		
	H_1 – *Sutton*	Confirmed
	H_2 – *Reeves*	Disconfirmed
	H_3 – *Desert Palace*	Disconfirmed
EEOC Selection		
	H_4 – *Sutton* (ADA	Confirmed
	H_5 – *Sutton* (Total)	Disconfirmed
	H_6 – *Reeves* (ADEA and Total)	Confirmed
Plaintiff Payouts		
	H_7 – *Sutton*	Uncertain
	H_8 – *Reeves* (ADEA)	Disconfirmed
	H_9 – *Reeves* (Total)	Disconfirmed
	H_{10} – *Desert Palace*	Regime Shift Found But Not Predicted
Settlement Payouts		
	H_{11} – *Sutton*	Confirmed
	H_{12} – *Reeves* (ADEA and Total)	Disconfirmed
	H_{13} – *Desert Palace*	Confirmed

In addition, with the exception of H_2 and H_3, all of the disconfirmed hypotheses can be explained by reference to evidence of jurisprudential regime shifts detected earlier in the litigation process. Thus, it is fair to say that there is fairly strong evidence that jurisprudential regimes theory explains a variety of litigant behavior that goes beyond the claims of proponents and the arguments of critics.

However, each EEOC charge of discrimination filed and every case pursued represents at least one person's life that has been turned upside down by allegedly unlawful actions. It is worth taking the time to think through what these results suggest not only for the theories of litigant behavior and judicial decision-making, but also for the substantive effects on the lives of people who have neither power nor privilege—the groups of people that the administrative state is designed to protect because they usually lack the resources to protect themselves.

In dispute generation, the evidence suggests that disabled people were particularly targeted and discouraged by the regime shift in *Sutton*; other claimants were less affected. As a substantive matter, the fact that *Sutton* comes into play less than ten years after the passage of the ADA had to feel like it represented a significant claw-back of rights that had been so hard-won. Given the extremity to which disability advocates put themselves to force Congress to act, to have such a broad swath of rights clawed back so quickly could only have been deeply discouraging. This is consistent with some of the findings that Harpur made in surveying disability advocates,[287] where most took a stance toward enforcing their own rights that could be best summarized as "What's the point?"

In selecting disputes for enforcement, the EEOC appears to be relatively strongly affected by regime shifts. This means that the EEOC appears to choose cases for enforcement at least in part on whether it thinks it can win. To some extent this is hardly an unexpected result, but it offers some counterargument to scholars who claim that the administrative state is inevitably and hopelessly subject to regulatory capture. Here is one example of an agency that pushes at the boundaries of its authority and, when those boundaries expand, so do its efforts. Contrariwise, when its authority contracts, it does not seek to go rogue and enforce outside its boundaries. In particular, this contradicts the argument of Hall, who finds that the only entities that reliably comply with Supreme Court decisions are lower courts that are subject to direct supervisory authority.[288]

But the agency's eagerness to pursue new grounds can have consequences. If the agency has misread the way that courts will

287. Harpur, *supra* note 223, at 1242–45.

288. MATTHEW E. K. HALL, THE NATURE OF SUPREME COURT POWER 5 (2010).

react to a regime shift, then it can lead to drastic reductions in plaintiff payouts. In the case of *Reeves*, this means that individuals who might have chosen not to pursue relief were persuaded to let the agency bring weak claims that left them with little or no relief. Pursuing a lawsuit imposes a psychological cost on a private litigant, particularly when the lawsuit is tied up with issues related to their identity and their profession. To chase that process through the months or even years that federal lawsuits can take, only to wind up with nominal relief or no relief at all, is crushing. Even if the case settles, regime shifts can lead to results that were not worth the time and energy expended. As seen with *Desert Palace*, striking when the iron is hot on a perceived regime shift can lead to a positive outcome, while waiting may complicate the picture.

All of these issues are compounded by the fact that EEOC claimants are frequently some of the least sophisticated litigants in the federal system. Thus, they are utterly dependent on the expert advice of the agency and their private counsel. When those lawyers see shifts in the legal regime, claimants are not equipped to contradict them. The power bestowed by expertise in this circumstance cannot be overstated. The responsibility that comes with that power seems to not always be responsibly exercised, either by private lawyers or by the agency itself.

CONCLUSION

The findings in this Article serve several purposes. First, they show that litigant behavior affects our ability to discern whether a particular court decision has changed the jurisprudential regime. This may help to explain the conflict between the theory's proponents and those who argue their findings are statistical artifacts. In addition, it demonstrates that changes in the legal regime *do* change the population of cases litigated, affecting our ability to make inferences about the law from case outcomes. However, it shows that the effect is not consistent, and also is contingent on other factors. This complicates the narrative from those following the Priest–Klein hypothesis, which argues in its strongest form that changes in the legal regime should cause *no change* in plaintiff win rates.

All of that said, several concerns remain. First, I am concerned about generalizability. The subset of cases under analysis here represents a very unrepresentative litigation situation. The plaintiff in these cases is an administrative agency

widely regarded as a sophisticated litigant. Courts frequently defer to the EEOC. Defendants in these cases have a broad distribution; some of them are highly sophisticated and represented by highly respected law firms, others are small businesses that may only have local, non-specialist counsel. I think it is reasonable to ask if these results can travel to other litigation contexts. Future work can replicate this analysis with a broader scope.

Second, these results are yet another example of evidence that the Priest–Klein hypothesis is more contingent and less applicable than initially thought. Thus, my findings should be considered part of the argument that law plays a role in determining litigation outcomes. Indeed, it appears that we can rely on these outcomes to let us determine whether a given court decision changes the legal regime.

However, it is reasonable to ask whether these findings are fair tests of the Priest–Klein hypothesis. Priest and Klein (and their progeny) only analyze the question of whether plaintiffs win. They do not ask questions about the amounts plaintiffs win because they usually do not have that information. Since this dataset includes settlement *amounts*, I am able to test whether the changes in the legal regime affect the amounts plaintiffs receive.

That last element leads me to conclude that this is a *better* test of litigation outcomes than whether plaintiffs win. Consider the case of the plaintiff who wins judgment in their favor, but damages of only $200. This plaintiff may have spent years in litigation, incurring costs along the way, including the emotional cost of uncertainty and the time they spend working on the case with the EEOC's attorneys. By any measure, the plaintiff who "wins" $200 cannot be treated equivalently to the plaintiff who wins tens of thousands. My analysis allows me to distinguish between nominal and actual victory, which gives us more information on the applicability of the Priest–Klein hypothesis. Since there are cases where win conditions are binary, there may still be circumstances where the hypothesis holds, but this analysis suggests that it has limited utility in situations where win conditions take a spectrum of values.

Most importantly, this analysis pushes back on the argument that courts are simply engaged in naked policymaking. My findings align my work with the analysis in Epstein, Landes, and Posner, who argued that trial courts engage in very little

ideological behavior.[289] If they did behave ideologically in these cases, we would expect there to be no difference in cases before and after changes in the legal regime.[290]

These findings also suggest that our legal institutions are more robust than many political scientists would suggest. Instead of being subject to the whims of judges, most litigation outcomes follow the law. Whether that is because of formal oversight mechanisms or informal norms is impossible to determine, but it is clear that when the law changes, the outcomes change as well.

This points me to my first suggestion for future research: investigating the causal mechanisms behind this robustness. While there is argument that lawyers and judges are acculturated into their professions, this question has not been systematically investigated.[291] There is some evidence that the oversight mechanisms of the court systems are fundamentally broken.[292] But it is less clear whether trial judges are actually freelancing in response to a lack of oversight. These findings suggest that concerns about lack of oversight may be overblown—although the findings on *Desert Palace* suggest that the story is more complicated.

In addition, while the actual decisions that judges make are important, the *perceived* causes for those decisions are also important for the legitimacy of the institutions that rule us. Work on the relationship between trial court decisions and both elite and mass perceptions of those decisions is an important component to the puzzle of the continued legitimacy of judicial institutions.

Ultimately, this piece offers important insights into the ways that courts interact with secondary populations. While the Supreme Court makes decisions that purport to instantly change the law that governs the United States, these results suggest that what they do, in at least some cases, is change the way that litigants decide to move their cases forward. Those masses of

289. EPSTEIN, LANDES & POSNER, *supra* note 51, at 216–17.

290. I do note that those null results are a *necessary* implication of ideological behavior, not a *sufficient* one. I am not testing the ideological basis for trial court decision-making in this Article.

291. *Cf.* Paul Horwitz, *Honor's Constitutional Moment: The Oath and Presidential Transitions*, 103 NW. L. REV. COLLOQUY 259, 266–69 (2008) (arguing that oaths impose particular obligations on the oath taker, such as lawyers being sworn into the bar).

292. Marin K. Levy, *Panel Assignment in the Federal Courts of Appeals*, 103 CORNELL L. REV. 65, 66, 115–16 (2017).

individual decisions, to move forward or to lay the dispute aside, affect the substantive rights of millions of Americans. Taken together, the results here suggest that the legal academy's focus on doctrinal development and normative advocacy misunderstands the relationships in the legal system. They also suggest that social scientists' focus on judicial decision-making, whether it focuses on legal or extra-legal factors, similarly leaves out an important factor. Instead, it encourages observers to consider the law as a dialogue among many participants, including litigants whose role is to consume the law that comes from courts and use it to define the boundaries of their choices. Like a bad hangover, that consumption can drag out for some time and have lots of knock-on symptoms.

For the bench and bar, an important component of this research is that it offers new levers of influence for them. The evidence in this analysis challenges the idea that the Supreme Court speaks magisterially and, thereby, American society changes. Instead, there is space for litigants, lawyers, and lower court judges to respond and resist Supreme Court efforts to shape American life. In a setting where the Court seems more insistent on its preeminence in defining the proper spheres of behavior for the American state *and* the American people,[293] that space for response and resistance becomes important. While litigants may be consuming the law that the Supreme Court produces, if that law induces a hangover, there is nothing that says that consuming it cannot lead to litigants returning it with unwanted additions and changes.

293. Nat'l Fed'n of Indep. Bus. v. Dep't of Lab. OSHA, 142 S. Ct. 661, 664–65 (2022) (per curiam) (claiming that a COVID vaccine mandate for private employers exceeded the agency's authority because it did not apply to workplace safety since COVID can be contracted at work or elsewhere).

APPENDIX

TABLE A1. REGRESSION RESULTS FOR LIKELIHOOD-RATIO TESTS

Model	Model 1 Coefficients	Model 2 Coefficients	Model 3 Coefficients	Model 4 Coefficients	Model 5 Coefficients
Judicial Ideology	-28511.5	-29205.1	-28500.9	-27607.9	-28463.3
Post-*Desert Palace*	-3616.57	7873.316	-3621.69	-2500.94	-3540.19
Post-*Sutton*	50534.2	53318.2	50536.77	45555.83	49317.54
Post-*Reeves*	-153530	-152816	-153517	-156326	-155644
Title VII	-49705.1	159365.4	-50082	90565.93	
ADA	-184774	17930.57	-185132		-139553
ADEA	565.0646	145601.7		93957.59	32273.22
Equal Pay Act	125646.3		125838.4	55618.75	102361.5
Constant	101190.9	121803	101197.7	100741.4	101309.6

TABLE A2. LIKELIHOOD-RATIO PAIRWISE RESULTS

Likelihood-Ratio Test Specification	Chi-square (X^2-statistic):	Probability test (likelihood of randomness)
Equal Pay Act (Model 2 compared to Model 1)	3.29	0.0698
ADEA (Model 3 compared to Model 1)	0.00	0.9965
ADA (Model 4 compared to Model 1)	1.67	0.1960
Title VII (Model 5 compared to Model 1)	0.15	0.7032

TABLE A3. ADA SHARE OF EEOC LITIGATION, PRE- AND POST-*SUTTON*, WITH PROBABILITY TEST

	Was case terminated post-*Sutton*?		
Does case assert an ADA claim?	No	Yes	Total
No	144	587	731
Yes	20	58	78
Total	164	645	809

Chi-square (X^2-statistic): 1.5396; Probability: 0.215

TABLE A4. CONTROL VARIABLES FOR PAYOUT AND SETTLEMENTS REGRESSIONS

Control Variable Name	Description
Judicial Common Space	The "gold standard" of measuring judicial ideology; controls for judge-driven effects.
Retaliation Allegation	1 if the complaint contained an allegation of retaliation; 0 elsewise. Controls for effects caused by differential proof systems in retaliation claims.
Disparate Impact Allegation	1 if the complaint advanced a disparate impact theory; 0 elsewise. Controls for effects caused by no requirement for proof of animus in disparate impact claims.
Pattern & Practice Allegation	1 if the complaint advanced an allegation of pattern and practice discrimination; 0 elsewise. Controls for effects of pattern and practice discrimination, which usually involves company-wide policy and many claimants.
Non-Pecuniary Relief Demanded	1 if complaint demands non-pecuniary relief; 0 elsewise. Controls for effects of cases where plaintiff must prove "emotional damages" or other non-economic claims.
Punitive Relief Demanded	1 if complaint demands punitive damages; 0 elsewise. Controls for effects of punitive damages, which can multiply recovery significantly.
Number of Complainants	Count of the number of complainants the EEOC represents. Controls for larger cases.

TABLE A5. BAYESIAN CHANGE-POINT MODEL ANALYSIS, CHARGE FILING[294]

Variable	*Sutton* Bayesian model	*Reeves* Bayesian model	*Desert Palace* Bayesian model
Clinton FE	.5965234	-.8709979	7633.981
Bush FE	-.1529779	.0188012	18869.29
Obama FE	-.0805006	.0207623	29917.81
Trump FE	-.0209373	.0042404	4957.645
Change-point mean (in Fiscal Years)	**2001.793**	**2000.402**	**2003.922**

TABLE A6. PANEL REGRESSION RESULTS, CHARGE RESOLUTION DATA; STANDARD ERRORS IN PARENTHESES; *** P<0.01, ** P<0.05, * P<0.1

	(1)	(4)	(7)	(10)	(13)
VARIABLES	*Sutton* ADA Merit Resolutions (H_4)	*Sutton* ADA Right to Sue (H_4)	*Reeves* ADEA Merit Resolutions (H_6)	*Gross* ADEA Merit Resolutions (H_6)	*Reeves*/*Gross* ADEA Merit Resolutions (H_6)
Fiscal Year	233.1***	280.1***	-7.945	38.71	-79.73
	(47.88)	(39.49)	(71.88)	(144.6)	(190.6)
Sutton	-2,105***	-4,233***			
	(726.5)	(599.2)			
Total Receipts	0.101***	0.109***	0.172***	0.183***	0.149**
	(0.0304)	(0.0251)	(0.0415)	(0.0598)	(0.0720)
Reeves			1,588		1,829
			(1,438)		(1,681)
Gross				94.04	1,081
				(2,266)	(2,614)
Constant	-463,136***	-552,828***	11,065	-82,342	156,354
	(96,927)	(79,843)	(144,534)	(292,852)	(385,302)
Observations	50	50	50	50	50

294. The change-point mean is the only meaningful result in this Table. The rest of the coefficients are provided only for completeness' sake.

TABLE A7. LOGISTIC REGRESSION *SUTTON* RESULTS, FJC DOCKET SHARE DATA; STANDARD ERRORS IN PARENTHESES; *** P<0.01, ** P<0.05, * P<0.1

VARIABLES	(1) FJC Litigation Share Data
Sutton in effect	-0.590***
	(0.193)
Constant	-4.459***
	(0.792)
Observations	28,762
District Court FE	YES
Disposition FC	YES
Marginal Effect *Sutton*	-.0061439***

TABLE A8. PANEL REGRESSION, EFFECT ON TOTAL RESOLUTIONS; STANDARD ERRORS IN PARENTHESES; *** P<0.01, ** P<0.05, * P<0.1

VARIABLES	*Sutton*/Total Charges	*Reeves*/Total Charges
Fiscal Year	-140.8	-326.2*
	(170.6)	(181.2)
Sutton	-1,498	
	(2,563)	
Reeves		7,346**
		(3,565)
Constant	333,291	699,174*
	(344,814)	(363,733)
Observations	50	50
District Court FE		
Disposition FC		
Number of Type Of Interest	2	2

TABLE A9. PAYOUTS REGRESSION RESULTS, *SUTTON*; STANDARD ERRORS IN PARENTHESES; *** P<0.01, ** P<0.05, * P<0.1

	(1)	(3)	(5)
VARIABLES	Raw-Dollar *Sutton*	Log-Dollar *Sutton*	Poisson *Sutton*
Judicial Ideology	-65,681	-5.506*	0.271***
	(58,696)	(2.920)	(0.00159)
Post-*Sutton*	43,596	1.830	-0.0365***
	(85,546)	(4.217)	(0.00246)
Retaliation Alleged	-6,229	0.879	-0.0211***
	(46,426)	(2.257)	(0.00130)
Disparate Impact Alleged	151,819*	-0.230	0.873***
	(81,099)	(4.049)	(0.00175)
Pattern & Practice Alleged	-168,878**	-2.450	-0.935***
	(68,076)	(3.425)	(0.00129)
Non-Pecuniary Relief Demanded	-211,817	-7.166	-0.693***
	(130,734)	(6.585)	(0.00250)
Punitive Relief Demanded	207,493*	4.434	1.136***
	(120,548)	(6.078)	(0.00216)
Number Complainants	25,187***	0.622*	0.0870***
	(6,900)	(0.347)	(0.000123)
Constant	4,386	5.742	8.041***
	(333,593)	(16.39)	(0.0163)
Observations	77	77	77
Circuit FE	YES	YES	YES

TABLE A10. PAYOUT REGRESSION RESULTS OF *REEVES* ON ADEA CASES; STANDARD ERRORS IN PARENTHESES; *** P<0.01, ** P<0.05, * P<0.1

	(1)	(3)	(5)
VARIABLES	Raw-Dollar *Reeves* ADEA	Log-Dollar *Reeves* ADEA	Poisson *Reeves* ADEA
Judicial Ideology	169,495	0.530	1.005***
	(184,475)	(3.075)	(0.00175)
Post-*Reeves*	-194,113	-4.623	-0.194***
	(189,163)	(3.088)	(0.00180)
Retaliation Alleged	158,834*	2.549	0.639***
	(91,750)	(1.540)	(0.00110)
Disparate Impact Alleged	-114,766	-1.384	-0.103***
	(216,199)	(3.542)	(0.00239)
Pattern & Practice Alleged	74,202	-0.305	-0.391***
	(215,724)	(3.505)	(0.00232)
Non-Pecuniary Relief Demanded	-55,912	-0.700	-0.205***
	(101,596)	(1.701)	(0.00106)
Number Complainants	41,330***	0.120	0.0632***
	(5,555)	(0.0947)	(5.59e-05)
Constant	-381,778	-4.564	7.839***
	(569,180)	(9.585)	(0.00740)
Observations	44	44	44
Circuit FE	YES	YES	YES

TABLE A11. *REEVES* PAYOUT REGRESSION RESULTS ON ALL CASES; STANDARD ERRORS IN PARENTHESES; *** P<0.01, ** P<0.05, * P<0.1

	(1)	(3)	(5)
VARIABLES	Raw-Dollar *Reeves* Total	Log-Dollar *Reeves* Total	Poisson *Reeves* Total
Judicial Ideology	47,919	0.0745	0.131***
	(112,963)	(0.944)	(0.000341)
Post-*Reeves*	-277,900*	-1.378	-0.258***
	(142,810)	(1.204)	(0.000329)
Retaliation Alleged	-140,147***	-0.131	-0.385***
	(40,798)	(0.341)	(9.66e-05)
Disparate Impact Alleged	382,030***	0.641	1.222***
	(85,648)	(0.710)	(0.000183)
Pattern & Practice Alleged	-198,290**	-0.418	-0.693***
	(86,704)	(0.721)	(0.000191)
Non-Pecuniary Relief Demanded	28,522	-1.742	1.177***
	(209,631)	(1.727)	(0.000593)
Punitive Relief Demanded	-8,561	2.056	-1.309***
	(202,296)	(1.675)	(0.000577)
Number Complainants	40,663***	0.181*	0.0756***
	(11,943)	(0.0993)	(2.34e-05)
Constant	2.546e+06***	5.176	15.27***
	(440,032)	(3.797)	(0.000689)
Observations	510	510	510
Circuit FE	YES	YES	YES

TABLE A12. *DESERT PALACE* PAYOUT REGRESSION RESULTS; STANDARD ERRORS IN PARENTHESES; *** P<0.01, ** P<0.05, * P<0.1

	(1)	(3)	(5)
VARIABLES	Raw-Dollar *Desert Palace*	Log-Dollar *Desert Palace*	Poisson *Desert Palace*
Judicial Ideology	-52,760	-1.548*	0.108***
	(93,851)	(0.933)	(0.000318)
Post-*Desert Palace*	35,930	1.177	0.102***
	(75,764)	(0.754)	(0.000239)
Retaliation Alleged	-174,840***	-0.746**	-0.445***
	(36,073)	(0.360)	(8.55e-05)
Disparate Impacted Alleged	384,334***	0.864	1.208***
	(77,482)	(0.770)	(0.000175)
Pattern & Practice Alleged	-225,844***	-1.031	-0.655***
	(77,398)	(0.771)	(0.000180)
Non-Pecuniary Relief Demanded	-17,790	-2.564	1.236***
	(185,576)	(1.835)	(0.000577)
Punitive Relief Demanded	-41,564	1.827	-1.399***
	(177,755)	(1.765)	(0.000551)
Number Complainants	45,011***	0.230***	0.0483***
	(7,657)	(0.0771)	(9.73e-06)
Constant	2.327e+06***	8.769**	15.70***
	(370,766)	(3.807)	(0.000564)
Observations	666	666	666
Circuit FE	YES	YES	YES

TABLE A13. *SUTTON* REGRESSION RESULTS ON SETTLED ADA CASES; STANDARD ERRORS IN PARENTHESES; *** P<0.01, ** P<0.05, * P<0.1

	(1)	(3)	(5)
VARIABLES	Raw-Dollar *Sutton* Settlements	Log-Dollar *Sutton* Settlements	Poisson *Sutton* Settlements
Judicial Ideology	16,922	-1.132	0.0560***
	(60,876)	(2.001)	(0.00173)
Post-*Sutton*	-29,541	-1.869	-0.591***
	(85,248)	(2.805)	(0.00246)
Retaliation Alleged	-32,637	-1.756	-0.280***
	(48,505)	(1.605)	(0.00129)
Disparate Impact Alleged	201,421**	2.824	1.471***
	(74,130)	(2.451)	(0.00186)
Pattern & Practice Alleged	-179,918***	-1.902	-1.276***
	(59,061)	(1.944)	(0.00128)
Non-Pecuniary Relief Demanded	-130,975	-0.550	-0.412***
	(115,437)	(3.823)	(0.00263)
Punitive Relief Demanded	162,566	0.247	1.034***
	(104,427)	(3.463)	(0.00220)
Number Complainants	21,054***	0.294	0.101***
	(5,948)	(0.196)	(0.000109)
Constant	170,897	21.39*	10.04***
	(332,326)	(10.99)	(0.0164)
Observations	54	54	54
Circuit FE	YES	YES	YES

TABLE A14. *REEVES* REGRESSION RESULTS ON SETTLEMENTS IN ADEA CASES; STANDARD ERRORS IN PARENTHESES; *** P<0.01, ** P<0.05, * P<0.1

	(1)	(3)	(5)
VARIABLES	Raw-Dollar *Reeves* ADEA Settlements	Log-Dollar *Reeves* ADEA Settlements	Poisson *Reeves* ADEA Settlements
Judicial Ideology	137,542	-0.184	0.973***
	(183,840)	(1.932)	(0.00174)
Post-*Reeves*	-163,282	-3.103	-0.232***
	(183,635)	(1.881)	(0.00178)
Retaliation Alleged	107,844	0.822	0.648***
	(94,687)	(1.017)	(0.00111)
Disparate Impact Alleged	-22,312	2.301	-0.0854***
	(214,731)	(2.202)	(0.00239)
Pattern & Practice Alleged	30,401	-2.389	-0.423***
	(214,468)	(2.175)	(0.00229)
Non-Pecuniary Relief Demanded	-35,168	0.00902	-0.234***
	(98,912)	(1.040)	(0.00106)
Number Complainants	38,532***	0.0589	0.0647***
	(5,690)	(0.0612)	(5.81e-05)
Constant	102,955	8.943	7.755***
	(614,774)	(6.610)	(0.00746)
Observations	40	40	40
Circuit FE	YES	YES	YES

TABLE A15. *REEVES* REGRESSION RESULTS ON SETTLEMENTS IN TITLE VII CASES; STANDARD ERRORS IN PARENTHESES; *** P<0.01, ** P<0.05, * P<0.1

	(1)	(3)	(5)
VARIABLES	Raw-Dollar *Reeves* Title VII Settlements	Log-Dollar *Reeves* Title VII Settlements	Poisson *Reeves* Title VII Settlements
Judicial Ideology	89,207	0.242	0.315***
	(116,772)	(0.806)	(0.000356)
Post-*Reeves*	-260,256*	-0.124	-0.449***
	(150,502)	(1.039)	(0.000344)
Retaliation Alleged	-146,197***	-0.250	-0.392***
	(41,357)	(0.284)	(9.92e-05)
Disparate Impact Alleged	393,333***	0.394	1.248***
	(87,345)	(0.594)	(0.000179)
Pattern & Practice Alleged	-209,066**	-0.110	-0.723***
	(88,465)	(0.604)	(0.000186)
Non-Pecuniary Relief Demanded	87,982	-0.281	0.754***
	(241,473)	(1.667)	(0.000682)
Punitive Relief Demanded	-67,700	0.792	-0.923***
	(225,655)	(1.558)	(0.000654)
Number Complainants	40,603***	0.125	0.0828***
	(12,265)	(0.0836)	(2.39e-05)
Constant	3.065e+06***	7.770**	15.14***
	(467,369)	(3.277)	(0.000693)
Observations	440	440	440
Circuit FE	YES	YES	YES

TABLE A16. *DESERT PALACE* REGRESSION RESULTS ON SETTLEMENTS; STANDARD ERRORS IN PARENTHESES; *** P<0.01, ** P<0.05, * P<0.1

	(1)	(3)	(5)
VARIABLES	Raw-Dollar *Desert Palace* Settlements	Log-Dollar *Desert Palace* Settlements	Poisson *Desert Palace* Settlements
Judicial Ideology	33,098	-0.580	0.309***
	(98,418)	(0.799)	(0.000341)
Post-*Desert Palace*	217,348*	1.808*	0.969***
	(130,835)	(1.060)	(0.000454)
Date of Final Resolution	-119.9*	-0.000374	-0.000494***
	(68.01)	(0.000549)	(2.23e-07)
Retaliation Alleged	-175,145***	-0.662**	-0.476***
	(36,663)	(0.298)	(8.70e-05)
Disparate Impact Alleged	396,649***	0.614	1.227***
	(78,720)	(0.632)	(0.000170)
Pattern & Practice Alleged	-246,703***	-0.807	-0.735***
	(79,214)	(0.638)	(0.000180)
Non-Pecuniary Relief Demanded	40,424	-1.043	0.772***
	(207,525)	(1.696)	(0.000610)
Punitive Relief Demanded	-71,740	0.824	-0.920***
	(194,695)	(1.592)	(0.000576)
Number Complainants	44,040***	0.157**	0.0626***
	(7,700)	(0.0628)	(1.06e-05)
Constant	4.487e+06***	16.29*	22.95***
	(1.101e+06)	(8.903)	(0.00331)
Observations	560	560	560
Circuit FE	YES	YES	YES