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## DO JUDGES CHERRY PICK PRECEDENTS TO JUSTIFY EXTRA-LEGAL DECISIONS?: A STATISTICAL EXAMINATION

ANTHONY NIBLETT\*

### I. INTRODUCTION

How important are legal precedents? How much discretion do judges have to apply or to ignore precedent? Do judges cite precedents merely to rationalize, post hoc, a decision that was driven by extra-legal considerations? Such questions have long been debated theoretically. Legal realists and legal skeptics have contended that judicial decisions may simply be made on a “hunch.”<sup>1</sup> In their models of judicial behavior, judges have biases and personal preferences that influence the outcomes of cases. Later, when writing opinions, judges may selectively use precedent, possibly manipulating the existing case law to provide precedential and often rhetorical authority for the decision.<sup>2</sup> The legal realist and legal skeptic theories therefore suggest

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1. E.g., Joseph C. Hutcheson, Jr., *The Judgment Intuitive: The Function of the “Hunch” in Judicial Decision*, 14 CORNELL L.Q. 274, 287–88 (1929) (contending that judicial decisions often rest on hunches and suggesting that “whether or not the judge is able in his opinion to present reasons for his hunch which will pass jural muster, he does and should decide difficult and complicated cases only when he has the feeling of the decision”). Joseph Chappell Hutcheson Jr. was a prominent federal judge at the time he wrote this piece. See *id.* at 277–78 (explaining the author’s experiences “after eleven years on the Bench following eighteen at the Bar”). For a discussion that uses the “attitudinal theory” as a starting place for explaining “what judges do when they are not just applying rules . . . to develop a positive decision-theoretic account of judicial behavior” in “the open area . . . in which a judge is a legislator,” see RICHARD A. POSNER, HOW JUDGES THINK 1–15, 19–31 (2008); for a discussion of the history and philosophy of the realism movement that concentrates on adjudication as “a powerful and coherent theoretical view” and acknowledges the “largely neglected, but substantial, contributions” made to the philosophy of law and legal realism, see Brian Leiter, *Legal Realism*, in A COMPANION TO PHILOSOPHY OF LAW AND LEGAL THEORY 261, 261 (Dennis Patterson ed., 1996); for a recent treatment on the economics of legal realism that develops “a perspective on judging that can usefully be understood as the modern manifestation of American Legal Realism” and maps out how “a more explicit integration of the Realists’ conceptual insights about law and judicial behavior might enrich the rapidly expanding economic work in this field,” see generally Matthew C. Stephenson, *Legal Realism for Economists*, 23 J. ECON. PERSP. 191 (2009).

2. See, e.g., LAURA KALMAN, THE STRANGE CAREER OF LEGAL LIBERALISM 46 (1996) (discussing Justice Abe Fortas’s practice of writing draft opinions for the United States Supreme Court without citations and asking his law clerks to “decorate” them with the necessary legal support).

that judges simply cite those precedents that best suit their story. This hypothesis is tested in this Article.

I ask: Do judges simply cherry pick precedents to justify an extra-legal decision? In a sample of California appeals cases examining unconscionable contract provisions the answer appears to be no. This finding suggests that legal realists and legal skeptics have overstated the degree to which precedent can be manipulated and have exaggerated the degree to which judges can misuse the discretion afforded them.

The question posed in this Article is not a new one. The approach taken to investigating this problem, however, is novel. I empirically examine the citation behavior of judges by looking at a pool of legal opinions deciding one extremely narrow issue: the unconscionability of arbitration clauses in standard-form contracts. Within this pool, I compare cited precedents to precedents that are ignored in the written opinion.

In the sphere of commercial law that I have chosen to investigate, the outcomes of cases correlate with the perceived political preferences of the judge who wrote the opinion in the case at hand.<sup>3</sup> This finding suggests the possibility of extra-legal decision making. The data, however, uncover no evidence of judges cherry picking precedents in those decisions where extra-legal behavior appears most likely. Put simply, for those cases that are at the margin—where the outcome might be explained in terms of political preference—the judges do not cite significantly more precedents with the same outcome.<sup>4</sup> The decisions of Republican judges in the sample tend to favor defendants, but when Republican judges decide in favor of defendants, they cite pro-defendant precedents just as frequently as Democratic judges who decide for defendants. Similarly, Democratic judges tend to favor plaintiffs in the sample, but they cite pro-plaintiff precedents with the same frequency as Republican judges when writing pro-plaintiff decisions.

Empirically analyzing citation behavior provides valuable insight into judicial discretion. Legal realists such as Jerome Frank,<sup>5</sup> Oliver

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3. See *infra* Part III.B.3.

4. See *infra* Part V.

5. See, e.g., JEROME FRANK, LAW AND THE MODERN MIND 138 (1930) [hereinafter FRANK, LAW AND THE MODERN MIND] (“For try as men will to avoid it, judging involves discretion and individualization.”); Jerome Frank, *Are Judges Human? Part One: The Effect on Legal Thinking of the Assumption That Judges Behave Like Human Beings*, 80 U. PA. L. REV. 17, 29 (1931) (“One who patiently observes will learn the unguessability of even upper court decisions and perceive that they are often functions of the chance composition of the bench.”); Jerome Frank, *Are Judges Human? Part Two: As Through a Class Darkly*, 80 U. PA. L. REV. 233,

Wendell Holmes,<sup>6</sup> and Karl Llewellyn<sup>7</sup> have contended that judges are afforded a great deal of discretion that may generate imprecision or indeterminacy of law.<sup>8</sup> This indeterminacy, especially in commercial law, generates transaction costs and imposes costs for the administration of the legal system.<sup>9</sup> Judicial discretion can manifest itself in many ways. For example, a court may disagree with the outcome that a bright-line rule generates in a particular case and use its discretion to introduce exceptions,<sup>10</sup> or a court may apply standards inconsistently with precedent.<sup>11</sup> Judges apply the relevant law to the facts of a

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235 (1931) [hereinafter Frank, *Are Judges Human? Part Two*] (“In sum, there is in actual practice no possibility of delimiting the uncertainty of future decisions . . . . All future lawsuits are gambles.”).

6. See, e.g., O. W. HOLMES, *The Path of the Law*, 10 HARV. L. REV. 457, 457, 465–66 (1897) (suggesting that the object of the study of law is “prediction,” criticizing the “fallacy” that legal decisions can be determined with mathematical precision and arguing that “[b]ehind the logical form lies a judgment as to the relative worth and importance of competing legislative grounds,” which is “the very root and nerve of the whole proceeding”).

7. See, e.g., K. N. LLEWELLYN, *THE BRAMBLE BUSH* 12–13 (4th prt. 1973) (rejecting the common notion that law is “a set of rules of conduct” and suggesting that law is actually what officials “do about disputes” (emphasis omitted)). See generally Ugo Mattei, *The Rise and Fall of Law and Economics: An Essay for Judge Guido Calabresi*, 64 MD. L. REV. 220, 230 (2005) (explaining that Llewellyn wanted to explore “‘legal life’” outside of libraries (quoting LAURA NADER, *THE LIFE OF THE LAW: ANTHROPOLOGICAL PROJECTS* 85–101 (2002))).

8. See FRANK, *LAW AND THE MODERN MIND*, *supra* note 5, at 138–39 (suggesting that judges have a great amount of discretion “in determining what is the law of the case”).

9. Moreover, these inconsistencies and the idiosyncratic use of exceptions cast doubt on the hypothesis that the common law converges to efficient rules. See, e.g., RICHARD A. POSNER, *ECONOMIC ANALYSIS OF LAW* 249–53 (7th ed. 2007) (explaining that “common law doctrines create incentives for people to channel their transactions through the market” and “to behave efficiently”); Nicola Gennaioli & Andrei Shleifer, *The Evolution of the Common Law*, 115 J. POL. ECON. 43, 46 (2007) (arguing that although judicial polarization “distort[s] the law away from efficiency,” it is nevertheless beneficial to the extent that it “may lead to better law”); Giacomo A. M. Ponzetto & Patricio A. Fernandez, *Case Law Versus Statute Law: An Evolutionary Comparison*, 37 J. LEGAL STUD. 379, 381–82 (2008) (stating that “case law is a continuous, never-ending process of evolution of legal rules that is characterized by probabilistic convergence toward greater efficiency and predictability” and concluding that convergence is “hindered, but never eliminated,” by judicial bias in decision making that results in “extremism and uncertainty”). Further, laws that are settled and predictable will be litigated less often than laws that are unsettled or inconsistent.

10. See, e.g., Anthony Niblett, Richard A. Posner & Andrei Shleifer, *The Evolution of a Legal Rule*, 39 J. LEGAL STUD. (forthcoming 2010) (on file with the Maryland Law Review) (analyzing the evolution of the economic loss rule, concluding that “appellate courts exercise a significant amount of discretion in deciding cases,” and rejecting “the hypothesis that, in commercial fields, the common law is predictable and efficient, or at least is moving there”).

11. See Anthony Niblett, *Tracking Inconsistent Judicial Behavior*, 1, 2–5, 24–27 (July 2010) (unpublished manuscript) (on file with the Maryland Law Review), available at [http://papers.ssrn.com/sol3/papers.cfm?abstract\\_id=1434685](http://papers.ssrn.com/sol3/papers.cfm?abstract_id=1434685) (using empirical analysis to show that California appellate courts inconsistently enforce arbitration provisions and sug-

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case. Judges not only have discretion over which facts to highlight,<sup>12</sup> but they also have discretion over which aspects of the relevant law to highlight.<sup>13</sup> Commercial areas of law—such as contracts, torts, and property—are largely governed by common law, and judges have discretion over the selection of precedents to be cited in the opinion.<sup>14</sup> If judges can use their discretion to justify extra-legal decisions after the fact, then the evolution of commercial law can be strongly influenced to reflect these biases even though it appears that decisions are consistent with precedent.

The existing empirical literature on citations of precedent has not addressed the specific question posed in this Article. The prior literature that examines judicial citations divides somewhat cleanly into two camps.<sup>15</sup> Generally, these theories suggest that the precedents cited are influential and inform the outcomes of cases. The first camp uses citations as a way to measure this influence, allowing researchers to test which decisions, which judges, which circuits, and which schools of thought have been most “influential.”<sup>16</sup>

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gesting that “the primary source of this inconsistency is the political ideology of each of the benches hearing cases”).

12. See Nicola Gennaioli & Andrei Shleifer, *Judicial Fact Discretion*, 37 J. LEGAL STUD. 1, 2–4 (2008) (identifying judicial bias and the reluctance of trial courts to be overruled on appeal as two distinct motives for the exercise of judicial fact discretion).

13. See FRANK, LAW AND THE MODERN MIND, *supra* note 5, at 138–39 (“The judge, in determining what is the law of the case, must choose and select, and it is virtually impossible to delimit the range of his choice and selection.”). R

14. Frank, *Are Judges Human? Part Two*, *supra* note 5, at 235 (“[T]here is in actual practice no possibility of delimiting the uncertainty of future decisions, and of saying that in property or commercial contract cases future decisions . . . are nicely predictable . . .”). R

15. I limit my discussion of the literature specifically to articles that examine judicial citations. But there is also literature on the economics of citation practices within academia that is not examined here. See generally, e.g., David N. Laband, *Article Popularity*, 24 ECON. INQUIRY 173 (1986) (concluding that “a select few economists may exert a dominant influence on advances in economic theory”); David N. Laband & Robert D. Tollison, *Intellectual Collaboration*, 108 J. POL. ECON. 632 (2000) (comparing formal academic co-authorship by economists with that of biologists and investigating the economic value of informal colleague comments to authors); George J. Stigler & Claire Friedland, *The Citation Practices of Doctorates in Economics*, 83 J. POL. ECON. 477 (1975) (applying an empirical analysis of the use of precedents among economic doctoral students and leading economists to consider the influence of choice of school).

16. See, e.g., RICHARD A. POSNER, *CARDOZO: A STUDY IN REPUTATION* 72–91 (1990) (suggesting that citation studies may be used to measure judicial reputation and quantitatively analyzing Justice Cardozo’s reputation through such a study); Mita Bhattacharya & Russell Smyth, *The Determinants of Judicial Prestige and Influence: Some Empirical Evidence from the High Court of Australia*, 30 J. LEGAL STUD. 223 (2001) (expanding on the existing literature and using judicial citation practice to investigate the determinants of judicial influence outside of North America and on Australia’s highest court); Gregory A. Caldeira, *On the Reputation of State Supreme Courts*, 5 POL. BEHAV. 83, 83–84, 104–05 (1983) (performing citation analysis on a selection of state supreme court cases to develop a general measure of judicial

The second camp assumes that citations reflect the biases of the judge writing the opinion.<sup>17</sup> These biases cast doubt on the influence and quality of the cited precedent.<sup>18</sup> Political bias, gender bias, and school bias are just some of the various types of biases that have been explored in the literature.<sup>19</sup> A number of studies uncover political bias, many of which examine citations in the aggregate instead of case-by-case.<sup>20</sup> A recent study by Professors Stephen Choi and Mitu Gulati uses opinion-level data to show that federal appellate judges are more likely to cite outside-circuit decisions written by judges who were appointed by the same political party.<sup>21</sup> This bias is stronger in “high

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reputation and concluding that “social diversity, judicial professionalism, and political ideology, in this order, constitute the most crucial determinants of [judicial] prestige”); Stephen J. Choi & G. Mitu Gulati, *Choosing the Next Supreme Court Justice: An Empirical Ranking of Judge Performance*, 78 S. CAL. L. REV. 23 (2004) (placing federal courts of appeals judges from 1998 through 2000 in a judicial “tournament” to empirically and objectively measure the merit of those judges and focusing on productivity, opinion quality, and judicial independence as criteria for evaluating merit); Russell Smyth & Mita Bhattacharya, *What Determines Judicial Prestige? An Empirical Analysis for Judges of the Federal Court of Australia*, 5 AM. L. & ECON. REV. 233 (2003) (examining judicial citations to analyze the determinants of judicial prestige in the Federal Court of Australia, an intermediate appellate court). Further, citations have been used to measure the influence of particular schools of thought. See generally William M. Landes & Richard A. Posner, *The Influence of Economics on Law: A Quantitative Study*, 36 J.L. & ECON. 385 (1993) (using citation analysis to quantitatively investigate the influence of economics on the law).

17. See generally, e.g., William M. Landes et al., *Judicial Influence: A Citation Analysis of Federal Courts of Appeals Judges*, 27 J. LEGAL STUD. 271, 319–25 (1998) (exploring, among other factors, the racial, gender, and school biases of judges).

18. See generally Steven Goldberg, *Federal Judges and the Heisman Trophy*, 32 FLA. ST. U. L. REV. 1237 (2005) (using “essentially anecdotal” evidence to argue against Choi and Gulati’s suggestion that successful lower federal judges make the best Supreme Court Justices); James D. Gordon III, *Cardozo’s Baseball Card*, 44 STAN. L. REV. 899, 903, 908 (1992) (reviewing POSNER, *supra* note 16) (criticizing Judge Posner’s method of evaluating judicial reputation in *Cardozo: A Study in Reputation* as “shallow empiricism” and suggesting that “[g]iven the limitations of the scientific methodology, it will be both surprising and lamentable if large numbers of judicial bibliometric studies begin to appear”).

19. See generally, e.g., David Klein & Darby Morrisroe, *The Prestige and Influence of Individual Judges on the U.S. Courts of Appeals*, 28 J. LEGAL STUD. 371, 387–91 (1999) (suggesting that judicial prestige can translate into judicial influence); Landes et al., *supra* note 17, at 271–76, 318–25 (employing a “human capital approach” to measure the influence of individual judges through empirical analysis of citations to published opinions and other identifying characteristics such as self-citations, law school quality, prior judicial experience, ABA rankings, and political affiliation).

20. See, e.g., Stephen J. Choi & G. Mitu Gulati, *Bias in Judicial Citations: A Window into the Behavior of Judges?*, 37 J. LEGAL STUD. 87, 89, 91 (2008) (finding that a judge’s political party “is correlated with the willingness of a judge to make outside-circuit citations” and noting that “[m]ost studies looking at citations focus on the aggregate numbers of citations a judge receives and do not look on a case-by-case basis at how a judge decides when to cite another opinion”).

21. *Id.* at 91, 118–21.

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stakes” subject matters that receive greater attention.<sup>22</sup> While bias may appear to diminish the proxy value of using citations as a measure of objective influence, it is unclear whether this bias is different than biases found in voting behavior.<sup>23</sup>

An assumption that underpins the existing literature in both camps is that the precedent “informs” the legal decision and, by extension, the outcome.<sup>24</sup> This Article explicitly tests the reverse causal chain: Does the outcome of a case inform which precedents are cited? I go a step further than simply asking whether judges are biased when selecting precedents. I ask whether citations data have *any* value or *any* import in determining the direction of the law.

This unique testing of the causal chain is this Article’s main contribution to the literature. The test works in the following way. I expect that pro-plaintiff decisions are more likely to cite pro-plaintiff precedents and pro-defendant decisions are more likely to cite pro-defendant precedents. This is indeed what the data reveal.<sup>25</sup> But positive correlation between the outcomes in cases and cited precedents cannot alone address the question of causation because this positive correlation is consistent with the legal realists’ story of *ex post* justification *and* the standard story of influential precedents.<sup>26</sup>

How, then, can causation be parsed out from correlation? The methodology, detailed in Part V, exploits both that voting behavior in

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22. *See id.* at 91, 95–96, 102–10, 119, 123. High stakes subject matter cases include individual rights and campaign finance; lower stakes cases include bankruptcy or taxation. *Id.*

23. Most of the empirical examinations of politics affecting judicial behavior focus on voting behavior rather than citation practices. *See generally, e.g.*, Thomas J. Miles & Cass R. Sunstein, *The New Legal Realism*, 75 U. CHI. L. REV. 831, 836–41 (2008) (exploring past New Legal Realism literature focused on judicial voting patterns). In addition, very little attention has been given to uncovering biases in so-called “mundane,” less putatively political areas of law. *See generally, e.g.*, Nancy Staudt et al., *The Ideological Component of Judging in the Taxation Context*, 84 WASH. U. L. REV. 1797, 1797–1800 (2006) (noting a lack of scholarship on the reasoning underlying judicial decisions in the business and financial contexts, filling in the gap by conducting an empirical analysis of every United States Supreme Court case from 1940 to 2005 decided under the Internal Revenue Code, and concluding that liberal judges are more likely to vote with the government while conservative judges tend to favor corporate taxpayers).

24. *See, e.g.*, Landes et al., *supra* note 17, at 271 (“A citation to an opinion of Judge X reflects either the precedential value of that opinion or its ability to influence the decision of another judge in a subsequent case.”).

25. *See infra* Parts IV.A–B, VI.C.

26. In the language of a statistician or econometrician, the outcome of a precedent is an “endogenous regressor” in the explanation of whether a precedent is cited or not. An endogenous regressor simply means that changes in the left-hand side variable may cause changes in the right-hand side variable. For a more thorough explanation of endogenous regressors, see JAMES H. STOCK & MARK W. WATSON, *INTRODUCTION TO ECONOMETRICS* 343–44 (2003).

the chosen sphere correlates with the politics of judges and that politics can be used as a predictor of the outcome of a case. The following two-part hypothesis will be tested: (1) The politics of the judge informs the decision, and (2) the extra-legal component of the decision—that is, the element of the outcome predicted by politics—is positively correlated with the citation of precedents with the same outcome. Ultimately, no evidence for this hypothesis is uncovered.<sup>27</sup>

The hypothesis rests on the presumption that if a judge wishes to illustrate that the weight of authority agrees with her extra-legal decision, she will furnish her written opinion with precedents that support her view. Therefore, precedents are more likely to have the same outcome as the case at hand. I expect that a pro-plaintiff decision reached on a hunch should be supported by more pro-plaintiff precedents. An alternative hypothesis suggests that a judge seeking to justify an extra-legal decision might distinguish precedents that stand in opposition to her decision.<sup>28</sup>

The novelty of this approach extends beyond the explicit test of causation. The methodology employed also differs from previous studies of citation behavior. Previous literature largely fails to take into account the pool of previous precedents available to a court at the time of a decision.<sup>29</sup> This study, instead, seeks to capture the pool of *all* possible precedents<sup>30</sup> by focusing on a single, narrow legal ques-

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27. See *infra* Parts V, VI.C.

28. See Frank B. Cross et al., *Citations in the U.S. Supreme Court: An Empirical Study of Their Use and Significance*, 2010 U. ILL. L. REV. 489, 501 (discussing how judges can distinguish, manipulate, and loosely follow precedents). Alternatively, factors other than outcomes may be a better indicator that judges are *ex post* rationalizing their decision. Judges may, for instance, cite precedents written by influential judges when rationalizing a decision. Cf. Daniel A. Farber, *Supreme Court Selection and Measures of Past Judicial Performance*, 32 FLA. ST. L. REV. 1175, 1183 n.29 (2005) (“[I]nfluential judges might sometimes be cited when they merely follow existing circuit precedent, rather than making new law.”). The data in this study, however, do not address these claims specifically.

29. For an exemplary couple of articles that deal with citations within one issue by analyzing the presence of citations to “outside” precedents and largely ignoring the pool of precedent cases, see Gregory C. Sisk et al., *Charting the Influences on the Judicial Mind: An Empirical Study of Judicial Reasoning*, 73 N.Y.U. L. REV. 1377 (1998), which specifically considers federal decisions handed down in 1988 that consider the constitutionality of the Sentencing Reform Act of 1984 and the 1987 Federal Sentencing Guidelines, and David J. Walsh, *On the Meaning and Pattern of Legal Citations: Evidence from State Wrongful Discharge Precedent Cases*, 31 LAW & SOC’Y REV. 337 (1997), which uses citations from a number of cases—primarily state supreme court decisions but also lower state court decisions and federal court decisions applying state law—that considered common law wrongful discharge.

30. I use “precedent” here to simply mean any case on point that (a) has preceded the case at hand and (b) was able to be cited. In my analysis below, I distinguish between binding precedents (supreme court decisions that bind the courts of appeal) and non-

tion in one jurisdiction: Whether an arbitration provision in a standard-form contract is unconscionable. I analyze not only when a precedent from the pool is cited, but I also explicitly track when precedents from the pool are *not* cited. This allows me to provide an answer to an important question: What is different about precedents that are cited compared to those that are not?

Further, some of the previous literature uses univariate tests of correlation or quantitative analyses that explore each variable in a data set separately to measure the similarity of cases and cited precedents. Univariate tests, which only examine the overlap between two variables, are not overly convincing since other factors such as time, context, or publication status may be driving the results.<sup>31</sup> My findings make such controls explicit and yield further interesting insights into judicial behavior.

Part II outlines the basic hypotheses to be tested. Part III describes the data. Part IV tests for positive correlation between outcomes of the cases and outcomes of the cited precedents. Part V tests the causal relationship between outcomes and citations. Part VI extends the investigation to shed more light on citation behavior by examining the quality of the citation and citation behavior in unpublished opinions. Part VII concludes.

## II. HYPOTHESES

When writing a legal opinion, there is a history of precedents from which a judge has discretion to draw in describing the common law.<sup>32</sup> What determines which precedents are cited? This Part presents two competing—and admittedly extreme—hypotheses. Methods for testing these hypotheses are also introduced.

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binding precedents (decisions from the same hierarchical level). *See also infra* note 54 and accompanying text. R

31. For example, Professor David J. Walsh uses *t*-tests to analyze the similarity of outcomes of cases and the precedents cited in the opinion, and he finds unconditional correlation in certain subsets of the data only. Walsh, *supra* note 29, at 343–50, 355–57. *T*-tests evaluate statistical significance. *See* ABA SECTION OF ANTITRUST LAW, *ECONOMETRICS* 16–17 (2005). A similar methodology employed with my dataset (but not further discussed in this Article) indicates that I yield significant differences in ideology between cited and non-cited precedents with *t*-tests; that is, the average difference in ideology between judges is greater in the precedents that are not cited than the average difference in ideology between judges when precedents are cited. This is significant at the 1% level. Taking a multivariate approach, however, and controlling for factors such as time, context, hierarchy, and publication status indicates that the difference between cited cases and non-cited cases is no longer significant. R

32. *See generally supra* text accompanying notes 5–14. R

A. *Hypothesis 1: Citations Reflect the Entire Pool of All Precedents on Point*

Judges may cite precedents for a variety of reasons.<sup>33</sup> Citations may provide the reader with information about the legal background.<sup>34</sup> If judges use pro forma citations that outline the history of the law<sup>35</sup> or are genuinely grappling with authority,<sup>36</sup> then we may assume that cited precedents accurately reflect the entire pool of precedents.

This hypothesis can be tested using citation data by comparing outcomes of cited precedents to outcomes of non-cited precedents. Because there would be no significant difference between the outcomes of precedents that are cited and those that are not cited if judges discussed all cases on point—or a sample of cases that accurately reflects the distribution—we would not expect to find a positive correlation between outcomes of cases and outcomes of cited precedents.

There are, however, many sensible reasons why outcomes of cited precedents may not reflect the entire precedent pool. If a large number of cases on point have been decided, it would be extremely labor intensive and burdensome to cite all relevant precedents.<sup>37</sup> Further, simply showing a positive correlation between the outcomes of cited precedents and the outcome of the case at hand may reflect omitted variables. For example, courts may be less likely to cite precedents as time elapses, a phenomenon that Professor William Landes and Judge

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33. I do not provide an exhaustive list of reasons. See Richard A. Posner, *An Economic Analysis of the Use of Citations in the Law*, 2 AM. L. & ECON. REV. 381, 383–87 (2000) for a more complete discussion of reasons and hypotheses.

34. See generally Choi & Gulati, *supra* note 20, at 99 (explaining that “routine” or “pro forma citations” include “boilerplate string cites that some judges may cut and paste for matters such as the standard of review”); Posner, *supra* note 33, at 384 (suggesting that citations can “incorporate a body of information by reference,” so the reader knows where to find the rest of the relevant background). R

35. See generally Posner, *supra* note 33, at 384 (explaining that citations “identify a source of information, so that the reader of the citing work can verify the accuracy of statements of fact made in it”). R

36. See generally *id.* at 385 (noting that citations can “identify works or persons with which or with whom the author of the citing work disagrees” and “provide an authoritative basis for a statement in the citing work”).

37. Cf. William M. Landes & Richard A. Posner, *Legal Precedent: A Theoretical and Empirical Analysis*, 19 J.L. & ECON. 249, 259 (1976) (“Arguably, whether a judge cites many or few cases is largely a matter of personal preference or taste for citing cases. One possibility is that the judge with little taste for citing cases will tend to cite only the most recent cases, either because he lacks information on the relevance of earlier decisions, wants to economize his time, or believes that more recent ones tend to have greater precedential significance.”).

Richard Posner describe as “depreciation of legal capital.”<sup>38</sup> Positive correlation in outcomes may reflect the existence of subsets of “like” cases within a precedent pool. Courts are more likely to cite precedents of similar background than those of different legal and factual contexts.<sup>39</sup> These factors can be controlled for using multivariate regression analysis.<sup>40</sup> If the positive correlation survives such controls and the outcomes of cited precedents align closely with the outcome in the case at hand, the hypothesis that cited precedents reflect all precedents from the pool can be rejected.

If outcomes of cited precedents and cases at hand are positively correlated, it may be for one of two reasons: Judges may be biased in their description of the law, selectively providing authority to justify decisions made extra-legally, or judges may simply cite those precedents that were influential in reaching the decision. The second hypothesis postulates a causal chain.

*B. Hypothesis 2: Citations Reflect Ex Post Justification of Decisions Reached Extra-Legally*

Judges may decide cases for extra-legal reasons and then select precedents to support their decisions. Under this hypothesis, written opinions present a biased description of the law that coerce the reader into believing that cited precedents that have the same outcome as the case at hand reflect the law, and thus justify a decision actually reached through extra-legal means. Judges may choose not to cite precedents that contradict their desired outcome in an opinion.<sup>41</sup>

Therefore, outcomes of cases will reflect outcomes of cited precedents. A causal relationship cannot simply be inferred, however, from such positive correlation. The positive correlation could instead re-

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38. *Id.* at 262–70. For a more extensive discussion of the phenomenon that cites age as one factor that contributes to “precedent vitality” and undertakes “a systemic empirical examination of how Supreme Court precedents evolve over the years, as the Court treats them positively or negatively,” see THOMAS G. HANSFORD & JAMES F. SPRIGGS II, *THE POLITICS OF PRECEDENT ON THE U.S. SUPREME COURT* 23, 55–77 (2006).

39. *Cf.* Landes & Posner, *supra* note 37, at 259 (explaining that economists “expect[ ] citation practices to be basically uniform across judges” and believe that “[i]f a judge cites more cases, it is not because his taste for citations is different but because the case before him is different—perhaps it has more issues, or its issues are less clearly controlled by some precedent”).

40. Multivariate regression analysis uses two or more right-hand side variables to explain the left-hand side variable. That is, the explanatory equation has at least two regressors. For more on multivariate analysis, see STOCK & WATSON, *supra* note 26, at 142–84, 241–65. Multivariate tests can be contrasted to univariate tests that have only one regressor. See generally *id.* at 91–130.

41. The causal chain under this hypothesis is as follows:

*preferences* → *outcomes* → *citations*

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flect the more traditional view of precedent and citations—that is, that judges use precedent as legal authority and “render the decision that is dictated by those authorities.”<sup>42</sup>

Using an instrumental variable approach, I provide a test of Hypothesis 2. Do outcomes “cause” citations? Instrumental variables are used in statistics and econometrics to determine the direction of causation when natural experiments are not available. If, for example, one suspects that a change in the left-hand side variable causes a change in the suspect explanatory, right-hand side variable, an “instrument” may be used for the suspect explanatory variable. An instrument is simply a variable that does not necessarily belong in the explanatory equation; the instrument is correlated with the suspect explanatory variable, conditional on the other right-hand side variables.<sup>43</sup>

Here, my instrument for the suspect variable (*outcomes*) is judicial preference. My measure of judicial preference is simple—perhaps even crude—but effective for this study: the political party of the governor who appointed the judge writing the opinion. Essentially, the argument is that the party of the appointing governor reflects judicial preferences.<sup>44</sup>

This instrumental variable approach may also help overcome bias from omitted factors that affect the outcome and the types of cases that are cited, because other factors may affect the outcome of the case and the tendency of a judge to cite a precedent but will not affect who appointed the judge.<sup>45</sup> One example of omitted variable bias is the inability to control for the relative quality of legal representation. In a given case, lawyers from one side may present a more cogent case, generating positive correlation between the outcomes of cited precedents and the outcome of the cases at hand.<sup>46</sup> The political appointer of the writing judge is unlikely to be correlated with such factors.

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42. See Cross et al., *supra* note 28, at 494–95 (“Justices examine the legal authorities cited by the parties, which substantially include precedents, and render the decision that is dictated by those authorities.”). This is visually represented in the following model:

$$\begin{aligned} & \textit{influential precedents} \rightarrow \textit{outcomes} \\ & \textit{influential precedents} \rightarrow \textit{citations} \end{aligned}$$

43. For a more detailed treatment of instrumental variables regression, see STOCK & WATSON, *supra* note 26, at 331–65.

44. See *infra* note 57. This is visually represented in the following model:

$$\textit{politics (preferences)} \rightarrow \textit{outcomes} \rightarrow \textit{citations}$$

45. See *supra* note 43.

46. Cf. Stephen Bright, Gideon—A Generation Later, *Introduction & Keynote Speakers*, 58 MD. L. REV. 1333, 1377 (1999) (explaining that a plaintiff must prove that “the lawyer’s performance was so deficient that it affected the outcome of the trial”); Stephen B. Bright, *Neither Equal Nor Just: The Rationing and Denial of Legal Services to the Poor When Life and Liberty*

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There may be other extra-legal dimensions of judicial preference that affect outcomes and require ex post legal justification in the written opinion. If judicial outcomes are driven by other extra-legal factors, such as sympathy for a particular party, and if such factors are not correlated with politics, the instrument will not pick them up.<sup>47</sup>

The validity of the instrument is discussed in greater detail in Parts V and VI.

### III. DATA

#### A. Source of Data

The dataset contains observations of 72 reported California Courts of Appeal cases and 7 California Supreme Court cases.<sup>48</sup> The cases answer a very narrow question: Is the arbitration clause in a standard-form contract unconscionable? The dataset contains—to the best of my knowledge—all reported appeals cases in this jurisdiction answering this narrow question as of February 2008.<sup>49</sup>

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*Are at Stake*, 1997 ANN. SURV. AM. L. 783, 791 (“Competent legal representation is essential to ensure that such decisions [on punishment] are as well informed as humanly possible.”).

47. See generally Amy Coney Barrett, *Stare Decisis and Due Process*, 74 U. COLO. L. REV. 1011, 1071 & n.233 (2003) (noting that identical parties should get equal treatment but acknowledging that factors like sympathy or prejudice may influence the outcome of a case). This is visually represented in the following model:

*political component of preferences* → *outcomes* → *citations*  
*other components of preferences* → *outcomes* → *citations*

48. The set also contains data from 102 unreported California Courts of Appeal cases. The citation behavior in these cases will be discussed in Part VI.B.

49. All data are obtained from LexisNexis. To obtain the set of cases, a search for “state court cases, combined” decided prior to February 18, 2008 that satisfied *all* of the following criteria was conducted: (1) either of the terms “contract!” or “agree!”; (2) the wild-card “unconscionab!”; (3) the wild-card “arbitrat!”; and (4) any of the following terms in either “Overview” or “Core Terms”—“standard-form,” “standard form,” “boiler-plate,” “boiler plate,” or “adhesi!”. The exact search string was: (CORE-TERMS(contract! OR agree!) OR OVERVIEW(contract! OR agree!)) AND (CORE-TERMS(unconscionab!) OR OVERVIEW(unconscionab!)) AND (CORE-TERMS(arbitrat!) OR OVERVIEW(arbitrat!)) AND (“standard-form” OR “standard form” OR “boiler-plate” OR “boiler plate” OR “adhesi!”).

The first criterion restricts the analysis to contract law cases. The second criterion attempts to limit the search to those cases where unconscionability was the primary doctrine under consideration. The third criterion limits the type of clause, so that unconscionability cases that involve illegal disclaimers or excessive pricing are not included. The fourth criterion restricts the search to cases where the contract is a standard-form contract. (The use of the wild-card “adhesi!” captures the use of the label “contracts of adhesion” and “adhesive contract”—labels commonly used by judges to describe standard-form contracts.) This search strategy captures all state appeals cases from all fifty states.

This search yields exactly 499 cases. The results were then restricted to California, yielding 199 cases. Of these, 18 cases did not make any findings on the unconscionability of an arbitration provision under California law and therefore could not be included in the

Variables of interest were coded including whether the contract was deemed unconscionable, the date the judgment was delivered, the district where the case was heard, the characteristics of the presiding judges, and the environment in which the contract was formed. Text parsing software was used to count the frequency of particular words and phrases that were used by the judge writing the opinion.<sup>50</sup> Thus, I was able to track the facts that the judge found important and compare similarities in the language used in the case and in the precedent.

A dataset of pairwise observations was generated. Each observation compares variables of interest in the case at hand to variables of interest in the precedent. I compared only those pairs where the decision in the case at hand was delivered *after* the reported precedent. For simplicity, this pairwise dataset was broken into two. The first dataset compares all reported courts of appeal cases to all reported courts of appeal precedents. This dataset contains 2,556 observations. The second dataset compares all reported courts of appeal cases to supreme court precedents.<sup>51</sup> This dataset contains 273 observations. Each observation is a “potential citation.” The precedents cited by the courts of appeal in each case were tracked, as were those precedents that are applicable but not cited. All citations considered in this Article come from the majority opinion in each case.<sup>52</sup>

Taking “potential citations” as the universe for study generates explanatory value that, to my knowledge, has not previously been explored. Take the following example. Suppose that we are interested in whether the cited precedents share a particular trait that is present in the case at hand. The study focuses on the outcome of the cases—that is, *liable (L)* or *not liable (NL)*.<sup>53</sup> The left panel in Figure 1 represents information commonly found in the empirical citation litera-

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dataset. Unconscionability in these particular cases was merely discussed by the court in the context of another claim, or perhaps the court decided not to issue a finding on unconscionability. Cases decided in California that did not use California law were also removed. Of the remaining 181 cases, 7 were decided by the California Supreme Court and 174 were decided by the California Courts of Appeal. Seventy-two of the 174 cases were reported—these cases formed the basis of the first dataset in this Article. The second dataset included the 7 California Supreme Court cases. The extension in Part VI.B uses the data from the 102 unpublished cases.

50. A list of the words and phrases that were analyzed can be found in the Appendix.

51. All cases from the California Supreme Court are reported cases.

52. Dissents are rare in the dataset. Only 5 of the 72 reported courts of appeal decisions contain a dissenting opinion (usually dissenting as to one small part of the majority decision). I do not report the properties of citations contained in dissents since the 5 dissenting judgments cite a total of only 6 courts of appeal cases—a number too small to generate results of any significance.

53. I use this particular trait throughout the example.

ture. The judge in the example cites two precedents that share the same trait of interest as the case at hand. The case at hand finds the defendant *liable*, as do the two precedents. But what inferences can be drawn from this observation? To establish that the judge overrepresented precedents that held the defendant *liable*, it must be established that the precedents that were not cited in the case at hand disproportionately held the defendant *not liable*. If the potential citations that are not cited are an accurate reflection of the entire pool of potential citations, then the judge writing the opinion will be citing a sample of precedents that are representative of the entire pool.

The panels on the right of Figure 1 represent different precedent pools. Different conclusions would be reached after examining these two precedent pools. The top panel illustrates a situation in which the two cited precedents were the only relevant precedents on point. It would be difficult to argue that the judge overrepresented *liable* precedents in this example. The bottom panel illustrates a situation in which the judge has nine potential precedents from which to choose. The judge only cites the two *liable* precedents and fails to cite the seven precedents that held the defendant *not liable*. Here, an inference that *liable* cases are overrepresented relative to the entire precedent pool is plausible.

<table border="1"> <tr> <th>Outcome in case at hand</th> <th>Outcome in cited precedents</th> </tr> <tr> <td style="text-align: center;"><b>L</b></td> <td style="text-align: center;">L</td> </tr> </table>		Outcome in case at hand	Outcome in cited precedents	<b>L</b>	L	<table border="1"> <tr> <th>Outcome in case at hand</th> <th>Outcome in cited precedents</th> <th>Outcome of cases in precedent pool</th> </tr> <tr> <td rowspan="2" style="text-align: center;"><b>L</b></td> <td style="text-align: center;">L</td> <td style="text-align: center;">L</td> </tr> <tr> <td style="text-align: center;">L</td> <td style="text-align: center;">L</td> </tr> </table>			Outcome in case at hand	Outcome in cited precedents	Outcome of cases in precedent pool	<b>L</b>	L	L	L	L		
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FIGURE 1: The panel on the left illustrates the information commonly found in the citation literature; the characteristics of a case are compared to the characteristics of cited precedents only. The panels on the right illustrate two potential scenarios consistent with this information. The top right panel shows that the two cited precedents are the *only* two precedents on point. The bottom right panel shows that the two cited precedents are two of nine precedents on point (potential citations) and the seven precedents not cited did not share the same characteristic as the cited precedents.

## B. Summary of Data

### 1. Citations

Of the 2,556 potential citations of courts of appeal precedents, there are only 239 actual citations of precedent. That is, on average, in a given opinion, a court of appeal cites less than 1 in 10 of its previous opinions. Citations of supreme court decisions are, not surprisingly, far more frequent. The courts of appeal have less discretion to cite supreme court precedents than courts of appeal precedents.<sup>54</sup> Of the 273 potential citations of supreme court precedents, there are 129 actual citations (or just under half).

### 2. Outcomes of Cases

The measurement of “outcome” is confined to the narrow determination of unconscionability: Does the court refuse to enforce the arbitration clause *as written* on the basis that it is unconscionable? The decisions are split fairly evenly throughout the dataset with 39 of the 72 courts of appeal cases finding that the provision was unconscionable (54.17%) and 5 of the 7 supreme court cases finding that the provision was unconscionable (71.43%). Turning to the pairwise datasets, the same outcome is found in the case at hand and in the precedent case in about half of the observations.<sup>55</sup>

### 3. Politics and Judicial Preferences

The enforcement of arbitration provisions in standard-form contracts appears to be an area of commercial law, like contract enforcement, where politics plays a significant role in determining the outcome.<sup>56</sup> I track the political party of the governors who appointed

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54. See generally JEFFERY A. HOGGE ET AL., 5-41 CALIFORNIA APPELLATE PRACTICE AND PROCEDURE § 41.31[1] (2010) (explaining that “court[s] of inferior jurisdiction” are required “to follow the decisions of a court exercising superior jurisdiction,” such that “decisions by the California Supreme Court are binding on all of the courts of California”); Keenan D. Kmiec, Comment, *The Origin and Current Meanings of “Judicial Activism,”* 92 CAL. L. REV. 1441, 1466–67 (2004) (explaining that the rule of “vertical precedent” requires lower courts to “abide by controlling precedent” and commenting “that the act of disregarding vertical precedent qualifies as one kind of judicial activism”).

55. The case and the courts of appeal precedent have the “same outcome” in 1,269 of the 2,556 potential citations (49.65%). The case and the supreme court precedent have the “same outcome” in 139 of the 273 potential citations (50.92%).

56. See, e.g., Stephen J. Ware, *Money, Politics and Judicial Decisions: A Case Study of Arbitration Law in Alabama*, 15 J.L. & POL. 645, 667–86 (1999) (finding that the outcomes of similar cases involving contracts with arbitration clauses in Alabama are correlated with the politics of presiding judges, particularly with the source of campaign funds); Niblett, *supra* note 11, at 2, 24–25 (showing that the conflicting ideologies of the panels in different contract cases is highly correlated with inconsistent decisions). The correlation that I am

each presiding judge to a court of appeal. This measure is used as a proxy for the ideology of the judge. While this method of measuring ideology is crude and imperfect, it provides insight into judicial preferences.<sup>57</sup> It is assumed that (1) Republican judges are more inclined to enforce arbitration provisions and decide in favor of the drafting party, the defendant, and (2) Democratic judges are more inclined to take into account equity issues and assist the weaker, nondrafting party, the plaintiff. In each case, the defendant is a corporation, and the plaintiff is an employee or a consumer.

In the pairwise dataset, a variable is generated that measures potential conflict between the ideology of the judge who writes the opinion and the outcome of the precedent. This variable, simply referred to as *conflicting politics*, is a dummy variable that takes the value 1 if the authoring judge was appointed by a Democratic governor and the pre-

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interested in—between the politics of the writing judge and the outcome of the case—is discussed in greater detail in Part V.

57. For more on the issue of using political appointment as a measure of ideology and judicial preference, see generally JEFFREY A. SEGAL & HAROLD J. SPAETH, *THE SUPREME COURT AND THE ATTITUDINAL MODEL REVISITED* 430–35 (2002) (“If the Supreme Court’s decision that handed George W. Bush the election tells us anything, it’s that the Supreme Court is more secure and more comfortable than it has ever been in pushing an agenda that is not only activist and conservative, but also blatantly partisan.”); Archibald Cox, *The Role of the Supreme Court: Judicial Activism or Self-Restraint?*, 47 MD. L. REV. 118, 135 (1987) (“Making or appearing to make appointments simply with an eye to obtaining a predictable vote on policy grounds tends to weaken public belief in ‘law,’ and also to make the actual style of decisionmaking still more political.”); Harry T. Edwards, *Collegiality and Decision Making on the D.C. Circuit*, 84 VA. L. REV. 1335, 1347–50 (1998) (criticizing a past study that uses appointing President as a proxy for ideology because, in particular, “the categories ‘Democrat’ and ‘Republican’ do not necessarily match sympathy towards greater or lesser degrees of environmental protection”); Lee Epstein & Gary King, *The Rules of Inference*, 69 U. CHI. L. REV. 1, 87–97 (2002) (examining the concept of validity by critiquing various empirical studies by scholars in the legal academy that have considered the impact of ideology on judicial preference); Joshua B. Fischman & David S. Law, *What is Judicial Ideology, and How Should We Measure It?*, 29 WASH. U. J.L. & POL’Y 133, 167–72 (2009) (examining the appointee’s party as a proxy for a judge’s ideology and stating, with caveats, that such a consideration “can be useful and significant predictors of judicial voting”); Michael J. Pitts, *What Will the Life of Riley v. Kennedy Mean for Section 5 of the Voting Rights Act?*, 68 MD. L. REV. 481, 527 (2009) (exploring further the “attitudinal model,” which suggests that Justices have ideological preferences that “creep into Court decision-making because traditional legal arguments, such as precedent, statutes, and legislative history are often indeterminate and leave judges with discretion to render policy judgments”); Gregory C. Sisk & Michael Heise, *Judges and Ideology: Public and Academic Debates about Statistical Measures*, 99 NW. U. L. REV. 743 (2005) (performing an empirical study of religious freedom decisions by the federal district and circuit courts with a particular focus on “variables that attempt to quantify the anticipated ideological leanings of judges” and concluding that although “ideology emerged as significant in certain aspects of [the] study,” it was not “ubiquitous or dominating”); Niblett, *supra* note 11, at 24 n.40 (recognizing that measuring political affiliation by the governor’s political party “is not a perfect measure of political ideology”).

cedent case favored the defendant *or* if the authoring judge was appointed by a Republican governor and the precedent was decided in favor of the plaintiff. The variable otherwise takes the value 0.<sup>58</sup>

#### 4. Controls

I also controlled for a variety of factors that are likely to affect citations. Two controls, in particular, are discussed in detail below.

##### a. Time Between Decisions

The earliest case in the dataset is from 1980.<sup>59</sup> The most recent case is from October 2007.<sup>60</sup> Cases are not distributed evenly over this twenty-seven year period. Over 70% of cases were decided in the last ten years of the dataset. For each observation in the pairwise datasets, I measure the length of time between the case at hand and the precedent cited. I find evidence to support Landes and Posner's depreciation of legal capital theory: Precedents are cited with less frequency as time elapses.<sup>61</sup> This is true of both courts of appeal precedents and supreme court precedents. Figure 2 tracks the trend of the incidence of citation between the case at hand and the precedent as time elapses. A clear downward trend is shown with both courts of appeal precedents (solid line) and supreme court precedents (dashed line).

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58. Although I examined other measures of politics and preferences, their validity as an instrument was questionable. For example, I tracked the differences in ideology of the judges who wrote the opinions, as well as the difference in ideology of the benches. Unlike the *conflicting politics* variable, and as explained in Part V, neither of these differences was significantly correlated with the similarity of the results between cases and precedents.

59. *Ramirez v. Superior Court*, 163 Cal. Rptr. 223 (Ct. App. 1980). The first unpublished decision is from 1977. *N.V. Heathorn, Inc. v. Regents of Univ. of Cal.*, 138 Cal. Rptr. 357 (Ct. App. May 13, 1977) (depublished). The first supreme court case in the sample, *Graham v. Scissor-Tail, Inc.*, 623 P.2d 165 (Cal. 1981), was decided in 1981.

60. *Murphy v. Check 'N Go of Cal., Inc.*, 67 Cal. Rptr. 3d 120 (Ct. App.), *modified*, No. A114442, 2007 Cal. App. LEXIS 2181 (Cal. Ct. App. Nov. 9, 2007). The most recent unpublished decision is from February 2008. *Price v. Plant Stand*, No. G038361, 2008 Cal. App. Unpub. LEXIS 1145 (Ct. App. Feb. 8, 2008). The most recent supreme court case in the sample, *Gentry v. Superior Court*, 165 P.3d 556 (Cal. 2007), was decided in August 2007.

61. See Landes & Posner, *supra* note 37, at 262–63 (“Although a precedent does not ‘wear out’ in a physical sense, it depreciates in an economic sense because the value of its information content declines over time with changing circumstances.”).

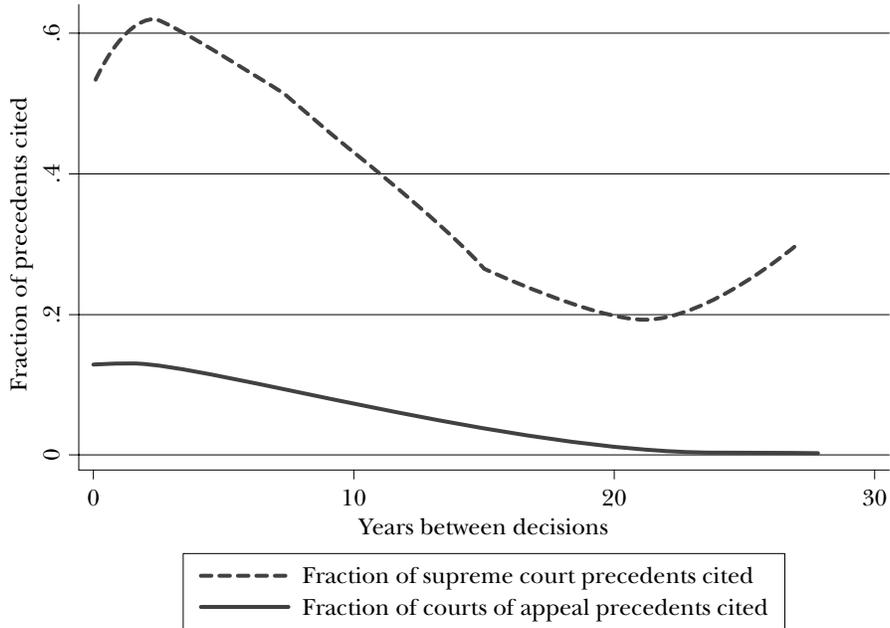


FIGURE 2: The frequency of a precedent’s citations declines as time elapses.

Figure 2 also shows that citations of courts of appeal precedents trend uniformly downward as time elapses. Citations of supreme court precedents yield a more intriguing shape with an up-tick in citations after twenty years have passed. My anecdotal explanation for this (which suggests that this is not a general result) relies on the fact that (as noted above) the vast majority of the courts of appeal cases come from the last decade. There were two supreme court decisions on point in the early to mid-1980s: *Graham v. Scissor-Tail, Inc.*<sup>62</sup> and *Dryer v. Los Angeles Rams*.<sup>63</sup> *Graham* is an on point decision that lays out the common law framework for determining the unconscionability of arbitration clauses.<sup>64</sup> *Dryer* is a much shorter decision that offers only minor clarifications in the law.<sup>65</sup> This difference in the importance of *Graham* and *Dryer* may suggest that the “trough” in citations is likely to persist in the future. That is, even though it is older, *Graham*

62. 623 P.2d 165 (Cal. 1981).

63. 709 P.2d 826 (Cal. 1985).

64. *Graham*, 623 P.2d at 172–78 (outlining the requirement that arbitration clauses have “‘minimum levels of integrity’”).

65. *Dryer*, 709 P.2d at 831–33 (explaining that “[t]he trial court concluded that the arbitration provisions failed to meet [the] ‘minimum levels of integrity’ required by *Graham* solely because of the remote—indeed, speculative—possibility of commissioner intervention,” but noting that neither *Graham*’s holding nor its reasoning “dictate[d] this result”).

will continue to gather more citations than *Dryer* because it is on point and lays out the applicable legal framework.

*b. Measuring Similarities in Context Between Cases*

Cases within the narrowly defined universe of the dataset used for analysis are not uniform. The contexts in which the cases arise differ. This will affect whether a case is cited or not.<sup>66</sup> “Context” is measured in two ways. The first way is very simple: I look at the contracting environment, that is, the type of contract involved. The cases are divided in two: employment contracts (37 of the 72 cases, or 51.39%) and sales contracts (35 of the 72 cases, or 48.61%). I am interested in uncovering whether courts are more likely to cite a precedent if it involves the same contracting environment.

The second measure of context is a little more complex. Courts may focus on different aspects of the facts in different cases.<sup>67</sup> This is controlled because a case that heavily discusses the complexity of the standard-form contract would be more likely to cite a precedent that also focuses heavily on contractual complexity compared to a precedent that focuses on other factors such as whether the arbitration process was biased. A measure of the similarity between the two types of written opinions is provided.

I generate a count of 312 key terms to capture the factors that the court emphasizes.<sup>68</sup> The key terms are divided into four themes: (1) complexity of the contract in question; (2) the weakness of the plaintiff; (3) factors negating plaintiff consent; and (4) the neutrality of the arbitration provision. I count the number of times each opinion mentions a key term when discussing the facts of each case,<sup>69</sup> relating to

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66. On the question of context and for the specific argument that rules of relevance of factors in a particular context guide whether a precedent applies in a given situation, see Frederick Schauer, *Precedent*, 39 STAN. L. REV. 571, 576–79 (1987).

67. See *supra* note 12 and accompanying text.

68. The list of these 312 key terms is provided in the Appendix.

69. I do not use the entire written opinion when calculating the scores to measure context. Rather, I focus attention on the facts of the case at hand. Specifically, I isolate the facts of the case that are relevant to the claimed unconscionability of the arbitration clause. Using the entire opinion would bias the results. When judges write decisions, they tend to make a number of references to the facts of other cases, either to show that this case is similar to or can be distinguished from the previous case. Cf. Cross et al., *supra* note 28, at 510 (examining how other commentators discuss the judicial ability to flexibly distinguish cases). Using the entire opinion would capture factual elements of other cases and would not accurately map the court’s portrayal of procedural and substantive concerns in these specific cases. At the other extreme, simply using the “facts” section of each case would not provide enough information; judges often lay out only the bare facts in these sections such as who is suing whom, what claims are being made, and what findings the trial court made. Many of the salient facts in the cases under study such as those related to contractual

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each of the four factors to generate a total for each category. I label that total  $c_j$ . I then create a measure of the *relative emphasis* placed on each factor in a particular written opinion.<sup>70</sup> Next, I compare the relative emphasis placed on each factor in a case and a given precedent by summing the difference in relative emphasis among each of the four factors to create a new measure.<sup>71</sup>

Intuitively, the resulting measure provides a quantitative evaluation of the “difference” between what courts emphasize in given opinions and precedents. For each case-precedent pair there is now a score.<sup>72</sup> A low score indicates that the relative emphasis placed on each of the four categories measured in the two respective cases is very similar. This means that each focused its attention on similar aspects of the facts. A high score suggests that the two decisions place relative emphasis on different factors. It is expected that when this “difference” is greater, the court is less likely to cite a precedent.<sup>73</sup>

complexity or how weak the plaintiff is perceived to be, are often discussed only in the “analysis” or “discussion” sections of the written opinions.

It is therefore necessary to strip down the written opinions so that only the information relevant to the mapping is retained. This is done by removing references to judicial and legislative history and any factual descriptions of previous cases that may appear. Further, only majority opinions are utilized in this analysis; dissenting and concurring opinions are not analyzed. In addition, quotations from other cases are largely eliminated unless the quotation is followed by a statement indicating similarity with the case at hand.

Moreover, the litigants’ contentions that the court expressly disagrees with are removed. For example, the opinion may cite a series of arguments by the defendant that the contract is not a contract of adhesion, but then conclude with “we disagree” or “we find these contentions unpersuasive.” Leaving these arguments in would not provide an accurate description of the court’s view of the procedural and substantive concerns of the provision.

Irrelevant information that may arise in the course of a written opinion is also removed. Courts, for instance, may discuss elements of a case that in no way relate to the unconscionability of the arbitration provision. For example, in *Camilo Lopez, Inc. v. Sialic Contractors Corp.*, No. B170164, 2005 Cal. App. Unpub. LEXIS 377 (Ct. App. Jan. 13, 2005), the court wrote: “We take this opportunity to indicate our displeasure at the language, tone and presentation of Lopez’s briefs. The sarcastic and caustic comments which permeate its briefs are indecorous and unprofessional. The repeated use of bolding, italicizing, underlining and parenthetical commentary rendered the briefs virtually unreadable.” *Id.* at \*9–10. If this quotation were left in the study, the measure of the complexity of the contract would be biased, as many of the words included in this paragraph are key words used to determine the complexity of the contract.

70. This measure,  $m_{j_i}$ , is created thus:

$$m_{j_i} = c_{j_i} / \sum c_{j_i}$$

71. This measure is created using the equation:

$$D_{ik} = 0.25 * \sum | m_{j_i} - m_{j_k} |$$

72. As illustrated:

$$0 = D_{ik} = 1$$

73. By way of illustration, across all the pairwise observations with published opinions, the highest score was 0.38. The two cases here were *Balandran v. Labor Ready, Inc.*, 22 Cal. Rptr. 3d 441, 445–47 (Ct. App. 2004), in which the court focused on whether an arbitra-

IV. CORRELATION: HYPOTHESIS 1 IS REJECTED: THE OUTCOMES OF CITED PRECEDENTS DO NOT REFLECT THE ENTIRE POOL OF PRECEDENTS

A. *Univariate Analysis*

What is different about those pairs where a precedent is cited and those pairs where a precedent is not cited? To answer this question, the statistical breakdown in Table 1 illustrates the difference between pairs where the case at hand cites the precedent case (*cited*) and pairs where the case at hand does not cite the precedent case (*not cited*). A statistical comparison of the two groups indicating whether selected differences between them are statistically significant—that is, whether the difference between the two factors is unlikely to have occurred by chance—is also presented. The data is broken down by court level to illustrate differences in the citation practices of supreme court precedents and courts of appeal precedents.

	DATASET 1: COURTS OF APPEAL PRECEDENTS			DATASET 2: SUPREME COURT PRECEDENTS		
	<i>Cited</i> (n=239)	<i>Not cited</i> (n=2,317)	<i>Difference</i> (t-stat)	<i>Cited</i> (n=129)	<i>Not Cited</i> (n=144)	<i>Difference</i> (t-stat)
Same outcome (% of cases)	62.34%	48.33%	<b>4.13***</b>	58.14%	44.44%	<b>2.27**</b>
Difference in time (years)	4.03	9.11	<b>-10.29***</b>	7.40	12.41	<b>-5.40***</b>
Same contractual type (% of cases)	69.04%	47.30%	<b>6.45***</b>	54.26%	47.92%	<b>1.04</b>
Difference in factors (score)	0.532	0.589	<b>-3.00***</b>	0.494	0.672	<b>-4.76***</b>
Same judge on both panels (% of cases)	7.11%	3.19%	<b>3.11***</b>	-	-	-
Same district (% of cases)	29.29%	24.17%	<b>1.74*</b>	-	-	-

TABLE 1: Differences between precedents that are cited and precedents that are not cited.

tion clause within an employment contract applied to pre-employment negotiations, and *Fittante v. Palm Spring Motors, Inc.*, 129 Cal. Rptr. 2d 659, 669–74 (Ct. App. 2003), in which the court placed emphasis on whether the arbitration term was substantively unconscionable. Given the difference in emphasis, as reflected in the high score, it is not surprising that *Balandran* did not cite *Fittante*.

The examination of differences in Table 1 suggests strong differences between precedents that are cited and those that are not cited. Of particular interest for the hypotheses is whether cited precedents are more likely to have the same outcome in the case at hand. Looking at the first dataset, 62.34% of cited courts of appeal precedents have the same outcome as the case at hand. Of the pool of courts of appeal precedents not cited, just 48.33% have the same outcome as the case at hand. This difference is significant at the 1% level, meaning that there is a 99% chance that the difference between the two numbers was not generated by coincidence but rather results from a meaningful difference.

Thus, the interpretation of the data appears to run counter to the claim that the outcomes of cited precedents reflect the entire pool of precedents. Pro-plaintiff decisions cite more pro-plaintiff precedents; pro-defendant decisions cite more pro-defendant precedents. The data on supreme court precedents also exhibit this positive correlation in outcomes.

An alternative way of presenting this result is to ask the following: Are we more likely to see a precedent cited if it has the same outcome as the case in hand? The data suggest so, as 11.74% of all potential citations of courts of appeals precedents result in citation when the case and the precedent share the same outcome. Only 6.99% of all potential citations of courts of appeal precedents result in citation when the outcomes differ. This differential of nearly 5 percentage points is significant, especially in light of the infrequency with which courts of appeal precedents are cited overall. Turning to supreme court precedents, 58.14% of potential citations result in citation when the case and precedent share the same result compared to just 44.44% of potential citations when the result differs. This differential is significant at the 5% level. The results presented thus far suggest that opinions are more likely to cite precedent with the same outcomes than those with different outcomes. To ensure that this interpretation is accurate, however, a more sophisticated model controlling for additional factors is explored below.

### *B. Multivariate Analysis*

I performed a series of statistical tests that controlled for a variety of factors that may affect citations to ensure that the above correlation persists. The basic equation used to conduct this analysis explored if—and to what relative extent—several variables affect the likelihood

that a case explicitly refers to a given precedent.<sup>74</sup> Included in the formula is one dependent variable—that is, the one that I would like to explain that measures whether a given case cites a given precedent.<sup>75</sup> In addition, four independent variables are examined—that might affect the outcome of the dependent variable: (1) whether a given case has the same outcome as a given precedent,<sup>76</sup> (2) the length of time that passes between two cases, (3) the contracting environment, and (4) differences in the factors stressed by the courts in each case. The equation was run twice. Model 1 looks at the 2,556 potential citations of courts of appeal precedents. Model 2 looks at the 273 potential citations of supreme court precedents. I report the marginal effects.<sup>77</sup>

The main takeaway from Table 2 is that even once I control for the other relevant variables, the outcome in a particular case is more likely to be the same as a precedent that is cited.<sup>78</sup> The coefficient on the variable measuring whether a case has the same outcome as a precedent is positive and significant in both Models 1 and 2. That is, judges are more likely to cite precedents that have the same outcome as the case at hand regardless of factors such as time, type of contract, or the issues emphasized.

For the courts of appeals, the results presented show somewhat marginal effects. There is a greater probability that the precedent will be cited if a pair of cases has the same outcome. This difference is significant at the 5% level.

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74. Specifically, a series of *probit* multivariate regressions of the following form was conducted:

$$\Pr(\text{cited}_{ik}) = a + \beta.\text{sameoutcome}_{ik} + \gamma.W_{ik} + e_{ik}$$

Probit estimators are more efficient estimators than ordinary least squares (“OLS”) when the dependent variable is binary (that is, only 0 or 1). See STOCK & WATSON, *supra* note 26, at 296–330. OLS estimators can generate predicted probabilities that are less than 0 or greater than 1. See *id.* at 302 (“The linearity that makes the linear probability model easy to use is also its major flaw. . . . [T]he estimated line representing the predicted probabilities drops below zero for very low values . . . and exceeds one for high values!”). I also ran the same tests using logit estimators. For an explanation of logit regressors, see *id.* at 307–09. There are no substantial differences in the results using probit or logit.

75. This variable, *cited<sub>ik</sub>*, takes the value 1 if the case at hand refers to the precedent explicitly at least once and otherwise takes the value 0.

76. This variable, *sameoutcome<sub>ik</sub>*, is a dummy variable that takes the value 1 if the case at hand (*i*) and the precedent (*k*) have the same outcomes, and takes the value 0 if the outcomes are different.

77. Reporting the marginal effects of the probit regression allows for easier interpretation of the coefficients.

78. Table 2 does not report results once I control for whether cases involve the same judge or the same district, nor does it report the results of including a dummy variable for each of the six districts in the California Courts of Appeal. Although I included these variables in models not reported here, they did not significantly change the results.

If a supreme court precedent has the same outcome as a particular case, it is also still more likely to be cited than if the outcome was different. In fact, the statistical significance is even stronger ( $p < .01$ ). Thus, even controlling for factors such as time and context, I reject the hypothesis that judges cite a representative sample of precedents.<sup>79</sup>

The results in Tables 1 and 2 also confirm the prior results regarding the control variables. Judges are more likely to cite recent precedent than older precedent, giving support to the hypothesis that legal capital depreciates over time.<sup>80</sup>

Similarly, the measures of context yield unsurprising results. First, courts of appeal cases cite precedents from the courts of appeal more frequently when the contracting environment of the two cases is similar. In contrast, courts of appeal cases that involve employment contracts are just as likely to cite supreme court precedents that involve consumer contracts as they are to cite precedents that involve employment contracts. This relationship is not significant, suggesting that this variable does not affect whether a supreme court case is cited by a court of appeal.

Further, when cases discuss similar factors, the precedent is more likely to be cited. The coefficient on the variable measuring differences in the factors discussed by courts in particular cases is negative and significant in each regression, which suggests that this variable explains to some extent the similarities and differences between legal opinions. Ultimately, it seems that where differences are high, the case is less likely to be cited.

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79. See, e.g., *supra* Part III.A.4.

80. See *supra* notes 37–38 and accompanying text.

VARIABLES	(1)	(2)
	Pr( <i>cited</i> ) COURTS OF APPEAL PRECEDENTS	Pr( <i>cited</i> ) SUPREME COURT PRECEDENTS
Same outcome	0.0318** (0.0144)	0.1465*** (0.0248)
Difference in years	-0.0086*** (0.0016)	-0.0175* (0.0101)
Same type of contract	0.0618*** (0.0155)	0.0587 (0.0541)
Difference of factors discussed	-0.0459* (0.0254)	-0.4363** (0.2083)
Observations	2556	273

Robust standard errors in parentheses (clustered at precedent level)  
 \*\*\* p<0.01, \*\* p<0.05, \* p<0.1

TABLE 2: Factors correlated with citation in the case-precedent pairs.

V. CAUSATION ANALYSIS: HYPOTHESIS 2 IS NOT ACCEPTED BECAUSE NO EVIDENCE SUGGESTS THAT THE OUTCOME OF A CASE DRIVES CITATIONS

There is a strong positive correlation between outcomes of cases and outcomes of cited precedents. This is consistent with the hypothesis that judges use citations to ex post justify their decisions.<sup>81</sup> However, the correlation is also consistent with the proposition that citations represent influential precedents that lead the judge to the outcome.<sup>82</sup> To address causality and concerns of omitted factors, politics is used as an instrument for explaining the outcome of a case at hand.<sup>83</sup> When the ideology of an authoring judge conflicts with the outcome in a given precedent, I hypothesize that it is less likely that the judge will hand down the same outcome as the precedent. To test this hypothesis, I use a two-stage test: The first stage predicts when a case will have the same outcome as a precedent in light of whether or not there is a conflict between the ideology of the judge in the case and the outcome of the precedent.<sup>84</sup> The second stage uses the new

81. See *supra* Part II.B.  
 82. See *supra* note 16 and accompanying text.  
 83. See *supra* Part III.B.3.  
 84. The formula for this step is:

$$\text{sameoutcome}^{\wedge}_{ik} = \alpha_1 + \beta_1 \cdot \text{conflictingpolitics}_{ik} + \gamma_1 \cdot W_{ik} + u_{ik}$$

With binary endogenous regressors, I cannot simply mimic the 2SLS IV estimation using probit analysis as it can yield inconsistent estimators. For a more thorough discussion of this issue, see JEFFREY M. WOOLDRIDGE, *ECONOMETRIC ANALYSIS OF CROSS SECTION AND PANEL DATA* 477–78 (2002). I follow the suggestion of Professors Joshua D. Angrist and Alan B. Krueger and use a linear probability model for the first stage of the procedure. Joshua D. Angrist & Alan B. Krueger, *Instrumental Variables and the Search for Identification: From Supply and Demand to Natural Experiments*, 15 J. ECON. PERSP. 69, 80 (2001).

predicted variable to estimate the probability of citation in a given case-precedent pair.<sup>85</sup>

Conflicting ideology between a writing judge and precedent is a valid instrument for creating the variable *same outcome*. For my purposes, the measure of judicial ideology is significantly correlated with the outcome in these 72 cases. When an authoring judge was appointed by a Republican, plaintiffs won just 43.48% (or 20 out of 46) of cases. Compare this to the 73.07% (or 19 of 26 cases) of cases won by plaintiffs when authoring judges were appointed by a Democratic governor. While not entirely conclusive, this difference is still significant at the 5% level. In addition, conflicting ideology and same outcome are significantly negatively correlated. This is illustrated in the two panels of Table 3. Table 3 shows that when the politics of the judge who wrote the opinion conflicts with the politics of the judge who wrote the precedent, the outcome is far less likely to be the same as in the precedent. When the politics of the judge writing the case is the same as the politics of the judge who wrote the precedent, the likelihood of a different outcome is greatly reduced. The negative correlation between the two variables is extremely strong. It is significant at the 0.01% level in both courts of appeal data (left) and supreme court data (right).

Conflicting politics	Same outcomes	
	0	1
0	18.51%	31.53%
1	31.85%	18.11%

BREAKDOWN OF THE 2,556 PAIRS WITH COURTS OF APPEAL PRECEDENTS

Conflicting politics	Same outcomes	
	0	1
0	18.31%	30.77%
1	30.77%	20.15%

BREAKDOWN OF THE 273 PAIRS WITH SUPREME COURT PRECEDENTS

TABLE 3: Breakdown of case-precedent observations illustrating the strong negative correlation between conflicting politics and same outcomes. When the politics of the judge conflicts with precedent, we are more likely to see a different outcome to the precedent. When the politics of the judge does not conflict with precedent, we are more likely to see the same outcome as in the precedent.

The key factor in determining validity of the instrument, however, is *conditional* correlation. That is, does the negative correlation between the outcomes of cases and the politics of the judge survive the introduction of other relevant right-hand side variables? The strength of this correlation—controlling for relevant covariates—is in-

85. This stage uses the formula:

$$\Pr(cited_{ik}) = a_2 + \beta_2 \cdot sameoutcome_{ik} + \gamma_2 \cdot W_{ik} + e_{ik}$$

icated in Table 4 where the first stage of the two-stage regression is shown. Table 4 suggests that if authoring judges have preferences that conflict with the outcomes of precedent, they are significantly less likely to hand down a decision similar to the precedent.<sup>86</sup>

VARIABLES	(1) <i>same outcome</i> COURTS OF APPEAL PRECEDENTS	(2) <i>same outcome</i> SUPREME COURT PRECEDENTS
Conflicting politics	-0.2831*** (0.0145)	-0.2424*** (0.0307)
Difference in years	-0.0071** (0.0033)	-0.0014 (0.0099)
Same type of contract	0.0202 (0.0191)	-0.0523* (0.0266)
Difference in context	0.0044 (0.0486)	-0.0583 (0.0984)
Constant	0.6895*** (0.0405)	0.7016*** (0.1700)
Observations	2556	273
R-squared	0.0830	0.0580

Robust standard errors in parentheses (clustered at precedent level)

\*\*\* p<0.01, \*\* p<0.05, \* p<0.1

TABLE 4: First stage of IV test predicting similarity of outcome of cases and precedents with politics.

Having said this, however, conflicting politics is by no means a perfect indicator of outcome in the cases in the database. Looking at the courts of appeal precedent data, just 8.30% of the variation in the *same outcome* factor can be explained by variation in politics and the controls.<sup>87</sup> Turning to the supreme court precedent data, just 5.80% of the variation is explained. The second requirement for validity of the instrument is that conflicting politics are not correlated with the error term in the original explanatory equation. While such a correlation could occur as a result of reverse causation, this is not an issue here because citations do not drive political appointment. Second, such a correlation could occur due to omitted factors (for example,

86. Large *t*-statistics are found for the coefficient on *conflicting politics* dummy, and large *f*-statistics for tests of joint significance are found in each regression. For more on *f*-statistics, see STOCK & WATSON, *supra* note 26, at 167–69. For the regression on courts of appeal precedents (Model 1), the *t*-statistic on the *conflicting politics* coefficient is -19.52 (*p* = 0.000) and the *f*-statistic test of joint significance is 287.65 (Prob > *f* = 0.000). For the regression on supreme court precedents (Model 2), the *t*-statistic on the *conflicting politics* coefficient is -7.90 (*p* = 0.000) and the *f*-statistic is 28.70 (Prob > *f* = 0.001).

87. These findings reflect the R<sup>2</sup> scores for each regression.

the quality of legal representation) that affect both citations and outcomes. Here, however, most such factors do not affect the political appointer of the judge in question. Of course, it is possible to imagine channels (other than through the outcome of the case at hand) by which the political ideology of the judge could affect citation practices; judges, for example, may be more likely to cite like-minded judges who were also more likely to be appointed by the same political party, *irrespective* of the outcome in the case at hand.<sup>88</sup> If this is true, then it weakens the validity of the instrument. In this particular sphere, however, no *prima facie* evidence affects citations through channels *other than* the outcome of the case. Controlling for relevant covariates, there is no significant correlation between the instrument: *conflicting politics* and *citations*.<sup>89</sup>

Using the predicted regressor, *same outcome (predicted)*, there is no significant causal relationship between citations and the element of the outcome that is explained by politics. These findings are shown in Table 5.

VARIABLES	(1) Pr( <i>cited</i> ) COURTS OF APPEAL PRECEDENTS	(2) Pr( <i>cited</i> ) SUPREME COURT PRECEDENTS
Same outcome (predicted)	0.0185 (0.0382)	-0.9973** (0.4088)
Difference in years	-0.0083*** (0.0015)	-0.0196** (0.0086)
Same type of contract	0.0820*** (0.0227)	-0.0321 (0.0540)
Difference of factors discussed	-0.0466* (0.0246)	-0.4570*** (0.1745)
Observations	2556	273

Robust standard errors in parentheses (clustered at precedent level)

\*\*\* p<0.01, \*\* p<0.05, \* p<0.1

TABLE 5: Second stage of IV test using predicted same outcome to estimate the probability of citation of precedent.

88. Cf. Workshop on Empirical Research in the Law, *On Tournaments for Appointing Great Justices to the U.S. Supreme Court*, 78 S. CAL. L. REV. 157, 175 (2004) (wondering whether “[k]nowing that citation counts will be taken as a measure of opinion quality, appellate judges might try to game the system, strategically citing ideologically similar colleagues, or engaging in a judicial form of log-rolling (‘I’ll cite you if you cite me’), in order to enhance the likelihood that they or like-minded colleagues will have a better chance of being selected for the Supreme Court”).

89. Indeed, as Table 5 shows, there is actually an insignificant positive correlation between politics and citations.

If the predetermined outcome in the case at hand *did* influence citation, one would expect positive, significant coefficients on *same outcome (predicted)*. The estimated equations do not yield such coefficients. The coefficient in Model 1, looking at citations of courts of appeal precedents, is positive but not significant, suggesting that there is no meaningful relationship between the two. The coefficient in Model 2, citations of supreme court precedents, is actually *negative* and significant at the 5% level. The fact that the explanatory variable is *same outcome (predicted)*, as generated by the ordinary least squares regression, means that the coefficients do not have the same meaningful interpretation as the coefficients in Table 2. In other words, because the independent variable was created for the model, one should not draw the conclusion that there is no relationship between outcome and citation for this particular model.

The findings concerning citations of supreme court precedents suggest that the element of the outcome that is explained by politics drives judges to cite precedents that gave the *opposite* outcome. For instance, when upholding arbitration provisions, Republican judges are more likely to cite the supreme court precedents that did not uphold the arbitration provision.

Another way to think about these findings is to ask the following questions. Are Republican judges more likely than Democratic judges to cite pro-defendant precedents when holding in favor of the defendant? The answer is no.<sup>90</sup> Are Democratic judges more likely than Republican judges to cite pro-plaintiff precedents when holding in favor of the plaintiff? Again, the answer is no.<sup>91</sup> This suggests that if there is extra-legal decision making in this sphere, it is not reflected in the citations of precedent. That is, judges do not appear to be cherry picking their precedents to justify extra-legal decisions.

## VI. DISCUSSION

### A. “Meaningful” Citations of Precedent

There is an issue of data quality when tracking the presence of citations, and I sought to address these issues in an additional step in

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90. In fact, Republican judges cite pro-defendant precedents less frequently than Democratic judges, but the difference is not statistically significant. Republican judges cite 5.68% of potential pro-defendant precedents when upholding the contract, whereas Democratic judges cite 10.63% of potential pro-defendant precedents in pro-defendant cases. The difference, however, is not statistically significant in simple univariate tests ( $p = 0.19$ ).

91. Democratic judges cite 17.85% of pro-plaintiff precedents when refusing to enforce the contract, whereas Republican judges cite 14.90% of pro-plaintiff precedents in pro-plaintiff cases. Again, this difference is not statistically significant ( $p = 0.28$ ).

my analysis, the results of which are discussed in this Section.<sup>92</sup> The mere presence of a citation may not necessarily capture anything resembling precedential value.<sup>93</sup> To account for the quality of the citation, I evaluate whether citations of courts of appeal precedents are meaningful and provide support for a specific proposition or whether they are simply throw-away citations. For example, a court may cite precedents to support procedural points such as whether the court can review new evidence or whether the court can review the unconscionability findings de novo, which may not capture information of interest to researchers focused on the evolution of law.<sup>94</sup> Nearly 10% (or 23 of the 239 citations) of courts of appeal precedents are of this type.<sup>95</sup>

Citations may simply cite general propositions about the substance of the law but not specifically support the decision in the case at hand.<sup>96</sup> For example, providing a citation for the proposition that unconscionability is a question of law for the court to decide might not justify the outcome in any meaningful sense.<sup>97</sup> Citations of this type can be found in 79 of the 239 citations of courts of appeal precedents.

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92. For more on this distinction and on the meaningfulness of different types of citations, see Charles A. Johnson, *Citations to Authority in Supreme Court Opinions*, 7 LAW & POL'Y 509, 512 (1985), in which the author eliminates procedural citations from the database used to assess the nature of citations to precedent by the United States Supreme Court, and Walsh, *supra* note 29, at 342, in which the author distinguishes "strong" from "weak" citations and creates a dataset to evaluate the use of precedent that includes cites only where cited opinions deal directly with the doctrine in question.

93. *Cf.* Frank B. Cross & Stefanie Lindquist, *Judging the Judges*, 58 DUKE L.J. 1383, 1391 (2009) (arguing that the "citation test" is controversial because citations "vary in importance" and noting that "some [citations] are centrally relied upon to reach decisions, whereas others are fodder for only a string cite").

94. *Cf. id.* at 1392 ("[A] judge setting forth the first explication of standards on a procedural issue such as summary judgment may be repeatedly cited simply because the issue so commonly arises in litigation.").

95. For example, in *Cohen v. DirecTV, Inc.*, 48 Cal. Rptr. 3d 813 (Ct. App. 2006), *aff'd on other grounds*, 101 Cal. Rptr. 3d 37 (Ct. App. 2009), the court cites *Fittante v. Palm Springs Motors, Inc.*, 129 Cal. Rptr. 2d 659, 664 (Ct. App. 2003), for the authority that "the validity and unconscionability of an arbitration agreement are matters of law subject to de novo review where, as here, no material facts are in dispute." *Cohen*, 48 Cal. Rptr. 3d at 816.

96. *Cf.* David C. Vladeck, *Keeping Score: The Utility of Empirical Measurements in Judicial Selection*, 32 FLA. ST. U. L. REV. 1415, 1432 (2005) (arguing that "cases are often cited not because of the trenchancy or novelty of their judicial analysis but because of either their memorable language or their ability to distill a complicated legal issue down to a multifactor test that judges and lawyers embrace for the sake of convenience").

97. Specifically, for example, see *Abramson v. Juniper Networks, Inc.*, 9 Cal. Rptr. 3d 422 (Ct. App. 2004), in which the court cites *Mercurio v. Superior Court*, 116 Cal. Rptr. 2d 671, 675-76 (Ct. App. 2002), for the general proposition that "courts will refuse to enforce unconscionable arbitration provisions." *Abramson*, 9 Cal. Rptr. 3d at 435.

Even if an opinion does cite a specific proposition with regards to the substance of the law of unconscionable provisions, there may be factors that mitigate the value of the citation. For instance, points of substance may be relegated to the footnotes of a decision.<sup>98</sup> There is footnoted support for specific propositions in 10 observations. Alternatively, the citation may be grouped with a number of other precedents.<sup>99</sup> These “see also” types of citations support substantive and specific propositions in 31 observations.<sup>100</sup>

To deal with citation quality measurement, I simply created a new variable (*citequality*) that takes the value 1 if a case provides a discussion of the facts of the precedent, if the case discusses the precedent at length, or if the precedent provides support for a specific proposition. The variable takes the value 0 if the citation is not meaningful such as when the citation is used to support a procedural point or a general proposition. Further, if the citation is merely a “see also” type of citation or is relegated to a footnote, it is regarded as equal to meaningless and coded as 0. Of the 239 citations, 102 (or 42.68%) are coded as “meaningful” citations.<sup>101</sup>

These data are used to supplement my findings regarding when precedents are cited. I ask, *conditional on a court of appeal precedent being cited*, are we more likely to see in-depth discussions of precedents that have the same outcome as the case in hand? The answer is no. I

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98. An example of this can be found in *Independent Ass'n of Mailbox Center Owners, Inc. v. Superior Court*, 34 Cal. Rptr. 3d 659 (Ct. App. 2005), in which the court cites *Stirlen v. Supercuts, Inc.*, 60 Cal. Rptr. 2d 138, 143 (Ct. App. 1997), for the specific proposition that “[a]n arbitration clause that is so one-sided as to deprive the weaker party of substantive claims, or the right to punitive damages recovery, is against public policy and unconscionable.” *Mailbox Ctr. Owners*, 34 Cal. Rptr. 3d at 672 n.7. Some readers may be concerned that this categorization of footnotes is incorrect. Even if I classify such citations as “meaningful,” the results do not change.

99. Cf. Cross & Lindquist, *supra* note 93, at 1391 (arguing that relying on citations as a measure of judicial quality is controversial because some citations “are fodder for only a string cite”).

100. For example, in *Gutierrez v. Autowest, Inc.*, 7 Cal. Rptr. 3d 267 (Ct. App. 2003), *modified*, No. A098704, 2004 Cal. App. LEXIS 16 (Ct. App. Jan. 8, 2004), the court quotes *Little v. Auto Stiegler, Inc.*, 63 P.3d 979, 983 (Cal. 2003), and cites *Fittante v. Palm Springs Motors, Inc.*, 129 Cal. Rptr. 2d 659, 670 (Ct. App. 2003), and *Flores v. Transamerica HomeFirst, Inc.*, 113 Cal. Rptr. 2d 376, 381 (Ct. App. 2001), as authority for the specific proposition that “[t]he procedural element of an unconscionable contract generally takes the form of a contract of adhesion.” *Gutierrez*, 7 Cal. Rptr. at 275.

101. I tried alternative dependent variables such as using a “score” that increases with each characteristic of a meaningful citation (for example, discussion of facts of a precedent) and decreases with each characteristic of a nonmeaningful citation (for example, a “see also” type of citation). These results are not reported but are of the general flavor of the reported results. The direction and significance of the coefficients do not change.

analyzed the quality of the 239 citations of courts of appeal precedents.<sup>102</sup> The results are presented in Model 1 of Table 6.

The subsample of actual citations suggests that the higher quality citations of courts of appeal precedents are more likely to be found when the case at hand delivers a different outcome—as evidenced by the significant negative coefficient on *same outcome*. This suggests that, *conditional on a precedent being cited*, greater effort is exerted by the court to distinguish a case from precedents with the opposing outcome.

I also ran the regression with *citequality* as the dependent variable using all 2,556 potential citations in Model 2. This assumes the citations that are not meaningful have the same weight or value as a non-citation. Here, in contrast with Model 1, there is no statistically significant relationship between the outcome and citation quality.

VARIABLES	(1)	(2)
	Pr( <i>citequality</i> ) CONDITIONAL UPON ACTUAL CITATION OF COURTS OF APPEAL PRECEDENT	Pr( <i>citequality</i> ) ALL POTENTIAL CITATIONS OF COURTS OF APPEAL PRECEDENT
Same outcome	-0.1619** (0.0631)	0.0027 (0.0068)
Difference in year	-0.0202** (0.0092)	-0.0041*** (0.0008)
Same type of contract	-0.0527 (0.0852)	0.0185** (0.0075)
Difference in factors discussed	0.1488 (0.1209)	-0.0086 (0.0128)
Observations	239	2556

Robust standard errors in parentheses (clustered at precedent level)

\*\*\* p<0.01, \*\* p<0.05, \* p<0.1

TABLE 6: (1) Conditional on a precedent being cited, judges are more likely to provide meaningful citations of precedents with different outcomes to the case to those precedents with the same outcome. (2) The outcomes of cases are not correlated with outcomes of precedents that have “meaningful” citations.

### B. Examining Citations of Precedent Within Unpublished Opinions

The dataset was extended once more to include unreported, unpublished opinions from the California Courts of Appeal. Unpublished opinions also cited published precedents. Data was collected

102. As above, I used a probit regression analysis here because the estimator is more efficient than the linear estimator. For a more specific explanation, see STOCK & WATSON, *supra* note 26, at 195–236, 302–07, 309–13.

on an additional 102 unpublished decisions from the same time period as the 72 reported California Courts of Appeal decisions and the 7 California Supreme Court decisions. This new pairwise dataset of unpublished opinions contains 5,949 potential citations of courts of appeal precedents and 554 potential citations of supreme court precedents.

If judges cite precedents with a view to shaping the law in the future, I expect that the differential between cited and non-cited precedents will be greater in cases where it matters most.<sup>103</sup> Published opinions are contributions to legal capital and are more likely to influence the future development of the law.<sup>104</sup> Therefore, I expect that unpublished opinions (1) will contain fewer citations than reported cases and (2) will more accurately reflect the state of the law, as indicated by weaker correlations between outcomes of cases and the precedents on which they rely.<sup>105</sup> This is exactly what the data suggest.

There is evidence of fewer citations in unpublished decisions: Just 7.93% of potential citations of courts of appeal precedents and 41.34% of potential citations of supreme court precedents are actually cited. Using the same form of analysis from Part IV on this new sample, there is no positive correlation between outcomes of cases and outcomes of cited precedents. The results are summarized in Table 7. Pro-plaintiff cases do not disproportionately cite pro-plaintiff precedents. The only control variable of significance is the amount of time between decisions. Notably, even with unpublished opinions, courts lean toward citing recent precedents.

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103. Cf. Choi & Gulati, *supra* note 20, at 99 (“[I]f judges do act with an ideological bias, we expect this bias to appear where ideology matters the most: published opinions that affect the development of precedent.”).

104. See *id.* (“Unpublished opinions, in contrast, provide judges with little ability to affect the development of the law.”); see also Orley Ashenfelter et al., *Politics and the Judiciary: The Influence of Judicial Background on Case Outcomes*, 24 J. LEGAL STUD. 257, 259 (1995) (discussing the representativeness of using published opinions only).

105. See John T. Wold, *Going Through the Motions: The Monotony of Appellate Court Decision-making*, 62 JUDICATURE 58, 63 (1978) (arguing that judges “take ‘shortcuts’ in writing opinions they have informally slated for nonpublication,” by writing shorter opinions, citing applicable cases without creativity, failing to check precedents “for accuracy of quotation or citation,” and being less critical of proposed unpublished opinions).

VARIABLES	(1)	(2)
	Pr( <i>cited</i> ) COURTS OF APPEAL PRECEDENTS	Pr( <i>cited</i> ) SUPREME COURT PRECEDENTS
Same outcome	-0.0006 (0.0009)	0.0570 (0.0401)
Difference in years	-0.0001** (0.0001)	-0.0241** (0.0112)
Same type of contract	0.0005 (0.0010)	0.0104 (0.1172)
Difference in factors discussed	-0.0016 (0.0028)	-0.1070 (0.2204)
Observations	5849	554

Robust standard errors in parentheses (clustered at precedent level)

\*\*\* p<0.01, \*\* p<0.05, \* p<0.1

TABLE 7: Multivariate regression indicating no correlation between outcomes of cases and outcomes of cited precedents when focusing on citations within unpublished opinions only.

C. *Behind the Results*

What conclusions can be drawn from the data? I should be clear about the limitations of my analysis. The following question has been answered: Do judges ex post rationalize decisions made extra-legally? Pro-plaintiff decisions are more likely to cite pro-plaintiff precedents. Pro-defendant decisions are more likely to cite pro-defendant precedents.<sup>106</sup> While evidence of extra-legal decision making appears in the data, the instrumental variable analysis suggests that judges are not simply cherry picking precedents. Or, at least, they are not cherry picking *to the extent* that ex post rationalization is a matter of selecting precedents with the same outcome.

I have not said anything about the complement to the question. The analysis does not necessarily suggest that the precedent informs decisions, nor does it necessarily suggest that cited precedents are the most influential or highest quality precedents. The data do not speak to such questions. As suggested above, there may be other extra-legal components to the decision making process—not captured by politics—that citations would be required to ex post justify.

An alternative explanation for the results is that ex post justification does not manifest itself in positive correlation of outcomes. Framing the decision within the law may not require judges to cite precedents that have the same outcome. Judges may make greater effort when ex post justifying extra-legal decisions and may therefore

106. See discussion *supra* Part IV.A–B.

distinguish cases with greater frequency. This can help explain the result that, conditional on citing a precedent, the quality of that citation improves if the precedent produced a different outcome from the case at hand. It may further help explain the counterintuitive correlation found between politically explained decisions and the citation behavior of supreme court precedent.

Overall in this sample of cases, pro-plaintiff precedents were more likely to be cited than pro-defendant precedents.<sup>107</sup> This citation practice is true for Republican-appointed judges as well as Democratic-appointed judges.<sup>108</sup> Perhaps in this narrow area of law and in this particular jurisdiction, *ex post* justification of decisions is not found because judicial opinions may simply respond to the plaintiff's case. That is, the legal opinion simply asks whether the plaintiff's case is in line with plaintiff-friendly precedents or whether it can be distinguished from plaintiff-friendly precedents. The later cases in the sample are disproportionately written by Republican-appointed judges because of the preponderance of Republican governors in California since 1983.<sup>109</sup> The *conflicting politics* dummy is therefore more likely to be triggered in cases where precedents have held a defendant liable.

Finally, the results may be driven by the choice of law on which I have focused. For example, the citation behavior of judges may vary depending on how often the issue is litigated. Many cases involving unconscionability reach the courts of appeal level in California, but not all published precedents can be reviewed and cited. In this narrow sphere of law, only a small proportion of potential citations—on average around 10%—are actually cited in any given opinion. Indeed, 21 of the 72 published courts of appeal precedents are *never* cited by other reported cases in the pool.<sup>110</sup> While there are a number of cases in this narrow sphere, the fact that there are only 72 pub-

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107. This is seen in the statistics: 69.04% of actual citations of courts of appeal precedents and 86.05% of actual citations of supreme court precedents cited a precedent where the plaintiff won.

108. Focusing only on the courts of appeal data, Republican-appointed judges cited 11.56% of potential pro-plaintiff precedents and just 5.49% of potential pro-defendant precedents, while Democratic-appointed judges cited 17.88% of potential pro-plaintiff precedents and just 6.73% of pro-defendant precedents.

109. California has had only one Democratic governor since 1983.

110. Six of these precedents, however, are cited by unreported decisions. All 7 of the supreme court precedents are cited at least once. For more on the frequency of citations and precedential value, the specific indication that over 400,000 United States cases have been cited only once, and a hypothesis as to why some cases are preferred as authority, see Thomas A. Smith, *The Web of Law* 37–41 (Univ. of San Diego Sch. of Law, Law & Econ. Research Paper Series, Paper No. 8, 2006), available at <http://law.bepress.com/sandiego/lwps/le/art8>.

lished cases—and therefore only 72 raw observations in my dataset—may cause some concern. From a statistical perspective, this is a relatively small  $n$ . This may, of course, affect some of my statistical results.

## VII. CONCLUSION

Judges are afforded a great deal of discretion in determining which precedents are relevant and should be followed.<sup>111</sup> This Article has presented a framework for examining citation discretion and has tested hypotheses of ex post justification using a sample of cases that investigate the enforcement of contracts in California. Based on this study, there is no evidence that judges are ex post rationalizing extra-legal decisions.<sup>112</sup>

First, the hypothesis that the cited precedents accurately reflected the pool of precedents on point is rejected.<sup>113</sup> The results indicate that the outcomes of cited precedents are positively correlated with the outcome in the case at hand. This finding is robust in light of several controls included in the analysis and the limited investigation of particular subsets of my data. Second, the instrumental variable approach suggests that judges do not merely use citations to ex post rationalize decisions.<sup>114</sup> While the instrument satisfies the criteria for validity, questions remain about whether there are stronger explanations for the extra-legal components of the decision than politics. There is evidence of extra-legal decision making as political preferences are significantly correlated with the outcome, but this extra-legal component does not appear to manifest itself in justifications of the decision that cite precedents with the same outcome. Beyond the primary findings, interesting dimensions of citation practice were also uncovered such as the strong influence of time and context.<sup>115</sup>

This study, focusing on just one law in just one jurisdiction, does not capture the entire scope of the phenomenon of judges potentially cherry picking precedents. It may be seen more broadly in other spheres of law. The framework presented here can be easily applied to other areas and perhaps results will differ.

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111. *See supra* text accompanying notes 5–14.

112. *See supra* Parts IV, V.

113. *See supra* Part IV.

114. *See supra* Part V.

115. *See supra* Part III.B.4.a–b.

APPENDIX

*Key terms searched for in determining the context of the decisions*

Complexity of the contract	Weakness of nondrafter	Negation of consent	Neutrality of arbitration provision
Ambiguity	advantage	accept	AAA
ambiguous	advantaged	accepted	adequate discovery
ambiguously	advantageous	acknowledged	all disputes
boiler plate	advantageously	acknowledged	American Arbitration
boilerplate	advantages	acknowledges	Association
bold	aged	adhere	approved by SEC
bold-print	asymmetric	adhesion	arbitral costs
buried	asymmetry	adhesive	arbitration cost
bury	businessman	agreed	arbitrator's fee
capitalize	businesswoman	agrees	bias
capitalized	capability	alternative	bilateral
capitals	capable	arm's length	bilaterally
clear agreement	disparate	attention	class action
clearly states	disparity	aware	class wide
complicated	educated	bill insert	class-action
conceal	education	bill stuffer	class-wide
confuse	elderly	choice	conflict of interest
confused	emigrant	coerce	conflict-of-interest
confuses	emigrate	coercive	consolidate
confusing	emigrated	cognizant	consolidation
conspicuous	equal bargaining	consent	cost of arbitration
conspicuously	equitable	consented	cost provision
different font	foreign	consents	cost to arbitrate
difficulty	foreigner	consult	costs of arbitration
disguise	ill health	deceive	costs provision
disguised	ill-health	deceived	costs to arbitrate
disguises	illiterate	deceptive	curtail (w/in3) rights
double space	immigrant	disclose	disqualify
double spaced	immigrate	disclosed	due process
double-space	immigrated	disclosure	due-process
double-spaced	inability	discuss	evenhanded
fine print	incapability	discussed	expensive
fine-print	incapable	discusses	external
handbook	inequality	duress	imbalance of rights
hidden	inequitable	execute	impartial
important	inferior	executed	integrity
inconspicuous	literate	executes	internal
inconspicuously	mentally	explained	JAMS
jargon	migrant	explanation	limitations of remedies
large font	migrants	force	limits remedies
legalese	migrate	forced	mutual
minuscule	migrated	give and take	mutuality
miscellaneous	old	hurried	NAF
ordinary	poorly	hurry	NASD
page(s)	professional	impose	National Arbitration Forum
plain language	sick	imposed	neutral
pre printed	sophisticated	imposition	New York Stock Exchange
preprinted	sophistication	induce	non mutual
prolix	Spanish	induced	non neutral
prominence	strength	initial	non-mutual
prominent	strong bargaining	initialed	non-neutral
same type	superior bargaining	involuntarily	NYSE
separate page	terminally	involuntary	one sided
shrink wrap	uneducated	knowing	one-sided
shrink wrapped	unemployed	knowledge	one-sidedness
shrink-wrap	unequal	knowledgeable	out-of-state forum
shrink-wrapped	unsophisticated	mislead	partial
single space	weak	misleading	reciprocal
single spaced	young	misled	reciprocity
single-space		misrepresent	relinquishment
single-spaced		misrepresentation	restrictions on remedies
small		misrepresented	retired California superior
standardized		mistake	court judge
straightforward language		mistook	retired judge
surprise		misunderstand	retired superior court judge
tiny		misunderstanding	safeguard
unambiguous		misunderstood	safeguards
unambiguously		modified	SEC approved
unclear		modify	SEC's approval
underline		negotiable	SEC's imprimatur
underlined		negotiate	small claims court

Complexity of the contract	Weakness of nondrafter	Negation of consent	Neutrality of arbitration provision
unintelligible upper case upper-case vague verbiage		negotiation non-disclosure nonnegotiable opt in opt out opted opt-in option opt-out pressure pressured read reading signatory signature signed silence silent threaten threatened told understand understanding understood voluntarily voluntary volunteer	unbiased unilateral unilaterally