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## Slipping Away from Justice: the Effect of Attorney Skill on Trial Outcomes

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# Slipping Away from Justice: The Effect of Attorney Skill on Trial Outcomes

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## I. INTRODUCTION

“It’s disgusting what he did, it’s disgusting . . . his ‘Dream Team’—‘Scheme Team’ maybe is more accurate.”<sup>1</sup>

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1. David Margolick, *Simpson Tells Why He Declined To Testify as Two Sides Rest Case*, N.Y. TIMES, Sept. 23, 1995, at A1.

Fred Goldman blamed the defense attorneys when a Los Angeles jury found O.J. Simpson not guilty of murdering his son, Ron Goldman, and Nicole Brown Simpson on October 3, 1995. Yet Goldman was not the only one who blamed the defense attorneys for the acquittal; much of the media agreed that Simpson was guilty and had escaped his rightful punishment. As one *New York Times* reporter lamented, "To watch Mr. Simpson slip away from justice . . . was an infuriating sight."<sup>2</sup> People who believed in Simpson's guilt cited Johnnie Cochran's decision to "play the race card"<sup>3</sup> and his clever catch phrases like "if it doesn't fit, you must acquit."<sup>4</sup> Others blamed the prosecuting attorneys. On the day after the verdict, author Scott Turow described the prosecutors as "doomed from the start" for their use of "ugly tactics that . . . aroused suspicions about the criminal justice system among members of racial minorities in Los Angeles and elsewhere."<sup>5</sup>

Yet O.J. Simpson is not the only defendant who—according to popular opinion—has slipped away from justice because of his attorneys' skill. A jury acquitted the late Michael Jackson of his child molestation charges in 2005.<sup>6</sup> That same year, actor Robert Blake escaped charges of murdering his wife, Bonny Lee Bakley.<sup>7</sup> And just two years earlier, a jury acquitted New York millionaire Robert Durst of murdering his neighbor, Morris Black.<sup>8</sup> All three men had very expensive, well-known defense attorneys, and all three men faced similar accusations of slipping away from justice in the press after their acquittals. More recently, Mary Winkler, a preacher's wife from Selmer, Tennessee who killed her husband and fled with her children to the Alabama coast, endured the same scrutiny from the popular

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2. Frank Rich, *Journal: The L.A. Shock Treatment*, N.Y. TIMES, Oct. 4, 1995, at A21.

3. Seth Mydans, *Not Guilty: The Lawyers: In the Joy of Victory, Defense Team is in Discord*, N.Y. TIMES, Oct. 4, 1995, at A11.

4. David Margolick, *Simpson's Lawyer Tells Jury That Evidence 'Doesn't Fit'*, N.Y. TIMES, Sept. 28, 1995, at A1.

5. Scott Turow, *Simpson Prosecutors Pay for Their Blunders*, N.Y. TIMES, Oct. 4, 1995, at A21.

6. See Paul Dale Roberts et al., Letters to the Editor, *Prosecutors Fumble Another Celebrity Case*, USA TODAY, June 15, 2005, at A10 (criticizing the verdict and discussing the prosecutors' mistakes in the Michael Jackson case).

7. See Greg Risling, *Blake Not Guilty of Wife's Murder: Prosecution 'Couldn't Put the Gun in his Hand,' Jury Foreman Says*, CHI. SUN-TIMES, Mar. 17, 2005, at 3 (detailing the shortcomings of the prosecution in the Robert Blake case).

8. See Gary Cartwright, *The Verdict: The Safe Money Had Robert Durst Getting Convicted of Murder in Seconds Flat. How on Earth Did He Get Off? Two Words: Dick DeGuerin*, TEXAS MONTHLY, Feb. 2004, at 54, 56 (discussing the role of Dick DeGuerin in Robert Durst's acquittal).

press during her trial. Despite being accused of first-degree murder, her “Dream Team” of defense attorneys made “murder no longer an issue.”<sup>9</sup> Instead, the jury convicted Winkler of voluntary manslaughter, and the judge sentenced her to only sixty-seven days in prison. As one journalist sarcastically noted after the verdict, “Mary Winkler’s defense lawyers did just what they had to do to convince a jury not to convict her of murder, even though she shot her sleeping preacher husband in the back with a shotgun.”<sup>10</sup> Even Winkler’s own defense attorney said after trial that “the verdict was *most probably* just.”<sup>11</sup>

Clearly, much of the media believes that an attorney can decide a case. Get a good enough attorney, the story goes, and you can get off on anything. Yet the belief in the power of a good attorney extends far beyond popular opinion—and all the way to the Supreme Court. In many opinions, Justices have expressed concern about the consequences of weak representation.<sup>12</sup>

But just how important is a good attorney? Can a skillful attorney actually change the verdict? More importantly, in criminal trials, can a good defense attorney let guilty people go free, or can a good prosecutor send innocent people to jail? Every day, as more high-profile defendants find themselves in court, the anecdotal evidence of this attorney skill effect continues to mount. Yet no one has decisively answered these questions—not only for high-profile defendants, but for the everyday defendant as well.

This Note will argue that a skillful defense attorney is not as powerful as popular opinion would lead us to believe. Here, I define skill as the qualities that an attorney brings to the courtroom independent of his case’s strength, such as rhetorical abilities, tactical strategies, and knowledge of the law. Regardless of their skill,

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9. Lawrence Buser, *Dream Team Took Jury Into Nightmare, Emerged With Murder No Longer an Issue*, COMMERCIAL APPEAL, Apr. 21, 2007, at A1.

10. *Id.*

11. *Wife Guilty of Manslaughter in Minister’s Killing*, N.Y. TIMES, Apr. 20, 2007, at A17 (emphasis added).

12. See, e.g., *Strickland v. Washington*, 466 U.S. 668, 710 (1984) (Marshall, J., dissenting) (noting that the “difficulties of estimating prejudice after the fact are exacerbated by the possibility that evidence of injury to the defendant may be missing from the record precisely because of the incompetence of defense counsel”); *McMann v. Richardson*, 397 U.S. 759, 771 (1970) (holding that “if the right to counsel guaranteed by the Constitution is to serve its purpose, defendants cannot be left to the mercies of incompetent counsel, and that judges should strive to maintain proper standards of performance by attorneys who are representing defendants in criminal cases in their courts”); *Powell v. Alabama*, 287 U.S. 45, 68–69 (1932) (discussing the consequences of inadequate access to counsel).

criminal defense attorneys do not have a statistically significant effect on the verdict or sentencing outcomes. Prosecuting attorneys, on the other hand, *can* influence trial outcomes. A jury is more likely to convict a defendant when the prosecutor has a high level of skill. Although important for many reasons, prosecutorial skill is particularly critical since the prosecution has the burden of proof in a criminal trial. This outcome that emphasizes the impact of prosecutorial skill—running so contrary to our everyday beliefs—suggests that we have been focusing on the wrong side. Just like Fred Goldman, we are quick to blame the defense attorneys when we think a high-profile defendant has slipped away from justice. For more low-profile defendants, we are overly preoccupied with the adequacy of, and the disparities in, defense attorneys. Yet we should really be concerned about the disparities in prosecutors.

To demonstrate the importance of prosecuting attorney skill, Part II of this Note first considers previous literature from law and other disciplines on the impact of attorney skill. Part III discusses the data set used to conduct this study, and Part IV outlines the model of the attorney skill effect. Part V gives the results of the data analysis and demonstrates the effect of prosecuting attorney skill on trial outcomes—and the lack of effect of defense attorney skill on trial outcomes. Part VI argues that public attention should shift away from defense attorneys and onto prosecutors. If we expect defendants to receive a fair trial, we need to devote more resources to ensuring that prosecutors are well qualified and adequately trained to eliminate the disparities between them. Part VII concludes by relating these results to the attorney skill effect so often discussed in the popular press.

## II. PREVIOUS WORK ON THE IMPACT OF ATTORNEY SKILL

Scholars from many disciplines have questioned the impact of attorney skill for years. This Part will chronologically explore the evolution of their efforts in the fields of law, psychology, and economics. For scholars in every field, two fundamental problems have stood in the way of exploring the attorney skill effect. First, and most obviously, measures of attorney skill are very difficult to quantify—and even more difficult to obtain. Unlike quarterbacks, practicing attorneys do not have ratings by which to evaluate their seasons of trial work. Second, even though we can easily observe that trends in trial outcomes differ between attorneys, it is almost impossible to attribute these differences solely to the attorney's skill. Variables such as location and practice area, as well as personal characteristics like

race and gender, could feasibly create systematic differences in attorney trial outcomes.<sup>13</sup>

Of all these other variables, perhaps the most significant is a process commonly referred to as attorney matching, where attorneys pair with clients based on the strength of their case.<sup>14</sup> Due to attorney matching, an attorney may have a high trial success rate that has nothing to do with his skill. For example, a famous defense attorney may succeed in getting his clients acquitted more frequently. But this attorney's high success rate may be simply due to the fact that the attorney only takes cases that he is likely to win. Similarly, clients may spend more money on high-priced attorneys when they are innocent in order to ensure their acquittal and public vindication. On the other side of the criminal justice system, more experienced prosecutors may be assigned to weaker cases in order to increase the probability that the accused criminal will be convicted and brought to justice.<sup>15</sup> Criminal trial outcomes and the attorneys working on each side are easily observable, but without more information about each case—such as the type of case, strength of the case, defendant characteristics, and victim characteristics—researchers cannot separate out the effects of each attorney's skill from the effects of attorney matching.<sup>16</sup>

Despite these data problems, scholars have attempted to untangle the attorney skill effect<sup>17</sup> because the question of attorney skill goes to the very heart of our criminal justice system.<sup>18</sup> The Sixth

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13. For example, a defense attorney may practice in a location with highly empathetic juries, leading to systemically better results for this attorney than a similar attorney practicing in a less defense-friendly jurisdiction. Similarly—whether fair or not—the race or gender of a defense attorney could plausibly affect trial outcomes. Juries in areas of the country highly sensitized to race may take greater note of the race of all attorneys involved.

14. See David S. Abrams & Albert H. Yoon, *The Luck of the Draw: Using Random Case Assignment to Investigate Attorney Ability*, 74 U. CHI. L. REV. 1145, 1146–48 (discussing the problem of attorney matching in evaluating the effect of attorney skill).

15. See *id.* at 1146 (explaining that “the nonrandom pairing of attorney and client in most cases makes it difficult, if not impossible, to distinguish between attorney ability and case selection”).

16. See *id.* at 1147 (noting that “case outcomes may reflect the matching process between clients and attorneys as much as the ability of the attorneys who represent their clients”).

17. Here, and throughout the paper, the “attorney skill effect” simply refers to the effect of attorney skill on trial outcomes. Attorney skill is defined in Part I as the qualities that an attorney brings to the courtroom independent of his case's strength, such as rhetorical abilities, tactical strategies, and knowledge of the law.

18. *E.g.*, Abrams & Yoon, *supra* note 14, at 1147–49 (investigating whether systematic differences in defense attorney ability exist and their effects on trial outcome); Floyd Feeney & Patrick G. Jackson, *Public Defenders, Assigned Counsel, Retained Counsel: Does the Type of*

Amendment's guarantee of the right to counsel<sup>19</sup> not only requires courts to allow defendants adequate access to counsel<sup>20</sup> but also commands them to *provide* indigent defendants with counsel.<sup>21</sup> More importantly, the Sixth Amendment requires that counsel do more than just appear to represent a client. In *United States v. Cronin*, the U.S. Supreme Court held that "if counsel entirely fails to subject the prosecution's case to meaningful adversarial testing, then there has been a denial of Sixth Amendment rights that makes the adversary process itself presumptively unreliable."<sup>22</sup> Yet long before the *Cronin* decision, the Supreme Court indicated that representation by a competent attorney is necessary to the proper functioning of our criminal justice system—where the accused are innocent until proven guilty, the innocent are acquitted, and the guilty are brought to justice.<sup>23</sup> As Justice George Sutherland reasoned in his famous opinion in *Powell v. Alabama*:

The right to be heard would be, in many cases, of little avail if it did not comprehend the right to be heard by counsel. Even the intelligent and educated layman has small and sometimes no skill in the science of law. . . . He requires the guiding hand of counsel at every step in the proceedings against him. Without it, though he be not guilty, he faces the danger of conviction because he does not know how to establish his innocence. If that be true of men of intelligence, how much more true is it of the ignorant and illiterate, or those of feeble intellect.<sup>24</sup>

Implicit in Sutherland's discussion of the right to counsel was his belief in the right to *effective and skillful* counsel. After all, an incompetent attorney would not be much of a "guiding hand."<sup>25</sup> According to Sutherland, an effective and skillful attorney could actually change the trial outcome by removing the "danger of conviction" of an innocent person.<sup>26</sup>

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*Criminal Defense Counsel Matter?*, 22 RUTGERS L.J. 361, 407–10 (1991) (exploring the differences between different types of defense attorneys); Daniel Linz et al., *Attorney Communication and Impression Making in the Courtroom: Views From Off the Bench*, 10 L. & HUM. BEHAV. 281, 281–301 (1986) (designing an experiment to test the effect of prosecution and defense attorney skill on trial outcomes).

19. U.S. CONST. amend. VI.

20. *Powell v. Alabama*, 287 U.S. 45, 71–73 (1932).

21. *Gideon v. Wainwright*, 372 U.S. 335, 343–45 (1963).

22. 466 U.S. 648, 659 (1984).

23. See, e.g., *McMann v. Richardson*, 397 U.S. 759, 771 (1970) (arguing that the Sixth Amendment right to counsel includes a right to *competent* counsel); *Powell*, 287 U.S. at 68–69 (discussing the consequences of inadequate access to counsel).

24. *Powell*, 287 U.S. at 68–69.

25. *Id.* at 69.

26. *Id.*

Concerns about the attorney skill effect predated even Justice Sutherland, however, and studies of attorney skill began as early as 1919.<sup>27</sup> Many of the earliest studies did not use controls: the authors simply compared trial outcomes between retained and appointed defense attorneys and drew inferences about their relative skill levels.<sup>28</sup> Because these studies were rudimentary at best—unable to control for any attorney-matching problems, strength of the underlying case, and even sometimes the type of case—their results often contradicted each other. In 1991, Floyd Feeney and Patrick G. Jackson reviewed the twelve previous studies that had compared the trial outcomes achieved by public defenders, court-appointed counsel, and privately retained counsel. Many of the studies they reviewed reached opposite results, and the authors concluded that “[t]he best research to date indicates that type of defense counsel . . . is not an important determinant of case outcomes.”<sup>29</sup> As a result, the authors endorsed the explanation of Dallin Oaks and Warren Lehman’s 1968 study, which concluded that criminal attorneys were too heterogeneous to analyze as a group:

The ranks of private lawyers doing criminal work include the few top men in the city, baffled family lawyers whose clients have fallen into the hands of the police, hacks who find their clients in the halls of the Criminal Courts building, corporation lawyers whose clients ask them to perform the work as a service and young men on the way up who mix criminal and civil practice . . . .<sup>30</sup>

In other words, the small sample size and the large variation in personal characteristics drove the repeatedly inconclusive results about the attorney skill effect.<sup>31</sup>

Around the time of Feeney and Jackson’s article, scholars from outside the legal field began to explore the attorney skill effect. In

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27. See REGINALD HERBER SMITH, *JUSTICE AND THE POOR* 123 (1919) (finding that retained counsel had higher acquittal rates than public defenders, but public defenders had better probation outcomes).

28. See, e.g., PETER W. GREENWOOD ET AL., *PROSECUTION OF ADULT FELONY DEFENDANTS IN LOS ANGELES COUNTY: A POLICY PERSPECTIVE* 52–56 (1973), available at <http://www.rand.org/pubs/reports/R1127/> (finding that assigned counsel had better acquittal rates, but public defenders had better sentencing outcomes); DALLIN H. OAKS & WARREN LEHMAN, *A CRIMINAL JUSTICE SYSTEM AND THE INDIGENT: A STUDY OF CHICAGO AND COOK COUNTY* 162–63 (1968) (finding that private counsel and publicly appointed counsel had similar trial outcomes); LEE SILVERSTEIN, *DEFENSE OF THE POOR IN CRIMINAL CASES IN AMERICAN STATE COURTS* 20–29 (1965) (finding retained counsel had better trial outcomes than appointed counsel).

29. Feeney & Jackson, *supra* note 18, at 407.

30. *Id.* at 409 (quoting Dallin H. Oaks & Warren Lehman, *Lawyers for the Poor*, in *THE SCALES OF JUSTICE* 91–93 (Abraham S. Blumberg ed., 1970)).

31. *Id.*



1986, psychologists Daniel Linz, Steven Penrod, and Elaine McDonald recognized the scarcity of good data to test the effect of attorney skill on trial outcomes,<sup>32</sup> so they designed an experiment to create their own data.<sup>33</sup> The authors solicited undergraduates to observe fifty trials.<sup>34</sup> After the trials ended, the undergraduates completed a survey rating each attorney's courtroom behaviors, such as articulateness, enthusiasm, and legal informativeness.<sup>35</sup> The authors also contacted both the jurors and the attorneys and asked them to fill out a similar questionnaire rating each attorney's performance.<sup>36</sup> The undergraduates systematically gave higher marks to the prosecutors.<sup>37</sup> Moreover, prosecutors' self-assessments closely matched the jurors' ratings, while defense attorneys vastly overrated their courtroom skills.<sup>38</sup> None of these skill ratings, however, had a statistically significant effect on trial outcome.<sup>39</sup> Linz, Penrod, and McDonald concluded that small sample size might be to blame for their inconclusive results.<sup>40</sup>

Due to lack of data, twentieth-century scholars had little luck determining the effect of attorney skill on trial outcome. Indeed, this data problem limited scholars' ability to answer the larger question: what outside factors, other than guilt or innocence, affect the outcome of a criminal trial? Within the last few years, however, some economists have obtained new data that have at last allowed them to examine the outside determinants of criminal trial outcome. For example, Richard Boylan studied the salaries of U.S. Attorneys from 1969 to 1999, finding that higher-paid U.S. Attorneys had higher conviction rates and generated longer prison sentences.<sup>41</sup> In a later study, Boylan demonstrated that federal prosecutors who successfully sought longer prison sentences were more likely to become a federal judge or a partner in large law firm after leaving the U.S. Attorney's

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32. Linz et al., *supra* note 18, at 281–302.

33. *Id.* at 284.

34. *Id.* at 286.

35. *Id.* at 286–87.

36. *Id.*

37. *Id.* at 296.

38. *Id.* at 297.

39. *Id.* at 299.

40. *Id.* at 300–01 (suggesting that future studies should gather ratings on attorney performance from judges and juries).

41. Richard T. Boylan, *Salaries, Turnover, and Performance in the Federal Criminal Justice System*, 47 J.L. & ECON. 75, 75–92 (2004).

office.<sup>42</sup> Boylan's results indicated that, in the courtroom, prosecutors had incentives to maximize prison terms instead of convictions or indictments.<sup>43</sup>

On the other hand, Radha Iyengar studied the two types of court-appointed attorneys for indigent defendants: public defenders, who were paid a predetermined salary for all their cases, and private attorneys appointed under the Criminal Justice Act,<sup>44</sup> who were paid on an hourly basis.<sup>45</sup> Iyengar found that defendants represented by public defenders were less likely to be found guilty and received shorter prison sentences than defendants represented by Criminal Justice Act attorneys. Although she was not able to control for attorney skill directly, she was able to control for factors such as the attorneys' experience, average caseload, law school quality, and wages. Iyengar concluded that these outside factors at least partially explained the differences in courtroom performance.<sup>46</sup>

While Boylan and Iyengar successfully identified some of the outside influences on attorney skill, no one successfully used empirical methods to determine the direct effects of attorney skill until 2007. In that year, Dan Abrams and Albert Yoon published the first empirical study evaluating the effects of attorney skill that systematically controlled for type of case, the race of the defense attorney, and most importantly, attorney-matching effects.<sup>47</sup> Abrams and Yoon collected a large data set of over 11,000 cases from the Clark County Public Defenders' Office in Nevada. For each case, the data contained information on the type of charge, whether the case went to trial, the public defender assigned to the case, how many years of experience the public defender had, the race of the public defender, and where the public defender went to law school.<sup>48</sup> The advantage of this data set was that in the Clark County Public Defenders' Office, public defenders were randomly assigned to cases.<sup>49</sup> Thus, even though Abrams and Yoon could not directly control for the strength of the case

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42. Richard T. Boylan, *What do Prosecutors Maximize? Evidence from the Careers of U.S. Attorneys*, 7 AM. LAW & ECON. REV. 379, 379–402 (2005).

43. *Id.* at 396.

44. 18 U.S.C. § 3006A(a)(3) (2006).

45. Radha Iyengar, *An Analysis of Attorney Performance in the Federal Indigent Defense System* 2–5 (Nat'l Bureau of Econ. Research, Working Paper No. 13187, 2007), available at <http://www.nber.org/papers/w13187>.

46. *Id.* at 28–30.

47. Abrams & Yoon, *supra* note 14, at 1176–77.

48. *Id.* at 1161–64.

49. *Id.* at 1149.

against the defendant, case strength did not create bias in their analysis because attorneys were randomly assigned to defendants. As a result, Abrams and Yoon were able to control for any attorney-matching effects that might bias their analysis.<sup>50</sup>

Although Abrams and Yoon did not have a direct measure of attorney skill, they did have data on each attorney's law school, race, and years of experience in the public defenders' office, which they used as proxies for each attorney's skill.<sup>51</sup> Using regression analysis, the authors found that experience was crucial: clients represented by attorneys who had been in the public defenders' office for a long time had better results.<sup>52</sup> On the other hand, the attorney's law school did not seem to affect the defendant's outcome.<sup>53</sup> Surprisingly, Abrams and Yoon also found an effect of the attorney's race on the defendant's outcome: Hispanic public defenders systematically achieved better results for their clients. The authors suggested that their exceptional performance might be due to the "potential language advantage" with their clients.<sup>54</sup>

Abrams and Yoon's paper was groundbreaking in assessing the effect of attorney skill. It was the first paper that had a large sample, sound empirical methods, and strong enough data to avoid the pitfalls that befell previous empirical analyses in this area. Nonetheless, their study still left several pressing questions unresolved. First, Abrams and Yoon only assessed the effect of the defense attorney's skill. But what effect did the prosecuting attorney's skill have on outcomes? Abrams and Yoon could say nothing about the effect of the prosecuting attorney's skill—and whether it was more important or less important than the defense attorney's skill. Moreover, because Abrams and Yoon's analysis only controlled for attorney-matching effects on the defendant's side, their estimates of the effect of the defense attorney's skill could still have been biased. Even though the public defenders were randomly assigned to the case, prosecutors were not. Prosecutors were likely assigned based on the strength of the case or even the perceived skill of the defendant's attorney.

Second, Abrams and Yoon used only proxies for attorney skill in their study. Whether an attorney's education or years of experience

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50. *Id.*

51. *See id.* at 1167–70 (discussing the effect that these attorney characteristics had on the duration and probability of incarceration).

52. *Id.* at 1173–75.

53. *Id.*

54. *Id.* at 1175.

have an influence on trial outcome is an interesting question, but it is a *different* question than the effect of actual skill. When we speak of attorney skill, we speak of an attorney's ability to research, to apply the research to his client's case, to investigate the facts, to convey these facts in court, to assess the strengths of his client's case, and most importantly, to be persuasive enough that the trier of fact should believe his version of the story.<sup>55</sup> Although these skills often come with time, experience is not perfectly correlated with skill. It is easy to imagine a young attorney who is so persuasive and effective in court that he gets better-than-average outcomes for his clients, despite his modest experience. Conversely, it is easy to imagine a highly experienced attorney who, despite the vast number of cases that he has tried, is inarticulate and utterly unpersuasive in court. Thus, although an attorney's years of experience and educational background are interesting—and could plausibly be correlated with actual skill—they are not perfect proxies for skill.

Third, Abrams and Yoon only looked at criminal trials where the defendant was represented by a public defender, which suggests that their estimate might also be subject to sample-selection bias.<sup>56</sup> Because all of the defendants used public defenders, these defendants were presumably poorer than the average defendant. If poor defendants are accused of different crimes than more affluent defendants, then Abrams and Yoon's data might oversample certain types of crimes where attorney skill is particularly effective. Moreover, Abrams and Yoon's data may oversample low-stakes cases. When a poor defendant is accused of a crime, he can choose either to use a free public defender or to attempt to raise money for a private attorney. This poor defendant is much more likely to ask his family, friends, church, and community for money to hire a private attorney in a high-stakes case. So for example, a defendant may just take a public defender when, if convicted, he will only get probation, but he may pool all his available resources to pay a private attorney when the

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55. For a concise definition of attorney skill as the term is used in this paper, see *supra* Part I.

56. Sample-selection bias arises "when the availability of the data is influenced by a selection process that is related to the value of the dependent variable. This selection process can introduce correlation between the error term and the regressor, which leads to bias . . ." JAMES H. STOCK & MARK W. WATSON, *INTRODUCTION TO ECONOMETRICS* 250 (2003). The classic example of sample selection bias occurs in a regression of wages on determinants like education, years of experience, and geographic location. Only people who have a job have wages, and the same factors that determine how much money a person makes also determine whether that person gets a job. *Id.* at 251.

penalty is jail time. An oversampling of low-stakes cases only matters if it somehow biases the estimate of the attorney skill effect. Yet perhaps attorney skill only makes a difference in these low-stakes cases; if so, then Abrams and Yoon's estimates would be biased upward. Thus, while Abrams and Yoon's estimates are more robust than the estimates of their predecessors, they remain problematic.

### III. GOING BEYOND ABRAMS AND YOON'S ANALYSIS: A NEW DATA SET

Abrams and Yoon's landmark paper was revolutionary in the study of the attorney skill effect, but its shortcomings—its failure to study prosecuting attorneys, its inability to assess attorney skill directly, and its failure to study cases with private defending attorneys—call into question both the accuracy and the external validity<sup>57</sup> of their estimates. Fortunately, a new data set avoids the problems of the Abrams and Yoon paper, allowing us to test the robustness of their results and to extend their research: *Evaluation of Hung Juries in Bronx County, New York, Los Angeles County, California, Maricopa County, Arizona, and Washington, DC, 2000-2001*.

The National Center for State Courts ("NCSC") originally compiled these data in order to evaluate the causes of hung juries.<sup>58</sup> The data set contains 320 observations from non-capital felony jury cases resulting in conviction, acquittal, or hung jury in four sites: the Bronx, Los Angeles, Phoenix, and the District of Columbia. For each case, the court clerk reported case characteristics and outcomes—including the race and gender of victims and defendants; the length of trial and deliberation process; the sentence; and the number of prosecution and defense witnesses, expert witnesses, and exhibits. These variables serve as important controls in this study; they indicate the amount of evidence presented by each side and serve as indicators of the strength each side's case. Abrams and Yoon argued that strength-of-case variables were not important in their study

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57. External validity is a term commonly used in econometrics that indicates whether the study is robust enough to extrapolate its results to other situations. See *id.* at 243–45 (discussing when an empirical study is subject to threats of external validity).

58. PAULA L. HANNAFORD-AGOR ET AL., INTER-UNIVERSITY CONSORTIUM FOR POLITICAL AND SOCIAL RESEARCH, EVALUATION OF HUNG JURIES IN BRONX COUNTY, NEW YORK, LOS ANGELES COUNTY, CALIFORNIA, MARICOPA COUNTY, ARIZONA, AND WASHINGTON, DC, 2000–2001, <http://www.icpsr.umich.edu/cocoon/ICPSR/STUDY/03689.xml> [hereinafter EVALUATION OF HUNG JURIES].

because of the random assignment of public defenders.<sup>59</sup> However, since this data set actually contains strength of case variables, it allows us to test their assertions.

For each case in this data set, the court clerk distributed a two-part survey to each juror, attorney, and judge. The first part was administered at the conclusion of the trial but before jury deliberation; the second part was administered after the verdict. Thus, the cases in this data set contain each attorney's, juror's, and judge's reaction to the verdict, and their evaluation of attorney skill, case complexity, and evidence. Again, this data set can go beyond the Abrams and Yoon study, for while they used proxies for attorney skill like education and years of experience, their data did not have a direct measure of attorney skill.

Tables I and II give summary statistics for the data set. The cases are almost equally distributed among the four sites, with the fewest observations coming from Bronx County, New York. The defendant sample is overwhelmingly male and black, while the victim sample is more heterogeneous. Approximately half of the cases resulted in convictions, and the majority were theft and drug-related crimes. Almost 80 percent of defendants in this sample were represented by court-appointed attorneys, suggesting that most defendants were indigent. Nevertheless, prosecutors averaged substantially fewer years of practice and previous criminal trials than the defense attorneys. When these cases at last went to trial, the prosecution tended to put on more evidence than the defense.

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59. Abrams & Yoon, *supra* note 14, at 1149.

TABLE I. DATA SUMMARY STATISTICS

Category	Variable	Observations	Percent of Total
Case Type (n=320)	Homicide	53	16.56
	Rape	18	5.62
	Robbery/Burglary/Larceny	88	27.50
	Assault (Non-Sexual)	34	10.62
	Drug-Related Crime	78	24.37
	Attempted Murder	10	3.12
	Weapons	16	5.00
	Other	23	7.19
Trial Outcome (n=320)	Conviction	153	47.81
	Acquittal	124	38.75
	Hung Jury	43	13.44
Sentence (n=142)	Less than 1 Year	7	4.93
	1 to 5 Years	40	28.17
	5 to 10 Years	29	20.42
	10 to 20 Years	21	14.79
	Over 20 Years	17	11.97
	Life	28	19.72
Defendant Characteristics (n=320)	Male	271	84.69
	Female	36	11.25
	Unknown Gender	13	4.06
	White (Non-Hispanic)	31	9.69
	Hispanic	86	26.88
	Black	181	56.56
	Other Non-White	16	5.00
	Unknown Race	6	1.88
Victim Characteristics (n=320)	Male	119	37.19
	Female	71	22.19
	Unknown Gender	130	40.63
	White (Non-Hispanic)	33	10.31
	Hispanic	67	20.94
	Black	76	23.75
	Other Non-White	14	4.38
	Unknown Race	130	40.63
Trial Characteristics (n=320)	Jury Sequestered	21	6.56
	Court-Appointed Defense Attorney	254	79.38
	Retrial	18	5.63
Site of Trial (n=320)	Los Angeles, CA	86	26.88
	Maricopa County, AZ	80	25.00
	Bronx County, NY	55	17.19
	Washington, DC	99	30.94

Notes: Evaluation of Hung Juries in Bronx County, New York, Los Angeles County, California, Maricopa County, Arizona, and Washington, DC, 2000-2001 data come from the Inter-University Consortium for Political and Social Research.

TABLE II. CASE CHARACTERISTICS		
Variable	Mean	Standard Deviation
<b>Evidence</b>		
Prosecution Witnesses	6.34	5.26
Prosecution Expert Witnesses	0.99	1.27
Prosecution Exhibits	17.50	27.42
Defense Witnesses	2.18	2.50
Defense Expert Witnesses	0.15	0.49
Defense Exhibits	5.12	11.67
<b>Experience</b>		
Prosecution Practice Years	8.61	5.13
Prosecution Previous Criminal Trials	39.36	35.59
Defense Practice Years	13.96	7.03
Defense Previous Criminal Trials	71.70	65.57
<b>Length of Trial</b>		
Trial Length in Days	4.71	3.47
Minutes of Jury Deliberation	23.74	18.49
<b>Attorney Ratings</b> (1=Not at all skillful to 7=Very skillful)		
Judge Rating of Prosecution	4.90	1.54
Judge Rating of Defense	5.10	1.45
Defense Rating of Prosecution	4.86	0.95
Prosecution Rating of Defense	5.01	0.90
Jury Rating of Prosecution	4.82	0.70
Jury Rating of Defense	4.41	0.76
<b>Complexity Ratings</b> (1=Not at all complex to 7=Very complex)		
Jury Rating of Case Complexity	3.66	0.81
Notes: <i>Evaluation of Hung Juries in Bronx County, New York, Los Angeles County, California, Maricopa County, Arizona, and Washington, DC, 2000-2001's</i> data come from the Inter-University Consortium for Political and Social Research.		

Notably, this data set contains forty-three cases that resulted in a hung jury. At first glance, it may appear that the researchers somehow oversampled hung jury cases—especially since the title of the data set is *Evaluation of Hung Juries*. Yet hung juries are much more common in criminal trials than most people believe—considering all twelve jurors must reach the same verdict. In fact, the National Criminal Justice Reference Service, a branch of the U.S. Department of Justice that collects data on the criminal justice system, estimates



that approximately 12 percent of trials result in a hung jury—matching almost perfectly with this data set's 13.44 percent.<sup>60</sup>

These detailed and unique observations have been previously unobtainable, preventing any broad studies of the effect of attorney skill in the courtroom. Accordingly, this new data set can provide valuable insight into the influence of attorney skill on the trial outcome of the everyday defendant.

#### IV. MODELING THE ATTORNEY SKILL EFFECT

Using the NCSC data, I begin my analysis with a basic probit model to estimate the effects of attorney skill on the trial outcome:

$$\Pr(\text{convicted} = 1) = \Phi[\beta_0 + \beta_1 \text{pattyskill}_i + \beta_2 \text{dattyskill}_i + \beta_3 \text{pwitness}_i + \beta_4 \text{pewitness}_i + \beta_5 \text{pexhibit}_i + \beta_6 \text{dwitness}_i + \beta_7 \text{dewitness}_i + \beta_8 \text{dexhibit}_i + \beta_9 \text{vfemale}_i + \beta_{10} \text{vblack}_i + \beta_{11} \text{vhispanic}_i + \beta_{12} \text{votherrace}_i + \beta_{13} \text{dfemale}_i + \beta_{14} \text{dblack}_i + \beta_{15} \text{dhispanic}_i + \beta_{16} \text{dotherrace}_i + \beta_{17} \text{crimeFE}_i + \varepsilon_i]$$

In this model, the dependent variable is the probability of being convicted. I model trial outcome in this manner because, like acquittal, most defendants consider a hung jury as a victory. A new trial requires considerable resources, so after a hung jury, the District Attorney's office must decide whether to pursue lesser charges or to pursue the case at all.<sup>61</sup> The variables *pwitness*, *pewitness*, and *pexhibit* (the number of prosecution witnesses, expert witnesses, and exhibits, respectively) control for the strength of the prosecution's case, while the variables *dwitness*, *dewitness*, and *dexhibit* (the number of defense witnesses, expert witnesses, and exhibits, respectively) control for the strength of the defense's case.<sup>62</sup> In

60. NAT'L CRIMINAL JUSTICE REFERENCE SERV., EMPIRICAL STUDY OF FREQUENCY OF OCCURRENCE, CAUSES, EFFECTS, AND AMOUNT OF TIME CONSUMED BY HUNG JURIES: FINAL REPORT (1975), available at <http://www.ncjrs.gov/App/Publications/abstract.aspx?ID=19026>. This study was originally published in 1975, and so is somewhat old. However, according to the researchers who compiled EVALUATION OF HUNG JURIES, *supra* note 58, most current statistics on hung juries come from studies in the 1960s and 1970s, further motivating their project.

61. Recently, a prosecutor's decision whether to pursue a retrial has received a lot of publicity after the second hung jury in a Miami terrorism case. Six men are accused of planning to bomb the Sears Tower in Chicago and the FBI offices in Miami, yet two different juries have been unable to reach a verdict. Curt Anderson, *Jury Hits Stalemate in Terrorism Retrial*, USA TODAY, Apr. 11, 2008, available at [http://www.usatoday.com/news/nation/2008-04-11-3235714827\\_x.htm](http://www.usatoday.com/news/nation/2008-04-11-3235714827_x.htm).

62. Individually, measures like the number of prosecution witnesses could be challenged as imperfect controls for the strength of the prosecution's case; for example, the prosecution could plausibly base their entire case on the testimony of one eyewitness. But together, these variables demonstrate the relative matchup of resources and evidence on both sides.

addition, the list of defendant and victim personal characteristic variables control for any potential effects of race or gender on trial outcome. The crime fixed-effect variables,<sup>63</sup> *crimeFE*, control for any systematic effects related to the type of crime and include dummy variables for homicide, rape, theft crimes, and drug crimes. Finally, the data set contains three different measures of prosecutor attorney skill, *pattyskill*, and defense attorney skill, *dattyskill*. As a result, I will estimate this basic model three times: the first using judge-evaluated attorney skill, the second using attorney-evaluated attorney skill, and the third using jury-evaluated attorney skill.

In the second part of my analysis, I will consider the influence of these same factors on sentence length using an ordered probit model:

$$\Pr(\textit{sentence} = j) = \Phi[\beta_0 + \beta_1 \textit{pattyskill}_i + \beta_2 \textit{dattyskill}_i + \beta_3 \textit{pwitness}_i + \beta_4 \textit{pewitness}_i + \beta_5 \textit{pexhibit}_i + \beta_6 \textit{dwitness}_i + \beta_7 \textit{dewitness}_i + \beta_8 \textit{dexhibit}_i + \beta_9 \textit{vfemale}_i + \beta_{10} \textit{vblack}_i + \beta_{11} \textit{vhisp}_i + \beta_{12} \textit{votherrace}_i + \beta_{13} \textit{dfemale}_i + \beta_{14} \textit{dblack}_i + \beta_{15} \textit{dhisp}_i + \beta_{16} \textit{dotherrace}_i + \beta_{17} \textit{crimeFE}_i + \varepsilon_i]$$

In this model, *j* ranges from one to seven and is based on the NCSC's rating of sentence length, with one representing less than a year in prison and seven representing life in prison. This analysis relies on the same controls as the previous regression analysis and will only focus on individuals whose prison sentence length is positive.<sup>64</sup>

In their 2007 study, Abrams and Yoon found that defense attorney skill had a statistically significant impact on trial outcome.<sup>65</sup> If the Abrams and Yoon results—not to mention the anecdotal evidence of attorney skill in the popular press—are correct, then the effect of defense attorney skill should be negative on trial outcome and sentence, while the effect of prosecution attorney skill should be positive. Thus,  $\beta_1$  should be greater than zero, and  $\beta_2$  should be less than zero in all three versions of both regressions. This model will therefore test the robustness of Abrams and Yoon's results as well as the media's claims.

63. The fixed-effect variables allow each type of crime to have its own intercept; this is useful if, for example, defendants in murder cases are systematically more likely to be convicted than defendants in theft cases. See WILLIAM H. GREENE, *ECONOMETRIC ANALYSIS* 193–200 (6th ed. 2008) (giving a technically rigorous discussion of fixed effects regression); STOCK & WATSON, *supra* note 56, at 278–83 (giving a basic overview of fixed effects regression).

64. Because only eleven defendants in the sample were convicted but not sentenced to prison, these defendants are not included in the sentencing analysis.

65. Abrams & Yoon, *supra* note 14, at 1173–74.

## V. RESULTS

The results demonstrate that Abrams and Yoon's analysis was, at best, incomplete. The skill of the prosecuting attorney has a statistically significant effect on trial outcomes, yet the skill of the defense attorney does not. Table III reports the results of a linear probability analysis of conviction, which is valuable for its ease of interpretation.<sup>66</sup> Table IV gives the results of the probit regression, and Table V reports the marginal effects from the probit regression. In each table, specifications one, two, and three use the judge, opposing attorney, and jury ratings, respectively, as a measure of attorney skill. Specification four adds the jury's case-complexity rating to the jury attorney skill rating specification. The jury case complexity variable can help control for jury confusion, which might have effects on jurors' perception of the attorneys and the trial outcome. To ease readability, Tables III, IV, and V only report the coefficients most relevant to this analysis. For the full regression results, see Appendix Tables I, II, and III.

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66. The results of a linear probability analysis are easier to interpret than the results of a probit analysis because the coefficients can be multiplied by one hundred and interpreted as percentage effects. STOCK & WATSON, *supra* note 56, at 299-302 (giving a basic overview of the linear probability model and how to interpret it). *But see* GREENE, *supra* note 63, at 772-73 (discussing problems with the linear probability model despite its ease of interpretation).

TABLE III. LINEAR PROBABILITY MODEL OF CONVICTION

	Judge Skill Ratings	Opposing Attorney Skill Ratings	Jury Skill Ratings	
	(1)	(2)	(3)	(4)
Prosecution Skill Rating	0.004 (0.020)	0.054 (0.036)	0.216** (0.039)	0.228** (0.039)
Defense Skill Rating	-0.021 (0.020)	-0.004 (0.034)	-0.031 (0.037)	-0.027 (0.037)
Jury-Rated Complexity	—	—	—	-0.065+ (0.038)
Court-Appointed Attorney	0.026 (0.073)	0.013 (0.076)	0.015 (0.072)	0.005 (0.073)
# of Pros. Witnesses	0.008 (0.009)	0.012 (0.009)	0.005 (0.008)	0.007 (0.008)
# of Pros. Experts	-0.022 (0.022)	-0.017 (0.021)	-0.040+ (0.022)	-0.032 (0.022)
# of Pros. Exhibits	0.003* (0.001)	0.003* (0.001)	0.003* (0.001)	0.003* (0.001)
# of Def. Witnesses	-0.021+ (0.013)	-0.018 (0.013)	-0.031** (0.011)	-0.032** (0.011)
# of Def. Experts	0.148* (0.058)	0.189** (0.060)	0.201** (0.050)	0.209** (0.052)
# of Def. Exhibits	0.001 (0.002)	0.001 (0.002)	0.001 (0.002)	0.001 (0.002)
Constant	0.325* (0.152)	-0.127 (0.263)	-0.641* (0.283)	-0.500+ (0.297)
Observations	317	297	315	315
Adjusted R-squared	0.14	0.18	0.23	0.24

+ significant at 10%; \* significant at 5%; \*\* significant at 1%

Notes: *Evaluation of Hung Juries in Bronx County, New York, Los Angeles County, California, Maricopa County, Arizona, and Washington, DC, 2000-2001* data come from the Inter-University Consortium for Political and Social Research. Dependent variable is a dummy variable equal to 1 if defendant was convicted. Specification 1 uses measures of attorney skill as rated by the judge. In specification 2, the skill measures come from the opposing attorney—the defense rates the prosecution, and the prosecution rates the defense. Specifications 3 and 4 both use the average of the jurors' attorney skill ratings, and specification 4 adds the jurors' average case complexity rating to control for any effects of juror confusion on attorney skill ratings. Controls for the race and gender of both the victim and defendant as well as crime fixed effects are included in each specification; for full results, see Appendix Table I. Heteroskedasticity-robust standard errors in parentheses. Dummy variables for missing values of victim race and victim gender included in regression but not reported.

TABLE IV. PROBIT MODEL OF CONVICTION

	Judge Skill Ratings	Opposing Attorney Skill Ratings	Jury Skill Ratings	
	(1)	(2)	(3)	(4)
Prosecution Skill Rating	0.020 (0.059)	0.133 (0.106)	0.717** (0.141)	0.762** (0.144)
Defense Skill Rating	-0.044 (0.060)	-0.014 (0.100)	-0.117 (0.126)	-0.105 (0.127)
Jury-Rated Complexity	—	—	—	-0.231+ (0.128)
Court-Appointed Attorney	0.152 (0.209)	0.116 (0.222)	0.082 (0.226)	0.055 (0.228)
# of Pros. Witnesses	-0.022 (0.030)	-0.001 (0.031)	-0.043 (0.032)	-0.037 (0.033)
# of Pros. Experts	-0.104 (0.068)	-0.084 (0.070)	-0.170* (0.079)	-0.145+ (0.081)
# of Pros. Exhibits	0.036** (0.009)	0.029** (0.008)	0.042** (0.010)	0.042** (0.011)
# of Def. Witnesses	-0.095* (0.039)	-0.082* (0.041)	-0.141** (0.040)	-0.144** (0.042)
# of Def. Experts	0.522* (0.207)	0.641** (0.215)	0.822** (0.215)	0.844** (0.216)
# of Def. Exhibits	0.012 (0.008)	0.013 (0.009)	0.011+ (0.007)	0.013+ (0.007)
Constant	-0.856+ (0.482)	-1.937* (0.810)	-4.096** (0.985)	-3.599** (1.012)
Observations	317	297	315	315
Pseudo R-Squared	0.19	0.22	0.28	0.29

+ significant at 10%; \* significant at 5%; \*\* significant at 1%

Notes: *Evaluation of Hung Juries in Bronx County, New York, Los Angeles County, California, Maricopa County, Arizona, and Washington, DC, 2000-2001* data come from the Inter-University Consortium for Political and Social Research. Dependent variable is a dummy variable equal to 1 if defendant was convicted. Specification 1 uses measures of attorney skill as rated by the judge. In specification 2, the skill measures come from the opposing attorney—the defense rates the prosecution, and the prosecution rates the defense. Specifications 3 and 4 both use the average of the jurors' attorney skill ratings, and specification 4 adds the jurors' average case complexity rating to control for any effects of juror confusion on attorney skill ratings. Controls for the race and gender of both the victim and defendant as well as crime fixed effects are included in each specification; for full results, see Appendix Table II. Heteroskedasticity-robust standard errors in parentheses. Dummy variables for missing values of victim race and victim gender included in regression but not reported.

TABLE V. PROBIT MODEL OF CONVICTION (MARGINAL EFFECTS)				
	Judge Skill Ratings	Opposing Attorney Skill Ratings	Jury Skill Ratings	
	(1)	(2)	(3)	(4)
Prosecution Skill Rating	0.008 (0.024)	0.053 (0.042)	0.286** (0.056)	0.304** (0.057)
Defense Skill Rating	-0.017 (0.024)	-0.005 (0.040)	-0.047 (0.050)	-0.042 (0.050)
Jury-Rated Complexity	—	—	—	-0.092+ (0.051)
Court-Appointed Attorney	0.060 (0.083)	0.046 (0.088)	0.033 (0.090)	0.022 (0.091)
# of Pros. Witnesses	-0.009 (0.012)	-0.001 (0.012)	-0.017 (0.013)	-0.015 (0.013)
# of Pros. Experts	-0.042 (0.027)	-0.033 (0.028)	-0.068* (0.032)	-0.058+ (0.032)
# of Pros. Exhibits	0.014** (0.004)	0.012** (0.003)	0.017** (0.004)	0.017** (0.004)
# of Def. Witnesses	-0.038* (0.016)	-0.033* (0.016)	-0.056** (0.016)	-0.057** (0.017)
# of Def. Experts	0.208* (0.083)	0.256** (0.086)	0.328** (0.086)	0.337** (0.086)
# of Def. Exhibits	0.005 (0.003)	0.005 (0.004)	0.005+ (0.003)	0.005+ (0.003)
Observations	317	297	315	315
Pseudo R-Squared	0.19	0.22	0.28	0.29
+ significant at 10%; * significant at 5%; ** significant at 1%				
Notes: <i>Evaluation of Hung Juries in Bronx County, New York, Los Angeles County, California, Maricopa County, Arizona, and Washington, DC, 2000-2001</i> data come from the Inter-University Consortium for Political and Social Research. Dependent variable is a dummy variable equal to 1 if defendant was convicted. Specification 1 uses measures of attorney skill as rated by the judge. In specification 2, the skill measures come from the opposing attorney—the defense rates the prosecution, and the prosecution rates the defense. Specifications 3 and 4 both use the average of the jurors' attorney skill ratings, and specification 4 adds the jurors' average case complexity rating to control for any effects of juror confusion on attorney skill ratings. Controls for the race and gender of both the victim and defendant as well as crime fixed effects are included in each specification; for full results, see Appendix Table III. Heteroskedasticity-robust standard errors in parentheses. Dummy variables for missing values of victim race and victim gender included in regression but not reported.				

TABLE VI. LINEAR PROBABILITY MODEL OF SENTENCING

	Judge Skill Ratings	Opposing Attorney Skill Ratings	Jury Skill Ratings	
	(1)		(2)	(3)
Prosecution Skill Rating	-0.053 (0.094)	0.155 (0.136)	-0.213 (0.295)	-0.293 (0.295)
Defense Skill Rating	0.106 (0.085)	0.197 (0.143)	-0.295 (0.243)	-0.346 (0.244)
Jury-Rated Complexity	—	—	—	0.292 (0.191)
Court-Appointed Attorney	-0.436 (0.311)	-0.422 (0.325)	-0.369 (0.327)	-0.246 (0.338)
# of Pros. Witnesses	0.086** (0.030)	0.084* (0.034)	0.063+ (0.033)	0.054 (0.035)
# of Pros. Experts	-0.190* (0.086)	-0.212* (0.093)	-0.147 (0.096)	-0.177+ (0.097)
# of Pros. Exhibits	-0.004 (0.004)	-0.005 (0.005)	-0.001 (0.005)	-0.001 (0.004)
# of Def. Witnesses	0.018 (0.059)	0.003 (0.062)	0.054 (0.065)	0.069 (0.067)
# of Def. Experts	-0.152 (0.355)	-0.019 (0.373)	-0.136 (0.366)	-0.037 (0.368)
# of Def. Exhibits	-0.004 (0.007)	-0.006 (0.008)	-0.006 (0.007)	-0.010 (0.007)
Constant	1.327 (0.859)	0.140 (0.971)	3.717+ (2.191)	3.379 (2.212)
Observations	141	132	141	141
Adjusted R-squared	0.42	0.41	0.41	0.42

+ significant at 10%; \* significant at 5%; \*\* significant at 1%

Notes: *Evaluation of Hung Juries in Bronx County, New York, Los Angeles County, California, Maricopa County, Arizona, and Washington, DC, 2000-2001* data come from the Inter-University Consortium for Political and Social Research. Dependent variable is a discrete sentencing variable ranging from 1 to 7, with 1 representing less than 1 year in prison, and 7 representing life in prison. Specification 1 uses measures of attorney skill as rated by the judge. In specification 2, the skill measures come from the opposing attorney—the defense rates the prosecution, and the prosecution rates the defense. Specifications 3 and 4 both use the average of the jurors' attorney skill ratings, and specification 4 adds the jurors' average case complexity rating to control for any effects of juror confusion on attorney skill ratings. Controls for the race and gender of both the victim and defendant as well as crime fixed effects are included in each specification; for full results, see Appendix Table IV. Heteroskedasticity-robust standard errors in parentheses. Dummy variables for missing values of victim race and victim gender included in regression but not reported.

TABLE VII. ORDERED PROBIT MODEL OF SENTENCING				
	Judge Skill Ratings	Opposing Attorney Skill Ratings	Jury Skill Ratings	
	(1)	(2)	(3)	(4)
Prosecution Skill Rating	-0.035 (0.084)	0.152 (0.123)	-0.196 (0.266)	-0.276 (0.265)
Defense Skill Rating	0.093 (0.084)	0.205 (0.126)	-0.292 (0.219)	-0.359 (0.223)
Jury-Rated Complexity	—	—	—	0.306+ (0.167)
Court-Appointed Attorney	-0.278 (0.291)	-0.244 (0.305)	-0.228 (0.298)	-0.095 (0.311)
# of Pros. Witnesses	0.082** (0.027)	0.081** (0.030)	0.061* (0.030)	0.050 (0.031)
# of Pros. Experts	-0.175* (0.083)	-0.201* (0.089)	-0.141 (0.090)	-0.169+ (0.092)
# of Pros. Exhibits	-0.003 (0.003)	-0.004 (0.004)	-0.001 (0.004)	-0.000 (0.004)
# of Def. Witnesses	0.013 (0.055)	0.003 (0.058)	0.047 (0.060)	0.055 (0.059)
# of Def. Experts	-0.009 (0.293)	0.132 (0.302)	0.030 (0.303)	0.127 (0.313)
# of Def. Exhibits	-0.004 (0.006)	-0.006 (0.007)	-0.006 (0.006)	-0.010+ (0.006)
Observations	141	132	141	141
Pseudo R-Squared	0.21	0.21	0.20	0.21
+ significant at 10%; * significant at 5%; ** significant at 1%				
Notes: <i>Evaluation of Hung Juries in Bronx County, New York, Los Angeles County, California, Maricopa County, Arizona, and Washington, DC, 2000-2001</i> data come from the Inter-University Consortium for Political and Social Research. Dependent variable is a discrete sentencing variable ranging from 1 to 7, with 1 representing less than 1 year in prison, and 7 representing life in prison. Specification 1 uses measures of attorney skill as rated by the judge. In specification 2, the skill measures come from the opposing attorney—the defense rates the prosecution, and the prosecution rates the defense. Specifications 3 and 4 both use the average of the jurors' attorney skill ratings, and specification 4 adds the jurors' average case complexity rating to control for any effects of juror confusion on attorney skill ratings. Controls for the race and gender of both the victim and defendant as well as crime fixed effects are included in each specification; for full results, see Appendix Table V. Heteroskedasticity-robust standard errors in parentheses. Dummy variables for missing values of victim race and victim gender included in regression but not reported.				



As Tables III, IV, and V demonstrate, the judge and opposing attorneys' ratings of skill are not statistically significant. In other words, the judges' and opposing attorneys' perceptions of attorney skill have no effect on trial outcome. However, the jurors' rating of the prosecuting attorneys' skill level is highly significant. According to specification four, for every one-point increase in a juror's rating of the prosecution, the probability of conviction increases by 30.4 percent. Moreover, for every one-point increase in a juror's rating of case complexity, the probability of conviction decreases by 9.2 percent. A skillful prosecuting attorney can drastically increase the defendant's chances of being convicted. Yet a skillful defense attorney has no such effect—the jurors' rating of defense attorney skill is never significant.

Overall, the results appear robust since the coefficients on the control variables are statistically significant and have the expected signs. Number of prosecution exhibits, for example, has a positive coefficient, and number of defense witnesses has a negative coefficient. These results make sense because more exhibits typically imply a stronger case against the defendant, while more defense witnesses typically imply a stronger case for the defendant. Interestingly, number of defense experts has a positive coefficient, so more defense experts actually increases the probability of being convicted. Perhaps a defendant only needs a large number of experts if the prosecution has a substantial amount of evidence against him. In all specifications, indigent defendants are not worse off even though they have government-paid, court-appointed attorneys. Other notable results include that female defendants are less likely to be convicted, while cases with female and black victims are more likely to result in conviction.<sup>67</sup>

Table VI gives the results of a linear probability analysis of sentencing, which is again useful for its ease of interpretation,<sup>68</sup> and Table VII reports the results of the ordered probit regression of sentencing. As in the previous tables, Tables VI and VII only report the most relevant coefficients; the full results are available in Appendix Tables IV and V. In these regressions, the significance of skill level disappears in all specifications. Attorney skill is no longer an important factor. In fact, the most significant factors in sentencing are type of crime—which should be expected due to minimum

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67. See app. tbl.I.

68. See *supra* note 66 (explaining how to interpret the results of a linear probability analysis).

sentencing laws—and the jury’s rating of case complexity. Perhaps the most complex cases are also more severe—for example, a first-degree murder case is likely more complex than a manslaughter or a drug case—which would explain the positive effect on sentence length.

## VI. DISCUSSION AND IMPLICATIONS

The results here are quite surprising. During celebrity trials, the media touts the importance of a good defense attorney<sup>69</sup>—a belief endorsed by the Abrams and Yoon study.<sup>70</sup> Yet according to these results, the skill level of the defense attorney plays *no role* in determining the outcome of a criminal trial in everyday cases with non-celebrity defendants. Instead, the prosecution’s skill level is crucial to the verdict. A guilty person may be more likely to walk free when the prosecution performs poorly, and an innocent person may be more likely to land in jail when the prosecution performs well.

Nevertheless, the results are highly intuitive. The prosecution—not the defense—has the burden of proving each element of the crime beyond a reasonable doubt.<sup>71</sup> In fact, while the defense will almost certainly put on some evidence to negate the prosecutor’s assertions, the defendant need not put on any evidence at all.<sup>72</sup> In a criminal trial, the ball is in the prosecution’s court; if it drops the ball, then a defendant win is almost inevitable. As a result, a defendant’s conviction or acquittal is determined by much more than just guilt or innocence.

Whenever factors other than guilt or innocence determine trial outcomes, then we must be concerned about the innocent being convicted and the guilty walking away. Recently, legal scholars have been concerned with other highly variable components of our criminal justice system—such as the decision to bring a defendant to trial or to exclude evidence—that may lead to erroneous trial outcomes.<sup>73</sup>

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69. See, e.g., Cartwright, *supra* note 8, at 1 (describing the media’s reaction to the verdict in the murder trial of millionaire Robert Durst); Risling, *supra* note 7, at 3 (describing the media’s reaction to the verdict in the murder trial of Robert Blake); Roberts et al., *supra* note 6, at A10 (describing the media’s reaction to the verdict in the child molestation trial of Michael Jackson).

70. Abrams & Yoon, *supra* note 14, at 1173–75.

71. 22A C.J.S. *Criminal Law* § 941 (2008).

72. 1 WHARTON’S CRIMINAL EVIDENCE § 2:9 (15th ed. 2008).

73. See, e.g., Daniel Givelber, *Meaningless Acquittals, Meaningful Convictions: Do We Reliably Acquit the Innocent?*, 49 RUTGERS L. REV. 1317, 1394 (1997) (“Advantages which may enhance the case of the guilty defendant such as the right to silence and to exclude relevant inculpatory evidence, work no benefit for the innocent. Instead, those advantages justify the

Variability in prosecutor skill would serve only to compound these other problems and lead to more verdicts that are erroneous.

The importance of the prosecution's skill also indicates the need for a policy change in District Attorney hiring and salary practices. As Tables I and II indicate, prosecutors are vastly less experienced than defense attorneys—in fact, many prosecutors are fresh out of law school. The relative inexperience of prosecutors is not just an anomaly of this data set. Indeed, a recent article in the *Florida Bar Journal* blamed the growth in prison inmate populations on the inexperienced state prosecutors who had almost complete discretion over the charges filed, plea deals offered, and sentences recommended.<sup>74</sup> “The problem of inexperienced prosecutors,” lamented the article, “is compounded by the budget crisis, which forces drastic turnover in prosecutors’ offices. Salary disparities exist between assistant state prosecutors and not just private attorneys, but also all other government attorneys, save those working in legal aid.”<sup>75</sup>

The problems created by low salaries in prosecutors’ offices reach far beyond Florida. In his nationwide study of U.S. Attorneys’ offices, Richard Boylan found that higher salaries led to improved attorney retention and performance.<sup>76</sup> Similarly, economists Jeff Biddle and Daniel Hamermesh pointed out the enormous earnings difference between attorneys in the private and public sectors—private attorneys make 158 percent more than their public counterparts—creating incentives for law students to obtain a private-sector job, if possible.<sup>77</sup> The incentives are particularly great for high achievers: the percentage returns to higher class rank and serving on

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prosecution's withholding from the accused and the factfinder evidence which might undermine the prosecution's case.”); Fred C. Zacharias & Bruce A. Green, *The Duty to Avoid Wrongful Convictions: A Thought Experiment in the Regulation of Prosecutors*, 89 B.U. L. REV. 1, 57 (2009) (arguing that “the problem of false convictions implicates aspects of prosecutors’ professional conduct that might plausibly be regulated by the legal ethics codes”). These variations in our criminal justice system led legal scholar John Merryman to joke that “if he were innocent, he would prefer to be tried by a civil law court, but that if he were guilty, he would prefer to be tried by a common law court.” JOHN HENRY MERRYMAN, *THE CIVIL LAW TRADITION: AN INTRODUCTION TO THE LEGAL SYSTEMS OF WESTERN EUROPE AND LATIN AMERICA* 132 (2d ed. 1985).

74. Aaron M. Clemens & Hale R. Stancil, *Unhandcuffing Justice: Proposals to Return Rationality to Criminal Sentencing*, FLA. B. J., Feb. 2009, at 54.

75. *Id.*

76. See Boylan, *supra* note 41, at 76 (finding that lower salaries lead to higher turnover, and higher turnover leads to lower output).

77. See Jeff E. Biddle & Daniel S. Hamermesh, *Beauty, Productivity, and Discrimination: Lawyers’ Looks and Lucre*, 16 J. LAB. ECON. 172, 193 (1998) (finding a raw earnings difference of 158 percent between attorneys in the private and public sectors that did not disappear after controlling for productivity).

a law journal are greater in the private sector.<sup>78</sup> This research suggests that because government jobs do not pay as well as private sector jobs, some prosecutors may take their job because they finished in the lower half of their law school class. Their high-achieving classmates snatched up all of the private-sector jobs, and the public sector was the only option available. The importance of prosecuting attorney skill level for trial outcomes, however, suggests that it would be beneficial for the government to increase pay. Not only would a pay increase improve the quality of attorneys working in prosecutors' offices by attracting the best and the brightest, but it would also reduce the incentives for good prosecutors to leave and take higher-paying jobs in the private sector.<sup>79</sup>

Even if cash-strapped government budgets prevent across-the-board pay increases for prosecutors, the government can still reduce variation in prosecutor skill and improve overall quality by providing training programs for prosecutors. District Attorneys' offices could institute mentoring programs that assign more experienced prosecutors to assist and support the newcomers. These offices could also prohibit new prosecutors from serving as chief counsel in felony cases until they have several months of experience. Finally, the offices could institute an internal monitoring system to track each prosecutor's performance. At the conclusion of a trial, the prosecution could ask third parties—perhaps victims, victims' families, or even judges—to complete a survey rating the performance of the prosecution.<sup>80</sup> The office could then require prosecutors with consistently bad ratings to work under prosecutors with good ratings. The ratings could also assist in promotion and retention decisions.<sup>81</sup>

Interestingly, the regression results also indicate that defendants represented by court-appointed attorneys fare no worse in the courtroom than defendants represented by private attorneys. The coefficient on the court-appointed attorney variable is never statistically significant, indicating no effect on trial outcome or

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78. *Id.* at 195.

79. These suggestions are strengthened by Boylan's result that lower salaries for U.S. Attorneys lead to higher turnover and lower job performance. Boylan, *supra* note 41, at 75.

80. These surveys could be similar to the ones used to create EVALUATION OF HUNG JURIES. See *supra* note 58 (describing the data collection process).

81. Other scholars have also argued for prosecutor training programs. *E.g.*, Zacharias & Green, *supra* note 73, at 19 (arguing that a training program "encourages prosecutors to temper overzealousness and consider how their behavior can produce untoward results").

sentencing.<sup>82</sup> Groups such as the American Civil Liberties Union (“ACLU”) advocate for change in this area, citing the inadequacy of the current public defense system:

The Sixth Amendment guarantees every person accused of a crime the right to an attorney for his or her defense, regardless of ability to pay, and the Fourteenth Amendment guarantees all citizens equal rights regardless of race or national origin. Yet all too often, these rights are violated by indigent defense systems that leave low-income people, including many people of color, without adequate representation.<sup>83</sup>

In fact, the principal motivation for economist Radha Iyengar to study the indigent defense system was the fact that “[t]he right to an equal and fair trial regardless of wealth is a hallmark of American jurisprudence.”<sup>84</sup> Yet contrary to her results and to the claims of the ACLU, the results of the present study suggest that indigent defendants *are* getting an equal and fair trial regardless of wealth. If such inequality exists, it occurs *before* trial during the plea bargaining stage,<sup>85</sup> not *at* trial itself. Once again, the results indicate that scholars concerned about inequitable trial outcomes have been focusing on the wrong side. They have been so preoccupied with the importance of the defense attorney that they forgot about the prosecution.

These results have certainly demonstrated the importance of a good prosecuting attorney, and the data set has allowed this Note to go beyond all previous studies of the attorney skill effect. Yet this data set—like all data sets—does have some limitations. For instance, this sample may obscure the effect of a good defense attorney. Because the sample contains only 320 observations, and 80 percent of the attorneys are court-appointed for indigent defendants, the sample may not contain any especially skillful defense attorneys. There are probably no Johnnie Cochrans in this sample. Nevertheless, the sample paints a more realistic picture of the everyday criminal justice system. No matter how much money the defendant spends on an attorney, he will probably never get a Johnnie Cochran. As a result,

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82. In case the court-appointed attorney variable is highly correlated with the defense attorney skill measure, all regression specifications were re-estimated without the court-appointed attorney dummy variable. The results did not change.

83. American Civil Liberties Union, *Criminal Justice: Indigent Defense*, <http://www.aclu.org/crimjustice/indigent/index.html> (last visited Oct. 30, 2009).

84. Iyengar, *supra* note 45, at 2.

85. Since this data set does not contain information on defendants who did not go to trial, we cannot test this possibility here. This possibility is worth mentioning here, however, because the results of the present study do not preclude possible inequitable treatment before trial.

the everyday defendant cannot simply hire a good lawyer to avoid conviction.

This Note has also failed to demonstrate that the effects of attorney skill last into the sentencing phase. Perhaps when judges or juries sentence convicted defendants, they only look at the strength of the case. However, the general lack of significance in the sentencing regressions suggests that attorney skill effects may be the victim of small sample size—only 142 of the defendants in this sample were sentenced to prison. Moreover, since sentencing data are only available in ranges, and not continuously in years, attorney skill effects may be further obscured. Because of minimum sentencing laws, attorney skill effects probably affect only one or two years in prison—not ten or twenty. These unfortunate limiting characteristics of the data set suggest a rich area for future research.

This data set has its limitations. Yet it offers a unique opportunity to observe the effect of raw attorney skill on trial outcomes without all the problems of the Abrams and Yoon study. Moreover, the results of this study suggest that perhaps we have been asking the wrong question. The media, the ACLU, and most scholars are disproportionately concerned with the effect of the defense attorney's skill on trial outcome. Instead, this study suggests that we should be much more concerned with the prosecuting attorney's skill. Perhaps scholarly focus has been heavier on defense attorney skill because the defendant can often control who represents him in court, but he cannot control who prosecutes him. Nevertheless, the results of this study suggest that the selection of the prosecutor can determine who goes to jail and who goes home.

## VII. CONCLUSION

O.J. Simpson's "Dream Team" may have been a "Scheme Team" as Fred Goldman suggested, but their skillfulness probably did not carry the day.<sup>86</sup> Instead, author Scott Turow had it right when he blamed the prosecution. Turow did not use Johnnie Cochran's racial appeals and clever slogans to explain the O.J. Simpson verdict; on the contrary, he believed:

Because the prosecutors routinely accepted even the most unlikely stories from police officers, they were unable to recognize Mr. Fuhrman [the lead police detective] as a genuinely bad character. By the time news of Mr. Fuhrman's background began to emerge, prosecutors were hip-joined to him, their star witness—a foul-mouthed racist

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86. Margolick, *supra* note 1, at A1.

cop, the latest poster boy of the Los Angeles Police Department, his image hanging on the wall of the public mind next to those of the officers who beat Rodney King.

*The jury made them pay.*<sup>87</sup>

The O.J. Simpson jury made clumsy prosecutors pay. Before this study, however, the results of the O.J. Simpson trial hardly seemed extendable to the everyday criminal trial. After all, the O.J. Simpson case was the “trial of the century.”<sup>88</sup>

Yet as the results of this Note have shown, the prosecution plays a critical role in everyday criminal trials. An unskilled prosecutor can increase the likelihood of a hung jury or acquittal—no matter how strong the case is against the defendant. Thus, while the O.J. Simpson trial was an extraordinary one, it was not an anomaly. Even in everyday trials, the jury will make clumsy prosecutors pay. And as long as the jury makes clumsy prosecutors pay, defendants will continue to slip away from justice.

*Jennifer Bennett Shinall\**

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87. Turow, *supra* note 5, at A21 (emphasis added).

88. Frank Rich, *Judge Ito's All-Star Vaudeville*, N.Y. TIMES, Oct. 2, 1994, at E17.

\* Candidate for Doctor of Philosophy in Law and Economics and Doctor of Jurisprudence, May 2012. I would like to thank Professor Paige Skiba for her suggestions and invaluable assistance in getting this data set into working form. The VANDERBILT LAW REVIEW editorial board also worked tirelessly to get this Note and the tables into a publishable format. A special thanks goes to my husband, Ricky, for his unfailing support, and to my mother, Kathy, for proofreading everything I have ever written from first grade to graduate school.

## APPENDIX

APPENDIX TABLE I. FULL LINEAR PROBABILITY MODEL OF CONVICTION				
	Judge Skill Ratings	Opposing Attorney Skill Ratings	Jury Skill Ratings	
	(1)	(2)	(3)	(4)
Prosecution Skill Rating	0.004 (0.020)	0.054 (0.036)	0.216** (0.039)	0.228** (0.039)
Defense Skill Rating	-0.021 (0.020)	-0.004 (0.034)	-0.031 (0.037)	-0.027 (0.037)
Jury-Rated Complexity	—	—	—	-0.065+ (0.038)
Defendant Female	-0.137 (0.096)	-0.133 (0.100)	-0.149+ (0.085)	-0.173+ (0.089)
Defendant Hispanic	-0.017 (0.106)	0.018 (0.113)	0.028 (0.105)	0.018 (0.105)
Defendant Black	-0.217* (0.098)	-0.179+ (0.105)	-0.167 (0.102)	-0.173+ (0.102)
Defendant Other Non-White	0.155 (0.151)	0.193 (0.150)	0.227 (0.140)	0.204 (0.140)
Victim Female	0.200* (0.082)	0.240** (0.083)	0.238** (0.077)	0.239** (0.077)
Victim Hispanic	0.037 (0.099)	0.091 (0.108)	0.041 (0.090)	0.072 (0.092)
Victim Black	0.250* (0.103)	0.310** (0.109)	0.272** (0.095)	0.286** (0.095)
Victim Other Non-White	0.301 (0.183)	0.453* (0.183)	0.252+ (0.152)	0.257+ (0.153)
Homicide	-0.050 (0.087)	-0.025 (0.087)	-0.073 (0.085)	-0.039 (0.090)
Rape	0.375** (0.133)	0.433** (0.139)	0.349** (0.114)	0.374** (0.113)
Robbery/Theft/Larceny	0.025 (0.075)	-0.023 (0.076)	0.018 (0.070)	0.023 (0.069)
Drug Related	0.159+ (0.081)	0.166+ (0.086)	0.153* (0.077)	0.161* (0.077)
Court-Appointed Attorney	0.026 (0.073)	0.013 (0.076)	0.015 (0.072)	0.005 (0.073)
# of Pros. Witnesses	0.008 (0.009)	0.012 (0.009)	0.005 (0.008)	0.007 (0.008)
# of Pros. Experts	-0.022 (0.022)	-0.017 (0.021)	-0.040+ (0.022)	-0.032 (0.022)
# of Pros. Exhibits	0.003* (0.001)	0.003* (0.001)	0.003* (0.001)	0.003* (0.001)



# of Def. Witnesses	-0.021+ (0.013)	-0.018 (0.013)	-0.031** (0.011)	-0.032** (0.011)
# of Def. Experts	0.148* (0.058)	0.189** (0.060)	0.201** (0.050)	0.209** (0.052)
# of Def. Exhibits	0.001 (0.002)	0.001 (0.002)	0.001 (0.002)	0.001 (0.002)
Constant	0.325* (0.152)	-0.127 (0.263)	-0.641* (0.283)	-0.500+ (0.297)
Observations	317	297	315	315
Adjusted R-squared	0.14	0.18	0.23	0.24

+ significant at 10%; \* significant at 5%; \*\* significant at 1%

Notes: *Evaluation of Hung Juries in Bronx County, New York, Los Angeles County, California, Maricopa County, Arizona, and Washington, DC, 2000-2001* data come from the Inter-University Consortium for Political and Social Research. Dependent variable is a dummy variable equal to 1 if defendant was convicted. Specification 1 uses measures of attorney skill as rated by the judge. In specification 2, the skill measures come from the opposing attorney—the defense rates the prosecution, and the prosecution rates the defense. Specifications 3 and 4 both use the average of the jurors' attorney skill ratings, and specification 4 adds the jurors' average case complexity rating to control for any effects of juror confusion on attorney skill ratings. Heteroskedasticity-robust standard errors in parentheses. Dummy variables for missing values of victim race and victim gender included in regression but not reported.

APPENDIX TABLE II. FULL PROBIT MODEL OF CONVICTION				
	Judge Skill Ratings	Opposing Attorney Skill Ratings	Jury Skill Ratings	
	(1)	(2)	(3)	(4)
Prosecution Skill Rating	0.020 (0.059)	0.133 (0.106)	0.717** (0.141)	0.762** (0.144)
Defense Skill Rating	-0.044 (0.060)	-0.014 (0.100)	-0.117 (0.126)	-0.105 (0.127)
Jury-Rated Complexity	—	—	—	-0.231+ (0.128)
Defendant Female	-0.361 (0.295)	-0.348 (0.315)	-0.514+ (0.311)	-0.590+ (0.328)
Defendant Hispanic	-0.057 (0.309)	0.058 (0.334)	0.070 (0.351)	0.006 (0.351)
Defendant Black	-0.680* (0.291)	-0.573+ (0.314)	-0.544 (0.336)	-0.588+ (0.330)
Defendant Other Non-White	0.557 (0.464)	0.647 (0.471)	0.871+ (0.487)	0.766 (0.485)
Victim Female	0.729** (0.247)	0.798** (0.259)	0.955** (0.257)	0.953** (0.264)
Victim Hispanic	0.292 (0.334)	0.462 (0.405)	0.524 (0.387)	0.655 (0.406)
Victim Black	0.882** (0.336)	1.078** (0.401)	1.215** (0.387)	1.282** (0.402)
Victim Other Non-White	0.784 (0.658)	1.259+ (0.676)	0.951 (0.625)	0.998 (0.632)
Homicide	-0.265 (0.269)	-0.184 (0.264)	-0.286 (0.270)	-0.183 (0.281)
Rape	1.053* (0.411)	1.236** (0.459)	1.131** (0.388)	1.222** (0.389)
Robbery/Theft/Larceny	0.038 (0.225)	-0.103 (0.229)	0.034 (0.234)	0.038 (0.236)
Drug Related	0.492* (0.226)	0.496* (0.238)	0.499* (0.239)	0.521* (0.239)
Court-Appointed Attorney	0.152 (0.209)	0.116 (0.222)	0.082 (0.226)	0.055 (0.228)
# of Pros. Witnesses	-0.022 (0.030)	-0.001 (0.031)	-0.043 (0.032)	-0.037 (0.033)
# of Pros. Experts	-0.104 (0.068)	-0.084 (0.070)	-0.170* (0.079)	-0.145+ (0.081)
# of Pros. Exhibits	0.036** (0.009)	0.029** (0.008)	0.042** (0.010)	0.042** (0.011)
# of Def. Witnesses	-0.095* (0.039)	-0.082* (0.041)	-0.141** (0.040)	-0.144** (0.042)

# of Def. Experts	0.522* (0.207)	0.641** (0.215)	0.822** (0.215)	0.844** (0.216)
# of Def. Exhibits	0.012 (0.008)	0.013 (0.009)	0.011+ (0.007)	0.013+ (0.007)
Constant	-0.856+ (0.482)	-1.937* (0.810)	-4.096** (0.985)	-3.599** (1.012)
Observations	317	297	315	315
Pseudo R-Squared	0.19	0.22	0.28	0.29
+ significant at 10%; * significant at 5%; ** significant at 1%				
Notes: <i>Evaluation of Hung Juries in Bronx County, New York, Los Angeles County, California, Maricopa County, Arizona, and Washington, DC, 2000-2001</i> data come from the Inter-University Consortium for Political and Social Research. Dependent variable is a dummy variable equal to 1 if defendant was convicted. Specification 1 uses measures of attorney skill as rated by the judge. In specification 2, the skill measures come from the opposing attorney—the defense rates the prosecution, and the prosecution rates the defense. Specifications 3 and 4 both use the average of the jurors' attorney skill ratings, and specification 4 adds the jurors' average case complexity rating to control for any effects of juror confusion on attorney skill ratings. Heteroskedasticity-robust standard errors in parentheses. Dummy variables for missing values of victim race and victim gender included in regression but not reported.				

APPENDIX TABLE III. FULL PROBIT MODEL OF CONVICTION (MARGINAL EFFECTS)				
	Judge Skill Ratings	Opposing Attorney Skill Ratings	Jury Skill Ratings	
	(1)	(2)	(3)	(4)
Prosecution Skill Rating	0.008 (0.024)	0.053 (0.042)	0.286** (0.056)	0.304** (0.057)
Defense Skill Rating	-0.017 (0.024)	-0.005 (0.040)	-0.047 (0.050)	-0.042 (0.050)
Jury-Rated Complexity	—	—	—	-0.092+ (0.051)
Defendant Female	-0.142 (0.113)	-0.137 (0.121)	-0.199+ (0.114)	-0.227+ (0.117)
Defendant Hispanic	-0.023 (0.123)	0.023 (0.133)	0.028 (0.140)	0.002 (0.140)
Defendant Black	-0.265* (0.109)	-0.225+ (0.120)	-0.214+ (0.129)	-0.231+ (0.125)
Defendant Other Non-White	0.212 (0.160)	0.244 (0.157)	0.312* (0.139)	0.280+ (0.148)
Victim Female	0.278** (0.085)	0.302** (0.087)	0.354** (0.081)	0.353** (0.083)
Victim Hispanic	0.115 (0.130)	0.181 (0.153)	0.204 (0.144)	0.251+ (0.144)
Victim Black	0.331** (0.111)	0.395** (0.124)	0.435** (0.111)	0.454** (0.111)
Victim Other Non-White	0.284 (0.196)	0.409** (0.137)	0.333* (0.165)	0.345* (0.160)
Homicide	-0.105 (0.106)	-0.073 (0.104)	-0.113 (0.106)	-0.073 (0.111)
Rape	0.360** (0.103)	0.406** (0.099)	0.380** (0.092)	0.399** (0.085)
Robbery/Theft/Larceny	0.015 (0.090)	-0.041 (0.091)	0.014 (0.093)	0.015 (0.094)
Drug Related	0.192* (0.085)	0.194* (0.090)	0.195* (0.090)	0.203* (0.089)
Court-Appointed Attorney	0.060 (0.083)	0.046 (0.088)	0.033 (0.090)	0.022 (0.091)
# of Pros. Witnesses	-0.009 (0.012)	-0.001 (0.012)	-0.017 (0.013)	-0.015 (0.013)
# of Pros. Experts	-0.042 (0.027)	-0.033 (0.028)	-0.068* (0.032)	-0.058+ (0.032)
# of Pros. Exhibits	0.014** (0.004)	0.012** (0.003)	0.017** (0.004)	0.017** (0.004)
# of Def. Witnesses	-0.038* (0.016)	-0.033* (0.016)	-0.056** (0.016)	-0.057** (0.017)

# of Def. Experts	0.208* (0.083)	0.256** (0.086)	0.328** (0.086)	0.337** (0.086)
# of Def. Exhibits	0.005 (0.003)	0.005 (0.004)	0.005+ (0.003)	0.005+ (0.003)
Observations	317	297	315	315
Pseudo R-Squared	0.19	0.22	0.28	0.29
+ significant at 10%; * significant at 5%; ** significant at 1%				
Notes: <i>Evaluation of Hung Juries in Bronx County, New York, Los Angeles County, California, Maricopa County, Arizona, and Washington, DC, 2000-2001</i> data come from the Inter-University Consortium for Political and Social Research. Dependent variable is a dummy variable equal to 1 if defendant was convicted. Specification 1 uses measures of attorney skill as rated by the judge. In specification 2, the skill measures come from the opposing attorney—the defense rates the prosecution, and the prosecution rates the defense. Specifications 3 and 4 both use the average of the jurors' attorney skill ratings, and specification 4 adds the jurors' average case complexity rating to control for any effects of juror confusion on attorney skill ratings. Heteroskedasticity-robust standard errors in parentheses. Dummy variables for missing values of victim race and victim gender included in regression but not reported.				

APPENDIX TABLE IV. FULL LINEAR PROBABILITY MODEL OF SENTENCING				
	Judge Skill Ratings	Opposing Attorney Skill Ratings	Jury Skill Ratings	
	(1)	(2)	(3)	(4)
Prosecution Skill Rating	-0.053 (0.094)	0.155 (0.136)	-0.213 (0.295)	-0.293 (0.295)
Defense Skill Rating	0.106 (0.085)	0.197 (0.143)	-0.295 (0.243)	-0.346 (0.244)
Jury-Rated Complexity	—	—	—	0.292 (0.191)
Defendant Female	-0.275 (0.384)	-0.255 (0.389)	-0.268 (0.409)	-0.184 (0.437)
Defendant Hispanic	-0.128 (0.534)	-0.037 (0.565)	-0.129 (0.540)	-0.239 (0.553)
Defendant Black	0.486 (0.477)	0.494 (0.526)	0.474 (0.476)	0.403 (0.481)
Defendant Other Non-White	0.315 (1.376)	0.366 (1.267)	0.205 (1.234)	0.307 (1.179)
Victim Female	0.688+ (0.367)	0.685+ (0.377)	0.553 (0.410)	0.573 (0.400)
Victim Hispanic	0.887+ (0.490)	0.793 (0.632)	1.160* (0.535)	0.915 (0.584)
Victim Black	0.406 (0.524)	0.506 (0.614)	0.496 (0.525)	0.454 (0.525)
Victim Other Non-White	1.510** (0.544)	0.978 (0.712)	1.266+ (0.640)	1.207+ (0.632)
Homicide	2.521** (0.337)	2.285** (0.383)	2.829** (0.315)	2.650** (0.357)
Rape	1.590** (0.582)	1.420* (0.587)	1.559** (0.595)	1.529* (0.586)
Robbery/Theft/Larceny	1.803** (0.329)	1.596** (0.354)	1.760** (0.308)	1.887** (0.331)
Drug Related	0.604 (0.392)	0.397 (0.417)	0.618 (0.398)	0.663+ (0.386)
Court-Appointed Attorney	-0.436 (0.311)	-0.422 (0.325)	-0.369 (0.327)	-0.246 (0.338)
# of Pros. Witnesses	0.086** (0.030)	0.084* (0.034)	0.063+ (0.033)	0.054 (0.035)
# of Pros. Experts	-0.190* (0.086)	-0.212* (0.093)	-0.147 (0.096)	-0.177+ (0.097)
# of Pros. Exhibits	-0.004 (0.004)	-0.005 (0.005)	-0.001 (0.005)	-0.001 (0.004)
# of Def. Witnesses	0.018 (0.059)	0.003 (0.062)	0.054 (0.065)	0.069 (0.067)

# of Def. Experts	-0.152 (0.355)	-0.019 (0.373)	-0.136 (0.366)	-0.037 (0.368)
# of Def. Exhibits	-0.004 (0.007)	-0.006 (0.008)	-0.006 (0.007)	-0.010 (0.007)
Constant	1.327 (0.859)	0.140 (0.971)	3.717+ (2.191)	3.379 (2.212)
Observations	141	132	141	141
Adjusted R-squared	0.42	0.41	0.41	0.42
+ significant at 10%; * significant at 5%; ** significant at 1%				
Notes: <i>Evaluation of Hung Juries in Bronx County, New York, Los Angeles County, California, Maricopa County, Arizona, and Washington, DC, 2000-2001</i> data come from the Inter-University Consortium for Political and Social Research. Dependent variable is a discrete sentencing variable ranging from 1 to 7, with 1 representing less than 1 year in prison, and 7 representing life in prison. Specification 1 uses measures of attorney skill as rated by the judge. In specification 2, the skill measures come from the opposing attorney—the defense rates the prosecution, and the prosecution rates the defense. Specifications 3 and 4 both use the average of the jurors' attorney skill ratings, and specification 4 adds the jurors' average case complexity rating to control for any effects of juror confusion on attorney skill ratings. Heteroskedasticity-robust standard errors in parentheses. Dummy variables for missing values of victim race and victim gender included in regression but not reported.				

APPENDIX TABLE V. FULL ORDERED PROBIT MODEL OF SENTENCING				
	Judge Skill Ratings	Opposing Attorney Skill Ratings	Jury Skill Ratings	
	(1)	(2)	(3)	(4)
Prosecution Skill Rating	-0.035 (0.084)	0.152 (0.123)	-0.196 (0.266)	-0.276 (0.265)
Defense Skill Rating	0.093 (0.084)	0.205 (0.126)	-0.292 (0.219)	-0.359 (0.223)
Jury-Rated Complexity	—	—	—	0.306+ (0.167)
Defendant Female	-0.237 (0.352)	-0.227 (0.340)	-0.248 (0.369)	-0.178 (0.396)
Defendant Hispanic	0.121 (0.459)	0.205 (0.493)	0.156 (0.457)	0.027 (0.483)
Defendant Black	0.667 (0.433)	0.700 (0.480)	0.680 (0.432)	0.589 (0.446)
Defendant Other Non-White	0.294 (1.210)	0.310 (1.100)	0.200 (1.076)	0.310 (1.041)
Victim Female	0.664+ (0.348)	0.636+ (0.353)	0.516 (0.367)	0.574 (0.362)
Victim Hispanic	0.688 (0.442)	0.517 (0.574)	0.928+ (0.477)	0.703 (0.508)
Victim Black	0.277 (0.460)	0.285 (0.558)	0.371 (0.458)	0.355 (0.463)
Victim Other Non-White	1.308* (0.541)	0.643 (0.653)	1.002+ (0.578)	0.961+ (0.571)
Homicide	2.359** (0.414)	2.085** (0.442)	2.665** (0.417)	2.533** (0.434)
Rape	1.225* (0.487)	1.031* (0.476)	1.196* (0.486)	1.185* (0.490)
Robbery/Theft/Larceny	1.613** (0.330)	1.431** (0.329)	1.560** (0.287)	1.741** (0.322)
Drug Related	0.504 (0.356)	0.315 (0.367)	0.522 (0.360)	0.583+ (0.350)
Court-Appointed Attorney	-0.278 (0.291)	-0.244 (0.305)	-0.228 (0.298)	-0.095 (0.311)
# of Pros. Witnesses	0.082** (0.027)	0.081** (0.030)	0.061* (0.030)	0.050 (0.031)
# of Pros. Experts	-0.175* (0.083)	-0.201* (0.089)	-0.141 (0.090)	-0.169+ (0.092)
# of Pros. Exhibits	-0.003 (0.003)	-0.004 (0.004)	-0.001 (0.004)	-0.000 (0.004)
# of Def. Witnesses	0.013 (0.055)	0.003 (0.058)	0.047 (0.060)	0.055 (0.059)



# of Def. Experts	-0.009 (0.293)	0.132 (0.302)	0.030 (0.303)	0.127 (0.313)
# of Def. Exhibits	-0.004 (0.006)	-0.006 (0.007)	-0.006 (0.006)	-0.010+ (0.006)
Observations	141	132	141	141
Pseudo R-Squared	0.21	0.21	0.20	0.21

+ significant at 10%; \* significant at 5%; \*\* significant at 1%

Notes: *Evaluation of Hung Juries in Bronx County, New York, Los Angeles County, California, Maricopa County, Arizona, and Washington, DC, 2000-2001* data come from the Inter-University Consortium for Political and Social Research. Dependent variable is a discrete sentencing variable ranging from 1 to 7, with 1 representing less than 1 year in prison, and 7 representing life in prison. Specification 1 uses measures of attorney skill as rated by the judge. In specification 2, the skill measures come from the opposing attorney—the defense rates the prosecution, and the prosecution rates the defense. Specifications 3 and 4 both use the average of the jurors' attorney skill ratings, and specification 4 adds the jurors' average case complexity rating to control for any effects of juror confusion on attorney skill ratings. Heteroskedasticity-robust standard errors in parentheses. Dummy variables for missing values of victim race and victim gender included in regression but not reported.