Down to the Last Strike:
The Effect of the Jury Lottery on Criminal Convictions

Scott Kostyshak* and Neel U. Sukhatme**

June 13, 2024

WORKING PAPER: PLEASE DO NOT REFERENCE OR CITE WITHOUT PERMISSION

Abstract

How much does luck matter to a criminal defendant in a jury trial? We use rich data on jury selection to causally estimate how parties who are randomly assigned a less favorable jury (as proxied by whether their attorneys exhaust their peremptory strikes) fare at trial. Our novel identification strategy is unique in that it captures variation in juror predisposition coming from variables unobserved by the econometrician but observed by attorneys. We find that criminal defendants who lose the "jury lottery" are more likely to be convicted than their similarly-situated counterparts, with a significant increase (18-20 percentage points) for Black defendants. Our results suggest that a considerable number of cases would result in different verdicts if retried with new (counterfactual) random draws of the jury pool, raising concerns about the variance of justice in the criminal justice system.

---

*Assistant Professor, Dept. of Economics, University of Florida—skostyshak@ufl.edu, 352-392-0403

**Professor, Georgetown University Law Center; Associate Dean for Research and Academic Programs; Affiliated Faculty, Georgetown McCourt School of Public Policy—neel.sukhatme@law.georgetown.edu, 202-662-4035

(first version: November, 2018). The authors thank participants at the 2019 American Law and Economics Association Annual Meeting (NYU Law School), the 2019 Conference on Empirical Legal Studies (Claremont-McKenna College), the 2020 Armenian Economic Association Annual Meetings, Boston University Law and Economics Workshop, the Chicago-Kent Law Faculty Colloquium, the University of Chicago Law and Economics Workshop, the University of Florida Department of Economics Workshop, the Georgetown Law Summer and Spring Faculty Workshops, the Georgetown McCourt School of Public Policy Faculty Workshop, the University of Missouri Department of Economics Workshop, and the Wake Forest University Department of Economics Workshop. The authors also thank David Abrams, George Akerlof, Michael Bailey, Rafael Calel, Adam Chilton, Tom DeLeire, Fran Flanagan, Michael Frakes, Brian Galle, Jonah Gelbach, Jacob Goldin, Anna Harvey, Todd Henderson, Randi Hjalmarsson, William Hubbard, Keith Hylton, David Hyman, Vardges Levonyan, Mike Meurer, Julie O'Sullivan, Anna Offit, Kyle Rozema, Steve Salop, David Sappington, David Schwartz, Mike Seidman, Abbe Smith, Josh Teitelbaum, and Kathy Zeiler for helpful comments. Finally, special thanks to Alex Billy, Sara Burriesci, Flora Feng, Jeremy McCabe, Andrea Muto, Ariel Polani, Arturo Romero Yáñez, Takayuki Sasaki, Douglas Turner, and Jonathan Zimmer for excellent research assistance. Any errors are the authors' own.

1 Introduction

> If it is a criminal case, or even a civil one, it is not the law alone or the facts that determine the results. Always the element of luck and chance looms large. A jury of twelve men is watching not only the evidence but the attitude of each lawyer, and the parties involved, in all their moves. Every step is fraught with doubt, if not mystery.
>
> —Clarence Darrow, "How to Pick A Jury," *Esquire* – May 1936

For many people, an ideal of the criminal justice system is that cases should be decided based on their merits, not based on who decides the case. Scholars have focused considerable attention on the variation of case outcomes driven by judges, but less on the variation driven by juries. This gap in attention is notable, since juries determine the guilt or innocence of 100,000 criminal defendants in the United States each year,[1] while affecting plea bargaining negotiations for hundreds of thousands more.

One reason why there is less causal research on the impact of juries as compared to judges is that measuring variation driven by juries is more difficult. Because most judges hear hundreds of cases each year, researchers can exploit the generally random process by which judges are assigned to cases, a process that litigants typically cannot manipulate. By contrast, jurors usually serve at most only once every few years, and often do not even sit on a final jury. And while there is random assignment of an initial pool of jurors to a case, there is a subsequent *non-random* process to select the "jury box", which contains the jurors who vote on the final verdict. Most notably, the prosecution and defense can exercise a limited number of "peremptory strikes" to block specific jurors in the pool from inclusion in the jury box. Such strikes are allowed in an attempt to remove jurors that, due to personal experience or preferences, might be greatly predisposed against a particular litigant.

These technical challenges have limited researchers when exploring the importance of the jury in shaping case outcomes. As some countries are moving away from trial by jury while others are incorporating more of it in their judicial systems,[2] policy makers would benefit from additional research highlighting the variation in oucomes due to juries.

A number of recent empirical papers have used this randomization to measure how natural variation in the jury pool affects case outcomes. A common approach has been to use ordinary least squares (OLS) to regress case outcomes on statistics that capture various pool demographics. Most notably, Anwar et al. [2012] find that juries created from all-white jury pools are significantly more likely to convict Black defendants than juries formed from pools that contain even one Black person, whether or not that person is not seated on the final jury. Anwar et al. [2014] conduct a similar analysis for age, arguing that increasing the number of older jury pool members raises conviction rates. More recent papers by Hoekstra and Street [2021] and Flanagan [2018] similarly rely on pool variation to find, respectively, that broader gender and racial variation in the jury pool can affect conviction rates.

[1] Judges guide the trial and often decide the type and degree of punishment if a jury finds the defendant guilty.

[2] `https://www.economist.com/international/2009/02/12/the-jury-is-out`

While undoubtedly pathbreaking, these prior OLS-based approaches are prone to measurement error because they rely on randomization in the jury pool, but only a subset of pool members are ever seated on the final jury. Some papers, such as Anwar et al. [2014], attempt to correct for this by using instrumental variables (IV). Specifically, they estimate the proportion of final seated jurors that share a certain demographic characteristic based on the proportion of initial pool members with that characteristic.[3] For this approach to yield a consistent estimator, however, one must also satisfy a strong assumption: the proportion of pool members who share a demographic characteristic must affect case outcomes *only* by influencing the proportion of final seated jurors with that characteristic.

The model of Anwar et al. [2012] walks through a specific case where the assumption required for IV might be violated. Suppose a prosecutor believes Black jurors favor Black defendants more than white jurors do. He might use some of his limited peremptory strikes to remove Black jurors in such a case, thereby preventing him from using those strikes on non-Black jurors who favor the defendant. As such, even if the final jury box does not include a Black juror, it will be more favorable for a Black defendant than if the initial pool had no Black members. In this scenario, the proportion of Black jurors in the initial jury pool correlates with the predisposition of *non-Black* seated jurors, which in turn affects the case outcome.[4]

More fundamentally, all of the identification strategies in the prior literature are limited to: (a) capturing differences in explicitly specified variables; (b) that are observable to the econometrician; (c) when the initial jury pool is created. Primarily, this includes demographic information, such as the racial, gender, and age composition of the jury pool. These prior approaches do not capture information revealed to the litigants but not directly measured in the data. And they are unable to exploit relevant information revealed after the jury pool is created. Most notably, this includes information gleaned by attorneys during voir dire,[5] the process by which attorneys question and actively interact with potential jurors in order to identify precisely what is of interest here—juror predisposition.

Our paper surmounts these prior limitations by introducing a new identification strategy that uses information revealed from attorneys' use of peremptory strikes to capture how random variation in juror predispositions affects case outcomes. Our key insight is that peremptory strikes contain information on how favorably a litigant views the randomly assigned jury pool. Specifically, we use strikes as proxies for hidden information on juror predispositions that is observable to attorneys during jury selection but is otherwise unobservable to the econometrician. As compared to prior identification strategies, our approach captures how case outcomes depend on random variation in jury pools that is otherwise unobservable to

[3] More recently, Hoekstra and Street [2021] exploit the randomness of the ordering of jurors in the pool rather than just using the pool composition as an instrument. Exploiting the ordering allows for the authors to "[isolate] the as-good-as-random variation in the gender composition of seated juries."

[4] Anwar et al. [2012] recognize this indirect channel would violate the exclusion restriction of IV in this setting, noting that IV requires a "strong assumption that the only channel through which the presence of Blacks in the jury pool affects trial outcomes is by increasing the likelihood of having Blacks on the seated jury. If, on the other hand, any of the indirect channels are important, the IV estimates do not have a clear interpretation . . . ."

[5] Information revealed during voir dire might include, for example, a prospective juror's occupation, whether he is married, has children, wears glasses, dresses in a suit, acts annoyed, has a strong build, or avoids eye contact during questioning.

the econometrician, but is observable to attorneys during jury selection. In addition, our identification strategy does not rely on the exclusion restriction required by IV.

Of course, a litigant's use of peremptory strikes not only reveals his view of the initial jury pool but shapes the final jury as well. To account for this, we focus primarily on cases at or just below the peremptory strike limit—the maximum number of strikes that state law permits a party to exercise in a case. By focusing on the strike limit, we can distinguish those cases in which a litigant might have wanted to strike more jurors but could not do so (because she ran out of her n strikes) versus those cases in which the litigant chose to stop just one strike short of the limit (i.e., she used only $n-1$ strikes). The identifying assumption is that these two sets of cases are on average the same except for underlying differences in jury composition.

We apply our identification strategy to data from a large, racially diverse county in Florida, for all non-capital felony and misdemeanor jury trials that took place from January 2015 through September 2017. Our results reveal that random variation in final jury composition has a significant effect on defendants, particularly Black defendants. We find that defendants who are assigned a jury pool for which they exhaust their peremptory strikes fare significantly worse in jury trials than similarly-situated defendants who use one less strike than the limit. This result is driven by Black defendants—for them, strike exhaustion raises the chances of conviction by 18–20 percentage points. As such, Black defendants in our sample are subject to more variation in the "jury lottery" in terms of the type of jury they might draw. We also provide reasons why our estimates are, if anything, likely to be a lower bound on the effect of jury composition on conviction rates.

Because our identification rests critically on the assumption that strike exhaustion is uncorrelated with non-juror determinants, we test this assumption using a rich set of observables. These include, for example, adding controls for other strikes used by parties (e.g., for cause strikes, peremptory strikes by the opposing party, and a dummy for whether the opposing party exhausted its strikes); total counts charged; defendant demographics; prior imprisonment history; attorney experience and education; type of offense charged; type and strength of prosecutorial evidence in a case; fixed effects for year of jury selection and presiding judge; and observable attributes of the jury pool, including juror race, gender, age, estimated income, and political affiliation. The coefficient of interest remains remarkably stable and significant across most specifications.

We also perform a number of robustness checks to indirectly test the assumption regarding unobservables. For example, we test among different subsets of our population that are especially likely to be similar, cross-check with different measures of defendant guilt, compare different measures of strike exhaustion, and conduct placebo tests using cases in which parties do not reach the strike limit. Our results remain robust across these different specifications.

Our paper provides at least two new contributions to the existing literature. First, our new identification strategy allows us to identify how variation in juror predisposition affects case outcomes without observing this predisposition directly. Previous approaches required one to pre-specify what observable juror characteristics (e.g., race, age, or gender) might affect juror predisposition; our identification strategy captures this variation as well as differences *within* observable groups. Indeed, our approach captures all feasible information on juror predispositions, as interpreted by attorneys and revealed during questioning, thereby uniquely positioning us to estimate the unconditional effect of the jury lottery on case outcome to the fullest extent possible.[6]

Second, our paper is the first to provide causal evidence on how peremptory strikes—and the limits placed on those strikes—affect conviction rates. This is important from a policy perspective: while all states permit some form of peremptory strikes in criminal cases, there is significant heterogeneity among courts in terms of the procedures they follow. We hope our research will inform ongoing policy debates as to the merits and demerits of these differing approaches. In particular, our results suggest that contrary to conventional wisdom, increasing peremptory strike limits could benefit defendants, particularly Black defendants, by decreasing the variance in outcomes for similarly-situated individuals.

The rest of the article proceeds as follows. The next section of the paper discusses the relevant prior literature. Section 2 provides more details on our identification strategy. Section 3 briefly lays out how jury selection and peremptory strikes are used in practice and describes our data. Section 4 presents our main results, and Section 5 provides numerous robustness checks. Section 6 briefly lays out potential policy implications of our study and concludes.

1.1 Related Literature

An early analysis of jury selection and peremptory strikes was conducted by Zeisel and Diamond [1977], who worked with a federal district court in Illinois to create "mock juries" composed of struck and unused jurors from jury selection conducted in 12 actual criminal trials. The mock jurors were then presented with abridged facts for the cases they would have adjudicated and were asked to render a verdict. Among other things, the authors found that peremptory challenges appeared to change the verdict for at least one case. Other studies, as summarized in Devine et al. [2001], have also used mock juries to study how jury composition affects outcomes.[7]

More recently, a seminal paper by Anwar et al. [2012] uses variation in people randomly assigned to a jury pool to measure how the racial composition of the jury pool affects trial outcomes. Using data from two rural Florida counties (Sarasota and Lake), they find that juries formed from all-white jury pools are 16 percentage points more likely to convict Black defendants relative to white defendants. This effect is eliminated if the pool happens to include at least one Black person, even if that person is not seated on the final jury.[8]

[6]Our proxy variable does not capture juror predispositions that are unobservable to the attorneys and are not revealed through questioning, but this variation seems unlikely to be captured by any method.

[7]Baldus et al. [2001] analyzed how peremptory strikes were used in 317 capital murder cases tried by jury in Philadelphia in the 1980s and 1990s. Diamond et al. [2009] find that peremptory strike use was related to juror race/ethnicity in 277 civil jury trials, but that parties' use of strikes tended to cancel one another out. A few other papers, such as Flanagan [2015] and Ford [2010], created formal models of the peremptory strike process.

[8]See also Alesina and La Ferrara [2014], who find higher reversal rates for minority defendants who were convicted of killing white victims in Southern states, suggesting bias at the trial level. Other recent empirical papers that explore the impact

Anwar et al. [2014] build on this work by using the same quasi-random variation in jury pools to measure how the age of jury pool members can affect conviction rates. The paper presents both reduced form (direct effect of pool variation on outcomes) and IV (effect of variation in the final seated jury on outcomes) estimates that show a strong effect, whereby defendants randomly assigned to "older" pools are much more likely to be convicted than those randomly assigned to "younger" pools. They also find that prosecutors used peremptory challenges to remove younger members in the jury pool, while defense attorneys used such challenges to remove older members.

More recent papers apply the same identification strategy to measure how variation in other characteristics of jury pool members affects case outcomes. Flanagan [2018] applies these OLS and IV techniques to North Carolina data, finding that the presence of more white jurors in a pool is associated with higher conviction rates, and that prosecutors tend to strike potential Black jurors and defense attorneys tend to strike potential white jurors. Hoekstra and Street [2021], using data from two other Florida counties (Palm Beach and Hillsborough), exploit the random pool assignment and, in a novel contribution, the random *ordering* of jurors in the pool to construct a weighted average of the characteristic of interest—in their case, whether a juror has the same gender as the defendant. The weight for each juror in the pool is the estimated unconditional probability that a juror in that position is selected into the box. They find that increasing the weighted average by one standard deviation (~10 percentage points) reduces conviction rates on drug charges by 18 percentage points.

2 Identification Strategy

In this section, we present and discuss the assumptions required for our proxy variable to yield a valid estimator of the effect of the jury lottery on criminal case outcomes. The first link in our identification chain recognizes that differences in average conviction rates for defendants who use all of their peremptory strikes (n strike group) and those who use one less strike than the limit ($n-1$ strike group) are most closely a measure of attorney beliefs regarding the favorability of a particular jury pool. The next link in the chain is from attorney beliefs to reality. It seems reasonable that attorneys are best positioned to assess the predisposition of potential jurors. Further, attorneys have strong incentives (greater than, for example, a judge) to develop skills to assess juror predisposition, given their role as advocates and repeat players in courts.

Our identification strategy solves an unobserved variable problem. Ideally, we would like to observe in some direct way whether potential jury members are more favorable to the prosecution or the defense, as compared to the average jury member. No such data contain this information, and it is difficult to even imagine an ethical and feasible method to capture such information directly. By measuring juror

of race within the criminal justice system include Rehavi and Starr [2014] (prosecutor charging decisions), Abrams et al. [2012] (likelihood of incarceration), and Arnold et al. [2018] (bail decisions).

predispositions through the lens of a litigant, our proxy variable picks up as much variation as feasibly possible given the data available.

Our basic identification strategy is driven by the subsample of cases in the n and $n-1$ categories. Without valid extrapolation, these results might be interesting only to the extent this category represents a considerable subpopulation. In fact, 61.6% of our cases involved parties who used either n or $n-1$ strikes (defendants used n or $n-1$ strikes in 54.7% of cases and prosecutors used n or $n-1$ strikes in 38.4% of cases).[9]

The main assumption needed for identification is that there is no difference on average between n strike trials and $n-1$ strike trials that affects case outcomes except for differences in jury composition. In practice we just need this assumption to hold conditional on non-jury-composition observables, since we test the sensitivity of our results in various conditional specifications. For example, one way in which the assumption might be violated would be if attorney quality is correlated with the number of strikes used; then the variation of trial outcomes between n and $n-1$ cases would pick up the effect of attorney quality. To address this possibility, we include various controls for attorney quality, such as practice experience, ranking of law school attended, use of strikes in other cases, and whether the defense attorney is a public defender.

Based on the framework discussed above, we can make an intuitive prediction: we expect a party's win rate to be higher for cases in which $n-1$ strikes are used, as compared to cases in which n strikes are used. The magnitude of this predicted effect, and whether it impacts prosecutors and defendants differently, likely depends on the underlying distribution of jurors. By exploring this prediction and estimating the difference between the two strike groups, we can estimate how random variation in jury composition affects case outcomes.[10]

3 Background on Jury Selection and Data

3.1 Background on jury selection

Applying our identification strategy to the data requires a deeper understanding of how jury selection works in Florida, which conducts the process in a manner typical of many states. The state maintains a list of potential jurors who may be summoned, based on individuals who have received a drivers' license or state identification card. Eligible jurors must be U.S. citizens, at least 18 years of age, who are not convicted of a felony and are residents of the county in which they are to be summoned.[11]

[9]The prevalence of the n and $n-1$ groups is not unique to our data. For example, tabulating North Carolina felony jury data from 2010–2012 as reported in Flanagan [2018], we can see prosecutors and defendants used either n or $n-1$ strikes in 17.4% and 41.0% of cases, respectively.

[10]We formalize this intuition in a model in the Appendix.

[11]Some jurors may request to be excused from participation for legitimate reasons, such as if they have already been summoned and reported within the last year, they are above 70 years of age, they have a medical condition that makes them unable to serve, they are an expectant mother, or they are not employed full-time and are a parent of a child less than 6 years

If a jury trial is anticipated on a particular date, a jury administrator will summon a certain number of potential jurors from an eligible list of jurors for that week. The administrator then picks a random subset of the jurors who appear for jury duty to create pools for particular cases. These groups of jurors, ordered by a randomly assigned number, are then sent to a specific courtroom, where jury selection can begin.[12]

The judge then begins voir dire—the process of asking potential jurors questions to whittle down the jury pool into the final jury. The judge first explains some basic aspects of the case to jurors and then asks each juror a standard series of questions that pertain to their ability to be fair and impartial during the trial. The prosecuting and defense attorneys can then ask follow-up questions for particular jurors. Both sets of attorneys are present for the entire questioning.

Typically the pool then leaves the room while the attorneys propose who should be struck from the pool for cause. These are individuals who, based on their answers during jury selection, an attorney argues will be unable to serve fairly and impartially at trial. The judge may agree to strike these individuals and may excuse others at her own discretion as well, if she finds they are unable to serve impartially or believes jury service would impose a considerable hardship on them. In the sample, 30.8%, 2.7%, and 2.3% of all potential jurors in jury pools were struck for cause by the judge, prosecution, or defense, respectively.

Next, jury selection proceeds to peremptory strikes. Unlike for cause strikes, an attorney need not provide any reason why a juror should be struck when exercising his peremptory strikes. The only requirement is that parties cannot strike based on the juror's race, gender, or national origin.[13]

If a juror is struck either for cause or via a peremptory challenge, the next juror in line will take his place. Both the prosecution and the defense know who this person is before they exercise their strike, and have the same type of information on the potential replacement as they do on the person they might strike, since all potential jurors are questioned in front of both parties before peremptory strikes begin.

---

of age. *See* Fl. St. § 40.013. A large number of jurors also fail to show up for their summons and do not provide an excuse. These jurors might be summoned again; some jurisdictions in the United States even pursue criminal charges against such jurors if they repeatedly do not show up when summoned, though such charges are rare.

[12]Following Anwar et al. [2012], we test whether jury pools are actually randomly assigned in our jurisdiction by regressing certain observable pool characteristics—the proportion of female and Black jurors, average juror age, median juror income (calculated based on the median income in the zip code in which a juror lives), proportion of registered Democrats, and proportion of registered Republicans—on various defendant, attorney, and case characteristics. Our results are shown in Appendix Table A.1. Out of the 96 pair-wise comparisons in the table, there are nine that are significant at the 5% significance level or less. The magnitudes of all coefficients are very small. While a joint F-test suggests as a whole this specification might have one or more coefficients that differ from 0, all of the estimated coefficients are very small. Moreover, including these summary jury pool measures in our regression specifications does not affect our results. On the whole, these results support an inference that the jury pools were indeed randomly constructed.

[13]See *Batson v. Kentucky*, 476 U.S. 79 (1986) (race); see also *J.E.B. v. Alabama ex rel. T.B.*, 511 U.S. 127 (1994) (gender). If a party suspects the opposing attorney is using his peremptory strikes in an impermissibly discriminatory manner, she may challenge a strike as it is made. The attorney who made the peremptory strike must then proffer a race- and gender-neutral reason for it. The party that issued the *Batson* challenge can then counter the striking party's explanation and explain why it is pretextual. The judge immediately decides whether to allow or disallow the peremptory strike; if she does the latter, then the attorney does not lose that peremptory strike and can use it to strike another juror.

Both sides are provided the same number of peremptory strikes, with the strike limit set by the type of case. In Florida, parties receive 3 peremptory strikes for misdemeanors (maximum punishment of up to one year imprisonment), 6 strikes for most felonies (maximum punishment of at least one year but less than life imprisonment), and 10 strikes for cases involving charged felonies in which a defendant might receive life imprisonment. In our data, 9.6% and 11.8% of all potential jurors were struck by peremptory challenges from the prosecution or the defense, respectively.

During the peremptory strike process, the parties start with the first remaining jury candidate and proceed one by one through the pool. First the prosecution decides whether to strike a candidate, then the defense. Once both parties have exhausted their strikes or affirmatively declined to use all of their strikes, then the final jury is set and comprises the first 6 people in the jury pool who have not yet been struck.[14] Throughout this process, the jurors are typically not present and are not informed why any particular juror was struck or by whom she was struck.

Table 1 illustrates how voir dire might play out in a hypothetical non-life-eligible felony case. In this example, jurors 1, 2, 7, 8, 13 and 20 were struck by the judge for cause, jurors 11 and 24 were struck by the defense for cause, and jurors 15 and 27 were struck by the prosecution for cause. The prosecution and defense then exercised their peremptory strikes, with the defense striking jurors 4 and 5, the prosecution striking juror 9, and so on. In the end, the defendant exhausted all 6 of his peremptory strikes, whereas the prosecutor only used 4 of her strikes. The final 6-person jury comprises jurors 3, 6, 10, 12, 21 and 25. In our data, 22.9% of jury pool members were neither struck nor used, and 20.0% ended up on the final jury.

3.2 Summary statistics for strike groups

Our data come from a large, racially diverse county in Florida, and they comprise all non-capital felony and misdemeanor cases in which jury selection was conducted from January 2015 through September 2017. The data include detailed information on the trial participants, including the name, race, gender, age, and address of the defendant, and the names of the presiding judge and attorneys in the case. Using data from the Florida Department of Corrections, we can also control for a defendant's criminal record by measuring the number of times he has previously been imprisoned in Florida state prison, and for how long. Using Florida state bar records, we supplement these data with information on the attorneys' practice experience and educational background. We also have data on which charges were brought against each defendant under which statutory provision. Our data further include information on the potential jurors in each case, including their demographic information, their juror number, and whether they were struck for cause (by the judge, prosecution, or defense), struck under a peremptory challenge (by the prosecution or the defense), not used in voir dire, or seated on the final jury.

[14]Most states use 12-person juries for most crimes. Florida differs in that it uses 6-person juries for all crimes except capital offenses, for which it uses 12-person juries. We exclude all capital cases in our analysis.

We have a total of 567 cases involving a single defendant[15] for which a jury was selected and neither party exceeded the peremptory strike limit.[16] Out of these cases, the parties tried the case to a jury verdict on 511 occasions (90.1% of the time). To the extent prosecutors and defendants can identify whether a particular jury is good or bad for them, we should expect jury selection to affect both cases that are tried to a verdict and cases that settle after voir dire is conducted. As such, in our primary specifications, we look at all case outcomes (not just cases that proceed to jury verdicts). Nonetheless, and as discussed below, we also test numerous specifications in which we limit our sample to cases in which a jury actually rendered a verdict, and we find our results remain largely similar.

We have a total of 213 misdemeanors, 273 non-life-eligible felonies, and 81 life-eligible felonies. They span the full range of criminal offenses commonly prosecuted in state court, including driving under the influence, battery, burglary, robbery, theft, drug offenses, sex offenses, and murder. The defendants are 55.38% Black, 31.39% white, 12.52% Hispanic, and 0.35% each Asian and Indian.

Our identification strategy is similar to regression discontinuity in that we are using strike limits as a way to distinguish jury pools with exogenous variation in how pro-prosecution or pro-defense they are. Our treatment groups in most specifications are parties who exhaust their peremptory strikes (n strikes); our control groups are parties who use one less strike than the limit ($n-1$ strikes), though we test other control groups (e.g., parties who used $< n$ strikes), as discussed below.

Figure 1 shows the distribution of cases by number of peremptory strikes used by defendants across different offense categories and for all cases in our dataset. For both non-life-eligible felonies and misdemeanors, the two strike categories with the most cases are the n and $n-1$ categories, with fewer cases in categories with fewer strikes. For life-eligible felonies, the distribution is shifted to the left and is centered on 6 strikes ($n-4$). Across all offense categories, the defendant exhausted all of his strikes in 31.1% of the cases, and used one less strike than the limit in 20.9% of the cases. These are the two most prevalent outcomes.

One can see the number of peremptory strikes used drops sharply above the limit for misdemeanors and non-life-eligible felonies, providing suggestive evidence that this limit is a binding constraint on defendants in a large number of cases in those offense categories.[17] The few cases (5.0%) with more strikes than the limit are the rare situations in which the presiding judge allowed defendants to use more than their allotted share (something we confirmed is uncommon through conversations with practicing attorneys in the jurisdiction).

[15]We exclude cases in which multiple defendants are tried jointly, since peremptory strike limits are different in those cases. We also exclude cases in which voir dire was conducted multiple times.

[16]We exclude 37 cases in which one or both parties exceeded the peremptory strike limit. As discussed below, these are likely the rare instances in which a judge raised the limit.

[17]By contrast, we see no such pattern for other, non-peremptory strikes. For example, Figures A.1, A.2, and A.3 in the Appendix show the distribution of judge-issued, defense, and prosecution for cause strikes, respectively, across the three broad offense categories. The spread of such strikes is much larger than in the peremptory strike context, with no discernible cutoff (since there is no limit to how many of these strikes can be granted).

Figure 2 shows an analogous distribution of cases by number of prosecution peremptory strikes. While the overall shape of the distribution is similar, one can see the modal number of strikes for prosecutors falls to $n-1$ for misdemeanors, $n-3$ for non-life-eligible felonies, and $n-6$ for life-eligible felonies. Overall, prosecutors use fewer peremptory strikes than defense attorneys; as such, the data suggest that peremptory strike limits are less likely to serve as binding constraints on their choice of jurors. Across all offense categories, prosecutors exhausted all of their strikes in 17.1% of the cases, and used one less strike than the limit in 21.4% of the cases. They exceeded the strike limit in 1.8% of cases.

We now examine whether various baseline case-specific characteristics vary across prosecution and defense strike classes. Table 2 shows mean values within strike classes for variables that capture defendant demographics and prior criminal history; attorney experience and education; and jury pool characteristics. Table 3 does the same for other case-specific characteristics, including which offenses are charged and the types of evidence against a defendant in a case. Both tables include results for two-sided t-tests for differences in means between the n and $n-1$ groups, as well as data on all cases in our sample and on cases with $n-2$ strikes (i.e., those in which a party used two less strikes than the limit).

We can see that broadly speaking, criminal defendants, attorneys, and case and jury pool characteristics[18] appear comparable across different strike groups. There are no statistically significant differences in terms of defendant age, or how likely a defendant is to be Black, Hispanic,[19] or female across the n and $n-1$ strike groups. Similarly, there are no statistically significant differences across the n and $n-1$ strike groups in terms of how many prison stints a defendant has previously served, though n strike defendants have served on average 0.6 years more prior years in state prison relative to the $n-1$ strike group (significant at the 10% level). Defendants in both strike classes face similar numbers of counts in the current case against them.

Regarding attorney characteristics, defense attorneys who use up their strikes appear to have attended slightly better ranked law schools[20] than their counterparts who had one strike remaining, and they face slightly less experienced prosecutors. To the extent law school ranking or attorney experience correlates with attorney quality, we might expect this to downward bias our estimate of the impact of defendant strike exhaustion on case outcomes. Otherwise, there are no statistically significant differences in terms of defense or prosecutor experience, whether the defense attorney is a public defender, or in terms of the ranking of the prosecutor's law school. At any rate, we control for these and other observable characteristics in our regressions below and find our results to be largely unchanged.

[18]One difference is that prosecutors appear to have slightly more registered Republicans in the jury pools from which they exhaust their strikes as compared to those pools in which they use one less strike than the limit. This should not impact cases in which defendants use n versus $n-1$ strikes.

[19]Because the data reported most ethnically Hispanic defendants as white, we use a standard R-package, ethnicolr, that relies on Florida voter registration and Wikipedia data to predict race and ethnicity based on first and last name. See https://github.com/appeler/ethnicolr. Following Arnold et al. [2018], we chose a cutoff of 0.7 predicted probability of being Hispanic in determining whether to categorize a white defendant as such. Our results do not depend substantially on which threshold we use to determine whether a defendant is Hispanic.

[20]Rankings are based on the 2018 U.S. News & World Report Rankings, commonly used to rank law school.

Turning to Table 3, there are no statistically significant differences between the n and $n-1$ strike classes at the 5% level in terms of the relative prevalence of common classes of cases.[21] Figures 3 and 4 show this graphically, comparing the relative frequency of these cases across strike classes for both the prosecution and defense. Once again, we see no large deviations in the relative prevalence of certain types of cases in particular strike categories. These results are largely confirmed by two-sided t-tests for all pairwise comparisons within offense groups and across strike classes.[22]

Because even similarly charged cases might include significant heterogeneity, we gathered more detailed information on each case by reviewing its probable cause affidavit. Such affidavits list the alleged facts in each case, as collected by law enforcement when an arrest is made and as sworn before a court. Importantly, the affidavits reveal what evidence the prosecution has, which helps determine the strength of the case. We coded for whether the affidavits describe any of the following types of prosecutorial evidence: (1) photos or video that show the defendant committing the alleged offense; (2) incriminating items that were recovered from the defendant's possession, dwelling, vehicle, or place of business; (3) physical (forensic) evidence that links the defendant to the offense and requires expert analysis and testimony (e.g., DNA, fingerprints, tire or shoe tracks, ballistic materials, trace fibers, recovered computer files); (4) documentary evidence (e.g., bank, business, phone, email, or medical records linking the defendant to the offense; breathalyzer or other intoxication test results); (5) a confession by the defendant to the police, whether orally or in writing; and (6) any admission by the defendant against his/her interest, which might fall short of a full confession and is not necessarily made as part of formal statement to police.[23] The presence of forensic evidence, recovered items, documentation, a confession, or an admission are strongly positively correlated with guilty outcomes for cases in our sample; the presence of surveillance photos/videos was also positively correlated with guilty outcomes, but below conventional levels of statistical significance.

Table 3 shows that there are no statistically significant differences between the n and $n-1$ strike classes at the 5% level in terms of the relative prevalence of these different kinds of prosecutorial evi-

[21] These are the same offense classes as used in Anwar et al. [2012] and subsequent papers. At the 10% significant level, it appears that defendants who exhaust their strikes are marginally more (less) likely to be charged with violent offenses other than murder (property offenses), and prosecutors who exhaust their strikes are marginally more likely to be involved in cases involving other offenses.

[22] A two-sided t-test for all 42 pairwise comparisons between the strike classes yields 41 insignificant comparisons at the 0.05 level, with only the relative frequency of drug crimes significantly decreasing between the $n-2$ and $n-1$ prosecution strike classes.

[23] In addition, we coded whether the underlying offense took place in a correctional setting (i.e., prison or jail) or not. This variable does not appear to be correlated with guilty outcomes in our sample, and including it as a control does not substantially affect the results. We also attempted to code for the presence and identity of eyewitnesses for each case; however, there was significant variation across different coders because this information was difficult to gather from the affidavits. At any rate, this variable does not appear correlated with guilty outcomes in our sample, and including it as a control does not substantially affect our results.

dence. This suggests there is no systematic difference between these strike classes in terms of the quality of evidence against the defendant.[24]

4 Results

4.1 Peremptory strikes and conviction rates

We now explore how case outcomes differ when a litigant exhausts all of her peremptory strikes (i.e., n strikes used) as compared to when she has exactly one strike remaining (i.e., $n-1$ strikes used).[25] Table 4 presents our main regression results. Following Flanagan [2018], our outcome variable is the proportion of charges on which a defendant was convicted[26] that were yet to be decided as of the date of jury selection (i.e., were not dropped or otherwise adjudicated before that date). Our preferred specification thus captures nuances in mixed outcome cases, such as when a jury convicts on one count but acquits on another.[27]

Our coefficient of interest is a dummy variable for "Exhausts Strikes." In columns (1)–(3), this is a dummy for whether the defendant used up all of his peremptory strikes (and = 0 otherwise); in columns (4)–(6), it is a dummy for whether prosecutor used up all of her peremptory strikes. Similarly the control variable $n/n-1$ Group is a dummy for whether the defendant used either n or $n-1$ peremptory strikes; in columns (4)–(6), it is a dummy for whether the prosecutor used either n or $n-1$ peremptory strikes. Because the number of strikes allowed differs across misdemeanors and the two classes of felonies, all specifications also include controls for whether the defendant was charged with a felony and whether it was a life-eligible offense. With these controls, Exhausts Strikes measures how outcomes differ in cases

[24]At the 10% significance level, forensic evidence appears five percentage points less likely to appear in the n strike cases relative to the $n-1$ strike cases. This does not appear to impact our results, however—when we include it as a control (or alternatively exclude cases with any forensic evidence from our sample), our results remain substantially the same.

[25]Like the previous literature, we use ordinary least squares with heteroskedastic robust standard errors. Our results remain largely robust if we cluster at the defendant attorney level or judge level.

[26]In 26 cases in the dataset, the court "withheld adjudication" on at least one count, which means the defendant was found guilty but was technically not deemed convicted by the presiding judge because the court believed he would be unlikely to recidivate. *See* Fl. St. § 948.01(2). Since our primary focus is on the effect of the jury rather than the judge on outcomes, we treat these cases as guilty outcomes, though our results remain substantially the same if we instead exclude these cases from our sample.

[27]Our results remain robust and substantially the same if we treat a defendant as guilty if he is convicted on at least one charged offense. But such specifications might introduce substantial measurement error, since they treat any mixed outcome case as a defendant loss (and mixed outcome cases comprise 25.4% of the dataset). To give a concrete example, one case in the dataset involves a defendant who was found not guilty by a jury of sexual battery and aggravated battery with a deadly weapon, but found guilty of assault. He received no additional prison term for the conviction (sentenced to time served). Characterizing this case as a total loss for the defendant is arguably inaccurate relative to our preferred approach, which categorizes this case with a guilty value of 0.333 to account for the fact that the defendant was found guilty on just one of the three charges he faced at the time of jury selection.

in which all peremptory strikes were used as compared to ones in which one less strike was used than the strike limit.[28]

Columns (2), (3), (5) and (6) also include dummy variables for whether the defendant was Black, Hispanic, or female, and controls for the defendant's age and the number of years he has previously served in Florida prison.[29] Columns (3) and (6) further include controls for the following case- and strike-specific characteristics: number of prosecution-, defendant-, and judge-directed for cause strikes (each tabulated separately); number of peremptory strikes used by the opposing side; a dummy for whether the opposing side exhausted its peremptory strikes; number of counts charged; and a fixed effect for the year in which jury selection occurred.

The coefficient on Exhausts Strikes is positive and significant at the 5% level in columns (1)–(3). The coefficient values indicate that strike exhaustion across all defendants increases the probability of conviction by about 12 percentage points. By contrast, the coefficient on Exhausts Strikes is insignificant for prosecutors.

As noted in the prior discussion on identification, if differences in jury composition are the only relevant differences between cases in which defendants use n rather than $n-1$ strikes (at least after controlling for observables), then these regression results have a causal interpretation. In particular, a defendant who receives a "bad" jury pool (i.e., one in which he is forced to exhaust his peremptory strikes) is about 12 percentage points more likely to be convicted of a crime than a similarly-situated defendant who does not use up his strikes.

4.2 Effects on Black and non-Black defendants

The above results suggest that strike exhaustion by a defendant is a better predictor of whether the defendant is found guilty than strike exhaustion by the prosecutor. We now examine whether this effect on defendants is homogenous or depends on a defendant's race.

Table 5 presents our results, using the same specifications from columns (1)–(3) in Table 4 but instead divided by race into Black and non-Black defendants. We find the probability of conviction increases by 18–20 percentage points for Black defendants who exhaust their peremptory strikes (n strikes used) as compared to Black defendants who use one less strike than the limit ($n-1$ strikes used). By contrast, the coefficients on peremptory strike exhaustion for non-Black defendants are between 0.04 and 0.07 and are statistically insignificant.

Our results indicate that Black defendants who exhaust their peremptory strikes are more likely to be convicted than Black defendants who use one less strike than the limit. Moreover, strike exhaustion increases conviction rates more for Black defendants than for non-Black defendants in our sample. Still,

[28] Our coefficient of interest on Exhausts Strikes remains largely similar if we apply the same regressions separately to misdemeanors and the two different classes of felonies, or if we exclude life-eligible felonies from our analysis, though some results lose significance as our decreased sample size increases our standard errors.

[29] Replacing or supplementing age with age^2 or $\ln(\text{age})$, or replacing years of past imprisonment with number of past imprisonments, does not meaningfully affect our results.

we cannot rule out that strike exhaustion has the same effect on non-Black defendants as it does for Black defendants when we generalize to the full population. In particular, when we regress the conviction dummy variable on whether a defendant is Black, whether a defendant expires his peremptory strikes, and the interaction of these two variables (along with controls for whether the underlying crime was a felony or a life-eligible crime), we obtain a positive but insignificant coefficient for the interaction. This null result might be due to our relatively large standard errors, which in turn are likely caused by the relatively small sample size of our study.

4.3 Within- versus between-group effects of jury composition

As noted, a key feature of our novel identification strategy is that it allows us to capture the full unconditional effect of the jury lottery—that is, differences across observable groups as well as differences within those groups. Previous approaches could only capture how pre-specified racial, gender or age differences in the randomly-assigned jury pool affect conviction rates. We build on this by further capturing how variation within identifiable race-gender-age groups (e.g., 30-year-old white men) might affect conviction rates.

We can compare whether within-group variation or between-group variation in prospective jurors has a greater effect on conviction rates in our sample. To do this, we include controls for the composition of the randomly-selected jury pool, before the parties exercise any strikes. We measure the proportion of Black jurors in the jury pool,[30] the proportion of women in the jury pool, the average age of members of the jury pool, the log median income of jury pool members (with each juror's income estimated by the median income for the zip code in which she resides, using U.S. Census data and 2017 inflation-adjusted dollars)[31], and the proportion of registered Democrats and registered Republicans in the jury pool prior to strikes.[32]

Table 6 shows the same specifications as in columns (1)–(3) of Tables 4 and 5, except with the race, gender, age, income, and political affiliation jury pool controls. Most of these controls do not have a statistically significant impact on case outcomes, with the exception of the proportion of registered Republicans in the jury pool and the effect of log median juror income, both of which are marginally statistically significant for Black defendants. Using standardized regression coefficients (not shown), we can see that increasing log median juror income (proportion of registered Republicans) in a jury pool by

[30] Anwar et al. [2012], who conduct their study in largely white counties, contrast cases in which there were any Black jury pool candidates versus cases in which there were none. Because our jurisdiction is significantly more diverse, there are very few cases in which there were no Black candidates in the jury pool (only 18 out of 567 total cases). As such, we cannot test their specifications here.

[31] Our results are substantially the same if we use median income instead of its log. We present log median income as a control in the regressions here rather than levels of median income because we expect the potential income effect to be concave.

[32] We obtained the political affiliation of potential jurors by matching jury pool data with Florida voter registration data using juror last name, zip code, and date of birth. We matched 70.89% of all jury pool members (17,077 out of 24,088) in our sample in this manner.

one standard deviation increases conviction rates by about 9.5 to 10.2 (10.7 to 11.6) percentage points for Black defendants.

Importantly, the coefficient on defendant peremptory strike exhaustion remains positive, statistically significant, and substantially the same magnitude as in prior specifications. The enduring importance of defendant strike exhaustion implies that within-group variation among jurors plays a more substantial role in case outcomes than previously recognized.

4.4 Bounds discussion

The large differences we find in conviction rates between the n and $n-1$ groups are likely just a lower bound on how jury composition affects case outcomes. This is true for at least two reasons. First, our results would measure the maximum possible difference between "unlucky" and "lucky" defendants only if defendants who used all of their n strikes were all assigned the worst possible jury pools, and defendants who used $n-1$ strikes were all assigned the best possible jury pools. One or both of these events seems unlikely; as such, the difference between the most unlucky and most lucky defendants is likely greater than what we measure here.

Second, it is unlikely that every defendant within the $n-$strike group ran out of peremptory strikes. Some of these defendants likely wanted to strike n, and exactly n, potential jurors and as such, the strike limit was not a binding constraint for them. Since we cannot distinguish those $n-$strike defendants from other $n-$strike defendants for whom the strike limit was binding (i.e., those who wanted to strike $n+1$, $n+2$, or more potential jurors), our estimate of the impact of jury composition is likely downward biased, with an actual impact even greater than what we measure here.

5 Robustness

The identifying assumption about differences between cases involving n and $n-1$ strikes forms the basis of our causal claim that variation in jury composition heavily affects whether a defendant—particularly a Black defendant—is convicted. In this section, we present other specifications to test various potential threats to our identification. In particular, we test whether variation in attorney quality, crimes charged, strength of prosecutorial evidence, judges, or other case-specific features might drive our results. We also test different outcome measures and measures of strike exhaustion, and we conduct placebo tests.

5.1 Attorney quality

Arguably the biggest concern might be that differences in attorneys are driving the results—namely, bad defense attorneys might be both more likely to use more peremptory strikes and more likely to attain worse outcomes for their clients at trial. We can try to control for any such differences through observable characteristics of the attorneys. In particular, we include controls for attorney experience (measured in years from their date of bar passage to the date of jury selection), ranking of law school attended, and whether or not the defense attorney is a public defender.[33]

Columns (1) and (5) of Table 7 present the results of including these covariates for all defendants and Black defendants, respectively, with the coefficient of interest once again whether the defendant exhausted his strikes in the category of cases for which either n or $n-1$ strikes were used. As one can see, the regression results remain largely unchanged (or are perhaps even slightly stronger) as compared to the coefficients in the baseline specifications presented earlier.

Still, it is possible that years of practice experience and quality of law school attended do not fully capture differences across attorneys, and that some unobservable attorney characteristics are driving both differences in the number of strikes used and the attorney's ability to help her client. As a further test, we include counts for the number of cases in our sample in which each attorney used n strikes and the number of cases in our sample in which they used $n-1$ strikes, to capture their propensity to exhaust or nearly exhaust their strikes. As a second test, we limit the sample to public defenders, who arguably share similar unobservable qualities with one another. As a third test, we limit the sample to public defenders and include the controls for number of cases in which attorneys used n and $n-1$ strikes.[34]

The results of these tests are presented in columns (2)–(4) and (6)–(8) of Table 7 for all defendants and Black defendants, respectively. Across all columns, we can see that including the controls for n and $n-1$ case counts and limiting the sample to public defenders does not weaken the results—if anything, the coefficient of interest is slightly stronger, while maintaining its statistical significance.[35]

5.2 Differences in crimes charged

Another concern might be that defendants who use n strikes are charged with different crimes that have different conviction rates than those who use $n-1$ strikes, even after controlling for broad offense groups (e.g., misdemeanors). We can control for this possibility through fixed effects based on more specific

[33] We code law schools that are unranked or whose rank is not published with a rank of 175 (the midpoint ranking if all unranked law schools were ranked), though our results are not sensitive to this choice. Also, measuring law school quality instead based on the natural log of law school ranking does not materially affect our results.

[34] As yet another test (not reported here), we limit our sample to cases involving attorneys who used both n strikes and $n-1$ strikes at least once in our sample. Such attorneys have already shown they are willing to exhaust or fall just short of the strike limit; as such, one might expect them to be similar to one another on unobservable dimensions that might bear on their decision to use strikes. Limiting the sample in this manner reduces the coefficient's magnitude and significance across all defendants, though it remains positive. However, it does not appreciably change the magnitude or statistical significance of the coefficient for Black defendants. In short, Black defendants who are represented by the subset of attorneys who have used both n and $n-1$ strikes in the sample, are public defenders, and have comparable practice experience and legal pedigree, are more likely to be convicted of a crime as compared to their Black defendant counterparts who use one less strike than the limit.

[35] We also test various specifications using prosecutor and defense attorney fixed effects (not reported here). Our coefficients remain positive and similar in magnitude, but the large number of fixed effects (138 different defense attorneys and 94 different prosecutors across 305 cases) greatly increases standard errors, often making the results insignificant.

offense categories under which the defendant was charged.[36] Following Anwar et al. [2012], we use the following offense categories: Homicide, Other Violent Offenses, Property Offenses, Drug Offenses, Sex Offenses, Weapons Offenses, and Other Offenses.[37]

Column (1) in Table 8 shows ordinary least squares regression results for all defendants after adding offense class fixed effects to the baseline regression; column (5) shows the same regression for Black defendants. Our coefficient of interest remains substantially the same and significant, suggesting that differences in crimes charged are not driving our results.

5.3 Heterogeneity in prosecutorial evidence

Even after controlling for crimes charged, one might be concerned that there is unobserved heterogeneity associated with the strength of the case that is correlated with the number of strikes used. This might be problematic if, for example, prosecutors typically have stronger evidence against defendants who use n strikes as compared to those who use $n-1$ strikes. To address this concern, we can quantify evidentiary strength by reviewing a case's probable cause affidavit, which lists the pertinent facts and provides detailed information on the types of evidence that law enforcement have collected at the time of arrest. Specifically, we can code for whether prosecutors have the following types of evidence that tie the defendant to the alleged crime: forensics, recovered incriminating items, surveillance photos/videos, documentation, a confession, or any admission of guilt by the defendant (including ones that fall short of a confession). We can also control for whether the crime occurred in a correctional facility or not.

We include all of these variables as controls in each specification presented in Table 8. While each of these types of evidence is separately positively correlated with guilty outcomes, none of them affects the magnitude or significance of our coefficient of interest (defendant strike exhaustion) when included as controls in our regressions. This provides greater confidence that unobserved differences in case or evidentiary strength are not driving our results.

5.4 Court/judge behavior

Yet another potential concern is that judges might treat parties who exhaust their strikes differently from those who do not. For example, suppose a judge is annoyed with a party who uses more peremptory strikes, because it causes voir dire to take more time, and hence the judge is more likely to rule against that party on motions raised in the subsequent litigation. This seems unlikely to be a concern at the voir dire stage, when most pre-trial motions have already been decided, and it seems even more unlikely when comparing cases in which $n-1$ strikes were used versus $n-$strike cases, since these two sets of cases are likely very similar in terms of resources expended by the court during jury selection.

[36] For a defendant who faces multiple charges, the classification is based on the lowest count remaining to be decided as of the date of jury selection (i.e., it was not dropped or otherwise adjudicated before that date).

[37] Our results remain largely robust if we use finer-grained offense categories instead.

Nonetheless, even if such an effect exists, we can arguably control for it by using judge fixed effects, which allow us to pick up judge-specific factors that might affect our result. Columns (2) and (5) in Table 8 present our results for all defendants and Black defendants, respectively. Once again, we can see our coefficient of interest on peremptory strike exhaustion remains substantially the same and is statistically significant.

5.5 Multiple effects

It is possible that more than one of the stories discussed in Sections 5.1 through 5.4 are true, and are working together to drive the results. While our limited sample size does not permit us to test all combinations that might affect the results, we can include all of the controls described above in a single regression. We present these results in columns (3) and (6) of Table 8, for all defendants and Black defendants, respectively. Again the coefficient of interest maintains the same magnitude and level of statistical significance, providing more suggestive evidence that our identification strategy is valid.

5.6 Other measures of strike usage

We can also show that our results remain robust to different measures of peremptory strike usage. For example, an alternate specification might compare the class of defendants who have exhausted their peremptory strikes (i.e., used n strikes) with any defendant who has used fewer strikes than the limit (i.e., $< n$ strikes). This change to our baseline specification involves removing $n/n-1$ Group as a control, and is shown in columns (1) and (5) of Table 9 for all defendants and Black defendants, respectively. As is apparent, the change does not materially affect our results and if anything increases the statistical significance.

As a further test, we can limit the sample to just those cases in which the prosecution exhausted all of its n strikes, or where it used $n-1$ strikes. While this approach greatly reduces the sample size, it also reduces the possibility of strategic interactions between prosecution and defense affecting our results. Columns (2) and (6) of Table 9 show our results when we limit the sample to cases in which the prosecution used up all its strikes; columns (3) and (7) show the results when the prosecution used $n-1$ strikes. Our results generally appear to be even stronger when we limit the sample either way—in other words, the effect of defendant strike exhaustion on conviction rates is even more pronounced when the prosecution has used either $n-1$ or all n of its strikes.

Finally, we might wish to limit the sample to just Black and white defendants, rather than comparing Black to non-Black defendants. This also does not significantly change results, as shown in column (4) of Table 9, primarily because the large majority of defendants in the sample is one of these two races.

5.7 Settlements and different outcome measures

If parties can identify whether a particular jury is good or bad for them, that might influence case outcomes even when the parties decide to settle a case after jury selection. As such, in our primary specifications, we look at all case outcomes, not just cases that proceed to jury verdicts. Looking at all case outcomes rather than just those that lead to verdicts also makes sense if we are concerned that parties might vary in their risk preferences and hence might vary in their willingness to settle after jury selection.[38]

Nonetheless, we can also compare our results to the subset of cases in which a jury issued a verdict. These specifications, as shown in Table 10, suggest our results remain relatively robust across different measures of guilt, with our coefficient of interest largely similar in magnitude to that in our primary specifications. This is true both when we look at all defendants (columns (1)–(5)) and when we limit our sample to Black defendants (columns (6)–(10)).

Columns (1) and (6) report a baseline similar to our primary specification, in which our outcome variable is the proportion of charges on which a defendant was found guilty in a jury verdict.[39] So if jury issues a guilty verdict on counts 1 and 3 but acquits on count 2, then the defendant would be assigned an outcome of 0.667. Again, our results are similar to our primary specifications and significant at at least the 10% level.

Columns (2) and (7) next report a specification in which a jury issued a guilty or not guilty verdict on at least one count, with cases coded as "guilty" if a defendant is found guilty on at least one count. Here, the coefficient on defendant strike exhaustion decreases slightly in magnitude and loses statistical significance. However, coding guilt in this manner introduces significant measurement error that might downward bias the coefficient, since 11.9% of cases that go to a verdict involve mixed verdicts in which a jury convicts on one count but acquits on another. Categorizing all of these cases as defendant losses is arguably inaccurate. Accordingly, the remaining columns in the table categorize these mixed verdict cases in other ways.

Columns (3) and (8) code a case as guilty or not guilty based on the jury's verdict on the lowest count it adjudicated, since the lowest count generally corresponds to the most severe charge a defendant faces. So if a jury issued a not guilty verdict on count 1 but a guilty verdict on count 2, then the case would be coded as not guilty. When coded this way, we can see our coefficients of interest increase and become statistically significant at the 10% level for all defendants and also when we limit our sample to Black defendants.[40]

[38]In our full dataset, there is a slight positive correlation between defendants who exhaust their strikes and those who proceed to trial, though the result is not statistically significant.

[39]Flanagan [2018] uses a similar specification as a robustness check.

[40]Still, focusing on the lowest charge is likely inaccurate for many cases as well. To illustrate, in one case in the dataset, a defendant was found not guilty by a jury on count 1 (second degree murder with a firearm) but guilty on count 2 (felon in possession of a firearm) and sentenced to 15 years in prison. Classifying a case like this as a defendant "win" is arguably misleading.

Columns (4) and (9) deal with mixed verdict cases by removing them altogether from the sample. The coefficient of interest in these specifications is similar in terms of magnitude and statistical significance to our primary specifications.

Columns (5) and (10) categorize mixed verdict cases as guilty if they result in a jail/prison sentence for the defendant and not guilty otherwise. Again, we find our coefficients are statistically significant and similar to our primary specifications.

5.8 Asymmetric information and risk aversion

Another concern might be that asymmetric information or differences in risk preferences somehow drive our results here. One issue might be if a defendant knows less about the potential replacement juror than the current juror under consideration. In that scenario, a more risk-averse defendant might be less likely to exercise a peremptory strike than a less risk-averse defendant. And if a defendant's risk aversion during jury selection is inversely related to the strength of the prosecutor's case—which might occur if a defendant who has a strong case feels less obliged to take risks during jury selection—then we might expect defendants who exhaust their peremptory strikes to have worse quality cases (and hence worse outcomes) than those who do not exhaust them.

Although such a concern might be relevant in other settings, it likely does not affect our results here. This is because in Florida, parties have the same information on all potential jurors in the pool, since all pool members are questioned at the outset by the judge and both parties.[41]

A variant of this concern might be that a defendant only knows the average predisposition for each juror in the pool rather than the full distribution of potential outcomes for each juror. To illustrate, suppose there are two otherwise identical Black defendants (defendant 1 and defendant 2), both charged with the same crime and facing the same potential prison sentence. Defendant 1 knows he is innocent and the evidence is on his side, so he expects there is a small chance of being found guilty. As such, he does not need to take chances during jury selection and can play it relatively safe. By contrast, defendant 2 knows she is guilty and the evidence is against her, so she expects a higher chance of being found guilty. This defendant's only chance of receiving a "not guilty" verdict or a hung jury is to take a chance on the jurors.

Suppose the current juror under consideration for either defendant is a white man, and the replacement juror is a white woman. Suppose also that all white men are identical in terms of their predisposition toward Black defendants, but there are two types of white women—those who are are very likely to convict a Black defendant and those who are very unlikely to convict a Black defendant—and that attorneys cannot distinguish between these two types. Even if, on average, a white woman is more likely to convict the defendant than a white male, defendant 2 might take a chance on the white woman since that is his

[41]There are apparently relatively few cases in which a court runs out of jury pool members, since jury administrators take into account the type of case scheduled for trial and a judge's preferences when deciding how many people to summon and how large the initial pool for a case should be. On the rare occasions when this occurs, information about potential jurors would be revealed in two separate stages of voir dire.

only chance to win the case. As such, defendant 1 and defendant 2 might diverge in terms of their striking behavior.

While such a concern might also be relevant, it is unlikely to affect our results because there is no reason to believe a higher variance juror (i.e., the white woman in the example above) will be more likely to appear in a replacement slot versus the current juror slot. This is because potential jurors are randomly ordered in the pool. So on average, risk aversion should not bias our results, even though it might affect how a particular defendant uses his strikes. Defendant 2 will end up with a riskier juror, but there is no reason to believe he is more likely to end up in the n strike class versus the $n-1$ strike class.[42]

5.9 Placebo tests

Finally, we can conduct placebo tests to test whether there is anything special about cases in which parties exhaust peremptory strikes. In particular, instead of comparing the difference between n and $n-1$ strike cases, we can imagine the strike boundary to be at $n-1$ instead, and hence compare $n-1$ and $n-2$ strike cases. If strike exhaustion really matters, as we posit it does, then we should see a much diminished effect[43] or no effect at all at the placebo boundary.

Table 11 shows the same specifications as in columns (1)–(3) of Tables 4 and 5, except now defendant strike exhaustion is defined at the placebo boundary. The coefficient on placebo strike exhaustion is insignificant and close to zero in all specifications, whether we look across all cases or just cases with Black defendants. These results provide additional support that differences in jury composition, and not just another variable correlated with an increased use of peremptory strikes, is what drives our primary results.

6 Conclusion

In this paper, we provide new evidence on how random variation in jury composition affects criminal case outcomes. Our approach differs from previous related empirical work, which focused only on the effect of individual variables observable at the time the initial jury pool was created.

The central insight is that the selection process from pool to box can be exploited to extract exogenous information on the jury pool biases. Specifically, strike exhaustion act as signals of how litigants

[42] Another potential concern might be that the variance of juror predispositions increases as the quality of a defendant's case worsens. Put differently, jurors on the whole might become riskier when defendants have a poor quality case. If so, we might expect defendants to use more strikes when they have worse cases, since they are facing a more extreme jury pool. If this theory were true, we should expect to see a steady increase in conviction rates as defendants use additional strikes, rather than a large jump at the strike exhaustion boundary. But as discussed in the next section, this is not what we see when we conduct placebo tests.

[43] One might still expect a jury pool to be more favorable to the striking party in an $n-2$ strike case than in an $n-1$ strike case because exercising the penultimate strike is still costly, as it removes the possibility of striking two prospective jurors later on in voir dire. Nonetheless, the cost of the penultimate strike should be less than the cost of the last strike if the only difference on average between different strike groups is differences in jury composition.

view the jury pool they have been randomly assigned. All else being equal, we expect a litigant who can accurately assess whether a juror favors her side would use more strikes when she has been assigned an unfavorable pool than when she has been assigned a favorable one. Thus, litigants' use of strikes enables us to identify juror predisposition, which is otherwise unmeasurable. The identifying assumption in the most conservative specification is that cases in which litigants use all of their peremptory strikes (n strike group) are on average identical to cases in which litigants use one less strike than the limit ($n-1$ strike group), except for differences in the jury pool they have been assigned.

Using recent data from a large, racially diverse county in Florida, we find our n and $n-1$ strike groups appear largely similar on a broad range of observable pre-trial characteristics, including defendant demographics and past imprisonment history, attorney experience and education, charged offenses, and type of prosecutorial evidence. We find defendants who use all n of their peremptory strikes are significantly more likely to be convicted than defendants who use one less strike than the limit. This result is driven by Black defendants, for whom strike exhaustion increases conviction rates by 18–20 percentage points. We explain why these results are likely a lower bound on the effect of jury composition on case outcomes. We also run multiple robustness checks with a battery of covariates and across various subsamples; our coefficient of interest remains remarkably stable and significant across nearly every specification.

A primary contribution of this paper is a new framework for identification, which enables to show that jury composition greatly affects case outcomes, particularly for Black defendants. In addition, we believe our paper makes a number of other contributions.

First, our results are the first empirical evidence of the causal impact of peremptory strikes on case outcomes. Scholars have questioned for decades whether peremptory strikes affect how a case turns out, or whether attorneys can correctly identify during the strike process which jurors are likely to be predisposed toward their side.[44] Our research suggests that peremptory strikes (and the limits placed on those strikes) do in fact affect case outcomes, and that attorneys are at least somewhat effective in using those strikes to shape the final jury.[45] Our results also suggest increasing peremptory strike limits for defendants would be one way to decrease the variance in outcomes for similarly-situated Black defendants.[46]

[44]See, e.g., Ford [2010], Zeisel and Diamond [1977].

[45]Even though the vast majority of criminal cases result in plea bargains that occur before jury selection, a long literature discusses how this bargaining process might occur in the shadow of the expected results of the jury trial. Compare Mnookin and Kornhauser [1978], Easterbrook [1983], Offit [2019] with Bibas [2004]. If this is true, the phenomenon we identify here might have a significant impact on pretrial negotiations, at least in cases the parties were considering taking to trial. For example, if Black defendants are risk-averse and they face more risk in the jury lottery than non-Black defendants, they might be more likely to settle cases before trial and on worse terms (because of weaker bargaining power) than they would in the absence of this heightened risk.

[46]The federal court system and a number of state courts already give defendants more peremptory strikes than prosecutors. See, e.g., Fed. R. Crim. P. 24(b)(2) (defense and prosecution get 10 and 6 strikes, respectively, in non-capital felonies); Maryland Rule 4-313(a)(2) (defense and prosecution get 20 and 10 strikes, respectively, for life-eligible crimes); Minn. Crim. P. R. 26.02(9) (defense and prosecution get 15 and 9 strikes, respectively, for life-eligible crimes); New Mexico Crim. P. R. 5-606(D)(b) (defense and prosecution get 12 and 8 strikes, respectively, for non-capital, life-eligible crimes).

To be sure, our paper cannot settle larger debates among scholars whether peremptory challenges are socially beneficial or harmful,[47] or whether they make it more likely that guilty people are convicted and innocent people are acquitted in jury trials. Answering the latter question in particular would require, among other things, knowing whether defendants actually committed the underlying crimes for which they are charged, which we cannot determine. Regardless, the wide variation in outcomes we measure, driven by pure chance and concentrated among Black defendants, raises concerns. This problem is magnified given that a large percentage of defendants in our data exhausted their strikes and thus might be affected by the phenomenon we identify here.

Relatedly, the approach we describe here might be a useful diagnostic tool to determine whether jury selection rules in a particular jurisdiction are problematic. Put differently, if parties are awarded a sufficient number of peremptory strikes, either the proportion of cases with n and $n-1$ strikes should be small, or we should not find any statistically significant difference in conviction rates between those groups. The presence of large differences might be evidence that random variation in jury composition is having an outsized effect on criminal case outcomes in that jurisdiction.

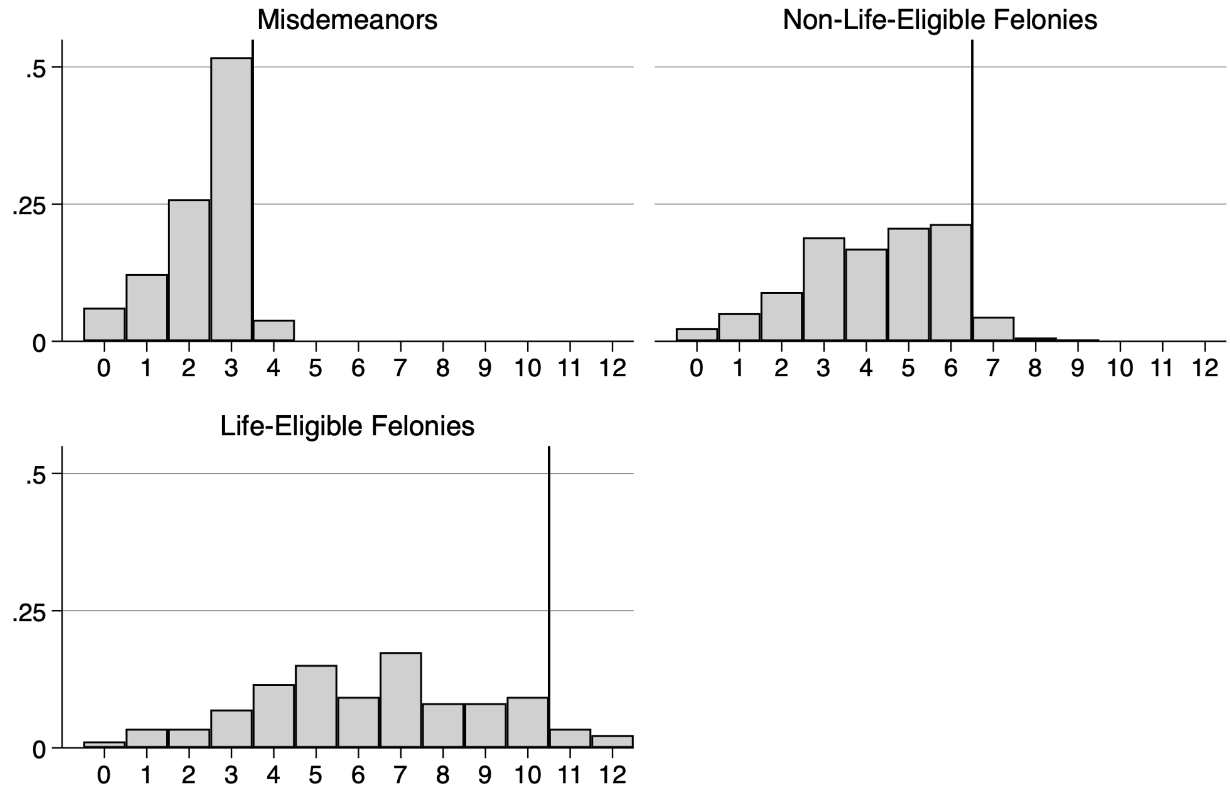
Finally, our paper raises deeper policy questions about how jury selection should be conducted so that the variance (across cases) of predispositions in the final jury is small while preserving the random assignment of jury pools. Several factors that affect jury composition need to be further isolated and examined. For example, should attorneys know who the next potential juror is when they decide whether to use a strike (as is the case in Florida but not in some other states)? Should attorneys be allowed to view the jury pool when making strike decisions, so as to see the age, race, and sex of potential jurors, even though the latter two are protected classes on which attorneys are not permitted to discriminate? How should the number of strikes be chosen and how should it relate to the final jury size? Should the usage of Batson challenges be extended to take into account the juror *replacing* the one being struck? Our paper motivates why answers to these questions could be important for the design of a fair voir dire process. As such, we hope our work sets the stage for further theoretical and empirical research on jury selection.

We have explored and identified an important determinant of justice. Fairness is multidimensional. Not only should the judicial system be designed for impartiality *on average* (across cases), a fair system should have low variance so that defendants can be confident their case is likely to receive a similar outcome to cases that are similar. By exposing this dimension of the justice system, we hope to spur future research in law and in mechanism design to achieve an optimized level of justice.

[47] See, e.g., Flanagan [2015], Babcock [1974], Howard [2010].

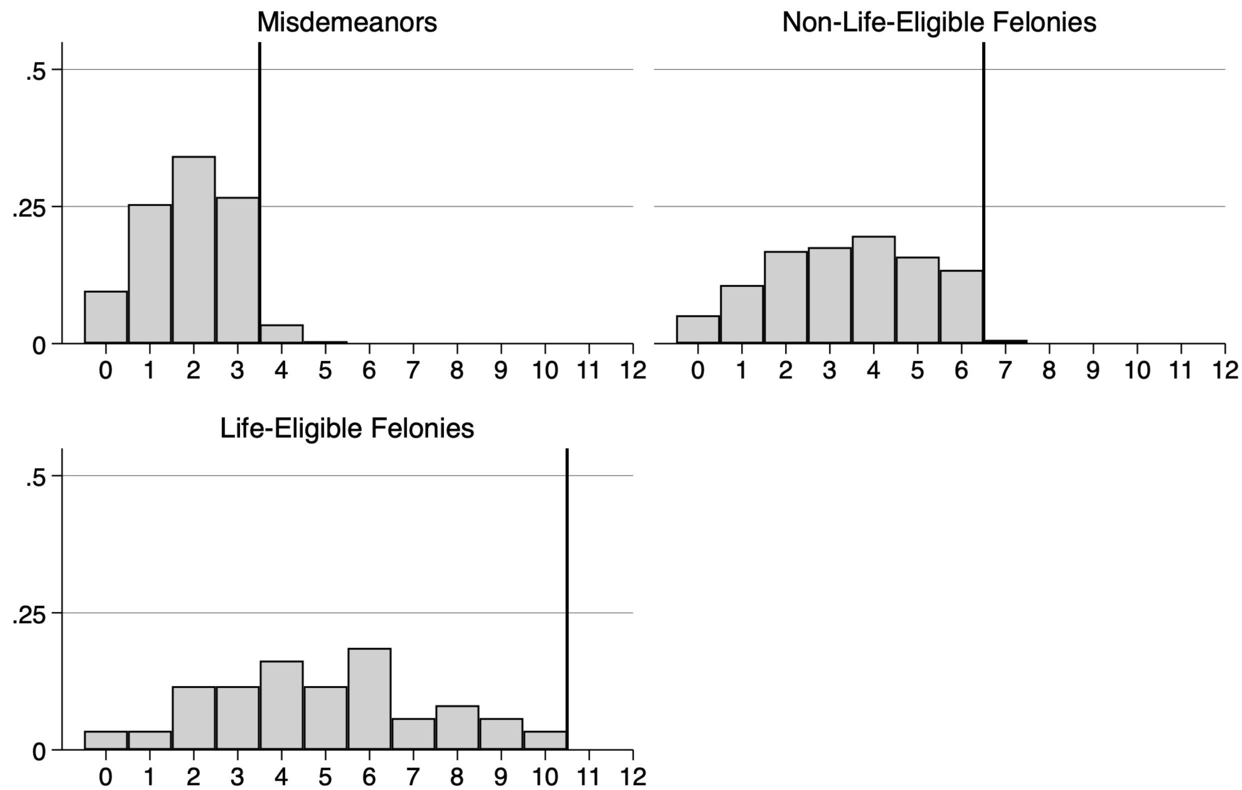
Figures

Figure 1: Peremptory Strikes: Defense



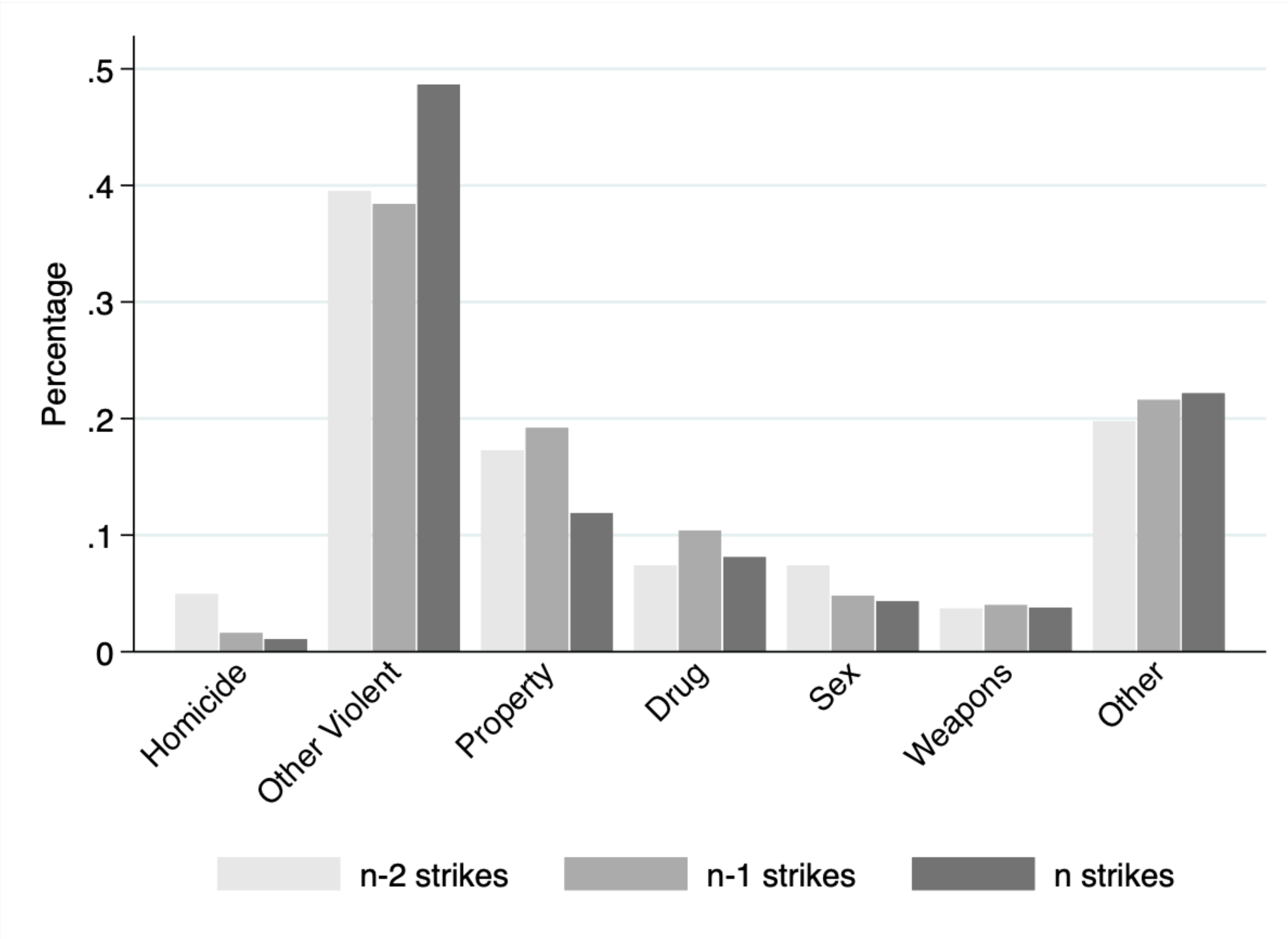
Notes: This figure shows histograms for the number of peremptory strikes used by the defense in the 604 trials from January 2015 through September 2017 in our full dataset (includes cases in which parties exceeded their peremptory strike limits). The peremptory strike limits for the three offense classes of misdemeanor, non-life-eligible felony, and life-eligible felony are 3, 6, and 10, respectively, as shown by the vertical lines.

Figure 2: Peremptory Strikes: Prosecution



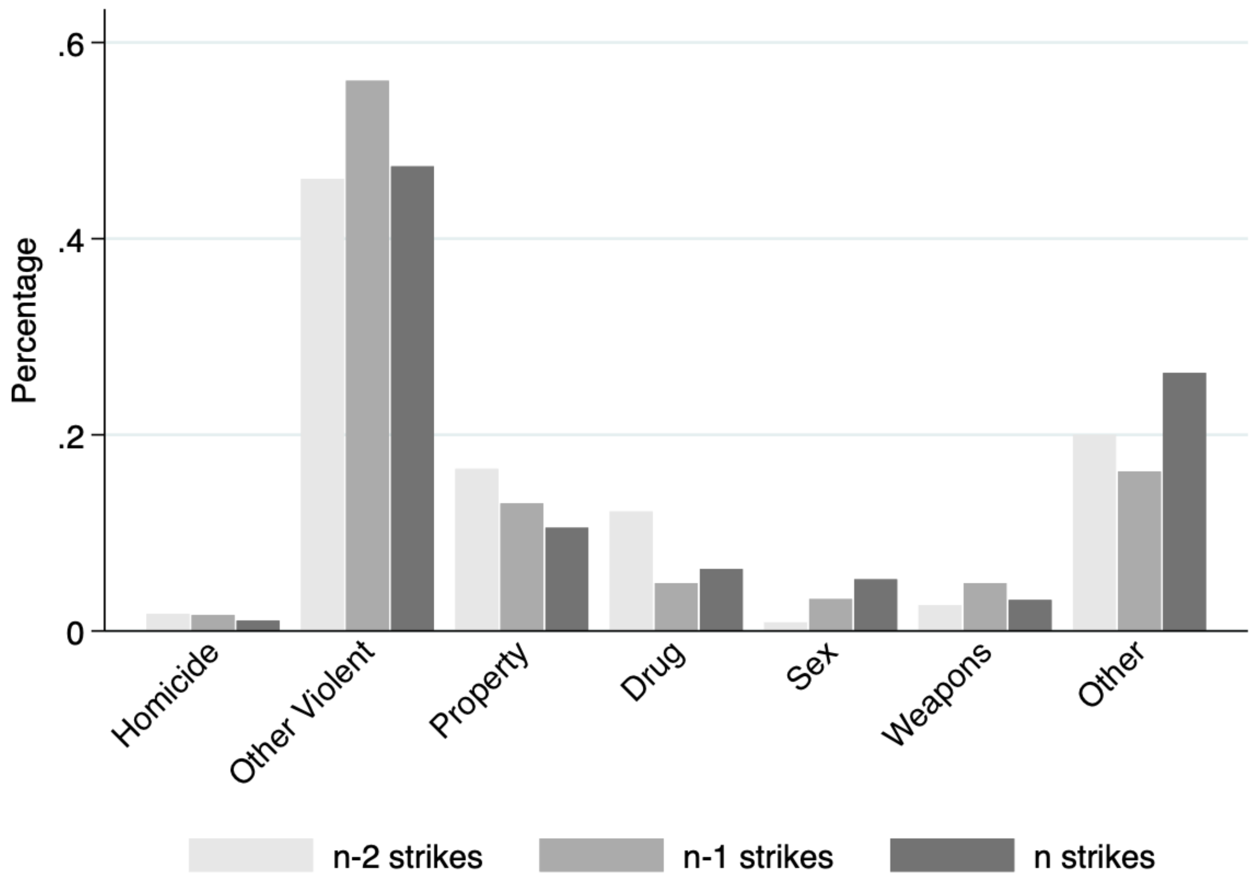
Notes: This figure shows histograms for the number of peremptory strikes used by the prosecution in the 604 trials from January 2015 through September 2017 in our full dataset (includes cases in which parties exceeded their peremptory strike limits). The peremptory strike limits for the three offense classes of misdemeanor, non-life-eligible felony, and life-eligible felony are 3, 6, and 10, respectively, as shown by the vertical lines.

Figure 3: Proportion of Crimes Across Peremptory Strike Classes: Defense



Notes: This figure shows the percentage of cases in each of seven different offense categories (as used in Anwar et al. 2012), across three different peremptory strike groups for defendants. The n, $n-1$ and $n-2$ strike groups comprise defendants who used all of their peremptory strikes, one less strike than the limit, and two less strikes than the limit, respectively.

Figure 4: Proportion of Crimes Across Peremptory Strike Classes: Prosecution



Notes: This figure shows the percentage of cases in each of six different offense categories (as used in Anwar et al. 2012), across three different peremptory strike groups for the prosecution. The n, $n-1$ and $n-2$ strike groups comprise prosecutors who used all of their peremptory strikes, one less strike than the limit, and two less strikes than the limit, respectively.

Tables

Table 1: Hypothetical Jury Selection Strike Sheet

		Seat #									
		1	2	3	4	5	6	7	8	9	10
	1	JC	JC	J-1	D-1	D-2	J-2	JC	JC	P-1	J-3
Row #	2	DC	J-4	JC	D-3	PC	D-4	P-2	D-5	P-3	JC
	3	J-5	D-6	P-4	DC	J-6	NU	PC	NU	JC	NU

Notes: This table shows a hypothetical jury selection strike sheet for a non-life-eligible felony case. The pool comprises 30 jurors, ordered from row 1, seat 1, to row 3, seat 10. JC: pool members struck for cause by the judge; DC and PC: pool members struck for cause by the defense and prosecution, respectively. D-# and P-#: pool members for whom the defense and prosecution, respectively, issued peremptory strikes, with the # of the strike used. J-#: pool members selected for final jury. NU: pool members who were neither struck nor used in the final jury.

Table 2: Defendant, Attorney and Pool Characteristics in n, $n-1$, and $n-2$ Strike Groups by Party

		Defense				Prosecution			
	All	n strikes	$n-1$ strikes	$n-2$ strikes	n v. $n-1$	n strikes	$n-1$ strikes	$n-2$ strikes	n v. $n-1$
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Defendant characteristics									
Age	36.84	37.52	35.39	38.32	2.13	36.43	38.12	37.53	-1.69
	(12.68)	(13.27)	(11.76)	(12.52)	(1.47)	(11.69)	(13.77)	(12.58)	(-0.97)
Black	0.55	0.52	0.52	0.49	-0.00	0.55	0.47	0.53	0.08
	(0.50)	(0.50)	(0.50)	(0.50)	(-0.02)	(0.50)	(0.50)	(0.50)	(1.11)
Hispanic	0.13	0.14	0.14	0.14	0.00	0.17	0.13	0.12	0.04
	(0.34)	(0.35)	(0.34)	(0.34)	(0.11)	(0.38)	(0.34)	(0.33)	(0.78)
Female	0.14	0.14	0.14	0.17	-0.00	0.13	0.10	0.17	0.03
	(0.34)	(0.35)	(0.35)	(0.38)	(-0.09)	(0.33)	(0.30)	(0.38)	(0.66)
# Prev. Imprisonments	0.55	0.43	0.52	0.74	-0.09	0.55	0.46	0.51	0.08
	(1.32)	(1.05)	(1.31)	(1.97)	(-0.66)	(1.30)	(1.37)	(1.33)	(0.46)
Years Prev. Imprison.	1.26	1.33	0.73	1.20	0.60*	1.37	1.20	1.00	0.18
	(3.51)	(4.12)	(1.90)	(3.46)	(1.72)	(3.88)	(3.78)	(2.47)	(0.34)
Attorney characteristics									
Defense Experience	9.71	7.82	9.66	10.31	-1.84	7.65	8.17	8.82	-0.52
	(10.32)	(10.02)	(11.01)	(10.50)	(-1.48)	(10.18)	(10.73)	(10.26)	(-0.36)
Prosecutor Experience	5.51	3.63	5.18	5.88	-1.55**	3.71	4.27	4.97	-0.56
	(5.66)	(4.33)	(6.38)	(4.82)	(-2.33)	(4.32)	(4.72)	(6.18)	(-0.90)
Def. Law Sch. Rank	87.86	80.99	95.02	93.27	-14.03**	92.28	80.83	89.83	11.46
	(54.84)	(53.70)	(57.15)	(56.66)	(-2.14)	(53.80)	(53.43)	(54.66)	(1.54)
Pr. Law Sch. Rank	100.33	96.90	96.48	97.62	0.42	95.55	101.37	94.76	-5.82
	(56.05)	(54.87)	(55.87)	(57.03)	(0.06)	(57.66)	(55.69)	(56.26)	(-0.74)
Public Defender	0.75	0.77	0.72	0.73	0.05	0.81	0.77	0.76	0.04
	(0.44)	(0.42)	(0.45)	(0.45)	(0.93)	(0.39)	(0.42)	(0.43)	(0.69)
Pool characteristics									
Prop. Black in Pool	0.13	0.13	0.13	0.14	-0.00	0.12	0.13	0.13	-0.01
	(0.06)	(0.06)	(0.07)	(0.06)	(-0.19)	(0.08)	(0.06)	(0.07)	(-1.23)
Prop. Female in Pool	0.54	0.53	0.53	0.54	0.00	0.53	0.53	0.55	0.00
	(0.09)	(0.09)	(0.10)	(0.09)	(0.02)	(0.09)	(0.09)	(0.10)	(0.23)
Avg. Age in Pool	46.91	47.12	46.70	46.53	0.42	47.05	46.79	46.82	0.26
	(3.19)	(3.31)	(3.60)	(2.69)	(1.03)	(3.24)	(3.19)	(3.64)	(0.59)
Median Pool Income	62,404	62,596	62,052	62,366	544	63,205	62,264	61,639	941
	(4,976)	(5,436)	(5,017)	(4,891)	(0.91)	(4,938)	(5,352)	(5,336)	(1.34)
Prop. Democrat in Pool	0.29	0.30	0.30	0.29	-0.00	0.29	0.30	0.30	-0.01
	(0.08)	(0.08)	(0.08)	(0.08)	(-0.34)	(0.07)	(0.09)	(0.09)	(-0.70)
Prop. Republican in Pool	0.21	0.21	0.21	0.21	0.00	0.23	0.21	0.20	0.02*
	(0.08)	(0.08)	(0.08)	(0.07)	(0.19)	(0.08)	(0.09)	(0.08)	(1.70)
Observations	567	185	125	81	–	95	123	115	–

Notes: This table compares baseline defendant, attorney, and jury pool characteristics across different peremptory strike groups. Column (1) shows mean values across all cases in our primary dataset. The n, $n-1$, and $n-2$ strike groups comprise parties who exhausted all of their peremptory strikes, one less strike than the limit, and two less strikes than the limit, respectively, for defendants (columns (2)–(4)) and prosecutors (columns (6)–(8)). Attorney experience is number of years between attorney admittance to the Florida state bar and date of jury selection. Law school rankings are calculated based on 2018 U.S. News & World Report rankings. Black, Hispanic, Female and Public Defender are dummy variables for these respective characteristics. Number of previous imprisonments is the number of previous stints a defendant had in Florida state prison; years of previous imprisonment is the total prison term for those stints. Proportion Black and proportion female are the proportion of Black and female jurors, respectively, in the pre-strike jury pool. Average age is the average age of pool members. Median pool income is calculated by estimating each juror's income by the median income in the zip code in which she resides (using U.S. Census data and 2017 inflation-adjusted dollars). Values in parentheses are standard deviations for columns (1)–(4) and (6)–(8), and t-values for two-sided t-tests in columns (5) and (9). ** = significant at 5% level, * = significant at 10% level.

Table 3: Case Characteristics in n, $n-1$, and $n-2$ Strike Groups by Party

		Defense				Prosecution			
	All	n strikes	$n-1$ strikes	$n-2$ strikes	n v. $n-1$	n strikes	$n-1$ strikes	$n-2$ strikes	n v. $n-1$
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Homicide	0.03	0.01	0.02	0.05	-0.01	0.01	0.02	0.02	-0.01
	(0.18)	(0.10)	(0.13)	(0.22)	(-0.38)	(0.10)	(0.13)	(0.13)	(-0.37)
Other Violent Offense	0.40	0.49	0.38	0.40	0.10*	0.47	0.56	0.46	-0.09
	(0.49)	(0.50)	(0.49)	(0.49)	(1.79)	(0.50)	(0.50)	(0.50)	(-1.28)
Property Offense	0.18	0.12	0.19	0.17	-0.07*	0.11	0.13	0.17	-0.02
	(0.38)	(0.32)	(0.40)	(0.38)	(-1.71)	(0.31)	(0.34)	(0.37)	(-0.56)
Drug Offense	0.09	0.08	0.10	0.07	-0.02	0.06	0.05	0.12	0.01
	(0.28)	(0.27)	(0.31)	(0.26)	(-0.67)	(0.24)	(0.22)	(0.33)	(0.45)
Sex Offense	0.07	0.04	0.05	0.07	-0.00	0.05	0.03	0.01	0.02
	(0.26)	(0.20)	(0.21)	(0.26)	(-0.20)	(0.22)	(0.18)	(0.09)	(0.72)
Weapons Offense	0.04	0.04	0.04	0.04	-0.00	0.03	0.05	0.03	-0.02
	(0.21)	(0.19)	(0.20)	(0.19)	(-0.10)	(0.18)	(0.22)	(0.16)	(-0.65)
Other Offense	0.18	0.22	0.22	0.20	0.01	0.26	0.16	0.20	0.10*
	(0.39)	(0.42)	(0.41)	(0.40)	(0.12)	(0.44)	(0.37)	(0.40)	(1.78)
Counts Charged	2.44	2.10	2.43	3.00	-0.33	2.27	2.04	2.51	0.23
	(2.37)	(1.87)	(2.69)	(3.60)	(-1.21)	(2.68)	(1.46)	(2.47)	(0.77)
Correctional Setting	0.02	0.02	0.01	0.02	0.01	0.00	0.03	0.01	-0.03**
	(0.13)	(0.13)	(0.09)	(0.16)	(0.69)	(0.00)	(0.18)	(0.09)	(-2.03)
Photos/Video	0.14	0.13	0.15	0.11	-0.02	0.11	0.13	0.17	-0.02
	(0.35)	(0.33)	(0.36)	(0.32)	(-0.59)	(0.31)	(0.34)	(0.37)	(-0.55)
Recovered Items	0.28	0.23	0.27	0.36	-0.04	0.23	0.20	0.34	0.03
	(0.45)	(0.42)	(0.45)	(0.48)	(-0.76)	(0.42)	(0.40)	(0.48)	(0.48)
Forensic Evidence	0.09	0.03	0.08	0.12	-0.05*	0.05	0.03	0.06	0.02
	(0.29)	(0.18)	(0.27)	(0.33)	(-1.68)	(0.23)	(0.18)	(0.24)	(0.72)
Documentation	0.22	0.15	0.18	0.28	-0.03	0.09	0.16	0.17	-0.07
	(0.41)	(0.36)	(0.38)	(0.45)	(-0.63)	(0.28)	(0.37)	(0.38)	(-1.60)
Confession	0.15	0.17	0.14	0.15	0.02	0.15	0.17	0.10	-0.02
	(0.36)	(0.37)	(0.35)	(0.36)	(0.54)	(0.36)	(0.38)	(0.30)	(-0.45)
Admission	0.40	0.37	0.38	0.49	-0.01	0.39	0.37	0.43	0.02
	(0.49)	(0.48)	(0.49)	(0.50)	(-0.20)	(0.49)	(0.49)	(0.50)	(0.23)
Observations	567	185	125	81	–	95	123	115	–

Notes: This table compares baseline case characteristics across different peremptory strike groups. Column (1) shows mean values across all cases in our primary dataset. The n, $n-1$, and $n-2$ strike groups comprise parties who exhausted all of their peremptory strikes, one less strike than the limit, and two less strikes than the limit, respectively, for defendants (columns (2)–(4)) and prosecutors (columns (6)–(8)). Correctional Setting is a dummy variable for whether the alleged offense took place in a prison/jail. Photos/Video, Recovered Items, Forensic Evidence, Confession, and Admission are dummy variables that describe the evidence in a case; they are as described in the text. Values in parentheses are standard deviations for columns (1)–(4) and (6)–(8), and t-values for two-sided t-tests in columns (5) and (9). ** = significant at 5% level, * = significant at 10% level.

Table 4: Effect of Strike Exhaustion on Conviction

	Defense			Prosecution		
	Guilty	Guilty	Guilty	Guilty	Guilty	Guilty
	(1)	(2)	(3)	(4)	(5)	(6)
Exhausts Strikes	0.12**	0.11**	0.12**	0.02	0.01	-0.01
	(0.05)	(0.05)	(0.05)	(0.06)	(0.06)	(0.06)
n/n-1 Group	-0.01	0.01	0.00	0.05	0.04	0.01
	(0.05)	(0.05)	(0.05)	(0.05)	(0.05)	(0.05)
Felony	0.30***	0.30***	0.24***	0.29***	0.28***	0.21***
	(0.04)	(0.04)	(0.06)	(0.04)	(0.04)	(0.06)
Life Eligible Crime	0.03	0.03	-0.04	0.03	0.03	-0.06
	(0.05)	(0.05)	(0.06)	(0.05)	(0.05)	(0.07)
Constant	0.29***	0.31***	0.27***	0.31***	0.35***	0.26***
	(0.04)	(0.09)	(0.09)	(0.04)	(0.09)	(0.09)
Observations	567	559	559	567	559	559
Adj. R-squared	0.10	0.10	0.10	0.09	0.09	0.10
Defendant Controls	NO	YES	YES	NO	YES	YES
Case Controls	NO	NO	YES	NO	NO	YES

Notes: This table presents OLS regressions of peremptory strike exhaustion on guilt. The dependent variable in all specifications is the proportion of charges pending as of the date of jury selection on which a defendant is eventually convicted. The primary coefficient of interest is Exhausts Strikes, which = 1 if the party used up its n strikes and = 0 if s/he used fewer ($< n$) strikes. n/n-1 Group = 1 if the party used either n or $n-1$ strikes and = 0 otherwise. Columns (1)–(3) measure defense exhaustion of peremptory strikes; columns (4)–(6) measure prosecution exhaustion of those strikes. Defendant Controls include defendant age and years of previous imprisonment in Florida state prison, and separate dummy variables for whether a defendant is female, Black or Hispanic. Case Controls include number of for cause strikes issued by the judge, prosecution, and defense; total number of counts charged; a dummy for whether the opposing party exhausted its peremptory strikes; and a fixed effect for the year in which jury selection occurred. Heteroskedastic robust standard errors are reported in parentheses. *** = significant at 1% level, ** = significant at 5% level, * = significant at 10% level.

Table 5: Effect of Strike Exhaustion on Conviction for Black and Non-Black Defendants

	Black Defendants			Non-Black Defendants		
	Guilty	Guilty	Guilty	Guilty	Guilty	Guilty
	(1)	(2)	(3)	(4)	(5)	(6)
Exhausts Strikes	0.19***	0.20***	0.18***	0.06	0.04	0.07
	(0.07)	(0.07)	(0.07)	(0.07)	(0.07)	(0.08)
n/n-1 Group	-0.07	-0.06	-0.07	0.07	0.08	0.08
	(0.06)	(0.07)	(0.07)	(0.07)	(0.07)	(0.07)
Felony	0.35***	0.34***	0.31***	0.28***	0.27***	0.16*
	(0.06)	(0.06)	(0.08)	(0.06)	(0.06)	(0.09)
Life Eligible Crime	0.02	0.03	0.01	0.05	0.06	-0.10
	(0.07)	(0.07)	(0.08)	(0.08)	(0.08)	(0.10)
Constant	0.25***	0.15	0.12	0.31***	0.40***	0.36***
	(0.06)	(0.10)	(0.11)	(0.06)	(0.11)	(0.13)
Observations	310	306	306	257	253	253
Adj. R-squared	0.12	0.12	0.11	0.08	0.07	0.09
Defendant Controls	NO	YES	YES	NO	YES	YES
Case Controls	NO	NO	YES	NO	NO	YES

Notes: This table presents OLS regressions of defendant peremptory strike exhaustion on guilt, split by defendant race. The dependent variable in all specifications is the proportion of charges pending as of the date of jury selection on which a defendant is eventually convicted. Columns (1)–(3) measure exhaustion of peremptory strikes by Black defendants; columns (4)–(6) measure exhaustion of strikes by non-Black defendants. The primary coefficient of interest is Exhausts Strikes, which = 1 if the defendant used up its n strikes and = 0 if s/he used fewer ($< n$) strikes. n/n-1 Group = 1 if the defendant used either n or $n-1$ strikes and = 0 otherwise. Defendant Controls are as described in Table 4, except a dummy variable for whether a defendant is Hispanic is only used in cols. (5)–(6). Case Controls are as described in Table 4. Heteroskedastic robust standard errors are reported in parentheses. *** = significant at 1% level, ** = significant at 5% level, * = significant at 10% level.

Table 6: Effect of Defendant Strike Exhaustion With Jury Composition Controls

	All Defendants			Black Defendants		
	Guilty	Guilty	Guilty	Guilty	Guilty	Guilty
	(1)	(2)	(3)	(4)	(5)	(6)
Exhausts Strikes	0.12**	0.11**	0.11**	0.20***	0.20***	0.19***
	(0.05)	(0.05)	(0.05)	(0.07)	(0.07)	(0.07)
n/n-1 Group	0.01	0.02	0.02	-0.07	-0.06	-0.06
	(0.05)	(0.05)	(0.05)	(0.06)	(0.06)	(0.07)
Felony	0.30***	0.29***	0.22***	0.34***	0.33***	0.30***
	(0.04)	(0.04)	(0.06)	(0.06)	(0.06)	(0.08)
Life Eligible Crime	0.03	0.04	-0.04	0.02	0.03	0.01
	(0.05)	(0.05)	(0.06)	(0.07)	(0.07)	(0.08)
Prop. Black in Pool	0.46	0.48	0.49	0.71	0.77*	0.82*
	(0.32)	(0.32)	(0.32)	(0.45)	(0.45)	(0.45)
Avg. Age in Pool	0.00	0.00	0.00	-0.00	-0.00	-0.00
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
Prop. Female in Pool	0.09	0.14	0.14	0.26	0.31	0.30
	(0.20)	(0.20)	(0.20)	(0.28)	(0.28)	(0.29)
Ln Median Pool Income	0.35	0.34	0.37*	0.51*	0.54*	0.55*
	(0.22)	(0.22)	(0.23)	(0.30)	(0.30)	(0.31)
Prop. Democrat in Pool	-0.29	-0.30	-0.30	-0.10	-0.03	-0.04
	(0.25)	(0.25)	(0.25)	(0.32)	(0.33)	(0.35)
Prop. Republican in Pool	0.40	0.38	0.37	0.62*	0.67**	0.65*
	(0.25)	(0.25)	(0.26)	(0.33)	(0.33)	(0.35)
Constant	-3.78	-3.78	-4.17*	-5.58*	-6.21*	-6.27*
	(2.46)	(2.49)	(2.53)	(3.27)	(3.33)	(3.44)
Observations	567	559	559	310	306	306
Adj. R-squared	0.10	0.11	0.11	0.13	0.14	0.13
Defendant Controls	NO	YES	YES	NO	YES	YES
Case Controls	NO	NO	YES	NO	NO	YES

Notes: This table presents OLS regressions of defendant peremptory strike exhaustion on guilt, with controls for jury pool composition. The dependent variable in all specifications is the proportion of charges pending as of the date of jury selection on which a defendant is eventually convicted. Columns (1)–(3) include all defendants; columns (4)–(6) are limited to Black defendants. Exhausts Strikes and n/n-1 Group are as described in Table 5. Defendant Controls are as described in Table 4, except dummy variables for whether a defendant is Black or Hispanic are only used in cols. (2)–(3). Case Controls are as described in Table 4. The various jury composition controls are as described in the text. Heteroskedastic robust standard errors are reported in parentheses. *** = significant at 1% level, ** = significant at 5% level, * = significant at 10% level.

Table 7: Effect of Defendant Strike Exhaustion on Conviction: Attorney Controls

	All Defendants				Black Defendants			
	Guilty	Guilty	Guilty	Guilty	Guilty	Guilty	Guilty	Guilty
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Exhausts Strikes	0.15***	0.17***	0.16***	0.21***	0.19***	0.23***	0.21***	0.25***
	(0.05)	(0.06)	(0.06)	(0.07)	(0.07)	(0.07)	(0.08)	(0.09)
n/n-1 Group	-0.04	-0.06	-0.03	-0.08	-0.10	-0.14*	-0.08	-0.13
	(0.05)	(0.05)	(0.06)	(0.07)	(0.07)	(0.07)	(0.09)	(0.09)
Felony	0.19***	0.21***	0.20**	0.19**	0.27***	0.29***	0.26**	0.27**
	(0.06)	(0.06)	(0.08)	(0.08)	(0.09)	(0.09)	(0.11)	(0.12)
Life Eligible Crime	-0.06	-0.04	0.03	0.03	-0.04	-0.01	0.08	0.10
	(0.06)	(0.06)	(0.08)	(0.08)	(0.08)	(0.08)	(0.09)	(0.09)
Constant	0.30***	0.32**	0.29**	0.34**	0.18	0.17	0.12	0.14
	(0.11)	(0.13)	(0.12)	(0.14)	(0.15)	(0.16)	(0.15)	(0.17)
Observations	527	527	393	393	293	293	227	227
Adj. R-squared	0.14	0.13	0.13	0.15	0.15	0.16	0.13	0.14
Defendant & Case Ctrls	YES	YES	YES	YES	YES	YES	YES	YES
Attorney Controls	YES	YES	YES	YES	YES	YES	YES	YES
Both Sides	NO	YES	NO	YES	NO	YES	NO	YES
PD Only	NO	NO	YES	YES	NO	NO	YES	YES

Notes: This table presents OLS regressions of defendant peremptory strike exhaustion on guilt, with controls for attorney characteristics. The dependent variable in all specifications is the proportion of charges pending as of the date of jury selection on which a defendant is eventually convicted. Columns (1)-(4) include all defendants; columns (5)-(8) are limited to Black defendants. Exhausts Strikes and n/n-1 Group are as described in Table 5. Defendant & Case Controls are as described in Table 4 (except dummy variables for whether a defendant is Black or Hispanic are only used in cols. (1)–(4)). Attorney Controls include separate controls for both prosecutor and defense attorney years of experience and ranking of law school attended. Both Sides controls for the number of other cases in our sample in which a prosecutor or defense attorney used up her peremptory strikes, and the number of other cases in our sample in which she used one less strike than the limit. PD Only limits the sample only to cases involving public defenders; the other columns include a dummy variable for whether the defense attorney was a public defender. Heteroskedastic robust standard errors are reported in parentheses. *** = significant at 1% level, ** = significant at 5% level, * = significant at 10% level.

Table 8: Effect of Defendant Strike Exhaustion on Conviction: Charge/Judge Fixed Effects

	All Defendants			Black Defendants		
	Guilty	Guilty	Guilty	Guilty	Guilty	Guilty
	(1)	(2)	(3)	(4)	(5)	(6)
Exhausts Strikes	0.12**	0.12**	0.16***	0.18**	0.19**	0.21***
	(0.05)	(0.06)	(0.06)	(0.07)	(0.08)	(0.08)
n/n-1 Group	-0.00	0.02	-0.04	-0.08	-0.05	-0.12
	(0.05)	(0.05)	(0.05)	(0.07)	(0.08)	(0.08)
Felony	0.17***	0.13	-0.01	0.22**	0.35	0.12
	(0.06)	(0.28)	(0.27)	(0.09)	(0.32)	(0.32)
Life Eligible Crime	-0.03	-0.04	-0.07	0.08	0.02	0.03
	(0.07)	(0.07)	(0.07)	(0.09)	(0.09)	(0.09)
Constant	0.44***	0.20	0.37*	0.13	0.09	0.08
	(0.14)	(0.15)	(0.21)	(0.20)	(0.21)	(0.30)
Observations	552	552	520	303	303	290
Adj. R-squared	0.11	0.10	0.15	0.13	0.09	0.14
Defendant & Case Ctrls	YES	YES	YES	YES	YES	YES
Charge Fixed Effect	YES	NO	YES	YES	NO	YES
Judge Fixed Effect	NO	YES	YES	NO	YES	YES
Attorney Controls	NO	NO	YES	NO	NO	YES

Notes: This table presents OLS regressions of defendant exhaustion of peremptory strikes on guilt, with the inclusion of various fixed effects. The dependent variable in all specifications is the proportion of charges pending as of the date of jury selection on which a defendant is eventually convicted. Columns (1)–(3) include all defendants; columns (4)–(6) are limited to Black defendants. Exhausts Strikes and n/n-1 Group are as described in Table 5. Defendant & Case Controls are as described in Table 4, except dummy variables for whether a defendant is Black or Hispanic are only used in cols. (1)–(3). Each specification also includes dummy variables for whether the alleged crime took place in a Correctional Setting, or whether the criminal affidavit described the following evidence against the defendant: Photos/Videos, Recovered Items, Forensic Evidence, Documentation, Admission, and Confession (all described in the text). Charge Fixed Effect controls for offense category (as defined in Anwar et al. 2012) of the first charged offense remaining as of the date of jury selection. Judge Fixed Effect controls for the judge assigned to the case. Attorney Controls include separate controls for both prosecutor and defense attorney years of experience and ranking of law school attended, as well as a dummy variable if the defense attorney was a public defender. Heteroskedastic robust standard errors are reported in parentheses. *** = significant at 1% level, ** = significant at 5% level, * = significant at 10% level.

Table 9: Effect of Defendant Strike Exhaustion on Conviction: Different Strike Definitions

	All Defendants				Black Defendants		
	Guilty (1)	Guilty (2)	Guilty (3)	Guilty (4)	Guilty (5)	Guilty (6)	Guilty (7)
Exhausts Strikes	0.12***	0.23**	0.21**	0.12**	0.15***	0.20	0.26*
	(0.04)	(0.10)	(0.09)	(0.05)	(0.06)	(0.14)	(0.15)
n/n-1 Group	–	-0.25*	-0.23**	-0.02	–	-0.29	-0.38**
		(0.14)	(0.11)	(0.06)		(0.21)	(0.16)
Felony	0.24***	0.27*	0.15	0.25***	0.33***	0.08	0.30
	(0.06)	(0.14)	(0.12)	(0.06)	(0.08)	(0.19)	(0.18)
Life Eligible Crime	-0.04	0.29**	-0.38	-0.04	0.03	–	-0.44*
	(0.06)	(0.14)	(0.24)	(0.07)	(0.08)		(0.23)
Constant	0.34***	0.05	0.57**	0.14	0.10	0.10	0.44
	(0.10)	(0.21)	(0.24)	(0.14)	(0.11)	(0.26)	(0.41)
Observations	559	92	121	484	306	51	56
Adj. R-squared	0.10	0.21	0.17	0.12	0.12	-0.02	0.26
Defendant & Case Ctrls	YES	YES	YES	YES	YES	YES	YES
Only Black & White Defs.	NO	NO	NO	YES	NO	NO	NO
# Prosecution Strikes	–	n	$n-1$	–	–	n	$n-1$

Notes: This table presents alternate specifications for OLS regressions of defendant peremptory strike exhaustion on guilt. The dependent variable in all specifications is the proportion of charges pending as of the date of jury selection on which a defendant is eventually convicted. Def. Exhausts Strikes is as described before in Table 5, except in columns (1) and (5) this variable = 0 if the defendant used fewer strikes than the limit (n strikes); in all other columns it = 0 if the defendant used just one less strike than the limit ($n-1$ strikes). n/n-1 Group is as described in Table 5. Defendant & Case Controls are as described in Table 4 (except dummy variables for whether a defendant is Black or Hispanic are only used in cols. (1)–(4) and cols. (1)–(3), respectively). Columns (2), (6), and (3), (7) limit the sample to cases in which the prosecution used n or $n-1$ of its strikes, respectively. Column (4) limits the sample by race to just Black and white defendants. Heteroskedastic robust standard errors are reported in parentheses. *** = significant at 1% level, ** = significant at 5% level, * = significant at 10% level.

Table 10: Effect of Defendant Strike Exhaustion on Conviction: Verdict-Only Outcomes

	All Defendants					Black Defendants				
	Guilty (1)	Guilty (2)	Guilty (3)	Guilty (4)	Guilty (5)	Guilty (6)	Guilty (7)	Guilty (8)	Guilty (9)	Guilty (10)
Exhausts Strikes	0.11*	0.07	0.11*	0.11*	0.13**	0.17**	0.12	0.14*	0.20**	0.20***
	(0.06)	(0.06)	(0.06)	(0.06)	(0.06)	(0.07)	(0.08)	(0.08)	(0.08)	(0.08)
n/n-1 Group	-0.01	0.03	0.00	-0.00	-0.05	-0.07	-0.03	-0.05	-0.09	-0.12
	(0.06)	(0.06)	(0.06)	(0.07)	(0.06)	(0.08)	(0.08)	(0.08)	(0.09)	(0.08)
Felony	0.23***	0.25***	0.23***	0.25***	0.25***	0.34***	0.33***	0.34***	0.35***	0.32***
	(0.07)	(0.07)	(0.07)	(0.07)	(0.07)	(0.09)	(0.09)	(0.09)	(0.10)	(0.09)
Life Eligible Crime	0.02	0.04	0.00	0.03	0.06	0.09	0.06	0.05	0.08	0.12
	(0.07)	(0.07)	(0.08)	(0.09)	(0.08)	(0.10)	(0.10)	(0.10)	(0.11)	(0.10)
Constant	0.27**	0.30***	0.24**	0.25**	0.28**	0.12	0.13	0.11	0.08	0.13
	(0.11)	(0.11)	(0.11)	(0.12)	(0.11)	(0.13)	(0.13)	(0.13)	(0.13)	(0.13)
Observations	491	491	491	434	491	274	274	274	245	274
Adj. R-squared	0.09	0.11	0.10	0.11	0.12	0.11	0.12	0.10	0.12	0.14
Defendant & Case Ctrls	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES
Proportion Guilty	YES	NO	NO	NO	NO	YES	NO	NO	NO	NO
Any Guilty Verdict	NO	YES	NO	NO	NO	NO	YES	NO	NO	NO
Min Count Verdict	NO	NO	YES	NO	NO	NO	NO	YES	NO	NO
No Mixed Verdicts	NO	NO	NO	YES	NO	NO	NO	NO	YES	NO
Guilty if Mixed + Jail	NO	NO	NO	NO	YES	NO	NO	NO	NO	YES

Notes: This table presents alternate specifications for OLS regressions of defendant peremptory strike exhaustion on guilt. All columns are limited to cases in which a jury issued at least one guilty or not guilty verdict; columns (1)-(5) are for all defendants and columns (6)-(10) are limited to Black defendants. Defendant & Case Controls are as described in Table 4, except dummy variables for whether a defendant is Black or Hispanic are only used in cols. (1)–(5). In columns (1) and (6), the dependent variable is the proportion of charges tried to the jury on which the defendant is found guilty by the jury. In columns (2) and (7), Def. Exhausts Strikes = 1 if the jury issued a guilty verdict on at least count, regardless of what it does on the other counts. In columns (3) and (8), a case is categorized as either guilty or not guilty depending on the verdict the jury issued on the lowest count tried to the jury. In columns (4) and (9), cases with a mixed guilty/not guilty verdict are excluded. In columns (5) and (10), cases with a mixed verdict are categorized as guilty if the defendant received a jail sentence (net of credit for time served); otherwise they are categorized as not guilty. Heteroskedastic robust standard errors are reported in parentheses. *** = significant at 1% level, ** = significant at 5% level, * = significant at 10% level.

Table 11: Effect of Placebo Defendant Strike Exhaustion

	All Defendants			Black Defendants		
	Guilty	Guilty	Guilty	Guilty	Guilty	Guilty
	(1)	(2)	(3)	(4)	(5)	(6)
Placebo Exhausts	-0.07	-0.05	-0.05	-0.09	-0.07	-0.09
	(0.06)	(0.06)	(0.06)	(0.08)	(0.08)	(0.08)
n-1/n-2 Group	0.01	-0.01	-0.00	-0.03	-0.05	-0.04
	(0.05)	(0.05)	(0.05)	(0.07)	(0.07)	(0.08)
Felony	0.26***	0.26***	0.19***	0.30***	0.29***	0.25***
	(0.04)	(0.04)	(0.06)	(0.05)	(0.06)	(0.08)
Life Eligible Crime	0.01	0.01	-0.07	-0.01	-0.00	-0.03
	(0.05)	(0.05)	(0.06)	(0.06)	(0.07)	(0.08)
Constant	0.37***	0.40***	0.34***	0.34***	0.27***	0.19*
	(0.03)	(0.08)	(0.09)	(0.05)	(0.10)	(0.11)
Observations	567	559	559	310	306	306
Adj. R-squared	0.09	0.09	0.09	0.11	0.11	0.11
Other Case Controls	NO	NO	YES	NO	NO	YES

Notes: This table presents OLS regressions of a placebo test of defendant peremptory strike exhaustion on guilt. The dependent variable in all specifications is the proportion of charges pending as of the date of jury selection on which a defendant is eventually convicted. The primary coefficient of interest is Placebo Exhausts, which = 1 if the defendant used one less strike than the limit ($n-1$ strikes), and = 0 otherwise. n-1/n-2 Group = 1 if the defendant used either $n-1$ or $n-2$ strikes and = 0 otherwise. Columns (1)–(3) measure placebo exhaustion of peremptory strikes for all defendants; columns (4)–(6) measure placebo exhaustion of strikes by just Black defendants. Defendant & Case Controls are as described in Table 4, except dummy variables for whether a defendant is Black or Hispanic are only used in cols. (2)–(3). Heteroskedastic robust standard errors are reported in parentheses. *** = significant at 1% level, ** = significant at 5% level, * = significant at 10% level.

Appendix

Model

We now formalize the intuition for our identification strategy and propose an extended framework in which to discuss assumptions. To begin, we parameterize the facts of a case t by a fact index F^t taking values in $[0,1]$, where 0 represents the weakest case conditions for convicting the defendant, and 1 represents the strongest.[1] We define a *juror predisposition function* (JPF) as a function $j\colon\ [0,1] \to [0,1]$ that takes as input the fact index and gives as output the juror's predisposition against a defendant with the corresponding fact index.[2] We define a *jury pool* of size P for trial t to be a sequence $j_1^t, \ldots j_P^t$ of JPFs, and a *seated jury* of size S to be a subsequence $\{j_{a_k}\}_{k=1}^S$ of length S of the jury pool.

When deciding whether to use a peremptory strike, an attorney must compare the seated jury at hand with the replacement candidate (the juror next in line to be considered if a strike is used). An important feature of our Florida data is that an attorney has the same level of information on all potential jurors when he makes his strike decision, including their observable characteristics and responses to questions they are asked during voir dire.[3]

After trial, the S jurors render their decision through a unanimous verdict.[4] We can thus aggregate their individual JPFs into a single verdict function:

$$G\left(j_{a_1}, \ldots, j_{a_S}; F\right)$$

$G(\boldsymbol{.})$ inputs the individual JPFs of the seated jury, as well as the facts of the case, and outputs 1 (guilty) or 0 (not guilty).[5] Instead of this function, for a specific trial t it is sufficient for both parties to consider a simpler function $G^t : \mathbb{R}^S \to \{0,1\}$, defined as:

$$G^t\left(j_{a_1}(F^t), \ldots, j_{a_S}\left(F^t\right)\right).$$

[1] In reality, F^t would be multi-dimensional, capturing information such as the evidence against a defendant, including testimonies, as well as other factors that affect the final verdict, such as the judge. We simplify to a one-dimensional F^t for tractability and because attorneys also likely act on a summary measure when deciding whether to strike potential jurors.

[2] Imagine on one extreme the JPF $\underline{j}\left(F^t\right) = 0$, which represents a juror who would support a "not guilty" verdict no matter the facts; and on the other extreme the JPF $\overline{j}\left(F^t\right) = 1$, which represents a juror who would support a "guilty" verdict no matter the facts. An impartial juror would have an identity JPF: $I\left(F^t\right) = F^t$.

[3] This is not the case in some other jurisdictions. For example, in Middlesex County, New Jersey, attorneys can observe the pool, but the next juror is revealed and questioned only when it is apparent that a new juror is needed for the juror box (e.g., a juror in the box has been excused because of a peremptory strike). In this scenario, an attorney could make his strike decision solely based on the distribution of observable characteristics of the pool.

[4] For simplification, we ignore the possibility of a hung jury in our model. These comprise a small percentage of cases in our dataset.

[5] Sentencing is decided by the judge, not by the jury. The domain of G could be generalized to $\{0,1\}^k$, where k is the number of counts the defendant is charged with. For simplicity, here and in the empirical section, we focus on 0/1 measures of guilty.

We assume G^t is strictly increasing in all directions.

We can now model the simplest scenario: whether an attorney should exercise his last peremptory strike after his opponent has already exhausted all of her strikes. Without loss of generality, suppose $j_{a_1}(F^t) \leq j_{a_2}(F^t) \leq \ldots \leq j_{a_S}(F^t)$, and let r be the index number of the replacement juror, which is non-stochastic because the replacement juror is known. For concreteness, we focus on the scenario in which the defense attorney has only one strike remaining. She will use this strike if:

$$G^t\left(j_{a_1}(F^t), \ldots, j_{a_S}\left(F^t\right)\right) > G\left(j_{a_1}(F^t), \ldots, j_{a_{S-1}}\left(F^t\right), j_r\left(F^t\right)\right).$$

Since G is strictly increasing, an equivalent condition is the following:

$$j_{a_S}\left(F^t\right) > j_r\left(F^t\right)$$

We will provide a sufficient condition for the strike decision to be independent of the facts of the case, F^t, after the following definition:

Definition 1. We say the jury pool $\{j_k\}_{k=1}^P$ is *F-separated* if the ordering of the JPFs $\{j_k\}_{k=1}^P$ is constant on the set F: for all $F^t, F^{t'} \in F$, $F^{t'} > F^t$, $1 \leq l \leq P$, $1 \leq m \leq P$, if $j_l\left(F^t\right) > j_m\left(F^t\right)$, then $j_l\left(F^{t'}\right) > j_m\left(F^t\right)$.[6]

Theorem 1. *If a jury pool is F-separated, then the decision to strike does not depend on $F^t \in F$.*

Proof. Suppose that a jury pool, $\{j_k\}_{k=1}^P$, is F-separated, and has the seated jury $\{j_{a_k}\}_{k=1}^S$ at the time the defense is deciding whether to use the last strike. Fix $F^t, F^{t'} \in F$. Without loss of generality, suppose that $F^t > F^{t'}$ and that $j_{a_1}(F^t) \leq j_{a_2}(F^t) \leq \ldots \leq j_{a_S}(F^t)$. Let r be the index number of the replacement juror. The defense will use the strike if $j_{a_S}\left(F^t\right) > j_r\left(F^t\right)$, which is equivalent to $j_{a_S}\left(F^{t'}\right) > j_r\left(F^{t'}\right)$ because the jury pool is F-separated. Hence, the strike is used (or not used) regardless of the value of the fact index. □

Assumption 1. *The composition of the seated jury at the time of the last strike decision of the defense does not depend on the fact index.*

Assumption (1) allows us to focus on solving the end of the peremptory challenge process.[7] It follows from Theorem 1 and Assumption (1) that the composition of the final seated jury is independent of the fact index. This independence has important implications. First, there should be no systematic differences in F^t (or anything outside of F^t) between n strike cases and $n-1$ strike cases, since the decision whether to strike does not depend on the facts of the case. Second, we can allow for asymmetric information or talent: Even if some attorneys are more skilled or have more information to identify F^t

[6]If F is an interval, the definition is equivalent to the JPFs not intersecting on F.

[7]This assumption could be relaxed by modeling the entire sequence of challenges. However, this simple framework and Theorem 1 capture the main intuition of the setting.

than other attorneys, this variation does not lead to a difference in the final seated jury. For this result to hold, we must assume one of two things. Either all attorneys must correctly identify $\{j_k\}_{k=1}^{P}$—that is, during voir dire they identify the JPF for all jurors for any set of facts, even though they might not know F^t. Alternatively, we can assume attorneys only correctly identify F^t and $\{j_k(F^t)\}_{k=1}^{P}$. That is, instead of assuming correct identification of all JPFs at all points, it is sufficient to assume correct identification of the fact index in that trial and the JPFs evaluated at that specific fact index.

We can now aggregate the above decision process to show what we identify with the difference in averages for the n strike and $n-1$ strike cases. Suppose there are N cases where all n strikes were used, and N_{-1} cases where $n-1$ strikes were used. When we compare the average conviction rates for the subset of n cases versus the average for the subset of $n-1$ cases, we get:

$$\hat{\gamma} = \frac{1}{N}\sum_{i=1}^{N} G\left(J_{b_{j_1}},\ldots,J_{b_{j_k}};F\right) - \frac{1}{N_{-1}}\sum_{i=1}^{N_{-1}} G\left(J_r,\ldots,J_{b_{j_k}};F\right)$$

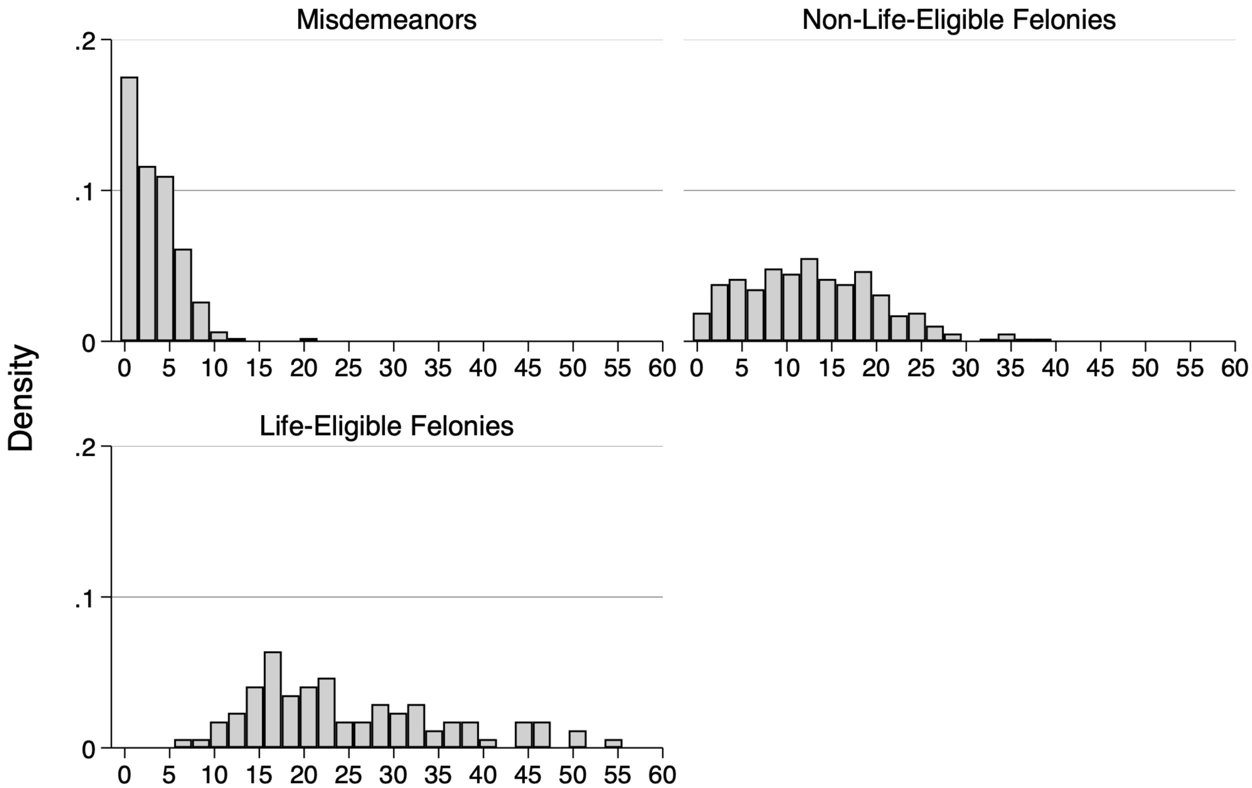
Viewing these cases as an i.i.d. sample, the Law of Large Numbers implies that:

$$\hat{\gamma} \xrightarrow{p} E\left\{G\left(J_{b_{j_1}},\ldots,J_{b_{j_k}};F\right)|J_{b_{j_1}} > J_r\right\} - E\left\{G\left(J_r,\ldots,J_{b_{j_k}};F\right)|J_{b_{j_1}} \leq J_r\right\}$$

Hence, we would expect $\hat{\gamma} > 0$ and for conviction rates to be higher when the defendant uses all of his peremptory strikes as compared to instances in which he uses one less strike than the limit.

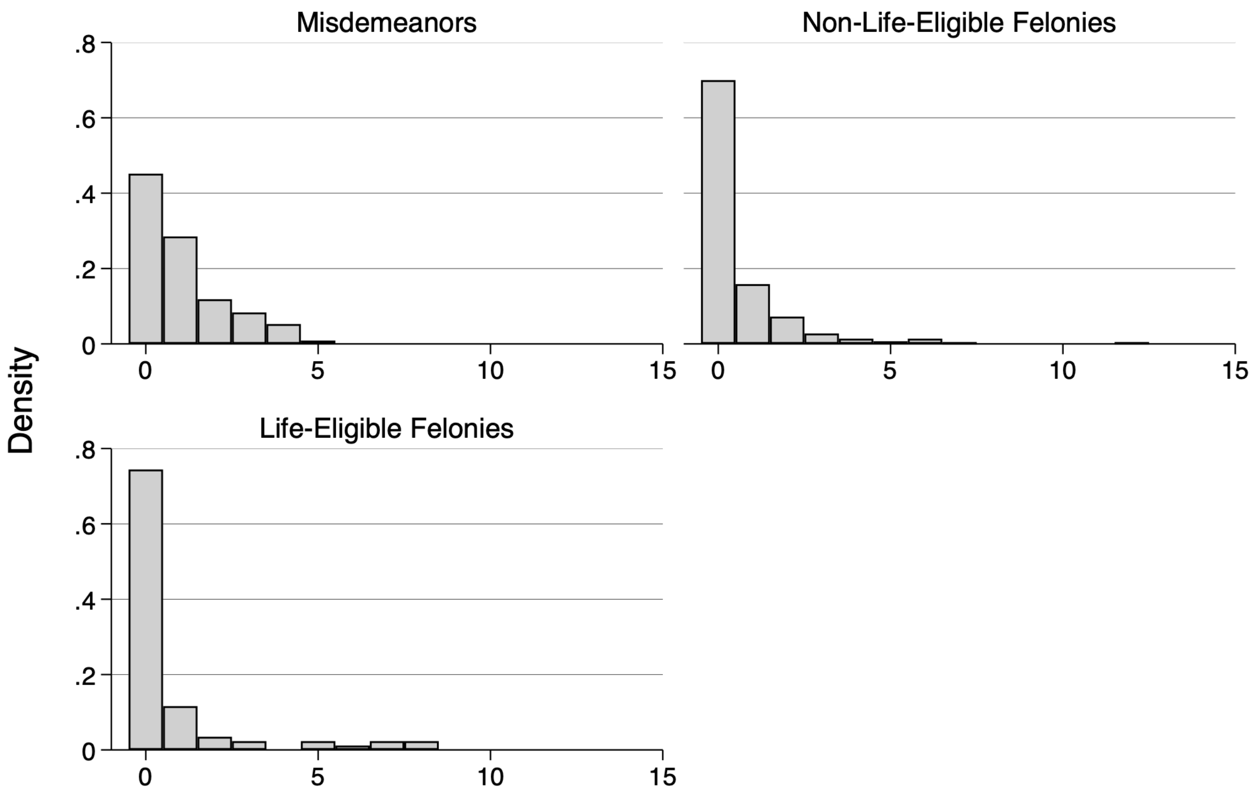
Appendix Figures

Figure A.1: For Cause Strikes: Judge Initiated



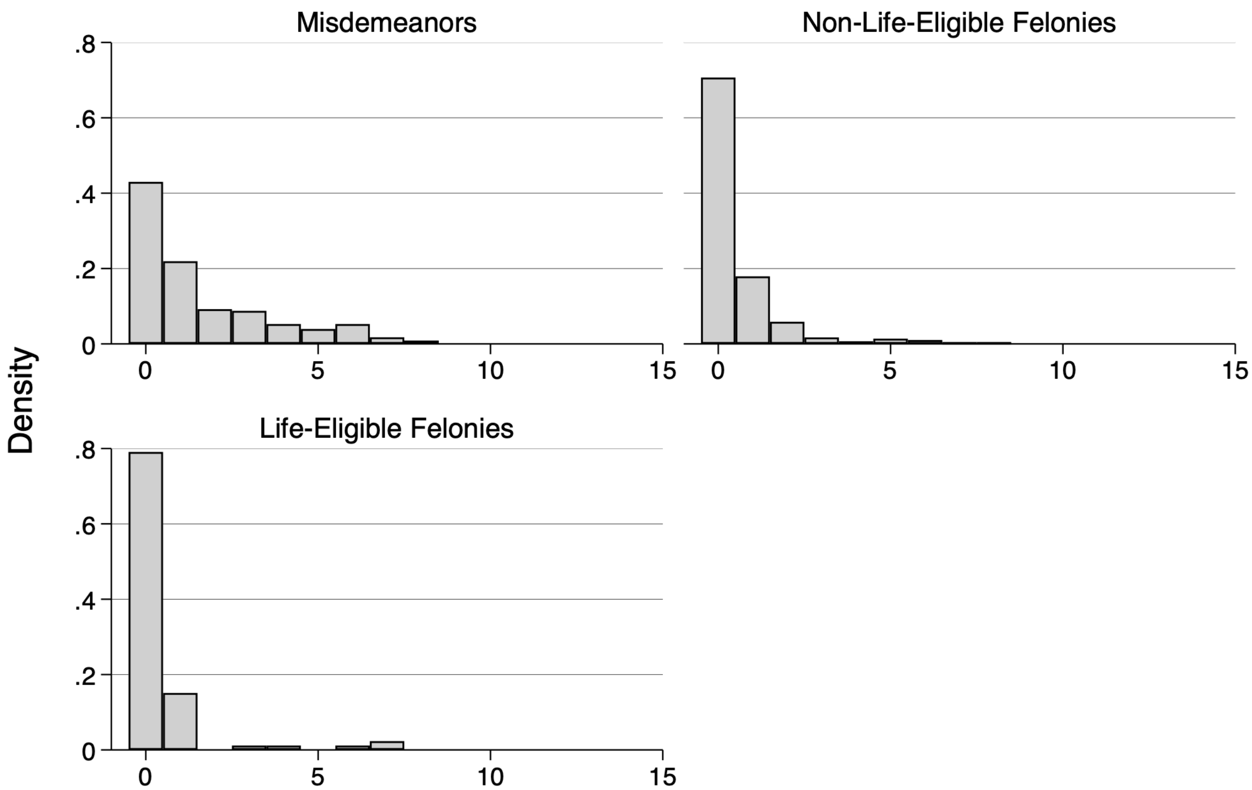
Notes: This figure shows a histogram for the number of for cause strikes initiated by the judge in the 604 trials from 2015 through September 2017 in our full dataset (includes cases in which parties exceeded their peremptory strike limits). Bin size = 2.

Figure A.2: For Cause Strikes: Defense Initiated



Notes: This figure shows a histogram for the number of for cause strikes successfully requested by the defense in the 604 trials from 2015 through September 2017 in our full dataset (includes cases in which parties exceeded their peremptory strike limits).

Figure A.3: For Cause Strikes: Prosecution Initiated



Notes: This figure shows a histogram for the number of for cause strikes successfully requested by the prosecution in the 604 trials from 2015 through September 2017 in our full dataset (includes cases in which parties exceeded their peremptory strike limits).

Appendix Tables

Table A.1: Jury Pool Characteristics v. Baseline Characteristics

	Jury Pool Characteristics					
	Prop. Black	Prop. Female	Avg. Age	Med. Income	Prop. Dem.	Prop. Rep.
Baseline Characteristics	(1)	(2)	(3)	(4)	(5)	(6)
Defendant characteristics						
Age	-0.00	0.00	-0.01	3.31	-0.00	-0.00
	(0.00)	(0.00)	(0.01)	(20.39)	(0.00)	(0.00)
Black	-0.00	0.02	-0.59*	-497.89	-0.01	-0.02**
	(0.01)	(0.01)	(0.34)	(535.79)	(0.01)	(0.01)
Hispanic	-0.00	0.01	-0.37	-427.62	-0.01	-0.00
	(0.01)	(0.01)	(0.44)	(721.95)	(0.01)	(0.01)
Female	-0.00	0.02	-0.98**	-448.22	0.01	-0.01
	(0.01)	(0.01)	(0.43)	(723.60)	(0.01)	(0.01)
# Prev. Imprisonments	-0.00	-0.01**	0.16	-164.98	-0.00	0.00
	(0.00)	(0.00)	(0.14)	(249.86)	(0.00)	(0.00)
Years Prev. Imprison.	0.00	-0.00	-0.07	-22.84	-0.00	-0.00
	(0.00)	(0.00)	(0.06)	(81.06)	(0.00)	(0.00)
Attorney characteristics						
Defense Experience	0.00	0.00	0.02	-50.35*	0.00	0.00
	(0.00)	(0.00)	(0.02)	(27.05)	(0.00)	(0.00)
Prosecutor Experience	-0.00	0.00**	-0.05**	-22.22	-0.00	0.00
	(0.00)	(0.00)	(0.02)	(43.51)	(0.00)	(0.00)
Def. Law Sch. Rank	-0.00	0.00	-0.01**	-2.31	0.00	-0.00
	(0.00)	(0.00)	(0.00)	(3.94)	(0.00)	(0.00)
Pr. Law Sch. Rank	-0.00	-0.00	-0.00	1.95	-0.00	0.00
	(0.00)	(0.00)	(0.00)	(4.06)	(0.00)	(0.00)
PD	-0.00	-0.00	0.10	-278.13	0.01	0.00
	(0.01)	(0.01)	(0.40)	(625.52)	(0.01)	(0.01)
Case characteristics						
Homicide	-0.01	-0.00	0.07	1,690.41	-0.00	0.00
	(0.01)	(0.02)	(0.55)	(1,161.04)	(0.02)	(0.02)
Other Violent Offense	-0.00	-0.00	-0.36	99.21	0.02*	-0.01
	(0.01)	(0.01)	(0.42)	(647.36)	(0.01)	(0.01)
Property Offense	-0.01	0.02*	-0.19	745.09	0.02**	-0.01
	(0.01)	(0.01)	(0.45)	(685.94)	(0.01)	(0.01)
Drug Offense	0.00	0.04**	-0.67	1,044.59	0.03*	-0.02
	(0.01)	(0.02)	(0.60)	(1,000.35)	(0.01)	(0.01)
Sex Offense	0.01	0.02	-0.77	-306.12	0.02*	-0.03**
	(0.01)	(0.02)	(0.52)	(996.50)	(0.01)	(0.01)
Weapons Offense	-0.01	0.02	-0.17	177.79	0.03*	0.02
	(0.01)	(0.02)	(0.74)	(1,268.88)	(0.02)	(0.02)
Counts Charged	-0.00	-0.00	0.01	109.51	0.00	-0.00
	(0.00)	(0.00)	(0.05)	(80.80)	(0.00)	(0.00)
Observations	527	527	527	527	527	527
Adj. R-squared	-0.02	0.02	0.01	-0.01	0.00	0.01
F-Statistic	0.49	2.15	1.39	0.87	1.10	1.45

Notes: This table shows OLS regression estimates (with heteroskedastic robust standard errors) of various baseline defendant, attorney, and case characteristics on jury pool characteristics as listed at the top of each column. Pool income is the median across jurors within a pool, with each juror's income estimated by the median income in the zip code in which she resides (using U.S. Census data and 2017 inflation-adjusted dollars). "Other Offense" is an omitted category for case characteristics. All variables are as described in previous tables and the text. The F-statistic jointly tests whether all coefficients are = 0 in a given regression. *** = significant at 1% level, ** = significant at 5% level, * = significant at 10% level.

References

David S Abrams, Marianne Bertrand, and Sendhil Mullainathan. Do judges vary in their treatment of race? *The Journal of Legal Studies*, 41(2):347–383, 2012.

Alberto Alesina and Eliana La Ferrara. A test of racial bias in capital sentencing. *American Economic Review*, 104(11):3397–3433, 2014.

Shamena Anwar, Patrick Bayer, and Randi Hjalmarsson. The impact of jury race in criminal trials. *The Quarterly Journal of Economics*, 127(2):1017–1055, 2012.

Shamena Anwar, Patrick Bayer, and Randi Hjalmarsson. The role of age in jury selection and trial outcomes. *The Journal of Law and Economics*, 57(4):1001–1030, 2014.

David Arnold, Will Dobbie, and Crystal S Yang. Racial bias in bail decisions. *The Quarterly Journal of Economics*, 133(4):1885–1932, 2018.

Barbara Allen Babcock. Voir dire: Preserving its wonderful power. *Stan L. Rev.*, 27:545, 1974.

David C Baldus, George Woodworth, David Zuckerman, and Neil Alan Weiner. The use of peremptory challenges in capital murder trials: A legal and empirical analysis. *U. Pa. J. Const. L.*, 3:3, 2001.

Stephanos Bibas. Plea bargaining outside the shadow of trial. *Harvard Law Review*, pages 2463–2547, 2004.

Dennis J Devine, Laura D Clayton, Benjamin B Dunford, Rasmy Seying, and Jennifer Pryce. Jury decision making: 45 years of empirical research on deliberating groups. *Psychology, public policy, and law*, 7(3):622, 2001.

Shari Seidman Diamond, Destiny Peery, Francis J Dolan, and Emily Dolan. Achieving diversity on the jury: Jury size and the peremptory challenge. *Journal of Empirical Legal Studies*, 6(3):425–449, 2009.

Frank H Easterbrook. Criminal procedure as a market system. *The Journal of Legal Studies*, 12(2): 289–332, 1983.

Francis X Flanagan. Peremptory challenges and jury selection. *The Journal of Law and Economics*, 58 (2):385–416, 2015.

Francis X. Flanagan. Race, gender, and juries: Evidence from north carolina. *The Journal of Law and Economics*, 61(2):189–214, 2018. doi: 10.1086/698193.

Roger Allan Ford. Modeling the effects of peremptory challenges on jury selection and jury verdicts. *Geo. Mason L. Rev.*, 17:377, 2010.

Mark Hoekstra and Brittany Street. The effect of own-gender jurors on conviction rates. *The Journal of Law and Economics*, 64(3):513–537, 2021.

Maureen A Howard. Taking the high road: Why prosecutors should voluntarily waive peremptory challenges. *Geo. J. Legal Ethics*, 23:369, 2010.

Robert H Mnookin and Lewis Kornhauser. Bargaining in the shadow of the law: The case of divorce. *Yale Law Journal*, 88:950, 1978.

Anna Offit. Prosecuting in the shadow of the jury. Available at SSRN: `https://ssrn.com/abstract=3225958`, 2019.

M Marit Rehavi and Sonja B Starr. Racial disparity in federal criminal sentences. *Journal of Political Economy*, 122(6):1320–1354, 2014.

Hans Zeisel and Shari Seidman Diamond. The effect of peremptory challenges on jury and verdict: An experiment in a federal district court. *Stan. L. Rev.*, 30:491, 1977.