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Attorney Empowerment in Voir Dire and the Racial Composition of Juries^{*†}

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Abstract

Giving attorneys more power in the voir dire (jury selection) process may allow them to 1) find grounds for dismissal of jurors whom they wish to strike on *a priori* grounds; 2) acquire information that enables them to identify favorably-inclined jurors more precisely; or both. Attorneys who are more skilled can better use such increased power to retain the jurors they prefer. We show theoretically that, because defense attorneys prefer non-white jurors *a priori*, the interaction of empowerment and defense attorney skill should produce juries with a greater proportion of non-whites if only the first mechanism is operative, but need not have this effect if the second is operative. We then examine these issues using a detailed dataset on all non-capital felony trials in four large and diverse counties over a two-year period. We find that skilled and empowered attorneys can indeed stack juries by retaining jurors predisposed to their side at a greater rate, and that the distribution of relative attorney skill in our data is such that defendants benefit on average. However, we find that empowerment has a small and insignificant impact on the racial composition of the seated jury, regardless of attorney skill.

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1 Introduction

Voir dire, more commonly known as jury selection, is the process by which a pool of potential jurors is questioned and pared down to the seated jury that will eventually hear a trial. The legal rationale for conducting voir dire, rather than simply drawing juries randomly from the population, is to ensure that juries can act impartially and competently in rendering their verdicts. However, there is a widespread notion that voir dire practices employed in many trials grant attorneys too much freedom to shape the jury, leading instead to the seating of biased juries and to the systematic exclusion of certain groups from jury service. Thus, jury selection is often perceived as harming defendants by leading to unfair trial outcomes, harming potential jurors by facilitating their discriminatory removal on the basis of race or other characteristics, and generally detracting from the trustworthiness of the legal system.¹

Our goal in this paper is to discover what attorneys can actually accomplish in voir dire. When attorneys are granted more freedoms, will the jurors who make it through the jury selection process be biased against defendants? Will they be less likely to belong to certain identifiable groups?

We seek to answer these questions first with a simple model of attorney behavior. Attorneys on a given side are assumed to have the objective of retaining jurors who are favorably predisposed to their side. Such predispositions are assumed to be correlated with an observable juror characteristic, for example, race. When attorneys are unempowered – that is, when they have limited freedoms in voir dire – they must pursue their objective by striking potential jurors according to this characteristic. We model empowerment as having two potential effects: it can improve the ability of attorneys to strike whichever potential jurors they choose to; and it can allow attorneys to acquire more accurate information about the predispositions of potential jurors than can be inferred from the observable characteristic alone. We allow the magnitude of both of these effects to depend on the skill of the attorneys on each side.

The model predicts that, when attorneys are empowered, more skilled attorneys will have greater success in retaining jurors favorably inclined to their side. Therefore, empowerment can indeed lead to a seated jury that is more prejudiced against the defendant than a randomly-drawn jury would be – but only when the prosecuting attorney holds a skill advantage over the defense attorney. Empowerment can likewise lead to a seated jury that is prejudiced in favor of the defendant when the defense attorney holds a skill advantage.

However, the model does not make a clear prediction concerning the effect of empowerment on the distribution of seated jurors by the observable characteristic. If empowerment only increases striking ability and prosecutors have an *a priori* preference for white jurors, they will leverage

¹King (1993) discusses these categories of harm from the perspective of the evolution of related Supreme Court decisions. Much of the popular and scholarly discussion on these matters focuses on exclusion from juries based on race, and even more specifically, on blacks excluded from juries in favor of whites. We take a broad perspective on the potential to exclude jurors on the basis of any observable characteristics. However, we also recognize the prevalence and importance of the focus on race, and adopt this focus ourselves in most of our empirical analysis.

empowerment to strike non-white jurors at a higher rate. But defense attorneys will likewise leverage empowerment to strike white jurors at a higher rate. If the defense attorney holds a skill advantage, the overall effect of empowerment will be a greater proportion of non-whites on the seated jury than in the pool of potential jurors. However, when the information revelation effect of empowerment is also present, the defense may be able to identify some white jurors who are preferred to some non-white jurors (and vice versa for the prosecutor). In this case, the interaction of empowerment and defense attorney skill need not lead to a higher proportion of non-whites on seated juries, and can, in fact, be associated with white jurors being retained at a very high rate.

Next, we test our theoretical predictions empirically, using a rich dataset on all non-capital felony trials in four large and diverse counties over a two-year period. The dataset includes information on the freedoms that were granted to attorneys during jury selection, on attorney skill, on observable characteristics of jurors, on the self-reported pre-deliberation leanings of jurors, and on numerous other characteristics of the trial and the parties involved.

The results of our empirical analysis are surprising. Attorney empowerment in jury selection, other things equal, is associated with a concentration of jurors who report having greater favoritism *towards* the defense prior to deliberations. This effect is almost entirely accounted for by trials in which the defense attorney held a skill advantage. We interpret this result as confirmation of the basic validity of our model.

However, neither skill nor empowerment nor their interaction is associated, in our data, with any substantial impact on the average composition of seated juries by observable characteristics, including, most notably, race. In the context of our model, this result implies that, for at least some of the trials in our dataset, attorneys leveraged empowerment in *voir dire* to learn more about potential jurors, rather than just to strike more effectively.

1.1 Related Literature and Our Contributions

Our paper connects two branches of related literature to which economists have contributed. The first comprises theoretical studies of attorney behavior in jury selection and potential impacts of this behavior on verdicts. The second comprises empirical studies of the impacts of race and other juror characteristics on trial outcomes.

There is a common and natural framework underlying much of the previous theoretical work. The foundation is an assumption that potential jurors are drawn from a unidimensional distribution of predispositions. Other things equal, the defense attorney will want to strike the potential jurors with the strongest inclinations towards the prosecution, and the prosecuting attorney will likewise want to strike the potential jurors with the strongest inclinations towards the defense.

Brams and Davis (1978) explore this framework from a game theoretic perspective, and characterize attorney optimal responses when facing various constraints related to jury size, sequence

of decisions, and number of strikes allowed.² Neilson and Winter (2000) make use of the same underlying framework to gauge the impact of allowing attorneys a greater number of strikes on two types of injustice: false convictions and false acquittals.³ A common finding of this literature is that the probability of a conviction will often be too high – relative either to a case in which juries are chosen randomly or to a case in which the likelihood of injustice is minimized – unless the defense attorney is allowed a greater number of strikes than the prosecuting attorney.

There have been several attempts to establish empirical evidence that certain observable juror characteristics are associated with different trial outcomes. Anwar, Bayer and Hjalmarsson (2011) find that juries formed from all-white jury pools convict black defendants at a substantially and statistically significantly higher rate than they convict white defendants. Lee (2010) uses state variation in the timing of jury reforms, and also finds that greater potential racial heterogeneity on juries is associated with lower conviction rates of minority defendants. Lehmann and Smith (2011) find that juries with a larger proportion of blacks are less likely to convict regardless of the race of the defendant, and that juries with lower average incomes are also less likely to convict. On the other hand, Hannaford-Agor et al. (2002) find little relation between jury racial composition and the probability of a hung jury.⁴

This empirical literature establishes that attorneys who wish to maximize the probability of a case being decided for their side *should want* to remove jurors on the basis of race and perhaps other observable characteristics, at least in the absence of other useful information. However, it has not satisfactorily explored how attorneys actually behave and what they accomplish in *voir dire*.⁵ The theoretical literature, on the other hand, has not incorporated empirical insights on juror race comprehensively, and can be usefully extended in this and other ways. We pursue such extensions theoretically, and we empirically confirm our model's predictions concerning what attorneys can accomplish.

²Roth, Kadane and Degroot (1977), Degroot and Kadane (1980) and Kadane, Stone and Wallstrom (1999) provide technical extensions on this theme.

³Kadane and Kairys (1979) design an algorithm to find the optimal number of strikes allowed under a slightly different definition of injustice. Ford (2010) examines the impact of varying the number of strikes allowed on conviction rates in a similar framework, without reference to an explicit definition of injustice. Feddersen and Pesendorfer (1998) consider the same types of injustice as Neilson and Winter (2000) in the context of strategic voting, but jurors in that context are *ex ante* identical, so there is no scope to examine aspects of jury selection.

⁴Shayo and Zussman (2011), Alesina and La Ferrara (2011) and Abrams et al. (2011) provide evidence that the race of judges, victims, and defendants can likewise affect the outcomes of non-jury trials. There are many other studies on race and trial outcomes that rely on mock trials and case studies of a handful of trials at a time. A fairly recent and comprehensive review of such studies is given by Sommers and Ellsworth (2003), and a more in-depth and critical review of the earliest of these studies is given by Pfeifer (1990). Bowers et al. (2001) include 340 trials in their analysis, but are unable to adequately control for heterogeneity across these trials. They find that the death penalty is three times as likely for a case with a black defendant whose victim is white and for which the jury consists of five or more white male jurors than for similar cases with more mixed juries.

⁵One potential exception is Diamond et al. (2009), who find, in their sample of small claims civil suits, that attorneys on both sides appeared to strike potential jurors largely on the basis of race, but that these strikes mostly offset one another. The authors do not discuss the impact of jury composition on the outcomes of the trials.

Our first contribution relative to previous theoretical work is that we make more realistic assumptions about the information available to attorneys. Brams and Davis (1978) assume that attorneys can observe juror predispositions, while Nielson and Winter (2000) assume that attorneys cannot observe predispositions but know that an observable juror characteristic is predictive of a stronger predisposition towards the prosecution. We assume instead that jurors are described by a two-dimensional set of characteristics: one, such as race, which is always observable; and a second, namely a predisposition to one side or the other, which can potentially be imperfectly observed if attorneys are empowered. When attorneys cannot observe predispositions at all, they have no choice but to rely on the common-knowledge correlation between the observable characteristic and unobservable leanings. But when empowerment allows attorneys to learn about juror inclinations, they can use this partial information *in addition to* the information already afforded by the observable characteristic. Our model thus includes the Nielson and Winter framework as a special case, while, in the general specification, avoiding the full-information assumption of Brams and Davis.

Our second contribution relative to previous theoretical work is that we broaden our focus beyond peremptory challenges. Peremptory challenges allow attorneys to strike any potential juror without stating a reason, with very few exceptions. In contrast, successful challenges for cause must be accompanied by an acceptable argument that the targeted potential jurors have demonstrated an inability to be impartial. Previous studies only treat attorney decisions concerning which potential jurors to strike with a limited number of available peremptory challenges, assuming that all potential jurors for whom an inability to be impartial could be demonstrated have been removed via challenges for cause in an unmodeled earlier stage. Our modeling assumptions reflect a more realistic situation in which attorneys attempt to use both strikes for cause and peremptory strikes to remove jurors with unfavorable predispositions to their side. The ability of attorneys in our model to remove and object to the removal of potential jurors – and the effect of empowerment on this ability – is handled in a reduced-form manner that can incorporate strikes made via either type of challenge.

Our third contribution is the inclusion of heterogeneity in attorney skill, which follows naturally from these first two. There is little potential role for attorney skill when juror predispositions are fully observable and only peremptory challenges are considered. However, the acquisition and interpretation of information, and persuasiveness in semantic arguments over what constitutes grounds for dismissal for cause, can reasonably be expected to depend on attorney facility. We hence allow for the possibility that more skilled attorneys can reap greater benefits from empowerment than less skilled attorneys in our model, and test for this possibility empirically.⁶

Our final noteworthy contribution is the consideration of juror predispositions directly, rather than trial verdicts. To be sure, the verdict is what all parties involved in the trial care about most.

⁶Abrams and Yoon (2007) and Shinall (2010) find that attorney skill is an important determinant of trial outcomes, but do not discuss this in the context of jury selection.

However, verdicts are a product of more than the predispositions of individual jurors. Of specific concern for our work is that verdicts are determined by jury deliberations, which can entail complex group dynamics and decision-making. Previous theoretical contributions skirt this issue by calculating the jury's overall probability of conviction as the simple product of each juror's probability of conviction, while noting the unsatisfactory nature of the implicit independence assumption. Instead, we take individual juror predispositions as the outcome variable of interest to attorneys. We consider this to be a desirable approach based on our postulation that attorneys are much better at assessing individual juror leanings than they are at predicting how the seated jurors will interact with one another in arriving at their final decision. Indeed, Gobert et al. (2009) suggest that techniques available to attorneys for assessing individual jurors are much more advanced than those for predicting deliberation dynamics, and that selecting "juries, not jurors" is still very much an emerging concept.

This paper is the most complete attempt to date to measure what attorneys can actually accomplish, given varying degrees of empowerment in jury selection, in terms of multiple characteristics of the seated jurors they manage to retain. We find that skilled and empowered attorneys can successfully retain jurors favorably inclined to their side on seated juries, but that they do not necessarily accomplish this simply by striking according to juror race. In fact, the attorneys in our sample end up altering the racial composition of the seated jury relative to the jury pool very little: equivalent to no more than one juror of a given race in either direction on a twelve-person jury. The implication is that empowerment in *voir dire* can allow attorneys to uncover valuable information about jurors and therefore to avoid using racial stereotypes as a crutch.

We believe that our results could lead to more constructive policy advice than recommendations that have been made to restrict attorney freedoms in jury selection. We will leave specific discussion of policy implications for the conclusion, along with an attempt to address the important question of how our results might generalize to regions that we have not been able to include in our empirical analysis.

The paper will proceed as follows. The second section introduces the model and presents its main results. The third section describes the dataset that we employ. The fourth section presents and discusses the empirical findings and a number of robustness checks. The final section concludes by discussing remaining challenges to be addressed and offering policy suggestions.

2 A Model of Attorney Empowerment and Jury Selection

2.1 Voir Dire in Practice and in the Model

A very common organizational framework for voir dire employed in some variation in most American criminal trials is known as the strike-and-replace method.⁷ Under this framework, the “panel” or “venire” of potential jurors – which we can take as being approximately randomly drawn from the county population – is first randomly ordered. Sometimes the entire panel is initially examined at this stage, and strikes for cause exercised. In either case, the first several panelists from the random order are provisionally seated and potentially examined in more depth. Once the attorneys are satisfied that none of the provisionally seated jurors can be removed for cause, they exercise peremptory challenges in some pre-determined alternating sequence, usually with the prosecutor going first. Those provisionally seated jurors who are thus excused are then replaced by the panelists next in line. This cycle then repeats itself, sometimes with the restriction that seated jurors who have already been “passed” on by both attorneys cannot be further examined or struck. The process ends when there are twelve jurors in the box who both attorneys have passed on, or when all peremptory challenges have been exhausted by both sides. In either case, the final seated jury is composed of the twelve jurors then seated, with the remaining venirepersons who have not already been struck also sent home.

The examination of potential jurors can be carried out primarily by the judge, by the judge with suggestions or a greater degree of participation from the attorneys, or primarily by the attorneys. Attorneys may also have access to the results of questionnaires filled in by potential jurors: either rudimentary surveys used by the court or, sometimes, “Supplementary Juror Questionnaires” (SJQs) that the attorneys have designed. The judge has full discretion over the decisions of how much attorney participation to allow and whether an SJQ is allowed. Individual judges tend to have strong views on how these decisions should be made, and to hold adamantly to them.

The primary concerns that are examined in any voir dire are the existence of personal relationships between panelists and any other parties involved in the trial, and the capacity to understand and contemplate the salient legal issues and evidence in a dispassionate and impartial manner. But many other subjects can also be explored, especially if attorneys have a high degree of participation. As voir dire questioning proceeds, attorneys challenge for cause by arguing that the responses of a given venireperson have revealed an inability to be impartial. The attorney on the opposite side is permitted to object to this argument and raise counter-arguments, and the judge must ultimately rule in favor of or against the strike. If the judge rules in favor of the strike, the potential juror is dismissed. Attorneys can strike an unlimited number of potential jurors for cause, as long as the

⁷Our understanding of the voir dire process is based on Gobert et al. (2009) and Starr and McCormick (2001), as well as informal interviews with a number of acquaintances in the legal profession. The discussion here will be cursory, but a more detailed description is provided by Lehmann and Smith (2011).

judge can be sufficiently convinced that disqualification has been demonstrated.

Attorneys also have a limited number of peremptory strikes at their disposal. These allow the removal of potential jurors without the requirement that bias or any other deficiency be demonstrated. States and some counties provide guidelines on the number of peremptory challenges that should be made available, but judges are under no obligation to follow these guidelines, and exceptions are often made for various reasons. Nonetheless, the total number of peremptory strikes available is generally small relative to the size of the panel. There is also a chance that a peremptory strike can be disallowed: if an attorney suspects that the motivation for a strike is discriminatory in nature, a “Batson objection” can be raised. If the opposing attorney cannot respond by stating a non-discriminatory justification for the strike, it will be disallowed.

We model a simplified strike-and-replace voir dire procedure. The panel is assumed to be infinite, with a composition across all venireperson characteristics fixed and mirroring that of the population.⁸ The panel is arranged according to a random ordering that attorneys do not observe, and the single panelist at the top of this order is provisionally seated. If either attorney chooses to strike this panelist and is successful, the panelist is dismissed, and the next panelist from the random ordering is provisionally seated. This process continues until an attempted strike is unsuccessful or neither attorney chooses to attempt a strike. At that time, the provisionally-seated panelist becomes the sole member of the one-person seated jury that will hear the trial.⁹

There is a probability that any strike attempt will be successful, and we allow this probability to differ by attorney and to depend on skill and empowerment. This is mostly meant to represent strikes for cause. With a certain probability, the panelist under examination will exhibit some traits that one of the attorneys can represent as demonstrating impartiality, at least to the point of rebutting any objections raised by the opposing counsel and convincing the judge. Empowered and skilled attorneys can better uncover such traits, and better argue over the semantics of how they reveal impartiality. However, this framework can be slightly modified to incorporate peremptory strikes too. First of all, peremptory strikes can have a less-than-perfect success rate due to Batson objections. More generally, though, we could allow attorneys to have a much higher (or perfect) success rate for a limited number of strikes. This is an extension that we consider worthwhile and that we are currently pursuing. In the meantime, we propose that our present approach is capable of providing most of the insights that the more general approach will.

We will proceed for the rest of this section within this simplified framework. After formalizing some notation and concepts, the roles of attorney empowerment and skill will be clarified. Our

⁸Courthouses can, in principle, bring in new panels for a given trial if the initially-drawn panel is exhausted by strikes, though this is rarely necessary. However, there have been concerns raised that panel composition does not accurately reflect population characteristics due to insufficient methods to construct and maintain master juror lists. Neither of these issues is important for our analysis.

⁹The one-person jury model can be interpreted as a single round of decisions in a series of rounds in which attorneys decide how to fill a 12-person jury one juror at a time, which is essentially the approach taken by Brams and Davis (1978). The relaxation of this assumption, though cumbersome, is therefore straightforward.

main goal is to evaluate the probability that the seated juror will possess various attributes, and characterize how this probability depends on attorney empowerment and skill.

2.2 Preliminaries

Suppose that an $\alpha \in (0, 1)$ proportion of the panel is non-white and a $1 - \alpha$ proportion is white.¹⁰ Each potential juror is described by two characteristics: his/her

1. race, $i = \{non - white (n), white (w)\}$ and
2. standard required to convict, $s = \{high (h), low (l)\}$.

Race is fully observable to both attorneys, but standards required to convict are not. We interpret someone with a high standard required to convict – that is, someone requiring very strong evidence against the defendant to vote guilty – as someone with a predisposition or inclination towards the defense. We therefore refer to h -types as those with a predisposition towards the defense, and l -types as those with a predisposition towards the prosecution. The defense attorney will always prefer seating an h -type to seating an l -type on the jury, and vice versa for the prosecution. $p_i \in (0, 1)$ is the proportion of jurors of race i who are predisposed to the defense (i.e. h -types), and the remaining $(1 - p_i)$ is the fraction who are predisposed towards the prosecution (i.e. l -types). These proportions are common knowledge to both attorneys.

We assume that the proportion of h -types is greater among non-whites than whites:

Assumption 1 (Race and Predispositions).

$$p_n > p_w \tag{1}$$

Hence, without any additional information about the panelists, the defense will always prefer a non-white panelist over a white panelist, and the prosecution will always prefer to seat a white venireperson rather than a non-white.

Given the proportion of non-whites/whites and the fraction of their predisposition types in the potential juror population, we define

$$p = \alpha p_n + (1 - \alpha) p_w \tag{2}$$

as the proportion of panelists with a predisposition towards the defense.

2.3 Roles of Attorney Empowerment and Skill

In voir dire, the judge may grant greater power to the attorneys, for example by allowing them to participate to a greater degree in the examination of potential jurors or to use a Supplementary

¹⁰We will focus on race for simplicity of exposition, but note that all results hold for any observable characteristic.

Juror Questionnaire. We assume that such greater power potentially allows attorneys to 1) find grounds to successfully strike any panelist with a higher probability than otherwise; and/or 2) to acquire additional information about the predispositions of panelists and thus to better identify potential jurors who are more favorably-inclined to their side. We will first discuss the expected racial composition and predispositions of the seated jury when attorneys are not empowered. We will then analyze the impact of attorney empowerment on these aspects of the seated jury when only the first mechanism is operative; when both mechanisms are operative; and when only the second mechanism is operative.

In our analysis, we allow the magnitude of both effects to depend on the relative skill levels of the attorneys. For simplicity of exposition, we assume that the defense attorney is more highly skilled than the prosecutor throughout our theoretical discussion. This is motivated by the fact that it is common in criminal trials for the defense attorney to be more experienced than the prosecutor. However, the model is symmetric from the perspective of each side, and our general conclusions hold whether the defense or the prosecutor is more skilled.

2.4 No Attorney Empowerment

Let β be the baseline probability that a strike by either side is successful. We assume that greater rhetorical skill is ineffective without some information beyond that revealed by a rudimentary judge-conducted examination of panelists. Thus, without empowerment, attorneys have no means to improve this success rate, regardless of any skill advantage.

With probability α , the first potential juror to be provisionally seated will be non-white. The prosecution will want to strike this panelist, since no information is available besides the panelist's observable race and the *a priori* knowledge that non-whites are more likely to be predisposed to the defense. With probability $(1 - \beta)$, the strike will be unsuccessful, and the non-white panelist will become the seated juror. With probability β , the strike will be successful, and the next panelist in the random order will be provisionally seated. There is then a probability of $(1 - \alpha)$ that this panelist will be white. The same reasoning can be repeated in this case, except that it will be the defense attorney who attempts to strike the provisionally-seated juror. It is clear from the symmetry of the reasoning and the equality of striking success rates that attorneys will be powerless to alter the probability that the seated juror will be non-white beyond the probability that any randomly-drawn panelist is non-white, namely α . It follows directly that the expected probability that the seated juror will be predisposed towards the defense is just p .

Proposition 1. *When attorneys are unempowered, regardless of attorney skill,*

- (i) *the probability that the seated juror will be non-white is the same, and*
- (ii) *the probability that the seated juror will be predisposed towards the defense is also the same*

as if the jury were randomly drawn from the panel.

2.5 Empowerment Increases Striking Ability Only

Assume now that attorney empowerment allows attorneys to find a broader basis for striking a panelist, thereby increasing the probability that an attempted strike will be successful. Let β_p be the prosecutor's striking success rate with attorney empowerment, and likewise, let β_d be the defense attorney's striking success rate when attorneys are empowered, where both of these are greater than the baseline striking success rate without empowerment. We further assume that the effect of empowerment on striking success is greater for the more highly skilled attorney, because attorneys with a greater rhetorical facility will be better able to support their claims of juror impartiality in the face of objections from their opponents. This assumption also allows for greater attorney skill to improve the chance of blocking an opponent's attempted strikes through more convincing counter-arguments. Since, as was discussed above, we are treating the defense attorney as the more highly skilled attorney for the sake of exposition, this implies that $\beta_d > \beta_p$. Combining these assumptions, we have:

Assumption 2 (Empowerment and Striking Ability).

$$0 < \beta < \beta_p < \beta_d < 1 \tag{3}$$

The voir dire proceeds as follows. Each attorney decides whether to strike the panelist at the top of the random order or not. If the panelist is not struck or an attempted strike fails, then the panelist will be seated. If the panelist is struck successfully, he/she is replaced by the next individual from the top of the randomly-ordered venire, and the attorneys decide to attempt a strike or not (and argue over the basis for the strike). The process continues until a juror is successfully seated.

Let V be the probability that the panelist who is eventually seated is non-white. Additionally, let V_n be the probability that the seated juror will be non-white if a non-white panelist is at the head of the queue. V_w is, then, the probability that the seated juror will be non-white if a white panelist is the head of the queue. Our assumption that the panel is of infinite size ensures that the identity of the juror at the head of the queue does not change the composition of the remaining panel.

With no additional information about the predispositions of the potential jurors aside from race, the defense (prosecuting) attorney will always try to strike when a white (non-white) panelist is at the head of the queue. Let's first consider the case in which a non-white panelist is at the head of the queue. With probability $(1 - \beta_p)$, the panelist will survive the prosecutor's strike request and end up as the seated juror. With probability β_p , he/she will be successfully struck and replaced by

a random panelist at the head of the queue. Therefore, V_n can be defined as

$$V_n = (1 - \beta_p) + \beta_p V. \quad (4)$$

Similarly, when a white is at the head of the queue, he/she will survive the defense attorney's strike with probability $(1 - \beta_d)$, or with probability β_d , will be struck and be replaced by a random panelist at the head of the queue. V_w is then

$$V_w = \beta_d V. \quad (5)$$

The overall expected probability that the panelist who is eventually seated is non-white is a weighted average of V_n and V_w , with weights given by the probability that a non-white will be at the head of the queue, α , versus a white, $(1 - \alpha)$:

$$V = \alpha V_n + (1 - \alpha) V_w. \quad (6)$$

This probability is then defined completely by the system of three equations (4), (5), and (6). Solving this system yields

$$V = \frac{\alpha(1 - \beta_p)}{(1 - \beta_d) + \alpha(\beta_d - \beta_p)}. \quad (7)$$

It is trivial to show from (7) that $V > \alpha$ as long as $\beta_d > \beta_p$, which holds by Assumption 2.

It is also intuitive and can be easily shown that the probability that the seated juror is predisposed to the defense – which we call U – is higher than that in the panel, p . This probability is defined as

$$U = p_n V + p_w(1 - V), \quad (8)$$

which, after substitution of (7) for V , yields

$$U = \frac{\alpha(1 - \beta_p)p_n + (1 - \alpha)(1 - \beta_d)p_w}{(1 - \beta_d) + \alpha(\beta_d - \beta_p)}. \quad (9)$$

It can be shown that $U > p$ as long as $\beta_d > \beta_p$ and $p_n > p_w$, which hold directly by Assumptions 1 and 2.

These results on the race and predisposition of the seated juror are intuitive. When empowerment increases striking ability only, the attorneys are simply able to more successfully strike on the basis of their *a priori* racial preferences; and when the defense attorney has a skill advantage, the defense is relatively more successful at striking than the prosecution, and so the outcome will more frequently benefit the defense attorney on net.

Proposition 2. *When empowerment only increases attorney striking ability, and the defense is more skilled,*

- (i) the probability that the seated juror will be non-white is higher, and
- (ii) the probability that the seated juror will be predisposed towards the defense is also higher than if the jury were randomly drawn from the panel.

2.6 Empowerment Increases Striking Ability and Provides Information on Juror Predispositions

Now suppose that empowerment not only increases the striking ability of the attorneys, but also allows attorneys to acquire additional information about the panelist and to better identify panelists who are favorably-inclined to their side.

We assume that, when empowered, attorneys observe a dichotomous signal $\theta \in \{High(H), Low(L)\}$ correlated with the panelists' predispositions. For simplicity, we assume that *only the more skillful attorney* of the two sides is able to observe θ .¹¹ A panelist of type s exhibits H with probability σ_s and exhibits L with probability $1 - \sigma_s$. We assume that the σ 's are independent of race and that $\sigma_h > \sigma_l$, so that any h -type panelist has a higher probability of exhibiting H than any l -type panelist. The probability that a panelist of race i exhibits H is then

$$\gamma_i = p_i\sigma_h + (1 - p_i)\sigma_l, \quad (10)$$

and, under our parameters assumptions about p_i and σ_h , we know that $\gamma_n > \gamma_w$. We can also express the conditional posterior probability that a juror of race i exhibiting H is actually an h -type as

$$\delta_{iH} = Pr[s = h|i, \theta = H] = \frac{p_i\sigma_h}{p_i\sigma_h + (1 - p_i)\sigma_l}. \quad (11)$$

Similarly, the conditional posterior probability that a juror of race i and signal $\theta = L$ is actually an h -type is

$$\delta_{iL} = Pr[s = h|i, \theta = L] = \frac{p_i(1 - \sigma_h)}{p_i(1 - \sigma_h) + (1 - p_i)(1 - \sigma_l)}. \quad (12)$$

Because the less-skilled prosecutor does not see θ , his strategy in voir dire remains the same as before. That is, the prosecutor will try to strike non-white panelists and will attempt to block the striking of white panelists by the defense.¹² However, the defense attorney, now having seen θ ,

¹¹One could also assume that both attorneys observe θ , but that the more skillful attorney can observe the signal more accurately. This alternative assumption, while more realistic, has not been employed here because it complicates the model without providing additional insight.

¹²In fact, we implicitly make a rather stronger set of assumptions about the skill-disadvantaged attorney in order to ensure that this behavior arises. In addition to not allowing the prosecutor to see the signal, we also assume that the prosecutor is unable to learn anything by observing the striking behavior of the defense attorney, or otherwise condition his own striking behavior on the anticipated or actual striking behavior of the defense attorney. The prosecutor thus behaves as though fully unaware of the potential existence of any signal. Allowing the less-skilled attorney to instead observe a less precise signal necessitates the relaxation of this package of assumptions, which can give rise to some anomalous cases.

updates her race-based *a priori* belief about any panelist’s predisposition with information revealed by θ , according to (11) and (12).

Note that, given our assumptions that $p_n > p_w$ and $\sigma_h > \sigma_l$, a non-white panelist exhibiting H has the highest probability of being an h -type (δ_{nH}), while a white panelist exhibiting L has the lowest probability of being an h -type (δ_{wL}). However, it remains unclear whether $\delta_{wH} > \delta_{nL}$. This inequality will hold only if and only if the following condition is met.

Condition 1 ($\delta_{wH} > \delta_{nL}$).

$$\frac{\sigma_h}{\sigma_l} \frac{1 - \sigma_l}{1 - \sigma_h} > \frac{p_n}{p_w} \frac{1 - p_w}{1 - p_n}. \quad (13)$$

This condition states that the odds ratio of exhibiting the signal H between h -types and l -types is greater than the odds ratio of being an h -type between non-whites and whites. Intuitively, $\delta_{wH} > \delta_{nL}$ – so that the defense attorney will prefer some white panelists to some non-white panelists – if and only if the signal θ is sufficiently more informative about juror predispositions than race alone.

2.6.1 Defense Attorney’s Reservation Striking Threshold

When empowerment enables the defense attorney to better identify panelists predisposed to the defense, the defense and prosecuting attorneys can now potentially “agree” on their preference to strike or to seat a panelist.¹³ We, therefore, slightly modify our assumptions on empowerment’s effect on attorneys’ striking abilities by assuming that 1) if neither attorney wants to strike the panelist at the head of the queue, that individual will be seated on the jury; and 2) if both attorneys want to strike a panelist, that panelist will be struck with probability 1. If the prosecution and the defense disagree, then the probabilities of successful strikes are defined by β_p and β_d , respectively, as before.

Given the primitive parameters of the model and the informativeness of θ , the defense attorney will take the static behavior of the prosecutor as given, and choose her own striking behavior to maximize the expected probability that the seated juror will be predisposed to the defense. The defense attorney’s strategy can be represented as the choice of a reservation threshold δ_R such that she will attempt to strike a panelist if $\delta_{i\theta} < \delta_R$ and argue against any attempted strikes otherwise. This can be described more formally as follows:

$$\delta_R \in \arg \max_{\underline{\delta}} U(\underline{\delta}) \quad (14)$$

where $U(\underline{\delta})$ is the probability that the seated juror will be predisposed towards the defense and is

¹³For example, the prosecution will always want to seat a white panelist, as before. However, now the defense may want to seat a white panelist if her posterior belief that the panelist is an h -type is sufficiently high.

defined implicitly by the following system of five equations:

$$U_{nH}(\underline{\delta}) = (1 - \beta_p)\delta_{nH} + \beta_p U(\underline{\delta}) \quad (15)$$

$$U_{wL}(\underline{\delta}) = (1 - \beta_d)\delta_{wL} + \beta_d U(\underline{\delta}) \quad (16)$$

$$U_{nL}(\underline{\delta}) = \begin{cases} (1 - \beta_p)\delta_{nL} + \beta_p U(\underline{\delta}) & \text{if } \delta_{nL} \geq \underline{\delta} \\ U(\underline{\delta}) & \text{otherwise} \end{cases} \quad (17)$$

$$U_{wH}(\underline{\delta}) = \begin{cases} \delta_{wH} & \text{if } \delta_{wH} \geq \underline{\delta} \\ (1 - \beta_d)\delta_{wH} + \beta_d U(\underline{\delta}) & \text{otherwise} \end{cases} \quad (18)$$

$$U(\underline{\delta}) = \alpha\gamma_n U_{nH}(\underline{\delta}) + \alpha(1 - \gamma_n)U_{nL}(\underline{\delta}) + (1 - \alpha)\gamma_w U_{wH}(\underline{\delta}) + (1 - \alpha)(1 - \gamma_w)U_{wL}(\underline{\delta}). \quad (19)$$

The structure of equations (15) to (19) can be explained as follows. Since, as was mentioned above, a non-white exhibiting H has the highest probability of being predisposed to the defense and a white exhibiting L has the lowest, it is always optimal for the defense attorney to try to argue against attempted strikes of a non-white H and to attempt to strike a white L . The less-skilled prosecutor will always want to strike any non-white and argue against attempted strikes of any white, as explained above, and the defense attorney anticipates this fully. These choices are reflected in equations (15) and (16) defining U_{nH} – the expected probability that the seated juror will be predisposed to the defense when a non-white exhibiting H is at the head of the queue – and U_{wL} – the corresponding probability when a white exhibiting L is at the top of the order – respectively.

The probabilities that the seated juror will be an h -type when a non-white exhibiting L or a white exhibiting H is at the head of the queue depend on the threshold $\underline{\delta}$ relative to the $\delta_{i\theta}$'s. For example, if a white exhibiting signal H is at the head of the queue, $\delta_{wH} \geq \underline{\delta}$ represents a case in which neither the defense nor the prosecution attempt to strike him. Then the expected probability that the seated juror will be an h -type when a white exhibiting H is at the head of the queue in (18) is exactly equal to δ_{wH} , since he will be seated with probability 1 given our assumptions about attorney striking success. However, if $\delta_{wH} < \underline{\delta}$, this represents a case in which the defense attempts to strike, and the panelist will hence only be seated with probability $(1 - \beta_d)$, or will be replaced by a random panelist with probability β_d .

Finally, the overall expected probability that a seated juror will be predisposed towards the defense, U , for a given threshold $\underline{\delta}$, is a weighted average of (15) through (18), with weights given

by the population proportions of the four $i\theta$ groups. There will in general be a range for $\underline{\delta}$ over which U is maximized; we do not need to characterize δ_R more precisely than noting that it belongs to this range. This probability is hence maximized when the defense attorney follows the strategy of attempting to strike any panelist with $\delta_{i\theta} < \delta_R$ and arguing against any attempted strikes otherwise.

2.6.2 Race and Predisposition of the Seated Juror

We will limit our discussion to two key cases regarding the striking behavior of the defense attorney and the strength of the signal.

Case 1: $\delta_{nH} > \delta_{nL} > \delta_R > \delta_{wH} > \delta_{wL}$

In this case, Condition 1 does not hold, so that the information revealed by the signal is not enough to overcome the defense attorney's *a priori* preference for non-whites. Further, non-whites represent a large enough fraction of the population and are likely enough to be predisposed to the defense that the defense attorney chooses to set her reservation threshold δ_R above δ_{wH} . The net result is that the defense attorney will attempt to strike any white panelist, regardless of θ , and will object to attempts by the prosecutor to strike any non-white panelists, again regardless of θ . Therefore, this case is effectively the same as when empowerment only increased the striking abilities of the attorneys. The probability that the seated juror will be non-white is higher than if a randomly selected juror were seated, as is the probability that the seated juror will be predisposed towards the defense.

Case 2: $\delta_{nH} > \delta_{wH} > \delta_R > \delta_{nL} > \delta_{wL}$

A more interesting case is when Condition 1 is met, so that the defense attorney is able to identify some white panelists with a higher probability, conditional on the signal, of being predisposed to the defense than some non-white panelists. Furthermore, assume that the fraction of the population exhibiting signal H is moderate enough that it is optimal for the defense attorney to set her reservation striking threshold δ_R above δ_{nL} . Thus, the defense attorney will argue against the prosecutor's attempted strikes of non-white panelists exhibiting H , as usual; but will herself attempt to strike non-white panelists exhibiting L , joining in with the prosecutor's efforts in this regard. Furthermore, the defense attorney will not attempt to strike any white panelists exhibiting H , and since the prosecutor will never attempt to strike any white panelist, a white panelist exhibiting H that is at the top of the random order of panelists will become the seated juror without objection from either side. Therefore, in this case, V , the expected probability that the panelist who eventually becomes the seated juror will be non-white, is defined by the following system of

equations:

$$V_{nH} = (1 - \beta_p) + \beta_p V$$

$$V_{nL} = V$$

$$V_{wH} = 0$$

$$V_{wL} = \beta_d V$$

$$V = \alpha \gamma_n V_{nH} + \alpha(1 - \gamma_n) V_{nL} + (1 - \alpha) \gamma_w V_{wH} + (1 - \alpha)(1 - \gamma_w) V_{wL}.$$

Solving for V , we have

$$V = \frac{\alpha \gamma_n (1 - \beta_p)}{1 - \alpha[1 - \gamma_n(1 - \beta_p)] - (1 - \alpha)[(1 - \gamma_w)\beta_d]}. \quad (20)$$

It can be shown that $V < \alpha$ always, as long as Condition 1 and our basic parameter assumptions hold.¹⁴ Therefore, the probability that the seated juror will be non-white is *less* in this case than if the jury were chosen randomly.

However, as in previous cases, U , the probability that the seated juror will be predisposed towards the defense, is higher than in the panel overall. The most straightforward way to calculate U is to use the specific form of equations (15) through (19) for this case, which is

$$U_{nH} = (1 - \beta_p)\delta_{nH} + \beta_p U$$

$$U_{nL} = U$$

$$U_{wH} = \delta_{wH} \quad (21)$$

$$U_{wL} = (1 - \beta_d)\delta_{wL} + \beta_d U$$

$$U = \alpha \gamma_n U_{nH} + \alpha(1 - \gamma_n) U_{nL} + (1 - \alpha) \gamma_w U_{wH} + (1 - \alpha)(1 - \gamma_w) U_{wL}.$$

Solving for U yields

$$U = \frac{\alpha \gamma_n (1 - \beta_p) \delta_{nH} + (1 - \alpha) \gamma_w \delta_{wH} + (1 - \alpha)(1 - \gamma_w)(1 - \beta_d) \delta_{wL}}{1 - \alpha[1 - \gamma_n(1 - \beta_p)] - (1 - \alpha)[(1 - \gamma_w)\beta_d]} \quad (22)$$

where it can be shown that $U > p$ under our parameter assumptions.¹⁵

¹⁴The demonstration of this claim is tedious, as the expression involves all of the parameters of the model through the definition of γ_i in equation (10). It is more straightforward to show a sufficient but not necessary condition in terms of the striking success rates of each attorney. This sufficient condition is $\beta_d > p_n/p_w \times (1 - \beta_p)$. Intuitively, this condition is saying that it must be sufficiently more likely that the defense attorney successfully strikes a white panelist exhibiting L (hence obtaining another draw from the panel) than it is for the prosecutor to fail to strike a non-white panelist exhibiting H (which would result in the voir dire terminating with a non-white seated juror). This condition is fairly stringent for many parameter values. However, as noted, the only necessary conditions are the baseline parameter assumptions we have made.

¹⁵Again, this claim is tedious to demonstrate algebraically. However, it is clear that it must hold, since this is the objective that the defense attorney is attempting to maximize. If U were less than p , the defense attorney would

In Case 2, we have thus shown an example for which empowerment (which increases attorney striking abilities *and* allows them to more precisely identify juror predispositions in this case) leads to a *lower* probability that the seated juror will be non-white, compared to the proportion of non-whites in the population.

Other Cases

Cases 1 and 2 are the most extreme of the many possible cases. Others are nonetheless interesting. When the proportion of the panel exhibiting the High signal is small, the defense attorney may choose to refrain from striking all but white panelists exhibiting L , achieving a probability that the seated juror is non-white slightly greater than α . On the other hand, when this proportion is large, the defense attorney may wish to attempt to strike all but non-white panelists exhibiting H ; depending on the specific value of σ_h , the probability that the seated juror is non-white can be slightly less than or slightly greater than α . We will avoid a detailed exposition of these cases for the present, as Cases 1 and 2 are sufficient to support the following Proposition.

Proposition 3. *When empowerment increases attorney striking ability and allows attorneys to better identify panelists' predispositions, and the defense attorney is more skilled,*

- (i) the probability that the seated juror will be non-white may be higher or lower, yet*
- (ii) the probability that the seated juror will be predisposed towards the defense is always higher than if the juror were randomly drawn from the panel.*

2.7 Empowerment Provides Information on Juror Predispositions Only

For completeness, we can also consider the set of cases for which only the second effect of empowerment is present. Specifically, assume now that empowerment does not increase attorney striking ability, but does allow the more skilled attorney to more accurately identify juror inclinations. Mechanically, this just involves setting $\beta_p = \beta_d = \beta$ in equations (15) to (19) and repeating the analysis of the previous section.¹⁶ We proceed here with an intuitive discussion of the salient results, without revisiting the formal analysis.

In all cases, the probability that the seated juror will be non-white is less than the population proportion non-white. Whenever the attorneys differ in their desire to strike a panelist, the panelist will be struck successfully with probability β on net. The attorneys always disagree on white panelists exhibiting L and on non-white panelists exhibiting H . However, the attorneys can potentially agree on the two intermediate groups, depending on the quality of the signal and the defense

change striking behavior – for example, to that of Case 1 above – and be able to obtain a different U greater than p .

¹⁶We retain the assumption that the attorneys have a perfect striking success rate when they would both attempt to strike a panelist, and a perfect success rate at seating a panelist whom neither of them attempts to strike.

attorney’s reservation striking threshold. The result is either that white panelists exhibiting H will be struck with probability zero (rather than β); non-white panelists exhibiting L will be struck with probability one (rather than β); or both.¹⁷ The net effect of any of these, in combination with the fixed and common probability of a panelist from the extreme groups being struck, is to reduce the probability that a white panelist will be struck relative to that of a non-white panelist being struck. The probability that the seated juror will be predisposed to the defense must always be at least p , since the skilled defense attorney can always replicate the outcome that would arise if there were no empowerment by disregarding the information obtained through empowerment.¹⁸ Thus, we have our final proposition.

Proposition 4. *When empowerment only allows attorneys to better identify panelists’ predispositions, and the defense attorney is more skilled,*

(i) *the probability that the seated juror will be non-white is lower, and*

(ii) *the probability that the seated juror will be predisposed towards the defense is higher*

than if the juror were randomly drawn from the panel.

2.8 Summary

Comparing Propositions 2 through 4 to Proposition 1, the model produces a clear and intuitive testable implication. When the defense attorney is relatively more skilled than the prosecutor, attorney empowerment in voir dire is associated with an increase in the probability that the seated juror will be predisposed to the defense. Predictions concerning the race of the seated juror are less clear. If the nature of attorney empowerment is only such that it improves the striking ability of attorneys, empowerment will be associated with an increase in the probability that the seated juror will be non-white. In direct contrast, if the nature of attorney empowerment is only such that it allows attorneys to learn more about juror predispositions, empowerment will be associated with a *decrease* in the probability that the seated juror will be non-white. And, therefore, if both mechanisms are operative, empowerment could be associated with either an increase or a decrease in the probability that the seated juror will be non-white. Since the precise nature of attorney

¹⁷The less skilled prosecutor does not observe the signal, as per our assumption stated earlier, but rather “agrees” with the defense attorney in these cases by virtue of his usual strategy of only attempting to strike non-whites.

¹⁸For the case in which Condition 1 does not hold and the defense attorney’s reservation striking threshold is greater than δ_{wH} but less than δ_{nL} , the results collapse to those when attorneys are unempowered. The defense attorney will attempt to strike any white panelist (and argue against attempted strikes by the prosecutor of any non-white panelist), regardless of the signals exhibited by the panelists. The probability that any given panelist will be successfully struck is β , and therefore, the probability that the seated juror will be non-white is α and the probability that the seated juror will be predisposed towards the defense is p . We consider this as somewhat of a degenerate case: empowerment only provides information, but so little information that it is as though attorneys are not empowered at all. We therefore group this case together with the baseline case in which attorneys are not empowered.

empowerment and of any information uncovered by attorneys in voir dire will be unobserved to us in our empirical analysis, we will not be able to test these predictions concerning race directly. Instead, our approach will be to attempt to draw inferences about the nature of empowerment from our results. Before presenting these results, we will describe our dataset.

3 The NCSC/ICPSR Hung Juries Dataset

Our empirical analysis relies on a detailed examination of 351 felony jury trials in four major county courts across the U.S. collected by Hannaford-Agor et al. (2002, 2003) for the National Center for State Courts (NCSC) during 2000 and 2001 and disseminated by the Interuniversity Consortium for Political and Social Research (ICPSR). Although the NCSC's main goal in the study was to provide an empirical evaluation of hung juries, the dataset is not limited to those trials ending in a hung jury, and indeed includes all felony trials held in these courts during the specified period of data collection.¹⁹ The four courts – Los Angeles County Superior Court in California, Maricopa County Superior Court in Arizona, Bronx County Supreme Court in New York, and District of Columbia Superior Court in Washington, D.C. – were chosen based on their high volume of felony jury trials and their willingness to cooperate with the data collection process and guidelines. These are all state trial courts of general jurisdiction and first instance (as opposed to appellate courts). Each court is fairly evenly represented in the dataset, with a single location accounting for about 85 trials each.²⁰

The NCSC study provides a comprehensive look at the trial and all the parties involved: the defendant(s) and the victim(s) (if any), the judge, the attorneys, and, most importantly for our analysis, the seated jurors. Unfortunately, we do not have information about the initial jury pool from which the seated jury was formed, except for the initial panel size.²¹

The case data in the NCSC study provide researchers with an extensive look at all the charges, the race and sex of the defendant(s) and victim(s), and the voir dire process that led to the seated jury. The NCSC questionnaires also asked the presiding judge of each trial his/her evaluation of the evidence, case complexity, attorney skills, and the likely verdict. The attorneys in each trial were asked about several of the same trial aspects, and about their legal experience and basic demographic information, including sex and age. The juror questionnaire provides a rich set of demographic information about each seated juror, including age, sex, race, education, income, and religious beliefs. The NCSC data also contain a description of the dynamics of each juror's opinion formation, and of pre- and post-deliberation perceptions and opinions regarding the defendant(s)

¹⁹The period of data collection varied by county, ranging from 4 to 11 months.

²⁰D.C. has a slightly larger representation, with about 97 cases.

²¹We can, however, infer the average characteristics of the panel from those of the county as a whole, since a panel is essentially a random sample of the county population. We include county fixed effects in all of our empirical specifications, in part to account for the composition of the panel as much as possible.

and victim(s). To our knowledge, the richness of the jury demographic and opinion information available in the NCSC study is unmatched by any other readily available dataset.

Regrettably, although the NCSC dataset contains information on 351 cases and 3,497 jurors in total, not every individual in our data answered every survey question. In our main specifications, which use responses from the case, judge, and juror modules of the dataset, we have data on 1,846 jurors across 251 trials. The attorney module has the lowest response rate, and in some specifications with attorney controls we have as few as 152 trials represented.

All data were collected confidentially, and no information was made available to any of the trial participants during the trial or during any subsequent appeals.²²

3.1 Variables of Interest

Before presenting summary statistics, we will give a detailed discussion of the empirical analogues we propose for some of the important theoretical variables from the model.

3.1.1 Juror Predispositions

Each juror was asked “Before you began deliberating with your fellow jurors at the end of the trial (after all of the evidence and the judge’s instructions had been presented), which side did you favor?”, which was answered on a scale of 1 (“Strongly favored the prosecution”) to 7 (“Strongly favored the defense”). We interpret higher responses on this seven-point scale as manifesting greater doubts on the part of a juror that sufficient incriminating evidence had been presented in relation to that juror’s personal (and otherwise unobservable) standard required to convict. Thus, jurors with higher responses to this question are assumed to have entered deliberations with a general mindset more inclined towards the defense.

We believe that this variable closely resembles the concept of juror predispositions as it was conceived and employed in the construction of the model, and in addition that it is a reasonable objective that attorneys would want to target in practice. The wording of the question can encompass subtle juror leanings deriving potentially from common life experiences with the defendant, the resonance of certain pieces of evidence and argument, and so on. These are things that attorneys could conceivably foresee in jury selection and shape arguments to further cultivate during the trial.

Jurors were also asked their general opinions of the defendant and any victims outside the context of the facts of the case, as well as a series of questions concerning the evolution of their opinions during and after deliberations. We feel that these questions are less accurate representations both of our notion of juror predispositions and of traits that attorneys realistically target.

²²These were conditions of the data collection agreements. We do not observe if any of the trials in the dataset have since been retried or heard by an appeals court. Further details on the NCSC study and dataset are discussed by Lehmann and Smith (2011).

Jurors could very likely find the defendant sincere and the victim untrustworthy and less believable, yet still lean towards believing the defendant guilty. Furthermore, attorney manuals have only very recently begun to espouse the idea that attorneys should “select juries, not jurors,” and are more likely to advise that deliberations are akin to a black box, so that the surer route to success is to focus on individual jurors during jury selection, rather than attempt to predict deliberation dynamics. This traditional view goes back to Clarence Darrow, who is said to have remarked that the outcome of a trial has virtually been decided by the time the jury has been sworn in.

All juror questionnaires were distributed at the conclusion of the trial. There is therefore a potential concern that this question on the side favored prior to deliberation is an inaccurate representation of jurors’ actual states of mind at that time. For example, if the final verdict was the most salient memory for jurors at the conclusion of the trial, it is possible that they engaged subconsciously in *ex post* rationalization in answering questions regarding earlier stages. We have not been able to satisfactorily diagnose this possibility in the data. However, we find reassurance in the general care taken by the survey designers, and in the fact that this section of the survey was carefully and transparently constructed to elicit information on the sequential nature of the opinion formation of jurors over the entire trial and deliberations. For example, the survey includes several reminders to respondents to consider precise times that they changed their minds.

3.1.2 Attorney Empowerment

The main case data contains an indicator for whether the voir dire was conducted by the judge alone, the judge with questions suggested by attorneys, the judge and attorneys together, or primarily attorneys. We combine the first two and the latter two categories to form a dummy variable for whether attorneys had high participation or low participation.²³

The main case data also contains a dummy variable for whether or not there was a Supplemental Juror Questionnaire used by the attorneys.²⁴ However, there is no other information available concerning SJQs. For example, we do not observe if it was requested by one side or the other if present; we do not know if a request was made and denied if not present; we do not observe if both sides had access to the responses if present; and we do not observe any of the specific information that it contained if present.

Two important points should be mentioned concerning these two variables. First, it is implicitly assumed that each side is equally empowered by either instrument (high participation or a questionnaire) in terms of the additional information about jurors that the defense and prosecu-

²³For all 345 trials for which a response to this question is available, only six had voir dire conducted primarily by attorneys, and only 36 had questions submitted by attorneys. Anecdotal evidence suggests that questions submitted by attorneys are often ignored by judges.

²⁴To reiterate, the survey instrument is in no way related to whatever SJQs may or may not have been used in any trials, and the attorneys did not have access to any of the information from the survey instrument at any stage of the trial.

ing attorneys can obtain with them. This is in line with the assumptions of our model. There is no information available concerning which attorney was more active in questioning or how any SJQs were used, so there does not appear to be any feasible alternative to making this uniformity assumption. As in the model, however, attorney skill will be examined as a way of identifying heterogeneity across sides in the use that was made of these freedoms when they were available.

Second, it seems plausible to assume that the presence (or lack thereof) of these attorney freedoms is exogenous to other aspects of the trial. Individual judges have wide latitude in determining how the jury selection process is conducted, and most judges strictly adhere to a set of personal preferences, especially in regard to the degree of participation afforded to attorneys in questioning potential jurors. Judges can occasionally be persuaded to change policies if an attorney on either side is sufficiently aware of the judge's usual policies and submits a motion for a more extensive process than is typically allowed. This seems to be very rare in the case of attorney participation, but perhaps not unheard of in the case of supplementary questionnaires.

Without this exogeneity, our estimation would suffer a severe shortcoming. If, for example, judges always allowed high attorney participation in trials for crimes that had received extensive negative media attention, high participation could be highly correlated with jurors leaning away from the defense (based in part on preconceived notions of the trial that the voir dire did not successfully weed out). But this correlation would be driven by the fact that both high participation and unfavorable opinions of the defendant are associated with the (unobserved in the dataset) media attention. If there were some degree of endogeneity of this sort (for example, if, for some trials, a questionnaire were allowed based on the motions of a defense attorney desiring as much information as possible on a venire of potential jurors suspected of holding extreme prejudices against the defendant), the estimated effects of attorney empowerment on juror predispositions would be biased in a negative direction.

For most of our specifications, we use a single dummy variable for whether or not attorneys were empowered in voir dire. We construct this variable to take a value of 1 *either* if attorneys had high participation or if there was an SJQ in use, and to take a value of 0 if attorneys had neither freedom. However, we will also explore the robustness of our results to alternative definitions of attorney empowerment.²⁵

The dataset also contains additional information about the voir dire process. We observe the number of peremptory challenges actually exercised by each side, but not how many were available nor how this was determined. We also observe the total number of strikes for cause, but not how many suggested strikes for cause were objected to or denied. There is a dummy variable

²⁵Two small idiosyncrasies of the dataset should be mentioned briefly. All of the trials taking place in Bronx County in the dataset had high attorney participation in questioning. On the other hand, none of the trials in the dataset taking place in Maricopa had a questionnaire available. All of our specifications control for the county in which the trial took place, and we do not otherwise foresee these issues presenting problems within our empirical strategy, which is more focused on the interaction of attorney skill and empowerment.

indicating whether any Batson objections were raised during voir dire, but if there were, we do not observe how many there were, which side raised them, whether they resulted in the finding of any actual violation, or what remedies were imposed, if any. There is also a dummy variable for whether the jury was anonymous, but there is no indication of whether this anonymity was from the perspective of record-keeping only or if the jury was literally empaneled in a separate room and voir dire conducted remotely. We observe the length of time voir dire took in hours, but not whether there was a time limit in place. Finally, we observe the total size of the initial panel. We have concerns about the completeness of the information furnished by these variables and about their exogeneity, but we do make use of them in some specifications.

3.1.3 Attorney Skill

The judge and each juror were asked separate questions of the form “How skillful was the [prosecutor/defense attorney] during the trial?”, which were answered on a scale of 1 (“Not at all skillful”) to 7 (“Very skillful”).²⁶

In the reductionist spirit of the model, we would like to have an indicator for which attorney was the relatively more skilled. It would seem uncontroversial to assume that the judge would provide the most objective assessment of attorney skill, so that an attractive option would be to construct a dummy variable for whether or not the judge gave one attorney a higher skill rating than the other. Unfortunately, the judge assigned the same skill rating to both attorneys in about half the cases.

We have therefore decided to construct a hybrid skill indicator for each attorney by calculating an average of the judge’s rating on the one hand, and an average juror rating on the other hand. We then calculate a relative skill index by subtracting the hybrid skill rating for the prosecutor from the hybrid skill rating for the defense attorney. Finally, we construct a dummy variable that we refer to as ‘Defense More Skilled’ that takes a value of 1 when this relative skill index is greater than zero.

We are aware of the arbitrary nature and potential shortcomings of this measure of relative attorney skill. As we will discuss below, we do check for correlations between our relative skill index and the few objective indicators of attorney experience that we observe (namely years in legal practice, previous criminal trial experience, and age). We have smaller samples for this purpose due to the lower attorney response rates, but we find some reassuring results. We will also discuss a number of robustness checks we have attempted.

A conceptual shortcoming of any of these skill measures is that they refer to skill in the trial arena as a whole, whereas, it could be argued, our model suggests that we should be focusing on

²⁶Attorneys were also asked similar questions about their own and their opponent’s skill. We do not make use of these variables due to concerns about subjectivity and for the pragmatic reason, as mentioned earlier, that attorneys had the lowest response rates.

attorney skill in voir dire exclusively. We respond to this, first of all, by noting that skill in various aspects of litigation are likely to be highly correlated, so that one lawyer displaying more skill than another during a trial as a whole should be a good proxy for that same lawyer having shown superior skill in any individual stage of the trial. However, the more important point is that we do not necessarily want to disregard the possibility that attorney skill in argument and presentation of evidence could affect individual juror predispositions, nor that attorneys could anticipate this during jury selection. This is neither ruled out by a more general interpretation of our theoretical framework nor by our empirical measurement of predispositions discussed above.

3.1.4 Other Variables

An important control variable in our analysis is the objective strength of evidence. For each trial, the judge was asked how close the case was based on the evidence that had been presented. The question was required to be answered on a scale of 1 (“Evidence strongly favored prosecution”) to 7 (“Evidence strongly favored defense”). Two dummy variables have been created from this variable. The first represents evidence strongly favoring the defense, and corresponds to responses of 6 or 7 (versus responses of 1 through 5). The second represents evidence strongly favoring the prosecution, and corresponds to responses of 1 or 2 (versus responses of 3 through 7). The overall excluded category therefore corresponds to a close trial in terms of the evidence favoring neither side very strongly.²⁷

For juror race, we focus on the broad white/non-white dichotomous distinction that we used to illustrate the model. We recognize that the more specific black/white distinction typically receives the greatest focus in discussions of public policy, but the more salient difference across our dataset as a whole is the broader one. The non-white category is predictably heterogeneous across the four counties, but the constituent members in each case are, in general, assumed by commentators and legal practitioners to hold greater sympathies for defendants than their white counterparts.

Other juror-level characteristics we can observe include education, income, age, gender, and religiousness. These are encoded categorically in the dataset, and we have converted them to dummy variables in some cases and continuous variables in other cases.²⁸

As has been mentioned before, we include county-level fixed effects in all of our specifications. We can also control for some other trial-level variates. The dataset provides a fairly detailed breakdown of trials by type of charge, which we coarsen into broad headings of violent, property and drug crimes. We can also control for defendant race and for the race and gender of any victims if present.²⁹ Finally, we can control for whether or not the defendant had counsel provided at no

²⁷We could be more flexible by treating all seven categories distinctly. As this approach does not affect our results, we have chosen to retain the more parsimonious treatment.

²⁸See Lehmann and Smith (2011) for additional details.

²⁹We can also observe the defendant’s gender, but we do not control for this because less than 10% of trials in the dataset had a female defendant. A handful of trials in our estimation samples have multiple defendants, but always

Table 1: Summary Statistics

	Mean	Std. Dev.	N	
Juror Predisposition (1 (pros.) - 7 (def.))	3.517	2.064	1846	(jurors)
Juror Non-White	0.520	0.500	1846	
Bronx	0.842	0.365	310	
LA	0.631	0.483	472	
DC	0.507	0.500	609	
Maricopa	0.202	0.402	455	
High Participation	0.622	0.486	251	(trials)
Questionnaire	0.203	0.403	251	
Either	0.685	0.465	251	
Both	0.139	0.347	251	
Defense Skill (judge; 1-7)	5.104	1.438	251	
Defense Skill (juror average; 1-7)	4.418	1.134	251	
Pros. Skill (judge; 1-7)	4.873	1.541	251	
Pros Skill (juror average; 1-7)	5.203	1.111	251	
Defense More Skilled (0-1)	0.450	0.499	251	

Source: Authors' tabulations from the NCSC/ICPSR Hung Juries Dataset (Hannaford-Agor et al., 2003).

cost, which we interpret as a rough indicator of the defendant's financial means.

3.2 Descriptive Statistics

Table 1 presents selected summary statistics for our dataset. The sample means are calculated for the observations that will be used in our main empirical specifications.

On average, jurors in our sample were mildly predisposed towards the prosecution prior to deliberations (comparing the mean response of 3.5 to the midpoint of 4 on the seven-point scale, with 7 representing "Strongly favored the defense"). However, individual responses are dispersed widely around this sample mean, and there was wide variation within many trials as well.

Slightly more than half of the seated jurors in our sample are non-white. This ranges from 84% in the Bronx to 20% in Maricopa. These differences across counties in the racial composition of seated juries largely correspond with the different racial compositions of the populations of these counties. The non-white category is primarily composed of blacks in D.C. and Hispanics in Maricopa, with both groups accounting for a large share in the Bronx and Los Angeles.

For nearly two thirds of the trials in our sample, attorneys enjoyed high participation in voir dire. This includes all of the trials in the Bronx, compared to 27% of trials in Los Angeles and about 60% of trials in D.C. and Maricopa. Supplemental Juror Questionnaires were available much less frequently (20% of trials), and were never available in Maricopa. Just over two thirds of the

of the same race.

trials in our sample granted either privilege to attorneys, which is our main indicator of attorney empowerment.

Judges tended to give defense attorneys slightly higher skill ratings than prosecutors on average, while the average of juror ratings for the prosecutor for a given trial exceeded that for the defense attorney on average across the trials in our sample. Our main indicator of relative attorney skill suggests that the defense attorney had a relative skill advantage over the prosecutor in 45% of trials.³⁰

4 Empirical Results

As has been discussed in detail, our theoretical model yields two predictions. The first is straightforward: when attorneys are empowered and the defense attorney is relatively more skilled, seated jurors will be more likely to be predisposed towards the defense. We will first present and discuss results that confirm this prediction for the trials in our dataset. We interpret this as confirmation of the basic validity of our model.

The second prediction is less amenable to empirical verification. The model suggests that skilled and empowered defense attorneys can end up manipulating the racial composition of juries in various ways in their pursuit of jurors with inclinations towards their side. The precise direction and magnitude of the effect on racial composition depends upon the mechanisms of empowerment that are operative and on the information that the attorneys for a given trial are able to learn about potential jurors, which we cannot observe. Therefore, if we observe skill and empowerment affecting the racial composition of juries in a certain direction, we cannot view this as confirmation of this prediction. Our approach will be to first test for a racial composition effect, and then to infer from this what we can about the benefits that attorneys are able to obtain from empowerment, under the assumption that our theoretical model is correct. We will also attempt to support our inferences by examining evidence on the number of strikes exercised during voir dire.

4.1 Question 1: Can Attorneys Retain Jurors Based on Predispositions?

Our chosen indicator of juror predispositions, as discussed in the previous section, is measured on an ordinal seven-point scale. The primary empirical strategy we pursue is to use this variable in its original form within an ordered logistic framework. The main explanatory variables of interest are

³⁰This is perhaps unexpectedly low, as there is a general perception of resource-constrained District Attorneys' Offices being at a disadvantage relative to large private law firms representing defendants. Indeed, the defense attorneys in our dataset tend to be older and more experienced than their counterparts. However, over half of the trials in our sample involved a public defender (ranging from a third of trials in the Bronx to nearly three quarters in Los Angeles), and there is also a general perception of Public Defenders' Offices competing ineffectively against private law firms for talented defense attorneys. In addition, anecdotal evidence suggests that exceptional law students are attracted to District Attorneys' Offices out of law school because of the opportunity to gain trial experience early in their careers.

Table 2: Prediction #1 (Ordered Logistic Regressions)

Dep. Var. = Juror Predisposition (1 (pros.) to 7 (def.))			
	(1)	(2)	(3)
Empowered	0.4262*** (0.1560)	0.2248 (0.1995)	0.1240 (0.1723)
EmpoweredXDefenseMoreSkilled		0.4654* (0.2783)	0.7072*** (0.1685)
DefenseMoreSkilled	0.5545*** (0.1333)	0.2443 (0.2190)	
EvidenceStrongerforDefense	0.3623 (0.2957)	0.3740 (0.2986)	0.3865 (0.3030)
EvidenceStrongerforProsecution	-0.6554*** (0.1425)	-0.6554*** (0.1416)	-0.6714*** (0.1417)
JurorIncome	0.0563 (0.0980)	0.0644 (0.0983)	0.0636 (0.0986)
JurorEducation	0.3384 (0.3118)	0.3168 (0.3194)	0.3212 (0.3208)
JurorRace(Non-White/White)	0.3328*** (0.0992)	0.3335*** (0.0999)	0.3310*** (0.0999)
JurorGender(Female/Male)	0.0402 (0.0745)	0.0363 (0.0750)	0.0351 (0.0751)
JurorReligiousness	0.0292 (0.0860)	0.0115 (0.0865)	0.0039 (0.0856)
JurorAge	-0.0017 (0.0033)	-0.0019 (0.0034)	-0.0018 (0.0034)
Wald χ^2	139.55	142.59	140.33
Log-likelihood	-3376.2	-3372.9	-3374.3
# jurors	1846	1846	1846
# trials	251	251	251

Notes: Robust standard errors, clustered at the trial level, are in parentheses. All trials with non-missing data for at least one juror are included. Each equation includes controls for county, crime type, and defendant and victim characteristics, all of which have been suppressed in the table, along with the estimated ordered logistic cut points. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$, based on two-tailed z-test.

our measure of attorney empowerment, our measure of relative attorney skill, and most importantly, their interaction. We also control for as many trial-level and additional juror-level characteristics as possible. Each observation is an individual juror, and standard errors are adjusted to account for intra-trial correlation. Table 2 presents our main results.

The first column of Table 2 purposely omits the interaction between attorney empowerment and relative skill. The results suggest that attorney empowerment and the presence of a more skilled defense attorney are separately associated with large and statistically significant increases in the probability that a seated juror will be predisposed towards the defense.³¹ Taking this at

³¹The table presents the ordered logistic estimated coefficients. These are marginal effects on the cumulative log-odds ratio of a juror's response being higher on the seven-point scale than a given cut-off category. Table 3

face value for the moment, this is an unexpected result: legal scholars and commentators typically assume that greater attorney involvement in voir dire is detrimental to the defense.

The second column of Table 2 reports what the theoretical model suggests is the “correct” specification, and the results are perhaps closer to expectations. The explanatory power of the model improves with the addition of the interaction effect, and moreover, this interaction effect accounts for much of the separate effects in the first specification.

The results in the second column provide qualified confirmation of the theoretical model. They suggest that when attorneys are empowered and the defense attorney is relatively more skilled, there is a significant and substantially higher probability that seated jurors will be favorably inclined towards the defense. The combination of skill and empowerment is crucial to deliver this effect in the model: empowerment can provide both attorneys with a greater ability to strike undesirable potential jurors, but it is the combination of this with the greater skill of the defense attorney that sharpens the defense attorney’s striking ability and allows it to be directed more profitably.

The model also predicts, when taken very literally, that the defense attorney’s greater skill should have no effect on the probability that a seated juror has favoritism for the defense when attorneys are not empowered. This is the case because the model was constructed such that skill is only useful to attorneys in utilizing the benefits that come with empowerment. The results in the second column of Table 2 broadly confirm this aspect of the model in substance: the estimated coefficient on ‘Defense More Skilled’ is positive but statistically insignificant. On the other hand, it would also seem reasonable that the skill of defense attorneys could be partially manifested in influencing juror inclinations through arguments during trial, which the imprecisely-estimated positive coefficient could be capturing.

The reason that the results in the second column are only a qualified confirmation of the model lies in the construction of our measure of relative attorney skill. The excluded category of this dummy variable represents trials in which the prosecutor was strictly more skilled than the defense attorney. The theoretical model is reversible according to which attorney is assumed to have the greater skill, so the opposite prediction should hold when it is the prosecutor who is more skilled: the probability that a seated juror is predisposed towards the *defense* should be *lower* when attorneys are empowered and the prosecutor is more skilled. We would therefore expect the estimated coefficient on the main empowerment effect to be negative. Instead, the results in the second column show that this effect is positive, though not statistically different from zero. This suggests that prosecutors in our dataset are less able to leverage empowerment and a skill advantage in jury selection than their colleagues across the aisle, though potential explanations for this remain elusive.

The third column of Table 2 takes the theoretical model literally and imposes the zero effect

below displays ordinary least squares regression results for the same specifications as Table 2, which provide a more straightforward interpretation of marginal effects.

of attorney skill when attorneys are not empowered. The effect of empowerment when the defense attorney is the more skilled becomes larger and more statistically significant, while the effect when the prosecutor is the more skilled remains positive but is closer to zero.

There are some important effects associated with the control variables that should be briefly examined. These effects are nearly identical across the three specifications. First, non-white jurors have a significantly and substantially higher likelihood of being predisposed to the defense than white jurors. This corresponds with a primitive assumption of our theoretical model and with much anecdotal and empirical evidence.

Second, when the evidence is strong for the prosecution's case, seated jurors are much less likely to report having favored the defense. This is a sensible and encouraging result. On the other hand, when the evidence is strong for the defense's case, seated jurors are only somewhat more likely to report a predisposition towards the defense, and this effect is not statistically significant. This result is sensible in direction, and though somewhat weak, should be considered in the context of the purposeful intention of the judicial system to avoid false convictions, which could lead many jurors to view weak or offsetting evidence (the excluded category) as being in favor of the defendant.

Perhaps the most interesting aspect of the estimated effects of strength of evidence is the context they provide for interpreting the magnitude of the effect of the interaction between empowerment and skill. The specification in the third column suggests that the successes of skilled and empowered defense attorneys in jury selection can be substantial enough to offset the disadvantage from being faced with evidence supporting the prosecution. This is a potentially unsettling finding from the perspective of debates on the role of jury selection in the service of justice.

Controls for county, type of crime, and defendant and victim characteristics have been suppressed in the table, and for the most part are not associated with important effects. One interesting result is that, conditional on there being a victim of the alleged crime, if the victim was female, jurors were significantly less likely to report a predisposition towards the defense.

Table 3 presents ordinary least squares regression results corresponding to the specifications in Table 2. The sets of results do not differ substantively, but the latter give a better sense of magnitudes.³² In column (2), the estimated coefficient on the interaction term is 0.52, implying that, when the defense attorney is more skilled, attorney empowerment leads to an increase by about half a point on the seven-point favoritism scale for the average seated juror.³³

³²The estimated ordered logistic cut-points for the specifications in Table 2 do not indicate a great deal of non-linearity in the underlying latent favoritism variable, lending some justification to treating the raw seven-point variable as a continuous, cardinal measure.

³³Results are also similar for random effects specifications, either GLS with the dependent variable in its original form, or probit, with the dependent variable transformed to a dummy variable for favoritism ratings of 5 or higher versus 4 or lower.

Table 3: Prediction #1 (OLS Regressions)

Dep. Var. = Juror Predisposition (1 (pros.) to 7 (def.))			
	(1)	(2)	(3)
Empowered	0.5138*** (0.1687)	0.2886 (0.2123)	0.1689 (0.1831)
EmpoweredXDefenseMoreSkilled		0.5161* (0.3010)	0.7999*** (0.1829)
DefenseMoreSkilled	0.6360*** (0.1460)	0.2864 (0.2379)	
EvidenceStrongerforDefense	0.4526 (0.3080)	0.4668 (0.3071)	0.4819 (0.3106)
EvidenceStrongerforProsecution	-0.7008*** (0.1506)	-0.7017*** (0.1495)	-0.7205*** (0.1489)
JurorIncome	0.0334 (0.1045)	0.0415 (0.1047)	0.0418 (0.1051)
JurorEducation	0.3519 (0.3024)	0.3349 (0.3050)	0.3382 (0.3058)
JurorRace(Non-White/White)	0.3775*** (0.1094)	0.3803*** (0.1099)	0.3801*** (0.1101)
JurorGender(Female/Male)	0.0691 (0.0823)	0.0675 (0.0824)	0.0659 (0.0826)
JurorReligiousness	0.0246 (0.0943)	0.0099 (0.0941)	0.0011 (0.0932)
JurorAge	-0.0008 (0.0035)	-0.0010 (0.0035)	-0.0010 (0.0036)
<i>F</i>	7.95	7.67	7.80
<i>R</i> ²	0.13	0.13	0.13
# jurors	1846	1846	1846
# trials	251	251	251

Notes: Robust standard errors, clustered at the trial level, are in parentheses. All trials with non-missing data for at least one juror are included. Each equation includes a constant term and controls for county, crime type, and defendant and victim characteristics, all of which have been suppressed in the table. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$, based on two-tailed z-test.

4.1.1 Alternative Measures of Attorney Empowerment

The results presented thus far make use of our main indicator of attorney empowerment, which, as discussed previously, is a dummy variable for whether either high attorney participation or a supplementary juror questionnaire were allowed. Table 4 repeats column (1) of Table 2 with several alternatives for capturing attorney empowerment. Column (4) of Table 4 reproduces column (1) of Table 2 verbatim.

The first two columns consider the two main instruments potentially available to attorneys separately. Comparing with column (4), it appears that the effect of a supplementary questionnaire is biased by the omission of the indicator for high participation. The third column presents a

specification fully capturing the effects of the two instruments in any combination. The large and positive effect from high participation is again present, while the effect of having a questionnaire alone is positive but insignificant. The total net effect from having both instruments together is positive but not statistically different from zero.³⁴

Table 4: Alternative Measures of Attorney Empowerment (Ordered Logistic Regressions)

	Dep. Var. = Juror Predisposition (1 (pros.) to 7 (def.))				
	(1)	(2)	(3)	(4)	(5)
HighParticipation	0.4186*** (0.1611)		0.5175*** (0.1751)		
Questionnaire		-0.0221 (0.1642)	0.2587 (0.3005)		
either				0.4262*** (0.1560)	0.4517*** (0.1672)
both			-0.5055 (0.3579)		
PeremptoryStrikes(Def.)					-0.0544* (0.0287)
PeremptoryStrikes(Pros.)					-0.0021 (0.0128)
VenireSize					-0.0008 (0.0057)
LengthVoirDire(hrs)					-0.0001 (0.0125)
Wald χ^2	126.05	118.83	142.23	139.55	141.36
Log-likelihood	-3376.9	-3385.7	-3374.0	-3376.2	-3246.5
# of jurors	1846	1846	1846	1846	1775
# of trials	251	251	251	251	239

Notes: Robust standard errors, clustered at the trial level, are in parentheses. All trials with non-missing data for at least one juror are included. Each equation includes controls for attorney skill, strength of evidence, juror characteristics, county, crime type, and defendant and victim characteristics, all of which have been suppressed in the table, along with the estimated ordered logistic cut points. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$, based on two-tailed z-test.

The specification in column (4) – representing our preferred measure of empowerment – is equivalent to that in column (3) under two parameter restrictions: the high participation effect is equal to the questionnaire effect; and the effect from having both instruments is equal to the negative of the effect of having either individually. The null hypothesis that this composite parameter restriction holds cannot be rejected against the two-sided alternative.³⁵

The advantage of our chosen measure of empowerment is that it can parsimoniously yet comprehensively capture the potential that attorneys had to acquire information on jurors for any

³⁴The p -value of the two-sided test is 0.20.

³⁵The Wald statistic with two degrees of freedom is 1.99, and the associated p -value of the test is 0.37.

given trial. Table 4 at least provides no evidence of a statistical basis for preferring an alternative measure. On the other hand, the same broadly holds true for measuring empowerment with the indicator for high participation alone, though it is a less comprehensive measure.³⁶ We have repeated selected specifications replacing our chosen measure of empowerment with high participation, and have found no substantive difference in results.

Finally, column (5) of Table 4 includes other variables related to attorney empowerment along with our baseline measure. The availability of a higher number of peremptory strikes unequivocally increases attorneys' ability to strike additional potential jurors. However, we do not observe the *potential* number of peremptory strikes available, but only the number actually exercised by each side. When attorneys are able to strike the jurors they would like to for cause, they may have no need for the total number of peremptory strikes allowed them. Therefore, a lower number of peremptory strikes actually exercised could indicate a low number available and hence a low level of empowerment on the one hand, or a high level of empowerment and unexercised peremptory challenges on the other hand. In addition, judges have a high degree of discretion over the number of peremptory strikes allowed, and often increase the number based on the severity of the charge and other aspects of the case. The inclusion of these additional variables adds little explanatory power to the regression, and provides little insight in general.

4.1.2 Evaluating our Measure of Attorney Skill

As discussed in the previous section, our measure of attorney skill is based on an index of relative skill constructed from survey questions asked of judges and jurors. We first argue that this index captures relevant aspects of actual attorney skill. Table 5 shows the results of linear regressions of the skill index on the few attorney characteristics for which data are available.³⁷

Previous legal experience of the defense attorney is associated with a greater relative skill rating. This effect is statistically significant but small. Previous legal experience of the prosecuting attorney has an offsetting negative effect, as would be expected, but this is small and not statistically significant. In addition, specific criminal experience for either attorney is not associated with an important effect on relative skill. On the other hand, there is an overwhelming positive and significant effect on the skill index when the defense attorney is a woman. We interpret this as potentially embodying a selection effect: female defense attorneys entrusted with lead counsel duties are likely to be more highly skilled and talented than their male counterparts if they suffer statistical discrimination throughout their career.³⁸ Thus, our chosen measure of relative attorney

³⁶The specification in column (3) collapses to that in column (1) under the restriction that the coefficients for 'Questionnaire' and 'both' are jointly equal to zero. The test of this null hypothesis against a two-sided alternative yields a Wald statistic with two degrees of freedom of 2.37 and a p -value of 0.31.

³⁷Each observation in the regressions in Table 5 is a trial, because all variables vary only at the trial level. As mentioned previously, attorney response rates were low compared to other parties, so the number of trials represented in this table is lower than in others.

³⁸See Lehmann (2011) for a discussion of this effect for blacks in large law firms.

Table 5: Determinants of Attorney Skill (OLS Regressions)

Dep. Var. = Index of Attorney Skill (def. rating minus pros. rating)			
	(1)	(2)	(3)
YearsPracticeDefense	0.0422*** (0.0156)	0.0405** (0.0185)	0.0380** (0.0175)
YearsPracticeProsec	-0.0117 (0.0261)	-0.0143 (0.0280)	-0.0112 (0.0239)
PrevCrimTrialDef	-0.0006 (0.0010)	-0.0005 (0.0011)	
PrevCrimTrialProsec	0.0007 (0.0027)	0.0011 (0.0026)	
AgeDefense	-0.0136 (0.0114)	-0.0097 (0.0111)	-0.0103 (0.0109)
AgeProsec	0.0057 (0.0185)	0.0066 (0.0190)	0.0089 (0.0187)
GenderDef(Female/Male)	0.4709** (0.2006)	0.5241** (0.2510)	0.5295** (0.2473)
GenderProsec(Female/Male)	-0.1504 (0.1971)	-0.0943 (0.2145)	-0.1196 (0.2020)
Controls	No	Yes	Yes
F	1.80	1.71	1.83
R^2	0.06	0.15	0.14
# of trials	174	152	152

Notes: Robust standard errors are in parentheses. Each equation includes a constant term, which has been suppressed in the table. In addition, the second and third columns include controls for county, crime type, and selected defendant and victim characteristics, which have also been suppressed in the table. See the text for a description of the dependent variable. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$, based on two-tailed z-test.

skill appears to be a valid indicator of at least some objective aspects of attorney skill.

We have attempted to gauge the robustness of our main results on juror predispositions to alternative treatments of attorney skill, with limited success. The primary concern with our measure of relative attorney skill, notwithstanding the results of Table 5, is that it is partially based on juror responses. Some jurors could have a tendency to report a predisposition towards the attorney they found to be skilled and vice versa, which would introduce simultaneity between our dependent and explanatory variables.³⁹ Even though we are most focused on the effect of the interaction between skill and empowerment on juror leanings rather than the direct skill effect itself, this potential simultaneity problem is one that we would like to address. The issue that we have encountered

³⁹The same effects appearing in Table 5 also appear in specifications focusing on the separate ratings for the defense or prosecution by judges and jurors. In addition, some specifications examining skill ratings reported by jurors indicate that higher juror religiousness is associated with a greater likelihood of giving the prosecutor a high skill rating. Other juror characteristics – most notably race – are not significantly correlated with the skill ratings given to either attorney. Since juror predispositions are correlated with race but not religiousness in our data, this suggests that the skill ratings given by jurors are not necessarily biased by their predispositions.

in attempting to do so is that the interaction effect becomes very imprecisely estimated when the estimating sample changes.

The results in Table 5 suggest a natural instrumental variables approach to diagnosing and correcting for any simultaneity between juror predispositions and our measure of attorney skill. Attorney experience and gender are correlated with the skill ratings of attorneys, but should be exogenous to the way that jurors responded to their surveys, making these variables good candidates to be used as instruments for our measure of skill in regressions explaining the predispositions of seated jurors. Two problems are encountered when attempting to implement such an IV strategy: the sample size is smaller because the number of trials for which attorney characteristics are available is lower than for other variables; and these attorney characteristics are not strong instruments for the attorney skill measure. Both of these problems lead to imprecisely-estimated coefficients, to such a degree that statistical comparisons with our main results on predispositions are not meaningful.

We have also attempted to replace our main skill measure with one based only on the skill ratings given by the judge, dropping all trials for which the attorneys received the same skill rating. This results in the loss of about half of the sample, and again, the coefficients are imprecisely estimated as a consequence. Modified Hausman tests against our main specifications indicate no statistical difference, but this is driven by the particularly wide confidence interval for the estimated interaction effect between empowerment and skill in the alternative specifications.

A less extreme variation on this is to base the indicator for a more highly skilled defense attorney on a skill index that varies by juror, with the jury component of the skill index calculated using only the responses of the other jurors from the same trial. This causes a small reduction in the sample size because some trials have just a single juror response. The results are not statistically or qualitatively different from our main results on juror predispositions, but are less precisely estimated.

Finally, we have employed measures of absolute attorney skill in some specifications in order to broaden our focus beyond simple skill asymmetries. The results suggest that empowerment increases the likelihood that a seated juror will be predisposed to the defense to a greater degree when the defense attorney has a higher skill rating from the judge. The effects of the prosecutor's skill rating are very imprecisely estimated, and more generally, these specifications have lower explanatory power than our main specifications. This is perhaps an indication that the attorney skill ratings, measured on a subjective seven-point scale, are more applicable for making relative comparisons within a trial.

4.1.3 Summary

We have discussed evidence that the more highly skilled attorney can indeed leverage empowerment in *voir dire* to retain jurors based on their predispositions. This result matches the main prediction

of our theoretical model.

Our results have been shown to be moderately robust to different specifications and alternative measures of attorney empowerment. For attorney skill, it may perhaps be more accurate to say that we are simply not able to convincingly demonstrate robustness or non-robustness given our data.

We now proceed to examine further predictions of our model, retaining our preferred measures of attorney empowerment and skill.

4.2 Question 2: Do Attorneys Retain Jurors Based on Race?

Table 6 examines the effect of attorney empowerment and skill on the probability that a seated juror is non-white. Our treatment of the two is the same as in Table 2: column (1) includes the main effects only; column (2) introduces their interaction in addition; and column (3) retains the interaction effect but restricts the main skill effect to be zero. The dependent variable is a dummy variable, with whites as the excluded category. All specifications include several trial-level controls.⁴⁰ Each observation is an individual juror, and standard errors are adjusted to account for intra-trial correlation. We have chosen the logistic functional form for the results we will focus on.⁴¹

The main message of Table 6 is that there is no effect of attorney empowerment in *voir dire* on the racial composition of seated juries, regardless of attorney skill. The estimated logit coefficients for the main effects and the interaction term are small in magnitude and statistically insignificant. Instead, the county fixed effects account for much of the explanatory power of the regressions. That is, whatever the attorneys accomplish in jury selection regarding the leanings of the jurors they retain, the racial composition of the seated jury is driven almost entirely by the demographics of the jury pool and other county-level effects.⁴²

The table presents coefficients rather than marginal effects. The estimated marginal effect of attorney empowerment on the probability of a seated juror being non-white from the specification in column (2) – averaged over the estimating sample, with the defense as the more highly skilled attorney and other variables unchanged – is 1.7 percentage points, with a 95% confidence interval of -4.3 percentage points to 7.7 percentage points. On a twelve-person jury, this amounts to the exclusion or inclusion of less than one additional non-white juror. In contrast, moving a trial from L.A. County (with a 50% non-white population) to Bronx County (with a 90% non-white

⁴⁰Juror characteristics have been omitted from these regressions to avoid simultaneity with the dependent variable. This accounts for the larger sample size compared to Table 2

⁴¹This functional form assumption does not appear to be important. Results are similar for linear specifications and for probit specifications with or without random effects. Further, multinomial logistic results do not indicate important differences for Blacks and Hispanics, the two primary non-white racial groups in the data.

⁴²These conclusions remain true when considering the alternative measures of attorney empowerment explored in the previous section.

Table 6: Prediction #2 (Logistic Regressions)

Dep. Var. = Juror Race (Non-White / White)	Prediction #2 (Logistic Regressions)		
	(1)	(2)	(3)
Empowered	0.1067 (0.1166)	0.1227 (0.1596)	0.1420 (0.1334)
EmpoweredXDefenseMoreSkilled		-0.0374 (0.2102)	-0.0843 (0.1297)
DefenseMoreSkilled	-0.0715 (0.1033)	-0.0468 (0.1672)	
LA	-1.1702*** (0.1839)	-1.1715*** (0.1843)	-1.1713*** (0.1843)
Maricopa	-3.1249*** (0.1996)	-3.1284*** (0.2030)	-3.1311*** (0.2026)
DC	-1.4966*** (0.1689)	-1.4974*** (0.1694)	-1.4948*** (0.1696)
Wald χ^2	339.13	345.83	340.73
Log-likelihood	-1409.8	-1409.8	-1409.9
# of jurors	2410	2410	2410
# of trials	282	282	282

Notes: Robust standard errors, clustered at the trial level, are in parentheses. All trials with non-missing data for at least one juror are included. Each equation includes a constant term and controls for crime type and defendant and victim characteristics, all of which have been suppressed in the table. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$, based on two-tailed z-test.

population) would increase the estimated probability of a seated juror being non-white by 21.3 percentage points on average, or 2.5 non-white jurors on a twelve-person jury.⁴³

Our theoretical model does not predict a zero effect on racial composition of seated juries. Rather, it predicts an effect – potentially large – in either direction depending on how attorneys leverage empowerment. If empowerment is effective simply by allowing attorneys to find convincing grounds to have jurors dismissed for cause, a more highly skilled defense attorney will strike more white panelists than otherwise. On the other hand, if empowerment provides attorneys with strong information regarding juror leanings (in addition to increasing striking ability or not), a more highly skilled defense attorney could choose to retain only those jurors appearing to be predisposed to the defense, regardless of race, and thus end up with a higher proportion of whites on the seated jury than would otherwise be the case. In the context of these predictions, we interpret our empirical results as indicating a mix of these various empowerment effects across the trials in our sample.

⁴³The change in racial composition due to empowerment, though small in a practical sense, may nonetheless be important for trial outcomes. The findings of Lehmann and Smith (2011), however, do not suggest that this is the case: using the same data as have been employed here, a ten percentage point increase in the proportion of blacks on seated juries was found to be about one quarter as effective at reducing conviction rates as increases in the strength of evidence for the defense.

That is, in at least some of the trials, attorneys were able to leverage empowerment to learn valuable information about jurors that would otherwise remain unobserved.

4.3 Supporting Evidence from the Number of Panelists Struck

Corollary predictions concerning the number of strikes exercised during voir dire can also be extracted from our model. As with predictions about race, these predictions about number of strikes are dependent upon the specific functions of empowerment. For example, when empowerment only increases attorney striking ability, Assumption 2 implies that more panelists will be struck in expectation, as both attorneys will have a greater striking success rate than otherwise. If, instead, empowerment only provides information on juror predispositions and the defense attorney chooses to strike white panelists exhibiting the low signal only, there will be fewer panelists struck in expectation, as no white panelists exhibiting the high signal will be struck.

Table 7 examines the effect of attorney empowerment and skill on the number of strikes exercised during voir dire. The same specifications as shown in Table 6 for race are employed, with the inclusion of additional controls for other aspects of the voir dire process. The dependent variable is the total number of strikes exercised, including all strikes for cause and the number of peremptory strikes exercised by each side.⁴⁴ Each observation is a trial, and the estimation procedure is ordinary least squares.

As with race, we find little significant effect of empowerment, skill, or their interaction on the number of strikes exercised during voir dire. The results in column (2) suggest that defense attorneys strike slightly fewer panelists when they have a skill advantage compared to prosecutors. However, neither the main empowerment effect (representing cases in which the prosecutor was relatively more skilled) nor the interaction with the indicator for the defense attorney being more skilled is statistically different from zero.⁴⁵ Both coefficients are estimated imprecisely, so that fairly substantial effects in either direction cannot be ruled out. Most of the explanatory power of the regressions comes from the county fixed effects: judge tendencies and state guidelines generally lead to fewer potential peremptory challenges being available to attorneys in Los Angeles and Maricopa compared to the other counties.

In principle, we could attempt to better characterize how the attorneys in our dataset leveraged empowerment in voir dire by examining the model's joint predictions concerning race and number of strikes, and comparing them with our empirical results. For example, if we had found that

⁴⁴Strikes for cause outnumber total peremptory strikes for many trials, but for some cases, the number of strikes for cause is reported as zero. We suspect that these are cases for which the entire venire was examined for cause out of the presence of court clerks, before the sequential exercise of peremptory challenges was begun (and also that the panel size reported for these trials refers to the size of the remaining venire after strikes for cause). We caution that the data on strikes for cause are therefore likely to be incomplete. However, results are similar when each of these three categories of strikes is examined individually, so we err on the side of using the more comprehensive dependent variable.

⁴⁵Indeed, the coefficients are jointly insignificant ($p=0.51$) and not statistically different from one another ($p=0.25$).

Table 7: Number of Strikes (OLS Regressions)

	Dep. Var. = Total Number of Strikes		
	(1)	(2)	(3)
Empowered	0.8289 (1.5546)	2.3235 (2.1382)	2.0174 (1.7572)
EmpoweredXDefenseMoreSkilled		-3.5136 (3.3140)	-2.7861 (2.2417)
DefenseMoreSkilled	-1.7173 (1.6996)	0.7275 (2.3599)	
LA	-12.2622*** (3.8777)	-12.2266*** (3.9086)	-12.2374*** (3.8969)
Maricopa	-10.0843** (4.1083)	-10.1940** (4.1215)	-10.1859** (4.1144)
DC	1.1096 (2.9243)	1.1691 (2.9421)	1.1342 (2.9325)
<i>F</i>	15.29	14.57	15.44
<i>R</i> ²	0.45	0.45	0.45
# of trials	276	276	276

Notes: Robust standard errors are in parentheses. All trials with non-missing data for at least one juror are included. Each equation includes a constant term and controls for crime type, defendant and victim characteristics, and length of voir dire, all of which have been suppressed in the table. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$, based on two-tailed z-test.

empowerment was associated both with a lower proportion of non-whites on seated juries and with a lower number of strikes in trials for which the defense attorney had a skill advantage, we could infer that attorneys were often in the situation like the second example mentioned above: i.e. with the information aspect of empowerment dominating, and with the high signal rare enough in the population that the defense attorney attempts to retain all but white panelists exhibiting the low signal.

Unfortunately, our present results do not shed further light on the role played by empowerment in the trials in our dataset. The small and insignificant effects on both racial composition and number of strikes could indicate varying prominence of the striking ability and information functions of empowerment across trials, along with variation in the quality of the signals of juror predispositions that skilled and empowered attorneys were able to obtain. In this sense, the results for number of strikes do provide some corroborating evidence in support of the results for racial composition. A strong effect on number of strikes in either direction combined with no effect on racial composition, on average for the sample, could not be convincingly explained within our model.

4.4 Alternative Interpretations

We interpret our results as providing reasonable confirmation of our model, and of the general hypothesis that attorney empowerment in voir dire can increase the ability of attorneys both to have potential jurors removed and to learn more about them. However, there are alternative explanations for why we might see the interaction of empowerment and skill leading to an effect on juror favoritism but not on juror race.

One such explanation is that attorneys may use voir dire less for striking and learning about jurors, and more for testing and laying the groundwork for arguments to be raised during trial. Skilled attorneys afforded this opportunity could potentially be indifferent about the race and any other characteristics of the jurors hearing the case, so long as those jurors could be sufficiently groomed and prepared beginning in voir dire. If attorneys often made use of empowerment in voir dire to this effect, one might expect to see fewer strikes for trials in which attorneys were empowered. However, as discussed above, there are also cases for which our model predicts that the combination of skill and empowerment will be associated with fewer strikes than otherwise. It is therefore not clear that these alternative hypotheses about the role of empowerment could be convincingly tested against one another based on number of strikes alone. However, it can be noted that column (1) of Table 7 shows a small and positive (though highly insignificant) net effect of empowerment on number of strikes in our data, which perhaps indicates that attorneys in our dataset did not always use empowerment in voir dire simply to prepare potential jurors.

Another concern is that our measure of attorney empowerment could be capturing unobserved judge effects. For example, judges who routinely grant in-depth attorney participation in voir dire may hold subtle sympathies for defendants and defense attorneys in general, which could also be reflected in other relevant aspects of the trial, such as the instructions given to the jury, the evidence admitted, and the attorney motions allowed. Unfortunately, the data include neither judge identifiers nor even basic demographic information. We plan to address this question in the future by leveraging cross-county variation in the data to test selected comparative statics of our model. If the empirical results of this exercise correspond more with the political leanings of judges in certain counties than with the model's predictions, such judge effects may deserve more serious consideration.

Finally, it should be mentioned that few criminal charges end up going to trial and terminating with a jury verdict.⁴⁶ There are manifold factors at play determining which cases see the courtroom, from the presence of mandatory minimum sentences to chance interactions between attorneys attempting to work out a plea bargain. It is unclear how we might meaningfully assess which of many potential selection effects might be affecting our results, and to what degree. We leave a more detailed investigation of this matter for future work.

⁴⁶The figure in 2004 was about 4% of nearly 84,000 felony defendants, according to the Bureau of Justice Statistics.

5 Conclusion

This paper is the most complete attempt to date to measure what attorneys can actually accomplish, given varying degrees of empowerment in jury selection, in terms of multiple characteristics of the seated jurors they manage to retain. We have constructed a model of attorney behavior in which empowerment can operate through multiple channels, and in which the benefits of empowerment can depend on attorney skill. We have demonstrated that, in this model, skilled and empowered attorneys can successfully stack the jury with favorably-inclined jurors, but that this is equally true for the defense as for the prosecution, and further, that this is not necessarily accomplished by manipulating the racial composition of juries in predictable ways. Finally, we have verified the validity of our model empirically, and found that its main prediction holds in our sample of felony trials in four large and diverse counties.

Our results are surprising in relation to previous work and to received wisdom, but less so in light of the more realistically sophisticated attorney behavior that we allow for. The attorneys in our sample end up altering the racial composition of the seated jury relative to the jury pool very little when empowered: equivalent to no more than one juror of a given race in either direction on a twelve-person jury. The implication in the context of our model is that, for at least some of the trials in our dataset, empowerment in *voir dire* allowed attorneys to uncover valuable information about jurors and to avoid using racial stereotypes as a crutch.

An important question is how our results might generalize to other regions not represented in our dataset. History is replete with examples of black defendants being convicted by all-white juries, and with first-hand accounts of prosecutors using peremptory challenges to remove each of the few black panelists summoned for potential jury duty only to be perfunctorily sent back home. Such examples do not invalidate our model or its predictions so much as they underline the necessity of exploring its comparative statics more carefully. In some counties, blacks may have very strong predispositions towards the defense on average, while making up a very small fraction of the population and juror roll. Other counties may be very racially diverse, but have very little variation in average predispositions across racial groups. In either of these examples, the benefit from empowerment for even the most skilled defense attorney is likely to be minimal, even if empowerment yields very detailed information about the jurors. This would especially be the case if minorities are underrepresented in jury pools and typical court procedures routinely make a large enough number of peremptory challenges available that the prosecution can easily remove any minority panelists. This situation may aptly describe many small jurisdictions, particularly in the south. It cannot be handled well within our current modeling framework. We therefore suspect that the strength of our empirical results – and perhaps the broader validity of our model – will not extend outside of large and racially diverse counties such as the ones represented in our dataset, and are leery of making any claims to the contrary without substantial further work.

Notwithstanding these concerns about generalizability, however, we do think that our present work can make a small contribution to policy discussions. Many commentators have raised concerns about jury selection practices, both from the perspective of potential unfairness to the defendant and potential discrimination against minority panelists. These commentators are virtually unanimous in their calls for the abolition of peremptory strikes, which are currently employed by the American justice system to a much greater degree than anywhere else in the world.⁴⁷ We submit that peremptory challenges are something of a red herring in these discussions. Specifically, our results suggest that inequality in attorney skill may be of equal importance – where this can be interpreted broadly as inequality in the resources and support available to each side for a given trial.

It would seem that collecting greater amounts of juror information should only aid in accomplishing the intended aim of *voir dire* – literally, to induce panelists to “speak the truth” about their biases and ability to be impartial. However, if one side lacks the skill or resources to interpret such information, this opens the door for opposing counsel to abuse this advantage by, far from seeking impartial jurors, seeking the most favorably-inclined jurors that can be slipped past the other side. Gobert et al. (2009) have suggested that large courthouses should employ analysts that both sides for a given trial can have equal access to in gaining assistance with jury selection. This seems like a worthwhile proposal to explore further, in that it could preserve the benefits of collecting a large amount of information from panelists, while leveling the playing field in how attorneys are able to use this information. On the other hand, if this only aided attorneys in arguing in greater detail over semantic points rather than in actually learning about the panelists, the time and resource costs of the court would perhaps need to be given greater consideration. Again, however, our results suggest that attorneys in our dataset utilized empowerment to acquire useful information rather than just to bolster striking success in at least some cases.

We have focused on juror race for the majority of the paper, but it should be reiterated that the model only relies on the existence of any observable characteristic that is correlated with juror leanings. What constitutes observability to attorneys is not altogether clear, but virtually all courthouses collect rudimentary information on panelists’ occupations and employment. Therefore, most basic sociodemographic information should be at least partially observable. Lehmann and Smith (2011) find a relation between average jury income and propensity to convict, which thus seems like an obvious characteristic to incorporate in our analysis, though early indications are that there is not a great correlation between individual juror income and predispositions in our data. More immediately, we plan to continue generalizing our model, and to explore its comparative statics theoretically and empirically.

In a sense, the present paper can be thought of as one step towards theoretically linking the empirical strategies of Anwar, Bayer and Hjalmarsson (2011) and Lehmann and Smith (2011). The former document a correlation between the composition of *jury pools* and verdicts; and the

⁴⁷See, for example, Hoffman (2006).

latter document a correlation between the composition of *seated juries* and verdicts. The present paper is about how jury selection transforms jury pools into seated juries. Can attorneys cause the distributions by race and predispositions of people on seated juries to differ from those of people in the corresponding starting jury pools? The second step would be to go from the distributions of race and predispositions on seated juries to the verdicts that those seated juries deliver, which would entail an examination of group dynamics in jury deliberations. As an extreme example, the work of Sunstein (2000) suggests that a jury with only a few members exhibiting weak pre-deliberation leanings towards conviction could lead to “group polarization” and a unanimous decision to convict under various deliberation conditions. We found indications that group decision-making effects were at play in Lehmann and Smith (2011), and their potential to arise is also discussed by Anwar, Bayer and Hjalmarsson (2011) in interpreting their results. Bowers et al. (2001) provide a valuable catalogue of types of racial interaction that occurred during deliberations in their sample of capital trials. We also plan to address this question of deliberation dynamics more carefully in future work as part of our overall project on jury selection.

The interest amongst economists in jury selection and legal findings, though for the most part very recent, should perhaps not be surprising. The legal system presents a high-stakes environment in which a number of individuals are making constrained choices, sometimes with strategic considerations. Such an environment provides a wide array of interesting questions to economists and decision scientists. Unfortunately – and unexpectedly given the volume of legal activity passing through courtrooms on a daily basis⁴⁸ – data that would enable such questions to be satisfactorily addressed are for the most part lacking. The few econometric studies discussed in the introduction have all made use of customized self-collected datasets of one sort or another. The dataset that we have employed is unique and of great value in the richness of the information it contains, but the number of trials it covers leaves something to be desired. The more general point is that growth in this part of the literature is currently severely constrained by data availability, not by any dearth of interesting questions remaining to be answered.

⁴⁸Mize et al. (2007) estimate that state courts alone conduct nearly 150,000 jury trials per year, despite a steady increase in the frequency of various non-trial dispositions – itself an interesting phenomenon.

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