

Social Interactions and the Content of Legal Opinions*

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Abstract

We explore the forces that determine rulings and citations within a court. Our model predicts: (1) social interactions between a judicial panel i and the authors of a prior judgement j increase the probability that i reaches the same decision as j and that i cites j as a persuasive authority; and (2) a political litigant in case i increases the probability that i cites j . Data from the English Court of Appeal confirm that panels randomly assigned to work with the authors of a prior dismissal are more likely to dismiss their appeal and cite the prior dismissal as a persuasive authority than panels without such contact. These effects are stronger when the panel is more experienced and when all of the authors of the prior judgement face the prospect of promotion.

Keywords: courts and judges, discretionary citations, social interactions.

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1 Introduction

It has been argued that, from society’s perspective, the content of a legal opinion –the clarification and expansion of the law, the identification of the legal issues involved, and the development of legal reasoning– matters more than the outcome itself, especially in appellate courts (Friedman 2006). Naturally, this claim has sparked interest in the forces that shape communication patterns between judges. So much so, that quantitative analysis of the content of legal opinions is now an important branch of empirical legal research.

Much of this literature uses citations of prior cases to examine the extent to which legal opinions adhere to precedent. Spriggs and Hansford (2001), for instance, examine whether ideological variables are associated with the probability that a US Supreme Court opinion declares it is overruling a precedent (see also Johnson 1986). In a similar vein, Benesh and Reddick (2002), Cross (2007), Corley (2009) and Lee (2004) use citations to study adherence to precedent by the US Courts of Appeals.

References to precedent are not the only possibilities, however. Under common law, judges can also refer to a statement of law in a prior case that, for one of the following reasons, is not a binding precedent: there is a material distinction between the facts of the two cases, the prior case was decided in a court whose opinions are not binding, or the statement of law was *obiter* (by the way) rather than part of the ratio of the case. Specifically, this reference can explain that the statement of law in the prior case has been applied as a ‘persuasive authority’, simply mention that the panel is aware of the prior case, or even explain that the statement of law has not been applied to a new set of facts.

Such *discretionary citations* are worthy of study for two reasons. First, discretionary citations can shape the evolution of case law.¹ This could be via the cited case, as the citation both expands the set of circumstances to which the legal principle in the cited case applies and reduces the likelihood

¹Reflecting this fact, an annual compendium (The Digest 2009) has been recording and categorizing the discretionary citations received by cases in the English Superior Courts since the 16th century.

that the cited case will later be set aside as ‘bad law’.² Or, as we document below, this could be via the citing case, as the incentive to cite a prior opinion as a persuasive authority biases the ruling at hand. Second, discretionary citations are worthy of study because they could influence judicial appointments. In England, for instance, promotion decisions are based on subjective assessments by senior judges. Discretionary citations may shape these assessments directly, as the use of a judge’s opinion as a ‘persuasive authority’ is taken as evidence of quality, and indirectly, by increasing visibility in legal journals.³ In the US, the judiciary plays less of a role in appointment decisions but there have been calls for promotions to be based on counts of citations (Choi and Gulati 2004a, 2004b).

This paper contributes to work on judicial communication by exploring the social and political forces that determine discretionary citations and their associated rulings. In Section 3, we develop a theoretical model in which a judicial panel decides on a ruling in case i and whether/how to cite some previous case j in its published opinion. In this model, citing case j has two advantages: it reduces the probability of a legal challenge in case i and also yields an *extra-legal* benefit from granting a favor to the judges in case j . Citing case j as a persuasive authority strengthens both effects but is only possible if the rulings in the two cases are aligned.

Our theoretical results run as follows. If the panel views the judgement in case j as sound, then the panel’s ruling is unbiased and case j is cited as a persuasive authority only if the cost of issuing this citation is low enough. However, if the panel regards the judgement in case j to be unsound and the cost of issuing citations is sufficiently low, then the panel’s ruling is biased by its desire to reap the benefits of citing case j as a persuasive authority. Comparative statics exercises show that: (1) higher extra-legal benefit increases both the probability that cases i and j share the same ruling and the probability that case i cites case j as a persuasive authority; and (2) the anticipation of a more damaging legal challenge

²Zander (2004), a leading English law text, discusses the weight given to precedent and argues that an important consideration is whether the decision in the prior case has been applied to new factual situations by subsequent cases.

³Table A1 shows that a case heard in the Civil Division of the English Court of Appeal is more likely to be discussed in a legal journal if it has recently received a discretionary citation, even after controlling for unobserved case fixed effects.

has an ambiguous effect on the probability of taking the same ruling but unambiguously increases the probability of citing case j .

We test these predictions using data from the Civil Division of the English Court of Appeal. As we discuss in Section 2, an important institutional feature of this court is that the allocation of appellate judges to panels is effectively random. A further feature is that discretionary citations fall into two broad categories: “applied” (as a persuasive authority) and “considered”. Our empirical strategy, set out in Section 4, exploits both of these features. Using a choice-based sample of ij pairs, we estimate binary response regression models of the probability that the panel in case i : (i) cites case j ; (ii) cites case j as “applied” rather than “considered”; and (iii) takes the same ruling as case j . Our first independent variable of interest is a proxy for the extra-legal benefit that the panel derives from citing case j as a persuasive authority. We construct this variable using *on-the-job* interactions. That is, following the sociological literature, we assume that extra-legal benefit is higher when a panel member has recently worked with a judge from case j on a third, unrelated case. Our second independent variable of interest is a proxy for the panel’s perception of the damage from a potential legal challenge. This variable is constructed using the political status of the litigants in case i , the idea being that the panel is likely to view an appeal to the House of Lords as more damaging (to, e.g., promotion prospects) if the government is a party to the case.

Our empirical results, set out in Section 5, are consistent with Prediction 1. We find that English appellate judges randomly assigned to work with the authors of a given opinion are substantially more likely to take the same ruling as those authors *and* cite their opinion as a persuasive authority than judges without such an interaction. Both effects are stronger when the judges in case i are more experienced, and when more of the judges hearing case j face the prospect of promotion. We interpret these findings as evidence that, after an interaction, the panel derives greater utility from enhancing the promotion prospects of the judges in case j . We refer to this mechanism as a strengthening of social preferences. Our results do not support Prediction 2, as we fail to reject the null hypothesis that the

marginal effect of a government litigant is zero in almost every regression. That said, data limitations warn against a conclusion that panel behavior is *not* influenced by political features of the case. We conclude Section 5 with a discussion of the policy implications of these results.

Related Literature There is now a substantial literature studying judicial citations.⁴ Although the possibility that favoritism between judges based on friendship or reciprocity might affect citation behavior has been noted in earlier work (Posner 2000, Solum 2005), to the best of our knowledge this notion has not been investigated empirically. The most closely related empirical paper is Choi and Gulati (2008) who study citations from outside courts in the US federal circuit and show that judges appointed by Republican presidents cite other Republican-appointed judges more often than Democrat-appointed judges, especially in ‘high stakes’ cases. One explanation for this finding is that judges are prone to cite colleagues with a similar ideology, and ideological similarities are more likely within political parties of appointment than across them. Another explanation is that judges are prone to cite colleagues with whom they interact, and interaction is more likely within political parties of appointment. Unfortunately, in Choi and Gulati’s setting, it is not possible to disentangle these effects. In contrast, our identification strategy based on the random allocation of appellate judges to panels in the Court of Appeal is designed to isolate the role of interactions from the role of ideology.⁵

Our paper is also related to the burgeoning literature on the existence of panel effects in the US circuit courts, whereby judges’ votes depend partly on the identity of the other members in the panel (Farhand and Wawro 2004, Fischmann 2006, Sunstein et al. 2004). While these studies focus on voting

⁴In addition to the work on adherence to precedent mentioned above, case-level citation analysis has been used to study the existence of shifts in legal doctrines (Cross and Smith 2006, Cross and Lindquist 2009), the importance of ideology (Abramowicz and Tiller 2005, Clark and Lauderdale 2009), as well as the extent to which cases are influential and well-grounded in previous jurisprudence (Fowler and Jeon 2008, Fowler et al. 2007). The use of citations to measure the influence of individual judges (Kosma 1998, Landes et al. 1998) and courts (Posner 2000) is also well established.

⁵In Blanes i Vidal and Leaver (2009) we explore a related question, namely whether interaction between a first instance judge and an appellate judge affects the propensity of the latter to reverse the former. Since these interactions are between judges serving at different levels of the judicial hierarchy, random allocation of appellate judges to panels in the Court of Appeal is not sufficient to identify the effect of interest. The ensuring selection problem prompts us to develop a different identification strategy based on transitions between panels.

decisions and contemporaneous panel effects, our research shows that panel effects can also occur in terms of citation behavior and be long lasting, operating beyond the case in which judges interact.

2 Institutional Background

Our analysis focuses on the Civil Division of the English Court of Appeal. In this section, we summarize the key features of, and tasks performed by, this court.

The Court of Appeal is the second most senior court in England, immediately above the High Court and below the House of Lords. It has 37 judges (Lord Justices) and the overwhelming majority of cases are heard by a panel of three judges. There are two divisions: criminal and civil. We restrict our analysis to the Civil Division. Panels in the Criminal Division are usually staffed by a Lord Justice and two High Court judges. While we have a good understanding of how judges are allocated to panels *within* courts, it is less clear what determines the allocation of justices between the High Court and the Criminal Division. In contrast, the Civil Division is overwhelmingly staffed by Lord Justices and most Lord Justices spend most of their time sitting in the Civil Division (hereafter the CA Civ). This self-contained nature of the CA Civ makes it ideal for our analysis.

Once a case has been allocated to the CA Civ and the size of the panel has been determined, the task of forming the panel falls to the CA Civ Listing Officer who is an employee of the court rather than a judge.⁶ Our conversations with a listing officer for the CA Civ suggest that the principles underlying panel formation run as follows. First, only “ticketed” judges can be chosen.⁷ Second, whenever possible, allocation follows the “cab-rank principle”. As judges dispose of their cases, they join the back of the rank and wait to receive a new case; as a case requiring a panel of size n arrives, the CA Civ Listing Officer matches it to the first n judges in the rank. Third, in the event of a tie (when more than n

⁶While the judge in charge of the division can exercise a supervisory role, our conversations with judges who have run divisions indicate that this prerogative is exercised in less than 3 percent of cases.

⁷CA Civ judges are automatically ticketed. Other judges can be ticketed at the discretion of the Head of Civil Justice.

judges join the rank at the same time), the panel is formed at random.

The main task facing a CA Civ panel is, of course, to make a ruling allowing or dismissing the appeal in the allocated case. The panel is also expected to publish its ‘opinion’, giving the reasons for its decision. Published opinions usually refer to the facts of the case, the different laws being applied, the legal issues being considered and resolved and, crucially for our purposes, citations of prior cases. In the English system, citations can be categorized along two dimensions: whether the panel has actively chosen to cite the case, and whether it agrees with the previous decision.⁸ We discuss both dimensions, starting with the issue of discretion.

When the facts of the cited case are identical to those of the citing case, the rule of precedent applies and the panel must “follow” the previous decision. If the facts are essentially the same, except for some minor but important distinction motivating a different decision, the panel can choose to “distinguish” the case from the prior case, but must still refer to it. Lastly, when the facts are identical, but the panel regards the previous decision as unsound (and is sitting in a higher court), it can choose to “overrule” the previous decision, but again must still refer to it. We categorize these citations as *precedential*. When the facts of the case do not have an obvious pre-existing equivalent, there may still be a prior case dealing with a similar legal issue. The panel can choose to cite this case, although it is not compulsory. The analysis below is based exclusively on these *discretionary* citations⁹.

If the panel agrees with the manner in which a prior case dealt with the legal issue at hand, it can highlight this fact in its published opinion (“case *x* applied”). Alternatively, the panel can highlight that there is a case that deals with a similar legal issue but not express explicit agreement with it (“case *x* is considered”). Finally, if the panel disagrees with the manner in which a prior case dealt with the legal issue, it also has the option to highlight this fact in its published opinion (“case *x* not applied”). In practice, this negative type of citation is extremely rare.

⁸See Slapper and Kelly (www.routledge.com/textbooks/9780415480963/cases.asp) and Westlaw 2007.

⁹Note that this focus on discretionary citations differentiates our approach from the model of ‘deference to precedent’ developed by Bueno de Mesquita and Stephenson (2002).

3 Theoretical Framework

3.1 The Model

There is a single player: a panel hearing a case i in the CA Civ. Our aim is to establish the forces influencing this panel’s ruling and its citation behavior towards some previous CA Civ case j . We present the basic set-up in next subsection, and then discuss key modeling choices. Discussion of empirical proxies is postponed until Section 4.

Set-up Our starting assumption is that there is a correct ruling, a “state of the world” $x \in \{0, 1\}$. For concreteness, we let $x = 0$ denote the state where the appeal should be dismissed (the first instance ruling was right) and $x = 1$ the state where the appeal should be allowed (the first instance ruling was wrong). Reflecting aggregate dismissal rates, the panel’s prior belief that $x = 0$ is denoted by $\mu > 1/2$.

The panel cannot observe x but can combine its own legal knowledge with the facts of the case to revise its prior belief. We equate this process with the generation of a private non-verifiable signal on x , $s \in \{0, 1\}$, with precision $p \in (\mu, 1)$. The panel can also consult prior cases. Since our objective is to derive predictions for a pair of cases, we focus on a particular prior case j that is known to tackle the same legal issue as case i .¹⁰ The sense in which case j might be informative about x should be obvious: observing that a panel in a previous case *tackling the same legal issue* took a particular ruling provides evidence that the same ruling should be applied in the current case. To capture this idea, we assume that the panel views the ruling in case j , $r_j \in \{0, 1\}$ as a further (verifiable) signal on x . The panel’s perception of the precision of this signal is denoted by $q \in (0, 1)$, and is its private information.

Having generated s and observed r_j , the panel (i) makes a ruling $r_i \in \{0, 1\}$ dismissing or allowing the appeal and (ii) sends a message m via its published opinion. We will say that the panel sends message $m = 0$ if it does not cite case j , message $m = 1$ if it highlights that case j has been “considered” because

¹⁰The panel is not influenced by, and makes no citation of, unrelated and/or unknown cases.

it tackles a similar legal issue, and message $m = 2$ if it highlights that case j has been “applied” as a persuasive authority.¹¹ We assume that messages must be verifiable, implying that the panel can only send $m = 2$ when taking the same ruling as case j .

After the panel has made its ruling and published its opinion, the parties may lodge a legal challenge to the House of Lords. To avoid further strategic considerations, we assume that the panel expects to see a legal challenge with (reduced form) probability ρ . The factors determining this probability of legal challenge are discussed in further detail below.

The panel incurs disutility from two sources: damage D from a legal challenge and cost C from exerting effort to cite case j in its published opinion. Set against this, is the *extra-legal* benefit B that the panel derives from casting the judges who heard case j in a positive light by including a citation of their case in its published opinion. To derive concrete predictions, we make the following functional form assumptions.

Assumption 1. The cost of effort is

$$C = \begin{cases} 0 & \text{if } m = 0 \\ c & \text{if } m = 1 \\ c(1 + \alpha) & \text{if } m = 2 \end{cases} . \quad (1)$$

The panel has a privately observed marginal cost of effort c . Sending $m = 1$ requires 1 unit of effort (to enter the citation in the correct place in the opinion, to cite the original source accurately and to clarify why the legal issues are the same), while sending $m = 2$ requires an additional α units of effort (to clarify why the previous case reaches a conclusion that lends credibility to the panel’s ruling).

¹¹We abstract from the possibility of negative discretionary citations highlighting that case j is “not applied”. In a previous version of this paper, we allow for negative citations and show that these are never optimally chosen. Furthermore, Table 1 confirms that they are rarely chosen in practice.

Assumption 2. The extra-legal benefit is

$$B = \begin{cases} 0 & \text{if } m = 0, 1 \\ b & \text{if } m = 2 \end{cases} . \quad (2)$$

The panel derives an *extra-legal* benefit by citing case j as a persuasive authority, but not from simply highlighting that case j tackles the same legal issue.

Assumption 3. Define $\gamma_{s,r_j,r_i} = \gamma_{s,r_j,r_i}(\mu, p, q)$ as the belief of a panel with signals s, r_j that ruling r_i is correct (matches x). The panel perceives the probability of legal challenge to be

$$\rho = \rho(\gamma_{s,r_j,r_i}, m) \text{ where } \frac{\partial \rho}{\partial \gamma_{s,r_j,r_i}} < 0, \frac{\partial \rho}{\partial m} < 0 \text{ and } \frac{\partial^2 \rho}{\partial \gamma_{s,r_j,r_i} \partial m} = 0. \quad (3)$$

In what follows, we will refer to γ_{s,r_j,r_i} as the panel's *confidence* in ruling r_i . The perceived likelihood of a legal challenge is lower when, *ceteris paribus*, the panel is (i) more confident in its ruling and (ii) sending a higher citation message. Both claims are intuitive and micro-founded in the game-theoretic model in Blanes i Vidal and Leaver (2006). The cross-derivative is assumed to be zero for simplicity.

Assumption 4. For any given level of confidence γ_{s,r_j,r_i} , the effort parameter satisfies

$$\alpha > \frac{[\rho(\gamma_{s,r_j,r_i}, m = 1) - \rho(\gamma_{s,r_j,r_i}, m = 2)] \cdot D + b}{[\rho(\gamma_{s,r_j,r_i}, m = 0) - \rho(\gamma_{s,r_j,r_i}, m = 1)] \cdot D}. \quad (4)$$

This is a sufficient condition to ensure the panel sometimes sends $m = 1$ (as observed in the data).

To summarize, the timing runs as follows: (1) The panel hearing case i in the CA Civ observes signals s, r_j and then takes ruling r_i and sends message m via its published opinion. (2) A legal challenge is lodged with (reduced form) probability $\rho = \rho(\gamma_{s,r_j,r_i}, m)$ and the panel's payoff is realized. It follows that the panel chooses r_i and m to maximize its expected payoff $-\rho \cdot D + B - C$.

Discussion The rationale for including D in this payoff is that: the panel members care about their reputation; and a challenge *on a point of law* before the House of Lords damages this standing. The rationale for B is that: the judges in case j care about their reputation; a citation regarding case j as a persuasive authority improves this standing; and the panel in case i benefits from granting this favor.

The promotions process is an obvious micro-foundation for why judges care about their reputations. Given the recent outcry at plans to appoint a barrister rather than an appellate judge to the new Supreme Court, it is clear that CA Civ judges care about the prospect of promotion (*The Times*, February 4 2010). There is also evidence that a judge's reputation for quality, as evidenced by appeal and citation rates, influences the prospect of promotion. According to the judiciary's representative body:

“The best testimony as to whether judges have the necessary qualities for promotion to the Court of Appeal is their track record since being appointed as judges. This, to a judicial colleague, will be demonstrated by the quality of their judgements.” The Judges' Council (2003, paragraph 99).

Consistent with the Council's claims, Salzberger and Fenn (1999) find that the percentage of a CA Civ judge's decisions subsequently reversed in the House of Lords is negatively associated with promotion to the highest rank of Law Lord.

A related micro-foundation is that judges care *directly* about their professional reputation (Posner 1993). For instance, a US Court of Appeals judge comments that some opinions are written:

“for the personal gratification that comes from being quoted, cited and republished in law school case books, with the side benefits of invitations to lecture, write in law reviews, and –not to be sneered at– attract the best and brightest law clerks.” Wald (2005, p. 1372).

To the extent that these concerns are universal, being cited as a persuasive authority might therefore increase an English CA Civ judge's utility even in the absence of promotion concerns, especially as discretionary citations, and in particular “applied” citations, are positively associated with quotations in law journals (Table A1).

We now turn to the notion that judges in case i might benefit from granting a favor to judges in case j . Given the scope for interaction between appellate judges, it seems plausible that the panel may have developed a social preference for the judges in case j and hence derive a benefit from advancing their career. More instrumentally, the panel might also anticipate a future return of the favor.

To conclude this discussion, we briefly explain why ideology does not feature in our theoretical model. In sharp contrast to the US, ideological differences are not considered to be an important driver of judicial decision-making in the English context. For example, a leading text on the English legal system notes that:

“It is not easy to characterize individual judges in the general terms often used by political scientists in other countries, and it is not useful to look for broad class-based ideological affinities. Indeed, neither we, nor usually the judges themselves, are likely ever to be able to be very clear about what drives their decisions in easily generalizable ways.” Robertson (1998, p. 17).

Griffith (1997, p. 336) concurs that ideological differences across judges are minor and argues that “as a group judges have one main bias: preserving the status quo” (cited in Zander 2004, p. 360). There are several reasons why ideology plays a more limited role in the English system, notably the more limited role of politicians in the appointments process, and the fact that the English judiciary is remarkably homogeneous in terms of gender, race, and social/educational background (see, e.g., The Sutton Trust 2005). For simplicity, we therefore abstract from ideological considerations in our theoretical and empirical models.

3.2 Analysis

For brevity, we focus on a panel with conflicting signals $s = 1, r_j = 0$; the solutions to the other three cases are easy to establish once this analysis is in place.

The panel’s own signal is evidence in favor of $r_i = 1$. Consequently, if the perceived precision of r_j is sufficiently low, the panel believes that $r_i = 1$ is more likely to be correct than $r_i = 0$ (and vice versa

if q is sufficiently high). Formally, applying Bayes' rule

$$\gamma_{1,0,1} = \frac{p(1-q)(1-\mu)}{(1-p)q\mu + p(1-q)(1-\mu)} = 1 - \gamma_{1,0,0}. \quad (5)$$

Since $\gamma_{1,0,1}$ is decreasing in q , there is a critical value of the precision parameter, $q^* \in (\mu, p)$, such that $\gamma_{1,0,1} = \gamma_{1,0,0}$. To maximize the probability of a correct decision, the panel should choose $r_i = r_j$ iff $q \geq q^*$. In what follows, we will say that there is *biased decision-making* if the panel chooses $r_i = r_j$ when $q < q^*$. Note that, from the perspective of judicial communication (i.e. net of effort costs), the panel should send $m = 2$ iff $q \geq q^*$.

Of course, to maximize its expected payoff the panel must also consider the effect of citing case j . Our first result establishes that this consideration can result in biased decision-making. To state this result, let \tilde{q} , \underline{c} and \bar{c} respectively denote the unique solutions to

$$[\rho(\gamma_{1,0,1}, m = 1) - \rho(\gamma_{1,0,0}, m = 2)] \cdot D + b - \alpha c = 0, \quad (6)$$

$$[\rho(\gamma_{1,0,0}, m = 1) - \rho(\gamma_{1,0,0}, m = 2)] \cdot D + b - \alpha c = 0 \quad (7)$$

$$[\rho(\gamma_{1,0,r_i}, m = 0) - \rho(\gamma_{1,0,r_i}, m = 1)] \cdot D - c = 0. \quad (8)$$

If $q = \tilde{q}$, then the panel is indifferent following the ruling in case j and citing case j as a persuasive authority, and following its own signal and highlighting that case j has been considered (equation (6)). If $c = \underline{c}$, then a panel following the ruling in case j is indifferent between citing case j as a persuasive authority, and highlighting that case j has been considered (equation (7)). If $c = \bar{c}$, then a panel taking either ruling is indifferent between highlighting that case j has been considered, and making no citation (equation (8)). Note that Assumption 4 implies $\underline{c} < \bar{c}$.

Proposition 1 (Characterization of Panel Behavior). *A panel with signals $s = 1, r_j = 0$:*

- i. Takes ruling $r_i = r_j = 0$ iff $q \geq \min \{\tilde{q}, q^*\}$.*
- ii. Sends message $m = 2$ (case j cited as a persuasive authority) iff $r_i = r_j$ and $c < \underline{c}$; and message $m = 0$ (case j not cited) iff $c > \bar{c}$.*

Proposition 1 is illustrated in Figure 1 (panel behavior for different values of q and c).¹² The main insight is that the panel’s twin concerns to avoid the damage of a legal challenge, and obtain the extra-legal benefit from an “applied” citation, combine to produce biased decision-making. Specifically, in the shaded region the panel chooses $r_i = r_j$ (even though this decision is marginally less likely to be correct than $r_i = s$) because it can then buttress its argument by appealing to a persuasive authority *and* cast the judges in case j in a positive light. Our second result establishes how a strengthening of these concerns impacts upon the values \tilde{q} , \underline{c} and \bar{c} that pin down the panel’s behavior.

Proposition 2 (Comparative Statics). *For a panel with signals $s = 1, r_j = 0$:*

$$\frac{\partial \tilde{q}}{\partial b} < 0, \quad \frac{\partial \underline{c}}{\partial b} > 0, \quad \frac{\partial \bar{c}}{\partial b} = 0$$

and

$$\frac{\partial \tilde{q}}{\partial D} \begin{cases} > 0 & \text{if } b - \alpha c > 0 \\ < 0 & \text{if } b - \alpha c < 0 \end{cases}, \quad \frac{\partial \underline{c}}{\partial D} > 0, \quad \frac{\partial \bar{c}}{\partial D} > 0.$$

Proposition 2 is illustrated in Figure 2. Panel (a) depicts two effects of doubling extra-legal benefit. In Region 1 ($q > q^*$ and $\underline{c}(b = 0.25) < c < \underline{c}(b = 0.5)$), case j is persuasive but, initially, the panel is unwilling to exert the effort to highlight this fact in its published opinion. After the increase in b , the panel cites case j as a persuasive authority because the higher extra-legal benefit is sufficient to compensate for the extra effort cost. In Region 2 ($\tilde{q}(b = 0.5) < q < \min \{\tilde{q}(b = 0.25), q^*\}$) and

¹²All figures are plotted for $\rho = \frac{1}{4} [1 - \gamma_{1,0,r_i} + \frac{2-m}{4}]$ with $\rho = 0.75$, $\mu = 0.6$, and $\alpha = 4$.

$c < \underline{c}(b = 0.5)$), case j should not be persuasive but, after the increase in b , the panel switches ruling to $r_i = r_j$ and cites case j as a persuasive authority because the extra-legal benefit is sufficient to compensate for the higher effort cost and expected damage.

Panel (b) shows four effects of doubling the damage from a legal challenge. As above, in Region 1 ($q > q^*$ and $\underline{c}(D = 4) < c < \underline{c}(D = 8)$), the increase in D removes a distortion from citation behavior as the panel becomes willing to exert the effort to cite case j as a persuasive authority. Similarly, in Region 2 ($\tilde{q}(D = 8) < q < \tilde{q}(D = 4)$ and $c < \underline{c}(D = 8)$), it adds a distortion to decision-making because the panel switches ruling and cites case j as a persuasive authority. In Region 3 ($\tilde{q}(D = 4) < q < \tilde{q}(D = 8)$), q is sufficiently low so that $\rho(\gamma_{1,0,1}, m = 1) < \rho(\gamma_{1,0,0}, m = 2)$. As a result, the increase in D removes a distortion in decision-making: the panel switches ruling to $r_i = s$ and fails to cite case j as a persuasive authority because the expected damage and effort cost outweighs the extra-legal benefit. Finally, in Region 4 ($\bar{c}(D = 4) < c < \bar{c}(D = 8)$), the increase in D reduces a distortion in citation behavior as the panel becomes willing to exert effort to highlight the similarity of legal issues.

Other Signals The analysis of a panel with conflicting signals $s = 0, r_j = 1$ is similar to the above; the only substantive difference is that the analogue to q^* is *greater* than p (because the prior points towards $x = 0$). The analysis of a panel with confirmatory signals $s = r_j$ is simple: the panel takes ruling $r_i = r_j$ for all parameters since this maximizes the probability of a correct *and* facilitates citation, and sends message $m = 2$ iff $c < \underline{c}$ and $m = 0$ iff $c < \bar{c}$.

3.3 Empirical Predictions

The panel's *perception* of the precision of r_j and its effort cost are its private information and so, from the econometrician's perspective, q and c are unobserved random variables. Applying the above results, the impact of a change in B and D can therefore be stated in the following probabilistic terms.

Prediction 1. *For a panel hearing CA Civ case i , and aware of previous CA Civ case j , an increase in extra-legal benefit b from citing case j as a persuasive authority:*

- i. increases the probability that the panel takes the same ruling as case j , $Pr[r_i = r_j]$;*
- ii. has no effect on the probability that the panel cites case j , $Pr[m \neq 0]$; and*
- iii. increases the probability that, conditional on citing, the panel cites case j as a persuasive authority $Pr[m = 2|m \neq 0]$.*

Prediction 2. *For a panel hearing CA Civ case i , and aware of previous CA Civ case j , an increase in the damage from a legal challenge D increases the probability that the panel cites case j , $Pr[m \neq 0]$.*

Starting with $Pr[r_i = r_j]$, the effect of an increase in b is positive due to the probability mass associated with Region 2 in Figure 2(a), and the analogous region for a panel with signals $s = 0, r_i = 1$. The effect of an increase in D is ambiguous due to the probability mass associated with Regions 2 and 3 in Figure 2(b). Turning to $Pr[m \neq 0]$, there is no effect of an increase in b because, under Assumption 4, the panel weighs the benefit of $m = 0$ against $m = 1$ and, under Assumption 2, this trade-off is independent of b . The effect of an increase in D is positive due to the probability mass associated with Region 4 in Figure 2(a), and the analogous regions for other signal configurations. Finally, for $Pr[m = 2|m \neq 0]$, the effect of an increase in b is positive due to the probability mass associated with Regions 1 and 2 in Figure 2(a), and the analogous region(s) for other signals. The effect of an increase in D is ambiguous due to the probability mass associated with Regions 1-3 in Figure 2(b).

4 Data and Estimation

4.1 Empirical Proxies

In this subsection we motivate our choice of empirical proxies, and then explain how these variables are constructed and used to test Predictions 1 and 2.

Extra-legal Benefit We look for an indicator of the strength of the panel’s social preference for the judges in case j and (for identification purposes) choose *on-the-job* interactions; i.e., instances where a panel member sits with a judge from case j on a third, unrelated case (hereafter an ‘interaction’ case).

The sociological literature on networks emphasizes that interactions can strengthen relationships and hence change social preferences (contact theory, Allport 1954). In our setting, the claim would be that an on-the-job interaction strengthens the panel’s (positive) social preference for the judges in case j . To be plausible, this requires: (i) the panel to have a weak social preference for the judges in case j in the absence of an on-the-job interaction; and (ii) contact to foster amity rather than hostility.

On (i), one might suspect that CA Civ judges have strong relationships prior to joining the bench. It is certainly true that CA Civ judges share the same narrow background. Most individuals in our sample attended a private school, followed by an Oxford or Cambridge college and a successful career at a leading London set. However, a common background does not imply social contact. Table 3 shows that relatively few of these individuals attended the same institution *at the same time* as another CA Civ judge.¹³ One might also suspect that CA Civ judges rapidly interact with all of their colleagues once on the appellate bench but, again, Table 3 shows that this does not happen. As such, it seems plausible on-the-job interaction will strengthen social relationships.

On (ii), Moody (2001) argues that interactions occurring in a context of status equality, and especially of cooperative interdependence, are more likely to promote friendship. Since the interactions

¹³Table 3 reports the percentage of pairs where a panel member and a judge from case j overlap at an institution. The highest figure is for social clubs and, even in this case, is just 15 percent.

that we study are between judges holding the same rank and require solving a legal problem together, Moody's findings suggest that contact should create a positive social preference. That is, in the language of economics, interaction should *increase* the utility that the panel member receives from a given increase in the utility of the judge from case j .

Turning to variable construction, recall from Section 2 that CA Civ panels (and hence on-the-job interactions) are formed via the cab-rank principle. Strict application of this principle suggests that it is the duration of the previous case that determines whether or not a judge will be assigned to a particular case. Since we are interested in the interactions between judges, an important caveat applies. Suppose that three judges are already working on a case together. As they dispose of that case, the three of them will join the cab-rank at the same time. As a result, these judges are more likely to be matched with each other in the immediate future than they are to be matched with other judges dealing with other cases. In short, in steady state there will be serial correlation in on-the-job interactions *even if the initial allocation is random*.

Our response to this issue is to construct our proxy for B using on-the-job interactions in the first (Michaelmas) term of the legal year when matches are effectively random. Figure 3 displays the number of cases brought to a close on each day of the legal years 2001/2, 2002/3, 2003/4 and 2004/5. In each plot, the y-axis represents the start of Michaelmas term, the second vertical line the end of Michaelmas term, the third vertical line the start of the second legal term and so on. Notice that cases are typically decided in term-time and that there is always a large spike on the final day of the legal year. These observations suggest that the panel formation process starts afresh each legal year, making the matches in Michaelmas term an initial (random) allocation. We test this hypothesis in Section 5.1.

To avoid a mechanical correlation with the elapsed time between j and i , we focus on interactions in three windows of time. The resulting proxy for B is a categorical variable. To illustrate how it is constructed, consider an ij pair where the two panels do not contain any common judges, at least one judge from i is a contemporary of a judge in j , and there is a single interaction case. If the interaction

case falls in the first Michaelmas Term: before j the pair is assigned to the *Before* category; after j and before i the pair is assigned to the *Between* category; and after i the pair is assigned to the *Placebo* category.¹⁴ Pairs with no interaction cases in any of these windows are assigned to the *None* category, and all other observations to the *Common Judge* or residual *Other* category.

We test Prediction 1(i) by comparing the proportion of citations in the *None* category with the proportion of citations in the *Before* and *Between* categories. Given random panel formation, a statistically significant difference across categories would be evidence that on-the-job interactions influence citation behavior. To assess the validity of our identifying assumption, we also compare the proportion of citations across the *Placebo* and *None* categories. Since an interaction occurring in the first Michaelmas Term after i cannot be anticipated and hence affect citation behavior itself, a difference could only arise if interactions are correlated with some other relevant characteristic. Finding a difference here would therefore amount to a rejection of our key identifying assumption. Our tests of Prediction 1(ii) and (iii) proceed in a similar fashion.

Damage from Legal Challenge We look for an indicator of the severity of damage to promotion prospects and opt for the political status of the litigants in case i . During our sample period, the government retained *de jure* control over the procedure for promoting judges from the Court of Appeal to the House of Lords, while senior judges retained *de facto* control (Blanes i Vidal and Leaver 2011). Consistent with this, Salzberger and Fenn (1999) find that legal challenges, but not rulings against the government, lower the chances of promotion to the rank of law lord. As such, a panel will likely perceive a legal challenge to be more damaging when the government is a litigant in case i , not because it fears a reprisal for disloyalty, but because such cases are likely to attract negative visibility.¹⁵ To construct our proxy for D we assign an ij pair to the *Government Litigant* category if either the appellant or

¹⁴The same procedure applies for multiple interaction cases, providing they fall in the same term. We use two separate windows before i to allow for timing effects.

¹⁵The panel may, however, perceive that there are penalties from being disloyal. We explore this issue in Table 6 below.

respondent in case i is a representative or department of central government.¹⁶ All remaining pairs are assigned to the *Other Litigants* category. We test Prediction 2 by comparing the proportion of pairs with a citation across these categories.

4.2 Data

Sources In the English system, official reporting of cases is provided by the Incorporated Council of Law Reporting for England and Wales. Historically, this was supplemented by unofficial reporting by a number of journals, specialist publications and newspapers. Today, firms such as LEXIS-NEXIS, Westlaw UK and Lawtel UK collect reports from all of the above sources and provide them to users in a searchable format. Our dataset consists of the universe of cases between 1 January 1980 and 31 December 2005 that are available on Westlaw UK.¹⁷

An advantage of the Westlaw Case Analysis service is that it provides the history of a case, including the ruling in any subsequent appeal and citations received from subsequent cases. Since this history is reported by category, we did not play any role in the coding of our dependent variables.¹⁸ Moreover, since the service lists the panel of judges hearing each case, it was also straightforward to construct our on-the-job interaction proxy for extra-legal benefit. Unfortunately, the service does not list or categorize the parties in each case and so, to construct our proxy for the damage from a legal challenge, we were forced to search case abstracts to establish whether the appellant or respondent was a representative or department of central government.

¹⁶The representatives are the Attorney General and Lord Chancellor, and the departments are HM Customs and Excise, Inland Revenue, Foreign Office, Home Office, and Treasury.

¹⁷Westlaw UK's coverage of reported cases is practically exhaustive and overlaps significantly with that of LEXIS-NEXIS. A full description of Westlaw's sources is available at www.westlaw.co.uk.

¹⁸Citations are coded by the Westlaw UK team when the actual citation category is not explicitly mentioned in the text. The different citation categories are well established in English jurisprudence and were already in place well before the appearance of Westlaw UK. See *The Digest* (2009), a compendium of case law from the 16th century onwards, for early examples of such citations as well as formal descriptions. Obviously, the ruling always appears explicitly in the text. However, since our ruling variable is constructed from the history of the corresponding *first instance* case, and the Westlaw Case Analysis service includes some cases on appeal but not at first instance, the ruling is only available for a subset of our CA Civ cases.

Our dataset was transferred electronically from the Westlaw Case Analysis service. Although we have no reason to suspect that measurement error was generated in this process (and have verified this manually for a random sample of 200 cases), it is possible that Westlaw UK is itself contaminated by measurement error (e.g. the Westlaw UK editorial team might inadvertently mis-code a citation as ‘applied’ instead of ‘considered’).¹⁹ As long as this measurement error is idiosyncratic, the point estimates reported below should not be affected, although the standard errors would be larger.

Sample Construction While it is straightforward to identify pairs of cases where a discretionary citation occurred, it is less obvious how to identify *censored* pairs where a discretionary citation could have, but did not, occur. To tackle this issue, we construct our estimation sample by first identifying a population of pairs and then drawing a choice-based sample. To identify the population, we index the CA Civ cases in our dataset in descending chronological order by $i = 1, \dots, 8,923$. Taking case $i = 1$, we then return to the remaining cases and find every judgment within the preceding 5 years.²⁰ Repeating this exercise for $i = 2, \dots, 8,923$ cases, we obtain a population of 23,551,316 ij pairs. Next we divide this population into ‘year of citing case’ strata and, for each strata, select all uncensored pairs and then draw (with replacement) ten times as many censored pairs. Since Westlaw appeared to change its own sampling methodology in 1993 (Table A2), we focus on the 13 strata between 1993 and 2005, giving us 15,445 observations, of which 14,050 are censored and 1,405 are uncensored. As Table 1 indicates, not all of these observations are usable. Specifically, our base sample excludes 2,144 pairs with no Michaelmas term after case i , and a further 279 pairs where the citation is non-discretionary.

¹⁹When interviewed by one of the authors, the Westlaw UK editorial team commented that, while technically possible, such a coding error was unlikely. As a robustness check, we drew a random sample of citations from our dataset and compared the coding with that of LexisNexis, an alternative provider of case law. The coding decisions were consistent in almost 90 percent of cases. Spriggs and Hansford (2000) report that Shepard’s Citations is similarly reliable.

²⁰To limit attenuation bias, it would be desirable to focus on pairs that tackle the same legal issue. Unfortunately, our dataset only contains (plausibly exogenous) information on the legal subject of each case and manual checks indicate that this is a poor proxy for the legal issue.

4.3 Estimation

We test Predictions 1 and 2 using comparisons of proportions and binary response regressions models. The main regression model (in error component form) that we use to explain the likelihood of a citation for pair of cases ij is

$$Citation_{ij} = \alpha_0 + \alpha_1 B_{ij}^{\text{Before}} + \alpha_2 B_{ij}^{\text{Between}} + \alpha_3 B_{ij}^{\text{Placebo}} + \alpha_4 D_{ij} + \mathbf{X}_{ij}\gamma + \varepsilon_{ij}. \quad (9)$$

The dependent variable takes the value 1 if case i cites case j , and 0 otherwise. The first three independent variables are our proxies for extra-legal benefit. These dummies take the value 1 if the pair ij belongs to the superscripted interaction category, and 0 otherwise.²¹ The fourth independent variable is our proxy for damage from a legal challenge and takes the value 1 if the government is a litigant in case i , and 0 otherwise. The row vector \mathbf{X}_{ij} contains a variety of case-level and judge-level controls (Table A3). This model is estimated using the 13,032 observations in the base sample.

The regression model that we use to explain the likelihood of an uncensored pair ij having an ‘applied’ citation is

$$Applied_{ij} = \beta_0 + \beta_1 B_{ij}^{\text{Before}} + \beta_2 B_{ij}^{\text{Between}} + \beta_3 B_{ij}^{\text{Placebo}} + \beta_4 D_{ij} + \mathbf{Z}_{ij} \phi + \epsilon_{ij}. \quad (10)$$

Here, the dependent variable takes the value 1 if the citation of case j by case i is ‘applied’, and 0 if it is ‘considered’. The first four independent variables are as defined in model (9), while the row vector \mathbf{Z}_{ij} contains a sub-set of the case-level and judge-level controls. This model is estimated using the 899 observations in the uncensored sub-sample.²²

²¹All specifications include dummies for the Common Judge and Other categories so that None is the excluded category.

²²The models in (9) and (10) can be estimated separately if there are no unobservables that determine both the (selection) decision whether to cite, and the (outcome) decision how to cite. To relax this assumption, we have also estimated the two models jointly using a binary sample selection specification where ε_{ij} and ϵ_{ij} are assumed to be joint normally distributed with covariance ρ . For identification, we require at least one exclusion restriction (a covariate in \mathbf{X}_{ij} but not in \mathbf{Z}_{ij}). We used the elapsed time between the two cases, whether case j was reported in *The Times* newspaper

Finally, the regression model that we use to explain the likelihood of pair ij having the same ruling (i.e. $r_i = r_j$) is

$$Same_{ij} = \lambda_0 + \lambda_1 B_{ij}^{\text{Before}} + \lambda_2 B_{ij}^{\text{Between}} + \lambda_3 B_{ij}^{\text{Placebo}} + \lambda_4 D_{ij} + \mathbf{X}_{ij} \varphi + v_{ij}. \quad (11)$$

In this model, the dependent variable takes the value 1 if the ruling in case i is the same as the ruling in case j , and 0 otherwise. The independent variables are as defined in model (9). This model is estimated using the 3,453 observations in the ruling sub-sample.

5 Results

Our main results are reported in Section 5.2 and discussed in Section 5.3. Before this, we present three pieces of evidence supporting the claim that the (initial) allocation of judges to cases is random.

5.1 Assessing our Identifying Assumption

We begin our assessment with a direct test for random panel formation. As explained in detail in Appendix A, we compare the proportion of initial matches in Michaelmas term with a given judge across his contemporaries. Under the hypothesis of random panel formation, the p -values obtained from these regressions (one for each CA Civ judge in our sample), should be distributed $U[0, 1]$. That is, the fraction of the regressions with p -values of 0.05 or less should be 0.05, the fraction of the regressions with p -values of 0.10 or less should be 0.10 and so on. To allow for differences in time allocation across the Criminal and Civil Divisions of the Court of Appeal, we report our results for all judges, and then disaggregate by time spent in each division.

law report, and the number of legal journal articles that mentioned case j in the period before case i . It seems plausible that these variables might affect the likelihood that the panel is aware of case j but not whether the panel finds case j persuasive. The results for the outcome equation are very similar to Table 5 below and are available upon request.

For the 27 judges with 100 percent of their cases in the CA Civ, there is strong evidence of random panel formation. The fraction of regressions with p -values of 0.05 or less is just 0.07 (2 judges). Moreover, as Figure 4 illustrates, the entire distribution closely approximates a uniform CDF. The evidence is more mixed for the judges who also hear cases in the CA Crim, due to the small amount of non-randomness introduced by movements across courts. For the 58 judges with 50-99 percent of their cases in the CA Civ, the fraction of regressions with p -values of 0.05 or less is 0.15 (9 judges). The overall distribution is slightly further from the 45 degree line. As such, we conclude that initial matches within our court of interest (the CA Civ) can realistically be viewed as random draws.

Our second test explores whether ‘contentious’ cases are assigned to particular judges. This serves as a robustness check because, if contentious cases are assigned non-randomly, it would be hard to believe that our proxies for B and D are orthogonal to case/judge characteristics. As set out in Appendix A, we compare the proportion of a caseload that is contentious across CA Civ judges. Here, a rejection of the null hypothesis of no significant differences would be evidence of non-random case assignment.

The first column of Table 2 is based on immigration cases. In this sample, a case is coded as contentious if the panel must reverse either a High Court judge or the Home Secretary. Averaging over the 78 CA Civ judges serving between 1993 and 2005, a little over 10 percent of the immigration caseload was contentious. Although there is variation across CA Civ judges, an F -test fails to reject the null hypothesis that these differences are due to chance. The same is true for a sample of planning cases, where a case is coded as contentious if the panel must reverse either a High Court judge or the Secretary of State for the Environment. The third column is based on housing cases. These cases are less contentious because the original decision was taken by a local council rather than a cabinet minister. Again, we fail to reject the null that the differences in caseload are due to chance.

Our final test is a comparison of observables. In the first five columns in Table 3 we compare observables across interaction categories. As one would expect if panels are formed at random, the mean number of interaction cases is broadly similar. On average, a pair in the Before category has

4.03 interaction cases in the Michaelmas Term after j (and none in the Michaelmas Terms after j and after i), a pair in in the Between category has 3.85 interaction cases in the Michaelmas Term before j (and none before j and after i), and a pair in the Placebo category has 3.89 interaction cases in the Michaelmas Term after i (and none before j and after j).

Turning to other judge and case-level observables, we find that, for all measures of social ties, the litigant dummy, the elapsed time from j to i , whether i and j share the same legal subject, whether j has appeared in *The Times* law reports, the number of times that j has been mentioned in legal journal articles, and whether there is a Law Lord in either panel, an F -test fails to reject the null hypothesis of no significant difference across interaction categories. Indeed, the only systematic difference is for the case-level panel size variables, where the mean in the None category is slightly smaller than in the other categories. Since this is likely to be a mechanical correlation, it seems fair to conclude that observables are well-balanced across interaction categories lending further support to our identifying assumption.²³

In the final three columns in Table 3 we compare observables across litigant categories. Consistent with the random allocation of cases to judges, the *judge*-level observables are well-balanced. For the interaction and social tie variables, an F -test fails to reject the null hypothesis of no significant difference across categories. As one might expect, however, case characteristics do differ with the political status of the litigants in case i , most noticeably the similarity of legal subjects and the composition of the panel in case i . For this reason, a simple comparison of proportions is not a credible test of Prediction 2, and we will need to rely on the regression models set out in Section 4.3.

5.2 Main Results

Citations Column (1) of Table 4 shows that the proportion of pairs with a citation is slightly lower for the Before and Between interaction categories than for the None interaction category. However, as the p -

²³Pairs in the Other category have a shorter elapsed time between j and i , and judges that are closer in age (picked up by contemporaneous university attendance) than pairs in the None category, and are not discussed any further below.

values from the corresponding probit regression indicate, we cannot reject the null hypothesis that these differences are significantly different from zero. Column (2) shows that the proportion of pairs with a citation is slightly lower for the Government category than for the Other Litigant category, and that this difference is weakly significant. In Column (3), we add case and judge-level controls. Although many of the case-level controls are strongly significant, the probit coefficients for the interaction categories change very little, and remain insignificant at conventional levels. The coefficient for the litigant category is relatively stable but, with a larger standard error, is no longer significant. The coefficient for the Common Judge category remains significant, suggesting that CA Civ judges may indulge in self-citation.

Column (1) of Table 5 shows that the proportion of uncensored pairs with an applied citation is higher for the Before and Between interaction categories than for the None interaction category. The differences are economically large (18 and 19 percentage points) and statistically significant (at the 10 and 5 percent levels). Column (2) shows that the proportion of uncensored pairs with an applied citation is slightly higher for the Government category than for the Other Litigant category but that this difference is not significantly different from zero. In Column (3), we add case and judge-level controls. The probit coefficients for the interaction categories again change very little, although the Before category now dips slightly below the 10 percent significance level. In contrast, the coefficient for the litigant category drops markedly, largely due to the inclusion of controls for the subject of case i .

Rulings In Panel A of Table 6, we report the proportion of pairs with the same ruling, irrespective of whether that ruling is to dismiss or allow the appeal. Column (1) shows that the proportion of pairs with the same ruling is higher for the Before and Between categories than for the None category. However, as the p -values from the corresponding probit regression indicate, we are unable to reject the null hypothesis that these differences are significantly different from zero. Column (2) shows that the proportion of pairs with the same ruling is slightly higher for the Government category than for the Other Litigant category but, again, this difference is not significantly different from zero. The addition

of controls in Column (3) has little impact on the interaction coefficients but almost doubles the litigant coefficient. All coefficients of interest remain insignificant, however, and an F -test for joint significance of *all* independent variables comfortably fails to reject the null hypothesis that all coefficients are equal to zero.

An obvious concern with Panel A is that the coefficients are assumed not to vary with the ruling in case j . This is restrictive because our theoretical framework highlights that the comparative statics of panel behavior differ with r_j . In particular, when case j allows its appeal, the threshold q^* is *greater* than p , suggesting that a change in extra legal benefit may have a limited impact. To relax this assumption, we re-estimate our models for the sub-sample of pairs where case j dismisses its appeal. Column (1) of Panel B shows that the proportion of pairs with the same ruling is higher for the Before category than for the None category and, moreover, that the difference is now economically large (14 percentage points) and statistically significant (at the 5 percent level). The probit coefficient for the Between category is also higher than in Panel A, although it remains insignificant at conventional levels. Column (2) shows that this is also true for the litigant dummy, while Column (3) shows that these findings are robust to the inclusion of controls. The coefficient for the Common Judge category is positive and weakly significant, suggesting that there may be persistence in CA Civ rulings. Finally, we complete our analysis by exploring whether the identity of the appellant/respondent impacts upon the panel's propensity to dismiss the appeal. Column (4) shows that a government appellant (respondent) is associated with a significantly lower (higher) dismissal rate than no government litigant.²⁴

5.3 Discussion

The results reported in Section 5.2 are consistent with all three parts of Prediction 1. In particular, we find statistically and economically significant differences in the proportion of pairs with the same

²⁴We have repeated this exercise for the sub-sample where case j allows the appeal. In these regressions (available upon request) the 'appellant' effect is larger but the 'respondent' effect is smaller and no longer significant.

ruling, and the proportion of uncensored pairs with an “applied” citation, across interaction categories. Of course, we can only take this as evidence that on-the-job interactions influence panel behavior if our assumption of random assignment is valid. Two observations (in addition to the evidence in Section 5.1) suggest that this identifying assumption is valid. First, we are unable to reject the null hypothesis of no difference in proportions across the Placebo and None categories. This is reassuring since it is hard to imagine that unobservables could drive our key findings but then fail to produce a difference in these placebo tests. Second, inclusion of a wide variety of controls leaves the magnitude of Before and Between coefficients virtually unchanged.

In contrast, we find no evidence to support Prediction 2; in fact, the proportion of pairs with a citation is actually *lower* for the Government Litigant category than for the Other Litigant category rather than higher as predicted. However, in view of the balancing tests and the sensitivity of the litigant coefficient to the inclusion of controls, it seems likely that there are omitted case characteristics. Thus our view is that, given the available data, there is insufficient evidence to rule out the possibility that panel behavior is influenced by the political status of the litigants.

Mechanism Having argued that on-the-job interactions matter, we now turn to the underlying mechanism. In Section 4.1, we suggested that interaction might increase the utility that the panel receives from enhancing the promotion prospects of the judges in case j , referring to this as a strengthening of social preferences. If this is true, then we should expect the effect of on-the-job interactions to be larger when (i) the panel in case i is more influential, and (ii) more of the judges hearing case j face the prospect of promotion. To explore this issue, we re-estimate our models using different sub-samples, first splitting by the experience of the panel in case i (above versus below average) and then by the rank of the panel in case j (with a member already promoted to the rank of law lord versus without). The results are reported in Table 7. Panel A confirms that there are no differences in the proportion of pairs with a citation across interaction categories save for one weakly significant coefficient in the

high experience sub-sample, and likewise for the litigant category save for one *negative* coefficient in the post-promotion sub-sample. Panel B shows that the differences in the proportion of uncensored pairs with an “applied” citation across interaction categories reported in Table 5 are indeed driven by the high experience and pre-promotion sub-samples (the coefficients are significantly different from zero only in these sub-samples). Similarly, Panel C shows that the differences in the proportion of pairs with the same ruling across interaction categories reported in Table 6 are driven by the high experience and pre-promotion sub-samples. While it is possible to construct alternative explanations, these consistent results certainly lend support to the view that panel behavior is shaped by socializing forces.

Policy Implication The main policy implication of our results relates to the design of judicial appointments systems. The above analysis suggests that, under a system where discretionary citations influence the prospect of promotion, social interactions may bias rulings (in marginal cases) and affect discretionary citation behavior. An effect on case outcomes would be of first-order importance because it would constitute a violation of natural justice. Moreover, if discretionary citations are affected, then they will serve as a poor performance measure. In particular, if wider social interactions have a similar effect to the random on-the-job interactions studied in this paper, then citation counts will be a *biased* as well as a noisy measure of judicial quality. As such, our results suggest that information generated during a selection exercise (test, role-play and interview data) may be at least as valuable as metrics based on prior, on-the-job performance (citations). Following the Constitutional Reform Act 2005, the independent Judicial Appointments Commission for England and Wales now runs ‘selection days’ for posts up to the level of High Court Justice. Our paper highlights that it may be worth extending this innovation to more senior judicial posts.

6 Concluding Remarks

Interest in the content of legal opinions is on the increase, both among scholars studying judicial communication and those searching for measures of judicial influence. To date, much of the research in this area has explored whether political factors such as ideology and institutions shape adherence to precedent in US courts. Aside from the novel English dataset, this paper departs from the literature in two respects. First, we focus on *discretionary* references to prior statements of law that are not binding precedents but could be applied (or not applied) as persuasive authorities. Second, we examine the effect of *social* factors in the form of on-the-job interactions between judges, as well as the effect of the political status of the litigants rather than the judges themselves.

Our results indicate that on-the-job interactions between judges shape both discretionary citations and their associated rulings, and are consistent with these effects being driven by a strengthening of socializing forces that arise as judges work together in case panels. As such, we provide the first quantitative empirical support for the widely held suspicion (Posner 2000, Solum 2005) that citation behavior can be influenced by social (in this case on-the-job) contact. We also provide the first evidence of a spillover from citations to associated rulings. These findings caution against the use of on-the-job performance metrics in the promotions process for judges.

Our results do not support the view that the political status of the litigants influences citation behavior. That said, since litigant status appears to be correlated with other unobserved characteristics of the case, it would be premature to interpret our findings as evidence that appellate judges are *not* sensitive to political features of their cases. Indeed, the fact that government appellants (respondents) are associated with significantly lower (higher) dismissal rates than other litigants suggests that the political sensitivity of the English judiciary would be a fruitful area for further research.

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Appendix A

Panel Formation We test for random panel formation by comparing the proportion of *initial cases* matched with a given judge across his *contemporaries*. Before illustrating the steps involved, it will be helpful to define precisely what we mean by initial cases and contemporaries.

Since our objective is to test for random initial allocations, the ideal strategy would be to restrict attention to committees formed on or shortly after the first day of the Michaelmas Term. Unfortunately, since we observe judgment but not commencement dates, this is not possible. Instead, we deem a case to be an *initial case* if its judgment date falls in the first term of the legal year. In view of the end of term effects apparent in Figure 3, the committees in these cases will plausibly be filled early in Michaelmas Term before the cab-rank principle (and the concomitant serial correlation) is at work.

Turning to the notion of contemporaries, it is clear that a match can only occur if the judges concerned have overlapping periods of service. We account for this potential source of non-randomness in two stages. First, we identify the set of appellate judges whose periods of service overlap with a given judge x . Then, given our focus on Michaelmas Term, we exclude judges who were either appointed before judge x 's first Michaelmas Term or after his last Michaelmas Term. We define the remaining individuals as judge x 's *contemporaries*; the idea being that these are judges who could have been matched with judge x on an initial case.

Of course, adopting the above definition of an initial case, raises the issue of whether to use all of a judge's cases in a Michaelmas Term when calculating his proportion matched with judge x . To mitigate the possibility of within-term serial correlation, we use a binary rather than continuous match variable. Specifically, we construct a dummy variable $matched_{nt}$ that takes the value 1 if contemporary n is matched with judge x in at least one case in Michaelmas Term t and 0 if contemporary n is not matched with, but still serves alongside, judge x in the same Michaelmas Term t .

Between January 1 1980 and December 31 2005, 123 individuals held the title Lord Justice. We have sufficient case data to undertake a test of random committee formation for 91 of these 123 judges. For each judge $x = 1, \dots, 91$ we:

1. Identify his set of contemporaries (and denote its cardinality by N_x).
2. For each contemporary $n = 1, \dots, N_x$, construct the dummy variables $matched_{nt}$ and $contemporary_n$ (an individual fixed effect).
3. Estimate the following equation,

$$matched_{nt} = \alpha + \beta_1 contemporary_1 + \dots + \beta_{N_x-1} contemporary_{N_x-1} + \varepsilon_{nt},$$

and obtain the p -value from an F -test for the joint significance of the $N_x - 1$ included contemporary dummies.

Under the hypothesis of random panel formation, the p -values obtained from these 91 regressions, should be distributed $U[0, 1]$. That is, the fraction of the regressions with p -values of .05 or less should be .05, the fraction of the regressions with p -values of .10 or less should be .10 and so on.

One final outstanding issue is time allocation across divisions. For instance, a criminal specialist who spends most of his time in the Criminal Division of the Court of Appeal is unlikely to be matched with a civil specialist who spends most of his time in the Civil Division of the Court of Appeal (particularly relative to another civil specialist). To explore this possibility, Figure 4 also reports our results disaggregated by time spent in the two respective divisions.

Case Assignment Our first test uses a sample of 1082 CA civ judge-CA Civ immigration case pairs. Following Ashenfelter et al (1995), we proceed in two stages. First, we identify a subsample of *contentious* cases where the government and/or the judiciary might wish to influence assignment. These are cases where the government is seeking a reversal of a judicial review decision by a High Court judge to reverse the Home Secretary’s decision to deny an individual leave to remain in the UK (i.e. the appellant is the Home Secretary). We refer to these cases as contentious because the CA Civ judge’s decision will either overrule a High Court judge or a cabinet minister. Then, we establish whether the fraction of a CA Civ judge’s immigration caseload that is contentious differs across judges and, if so, whether we can reject the null hypothesis that these differences are due to chance. Formally, we regress a variable indicating whether a pair is contentious or not on a constant and a set of judge dummies, and then report the p -value from an F -test for joint significance in the first column in Table 2.

To provide a further robustness check, we repeat the above exercise using a sample of 564 planning case pairs. Here, the contentious cases are where the government is seeking a reversal of a judicial review decision by a High Court judge to reverse the Secretary of State for the Environment’s decision to deny an individual or a company a planning application. We also consider a small sample of 353 housing case pairs where the appellant is a local council seeking a reversal of a judicial review decision by a High Court judge to reverse the council’s decision to deny an individual social housing. These results are reported in the second and third columns of Table 2 respectively.

Appendix B

Proof of Proposition 1. Immediate from equations (6)-(8) and the fact that $c < \underline{c} \Leftrightarrow \tilde{q} < q^*$. □

Proof of Proposition 2. Since $\gamma_{1,0,1}$ is decreasing in q and ρ is decreasing in $\gamma_{1,0,1}$, the term in square brackets in (6) is increasing in q . If b increases, then this term must decrease to restore the equality, hence \tilde{q} is decreasing in b . The signs of the derivatives of \underline{c} and \bar{c} wrt to b are immediate from (7) and (8). For any $b - \alpha c > 0$, the term in square brackets in (6) is negative. So, if D increases, this term must increase to restore the equality, hence \tilde{q} is increasing in D . Conversely, for any $b - \alpha c < 0$, the term in square brackets in (6) is positive. So, if D increases, this term must decrease to restore the equality, hence \tilde{q} is decreasing in D . Given Assumption 3, the term in square brackets in both (7) and (8) is positive, implying that the derivatives of \underline{c} and \bar{c} wrt to D must also be positive. □

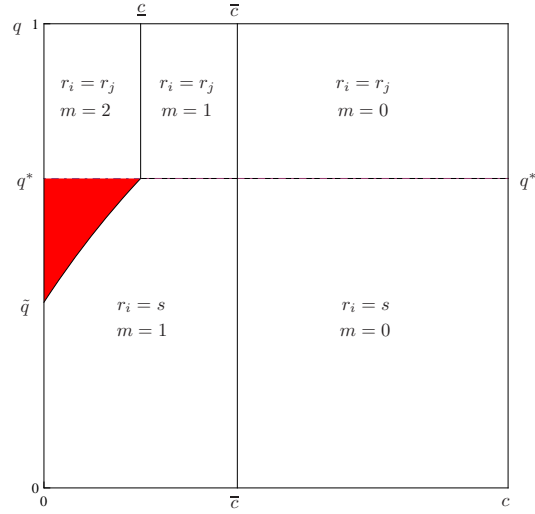


Figure 1: Behavior of a Panel with Conflicting Signals ($s = 1, r_j = 0$).

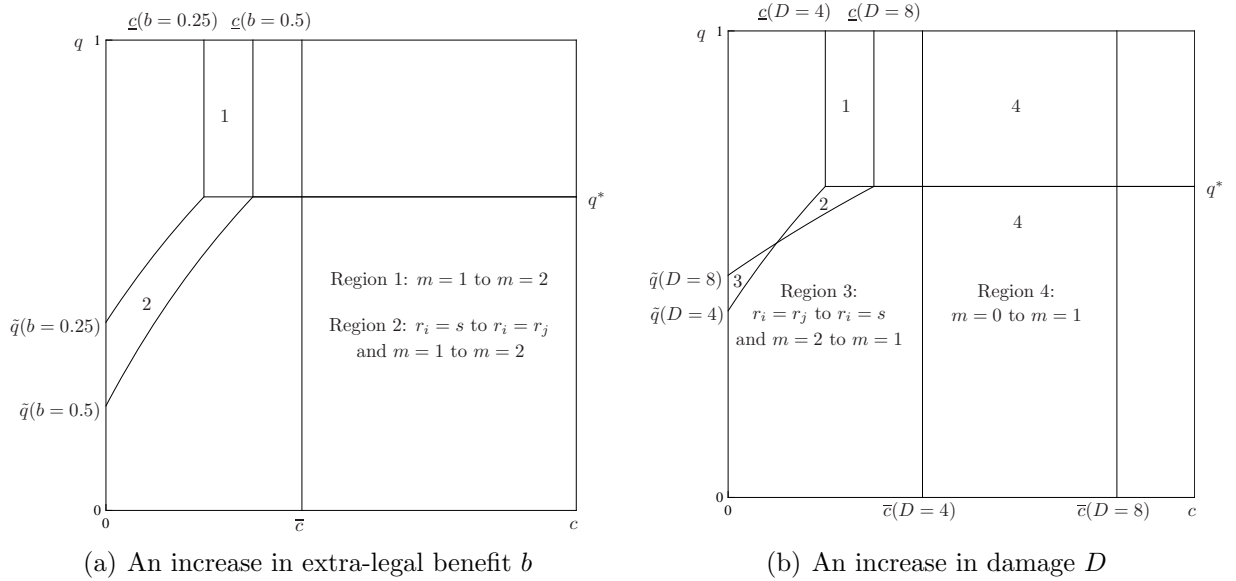
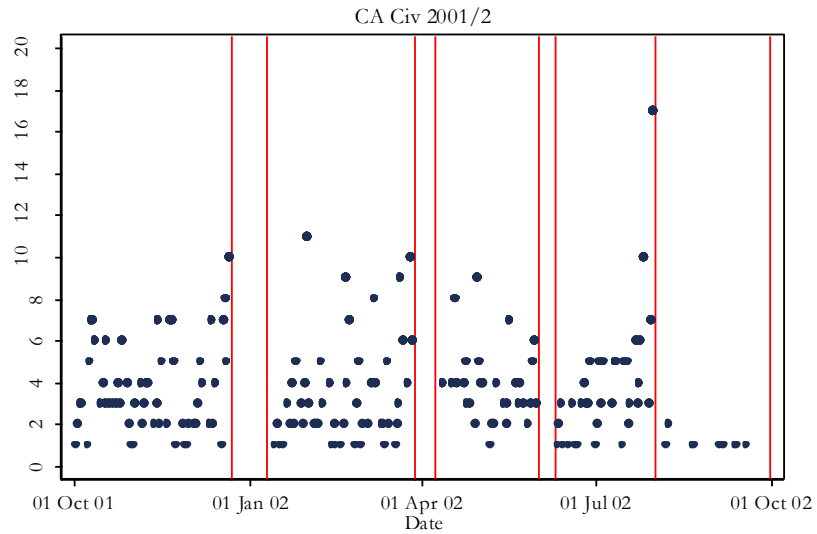
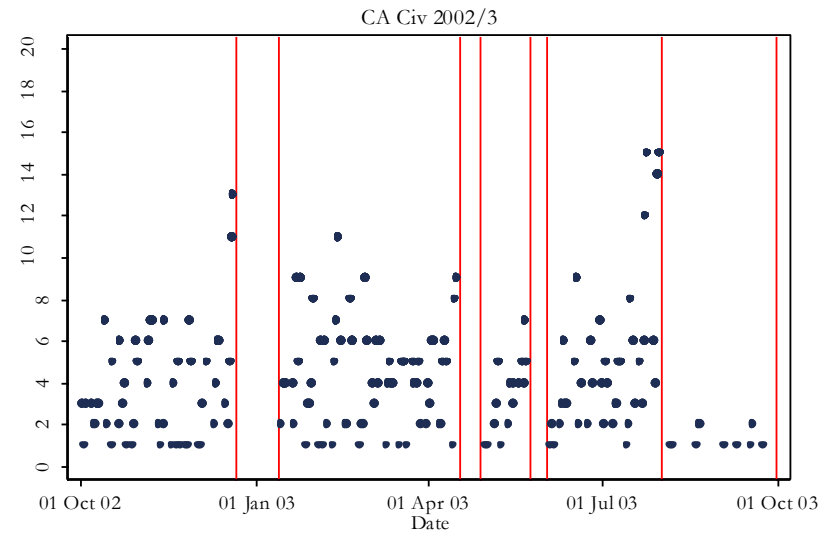


Figure 2: Comparative Statics for a Panel with Conflicting Signals ($s = 1, r_j = 0$).

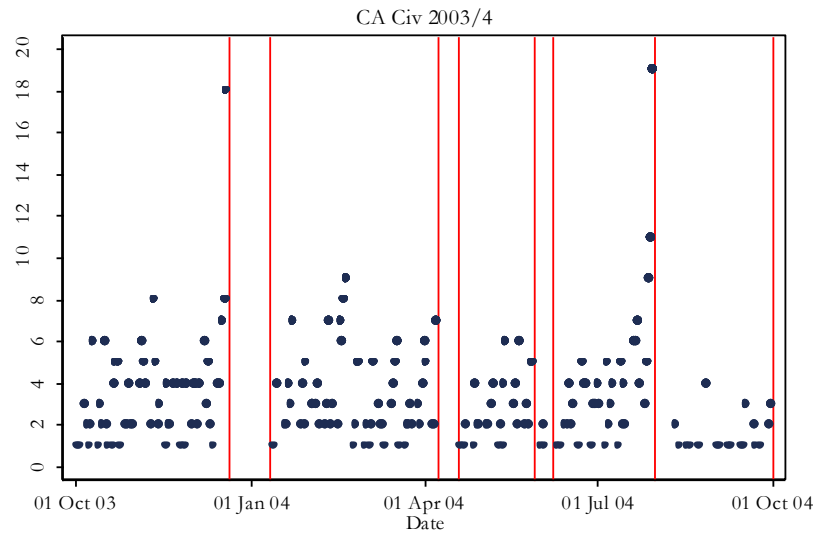
Figure 3. Number of Cases Decided on Each Day of the Judicial Year (2001-2005)



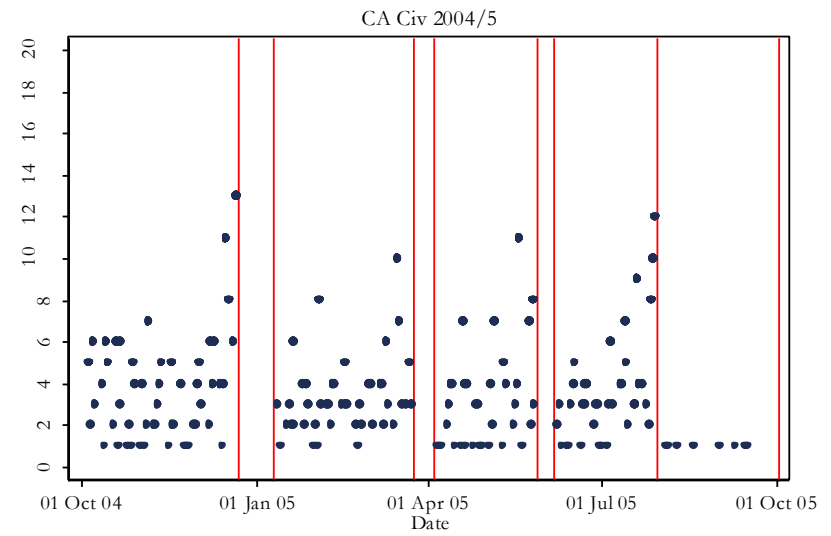
Note: 642 cases (10 in long vacation); lines mark vacations.



Note: 735 cases (13 in long vacation); lines mark vacations.

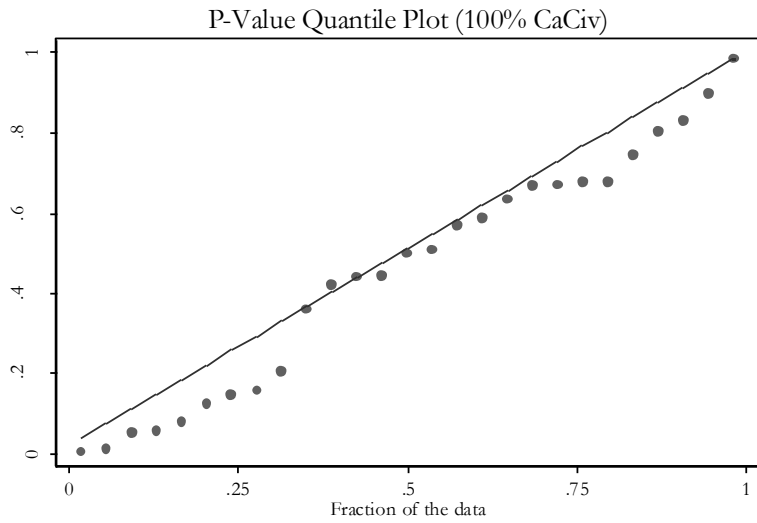


Note: 635 cases (28 in long vacation); lines mark vacations.

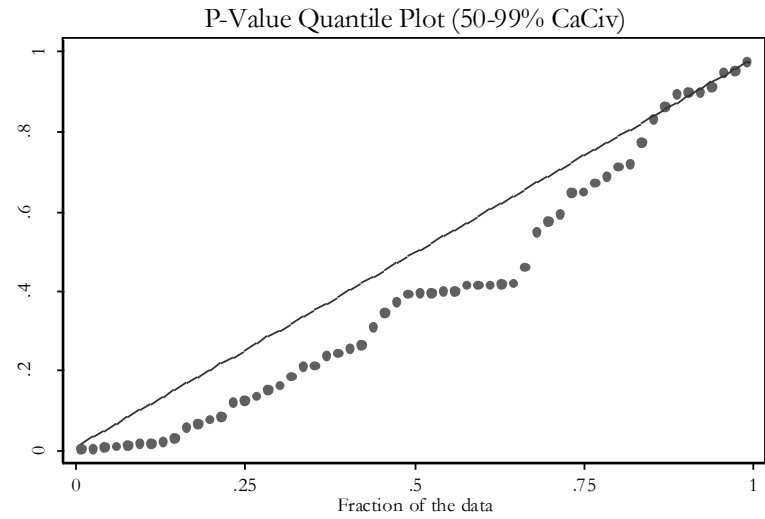


Note: 593 cases (8 in long vacation); lines mark vacations.

Figure 4. Results of Random Panel Formation Tests



Note: 27 Judges; p-value < .05 for 7% of the data.



Note: 58 Judges; p-value < .05 for 15% of the data.

Table 1. Sample Sizes

	Base Sample	Uncensored Sub-sample		Ruling Sub-sample
		All	Negative	
Usable Observations	13032	899	5	3453
By Interaction Category				
Before	721	36	0	178
Between	975	52	0	251
Placebo	729	43	0	152
None	824	52	0	190
Common Judge	1609	199	2	490
Other	8174	517	3	2192
By Litigant Category (case i)				
Government	1968	110	0	992
Other Litigants	11064	789	5	2461

Notes: Tables 3 and 4 are based on 13032 pairs in the base sample. Table 5 is based on 899 pairs in the uncensored sub-sample. Table 6 is based on 3453 pairs in the ruling sub-sample where the ruling (dismissing or allowing the appeal) is available for *both* case i and j .

Table 2. Assignment of Contentious Cases

	Immigration Cases	Planning Cases	Housing Cases
Case coded as contentious if the appellant is	The Home Secretary	The Secretary of State for the Environment	A Local Council
Summary Statistics			
Number of judges	78	78	78
Number of judge-case pairs	1028	564	353
No. of cases with characteristic	108	68	144
Mean proportion of a judge's caseload with characteristic	0.105	0.121	0.408
S.D. of above proportion	0.073	0.099	0.214
F-test for judge dummies			
<i>p</i> -value (no year dummies)	0.895	0.900	0.494
<i>p</i> -value (with year dummies)	0.935	0.899	0.605

Notes: The cases are from the Westlaw Sample for 1993 to 2005 (see Table A2). Year dummies are included to allow for changes in mean proportions over time (and hence judges). In immigration and housing cases the other party is always an individual. In planning cases the other party may be an individual, a company, or a local council.

Table 3. Balancing Tests

	Interaction Category					Litigant Category		
	Before	Between	Placebo	None	<i>F</i> -test	Govt	Other	<i>F</i> -test
	Mean	Mean	Mean	Mean	<i>p</i> -value	Mean	Mean	<i>p</i> -value
Number of Interaction Cases in:								
Michaelmas Term Before <i>j</i>	4.03	0	0	0		3.837	3.815	.89
Michaelmas Term After <i>j</i>	0	3.85	0	0		4.353	4.338	.92
Michaelmas Term After <i>i</i>	0	0	3.89	0		3.267	3.275	.96
Litigant Category (case <i>i</i>)								
Government	.1318	.1354	.1618	.1674	.14	1	0	
Controls								
Time Difference, yrs	2.397	2.499	2.444	2.415	.34	2.460	2.377	.01
1[Same Subject]	.0634	.0633	.0618	.0679	.97	.0342	.0624	.00
Citable Case <i>j</i>								
1[Times Law Report]	.4958	.5169	.4931	.5024	.80	.4991	.5274	.03
Number of Legal Journal Articles	3.473	3.231	3.081	3.237	.58	3.510	3.513	.98
Panel Size	2.712	2.624	2.518	2.431	.00	2.580	2.586	.69
1[Law Lord in Panel]	.1642	.1544	.1568	.1371	.56	.1272	.1361	.31
Citing Case <i>i</i>								
Panel Size	2.662	2.684	2.715	2.565	.00	2.886	2.683	.00
1[Law Lord in Panel]	.1627	.1208	.1790	.1475	.01	.1785	.1394	.00
Social Ties								
1[At School Together]	.0352	.0349	.0313	.0426	.72	.0360	.0404	.38
1[At University Together]	.1115	.1004	.0931	.0855	.45	.1168	.1139	.74
1[Same Chambers]	.0794	.0951	.1111	.0757	.12	.0815	.0874	.43
1[Same Club]	.1438	.1358	.1588	.1287	.47	.1557	.1460	.32

Notes: Means corrected for choice based sampling. Details of the variable construction are provided in Table A3.

Table 4. Proportion of Pairs with a Citation

		Comparison of Proportions (1)				Comparison of Proportions (2)				Plus Controls (3)		
		Propn	Probit Coef	Probit SE	p-value	Propn	Probit Coef	Probit SE	p-value	Probit Coef	Probit SE	p-value
Interaction Category												
	Before	.0030	-.0764	.0532	.15					-.0686	.0748	.36
	Between	.0034	-.0502	.0485	.30					-.0610	.0713	.39
	Placebo	.0037	-.0284	.0511	.58					.0151	.0701	.83
	None	.0042	<i>Excluded Category</i>							<i>Excluded Category</i>		
	Common Judge	.0084	.1710	.0395	*** .00					.1516	.0592	** .01
Litigant Category												
	Government					.0037	-.0479	.0251	* .06	-.0410	.0415	.32
	Other Litigants					.0045	<i>Excluded Category</i>			<i>Excluded Category</i>		
Controls												
	Time Difference, yrs									-.0384	.0110	*** .00
	1[Same Subject]									.9372	.0290	*** .00
	Citable Case <i>j</i> :											
	1[Times Law Report]									.1939	.0280	*** .00
	Number of Legal Journal Articles									.0214	.0017	*** .00
	Panel Size									.0482	.0219	** .03
	1[Law Lord in Panel]									.1893	.0312	*** .00
	Citing Case <i>i</i> :											
	Panel Size									.0469	.0250	* .06
	1[Law Lord in Panel]									-.0216	.0369	.56
	Subject & Social Tie Dummies?											
			No				No				Yes	
	<i>F</i> -Statistic (<i>p</i> -value)		9.72 (.00)				3.63 (.06)				46.14 (.00)	
	Subpopulation, PSUs, Strata		13032, 15455, 13				13032, 15455, 13				13032, 15455, 13	

Notes: Linearized robust SEs corrected for choice based sampling. Dummy for ‘other’ interaction category always included. In columns (1) and (2) proportions are obtained via STATA’s proportion estimator for survey data (svy: prop) and multiplied by 100.

Table 5. Proportion of Uncensored Pairs with an ‘Applied’ Citation

	Comparison of Proportions (1)				Comparison of Proportions (2)				Plus Controls (3)		
	Propn	Probit Coef	Probit SE	<i>p</i> -value	Propn	Probit Coef	Probit SE	<i>p</i> -value	Probit Coef	Probit SE	<i>p</i> -value
Interaction Category											
Before	58.33	.4538	.2742 *	.10					.4221	.2755	.13
Between	59.62	.4868	.2485 **	.05					.4774	.2519 *	.06
Placebo	51.16	.2726	.2597	.29					.2892	.2647	.28
None	40.38	<i>Excluded Category</i>							<i>Excluded Category</i>		
Common Judge	49.25	.2245	.1969	.25					.2070	.2020	.31
Litigant Category (case <i>i</i>)											
Government Litigant					49.09	.0583	.1276	.65	.0064	.1660	.97
Other Litigants					46.77	<i>Excluded Category</i>			<i>Excluded Category</i>		
Controls											
Time Difference, yrs									.0139	.1076	.73
1[Same Subject]									-.0755	.0934	.42
Citable Case <i>j</i> :											
1[Times Law Report]									.0553	.1076	.61
Number of Legal Journal Articles									.0072	.0059	.22
Panel Size									.2067	.0851 **	.02
1[Law Lord in Panel]									-.0251	.1016	.80
Citing Case <i>i</i> :											
Panel Size									-.0159	.0873	.86
1[Law Lord in Panel]									-.0057	.1285	.96
Subject & Social Tie Dummies?			No				No			Yes	
<i>F</i> -Statistic (<i>p</i> -value)			3.07 (.00)				0.21 (.65)			1.77 (.01)	
Subpopulation, PSUs, Strata			899, 15455, 13				899, 15455, 13			899, 15455, 13	

Notes: Linearized robust SEs corrected for choice based sampling. Dummy for ‘other’ interaction category always included. In columns (1) and (2) proportions are obtained via STATA’s proportion estimator for survey data (svy: prop) and multiplied by 100. Table A4 reports results with a selection model.

Table 6. Proportion of Pairs with Same Ruling

	Comparison of Proportions				Comparison of Proportions				Plus Controls					
	Propn	(1)			Propn	(2)			(3)			(4)		
		Probit Coef	Probit SE	p-value		Probit Coef	Probit SE	p-value	Probit Coef	Probit SE	p-value	Probit Coef	Probit SE	p-value
Panel A. Ruling Sub-sample														
Before	54.70	.2244	.1440	.12					.2321	.1448	.11			
Between	52.20	.1615	.1341	.23					.1596	.1350	.24			
Placebo	50.14	.1099	.1498	.46					.0965	.1504	.52			
None	45.76	<i>Excluded Category</i>							<i>Excluded Category</i>					
Common Judge	50.34	.1148	.1198	.34					.1106	.1222	.37			
Government Litigant					53.12	.0396	.0522	.45	.0793	.0580	.17			
Other Litigants					51.55	<i>Excluded Category</i>			<i>Excluded Category</i>					
Government Appellant														
Government Respondent														
Controls?		No				No			Yes					
F-Statistic (p-value)		1.53 (.13)				0.57 (.45)			1.22 (.20)					
Subpopulation, PSUs, Strata		3453, 15455, 13				3453, 15455, 13			3453, 15455, 13					
Panel B. Case j Dismisses														
Before	60.25	.3519	.1823	** .05					.3657	.1831	** .05	.3380	.1825	* .06
Between	56.73	.2614	.1722	.13					.2668	.1726	.12	.2485	.1729	.15
Placebo	56.07	.2448	.1952	.21					.2396	.1972	.22	.1975	.2004	.32
None	46.34	<i>Excluded Category</i>							<i>Excluded Category</i>					
Common Judge	57.16	.2724	.1546	* .08					.2797	.1574	* .08	.2567	.1576	* .10
Government Litigant					59.82	.0571	.0677	.40	.0736	.0759	.33			
Other Litigants					57.59	<i>Excluded Category</i>			<i>Excluded Category</i>					
Government Appellant												-.4594	.1236	*** .00
Government Respondent												.2838	.0858	*** .00
Controls?		No				No			Yes					
F-Statistic (p-value)		1.97 (.04)				0.71 (.40)			1.13 (.29)					
Subpopulation, PSUs, Strata		2048, 15455, 13				2048, 15455, 13			2048, 15455, 13					

Notes: Linearized corrected robust SEs. Dummy for 'other' interaction category always included. Where specified, regressions include the controls listed in Tables 4-5.

Table 7. Results for Sub-samples

		By Experience of Panel in Citing Case <i>i</i>						By Rank of Panel in Citable Case <i>j</i>						
		High (1)			Low (2)			Post-Promotion (3)			Pre-Promotion (4)			
		Probit Coef	Probit SE	<i>p</i> -value	Probit Coef	Probit SE	<i>p</i> -value	Probit Coef	Probit SE	<i>p</i> -value	Probit Coef	Probit SE	<i>p</i> -value	
Panel A. Citation														
	Before	-.1852	.1076	*	.09	-.0195	.1105	.86	-.1607	.1566	.31	-.0187	.0857	.83
	Between	-.0513	.0989	.60	-.1042	.1028	.31	-.1666	.1507	.27	-.0162	.0813	.84	
	Placebo	-.1320	.1133	.24	.0997	.0916	.28	-.0329	.1570	.83	.0433	.0799	.59	
	Common Judge	.0534	.0860	.54	.1933	.0833	**	.0234	.1307	.86	.1913	.0679	***	.00
	Government Litigant in Case <i>i</i>	-.0761	.0597	.20	.0136	.0600	.82	-.1847	.0920	**	.05	.0160	.0434	.71
	Subpopulation, PSUs, Strata	6479, 15455, 13			6553, 15455, 13			1937, 15455, 13			11095, 15455, 13			
Panel B. Applied Citation														
	Before	.9198	.3909	**	.02	-.1769	.4252	.68	.1340	.5498	.81	.6106	.3366	* .07
	Between	.6906	.3636	*	.06	.2718	.3688	.46	.0127	.4473	.98	.7819	.3161	** .01
	Placebo	.0168	.4506	.97	.2195	.3532	.53	-.0743	.4884	.88	.5186	.3285	.12	
	Common Judge	.5807	.2897	**	.05	-.1847	.3001	.54	-.0382	.3541	.91	.4377	.2611	* .09
	Government Litigant in Case <i>i</i>	.0596	.2131	.78	.0596	.2948	.84	-.0309	.3094	.92	-.0020	.2012	.99	
	Subpopulation, PSUs, Strata	471, 15455, 13			428, 15455, 13			255, 15455, 13			644, 15455, 13			
Panel C. Same Ruling (<i>j</i> dismisses)														
	Before	.5602	.2384	**	.02	.1543	.2861	.59	.1632	.5735	.78	.4194	.1998	** .04
	Between	.3053	.2454	.21	.2497	.2444	.31	.5246	.5158	.31	.2937	.1883	.12	
	Placebo	.2759	.3071	.37	.1721	.2637	.51	.8015	.5740	.16	.1508	.2151	.48	
	Common Judge	.4119	.2118	**	.05	.2144	.2367	.37	.6384	.4917	.19	.2435	.1705	.15
	Government Litigant in Case <i>i</i>	.0357	.1019	.72	.1196	.1156	.30	-.0565	.2276	.80	.0681	.0823	.41	
	Subpopulation, PSUs, Strata	1099, 15455, 13			949, 15455, 13			324, 15268, 12			1724, 15455, 13			

Notes: Linearized corrected robust SEs. Dummy for ‘other’ interaction category always included. All regressions include the controls listed in Tables 4-5.

Coding: ‘low experience’ if the average CA Civ tenure of the citing panel is below the mean/median of 5 years, and high experience otherwise;

‘high status’ if a Law Lord is a member of the panel in case *j*, and low status otherwise.

Appendix Table A1. Discretionary Citations and Articles in Legal Journals

Dependent Variable: Number of legal journal articles mentioning CA Civ case i in year t

	OLS (1)			Poisson (2)		
	Coef	SE	p -value	Coef	SE	p -value
No. of applied citations of case i in year t	0.322	0.018	.00	0.729	0.024	.00
No. of considered citations of case i in year t	0.215	0.015	.00	0.638	0.026	.00
Case Fixed Effects?		Yes			Yes	
Time Period Fixed Effects?		Yes			Yes	
Number of Observations		42,0012			42,0012	
Test: applied & considered coefficients differ, p -value		.00			.01	

Notes: Citations across all courts. In Column (1), all variables are in logs, and in Column (2), all independent variables are in logs.

Both columns include case fixed effects to control for unobservables associated with both the likelihood of a case being mentioned in a legal journal and its receipt of discretionary citations. The second Poisson specification accounts for the high prevalence of zeros in the dependent variable, as well as the fact that the dependent variable takes a countable number of values.

Appendix Table A2. Population and Sample Sizes, by Strata (Case i year)

Strata	Out of Sample				Strata	In Sample			
	Westlaw Sample	Pair Population Size (M_b)	Uncensored Pairs (U_b)	Censored Pair Sample Size		Westlaw Sample	Pair Population Size (M_b)	Uncensored Pairs (U_b)	Censored Pair Sample Size ($m_b - U_b$)
1980	28	375	0		1993	355	111,404	17	170
1981	29	1,209	0		1994	427	295,749	37	370
1982	20	1,328	0		1995	633	764,641	73	730
1983	33	3,047	2		1996	709	1,313,687	101	1010
1984	36	4,585	1		1997	710	1,758,086	97	970
1985	22	2,826	0		1998	678	1,908,762	96	960
1986	22	2,672	2		1999	801	2,505,987	112	1,120
1987	21	2,582	2		2000	958	3,231,716	126	1,260
1988	14	1,504	0		2001	891	3,198,076	114	1,140
1989	12	1,014	0		2002	621	2,257,840	138	1,380
1990	14	1,056	0		2003	745	2,712,193	219	2,190
1991	26	1,908	0		2004	628	2,214,609	168	1,680
1992	86	9,287	0		2005	404	1,246,173	107	1,070
Total	363	33,384	7	0	Total	8,560	23,517,932	1,405	14,050

Notes: Westlaw Sample refers to the number of Court of Appeal (Civil Division) cases in the given year available on Westlaw UK. Pair population size refers to the number of *Within 5 year Court of Appeal (Civil Division)* pairs.

Appendix Table A3. Details of Control Variable Construction

Variable	Construction
Time Difference	The number of days (divided by 365.25) between the dates of Case <i>i</i> and <i>j</i> .
1[Same Subject]	Equal to 1 if Case <i>i</i> and <i>j</i> share the same subject based on Weslaw's 100 part classification of legal subjects, and 0 otherwise.
Citable Case <i>j</i>	
1[Times Law Report]	Equal to 1 if Case <i>j</i> is reported in <i>The Times</i> newspaper's Law Report section in the week following Case <i>j</i> .
Number of Legal Journal Articles	The number of articles in legal journals that have referred to Case <i>j</i> by the date of Case <i>i</i> .
Panel Size	The number of judges hearing Case <i>j</i> .
1[Law Lord in Panel]	Equal to 1 if one or more of the judges hearing Case <i>j</i> is a Law Lord at the date of Case <i>j</i> .
Citing Case <i>i</i>	
Panel Size	The number of judges hearing Case <i>i</i> .
1[Law Lord in Panel]	Equal to 1 if one or more of the judges hearing Case <i>i</i> is a Law Lord at the date of Case <i>i</i> .
Subject Dummies	A set of six dummies (Chancery, Civil, Crime, Employment, Family, Public) for Case <i>i</i> based on Weslaw's 100 part classification of legal subjects.
Social Ties	
1[At School Together]	Equal to 1 if at least one judge pair (one from Case and one from Case) attended the same secondary school at the same time (measured as same date of birth +/- 5 years), and 0 otherwise.
1[At University Together]	Equal to 1 if at least one judge pair attended the same university (or college if Oxford or Cambridge) at the same time (measured as same date of birth +/- 3 years), and 0 otherwise.
1[Same Chambers]	Equal to 1 if at least one judge pair practiced as a barrister from the same chambers prior to becoming a judge, and 0 otherwise.
1[Same Club]	Equal to 1 if at least one judge pair are members of the same Gentleman's or sports club.