THE LAW'S DELAY: A TEST OF THE MECHANISMS OF JUDICIAL PEER EFFECTS

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ABSTRACT

The presence of "peer effects"—that an ideologically homogenous panel decides a case in a more characteristically partisan way than an ideologically diverse panel—is a standard finding in studies of appellate decision-making, but the mechanisms that generate peer effects are not well understood. This article examines a previously overlooked implication that the leading theories of peer effects hold for the speed of judicial decision-making. One set of theories asserts that peer effects result from preference-revealing interactions among judges, such as deliberation or negotiation. These interactions are potentially time-consuming. Other theories, such as whistleblowing and dissent aversion, claim that peer effects result from a judge's response to existing knowledge of her colleagues' preferences. These responses are potentially instantaneous. A simple prediction is that if bargaining or deliberation, rather than whistleblowing or dissent aversion, causes peer effects, ideologically mixed panels should be slower to render decisions than ideologically homogenous panels. The article tests this prediction against a sample of administrative law decisions that have previously been shown to exhibit strong peer effects. The article's main estimates show that the ideological diversity of a panel does not correlate with the speed of decision-making. This finding suggests that preference-revealing interactions do not cause judicial peer effects. But, the results show that law, specifically deference standards, influence the speed of decision-making. A court is substantially quicker when validating rather than invalidating an agency decision, regardless of the panel's affinity for the substance of the agency decision.

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1. INTRODUCTION

The presence of "peer effects", that an ideologically homogenous panel decides a case in a more characteristically partisan way than an ideologically mixed panel, is now a standard finding in studies of appellate decision-making. This pattern is a persistent feature of appellate decision-making, arising in almost all areas of law (Cross 2007; Hettinger, Lindquist, & Martinek 2006; Sunstein et al. 2004). It is even present in administrative law where standards of deference should reduce the influence of judicial policy judgments (Revesz 1997; Cross & Tiller 1998; Miles & Sunstein 2006, 2008).

Despite its near ubiquity, the mechanisms generating peer effects are not well understood. Scholars have advanced numerous potential explanations. These theories include whistleblowing (Cross & Tiller 1998), bargaining or log-rolling (Farhang & Wawro 2004), dissent aversion (Epstein, Landes, & Posner 2011; Posner 2008), and the dynamics of deliberation, such as how individuals in groups react to agreeable (and disagreeable) arguments (Sunstein et al. 2006). Little progress has been made on empirically testing which of these theories best explains the existence and persistence of peer effects. The almost singular focus in the empirical scholarship of judging on the political direction of a court's decision (which is usually proxied by a measure of the prevailing party's identity) partly accounts for the lack of progress.[2] It is not possible to distinguish the competing theories of peer effects by studying this outcome because the theories offer similar predictions about the content of a court's decision.

To overcome this observational equivalence, this article examines a different dimension of judicial performance. In so doing, the article draws inspiration from studies of legislative bargaining. In the seminal model of Romer & Rosenthal (1978, 1979), an agenda-setter advances a policy proposal, which is compared to a default alternative and subjected to an up-or-down vote. A key result is that the agenda-setter enjoys the "power to propose", or the ability to bias outcomes in his favor. In an environment of complete information, voters do not exercise their veto. But when there is asymmetric information about players' preferences, vetoes occur, and they are more likely when the expected distance between the ideal points of the players is greater (Cameron & McCarthy 2004; Cameron 2000). A sizable empirical literature tests the

2 An exception is the work of Choi, Gulati, and Posner who consider outcomes such as number of opinions judges write and the number and source of citations they receive. *See* Choi, Gulati, & Posner (2009a,b, 2010, forthcoming).

predictions of the legislative bargaining models by examining veto probabilities and policy outcomes.[3]

An emerging strand of this literature considers the implications for the speed of legislative decision-making and coalition formation (Diermeier & van Roozendaal 1998; Woon & Anderson forthcoming). The central prediction is that when all players have complete information, agreement is reached immediately, but when there is asymmetric information about players' preferences, there is a signaling game. Players in the game signal that their ideal points are more extreme than they actually are in order to trade the opportunity of immediate acceptance of an offer against the prospect of obtaining an outcome closer to their ideal point. Thus, private information about preferences produces delay. A related prediction is that the greater the divergence in players' preferences, the greater the delay. Empirical tests of this theory have examined the speed of the formation of coalition governments (Diermeier & van Roozendaal 1998; Martin & Vanberg 2003; Golder 2010) and Congressional appropriations (Woon & Anderson forthcoming).

These ideas parallel several of the theories of peer effects in appellate decision-making. Some theories assume complete information about the preferences of judicial colleagues, whereas others imagine a process of reaching agreement when information about colleagues' preferences is private. For example, the whistleblowing hypothesis implicitly assumes complete information about judicial preferences. The mere presence of a judge whose policy preferences are known to her panel colleagues is sufficient to constrain them from disregarding legal doctrine in favor of their desired policy outcome.[4] In essence, whistleblowing is a theory of deterrence. Similarly, dissent aversion is a theory of complete information. It requires a judge to know or accurately predict the panel majority's preferred outcome. When the judge's (private) costs of dissent exceed the benefits, she acquiesces to the majority's view even though she does not agree with the majority opinion. Although the whistleblowing and dissent aversion theories do not rule out substantial time-consuming interactions among judges, they do not necessarily require it.

By contrast, theories of bargaining and deliberation involve a search for agreement when colleagues' ideal points may be private. In the bargaining account, judges are akin to horse-swapping legislators. Offers of proposed

3 Cameron (2000) reviews this literature.

4 "[T]he prospect of a 'whistleblower' on the court – that is, the presence of a judge whose policy preferences differ from the majority's and who will expose the majority's manipulation or disregard of the applicable legal doctrine (if such manipulation or disregard were needed to reach the majority's preferred outcome) – is a significant determinant of whether judges will perform their designated role as principled legal decisionmakers." (Cross & Tiller, 1998, 2156).

trades are advanced and met with counteroffers as judges probe and learn the location of their colleagues' preferences. Haggling (perhaps multiple rounds of it) eventually gives way to agreement and exchange. These negotiations necessarily require judges to interact with each other, and interactions may consume time. Similarly, the deliberation account envisions judges communicating with each other but without the crass wrangling of the bargaining model. Rather, judges in the deliberation model consider the arguments of their peers, and the social context in which the arguments are made influences the judge's thinking. For example, a judge may conform when confronted by a unanimously held view, or she may go to an extreme when discussing an issue with like-minded colleagues (Sunstein et al. 2006). These exchanges of ideas reveal information and may require time-consuming interactions among judges.

When the leading theories of peer effects are grouped in this way, according to their assumptions about the completeness of information, they offer different predictions for the length of time a court will take to render a decision. Interactions among judges that reveal information about their preferences, such as negotiations or deliberations, require time. But reactions to existing knowledge of colleagues' views, as in the whistleblowing and dissent aversion theories, may occur instantaneously. A simple prediction is that if deliberation or negotiation among judges causes peer effects, an ideologically diverse panel will take longer to render a decision than an ideologically homogenous panel. Conversely, if whistleblowing or dissent aversion causes peer effects, panel composition should not correlate with the waiting time for a decision. The relationship between a court's ideological composition and the waiting time for its decision, or loosely speaking the law's delay,[5] thus provides a test of the mechanisms of peer effects.

The article tests this prediction against Miles & Sunstein's (2006, 2008) set of administrative law decisions. These data are well suited to the examination of peer effect mechanisms because they showed that large peer effects are present in the data. Administrative law is a particularly attractive area in which to study judicial collegiality because it also provides the opportunity to examine how legal doctrine influences peer effects. Several studies have found that when the substance of an agency decision aligns with the panel majority's presumed policy preference, the panel is much more likely to validate the agency decision (Revesz 1997; Cross & Tiller 1998; Smith & Tiller 2002). This pattern suggests that deference standards alter the ability of mixed panels to render characteristically ideological decisions. Intuitively, one might predict that this would also have implications for the speed of decision-making. In particular, an

5 William Shakespeare, *Hamlet*, act 3, sc. 1, line 72.

ideologically homogenous panel should review an agency decision in alignment with its policy preference more quickly than an ideologically diverse panel would review an agency decision that is not in alignment with its policy preference. But from the perspective of the bargaining model, there is no reason to expect that a shift in the direction of the deference standard should make the revelation of information more difficult or prolong the strategic interaction among judges. Given the importance of panel alignment to the likelihood of validation, the article also tests whether panel alignment correlates with the speed of decision-making.

To foreshadow the main findings, the baseline estimates show that panel composition does not correlate strongly with the speed of a panel's decision-making. Some estimates even indicate that ideologically diverse panels reach decisions faster than ideologically homogenous ones. These results suggest that whistleblowing or dissent aversion, rather than deliberation or bargaining, better describe the behavior of judges on appellate panels. In addition, the estimates indicate that legal doctrine influences the behavior of judges. A panel is quicker when validating rather than invalidating an agency decision, a pattern that is consistent with a court conducting a less searching review when deferring to an agency. Finally, the estimates show that few judicial characteristics correlate with faster decision-making.

2. DATA

The data examined in this article are drawn from Miles & Sunstein's (2006) set of opinions in which federal appellate courts applied *Chevron* and from their (2008) set of appellate opinions in which courts applied "hard look" or arbitrary and capricious review under *State Farm*. They extracted these opinions from standard legal databases and limited their attention to opinions reviewing the decisions of the Environmental Protection Agency (EPA) and the National Labor Relations Board (NRLB). The "hard look" opinions spanned the period from 1996 to 2006.

Miles and Sunstein coded the ideological content of the agency decision simply and crudely, by reference to the identity of a party challenging it. If an industry group or corporation challenged the agency decision, they coded it as "liberal". By contrast, if a public interest group or labor union challenged the agency decision, they coded it as "conservative". The justification for this coding was that if a corporation or industry group challenged an EPA or NLRB decision, courts likely perceived the agency's decision as liberal. Similarly, if a public interest group or labor union challenged an EPA or NLRB decision, courts likely perceived the agency's decision as conservative. In a small number of decisions, Miles and Sunstein deviated from this coding when it would produce obvious errors (such as when a conservative public interest group, Focus on the Family, challenged an agency decision). Despite the crudeness of this coding, it has the virtue of employing an objective criterion in a mechanical fashion. Miles and Sunstein also gathered information on the identities of the panel members, and following the convention of the literature, they used the political party of each judge's appointing president as a proxy for the judge's presumed ideological preference.

As described above, Miles and Sunstein found similar patterns of ideological voting in the set of *Chevron* opinions as they did in the set of opinions applying "hard look" review. For that reason and to increase the sample size, the two datasets were appended here. A fixed effect for whether the opinion was one in which the court applied *Chevron* was included in the regression analysis below to account for any differences between the two sets of opinions. Also, Miles and Sunstein demonstrated that patterns of ideological voting were nearly symmetric. Democratic appointees were just as likely to engage in stereotypically partisan voting behavior as Republican appointees were, albeit in the opposite direction—validating liberal agency decisions and invalidating conservative ones rather than the other way around.

The units of analysis in this article are opinions rather than individual judge votes as in Miles and Sunstein because the central outcome of interest, the length of time required to reach a decision, varies across cases rather than panel members. To measure time-to-decision, each opinion was retrieved again, and the dates of oral argument and opinion issuance were gathered from the opinion headers. These dates could not be located for every opinion. In earlier years, some circuits did not follow the convention of reporting the dates of oral arguments in the opinion headers, and this caused some attrition from the sample. When oral argument dates were not reported in an opinion's header, the docket sheet in PACER was checked, and some dates were located in this way. But for older opinions in certain circuits, even PACER did not contain the oral argument dates, and inquiries were made at the offices of the court clerk for each of these circuits. They were found for 212 of the 227 decisions in Miles & Sunstein (2006) and for 636 of the 653 decisions in Miles & Sunstein (2008). Most of the opinions for which oral argument dates could not be found were issued by the Fifth and Tenth Circuits. The regression analysis below includes fixed effects for each circuit in an effort to control for these differences in the availability of decision times, but as shown below, the inclusion of fixed effects has little impact on the main estimates.

The Appendix Table A1 presents the means and standard deviations of variables in the sample. It shows that the patterns reported by Miles and Sunstein are present even after excluding the handful of decisions for which decision times could not be measured. Nearly 64 percent of the decisions validated the agency decision, and the panels in nearly 73 percent of the decisions were ideologically mixed in that they were composed of judicial appointees from both political parties. In about 39 percent of the cases, the ideological direction of the agency decision (liberal or conservative) aligned or matched with the presumed ideological preference of the panel majority. (Panels with a majority of Democratic appointees were presumed to possess a liberal preference, and conversely, panels with a majority of Republican appointees were presumed to have a conservative preference.)

The other features of the data are typical of samples of administrative law cases. About 18 percent of opinions were accompanied by a dissent, concurrence, or both. The overwhelming majority of the data involved applications of "hard look" review rather than *Chevron*. In addition, most of the opinions involved the NLRB rather than the EPA. The EPA decisions were tightly correlated with applications of *Chevron* and the D.C. Circuit as one would expect, because EPA decisions often involve prolonged litigations over EPA regulations and interpretations that are usually heard in the D.C. Circuit. This fact makes it difficult to disentangle how an EPA case differs from a *Chevron* case or a D.C. Circuit case. While included as separate control variables, care should be taken in interpreting their coefficients in the regression analysis below.

The Appendix Table A1 also shows that the average number of days between oral argument and issuance of an opinion was nearly 131 days. Like most duration measures, the time-to-decision is skewed with a very small number of cases waiting several years before the courts rendered their decisions. Consequently, the median (ninety-eight days) is lower than the average. Decision times also varied widely across circuits, with cases in the Ninth and Tenth Circuits experiencing average waits (207 and 215 days, respectively) more than twice those in the First Circuit (84 days). The substantial variation across appellate courts is another reason why the regression analysis here includes fixed effects for circuits. By contrast, the number of pages an opinion spanned was less skewed. Its mean of almost thirteen pages was only slightly larger than its median of eleven pages.

The speed with which a court renders a decision may depend on the volume of cases it faces and the resources available to it. Tallies of cases terminated by each circuit court in each year were collected from the Administrative Office of U.S. Courts.[6] The resources of a court consist of the number of judges it

6 http://www.uscourts.gov/Statistics/FederalJudicialCaseloadStatistics/FederalJudicialCaseload Statistics_Archive.aspx.

employs and perhaps more importantly, the talents of the empaneled judges. Information on the number and characteristics of the judges was gathered from the Biographical Directory of the Federal Judicial Center.[7] The Appendix Table A1 shows that the average panel member was sixty-one years old while the average author of an opinion was about fifty-seven years old. The judges averaged about fourteen years of service on the federal bench before hearing the case in the sample. Fewer than 10 percent of the judges in these cases sat by designation from other courts (both district courts and other circuits), but nearly 30 percent of the judges had senior status. Roughly 80 percent of the judges had prior experience in private practice, and about a third had previously served as either a prosecutor or judge (in either a state court or federal district courts). Slightly fewer than half had attended an elite law school.[8] Only a fifth of the judges were female, and an even smaller share (15 percent) were racial minorities.

3. EMPIRICAL STRATEGY

The article's main empirical results are presented in two steps. The first analysis confirms that the patterns of politicized voting reported by Miles & Sunstein (2006, 2008) persist in this article's sample and that they are robust to controls for circuits, years, and some basic case attributes. These regressions estimate the likelihood that the panel validates the agency decision, and the estimating equation takes the form:

$$\Pr\left(Y_{jit}\right) = Align_{jit}\beta_{\text{Align}} + Mixed_{jit}\beta_{\text{Mixed}} + Align_{jit} * Mixed_{jit}\beta_{\text{Align}*\text{Mixed}} + X_{jit}\theta + \alpha_i + \alpha_t + \varepsilon_{jit}. \tag{1}$$

The variable Y_{jit} is a binary variable taking the value one when the court validates the agency decision in case j in circuit i and year t and zero otherwise. The variable $Align_{jit}$ is an indicator variable taking the value one when the agency decision in case j is aligned with the panel's presumed policy preference and zero otherwise. Similarly, the variable $Mixed_{jit}$ is an indicator variable taking the value one when the panel hearing case j consisted of appointees of both political parties and zero otherwise. Vector X_{jit} contains other characteristics of case j such as whether it involved review of an EPA decision or an application of *Chevron*. The terms α_i and α_t are fixed effects for circuit i and year t, respectively, and ε_{jit} is an error term.

7 http://fjc.gov/history/home.nsf/page/judges.html.

8 The concept of "elite" is inherently arbitrary. Here it is defined narrowly as Chicago, Columbia, Harvard, Michigan, NYU, Pennsylvania, Virginia, and Yale.

The second step in the empirical analysis is to examine the relationship between panel composition, alignment, and the length of time it takes a court to render its decision. Hazard models are well suited for this type of analysis. They allow for the analysis of the time until a particular event, where the outcome of interest is the continuous variable of waiting time, rather than the binary variable of the mere occurrence of the event. Hazard models also permit the analysis to include observations that are (right) censored, meaning that the event of interest has not yet occurred at the moment of observation, but censoring is not a feature of these data as all of the decisions in the sample have been rendered. The time-until-decision τ in case j, circuit i, and date t is modeled as a hazard or probability of a decision being reached on the next date conditional on having not been decided before that date:

$$h_{jit}\left(\tau|Align_{jit}, Mixed_{jit}, X_{jit}\right) = h_0(\tau)\exp(Align_{jit}\delta_{\text{Align}}+Mixed_{jit}\delta_{\text{Mixed}} \\ +Align_{jit} * Mixed_{jit}\delta_{\text{Align}*\text{Mixed}}+X_{jit}\gamma+\mu_i+\mu_t). \quad (2)$$

The model includes the same set of explanatory variables as the regression on validation did in Equation (1). Here, the covariates in the exponential term shift the baseline hazard function, $h_0(\tau)$, with positive coefficients indicating increases in the hazard and reductions in the waiting time. A functional form, such as Weibull or Gompertz, for the baseline hazard may be assumed, but this arbitrary assumption may bias the estimates. Instead, estimates from Cox proportional hazard models are reported.

4. RESULTS—STANDARD PATTERN OF PEER EFFECTS IN ADMINISTRATIVE LAW DECISIONS

Miles & Sunstein (2006, 2008) analyzed these data at the level of individual judges, and they found two prominent patterns. The first was that when the agency's decision aligned with a judge's presumed ideological preference, the judge was more likely to vote to validate the agency decision. The second was that this tendency to validate preferred agency decisions was more pronounced when panels were ideologically homogenous. That is, when a panel was comprised exclusively of appointees of a single political party, a member of the panel was more likely to vote to validate a preferred agency decision than when she sat on a panel with at least one appointee from the other political party. Similarly, members of politically unified panels were more likely to invalidate a disfavored agency decision than were members of politically mixed panels. The voting behavior of Democratic and Republican appointees on politically diverse panels was often statistically indistinguishable, and the likelihood of voting to

Table 1. Likelihood of validating agency decisions: mean differences

	All (1)	Agency decision aligned with panel majority's presumed preference (2)	Agency decision did not align with panel majority's presumed preference (3)	Difference of (2)–(3)
(A) All	0.639 (0.022)	0.739 (0.037)	0.575 (0.032)	0.164** (0.037)
(B) Panel was ideologically mixed	0.644 (0.024)	0.702 (0.035)	0.603 (0.031)	0.099** (0.044)
(C) Panel was not ideologically mixed	0.626 (0.032)	0.867 (0.033)	0.510 (0.036)	0.357** (0.054)
Difference of (B)–(C)	0.018 (0.029)	−0.165** (0.037)	0.093** (0.043)	−0.258** (0.070)

Note: Standard errors are in parentheses. An * denotes differences statistically significant at the 10% level, and ** denote differences statistically significant at the 5% level.

validate was not affected by whether the agency's decision aligned with the individual judge's presumed ideological preference.

Table 1 confirms that these patterns persist in the data, even after the loss of a small number of observations due to missing information on decision times. Table 1 presents mean differences in the rates at which courts validated agency decisions according to whether the panel was ideologically mixed and whether the agency decision aligned with the panel majority's presumed ideological preference. Column (1) shows that overall the courts validated these agencies' decisions nearly two-thirds of the time, and on first inspection, ideological diversity on a panel appears to have little impact. Ideologically mixed panels validated at nearly an identical rate as mixed panels (64 percent versus 63 percent). But these aggregate validation rates fail to consider how deference standards both permit and constrain judges from voting in characteristically partisan ways. *Chevron*'s requirements of ambiguity and unreasonableness and *State Farm*'s standard of arbitrary and capricious facilitate validation of agency decisions that accord with a court majority's policy preference, and they impede invalidation of agency decisions contrary to the court majority's preference. Thus, the likelihood of characteristically partisan voting—and the probability that a panel's ideological composition influences that likelihood—should depend on whether the agency decision aligns with the panel majority's presumed preference.

The rest of Table 1 confirms this prediction. The figures in row (A) show that a panel is more likely to validate an agency decision when it aligns with the panel majority's presumed ideological preference, and this difference is sizable: 16 percentage points. The remaining rows demonstrate that the effect of panel alignment is most pronounced among panels comprised of appointees from a single political party. Both ideologically mixed and unified panels validated agency decisions that aligned with the panel majority's presumed preference at higher rates than agency decisions that did not align. The increased rate of validation for aligned decisions is about 10 percentage points on mixed panels, but it is nearly 36 percentage points on unified panels. These validation rates suggest that panels often review agency decisions in characteristically partisan ways but that this tendency is moderated on ideologically mixed panels.

The see-sawing rates of validation in the center of Table 1 further illustrate this point. When an agency decision aligns with a panel majority's presumed preference, the presence on the panel of one appointee from the opposing political party lowers the validation rate by nearly 17 percentage points. But when the agency decision does not align with a panel majority's presumed preference, the presence on the panel of one appointee from the opposing political party raises the validation rate by more than 9 percentage points. In other words, an ideologically unified panel is more likely than a mixed panel to validate an agency decision that aligns with its preference, but it is less likely than a mixed panel to validate an agency decision that does not align with its preference. This see-sawing pattern implies that the effect of panel alignment on the likelihood of validation is about 26 percentage points smaller for mixed panels than politically unified panels. These averages suggest that ideological diversity on panels reduces the frequency of characteristically ideological outcomes.

These differences are quite large, but their magnitudes are not implausible in view of the results reported by Miles & Sunstein (2006, 2008) and in view of the high standards of deference courts apply to agency decisions. *Chevron* requires reviewing courts to give considerable deference to agency interpretations of law. They may invalidate an agency's interpretation only when the agency has violated an unambiguous statute or has given an unreasonable construction of an ambiguous statute. A common justification of *Chevron*'s command is that resolution of ambiguities inevitably involves policy judgments that are best left to political actors rather than courts. Under "hard look" review, a court may invalidate an agency action only when it is genuinely arbitrary or capricious.[9] These standards make it easier for a court to affirm an agency decision and more difficult to invalidate it. This has an implication for the frequency of validation. When an agency's decision aligns with the panel majority's presumed policy

9 The much-cited examples of agency actions meeting these include reliance on factors Congress did not intend the agency to consider, failing to consider an important aspect of the problem, offering an explanation running counter to the evidence, or is so implausible that it cannot be ascribed to a difference in view or the product of agency expertise. *State Farm*, 463 U.S., 43.

preference, the standard of deference makes validation relatively easy. But when an agency's decision does not align with the panel majority's presumed policy preference, the standard of deference makes invalidation more difficult. The sharp increase in validation triggered by alignment is therefore not implausible.

To control for other potential influences on the likelihood of validation, Table 2 reports the results of regressions in the form of Equation (1). The table begins with a relatively parsimonious specification that includes only fixed effects for circuits and years, and each of the subsequent regressions adds progressively more control variables. The main estimates in columns (1) through (5) are from ordinary least squares regressions, and column (6) reports probit results to confirm that the results do not depend on the choice of functional form.

The estimates in the regression in column (1) confirm that the patterns observed in summary statistics are not specific to any particular circuit or year. The coefficient on the indicator for a mixed panel implies that when an agency decision does not align with a panel's presumed preference, a mixed panel has a validation rate that is about 10 percentage points higher than a politically uniform panel. The estimate for an aligned panel implies that an ideologically mixed panel is more likely to validate an agency decision aligned with the presumed ideological preference of its majority than it is to validate one contrary to that presumed preference. Lastly, the estimate on the interaction of these two terms implies that the effect of panel alignment on the likelihood of validation is about 24 percentage points smaller for mixed panels than politically unified panels. This result confirms that the patterns observed in the summary statistics persist after controlling for average differences in validation rates across circuits and years.

The remaining columns demonstrate that this pattern of moderation in the presence of ideological diversity is robust to the presence of other control variables. The coefficient estimates for panel diversity, alignment, and their interaction retain their statistical significance and exhibit remarkably stable magnitudes in the subsequent columns. The results suggest that panel alignment and panel composition are key determinants of the probability of validation, and these patterns are not explained by other observable characteristics of the cases.

The estimates for the other control variables are interesting in themselves and deserve brief mention. Relative to a case involving arbitrary and capricious review, a decision in which the court applied *Chevron* did not have a substantially different likelihood of validation. Yet, which agency was involved in the case closely correlated with the standard of review applied. Cases reviewing decisions of the EPA rather than the NLRB had a probability of validation that was 6–9 percentage points higher. Beginning in column (3), the regressions included controls for the political direction of the panel majorities and the

Table 2. Likelihood of validating agency decisions: regression estimates

Explanatory Variable	(1)	(2)	(3)	(4)	(5)	(6)
(Ideologically mixed panel) × (agency decision aligned)	−0.238** (0.068)	−0.236** (0.068)	−0.246** (0.040)	−0.252** (0.073)	−0.247** (0.077)	−0.312** (0.101)
Agency decision aligned with panel majority's presumed preference	0.344** (0.060)	0.343** (0.054)	0.376** (0.070)	0.366** (0.069)	0.341** (0.071)	0.370** (0.077)
Ideologically mixed panel	0.104** (0.039)	0.0103** (0.039)	0.111** (0.039)	0.124** (0.043)	0.112** (0.045)	0.118** (0.047)
Chevron applied	–	−0.047 (0.037)	−0.034 (0.040)	−0.024 (0.042)	−0.019 (0.041)	−0.013 (0.045)
Agency was the EPA	–	0.073** (0.035)	0.093** (0.037)	0.086** (0.043)	0.066 (0.042)	0.073 (0.049)
Agency decision was liberal	–	–	0.078 (0.063)	0.062 (0.068)	0.043 (0.057)	0.048 (0.067)
At least two Democratic appointees on panel	–	–	−0.037 (0.055)	−0.017 (0.052)	0.011 (0.054)	0.010 (0.058)
(Log) opinion length in pages	–	–	–	−0.085** (0.023)	−0.092** (0.025)	−0.109** (0.028)
Per Curiam opinion	–	–	–	0.149 (0.112)	0.165 (0.126)	0.178 (0.106)
Separate opinion	–	–	–	−0.088 (0.081)	−0.083 (0.090)	−0.083 (0.092)
(Log) cases terminated in circuit per circuit judge	–	–	–	–	0.156 (0.146)	0.179 (0.151)
Total number of circuit judges	–	–	–	–	−0.012 (0.008)	−0.014 (0.009)
Specification	OLS	OLS	OLS	OLS	OLS	Probit (marginal effects)
R^2/pseudo-R^2	0.0838	0.0868	0.0903	0.1101	0.1108	0.0925

Note: $N = 848$. All equations include fixed effects for circuits and years. Standard errors are clustered by circuits. OLS refers to Ordinary Least Squares regression. An * denotes coefficients statistically significant at the 10% level, and ** denote coefficients statistically significant at the 5% level.

agency decision. The coefficients for these variables were small, and none attained statistical significance. Importantly, they suggest that the key patterns—the increase in characteristically partisan decisions when an agency decision aligned with a panel's preference and the moderating effect of ideological diversity on a panel—were symmetric. Panels with a majority of Democratic appointees were just as likely to engage in stereotypically partisan decision-making as Republican appointees were, and these tendencies were just as likely to arise when the agency decision was liberal as when it was conservative.

One of the few covariates with a strong estimated relationship to validation is the length of the panel's opinion. The estimate for the (log) pages of an opinion correlated negatively with the likelihood of validation. The coefficient implies that increasing the length of an opinion by one standard deviation above the mean (from about thirteen to twenty-one pages) was associated with a probability of validation that was 26 percentage points lower. Relative to the mean validation rate of 64 percent, it is a sizable movement. The estimate is consistent with the notion that the standards of deference in *Chevron* and *State Farm* allow courts to conduct a briefer review when validating (rather than invalidating) an agency decision. Similarly, the estimates imply *per curiam* decisions are more likely when a court validates and a judge authoring an opinion separate from the majority (either a concurrence or a dissent) is more likely when invalidating. But neither of these estimates is statistically significant. Lastly, the measures of caseload pressure and judicial staffing—the (log) of cases terminated annually in a circuit per judge and the total number of judges in a circuit—are not statistically significant. These estimates should not be read as indicating that workload and staffing are unimportant. Rather, much of the variation in these variables is across circuits, and the circuit fixed effects thus capture much of the movement in these variables.

5. RESULTS—THE SPEED OF DECISION-MAKING

Tables 1 and 2 confirmed that the peer effects Miles & Sunstein (2006, 2008) observed are apparent when the data are analyzed at the level of cases rather than individual judge votes and that they are robust to a variety of other control variables. This section turns attention to the speed of decision-making and tests which types of mechanisms might explain the peer effects. Table 3 begins by examining average times-to-decision. It shows that the overall average wait for a decision is about 131 days or roughly 4.3 months. In contrast to validation rates, the average length of time a court takes to render a decision varies little by panel alignment or panel composition. All of the differences are small in size, and none are statistically significant.

Table 3. Days until a decision: mean differences

	All (1)	Agency decision aligned with panel majority's presumed preference (2)	Agency decision did not align with panel majority's presumed preference (3)	Difference of (2)–(3)
(A) All	130.862 (14.139)	126.451 (15.801)	133.715 (13.997)	−7.264 (8.564)
(B) Panel was ideologically mixed	125.819 (13.805)	122.721 (14.582)	128.039 (14.167)	−5.318 (8.095)
(C) Panel was not ideologically mixed	144.413 (17.888)	139.280 (23.402)	146.897 (17.642)	−7.617 (17.063)
Difference of (B)–(C)	−18.594 (13.774)	−16.559 (17.865)	−18.858 (14.644)	2.299 (16.522)

Note: Standard errors are in parentheses.

For example, the waiting time for a decision from a politically mixed panel is about eight days shorter when the panel majority's preference aligns with the agency decision than when it does not align. Relative to the average wait time, this is a difference of only 5 percent. The influence of diversity in panel composition is larger. The average wait time for a politically mixed panel is nineteen days shorter than for a politically unified panel, a difference of 15 percent. But the differences in wait times by panel composition remain statistically indistinguishable from zero, and unlike validation rates, they do not vary by panel alignment. Moreover, these differences do not support the hypothesis that peer effects result from asymmetric information about a judge's preferences and the ensuing strategic interactions. In that account, the wait times for decisions from mixed panels should be longer than those from unified panels. Instead, the summary statistics of Table 3 show that they are slightly shorter. Although these differences are not statistically significant, they cast doubt on the deliberation and bargaining explanations.

The remaining tables report estimates from Cox proportional hazard models to control for the potentially numerous factors influencing decision-times. Again, the models estimate the hazard risk or probability that a decision is rendered at a date, conditional on the court not having yet made the decision. Positive coefficients indicate a higher hazard risk or shorter wait for a decision. Table 4 presents the initial estimates. The equation in column (1) includes main effects for panel composition and alignment. It shows that, consistent with the average wait times in Table 3, the hazard for a decision has a weak correlation with panel composition and alignment. When the regression includes an interaction of these two terms, as in column (2), the effects become slightly larger.

Table 4. Speed of decision-making: Cox proportional hazard estimates

Explanatory Variable	(1)	(2)	(3)	(4)	(5)	(6)
Court validated agency decision	–	–	–	0.341** (0.083)	0.281** (0.067)	0.275** (0.062)
(Ideologically mixed panel) × (agency decision aligned)	–	−0.182 (0.162)	−0.227 (0.179)	−0.131 (0.189)	−0.259 (0.187)	−0.236 (0.191)
Agency decision aligned with panel majority's presumed preference	0.073 (0.064)	0.211 (0.170)	0.271 (0.180)	0.134 (0.203)	0.170 (0.168)	0.161 (0.172)
Ideologically mixed panel	0.058 (0.101)	0.119 (0.121)	0.141 (0.128)	0.110 (0.137)	0.229 (0.146)	0.210 (0.151)
Chevron applied	–	–	−0.038 (0.077)	−0.026 (0.077)	0.023 (0.073)	−0.004 (0.078)
Agency was the EPA	–	–	−0.293** (0.074)	−0.325** (0.075)	−0.255** (0.110)	−0.204* (0.111)
Agency decision was liberal	–	–	–	–	−0.035 (0.086)	−0.060 (0.079)
At least two Democratic appointees on panel	–	–	–	–	0.178 (0.126)	0.179 (0.128)
(Log) opinion length in pages	–	–	–	–	−0.557** (0.101)	−0.569** (0.100)
Per Curiam opinion	–	–	–	–	0.026 (0.158)	0.052 (0.174)
Separate opinion	–	–	–	–	−0.250** (0.126)	−0.217 (0.137)
(Log) cases terminated in circuit per circuit judge	–	–	–	–	–	−0.127 (0.347)
Total number of circuit judges	–	–	–	–	–	−0.003 (0.029)

Note: $N = 848$. All equations include fixed effects for circuits and years. Standard errors are clustered by circuits. An * denotes coefficients statistically significant at the 10% level, and ** denote coefficients statistically significant at the 5% level.

The magnitudes of these estimates are most readily evaluated by expressing the implied hazard rate as a ratio of the baseline hazard. For example, when a unified panel reviews an agency decision that aligns with its presumed preference, the hazard probability for issuance of a decision is 24 percent higher than when the agency decision does not align. When reviewing an agency decision that does not align with the panel majority's presumed preference, the chance that an ideologically mixed panel renders its decision on any given day is 13 percent higher than that of a unified panel. Also, the hazard probability for a mixed panel's review of an aligned agency decision is 16 percent higher than that of a unified panel's review of an unaligned decision.[10]

Looking across the columns, these estimates are fairly stable, even when the equations include richer sets of control variables. They appear most sensitive in column (5), when the regression includes controls for panel alignment, panel composition, and the form of the court's decision. There, the coefficient on the interaction of panel alignment and composition and the coefficient for a mixed panel are somewhat larger (in absolute value). On the whole, the estimates for panel composition, alignment, and their interaction imply socially meaningful differences in the speed of decision-making. But none of these estimates is statistically significant. By contrast, the estimates for several other features of these cases imply much larger impacts on the hazard probability, and they attain statistical significance. When compared to these other coefficients in the model, the ideological composition of a panel does not appear to be the most important determinant of the waiting time for a decision. Importantly, even if this difference were taken at face value, its sign is contrary to the prediction that peer effects result from time-consuming interactions among judges.

Another perspective on these estimates is provided by recalling the results in Tables 1 and 2 in which panel alignment and composition predicted the likelihood that the court would validate an agency decision. In the context of these *Chevron* or *State Farm* decisions, validation implies that the court deferred to the agency, and a plausible prediction is that deference is a less time-consuming judicial task. When a court defers and validates an agency decision, as it does when applying *Chevron* or *State Farm*, its task is a less searching and demanding inquiry than when it declines to defer and fully engages in the substance of the agency's decision. Indeed, Table 2 showed that validation correlated with shorter decisions. A less rigorous review should require less of a court's time, and thus, predictors of deference should correlate with faster decision times.

10 To see this, note that $\exp(-0.182 + 0.211 + 0.119) = 1.1595$.

The estimates in Table 4 offer only weak support for this prediction. Panel alignment, composition, and their interaction do not strongly correlate with the hazard probability for a decision, even though all three of these variables predicted substantial increases in the probability a court validates an agency decision. To examine directly the relationship between the hazard for a decision and deference, the regression in column (4) includes an indicator variable for whether the court validated the agency decision. An important caveat is that validation is itself a function of the other right-hand side variables, and thus, its coefficient estimate may be biased. But, as this is perhaps the first examination of waiting times for a decision, it is included to document whether decision times correlate with deference.[11] The estimates in columns (4) through (6) show that validation correlates strongly with the waiting time for a decision. These coefficients are statistically significant and imply that when a court validates (rather than invalidates) an agency decision, the hazard for a decision is between 30 percent and 40 percent higher. This substantial increase is even more impressive because of the presence of other control variables with which validation correlates. At the most general level, the estimates suggest that a court requires more time to conduct a closer review. In cases involving one of administrative law's standards of review, a panel is quicker to validate and slower to invalidate an agency decision.

The results have implications specifically for administrative law. Miles & Sunstein (2006, 2008) advanced no direct claims about the causal effect of *Chevron* deference or "hard look" review on the rates at which courts validated agency decisions. Rather, they discussed how case selection and other complications made it exceedingly difficult to infer from their estimates whether the observed validation rates were too high, too low, or just right. The estimates in column (4) provide a helpful clue as to whether these standards influence the behavior of judges. The substantial reduction in the time-to-decision when a court validates an agency decision (or conversely, the sizable increase when it invalidates) suggests that the deference commands of *Chevron* and "hard look" influence the behavior of courts reviewing agency decisions. The doctrines appear to facilitate decision-making in one instance and to constrain it in the other. The estimates, of course, have limitations. They cannot reveal whether on the whole the decision-making of the courts would be faster or slower in the absence of the doctrines. But loosely speaking, the results are consistent with the idea that the doctrines "matter" in the sense of altering the work of courts.

11 Moreover, when the hazard is estimated as a function of merely validation, *Chevron*, the EPA, and circuit and year fixed effects, the estimated coefficient on the validation indicator is 0.355 (standard error = 0.076). This regression is not shown in order to conserve space in the Table 4, but it suggests that the magnitude of any bias is modest.

Litigation delays have long been a source of complaint about the legal system, but the length of time courts require to render decisions has received little scholarly attention. For this reason, the estimates for the various control variables are worthy of close examination. The regression in column (3) adds controls for whether the case involved an appeal from a decision of the EPA and whether the court applied the *Chevron* framework for questions of statutory interpretation rather than arbitrary and capricious review for a mixed question of law and fact. Again, these variables are highly correlated. The courts employed the *Chevron* framework in about 62 percent of the EPA cases, and appeals from EPA decisions accounted for nearly 75 percent of all *Chevron* applications in the data. But taking the point estimates at face value, they indicate that the waiting time for a decision does not vary with the type of review that the court conducts. The coefficient for *Chevron* is very close to zero. By contrast, the coefficient for an appeal from the EPA is negative and statistically significant. It suggests that an appeal from the EPA has a hazard for a decision that is 75 percent that of an appeal from the NLRB. Many of the EPA cases in the data involve complex regulations and constructions of reticulated statutes such as the Clean Air Act, and the longer decision times for these cases are consistent with their conceptual difficulty.

The equation in column (5) includes controls for the ideological direction of the agency decision, the partisan composition of the panel, and the form of the court's decision. The ideological content of the agency decision has no relationship to the hazard probability, which accords with the intuition that it should take a court just as long to review a liberal agency decision as a conservative one. A panel consisting of at least two Democratic appointees appears to issue decisions slightly quicker (than a panel with at least two Republican appointees), but the estimate is not statistically meaningful.

Per curiam opinions appear to take no longer to produce than signed opinions. But the presence of a separate opinion (a dissent or concurrence) and the length in pages of the decision correlate with the speed of decision-making. When a judge authors a separate opinion, the hazard rate is 22 percent lower than when all judges join a single opinion. Similarly, courts take more time to produce longer opinions. Greater length appears to be a costly in the metric of time. An increase of merely one additional page over the average length of the opinion reduces the hazard probability by 5 percent. The reason why greater length requires more time is not obvious. One possibility is simply that writing is a time-consuming activity, and thus, the more a judge writes, the longer it takes to complete the opinion, and the longer it takes for colleagues to review and agree to it. Another possibility is that length corresponds to unobserved complexity, and richer and more nuanced concepts require both more time to think through and more length to explicate.

The final column of Table 4 adds controls for the circuit's caseload and its tally of judges. The total number of judges in a circuit does not correlate with the speed of decision-making. The coefficient on the caseload measure suggests that, consistent with common intuition, a court with a heavier docket is slower to produce its decisions. But this estimate is not statistically significant.

Table 5 replicates the specification in the final column of Table 4 on various subsamples of the data. The regressions in columns (1) and (2) show that the main results do not depend on whether Democratic or Republican appointees comprise a majority of the panel members. Panels with two Democratic appointees and one Republican appear slower to decide cases when the agency decision is liberal, compared to when three Democratic appointees consider a conservative decision. This difference is not statistically significant, but the point estimates in column (2) suggest that the converse is not true for Republican-majority panels.[12] In addition, panels with at least two Democratic appointees take more time to decide cases involving EPA decisions and less time to issue *per curiam* opinions. The reasons for these differences are not obvious, but the general pattern of results for panel composition and alignment—the absence of a strong relationship to the speed of decision-making—persists for both types of panel majorities.

In columns (3) and (4), the data are split into decisions applying *Chevron* and those applying *State Farm*'s arbitrary and capricious review, and here some differences emerge. Among *Chevron* cases, a court's choice to validate (rather than invalidate) an agency decision does not correlate with the hazard probability. This result is puzzling because many of the *Chevron* cases involve the EPA, and cases involving environmental law tend to be especially complex. Thus, one would expect that deferring to an agency would produce significant time savings for the court. But a possible explanation for this result is that the EPA cases are not just more complex in the subtlety of their legal questions. They also tend to contain multiple issues, each one requiring a separate application of the *Chevron* framework, each with its own recitation of *Chevron*'s steps. By contrast, the NLRB cases were likely to contain only a single issue. This difference in the nature of the cases may make it difficult to detect any impact of deference in the *Chevron* cases.

Another important difference in columns (3) and (4) is that in the *Chevron* cases, a politically mixed panel is associated with a substantially quicker decision time. For example, a mixed panel reviewing an agency decision that does not align with its majority's presumed preference has a hazard probability 58

12 That is, panels with two Republican appointees and one Democrat do not appear slower to decide cases when the agency decision is conservative, relative to when three Republican appointees consider a conservative decision.

Table 5. Speed of decision-making within selected subsamples: Cox proportional hazard estimates

Explanatory Variable	(1)	(2)	(3)	(4)	(5)	(6)
Court validated agency decision	0.400** (0.131)	0.218** (0.080)	−0.016 (0.118)	0.344** (0.064)	0.124 (0.125)	0.364** (0.066)
(Ideologically mixed panel) × (agency decision aligned)	−0.313 (0.288)	−0.032 (0.250)	−0.193 (0.266)	−0.218 (0.227)	−0.679** (0.335)	−0.093 (0.234)
Agency decision aligned with panel majority's presumed preference	−0.016 (0.275)	−0.063 (0.169)	0.102 (0.267)	0.195 (0.174)	0.607** (0.301)	0.035 (0.172)
Ideologically mixed panel	0.174 (0.470)	0.214 (0.170)	0.460** (0.142)	0.192 (0.183)	0.450** (0.185)	0.098 (0.220)
Chevron applied	0.221 (0.084)	−0.047 (0.104)	–	–	0.039 (0.156)	−0.075 (0.149)
Agency was the EPA	−0.622** (0.109)	−0.144 (0.128)	−0.261** (0.102)	−0.251* (0.150)	−0.140 (0.148)	−0.422** (0.172)
Agency decision was liberal	–	–	0.098 (0.191)	−0.061 (0.119)	0.018 (0.154)	−0.167 (0.126)
At least two Democratic appointees on panel	–	–	−0.159 (0.136)	0.166 (0.145)	0.204 (0.176)	0.139 (0.217)
(Log) opinion length in pages	−0.715** (0.078)	−0.510** (0.169)	−0.460** (0.114)	−0.664** (0.106)	−0.432** (0.123)	−0.792** (0.081)
Per Curiam opinion	0.466* (0.244)	−0.128 (0.241)	−0.597** (0.140)	0.407* (0.230)	−0.113 (0.248)	0.289 (0.325)
Separate opinion	−0.360* (0.192)	−0.244 (0.181)	−0.350** (0.129)	−0.247 (0.165)	−0.224 (0.136)	−0.237 (0.205)
Subsample	Panels with Democratic appointee majorities	Panels with Republican appointee majorities	Decisions applying *Chevron*	Decisions applying Arb. & Cap. review	D.C. Circuit decisions	Decisions from other circuits
N	325	523	212	636	291	557

Note: All equations include fixed effects for circuits and years. Standard errors are clustered by circuits. An * denotes coefficients statistically significant at the 10% level, and ** denote coefficients statistically significant at the 5% level.

Table 6. Speed of decision-making controlling for judicial characteristics: Cox proportional hazard estimates

Explanatory Variable	**(1)**	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**
Court validated agency decision	0.281** (0.061)	0.278** (0.058)	0.274** (0.065)	0.293** (0.064)	0.293** (0.062)	0.276** (0.057)
(Ideologically mixed panel) × (agency decision aligned)	−0.247 (0.189)	−0.252 (0.175)	−0.194 (0.154)	−0.284 (0.187)	−0.276 (0.180)	−0.248 (0.159)
Agency decision aligned with panel majority's presumed preference	0.158 (0.158)	0.152 (0.134)	0.093 (0.112)	0.175 (0.170)	0.162 (0.165)	0.157 (0.148)
Ideologically mixed panel	0.228 (0.145)	0.200 (0.140)	0.171 (0.132)	0.244 (0.158)	0.244* (0.148)	0.234* (0.134)
Judicial characteristics						
Age	0.002 (0.003)	−0.005 (0.004)	−0.005 (0.003)	−0.004 (0.011)	−0.006 (0.011)	−0.0001 (0.0126)
Years on the Bench	−0.005 (0.010)	−0.001 (0.010)	0.0003 (0.0108)	0.009 (0.015)	0.008 (0.014)	0.008 (0.012)
Sitting by designation	−0.124 (0.280)	−0.152 (0.303)	−0.180 (0.314)	0.583** (0.281)	0.598* (0.293)	0.657** (0.284)
Senior status	0.166 (0.234)	0.180 (0.209)	0.272 (0.281)	0.035 (0.345)	0.011 (0.327)	−0.088 (0.322)
Prior experience in private practice	–	0.351** (0.128)	0.354** (0.151)	–	0.308 (0.376)	0.405 (0.455)
Prior experience as a prosecutor	–	0.037 (0.108)	0.056 (0.091)	–	−0.174 (0.116)	−0.203 (0.112)
Prior experience as a judge	–	0.101 (0.135)	0.042 (0.116)	–	0.133 (0.139)	0.079 (0.123)
Attended an elite law school	–	0.128 (0.111)	0.167 (0.123)	–	0.270 (0.205)	0.272 (0.196)
Female	–	–	0.272 (0.281)	–	–	−0.377 (0.321)
Minority	–	–	0.228 (0.231)	–	–	0.639* (0.347)
Judicial characteristics of:	Author of majority opinion	Author of majority opinion	Author of majority opinion	Average panel member	Average panel member	Average panel member

Note: $N = 848$. All equations include fixed effects for circuits and years. Standard errors are clustered by circuits. The regressions also include controls for whether the case involves an application of *Chevron*, whether it is an appeal from an EPA decision, whether the agency decision was liberal, whether Democratic appointees were a majority of the panel, the length of the opinion, whether the opinion was *per curiam*, the presence of a separate opinion, the circuit caseload per judge, and the number of judges in the circuit. These estimates are not reported in order to conserve space. An * denotes coefficients statistically significant at the 10% level, and ** denote coefficients statistically significant at the 5% level.

percent higher than that of a politically unified panel. When the mixed panel reviews an aligned agency decision, the hazard is roughly 45 percent higher. By contrast, the estimates among *State Farm* decisions appear similar to the earlier results, implying increases of about 18 percentage points which are not statistically significant.

It is difficult to know whether these contrasting estimates for mixed panels reflect a difference in the mode of the court's legal analysis or a difference in the court. Due to its specialization in administrative law, the D.C. Circuit heard a disproportionate share of the cases in the dataset, and *Chevron* cases in particular were more common in the D.C. Circuit. In the data, 36 percent of the D.C. Circuit's cases involved applications of *Chevron*, whereas they comprised only 19 percent of the cases in other circuits. The final two columns stratify the data by whether the court hearing the appeal was the Court of Appeals for the D.C. Circuit. A comparison of the coefficients on the mixed panel indicator shows the same pattern: a faster decision time when the panel is ideologically diverse, regardless of panel alignment. This pattern is again contrary to the hypothesis that deliberation or negotiation generates peer effects.

A final set of factors that may influence the speed of a court's decisions is the talents and skill of the judges. Table 6 reports hazard models that include progressively more measures of judicial and prior professional experience. These measures include the judge's age, years on the bench prior to hearing this case, whether the judge is currently of senior status, and whether the judge is sitting by designation from another court. Metrics of experience before joining the bench are whether the judge attended an elite law school and whether the judge previously served on a state court or federal district court and whether she has experience as a prosecutor or in private practice. Finally, there are two demographic controls, for race and gender, because recent studies have found that these characteristics may influence decision-making in certain categories of cases (Cox & Miles 2008a,b; Boyd, Epstein, & Martin 2010). These measures were calculated in two ways: first as taking on the values of the author of the majority opinion, and then as averages of the values of all panel members.[13]

Table 6 offers two main lessons. The first is that the presence of these various controls has little influence on the estimates for panel composition, alignment, and their interaction. The relationship between the speed of decision-making and panel composition and alignment appears unaffected by the degree of professional and judicial experience of the court. The second lesson of Table 6 is that the experience and identities of the individual panel members

13 When the opinion was *per curiam*, the values for the author of the majority opinion were assigned zero values.

bear relatively weak relationships to decision times. Only a few of these measures have statistically significant estimates, and many of them imply only small effects on the hazard probability. The estimates indicate that when the author of the majority opinion previously worked in private practice, the hazard is 42 percent higher, and when a panel has one member sitting by designation, the hazard is 22 percent higher. On the whole, the estimates for this set of explanatory variables do not show such strong or consistent patterns that firm conclusions can be reached about which types of judges are quickest in their decisions.

6. CONCLUSION

Peer effects are one of the most persistent regularities of judicial behavior, but their causes are not well understood. This article analogizes the leading theories of peer effects to an emerging strand of the legislative bargaining literature and advances a prediction for the speed of decision-making. Results from a set of administrative law decisions do not support the view that preference-revealing interactions, such as negotiation or deliberation, cause peer effects. Instead, they are consistent with whistleblowing or dissent aversion generating peer effects. The results should be read as an initial exploration of this question. Some of the estimates are sensitive to the particular circuit and type of legal question, suggesting that different mechanisms may operate in different contexts. Analysis of other cases in other areas of law is needed to determine whether the patterns observed here obtain generally.

The speed of court decision-making deserves more study. Litigation delay is widely believed to impose significant social costs, and the court's contribution to delay is essentially unknown. Basic facts about how the speed of decision-making varies across circuits, areas of law, and their intersection with judicial experience await exploration.

Generally, more research is needed on the mechanisms of peer effects, and progress is likely to come both from studies of observational and experimental data. With respect to the former, the examination of dimensions of judicial output other than the much-studied political content of a court's decision may hold the greatest promise for progress. For example, one might predict that deliberation or negotiated compromise improves the content of a decision by accommodating conflicting viewpoints. If panel effects result from deliberation or bargaining, the court's opinion should be of higher "quality" and appeal to a wider audience of future courts. Increasingly, researchers use citation counts as quality measures (Landes et al. 1998; Cross & Lindquist 2006; Choi et al. 2009a,b; Choi & Gulati 2004a,b), despite serious criticisms of them (Abramowicz & Tiller 2009; Choi &

Gulati 2008). With few exceptions (Choi & Gulati 2008; Epstein, Landes, & Posner 2011), their relationship to panel effects has not been closely examined. This possibility is an avenue for future work.

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Appendix

Table A1. Summary statistics

Variable	Mean (standard deviation)	Judicial characteristics of . . .	. . . Author of majority opinion	. . . Average panel member
Court validated agency decision	0.639 (0.481)			
Duration	130.862 (111.440)			
(Ideologically mixed panel) × (agency decision aligned)	0.304 (0.460)			
Agency decision aligned with panel majority's presumed preference	0.393 (0.489)	Age	56.631 (18.663)	61.535 (5.426)
Ideologically mixed panel	0.729 (0.445)	Years on the Bench	13.552 (8.942)	14.449 (4.829)
Chevron applied	0.250 (0.433)	Sitting by designation	0.041 (0.199)	0.063 (0.131)
Agency was the EPA	0.300 (0.458)	Senior status	0.248 (0.432)	0.290 (0.275)
Agency decision was liberal	0.811 (0.391)	Prior experience in private practice	0.785 (0.411)	0.862 (0.198)
At least two Democratic appointees on panel	0.383 (0.486)	Prior experience as a prosecutor	0.305 (0.461)	0.339 (0.269)
		Prior experience as a judge	0.370 (0.483)	0.336 (0.297)
(Log) opinion length in pages	2.392 (0.564)	Attended an elite law school	0.443 (0.497)	0.482 (0.331)
Per Curiam opinion	0.050 (0.217)	Female	0.200 (0.401)	0.203 (0.222)
Separate opinion	0.179 (0.384)	Minority	0.150 (0.357)	0.140 (0.195)
(Log) cases terminated in circuit per circuit judge	184.119 (75.131)			
Total number of circuit judges	18.625 (8.296)			

Note: means and standard deviations in parentheses.