Panel Effects as Social Interactions

Joshua Fischman[1]

Abstract

Recent empirical studies of voting behavior in circuit courts have devoted much effort to documenting and explaining the fact that judges' votes appear to be significantly influenced by their panel colleagues – a phenomenon referred to as "panel effects." While most of these studies examine the relationship between judges' votes and their colleagues' characteristics ("contextual effects"), most theories that seek to explain panel effects assume that judges are affected by their colleagues' votes ("endogenous effects.") Applying insights from the econometrics of social interactions, this article reexamines the data from ten prior empirical studies of circuit court voting behavior as well as one novel data set, characterizing the effects of collegial influence alternatively as contextual and endogenous effects. When characterized as endogenous effects, panel effects are almost always significant, uniform in magnitude, and extremely large – approximately 80% as large as they would be on a court that did not permit dissents. These results pose a difficulty for many theoretical accounts of panel effects, which seek to explain deviations from purely independent voting. I argue that panel effects most likely reflect a strong norm of unanimity on the federal courts of appeals.

I. *Introduction*

Recent empirical studies on decision-making in federal circuit courts have documented the existence of "panel effects" and devoted substantial attention to understanding what causes them. The term "panel effects" – although never precisely defined – has in fact been used to refer to two distinct phenomena (Stephenson 2010). The first usage, which is the primary focus

[1] Associate Professor, University of Virginia School of Law, jfischman@virginia.edu. I thank Renee Birenbaum, Will Carlson, and Joy Williamson for excellent research assistance. I thank the following people for generously sharing their data: Christina Boyd, Lee Epstein, and Andrew Martin; David Law; Nick Linder and John Niles; Thomas Miles and Cass Sunstein; Jennifer Peresie; Richard Revesz; Gregory Sisk, Michael Heise and Andrew Morriss; Cass Sunstein, David Schkade, Lisa Ellman, and Andres Sawicki.

of this paper, refers to the fact that judges' votes appear to be strongly influenced by the characteristics their panel colleagues. Thus, judges are more likely to vote in a conservative direction when empanelled with two Republican appointees than when empanelled with two Democratic appointees.

The second usage of the term "panel effects," which Stephenson (2010) calls the "diversity effect," refers to the fact that ideologically homogeneous panels – those composed of all-Democratic or all-Republican appointees – appear to behave in a more ideologically extreme manner than panels that are ideologically mixed. Although both meanings of the term "panel effects" address the complexity of internal panel dynamics, the first usage analyzes individual votes, while the second analyzes panel dispositions. The first shows only that panel voting is highly interdependent, while studies of the diversity effect make the more ambitious claim that panel dynamics may enable the minority judge to prevail over the majority.

Various theories have been proposed to account for these effects. Several commentators have suggested that panel effects are due to deliberation, whereby judges may change their votes as a result of persuasion by their colleagues. (Edwards 2003; Edwards and Livermore 2009) Posner (2008), however, argues that "judges do not engage in much collective deliberation," and attributes panel effects to "dissent aversion,"[2] arguing that judges in both the majority and the minority may compromise their positions in order to reach a unanimous decision. Cross and Tiller (1998) suggest that panel effects may be the result of judicial "whistleblowing": the threat that a dissenting judge could "blow the whistle" would deter a majority decision that is contrary to precedent. A homogeneous panel, however, would not have a potential whistleblower to curb

[2] Posner's "dissent aversion" closely resembles Revesz's (1997) "dissent hypothesis," the concept of "collegial concurrence" discussed in Sunstein et al. (2006), and Howard's (1981) "consensus norm."

such ideological extremism. Finally, Sunstein et al. (2006) provide a “group polarization” thesis to explain panel effects. Under this theory, deliberation can have a pathological effect when judges are like-minded, and can cause them to take extreme positions. Formal game-theoretic models have been constructed to explain each of these theories of panel effects,[3] and some papers have sought to distinguish among these theories using empirical tests. (Boyd et al. 2009, Kim 2009).

Further complicating the landscape are a variety of empirical studies documenting panel effects, with inconsistent results. While most of these studies do find significant panel effects, Sunstein et al. (2006) find two types of cases that defy the pattern. The first type includes cases characterized by “non-ideological voting,” such as cases involving criminal appeals, punitive damages, or standing, where a judge’s votes does not appear to be significantly influenced by the judge’s political party or by the party affiliations of her panel colleagues. The second type of case involves highly ideologically charged cases, such as cases involving capital punishment and abortion, in which judges have “entrenched views.” In these cases, a judge’s own party is a significant predictor, but the party affiliations of a judge’s panel colleagues are not significant predictors of the judge’s vote.

In the areas of case law that do reveal panel effects, the magnitude of these effects is heterogeneous. Finally, the existence of panel effects may be contingent on how judicial ideology is measured for empirical purposes. Most studies rely on the party of the president who appointed a judge as a proxy for the judge’s ideological disposition, but some studies rely upon “common space” measures of ideology (Giles et al. 2001, Epstein et al. 2007) or examine the

[3] See Spitzer and Talley (2009) for a formal model of panel deliberation, Fischman (2008, 2009) for models of dissent aversion, Kastellec (2007) for a model of whistleblowing, and Glaeser and Sunstein (2009) for a model of “credulous Bayesianism” that can explain group polarization.

impact of a judge's race and gender, and panel effects are not necessarily consistent for each measure. Boyd et al. (2009), for example, reexamine the Sunstein et al. (2006) data set, and find "gender-based" panel effects (meaning that a judge is influenced by the gender of his or her colleagues) in cases involving sex discrimination, but not in other areas. Cox and Miles (2008) find "race-based" panel effects in cases involving the Voting Rights Act, but Farhang and Wawro (2004) do not in employment discrimination cases. However, they do find that male judges (but not female judges) are influenced by the gender of their panel colleagues.

This article argues that much of the confusion about the meaning of panel effects, when they exist, and what causes them, can be resolved with clearer thinking about empirical methodology. In particular, I argue that panel effects can be understood to be part of the broader economic literature on "social interactions" or "peer effects," in which economists have studied how individuals are affected by their neighbors or peers, for example in levels of educational achievement (e.g., Bobonis and Finan 2009; Sacerdote 2001), choice of housing location (e.g., Bayer et al. 2008; Ioannides and Zabel 2008), or propensity to engage in criminal activity (e.g., Bayer et al. 2009; Ludwig and Kling 2007). Although this literature has developed important insights regarding econometric techniques for studying these social interactions, these insights have not been absorbed in the study of panel effects.[4]

Particularly relevant is Manski's (1993) distinction between "endogenous effects," whereby an agent is affected by her peers' *actions*, and "contextual effects," whereby an agent is affected by her peers' *characteristics*. Manski demonstrates that these effects are not distinguishable in standard linear regression models; in nonlinear models, they are distinguishable in theory but difficult to isolate in practice (Manski 2000). Nevertheless, the

[4] One exception is an unpublished manuscript by Cameron and Cummings (2003).

interpretation of regressions involving social interactions may vary substantially depending on whether these interactions are characterized as endogenous or contextual effects. In the literature on panel effects, most empirical models are interpreted as contextual effects, yet most of the theories underlying panel effects assume endogenous effects.

In this article, I reexamine the results from ten previously published studies of circuit court decision-making as well as one novel data set, and report the results on panel effects as both contextual and endogenous effects. These studies include cases in such diverse areas as administrative law, sex discrimination, asylum law, criminal appeals, capital punishment, religious freedom, and abortion. When characterized as endogenous effects, I find that panel effects are almost uniformly significant: with the exception of the abortion data, which includes a small and highly selective group of cases, endogenous effects are always significant at the 1% level. More importantly, the endogenous effects are large and surprisingly consistent in magnitude. In most of the data sets, a universal coefficient of 0.4 in a linear regression model can explain the impact of a colleague's vote on a judge's decision: each colleague who votes in the liberal direction increases a judge's liberal voting probability by 40%. Furthermore, the size of this effect is consistent irrespective of the measure of ideology that is employed in the regression. By contrast, when panel effects are characterized as contextual effects, as in most prior studies, the magnitude and significance of panel effects varies substantially both by case type and by ideology measure.

To interpret the magnitude of these endogenous effects, I compare the results with two theoretical models of voting in collegial courts. On one end of the spectrum is a "purely collective" court in which each decision is announced unanimously. On the other end is a "purely individualistic" court in which each judge arrives at a judgment independently. I show that voting behavior in circuit court panels is much closer to a "purely collective" court.

These results pose some difficulty for some of the theoretical accounts of panel effects, such as whistleblowing or persuasion. These theories start from a presumption of "purely individualistic voting," and seek to account for deviations from this presumption. I argue that it may make more sense to conceive of circuit court panels as "mostly collective," starting from a presumption of unanimity and seeking to explain what motivates judges to dissent in the occasional case.

The organization of this paper proceeds as follows. Section II provides some background on the econometrics of social interactions, and how this framework may be applied to panel voting. Section III provides a brief overview of the various data sets analyzed in this study and discusses when modifications were necessary to make the data set consistent with the empirical methodology employed here. Section IV presents the results. Section V develops the argument that panel effects are most consistent with a theory of collective voting, showing that panel effects are nearly as strong as they would be on a court that did not permit dissents. It also questions whether competing theories can explain the magnitude and uniformity of panel effects. Section VI addresses the "diversity effect" – the empirical regularity that ideologically homogenous panels behave differently from mixed panels – and demonstrates that it is in fact consistent with the theory of collective voting. Section VII discusses some of the implications of the results in this paper.

II. Empirical Framework

a. Econometrics of Social Interactions

In a wide variety of circumstances, it is often the case that members of a group may exhibit similar behavior. This has been widely observed for three-judge appellate panels. Manski (1993) describes three kinds of effects that can account for this similarity:

- "endogenous effects," which cause an individual's behavior to vary with the *behavior* of the group;
- "contextual effects," which cause an individual's behavior to vary with the *characteristics* of the group;
- "correlated effects," wherein individuals with correlated characteristics self-select into groups, or individuals within a group face are subject to similar unobserved influences.

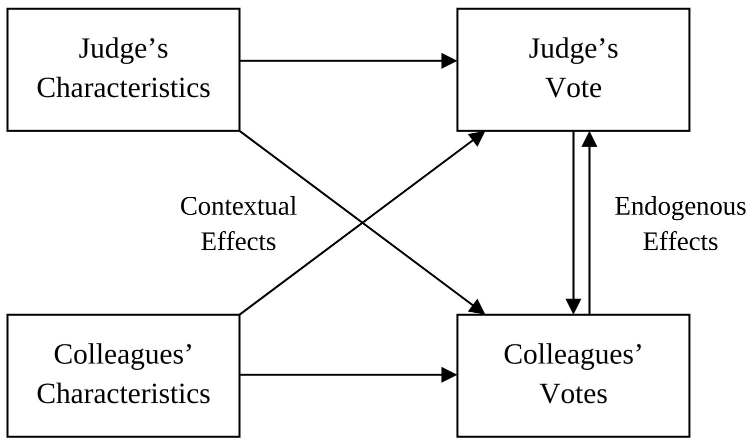
Figure 1 illustrates the difference between endogenous and contextual effects. Both can explain why a judge's votes would be correlated with her colleagues' characteristics; in fact, Manski (1993) shows that they are observationally equivalent in linear regression models. But the causal mechanism is different: with contextual effects, a judge's vote is directly affected by her colleagues' characteristics, irrespective of their votes; with endogenous effects, the colleagues' characteristics predict their votes[5], which in turn have an influence on the judge's vote.

The best way to distinguish between these two effects is to consider whether a variable is determined during the disposition of the case or outside the period of observation. Characteristics such as race, gender, and party of appointment are all fixed at the moment a judge

[5] I avoid saying that judicial characteristics are directly causal. It does not make sense to say that a judge's vote is *caused* by the number of X chromosomes she has, the amount of melanin in her skin, or the president who appointed her. To the degree that gender, race, and party of appointing president have predictive power, it is as *proxies* for ideology – the judge's propensity to decide cases in favor of particular classes of plaintiffs.

assumes the bench, and are therefore contextual. On the other hand, suppose that one judge is particularly persuaded by an argument in the plaintiff's brief. Under a theory of contextual effects, this would have no impact on the votes of the judge's colleagues. If the effects are endogenous, however, then *any* influence on a judge's vote, including the plaintiff's argument, will also affect his colleagues.

Figure 1: Contextual and Endogenous Effects in Panel Voting



If panel effects were entirely attributable to contextual effects, then it would be easy to estimate the magnitudes of these effects, since the causation is clearly unidirectional. Endogenous effects, however, result in two-way causation. Any influence that might make judge *i* more likely to vote in the liberal direction would also make judges *j* and *k* more likely to do so. Since judge *j*'s and *k*'s votes also affect judge *i*, this would, reinforce the original effect on judge *i*, and so on. Thus, any influence on any judge's vote would result in a self-reinforcing feedback loop among all the judges of the panel.

Many of the proposed explanations of panel effects are exclusively or primarily theories of endogenous effects. For example, "dissent aversion" is an endogenous effect: a judge's willingness to vote in a particular direction is affected by the other judges' *votes*. Under this theory, judges' votes may well be correlated with their colleagues' characteristics, but only insofar as these characteristics predict the colleagues' votes. Similarly, "whistleblowing" is a theory of endogenous effects: under this theory, the majority is only influenced by the minority judge's actions. Knowing whether the minority judge is willing to dissent is sufficient; the minority judge's characteristics do not add any additional predictive value.

Although many studies of panel effects employ language suggestive of contextual effects, none of the theories of panel effects is cast purely in terms of contextual effects. For example, Peresie (2005) shows that male judges are influenced by the presence of a female judge on a panel. One of her proposed explanations for this effect is that "male judges defer to female judges because [they] view them as more credible and persuasive in gender-coded cases." Under this theory, both the female judges *actions* (their willingness to support a plaintiff) and their *characteristics* (specifically, gender) are jointly affect their male colleagues' votes.

Deliberation-based explanations of panel effects may also plausibly involve both types of effects. Clearly, some of the effect must be endogenous: it is difficult to imagine a colleague persuading a judge to vote for a position that the colleague did not in fact support. However, if liberal judges try more forcefully to persuade their colleagues to favor the liberal position, and likewise for conservative judges, then the judges' characteristics (ideology) might also influence their colleagues' votes. Alternatively, the contextual effects may act in the opposite direction: it is plausible that conservative judges who support a liberal outcome might be most persuasive to their colleagues.

No plausible theory relies solely on contextual effects. It is hard to imagine that a judge's ideology or personal characteristics would affect her colleagues' votes irrespective of the position that the judge actually took in the case. The more important question is whether panel effects are solely endogenous effects (as in the "dissent aversion" and "whistleblowing" theories) or whether they are a combination of endogenous and contextual effects.

In theory, correlated effects should not be an issue in judicial panels, due to the random assignment of judges to panels.[6] In this sense, the study of judicial voting in panels is simpler than many studies of peer effects in which group composition is endogenous, and similarity in group outcomes may be due to self-selection. However, correlated effects may arise in subtle ways. First, if cases are settled on account of the composition of the panel, then certain cases may be removed from the data in ways that induce correlation between the characteristics of the remaining cases and the panel assigned to them. In all circuits aside from the D.C. Circuit, panel composition is announced shortly before oral argument (Revesz 2000, Jordan 2007), once most of the litigation costs have already been incurred. The degree to which this late panel announcement affects settlement is still an open question; Wagner and Petherbridge (2004) provide anecdotal evidence that early announcement of panel composition may induce settlement in some instances.

In addition, correlated effects may be an issue in studies that exclude unpublished opinions. Since the decision whether to publish is made by the judges on the panel, it may well be affected by the judges' characteristics. Furthermore, the decision not to publish may well be a compromise among judges who disagree about the correct outcome (Law 2003; Wald 1999).

[6] It is worth noting that assignment is only random within a particular circuit and time period. Boyd et al. (2010) argue against relying on random assignment but are unspecific as to the reasons, citing only the difficulty of inter-circuit comparisons and the fact that senior judges might remove themselves from the pool for certain broad classes of cases. The key question is whether there might be correlation between the characteristics of a case and the characteristics of the judges assigned to it.

Thus, by excluding unpublished opinions, empirical researchers may be introducing correlated effects in a setting where they were initially absent.

The same problem may arise if cases are selected on the basis of an endogenous characteristic. For example, Sunstein and Miles (2006) examine circuit court cases that apply *Chevron U.S.A., Inc. v. Natural Resources Defense Council.*[7] Although they do not state explicitly how they collected their cases, a search for “Chevron” would only include cases in which the panel opinion chose to cite *Chevron* explicitly. Yet many cases involving agency deference do not include citations to *Chevron* (Eskridge and Baer 2008). Unless the decision to cite *Chevron* is uncorrelated with the characteristics of the judges, the authors’ selection criteria could have introduced correlated effects. This could easily be remedied in a number of ways, for example, by selecting cases in which the parties’ briefs cited *Chevron*.

The discussion below assumes an absence of correlated effects. In Section IV, however, I reanalyze data sets from previous studies of panel effects, some of which exclude unpublished opinions or select cases on the basis of endogenous characteristics. It is possible, therefore, that the some of the interaction effects found in these data sets may in fact be attributable to correlated effects. The degree to which this might distort the findings in these studies is outside the scope of this paper, but is a worthwhile question for future research.

b. Specifications for Contextual and Endogenous Effects

Although most theories of panel effects conceive them primarily as endogenous effects, most empirical studies estimate them as though they were contextual. While it would be difficult

[7] 467 U.S. 837 (1984).

– if not impossible[8] – to separately identify endogenous and contextual effects in panel voting, the magnitude and significance of the effects may vary substantially depending on how they are characterized.

In this section, I discuss several models that can be used to estimate the significance and magnitude of panel effects. The first assumes purely contextual effects. While this assumption is the least plausible, it is most consistent with the prior literature. The remaining models assume purely endogenous effects.

i. Contextual Effects

The contextual effects model assumes that each judge is influenced by the characteristics of her panel colleagues. Consider a panel consisting of judges $i, j,$ and k, who are deciding case t. Let v_{it} be a dichotomous variable that denotes the vote of judge i in case t, where $v_{it} = 1$ will typically represent the "liberal" outcome and $v_{it} = 0$ will represent the "conservative" outcome. Let p_{it} represent characteristics of judge i in case t, such as whether the judge was appointed by a Democratic president, or the judge's gender of race. In most regressions, p_{it} will be a scalar, but in some cases, it will be a vector. Finally, let x_t be a vector containing a constant term in addition to characteristics of case t, such as the direction of the lower court opinion or the type of claim that is presented on appeal. As long as cases are randomly assigned to panels, it is not necessary to include case characteristics to estimate the influence of judge characteristics.

[8] Manski (1993) equivalence result only holds for linear models. In theory, it could be possible to separately identify endogenous and contextual effect by exploiting non-linearities in the regression model, but this is difficult without a priori knowledge of the functional form. Brock-Durlauf (2007) propose a semi-parametric estimator that can separately identify both endogenous and contextual effects, but their estimator requires greater variation in the regressors than is available in the panel voting data.

Equation (1) provides a standard linear model of contextual effects, which employs a similar specification as most existing studies of panel effects.[9] The first term in each equation captures the influence of each judge's own characteristics on each judge's own vote. The second term captures the influence of the panel colleagues' characteristics; when p_{it} represents party of appointment, for example, the term inside the parentheses represents the number of Democratic panel colleagues. The third term captures the influence of case characteristics on each judge's vote. In this regression model, it does not matter whether p_{it} is a scalar (representing a single characteristic) or a vector (representing multiple characteristics).

$$\begin{aligned} v_{it} &= b_{own}p_{it} + b_{other}\left(p_{jt} + p_{kt}\right) + b_{case}x_t + \varepsilon_{it} \\ v_{jt} &= b_{own}p_{jt} + b_{other}(p_{it} + p_{kt}) + b_{case}x_t + \varepsilon_{jt} \\ v_{kt} &= b_{own}p_{kt} + b_{other}\left(p_{it} + p_{jt}\right) + b_{case}x_t + \varepsilon_{kt} \end{aligned} \tag{1}$$

Although Equation (1) lists three equations – one for each vote in a case – the model can be estimated using ordinary least squares regression, treating each equation as a separate observation. However, the error terms ε_{it} will generally not be independent, and some care must be taken in computing correct standard errors. (See subsection (c) below.)

ii. Endogenous Effects – Simultaneous Equations Model with a Single Judicial Characteristic

Equation (2) provides a model of endogenous effects. It looks similar to equation (1), except that the *votes* of each judge's panel colleagues are on the right-hand side of each equation, instead of their *characteristics*. Note that the votes of the three judges are determined

[9] Most studies of panel effects that report regression results use logit or probit models, which are specifically suited to dichotomous dependent variables. Comparison of endogenous and contextual effects is far more difficult in a non-linear model. For a comparison between the linear model used here and other models used in the literature, see subsection (e) below.

simultaneously in this model. Since the votes of the panel colleagues are endogenous variables, this model cannot be estimated consistently by ordinary least squares.

$$\begin{aligned} v_{it} &= \beta_{own} p_{it} + \beta_{other}(v_{jt} + v_{kt}) + \beta_{case} x_t + \varepsilon_{it} \\ v_{jt} &= \beta_{own} p_{jt} + \beta_{other}(v_{it} + v_{kt}) + \beta_{case} x_t + \varepsilon_{jt} \\ v_{kt} &= \beta_{own} p_{kt} + \beta_{other}(v_{it} + v_{jt}) + \beta_{case} x_t + \varepsilon_{kt} \end{aligned} \tag{2}$$

The coefficient β_{other} will always be a scalar. Assume for the purposes of the current discussion that p_{it} is a scalar, representing only a single judicial characteristic, so that β_{own} is also a scalar.

Equation (2) may be estimated by solving for a reduced form or by using instrumental variables for the endogenous colleagues' votes. To use the former method, the system of simultaneous equations in (2) may be solved, yielding the following reduced form:[10]

$$\begin{aligned} v_{it} &= \pi_{own} p_{it} + \pi_{other}(p_{jt} + p_{kt}) + \pi_{case} x_t + \tilde{\varepsilon}_{it} \\ v_{jt} &= \pi_{own} p_{jt} + \pi_{other}(p_{it} + p_{kt}) + \pi_{case} x_t + \tilde{\varepsilon}_{jt} \\ v_{kt} &= \pi_{own} p_{kt} + \pi_{other}(p_{it} + p_{jt}) + \pi_{case} x_t + \tilde{\varepsilon}_{kt} \end{aligned} \tag{3}$$

where

$$\begin{aligned} \pi_{own} &= \frac{\beta_{own}(1-\beta_{other})}{(1+\beta_{other})(1-2\beta_{other})} \\ \pi_{other} &= \frac{\beta_{own}\beta_{other}}{(1+\beta_{other})(1-2\beta_{other})} \\ \pi_{case} &= \frac{\beta_{case}}{(1-2\beta_{other})} \end{aligned} \tag{4}$$

[10] For a comprehensive discussion of the properties of the simultaneous equations linear probability model, see Heckman and Macurdy (1985).

Note that the reduced form in equation (3) looks almost exactly like the contextual effects regression in equation (1). In fact, estimating equation (3) by ordinary least squares will yield exactly the same estimates of $\pi_{own}, \pi_{other}, \pi_{case}$ as estimating equation (1) will yield of $b_{own}, b_{other}, b_{case}$.[11] The crucial difference lies in the interpretation of the results. In the contextual effects regression, b_{other} and b_{case} are understood to be directly causal, representing the impact of panel colleagues' characteristics and case characteristics on a judge's vote. In the reduced form of the endogenous effects model, the coefficients π_2 and π_3 have no such interpretation. As shown in equation (4), they are functions of the coefficients from the structural model, $\beta_{own}, \beta_{other}, \beta_{case}$. In order to estimate the coefficients from the structural model (2), it is necessary to solve for $\beta_{own}, \beta_{other}, \beta_{case}$ in terms of $\pi_{own}, \pi_{other}, \pi_{case}$:

$$\begin{aligned}
\beta_{own} &= \frac{(\pi_{own} + 2\pi_{other})(\pi_{own} - \pi_{other})}{(\pi_{own} + \pi_{other})} \\
\beta_{other} &= \frac{\pi_{other}}{(\pi_{own} + \pi_{other})} \\
\beta_{case} &= \left(\frac{\pi_{own} - \pi_{other}}{\pi_{own} + \pi_{other}}\right)\pi_{case}
\end{aligned} \tag{5}$$

Note that even though equations (1) and (3) are identical, the significance of "panel effects" depends upon how they are interpreted. In some data sets analyzed in this paper, the estimate of b_{other} in equation (1) (and hence π_{other} in equation (3)) is statistically insignificant, while the estimate of β_{other}, as given in equation (5), is highly significant. The significance of case characteristics may similarly depend on the model assumptions.

iii. Endogenous Effects – Simultaneous Equations Model with Multiple Judicial Characteristics

[11] Note that in the reduced form, $\tilde{\varepsilon}_{it}$ will be a linear combination of $\varepsilon_{it}, \varepsilon_{jt}, \varepsilon_{kt}$.

Now suppose that p_{it} is a vector, for example, in a regression that simultaneously considers the impact of party and gender on judicial voting. In an endogenous effects model, these characteristics only affect colleagues' votes through the judge's own vote. Thus, there must be a functional relationship between the impact of the judge's party and the impact of the judge's gender on colleagues' votes. Let $p_{it} = (p_{it,1}, \dots, p_{it,m})$ and $\beta_{own} = (\beta_{own,1}, \dots, \beta_{own,m})$. In the reduced form, π_{own} and π_{other} will similarly be $m \times 1$ vectors, and for $l = 1, \dots, m$,

$$\begin{aligned}\pi_{own,l} &= \frac{\beta_{own,l}(1-\beta_{other})}{(1+\beta_{other})(1-2\beta_{other})} \\ \pi_{other,l} &= \frac{\beta_{own,l}\beta_{other}}{(1+\beta_{other})(1-2\beta_{other})}\end{aligned} \tag{6}$$

Thus, the ratio between $\pi_{own,l}$ and $\pi_{other,l}$ must be the same for all l. In general, estimating a vector version of the reduced form in (3) will not guarantee that this ratio remain fixed; as a consequence, there will be l different estimates of β_{other}, each derived from equation (6), using different values of $\pi_{own,l}$ and $\pi_{other,l}$. This approach can be useful for testing the assumptions of the model; if the estimates of β_{other} vary substantially based on the characteristic used to derive them, then the endogenous effects model cannot be correct.

For deriving estimates of the structural parameters, however, the above approach does not perform as well, since the estimates can vary based on which estimate of β_{other} is used. Instead, equations (3) and (6) can be combined to obtain

$$v_{it} = \pi_{own}p_{it} + \frac{\beta_{other}}{1-\beta_{other}}\pi_{own}(p_{jt}+p_{kt}) + \pi_{case}x_t + \tilde{\varepsilon}_{it}, \tag{7}$$

which can be estimated by nonlinear least squares. Since this approach restricts the ratio between $\pi_{own,l}$ and $\pi_{other,l}$ to be constant for all l, it should not be used when preliminary estimates reject this assumption. Using the estimates of π_{own}, π_{case}, and β_{other} derived from (7), it is straightforward to solve for β_{own} and β_{case} using equation (4).

iv. Endogenous Effects – Instrumental Variables Estimators

Alternatively, the system of equations in (2) can be estimated by instrumental variables regression. In this model, each judge's vote is regressed on the colleagues' votes, and the colleagues' characteristics are used as instrumental variables to control for the endogeneity of the votes. Like the first estimation method, this technique requires that panel effects be purely endogenous, so that a judge's characteristics affects his colleagues only through his vote.[12]

When a single judicial characteristic is analyzed, the instrumental variables estimator will be exactly equivalent to the structural estimates derived from the simultaneous equations model [explanation will be added in a later draft]. However, some readers may find the instrumental variables approach more intuitive: the estimated effect of colleagues' votes can be interpreted as a local average treatment effect. I only report instrumental variables estimates when multiple characteristics are used to predict judges' voting behavior. In these regressions, each equation is estimated separately by two-stage least squares.[13]

In regressions only including a single judicial characteristic, I report results from the contextual effects model and the estimates of the structural parameters from the endogenous

[12] Otherwise, the colleagues' characteristics would be correlated with the error term associated with the judge's vote, violating the exclusion restrictions essential to the model.

[13] For greater efficiency, the system of equations consisting of the three votes in each case can be estimated by three-stage least squares. I found the differences in the results to be slight, and therefore do not report any three-stage least squares estimates.

effects simultaneous equations model. When regression include multiple judicial characteristics, I report results from the contextual effects regression and the endogenous effects models estimated by both nonlinear least squares and by instrumental variables.

c. Standard Errors

Some attention must be paid to computing the standard errors properly. Most studies of panel effects treat each judge's vote in each case as an independent observation, thus assuming that the error terms ε_{it} are uncorrelated.[14] This assumption, however, is almost always violated in several ways. First, suppose that case t is a weak case (say, for a liberal plaintiff), in a way that is not captured by the case characteristics x_t. Thus, some unobserved case characteristic will make all three judges more likely to vote in the conservative direction, and hence will result in a positive correlation among the error terms ε_{it}, ε_{jt}, ε_{kt}. In general, any unobserved characteristic of a case which will have a common effect on all three judges will induce correlation in the error terms in a similar fashion.

Second, if the equation estimated is the reduced form of the endogenous effects model given in equation (3), the error terms $\tilde{\varepsilon}_{it}, \tilde{\varepsilon}_{jt}, \tilde{\varepsilon}_{kt}$ will be linear combinations of the error terms in the simultaneous equation model (2). The interdependence of the voting behavior will thus induce additional correlation among the error terms in the reduced form model.

In addition, the error terms associated with a particular judge's votes may be correlated across cases. To illustrate, consider a regression that uses the party of the appointing president as

[14] One exception is Revesz (1997), which accounts for correlation of error terms by judge, although not by case. Revesz used a two-step approach, following Ashenfelter et al. (1995), which estimates fixed effects for each judge and then regresses those effects on the party variable. This has a similar effect as the clustering approach employed here, however, the Revesz approach weights each judge equally whereas the clustering approach weights each vote equally.

a proxy for ideology. If judge i is a liberal Republican, then judge i's votes will generally be more liberal than predicted by the regression model, and hence many of the error terms ε_{it} will be positive. Similarly, if judge j is a conservative Democrat, then many of the error terms ε_{jt} will be negative. Thus, the error terms associated with a particular judge's votes will typically be correlated with each other.

Ignoring these sources of correlation will typically (though not always) cause studies to underestimate standard errors, and hence overstate the significance of their results.[15] Therefore, in the contextual effects and the simultaneous equations endogenous effects models, all standard errors computed in this paper are clustered at both the case level and the judge level, using the variance estimator of Thompson (forthcoming) and Cameron et al. (forthcoming).[16] The standard errors for all transformed variables given in equations (5), (7), and (8) are computed using the delta method.

d. Judicial Characteristics

The judicial characteristics analyzed here vary among the data sets, depending on the information collected and the relevance of the characteristics to the types of cases being examined. I give a brief overview here.

[15] This can especially be an issue in studies of single circuits, where each judge is observed many times but the number of judges observed is small, as well as studies of the effect of judges' gender, since there are relatively few female judges. Imagine, for example, a study of the impact of judge gender using data from the First Circuit. Such a study might collect data on hundreds of votes by female judges, but they would clearly not be independent observations, since there is only one active female judge on the First Circuit. Treating each vote by Judge Sandra Lynch as representing an independent draw from some hypothetical pool of female judges would clearly overstate the significance of any finding on judge gender. While no existing study makes such an extreme error, distortions may nevertheless be present. The data from Peresie (2005) include 280 votes by female judges, and the data from Boyd et al. (2010) include 170 votes by female judges, but in both cases, approximately half of the votes by female judges can be attributed to ten judges.

[16] Note that even clustering on both case and judge is imperfect. This would still not account for correlation between a judge's vote and the votes of his panel colleagues in other cases, or the correlations among his panel colleagues in different cases.

Party of Appointing President. Each judge is classified on the basis of whether he or she was appointed by a Democratic or Republican President. While admittedly a simplistic measure of judicial ideology, this variable has been demonstrated to robustly correlated with judicial voting behavior across a wide variety of issue areas (Pinello 1999). Its use has become sufficiently standard that it was already coded and tested in all prior studies of panel effects reanalyzed here. I report the effect of the party variable for each of the data sets examined here, and it is uniformly significant.

Gender. A number of recent studies have examined whether there are systematic differences between male and female judges, both in their own voting behavior and whether they are observed to influence panel colleagues (Peresie 2005, Boyd et al. 2010). Some of these studies have found significant differences between the sexes, primarily in cases involving sex discrimination (see Boyd et al. 2010 for a comprehensive summary). In data sets where gender was already coded, I perform separate regressions with gender as a predictive variable.

Previous Work Experience. Some studies have examined whether judges' behavior can be predicted by their prior work experience. In the examination of sentencing cases, I include a variable denoting whether a judge had previously served as a prosecutor.

Voting Rate in Other Cases. In the data sets in which there are a large number of observed votes per judge, I construct a simple measure based on a judge's voting rate in the entire data set. To avoid circularity, I exclude the votes in each case when calculating the judges' voting rates for that case. When a judge has fewer than 20 votes in the data set, I use the average voting rate for judges appointed by presidents of the same party.

For the Fourth Circuit death penalty data set, I also use each judge's voting rate in the criminal appeals data set to predict voting behavior in death penalty cases. Because the data sets do not overlap, the current case does not need to be excluded. The advantage of this method is that I have data on many more criminal appeals cases in the Fourth Circuit (426) than death penalty cases (156), so that each judge's voting behavior can be more accurately predicted.

A judge's voting behavior in comparable cases is a good predictor of a judge's vote in a particular case, and in most instances, this measure is a superior to the others in explanatory power. It does not explain what factors influence a judge's ideology, however, and the magnitude of the effect of the voting rate variable does not have any natural interpretation. Nevertheless, when available, this variable is useful for measuring the magnitude and significance of panel effects, as well as for estimating the impact of panel composition on case outcomes.

Judicial Common Space Scores. This measure, based on the findings of Giles et al. (2001) and adapted by Epstein et al. (2007), has become popular in recent years as an alternative to the party of the appointing president. These scores are derived from the Poole and Rosenthal (1997) ideal point estimates for senators and the Poole (1998) ideal point estimates for presidents. When one or more of a judge's home-state senators are of the same party as the appointing president, the scores account for the influence of senatorial courtesy using the Poole-Rosenthal ideal point estimate for the home-state senator of the same party (or the average, when both senators belong to the same party.)

The use of these scores was advocated by Epstein and King (2002) and has become popular in recent years. However, several studies that compared common space scores with party of appointing president found both measures to be nearly equivalent in their ability to predict judicial voting (Boyd et al. 2010, Fischman and Law 2009, Peresie 2005, Sisk and Heise 2005). Both in terms of statistical significance, goodness of fit, and magnitude of the effect, all of these studies obtained indistinguishable results using the two measures. In data sets for which common space scores were coded, I compared them to party of appointment, and the results were nearly identical.[17] I only report results using party of appointment, since the results are easier to interpret: the coefficient on the party variable represents the difference in voting rate between the average Democrat and the average Republican. In addition, the binary nature of the party variable makes it more suitable for use in the linear probability model.

e. Comparison with Other Models

The use of a linear probability model to examine panel voting is admittedly unusual. The advantage of the linear model in this context is the equivalence of the contextual effects model in equation (1) and the reduced form of the endogenous effects model in equation (3), which facilitates easy estimation of the endogenous effects model and comparison between the two. The primary disadvantages of linear probability models are that they may generate fitted probabilities outside the unit interval and that the linearity restriction is unrealistic for binary outcomes. These shortcomings are ameliorated, however, when the regressors are discrete or bounded (Wooldridge 2001, p. 456). In most regressions I report, the regressors are dichotomous (such as whether a judge is a Democrat) or trichotomous (such the number of Democratic panel colleagues). In addition, linear probability models have been shown to

[17] This should not be surprising, since the measures are highly correlated. The correlation between common space scores and party of appointing president is 0.86 in the Boyd et al. (2010) sex discrimination data, 0.88 in the Peresie (2005) sex discrimination data, 0.86 in the Law (2003) asylum data, and 0.84 in the Sisk et al. (2004) free exercise data.

perform reasonably well for estimating marginal effects (Angrist 2001), rather than predicting probabilities.

For each of the contextual effects regression in this paper, I also ran a comparable probit regression. In almost all instances, the marginal effects were indistinguishable. In addition, in most regressions, all fitted probabilities are within the unit interval. Nevertheless, the limitations of the linear probability model should be kept in mind when interpreting the results.

Most prior studies of panel effects that report regression results use logit or probit models (e.g., Revesz 1997, Farhang & Wawro 2004, Peresie 2005, Boyd et al. 2010) and use colleagues' characteristics as regressors. Thus, these models should be understood to be models of purely contextual effects. Since these models are not equivalent to the reduced form of an endogenous effects model, they are misspecified if endogenous effects are present.

Boyd et al. (2010) use a semi-parametric matching technique to estimate the causal effect of panel colleagues' gender on judicial voting. This approach pares down the data so that cases in which judges have female panel colleagues have characteristics comparable to cases in which judges have only male colleagues. This matching approach, also employed by Linder and Niles (forthcoming), can facilitate more reliable estimates, but it is only appropriate under the assumption of contextual effects. If the judges' observed votes are equilibrium outcomes determined through panel interactions, then it makes no sense to remove some of the votes from the data.

Fischman's (2009) "consensus voting" model, on the other hand, assumes purely endogenous effects. This approach assumes that judges incur a "cost of dissent" when disagreeing with panel colleagues, but does not consider the impact of judges' characteristics.

The consensus voting model has several advantages over the current framework, particularly that is has more appealing structural foundations and it is superior for predicting voting probabilities. However, it requires a much larger number of observations per judge, and cannot be used to compare the assumptions of contextual versus endogenous effects. Finally, the simplicity of the linear model used here and the ease in implementation and interpretation may recommend it as a useful tool for exploratory analysis.

Note that the linear endogenous effects model in equation (2) assumes the endogenous effects are symmetric: each judge's vote exerts the same effect on each of the judge's colleagues, and vice versa. By contrast, the consensus voting model in Fischman (2009) assumes that the effect is exerted by the majority on the minority, while the more general model in Fischman (2008) separately estimates a majority and minority effect. Note that the simple linear framework given here cannot be used to test competing theories of minority versus majority acquiescence.

III. Data

The data examined in this paper consists of data sets taken from previous studies of panel voting, data sets modified from previous studies, and one newly constructed data set. The data sets borrowed from the previous studies are a sample of the prior literature and are not meant to be exhaustive. They were chosen on the basis of availability, size, the results reported. A summary of the data sets is provided in Table 1.

a. Judicial Review of Agency Decisions

Much of the prior literature on panel effects has focused on administrative law and the role of the judiciary in its supervision of the administrative state. The seminal Revesz (1997) study of EPA cases in the D.C. Circuit was the first paper to identify the presence of panel effects, finding both significant differences between Democratic and Republican appointees and also large effects based on the characteristics of a judge's panel colleagues. Cross and Tiller (1998) found similar patterns in cases implicating *Chevron*. Subsequently, Miles and Sunstein (2006, 2008) found similar effects in cases involving applications of *Chevron* and review of agency decisions under the "arbitrary and capricious" and "substantial evidence" standards.

I reexamine the Revesz (1997) data and the Miles and Sunstein (2006) data on cases involving *Chevron* deference. The Revesz data only involves cases involving the EPA, and all cases are from the D.C. Circuit from 1971 – 1995. The data includes unpublished opinions (Revesz 1999). The Miles and Sunstein data includes challenges involving two agencies, the EPA and the NLRB, from all circuits during the years 1985 – 2005.

In the Miles and Sunstein data, a decision is coded as "liberal" if it affirms the agency decision against an industry group challenge or if it vacates or remands the agency decision in a challenge by a public interest group. The Revesz data codes cases in the same manner, except that it codes claims separately in cases involving cross-challenges. Thus, if a panel affirms an agency decision that is subject to both an interest group and a public interest challenge, the case would be coded as one liberal decision and one conservative decision.

b. Sex Discrimination

Several recent studies have examined gender differences in appellate judging, typically focusing on cases involving sex discrimination, where gender differences would be expected to be more salient. I reexamine the data in two of these studies: Peresie (2005) and Boyd et al.

(2010). The data in the latter case is modified from the sex discrimination data used in Sunstein et al. (2006).

Although the two data sets are similar, and even overlap to some degree, there are a few differences. The Boyd et al. data includes 415 cases from 1995 – 2003, but only includes published opinions. The Peresie data includes 554 cases, including unpublished opinions, from 1999 – 2001. The Boyd et al. data includes sex discrimination cases but not sexual harassment cases; the Peresie data includes both. Both reported that both common space scores and gender were significant predictors of voting, but gender was only significant in the Boyd et al. study when the authors employed semi-parametric matching. In both data sets, outcomes are coded on the basis of whether they provide some relief for the plaintiff in a discrimination case.[18]

c. Criminal Appeals

Sunstein et al. (2006) examined voting behavior of circuit court judges in 23 different areas of law, and found significant political differences as well as significant panel effects in most of these areas. Their analysis of criminal appeals was a surprising exception: they found no significant differences between Democratic and Republican appointees, nor did they find a significant effect from the political affiliation of judges' panel colleagues. This finding was inconsistent with several prior studies that found significant ideological differences among judges in criminal cases. Although it was not the only example in which Sunstein and coauthors did not find political or panel effects, most of the other examples involved small data sets or areas of law in which ideological differences would not be expected.

[18] I do not reexamine the full sex discrimination data set from Sunstein et al. (2006), which did not report results by judge gender. Kim (2009) also examined a subset of this data set, and Farhang and Wawro (2004) reported panel effects in a different set of sex discrimination cases.

Unlike the other data sets reanalyzed here, the Sunstein et al. criminal appeals data required substantial revision. The original data included 1388 cases from the Third, Fourth, and D.C. Circuits from 1995 – 2004. The data for each circuit was collected from a separate webpage that maintained a list of decisions for that circuit. Cases were coded as liberal if they provided some relief to the criminal defendant. The data mostly excludes unpublished opinions, but does not consistently do so.

The original data did not include judge identifiers or cases citations, and only identified cases using the last name of the defendant. Using the last name of the defendant, the year and circuit of decision, and the appointing presidents for the judges on the panel, I was able to identify all but one of the cases.[19] I excluded seven cases from the data,[20] leaving a total of 1381 cases, and corrected a substantial number of miscodings of the judges' appointing presidents.

Nevertheless, there are some inconsistencies in the data that should raise concern. The Third Circuit data contains four times as many cases in 2002 as in any other year, but the success rate for defendants in 2002 is only 9% compared to 32 – 46% for the other years in the data. The reason for this appears to be the inclusion of unpublished opinions in 2002, but not for the other years. In the Fourth Circuit, however, the year 2002 is omitted entirely.

Since a comprehensive coding of criminal appeals cases is outside the scope of the current project, I analyze the criminal appeals data excluding the year 2002. Nevertheless, given

[19] Some defendants with common or misspelled last names were difficult to match, and it is possible in some instances that I identified a different case than the one coded in the original data set. In some instances, I matched cases under the assumption that they were decided close in time to cases with similar numbers according to the original numbering in the data.

[20] Of the excluded cases, three were civil forfeiture cases, one was decided by a quorum of two judges, one was an en banc opinion, one was a duplicate of another case that was already coded, and one was an opinion that was vacated and withdrawn from the Federal Reporter.

the discrepancies revealed in the data, the results from this data set should be interpreted with some degree of caution.

d. Death Penalty

The Sunstein et al. (2006) findings in death penalty cases were another – though less surprising – exception to the overall pattern regarding panel effects. Here, the authors found significant differences between Democratic and Republican appointees, but no significant panel effect. They interpreted this as evidence that "judges vote their convictions" in cases involving capital punishment.

The original death penalty data set consisted of 181 cases from 1995 – 2002, averaging around 23 cases per year. If this number appears low, there is a reason: the authors identified cases in Lexis using the search term "capital punishment" (Sunstein et al. 2004). However, this phrase is not used in many death penalty opinions, and the Sunstein death penalty data appears to omit a majority of relevant cases.

I used alternative search terms to identify death penalty cases in the Fourth Circuit.[21] In just the Fourth Circuit, I found 156 death penalty cases during the same time period, whereas the original Sunstein data had 21 cases from the Fourth Circuit. There are some substantial differences between the Sunstein data and my Fourth Circuit data as well. The Sunstein data has a 76% unanimity rate, far lower than in most of the other data sets examined here; the Fourth Circuit data is 92% unanimous, which is in line with the other data. Also the death row

[21] I searched the Westlaw database "CTA4" using the search string "(capital /s (punishment murder crime offense)) (sentence! /s death) & da(aft 1/1/1995 & bef 12/31/2004)", and excluded cases that did not involve a death sentence or where the possibility of a death sentence was merely speculative. I selected the Fourth Circuit because it has a relatively large number of death penalty cases and because the voting behavior from the criminal appeals data provided a useful proxy for predicting death penalty votes. The other circuits included in the criminal appeals data, the Third Circuit and the D.C. Circuit, did not have enough death penalty cases to be worthy of study.

defendants get relief in 29% of the cases in the Sunstein data, but only 9% in the Fourth Circuit data.

e. Abortion

As with the death penalty cases, Sunstein et al. (2006) also found party effects but no panel effects in cases involving abortion rights. The authors' abortion data set was rather small, consisting of 89 cases from 1982 – 2002, fewer than five cases per year.[22] Opinions in which a judge provided any support for the pro-life side were coded as "pro-life"; otherwise, they were coded as "pro-choice."

f. Immigration

The immigration data come from Law (2005) and contains almost 2000 Ninth Circuit cases involving asylum determinations from 1992 – 2001. All cases are appeals by asylum petitioners from adverse decisions in administrative immigration courts. The data include unpublished opinions; in fact, 92% of the cases are unpublished. Cases are coded on the basis of whether the panel provides any relief to the asylum petitioner. Although the initial analysis did not consider panel effects, Fischman and Law (2009) subsequently demonstrated the presence of strong panel effects in this data, using a simple model of contextual effects.

g. Sentencing

Although one might expect judicial voting patterns in sentencing cases to be representative of criminal appeals more generally, the application of the Federal Sentencing Guidelines present unique issues that might suppress the role of judicial ideology or might

[22] The cases were identified using Lexis search terms "core-terms (abortion) and date aft 1982 and constitutional" and "abortion and constitution!".

implicate it in unusual ways. Most notably, the Supreme Court's precedents in *Blakely v. Washington*[23] and *United States v. Booker*[24] involved unusual coalitions of liberal and conservative justices.

I reexamine the data set in Linder and Niles (forthcoming), which contains 1169 sentencing decisions from 2003 – 2007, all from the Ninth Circuit. The data includes unpublished opinions, which comprise 80% of the case, but excludes decisions that simply ordered remands in light of new controlling precedent (typically *Blakely* and *Booker*.) The original data excluded votes cast by judges sitting by designation, which I subsequently added. Linder and Niles considered the effect of party, gender, and prior experience as a prosecutor, district judge, and state trial judge, but did not explore panel effects. I coded the judges sitting by designation for party, gender, and prosecutorial experience, but did not consider prior judicial experience.

h. Free Exercise Clause

Sisk et al. (2004) examined the impact of judicial characteristics in published cases involving the religion clauses of the First Amendment. Their data set included decisions by district judges and circuit court panels, and circuit courts sitting en banc. Their original data set consisted of 1484 votes, 64% of which were from circuit court panels. Of these cases, 82% involved the Free Exercise Clause. The authors considered multiple ways of jointly coding Free Exercise and Establishment Clause outcomes, since judicial characteristics might have different effects in the two types of cases. Instead, I simply excluded the Establishment Clause cases,

[23] 542 U.S. 296 (2004).
[24] 543 U.S. 220 (2005) .

which constitute a minority of the data set. Given that the original data was not restricted to circuit court panels, the authors did not test for the presence of panel effects.

Retaining only the decisions by three-judge circuit court panels involving the Free Exercise Clause, there were 259 cases involving 777 votes. Sisk et al. consider a multitude of judicial characteristics: party, race, gender, religion, ABA qualification rating, law school prestige, and prior work experience. To keep the analysis manageable, I focused on the following characteristics: party, gender, religion, and law school prestige.

IV. Results

For each data set reexamined here, I report results from regressions that assume alternatively contextual and endogenous effects. The contextual effects estimates are derived from linear probability models as in equation (1). The endogenous effects models vary according to the judicial characteristics used. When a single characteristic is analyzed, the estimates reported are coefficients from the structural model represented by the system of simultaneous equations in (2). When regressions examine multiple judicial characteristics, I report estimates from both nonlinear least squares and instrumental variables regressions.

Note that the magnitudes of the effect of judicial characteristics are not comparable between the two models; in nearly all instances, the coefficient is smaller in the endogenous effects regressions. The reason for this difference is that the endogenous effects model assumes a multiplier effect. Thus, the effect of a judge's party will affect her vote, which in turn affects her colleagues' votes, and further reverberates among all three judges on the panel.

The striking result I find is that the endogenous effects models provide estimates of the impact of colleagues' votes that are highly uniform across most of the data sets. In general, each colleague's vote makes a judge about 40% more likely to vote in the same direction. Equivalently, the impact of colleagues' characteristics in most contextual effects regressions is approximately two-thirds of the impact of a judge's own characteristics.[25]

a. Judicial Review of Agency Decisions

Table 2 provides results for both regressions on the Revesz (1997) and Miles and Sunstein (2006) data sets of cases involving judicial review of agencies. Consistent with both studies, the results reveal significant party and panel effects in all regressions. According to the contextual effects model, a Democratic appointee is about 14% more likely to cast a liberal vote than a Republican appointee in the Revesz data, and 18% more likely in the Miles and Sunstein data. In both data sets, the number of Democratic panel colleagues also has a significant impact on a judge's vote: each Democratic colleague increases the likelihood of a liberal vote by 9% in the Revesz data and 12% in the Miles and Sunstein data, although the effect is more strongly significant in the latter.

The endogenous effects models assume that judges are influenced by colleagues' votes, rather than their characteristics. In these regressions, the effects are strongly significant and nearly identical in magnitude: each liberal vote by a colleague increases the probability of a liberal vote by approximately 40%, in an absolute sense.

Interestingly, the two data sets reveal highly disparate effects of a case being an industry challenge to an agency decision. In the Revesz data, this variable is insignificant; in the Miles and Sunstein data, industry challenges are about 25% more likely to result in a liberal vote, a difference which is highly significant. Agency decisions challenged by industry groups are

[25] This equivalence follows directly from equation (5).

coded as liberal decisions, so a court exercising deference should be more likely to reach the liberal outcome. The Miles and Sunstein data only includes cases involving agency interpretations of statutory language, where courts are most likely to be deferential. In addition, the citation of *Chevron* may itself be endogenous, if panels that are more inclined to defer to agencies are also more inclined to cite *Chevron*.[26]

b. Sex Discrimination

Table 3 displays results for the sex discrimination data from Boyd et al. (2010) and Peresie (2005). The results include both contextual and endogenous effects regressions, examining differences in voting behavior by party, gender, and both characteristics together.

When voting is analyzed by party alone or gender alone, contextual effects regressions from both data sets reveal significant panel effects. Democratic judges are significantly more likely to support plaintiffs in sex discrimination cases than Republican judges, and the likelihood increases significantly for each Democratic colleague. Similarly, female judges are more likely to support plaintiffs, and female panel colleagues also have a significant effect on votes.

When gender and party are examined together, all effects are still significant in the Peresie data, but the contextual effects regression (model 3) does not reveal significant panel effects in the Boyd et al. data. Part of the reason for this is that party and gender are significantly

[26] It is also possible that the discrepancy between the Revesz data and the Miles and Sunstein is due to changes in deference over time, since the Revesz data covers an earlier time period. I reran the regressions on the Revesz data including an interaction term for (Industry Challenge) × (Post *Chevron*), which was not significant. Nevertheless, it is possible that with a larger data set that spans the pre- and post-*Chevron* eras might reveal changes in deference over time. Another possibility is that the discrepancy is due to coding differences between the two data sets.

correlated during the period in question.[27] As a result, the effect of gender appears smaller when both characteristics are included together.

In all endogenous effects regressions, the effect of colleagues' votes is highly significant, and the magnitude of the effect is relatively close to 0.4 whether measured by party, gender, or both. Gender is consistently significant in the Peresie data, but the effect of gender is weaker in the Boyd et al. data.

c. Criminal Appeals

The results from my reexamination of the criminal appeals cases contradict the findings of Sunstein et al. (2006). The results displayed in Table 4 reveal significant differences between Democratic and Republican appointees: Democratic appointees are approximately 6% more likely to favor criminal defendants than Republican appointees, and each Democratic colleague makes a judge 4% more likely to support a defendant. Note that this result holds even in the contextual effects regression, which is comparable to the methodology used by Sunstein and coauthors.[28]

Because of the large number of observations per judge, I also use each judge's voting rate in other cases as an independent variable. Not surprisingly, the results are highly significant: a judge's voting record in other cases is an effective predictor of a judge's vote. The results also show that each judge is strongly affected by his colleagues' voting records. When sufficient

[27] In the Peresie data, 73% of votes cast by female judges were cast by Democratic appointees, whereas only 40% of votes cast by male judges were cast by Democrats. In the Boyd et al. data, 68% of female votes were from Democratic appointees, compared to 32% for male judges.

[28] The panel effects are in fact significant even in the original data set with the year 2002 included, as long as year and circuit dummy variables are included in the regression. Sunstein and coauthors only compared voting rates by panel composition without controlling for year or circuit.

observations are available, judges' voting rates are highly effective for testing the presence of panel effects.

Turning to the endogenous effects regressions, the results once again fit the same pattern: panel effects are strongly significant, and each judge's vote increases the likelihood of a pro-defendant vote by approximately 0.4. The coefficient is estimated at 0.39 when using party and 0.43 when using voting rate, suggesting that the magnitude of endogenous effects is robust to the method by which it is measured.

d. Death Penalty

Table 5 reports results from the death penalty cases examined by Sunstein et al., as well as from my data set of death penalty cases in the Fourth Circuit. The estimates from these two data sets appear quite different. In the Sunstein et al. data, there are significant party effects in the contextual effects regression, but the number of Democratic colleagues has an insignificant effect. In the Fourth Circuit data, however, a judge's own party is significant, as is the number of Democratic panel colleagues: Democratic appointees are approximately 13% more likely than Republicans to grant some form of relief to death-row defendant, and each Democratic panel colleague increases the likelihood of a pro-defendant vote by about 8%. The effect of panel colleagues is even more significant when measured using the judges' voting rates in criminal appeals cases, but not when using votes in other death penalty cases.[29]

[29] The voting rate in other death penalty cases has weaker explanatory power than even the party variable in these regressions. One reason why the death penalty voting rate works poorly may be that pro-defendant votes are rare – comprising only 7% of all votes in the data – and so far more observations per judge may be necessary for a judge's voting rate to be an accurate predictor.

When interpreted as endogenous effects, the results once again differ between the two data sets. In the Sunstein data, the effect of other colleagues' votes is not significant, and the standard error is quite large. While this data does not provide any evidence of panel effects, it hardly provides conclusive evidence that judges' views are "entrenched." In the Fourth Circuit data, the effect of colleagues' votes is significant at the 1% level in all endogenous effects regressions (models 6-8), whether measured by party, voting rate in death penalty cases, or voting rate in criminal appeals. The endogenous effects regressions estimate that each colleague's pro-defendant vote increases the likelihood of a pro-defendant vote by a range of 0.3 – 0.35. This estimate is somewhat smaller than the effects in other cases, although the standard errors here are too wide to conclusively show that the difference is significant.

e. Abortion

In the abortion cases, my results are largely in accord with the conclusions of Sunstein and coauthors. The contextual effects regression results, displayed in Table 6, merely replicate their findings: Democratic judges are about 20% more likely to decide in favor of abortion rights than Republicans. The number of Democratic panel colleagues does not have a significant effect on a judge's vote.

Panel effects are still insignificant in the endogenous effects regression. The only difference is that the standard errors appear to be substantially wider. Although the insignificance of panel effects in these cases is notable, the estimates are hardly precise enough to preclude the possibility of even large endogenous effects.

This data set was the only data set analyzed that did not reveal significant panel effects in the endogenous effects regression. Given the polarizing nature of the abortion debate, perhaps this is not surprising. It should be kept in mind, however, that this is a small and highly selective group of cases, consisting of only 89 cases over a 20 year period. It is possible that the lack of statistical significance is due to the small sample, rather than to anything unique about voting in abortion cases.

f. Immigration

Consistent with the previous analysis in Law (2005) and Fischman and Law (2009), the results in Table 7 reveal strong party and panel effects in asylum cases. Democratic appointees favor asylum claimants 13% more often than Republican appointees, and each Democratic panel colleague increases the likelihood of a pro-asylum vote by about 8%. Panel effects are also strong when measured by voting rate: judges are significantly more likely to favor asylum claimants when empanelled with judges who are typically more sympathetic to asylum claims.

In the endogenous effects model, the regressions using party and voting rate both yield similar estimates. Consistent with the results in the other data sets, each pro-asylum vote by a panel colleague increases the probability that a judge will cast a pro-asylum vote by about 0.4.

g. Sentencing

The regressions on the sentencing data, which are reported in Table 8, examine three judicial characteristics – party, gender, and whether a judge was formerly a prosecutor – as well as the judge's voting rate in other sentencing cases. Party has a small effect on voting that is significant at the 10% level: Democrats are about 4% more likely than Republicans to grant relief to defendants in sentencing cases. However, panel effects are not significant when measured as contextual effects. When examined separately, gender and prosecutorial experience are insignificant predictors of a judge's own vote or of colleagues' votes. When party, gender, and prosecutorial experience are examined jointly (in model 5), the prosecutor variable and the number of Democratic panel colleagues become weakly significant. This appears to be due to the fact that former prosecutors in this data are more likely to be Democrats[30]; because the party and the prosecutor variables have opposite effects, they are both more precisely estimated in a joint model.

The voting rate variable (in model 4) reveals strong panel effects. Thus, a judge is significantly more likely to provide relief to a defendant in a sentencing case if empanelled with colleagues who are generally sympathetic to sentencing claims. The difference between the estimation of panel effects in model 4 and in the other contextual effects regressions demonstrates the importance of relying on a judicial characteristic with strong predictive value.

The endogenous effects regressions using party and prosecutorial experience (models 6 and 8) both show that judges are strongly affected by colleagues' votes. Although the estimates were marginally significant in the contextual effects regressions, they are strongly significant when cast as endogenous effects. Once again, each colleague's pro-defendant vote increases the likelihood of a pro-defendant vote by about 0.4. This is true whether measured using party, prosecutorial experience, or voting rate.

The regressions on gender, while not particularly revealing, are useful for understanding the contrast between the two types of regressions. Model 2 shows that gender has almost no predictive value in sentencing cases: a judge's vote does not appear to be affected by the judge's own gender of the number of female panel colleagues. Some might conclude from this latter result that there are no panel effects associated with gender in sentencing cases. Yet this hardly

[30] 72% of votes cast by former prosecutors were from Democratic appointees, compared to 52% of votes cast by non-prosecutors.

demonstrates that judges are not affected by their colleagues; it merely shows that collegial influence cannot be measured using the judges' gender. In the endogenous effects regression (model 7), the impact of colleagues votes is also insignificant, however, the magnitude of the standard error makes it clear that the regression is truly uninformative.

h. Free Exercise Clause

The Free Exercise cases, reported in Table 9, also reveal significant panel effects in the party regressions. Democratic judges are sympathetic to free exercise claims about 10% less often than Republicans, and each Democratic panel colleague decreases the probability of a vote supporting a free claim by about 6%. When reported as endogenous effects, this regression reveals that each colleague's vote in favor of a free exercise claim increases the probability that a judge will support a free exercise claim by about 0.37.

Model 2 includes additional judicial characteristics considered by Sisk et al. (2004): gender and indicator variables for religion and "elite" law school graduates. In this regression, none of the colleague characteristics are significant (due to mild multicollinearity), but they are jointly significant at the 10% level. Interestingly, the endogenous effects regressions (models 4 and 5) report highly significant panel effects: each colleague's vote in favor of a free exercise claimant increases the likelihood of a pro-free-exercise vote by more than 0.45 in both the nonlinear least squares and the instrumental variable regressions. These results appear to be a bit higher than in some of the other data sets, but it is unlikely that the difference is significant.

V. Discussion

The results here are broadly consistent with prior studies of panel effects, but present a more uniform pattern. While most previous studies have shown that judges' votes are significantly influenced by their panel colleagues, these studies have reported effects of varying magnitudes. Although the results of Sunstein et al. (2006) confirmed the general trend, the authors also identified a number of areas of case law, such as criminal appeals and death penalty cases, in which judges appeared to be impervious to the influence of panel colleagues.

When characterized as endogenous effects, collegial influence is almost universally significant and nearly uniform in magnitude. One simple theory can explain all of the panel effects identified in this paper: each colleague's vote increases the likelihood that a judge will vote in the same direction by about 40%. This rule seems to hold true for high-profile and low-profile cases, for highly contentious as well as for mundane cases, and across all circuits.

Manski's (1993) central insight was that no linear regression model can separately identify contextual and endogenous effects. Consequently, no single regression reported in this paper could have distinguished between these two effects. It is only by analyzing the entire landscape of data sets that a different picture emerges. Collegial influence is robustly explained by a single coefficient when characterized as an endogenous effect, but appears to fluctuate substantially by characteristic and area of case law if characterized as a contextual effect.

To better interpret the magnitude of the endogenous effects, consider the manner in which different appellate courts announce judgments. On one end of the spectrum are "purely individualistic courts" in which each judge is expected to arrive at an independent decision about the proper disposition of the case. In such courts, judges would never deliberate, communicate, or even strategically anticipate their colleagues' decisions. Courts that announce judgments seriatim, such as the British Law Lords before the 1980s (Ginsburg 1990), most English courts before the 18th Century, or the early years of the U.S. Supreme Court (Henderson 2008), might be an approximation of such an "individualistic court," although the practice of seriatim opinions did not preclude deliberation. Judges in some types of tournaments or sporting events, such as boxing or figure skating, might behave in a more purely individualistic manner, but it would strain terminology to characterize a group of such judges as a "court."

In a purely individualistic court, there could be no panel effects. If each judge decides in a vacuum, there is no mechanism by which a judge could be influenced by a colleague's characteristics or actions. Thus, an endogenous effects regression on voting data from such a court should yield estimates of β_{other} that are close to zero. This does not mean that the votes of judges on such a court could not be correlated; the judges may well be in agreement in easy cases. But judges' decisions should not be affected by identities of their panel colleagues.

On the opposite end of the continuum are "purely collective courts" that always announce judgments unanimously. This is the standard practice, for example, in the European Court of Justice and the French Cour de Cassation (Law 2009). In such a court, it may seem peculiar to even refer to a judge's participation in a unanimous judgment as a "vote." If one did so, however, one should expect to find that judges' "votes" are strongly influenced by their panel colleagues. If panels were always comprised of three judges, an endogenous effects regression would always yield an estimate of β_{other} that was equal to one-half.[31]

[31] When votes are always unanimous, it must always be true that $v_{it} = 0.5(v_{jt} + v_{kt})$. Alternatively, it must hold in the reduced form regression that $\pi_{own} = \pi_{other}$, by symmetry. Thus, equation (5) provides $\beta_{other} = 0.5$.

TABLE 10: MAGNITUDE OF ENDOGENOUS EFFECTS IN DIFFERENT TYPES OF COURTS

Type of Court	Magnitude of Endogenous Effect
Purely Individualistic Court	$\beta_{other} = 0$
U.S. Circuit Courts	$\beta_{other} \approx 0.4$
Purely Collective Court	$\beta_{other} = 0.5$

Table 10 compares the estimated coefficients on colleagues' votes derived in this paper with the theoretical coefficients that would obtain in purely individualistic and purely collective courts. Since most regressions reported here yields estimates of β_{other} close to 0.4, the results indicate that circuit court panels are far closer to a purely collective court than they are to an individualistic court.

This simple observation may be helpful for understanding the reasons for the presence of panel effects in the data. In a purely collective court such as the European Court of Justice, one would almost certainly find panel effects. Regression analysis would most likely reveal that judges' "votes" are significantly influenced by the political orientation of their colleagues, and possibly also by their demographic characteristics. But it would be hard to imagine that anyone would find such a result interesting. Certainly, no one would describe panel effects on such a court as "curious" (Posner 2008, p. 31), "striking" (Cross 2007, p. 166), or "surprisingly strong" (Revesz 1997). Similarly, no one would attribute panel effects in a collective court to deliberation or whistleblowing, and no one would bother to construct a formal model to explain them.

Of course, the federal circuit courts are not purely collective – they do permit dissents – but they are far more similar to a purely collective court than many commentators have acknowledged. The mindset that has dominated the literature on panel effects has been to take individualistic voting as a baseline assumption for judicial voting behavior, and construct theories that can explain departures from this baseline. The difficulty with this approach is that the departures from individualistic voting are simply too large and too universal to be attributable to strategic or informational theories of judicial behavior. Instead, it may make more sense to conceive of circuit court panels as "mostly collective courts" – not quite as unitary as the Cour de Cassation or the European Court of Justice, but a close approximation.

For example, if panel effects can be attributed to the threat of reversal due to a potential "whistleblower" on a panel, as hypothesized by Kastellec (2007) and Kim (2009), then one should expect to see larger panel effects in cases that are more likely to be reviewed en banc or by the Supreme Court. In the context in which Cross and Tiller (1998) first formulated the whistleblowing hypothesis – D.C. Circuit review of agency decisions warranting *Chevron* deference – the threat of en banc or Supreme Court reversal is sufficiently tangible to act as a constraint on ideologically inclined judges. But this does not explain why equally strong panel effects are found in immigration and sentencing cases, most of which are decided by unpublished disposition, and very few of which have any reasonable prospect for further review.

The same objection applies to theories of panel effects that rely upon judicial persuasion. It is no doubt true, as Judge Edwards (2003) has written, that "judges have a common interest … in getting the law right, and that, as a result, [they] are willing to listen, persuade, and be persuaded." It does not seem plausible, however, that persuasion could explain the magnitude of panel effects found in the data. In the opinion of Judge Wald (1999), "it is rare that a third judge with a minority point of view is able to 'persuade' the other two to come round." The endogenous effects regressions suggest that each colleague's vote increases the likelihood of a vote in the same direction by 40%; thus two colleagues voting in the same direction would increase the likelihood by 80%. Circuit judges are presumably open-minded, but they also have substantial expertise and strong opinions: it does not seem believable that they are this easily influenced by the opinions of their colleagues. Furthermore, if persuasion were a substantial effect, we should expect to see stronger panel effects in complex cases and weaker panel effects in routine cases. But panel effects appear to be just as strong in asylum and criminal appeals as they are in cases involving review of EPA regulations.

Rather than ask why circuit court judges deviate from individualistic voting, it may be more productive to ask why they deviate from collective voting. In other words, why do judges dissent at all? Circuit judges have reported that dissents are reserved for "extraordinary circumstance[s]" (Pryor 2008) or "disagree[ments] over deeply felt values" (Altimari 1993). Judge Posner writes that judges are averse to dissents because they "fray collegiality" (2008) and because they impose additional work on themselves and their colleagues (Landes and Posner 2009; Posner 1993). Judge Edwards disputes the notion "that there are relatively few dissents in appellate courts because judges prefer to avoid the extra work," but he agrees that "judges do not waste time writing separately just to offer their 'voice' to the court's disposition; [they] dissent when they disagree over matters *that they perceive to be important*" (Edwards and Livermore 2009, emphasis added).

Although dissent may be partially a matter of conscience, it is also widely understood that dissent serves as a signal that the case may merit reconsideration en banc or by the Supreme Court. A dissent may also cause other circuits to hesitate before following the majority's reasoning, which could lead to a circuit split and increase the likelihood of Supreme Court review (Scalia 1994). However, judges are also careful not to dilute the signaling value of their dissents by "crying wolf too often" (Ginsburg 1990).

VI. Homogeneous and Mixed Panels: The "Diversity Effect"

A separate strand of the literature on panel voting focuses on the "diversity effect" (Stephenson 2010) – the differences in decision-making between "homogeneous panels" (those comprised of three judges from the same party) and "mixed panels," which include both Democratic and Republican appointees. A universal feature of these studies is that homogeneous panels appear to be more ideologically extreme than mixed panels: panels of three Republicans appear to be ultra-conservative, and panels of three Democrats appear to be ultra-liberal.

To illustrate, consider the voting patterns in sex discrimination cases reported by Kim (2009), which are reproduced in Table 11. The difference in decision rates between homogeneous and mixed panels is stark. All-Democratic panels reach pro-plaintiff outcomes 30% more often than panels consisting of two Democrats and one Republican. Similarly, all-Republican panels are 12% less likely to reach a pro-plaintiff outcome than a mixed panel with a Republican majority.

TABLE 11: VOTING RATE IN SEX DISCRIMINATION CASES, 1995 – 2002, BY PANEL COMPOSITION

Panel Composition	Number of Observations	Percent Liberal Outcomes
RRR	186	26%
RRD	354	38%
DDR	199	49%
DDD	48	79%

Note that this diversity effect is distinct from the effect of panel composition that is the primary focus of this paper – that judges' votes are affected by their panel colleagues. Here, the unit of analysis is the panel disposition; whether a judge dissented is not relevant.

Some have interpreted the substantial differences in voting rates between mixed and homogeneous panels as evidence that the minority judge exerts a large influence on the panel decision, and conclude that dissent aversion cannot explain the panel effects found in the data. According to this view, the diversity is not only evidence of the fact that voting is independent, but also that the panel majority sometimes acquiesces to the minority. As Kim (2009) presents the argument,

> [J]udges in the ideological majority are observed to vote differently when a judge affiliated with the opposing party is on the panel. Thus, the phenomenon of "panel effects" encompasses *two* distinct effects: first, that judges in the majority vote differently (in a less stereotypically ideological fashion) than judges on a homogeneous panel; and second, that judges in the minority vote differently (still less stereotypically ideologically) than judges in the majority.
>
> * * *
>
> Although … theories of "suppressed dissent" offer a plausible account of why dissents are relatively infrequent on the courts of appeals, they cannot explain panel effects more generally. As noted above, panel composition influences not only the behavior of the minority judge, but the behavior of the judges who comprise the panel majority as well.

Kastellec (forthcoming) makes a similar claim: "If panel effects were small or nonexistent—that is, if judges voted independently of their colleagues' preferences, and voting on panels occurred as predicted by a median voter theorem—we would expect to observe little to no difference between panels with two Democratic [Republican] appointees and panels with three Democratic [Republican] appointees."

Sunstein et al. (2006, p. 14) argue that the differences between homogeneous and mixed panels provide evidence of "group polarization," meaning that "like-minded people move toward a more extreme position in the same direction as their predeliberation views." According to this view, the Democratic judges described in Table 11 might be 49% likely to support sex discrimination plaintiffs if deciding independently. The fact that all-Democratic panels support plaintiffs 79% of the time suggests that these judges become more ideologically extreme when deliberating together.

While the voting patterns seen in these studies are certainly consistent with theories of whistleblowing, persuasion, or group polarization, they are not inconsistent with theories of dissent aversion. In fact, they are not even inconsistent with independent voting; such a pattern could arise in a purely individualistic court. The reason is that party of appointment is at best a crude predictor of judges' voting behavior (Fischman and Law 2009); not all Republicans are conservative, and not all Democrats are liberal. Thus, a panel of three Republicans is more likely to have a majority of solid conservatives than a panel with only two Republicans. Similarly, a panel comprised of three Democrats will be more likely to have a solid liberal majority.

If there is some ideological overlap between the parties – if some Republicans are more liberal that some Democrats, at least on particular issues – then an all-Republican panel will be expected to have a more conservative median judge than a panel with only two Republicans.

Similarly, a panel with only Democrats will have a more liberal median than a panel with two Democrats. Thus, outcome rates should naturally vary with panel composition, even if judges votes are completely independent.

To illustrate, suppose that in sex discrimination cases, Republican judges favor plaintiffs 35% of the time and that Democrats favor plaintiffs 65% of the time. In addition, assume that there are no panel effects: each judge's vote is completely independent of the others' votes, and outcomes are determined by majority vote. Table 12 provides the probability of a panel outcome in favor of a plaintiff.[32]

TABLE 13: PROBABILITY OF PANEL VOTE IN FAVOR OF PLAINTIFF, WHEN REPUBLICANS FAVOR PLAINTIFFS 35% OF THE TIME AND DEMOCRATS FAVOR PLAINTIFFS 65% OF THE TIME, AND VOTING IS INDEPENDENT

Panel Composition	Percent Liberal Outcomes
RRR	28%
RRD	42%
DDR	58%
DDD	72%

These results, while not perfectly matching the statistics from Kim (2009), reveal a similar pattern. Yet, by assumption, the votes were completely independent. If instead we had assumed that all dissents were suppressed, the predictions would have been identical; the predictions regarding panel outcomes do not depend on whether there was a dissenting opinion.

[32] If the probability of a liberal vote is p for a Republican judge and q for a Democratic judge, then the probability of a liberal decision on an RRR panel is $3p^2(1-p)+p^3$ and the probability of a liberal decision on a RRD panel is $p^2(1-q)+2pq(1-p)+p^2q$. The probabilities for DDD and DDR panels can be determined symmetrically.

Certainly, none of this analysis refutes the claim that judges in the minority exert influence on panel decisions, or that group polarization leads some panels to extreme decisions. Having a potential "whistleblower" on a panel may well curb ideological excess on the part of the majority in some cases. A would-be dissenter may occasionally persuade the majority of the merits of her position. Judges could conceivably become more ideologically extreme homogeneous panels deliberate. Yet none of these observed voting patterns should be taken as strong evidence of minority influence: patterns of "extreme" voting by homogeneous panels are perfectly consistent with theories of dissent aversion, and even with strictly independent voting.

VII. Implications

The analysis in the paper has shown that panel effects are more ubiquitous and larger in magnitude than many had realized. In fact, voting behavior in circuit court panels appears to more closely resemble the assumption of perfectly collective voting than the assumption of perfectly individualistic voting.

This has implications for the longstanding debate about the merits of collective versus individualistic voting.[33] Proponents of collective voting argue that a single judgment promotes clarity and allows the court to fashion more coherent doctrinal rules. Some also argue that unanimity bolsters the legitimacy of the courts; in the words of Judge Learned Hand (1958), dissent "cancels the impact of monolithic solidarity on which the authority of a bench of judges so largely depends." Scalia (1994), however, disagrees, noting that "the argument can be made that artificial unanimity … deprives genuine unanimity of the great force it can have when that force is most needed."

[33] See Henderson (2007) and Ginsburg (1990) for in-depth treatments of this debate.

Others have endorsed individualistic voting on the grounds that it promotes judicial accountability. In the words of Fiss (1983), "[W]e insist that each judge – as an individual and as an official – accept full responsibility for his decisions by signing his opinion and disclosing his vote." Judge Edwards (2003), among others, rejects this view. "If the end product looks different from what a judge had in mind at the beginning of the process [of deliberation], that fact reflects the very nature of the group process in which each judge can only contribute to a group product that is ultimately attributable to the court.… This is neither suspect nor tragic, for a judge's job is not 'self-expression' through the law."

Irrespective of one's position in this debate, it should be clear that there is no strict dichotomy between courts in which judges' votes are transparent and courts in which votes are opaque. Courts can permit dissent and still be highly collective: the mere fact that circuit court opinions occasionally include dissents does not mean that each judge is truly "disclosing his vote." A unanimous opinion should not be interpreted to mean that each judge would have independently arrived at the same disposition, let alone followed the same reasoning; such an interpretation cannot be reconciled with the ubiquity of panel effects. A unanimous opinion reveals only that the judges were able to find some common ground, or were able to forge a satisfactory compromise, or perhaps that no judge disagreed strongly enough to make a dissent worthwhile.

Understanding the voting norms in a court is therefore essential to any effort to analyze the determinants of judicial behavior. In a purely individualistic court, where each judge's vote is an accurate reflection of the judge's preferred disposition, these determinants are easily analyzed. Anyone trying to understand the differences between Democratic and Republican appointees, or between male and female judges, could simply compare the voting rates for each group. Similarly, the dissent rate would be a useful (if imperfect) measure of the degree to which case outcomes depend on the "personality" of the judge. An alternative approach would be to compare the voting rates of the most liberal and the most conservative judge; this discrepancy would approximately represent the proportion of cases in which these two judges would disagree.

In a purely collective court, such simple comparisons are clearly not appropriate. The dissent rate is necessarily equal to zero, but no one would seriously claim that this proves that the law is determinate. A judge's "liberal" voting rate does not accurately reflect how often a judge would prefer to support the "liberal" side, since the voting rate includes instances when the judge is outvoted. Similarly, the difference in voting rates between two judges does not accurately measure how often the two judges would disagree, nor does the difference in voting rates between Democrats and Republicans measure the effect of replacing a judge from one party with a judge from the other. In order to correctly understand judges' voting behavior in a purely collective court, it is essential to employ statistical techniques that properly account for the influence of panel colleagues.

When discussing voting data from federal circuit courts, it is essential to remember that voting is still highly collective. Yet it is still common for observers to analyze voting data from circuit courts as though they were purely individualistic. Most notably, some have cited the low dissent rate as evidence that judicial ideology has a limited influence on judicial decision-making in the courts of appeals (Tamanaha 2009; Edwards 1991). Given the highly collective nature of panel voting, however, it should be clear that dissent rates are not meaningful statistics.

In addition, since a judge's voting rate is influenced by collegial interactions, that voting rate is not an accurate indicator of the judge's "true" voting propensity. In general, a judge's actual voting rate will be more moderate than what would have obtained if the judge always revealed her true preference. A conservative judge, for instance, will occasionally be pulled in the liberal direction by liberal panel colleagues, but will less frequently be pulled in the conservative direction by colleagues who are even more conservative. Thus, the judge's voting rate will understate the judge's conservatism. Thus, failure to control for panel effects will cause empirical studies to underestimate the impact of judicial composition on case outcomes (Revesz 1997). This is true for studies that report individual judges' voting rates (e.g., Ramji-Nogales et al. 2007) as well as for studies that use regression analysis to study the effect of various judicial characteristics.

VIII. Conclusion

[To be completed.]

Bibliography

Michael Abramowicz and Emerson H. Tiller, Citation to Legislative History: Empirical Evidence on Positive Political and Contextual Theories of Judicial Decision Making, 38 Journal of Legal Studies 419 (2009).

Frank X. Altimari, The Practice of Dissenting in the Second Circuit, 59 Brooklyn Law Review 275 (1993).

Orley Ashenfelter, Theodore Eisenberg, and Stewart J. Schwab, Politics and the Judiciary: The Influence of Judicial Background on Case Outcomes, 24 Journal of Legal Studies 257 (1995).

Christina L. Boyd, Lee Epstein, and Andrew D. Martin, Untangling the Causal Effects of Sex on Judging, American Journal of Political Science (forthcoming, 2010).

William A. Brock and Steven N. Durlauf, Identification of Binary Choice Models with Social Interactions, 140 Journal of Econometrics 52 (2007).

A. Colin Cameron, Jonah B. Gelbach, and Douglas L. Miller, Robust Inference with Multi-way Clustering, Journal of Business and Economic Statistics (forthcoming, 2010).

Charles M. Cameron and Craig P. Cummings, Diversity and Judicial Decision-Making: Evidence from Affirmative Action Cases in the Federal Courts of Appeals, 1971-1999 (2003) (unpublished manuscript).

Stephen J. Choi and G. Mitu Gulati, Trading Votes for Reasoning: Covering in Judicial Opinions, 81 Southern California Law Review 735 (2008).

Adam B. Cox and Thomas J. Miles, Judging the Voting Rights Act, 108 Columbia Law Review 1 (2008).

Frank B. Cross, Decision Making in the U.S. Courts of Appeals, Stanford University Press (2007).

Frank B. Cross and Emerson H. Tiller, Judicial Partisanship and Obedience to Legal Doctrine: Whistleblowing on the Federal Courts of Appeals, 107 Yale Law Journal 2155 (1998).

Robert H. Dorff and Saul Brenner, Conformity Voting on the United States Supreme Court, 54 Journal of Politics 762 (1992).

Harry T. Edwards, The Effects of Collegiality on Judicial Decision Making, 151 University of Pennsylvania Law Review 1639 (2003).

Harry T. Edwards, The Judicial Function and the Elusive Goal of Principled Decisionmaking, 1991 Wisconsin Law Review 837.

Harry T. Edwards and Michael A. Livermore, Pitfalls of Empirical Studies that Attempt to Understand the Factors Affecting Appellate Decisionmaking, 58 Duke Law Review 1895 (2009).

Lee Epstein and Gary King, The Rules of Inference, 69 University of Chicago Law Review 1 (2002).

Lee Epstein, Andrew D. Martin, Jeffrey A. Segal, and Chad Westerland, The Judicial Common Space, 23 Journal of Law, Economics, and Organization 303 (2007).

Lee Epstein, Jeffrey A. Segal, and Harold J. Spaeth, The Norm of Consensus on the U.S. Supreme Court, 45 American Journal of Political Science 362 (2001).

William N. Eskridge and Lauren E. Baer, The Continuum of Deference: Supreme Court Treatment of Agency Statutory Interpretations from *Chevron* to *Hamdan*, 96 Georgetown Law Journal 1083 (2008).

Sean Farhang and Gregory Wawro, Institutional Dynamics on the U.S. Court of Appeals: Minority Representation Under Panel Decision Making, 20 Journal of Law, Economics, and Organization 299 (2004).

Joshua B. Fischman and David S. Law, What Is Judicial Ideology, and How Should We Measure It?, 29 Washington University Journal of Law & Policy 133 (2009).

Joshua B. Fischman, Decision-Making Under a Norm of Consensus: A Structural Analysis of Three-Judge Panels, working paper available at http://papers.ssrn.com/sol3/papers.cfm?abstract_id=912299 (2008).

Joshua B. Fischman, Estimating Preferences of Appellate Judges: A Model of “Consensus Voting,” working paper available at http://papers.ssrn.com/sol3/papers.cfm?abstract_id=1361348 (2009).

Owen M. Fiss, The Bureaucratization of the Judiciary, 92 Yale Law Journal 1442 (1983).

Micheal W. Giles, Virginia Hettinger, and Todd Peppers, Picking Federal Judges: A Note on Policy and Partisan Selection Agendas, 54 Political Research Quarterly 623 (2001).

Ruth Bader Ginsburg, Remarks of Writing Separately, 65 Washington Law Review 133 (1990).

Edward L. Glaeser and Cass R. Sunstein, Extremism and Social Learning, 1 Journal of Legal Analysis 263 (2009).

James J. Heckman and Thomas E. Macurdy, A Simultaneous Equations Linear Probability Model, 18 Canadian Journal of Economics 28 (1985).

M. Todd Henderson, From Seriatim to Consensus and Back Again: A Theory of Dissent, 2007 Supreme Court Review 283.

J. Woodford Howard, Courts of Appeals in the Federal Judicial System, Princeton University Press (1981).

Samuel P. Jordan, Early Panel Announcement, Settlement and Adjudication, 2007 Brigham Young University Law Review 55.

Jonathan Kastellec, Panel Composition and Judicial Compliance on the U.S. Courts of Appeals, 23 Journal of Law, Economics & Organization 421 (2007).

Jonathan Kastellec, Panel Composition and Voting on the United States Courts of Appeals, Political Research Quarterly (forthcoming).

Pauline T. Kim, Deliberation and Strategy on the United States Courts of Appeals: An Empirical Exploration of Panel Effects, 157 University of Pennsylvania Law Review 1319 (2009).

William M. Landes and Richard A. Posner, Rational Judicial Behavior: A Statistical Study, 1 Journal of Legal Analysis 775 (2009).

David S. Law, Strategic Judicial Lawmaking: Ideology, Publication, and Asylum Law in the Ninth Circuit, 73 University of Cincinnati Law Review 817 (2005).

David S. Law, A Theory of Judicial Power and Judicial Review, 97 Georgetown Law Journal 723 (2009).

Charles F. Manski, Identification of Endogenous Social Effects: The Reflection Problem, 60 Review of Economic Studies 531 (1993).

Charles F. Manski, Economic Analysis of Social Interactions, 14 Journal of Economic Perspectives 115 (2000).

Thomas J. Miles and Cass R. Sunstein, Do Judges Make Regulatory Policy? An Empirical Investigation of Chevron, 73 University of Chicago Law Review 823 (2006).

Jennifer L. Peresie, Female Judges Matter: Gender and Collegial Decisionmaking in the Federal Appellate Courts, 114 Yale Law Journal 1759 (2005).

Daniel R. Pinello, Linking Party to Judicial Ideology in American Courts: A Meta-Analysis, 20 Justice System Journal 219 (1999).

Keith T. Poole and Howard Rosenthal, Congress: A Political-Economic History of Roll Call Voting, Oxford University Press (1997).

Richard A. Posner, What Do Judges and Justices Maximize? (The Same Thing Everybody Else Does), 3 Supreme Court Economic Review 1 (1993).

Richard A. Posner, How Judges Think, Harvard University Press (2008).

Robert Post, The Supreme Court Opinion as Institutional Practice: Dissent, Legal Scholarship, and Decisionmaking in the Taft Court, 85 Minnesota Law Review 1267 (2001).

William H. Pryor Jr., The Perspective of a Junior Circuit Judge on Judicial Modesty, 60 Florida Law Review 1007 (2008).

Jaya Ramji-Nogales, Andrew Schoenholtz, and Philip G. Schrag, Refugee Roulette: Disparities in Asylum Adjudication, 60 Stanford Law Review 295 (2007).

Richard L. Revesz, Environmental Regulation, Ideology, and the D.C. Circuit, 83 Virginia Law Review 1717 (1997).

Richard L. Revesz, Ideology, Collegiality, and the D.C. Circuit: A Reply to Chief Judge Harry Edwards, 85 Virginia Law Review 805 (1999).

Richard L. Revesz, Litigation and Settlement in the Federal Appellate Courts: Impact of Panel Selection Procedures on Ideologically Divided Courts, 29 Journal of Legal Studies 685 (2000).

Bruce Sacerdote, Peer Effects with Random Assignment: Results for Dartmouth Roommates, 116 Quarterly Journal of Economics, 681 (2001).

Antonin Scalia, The Dissenting Opinion, 1994 Journal of Supreme Court History 33.

Gregory C. Sisk and Michael Heise, Judges and Ideology: Public and Academic Debates About Statistical Measures, 99 Northwestern Law Review 743 (2005).

Gregory C. Sisk, Michael Heise and Andrew P. Morriss, Searching for the Soul of Judicial Decisionmaking: An Empirical Study of Religious Freedom Decisions, 65 Ohio State Law Journal 491 (2004).

Matthew Spitzer and Eric Talley, Strategic Information Acquisition and Mixed Judicial Panels, working paper available at http://repositories.cdlib.org/berkeley_law_econ/Fall2009/19.

Matthew C. Stephenson, Statutory Interpretation by Administrative Agencies, in Research Handbook in Public Law and Public Choice, Daniel Farber and Anne Joseph O’Connell eds. (2010).

Cass R. Sunstein, David Schkade, Lisa M. Ellman, and Andres Sawicki, Are Judges Political?: An Empirical Analysis of the Federal Judiciary, Brookings Institution Press (2006).

Brian Z. Tamanaha, The Distorting Slant in Quantitative Studies of Judging, 50 Boston College Law Review 685 (2009).

Samuel B. Thompson, Simple Formulas for Standard Errors that Cluster by Both Firm and Time, Journal of Financial Economics (forthcoming, 2010).

R. Polk Wagner and Lee Petherbridge, Is the Federal Circuit Succeeding? An Empirical Assessment of Judicial Performance, 152 University of Pennsylvania Law Review 1105 (2004).

Patricia M. Wald, A Response to Tiller and Cross, 99 Columbia Law Review 235 (1999).

Jeffrey M. Wooldridge, Econometric Analysis of Cross Section and Panel Data, MIT Press (2001).

TABLE 2: PROBABILITY OF A "LIBERAL" VOTE IN JUDICIAL REVIEW OF AGENCY DECISIONS

Data Set	Revesz (1997)		Miles & Sunstein (2006)	
Panel Effects	Contextual	Endogenous	Contextual	Endogenous
	(1)	(2)	(3)	(4)
	LPM	SEM	LPM	SEM
Judge Characteristics:				
Democrat	0.140***	0.068**	0.180***	0.086***
	(0.053)	(0.032)	(0.029)	(0.017)
Colleague Characteristics:				
Number of Democratic Colleagues	0.091*		0.119***	
	(0.055)		(0.032)	
Colleague Votes		0.394***		0.397***
		(0.074)		(0.034)
Case Characteristics:				
Industry Challenge	0.107	0.022	0.253***	0.205***
	(0.076)	(0.024)	(0.054)	(0.068)
R-squared	0.132		0.257	
Number of Cases	154	154	252	252
Number of Votes	543	543	756	756

Notes: * means significant at the 10% level; ** means significant at the 5% level; *** means significant at the 1% level. "Liberal" vote means upholding an agency decision subject to an industry challenge or invalidating an agency decision subject to a challenge by a public interest group or labor union. "LPM" indicates linear probability model; "SEM" indicates structural parameters estimated from simultaneous equations model. All models include year fixed effects; the Miles and Sunstein data also include circuit fixed effects. Standard errors are clustered by case and judge. Revesz (1997) data includes all EPA cases in the D.C. Circuit from 1971 – 1995. Miles and Sunstein (2006) data includes all EPA and NLRB decisions applying *Chevron* deference in all circuits from 1985 – 2005.

TABLE 3: PROBABILITY OF A PRO-PLAINTIFF VOTE IN SEX DISCRIMINATION CASES

Data Set	Boyd et al. (2010)							Peresie (2005)						
Panel Effects	Contextual			Endogenous				Contextual			Endogenous			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)
	LPM	LPM	LPM	SEM	SEM	NLS	IV	LPM	LPM	LPM	SEM	SEM	NLS	IV
Judge Characteristics:														
Democrat	0.149***		0.138***	0.063***		0.056***	0.057***	0.129***		0.107***	0.068***		0.064***	0.057***
	(0.029)		(0.030)	(0.017)		(0.020)	(0.017)	(0.025)		(0.025)	(0.014)		(0.017)	(0.013)
Female		0.114***	0.065		0.055*	0.035	0.033*		0.160***	0.127***		0.039***	0.017*	0.028***
		(0.045)	(0.043)		(0.030)	(0.026)	(0.019)		(0.034)	(0.034)		(0.015)	(0.010)	(0.013)
Colleague Characteristics:														
# Democratic Colleagues	0.105*		0.098***					0.080***		0.060***				
	(0.027)		(0.028)					(0.024)		(0.025)				
# Female Colleagues		0.074*	0.040						0.134***	0.116***				
		(0.040)	(0.040)						(0.032)	(0.033)				
Colleague Votes				0.412***	0.394***	0.398***	0.411***				0.382***	0.454***	0.418***	0.423***
				(0.030)	(0.067)	(0.053)	(0.029)				(0.044)	(0.011)	(0.027)	(0.021)
Case Characteristics:														
Pro-Plaintiff Lower Court	0.255***	0.250***	0.257***	0.045***	0.053	0.052*	0.046**							
	(0.050)	(0.051)	(0.050)	(0.016)	(0.035)	(0.029)	(0.017)							
R-squared	0.149	0.120	0.152					0.060	0.065	0.084				
Number of Cases	415	415	415	415	415	415	415	554	554	554	554	554	554	554
Number of Votes	1245	1245	1245	1245	1245	1245	1245	1662	1662	1662	1662	1662	1662	1662

Notes: "LPM" indicates linear probability model; "SEM" indicates structural parameters estimated from simultaneous equations model; "NLS" indicates nonlinear least squares; "IV" indicates estimates from instrumental variables regression. All models include year and circuit fixed effects. Standard errors are clustered by case and judge. Boyd et al. (2010) data includes published sex discrimination cases from 1995 - 2002. Peresie (2005) data includes published and unpublished decisions in sex discrimination and sexual harassment cases from 1999 – 2001.

* means significant at the 10% level; ** means significant at the 5% level; *** means significant at the 1% level.

TABLE 4: PROBABILITY OF A PRO-DEFENDANT VOTE IN CRIMINAL APPEALS

Data Set	Sunstein et al. (2006) Criminal Appeals			
Panel Effects	Contextual		Endogenous	
	(1)	(2)	(3)	(4)
	LPM	LPM	SEM	SEM
Judge Characteristics:				
Democrat	0.061*** (0.024)		0.031 (0.026)	
Voting Rate in Other Cases		0.560*** (0.097)		0.179*** (0.074)
Colleague Characteristics:				
Number of Democratic Colleagues	0.039** (0.017)			
Sum of Colleagues' Voting Rates in Other Cases		0.434*** (0.104)		
Colleague Votes			0.388*** (0.091)	0.437*** (0.031)
R-squared	0.013	0.025		
Number of Cases	1136	1136	1136	1136
Number of Votes	3408	3408	3408	3408

Notes: "LPM" indicates linear probability model; "SEM" indicates structural parameters estimated from simultaneous equations model. All models include year and circuit fixed effects. Standard errors are clustered by case and judge. Data includes criminal appeals from 3rd, 4th, and D.C. Circuits, 1995 – 2004 with 2002 omitted.

* means significant at the 10% level; ** means significant at the 5% level; *** means significant at the 1% level.

TABLE 5: PROBABILITY OF A PRO-DEFENDANT VOTE IN DEATH PENALTY CASES

Data Set	Sunstein et. al (2006) Death Penalty		Fourth Circuit Death Penalty					
Panel Effects	Contextual	Endogenous	Contextual			Endogenous		
	(1) LPM	(2) SEM	(3) LPM	(4) LPM	(5) LPM	(6) SEM	(7) SEM	(8) SEM
Judge Characteristics:								
Democrat	0.160*** (0.041)	0.157*** (0.039)	0.122*** (0.025)			0.085*** (0.026)		
Voting Rate in Other Death Penalty Cases				0.540*** (0.142)			0.406*** (0.134)	
Voting Rate in Criminal Appeals Cases					0.829*** (0.147)			0.509*** (0.061)
Colleague Characteristics:								
Number of Democratic Colleagues	0.004 (0.033)		0.057** (0.028)					
Sum of Colleagues' Voting Rates in Other Death Penalty Cases				0.227 (0.173)				
Sum of Colleagues' Voting Rates in Criminal Appeals Cases					0.453*** (0.153)			
Colleague Votes		0.023 (0.200)				0.319*** (0.072)	0.296*** (0.103)	0.353*** (0.041)
R-squared	0.205		0.123	0.091	0.132			
Number of Cases	208	208	156	156	156	156	156	156
Number of Votes	624	624	468	468	468	468	468	468

Notes: "LPM" indicates linear probability model; "SEM" indicates structural parameters estimated from simultaneous equations model. All models include year fixed effects; the regressions on the Sunstein et al. data also include circuit fixed effects. Standard errors for Sunstein et al. data are clustered only by case; standard errors for Fourth Circuit data are clustered by case and judge. Sunstein et al. data includes a sample of death penalty appeals from all circuits, 1995 – 2004. Fourth Circuit data includes all death penalty cases from 1995 – 2004.

* means significant at the 10% level; ** means significant at the 5% level; *** means significant at the 1% level.

TABLE 6: PROBABILITY OF A VOTE FAVORING ABORTION RIGHTS

Data Set	Sunstein et. al (2006) Abortion	
Panel Effects	Contextual	Endogenous
	(1)	(3)
	LPM	SEM
Judge Characteristics:		
Democrat	0.199***	0.196***
	(0.061)	(0.056)
Colleague Characteristics:		
Number of Democratic Colleagues	0.018	
	(0.047)	
Colleague Votes		0.082
		(0.190)
R-squared	0.043	
Number of Cases	89	
Number of Votes	267	

Notes: "LPM" indicates linear probability model; "SEM" indicates structural parameters estimated from simultaneous equations model. Standard errors are clustered by case and judge. Data includes abortion cases in all circuits from 1983 – 2002.

* means significant at the 10% level; ** means significant at the 5% level; *** means significant at the 1% level.

TABLE 7: PROBABILITY OF A VOTE SUPPORTING PETITIONER IN ASYLUM CASES

Data Set	Law (2005)			
Panel Effects	Contextual		Endogenous	
	(1) LPM	(2) LPM	(3) SEM	(4) SEM
Judge Characteristics:				
Democrat	0.126*** (0.034)		0.059 (0.047)	
Voting Rate in Other Cases		0.805*** (0.061)		0.422*** (0.097)
Colleague Characteristics:				
Number of Democratic Colleagues	0.083** (0.014)			
Sum of Colleagues' Voting Rates in Other Cases		0.500*** (0.050)		
Colleague Votes			0.398*** (0.073)	0.383*** (0.029)
R-squared	0.083	0.191		
Number of Cases	1892	1892	1892	1892
Number of Votes	5676	5676	5676	5676

Notes: "LPM" indicates linear probability model; "SEM" indicates structural parameters estimated from simultaneous equations model. All models include year fixed effects. Standard errors are clustered by case and judge. Data includes all asylum appeals from the 9th Circuit from 1992 – 2001, including unpublished opinions.

* means significant at the 10% level; ** means significant at the 5% level; *** means significant at the 1% level.

TABLE 8: PROBABILITY OF A PRO-DEFENDANT VOTE IN FEDERAL SENTENCING CASES

Data Set	Linder and Niles (2010)										
Panel Effects	Contextual					Endogenous					
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
	LPM	LPM	LPM	LPM	LPM	SEM	SEM	SEM	SEM	NLS	IV
Judge Characteristics:											
Democrat	0.035* (0.020)				0.042** (0.020)	0.015 (0.019)				0.018** (0.008)	0.018** (0.007)
Female		-0.009 (0.020)			-0.011 (0.019)		-0.004 (0.019)			-0.005 (0.009)	-0.004 (0.006)
Former Prosecutor			-0.031 (0.023)		-0.037* (0.023)			-0.014 (0.020)		-0.016* (0.008)	-0.016** (0.007)
Voting Rate in Other Cases				0.494*** (0.110)					0.158*** (0.078)		
Colleague Characteristics:											
# Democratic Colleagues	0.024 (0.016)				(0.029)* (0.016)						
# Female Colleagues		-0.006 (0.016)			(-0.009) (0.018)						
# Former Prosecutors			-0.021 (0.018)		-0.026 (0.018)						
Sum of Colleagues' Voting Rates in Other Cases				0.385*** (0.094)							
Colleague Votes						0.413*** (0.117)	0.402 (0.476)	0.402*** (0.135)	0.438*** (0.031)	0.410*** (0.046)	0.410*** (0.046)
R-squared	0.007	0.004	0.006	0.019	0.010						
Number of Cases	1118	1118	1118	1118	1118	1118	1118	1118	1118	1118	1118
Number of Votes	3354	3354	3354	3354	3354	3354	3354	3354	3354	3354	3354

Notes: "LPM" indicates linear probability model; "SEM" indicates structural parameters estimated from simultaneous equations model; "NLS" indicates nonlinear least squares; "IV" indicates estimates from instrumental variables regression. All models also include post-*Booker* dummy. Standard errors are clustered by case and judge. Data includes both published and unpublished sentencing decisions in the 9th Circuit from 2003 - 2007.

* means significant at the 10% level; ** means significant at the 5% level; *** means significant at the 1% level.

TABLE 9: PROBABILITY OF A VOTE SUPPORTING FREE EXERCISE CLAIM

Data Set	Sisk, Heise, and Morriss (2004)				
Panel Effects	Contextual		Endogenous		
	(1)	(2)	(3)	(4)	(5)
	LPM	LPM	SEM	NLS	IV
Judge Characteristics:					
Democrat	-0.099***	-0.095**	-0.055***	-0.072*	-0.043
	(0.034)	(0.038)	(0.016)	(0.040)	(0.026)
Female		-0.076		-0.077	-0.011
		(0.077)		(0.072)	(0.037)
Jewish		0.093		0.091	0.016
		(0.057)		(0.064)	(0.034)
Catholic		-0.021		-0.046	0.039
		(0.046)		(0.049)	(0.030)
Mainline Protestant		-0.019		-0.017	-0.007
		(0.049)		(0.050)	(0.023)
Elite Law School Grad		0.010		0.026	-0.022
		(0.038)		(0.040)	(0.019)
Colleague Characteristics:					
Number of Democratic Colleagues	-0.058*	-0.055			
	(0.034)	(0.038)			
Number of Female Colleagues		-0.072			
		(0.069)			
Number of Jewish Colleagues		0.084			
		(0.060)			
Number of Catholic Colleagues		-0.057			
		(0.046)			
Number of Mainline Protestant Colleagues		-0.014			
		(0.047)			
Number of Elite Law School Grad Colleagues		0.033			
		(0.039)			
Colleague Votes			0.371***	0.484***	0.468***
			(0.069)	(0.043)	(0.044)
R-squared	0.113	0.137			
Number of Cases	259	259	259	259	259
Number of Votes	777	777	777	777	777

Notes: "LPM" indicates linear probability model; "SEM" indicates structural parameters estimated from simultaneous equations model; "NLS" indicates nonlinear least squares; "IV" indicates estimates from instrumental variables regression. All models include year and circuit fixed effects. Standard errors are clustered by case and judge. Data includes Free Exercise Clause cases from all circuits, 1986 – 1996.

* means significant at the 10% level; ** means significant at the 5% level; *** means significant at the 1% level.