

How Interpersonal Contact Affects Appellate Review

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Prominent explanations for appellate review prioritize the ideological alignment of the lower and reviewing courts. We suggest that interpersonal relationships play an important role. The effect of an appellate judge's ideology on her decision to reverse depends on the level of interpersonal contact between the trial and appellate judge due to information provided by social and professional interactions. Relying on a dataset of all published Fourth Amendment search and seizure decisions from 1953-2010, we find that interpersonal relationships can dampen the effect of ideology in appellate review. When an appellate and trial court judge have frequent contact, the effect of ideology on the appellate judge's decision to reverse is essentially imperceptible. These findings speak to the importance of relationships in principal-agent arrangements generally and have implications for the structure of the federal judiciary and our understanding of the limits of ideological judicial decisionmaking.

Scholars widely believe the relationship among an appellate judge's preferences, the ideological direction of the lower court decision, and the lower court judge's preferences affect the appellate judge's decision to reverse the lower court (Haire, Lindquist and Songer 2003; Cameron, Segal and Songer 2000). We suggest an interpersonal element to this principal-agent relationship. Appellate judges have an enormous caseload; they cannot give each case a full review. We expect that appellate judges vary in the deference they give to the decisions of their trial court counterparts based on their knowledge of those colleagues.

When an appellate judge has more frequent contact with a district court judge, she knows more about that judge's capabilities. Sometimes this information suggests a presumption of reversal, as when the appellate judge believes the lower court judge to be a poor judge. At other times, it leads to a supposition of affirmance. This private information creates heterogeneity in the relationship between the appellate court judge's ideological alignment with the lower court's decision and the likelihood she votes to reverse. In the absence of this information, other easily accessible considerations, namely ideology, shift to the forefront of the judicial calculus. Therefore, we predict that the effect of ideological alignment on the decision to reverse dissipates when the trial and appellate judges have frequent interpersonal contact. We test our theory with a dataset of all published Fourth Amendment search and seizure decisions from 1953 to 2010 which appeal the decision of an identifiable U.S. District Court judge. The results support this theory, demonstrating that the role of ideology is substantially dampened when the trial and appellate judges have frequent interpersonal contact, which we measure by the two judges having offices in the same courthouse. Under these conditions, the effect of ideology on the probability of reversal is essentially imperceptible. We conclude that interpersonal contact plays a powerful role in oversight.

These results represent an important advance in our understanding of principal-agent relationships, demonstrating that interpersonal relationships among principals and agents can diminish the role of ideological considerations in oversight. They extend a growing literature illuminating the conditions under which ideological decisionmaking on appellate courts is not

apparent (Bartels 2009). Moreover, our results demonstrate that the physical structure of the federal judiciary shapes interpersonal relationships among judges: less ideological oversight comes from more frequent interaction between principals and the agents they supervise.

Ideology, Interpersonal Contact, and Reversal

Principal-agent theory suggests that judges will want to exercise oversight with minimal effort, especially when their ability to monitor lower court judges is constrained by resource limitations. Intermediate appellate court judges cannot give each case a full and careful review (Haire, Lindquist and Songer 2003). When a lower court decision is likely to be correct (from a legal, political, and/or hierarchical perspective), there is no reason to expend considerable effort reviewing it. Instead, rational appellate judges rely on heuristics about the case and the lower court proceedings to determine which cases need close scrutiny rather than fleeting attention. When reviewing an appeal, judges can quickly discern (a) the ideological direction of the lower court's decision and (b) the identity of the lower court judge. Both are valuable heuristics (Cameron, Segal and Songer 2000).

First, appellate judges generally prefer to affirm lower court decisions that align with their policy preferences and reverse those that do not (Segal and Spaeth 2002). In the absence of compelling information about other aspects of the trial court decision, the appellate court judge's ideological alignment with the lower court decision plays a primary role in her decisionmaking calculus. But, judges, especially lower court judges, have goals beyond policy, including efficiency, prestige-building, and avoiding reversal (Baum 1997; Zorn and Bowie 2010). As a result, district judges' decisions do not necessarily reflect their ideology. When an appellate judge knows about the district judge's skills and biases regarding these other issues, that information may affect the reversal decision. This may particularly be the case when the appellate judge knows the decision runs counter to the district judge's ideological preferences (Cameron, Segal and Songer 2000; Haire, Lindquist and Songer 2003).

Thus, the identity of the trial judge provides the appellate court judge with information

about both the quality and ideological consistency of the trial court decision. The value of this cue increases as the appellate judge has more information about the trial judge. When the appellate judge has had regular contact with the trial judge, she is more confident in her assessment of the extent to which that lower court judge exhibits traits such as attention to detail or bias that may color her decision about reversal.

Geography can help appellate court principals form their assessments of a trial judge's (agent's) skill. Judges on the U.S. Courts of Appeals review decisions made by U.S. District Court judges in their circuit. Although federal circuit judges travel to hear oral arguments with their colleagues, they spend the majority of their time in home chambers in cities dispersed throughout the circuit. Those chambers are often in courthouses that are also home to U.S. District Judges. In 2010, for example, there were an average of five district judges working in courthouses with at least one circuit judge. When two judges' chambers are located in the same courthouse, frequency of contact and overlapping social networks are increased. This geographic proximity allows appellate judges to acquire information about their local trial court counterparts they would otherwise have difficulty gathering. That frequent interaction can affect political outcomes is demonstrated by studies using shared living space or desk proximity to estimate impacts on legislative behavior (e.g., Masket 2008). Judges have also noted that interpersonal contact with other colleagues affects their behavior (e.g., Roberts 2006). Appendix A presents additional qualitative evidence on this point.

The private information resulting from these interpersonal interactions should condition the role of ideology in the appellate judge's decisionmaking calculus. On the one hand, when two judges do not know each other well, the appellate judge has less information about the lower court decision's quality because her ability to develop an independent assessment of the trial judge's characteristics has been substantially limited. Under these circumstances, ideological direction of the ruling below (the primary available cue) plays a comparatively larger role in the decision to reverse, exactly as some attitudinal theories of appellate decisionmaking predict (Sunstein et al. 2006). Thus, *appellate judges, especially those with*

limited interpersonal contact with the trial judge, will be less likely to reverse a lower court decision as their ideological alignment with the decision increases.

On the other hand, frequent interpersonal contact can provide an appellate judge with a rich store of private information. She may learn that a trial judge is lower quality than his credentials indicate or that he performs his job with a great deal of care. To the extent that this private information suggests that the trial court judge has decided the case correctly, the appellate judge should defer to his decision even when ideological considerations would suggest otherwise. At the same time, when that private information suggests that a judge is not very good at his job, the appellate judge might be *more* likely to reverse, even when the decision might initially appear to be aligned with her ideological preferences. After all, because their decisions are subject to further review, intermediate appellate court judges care about the legal correctness of a decision as well as its ideological implications (Bowie, Songer and Szmer 2014).

In this way, private information, formed from interpersonal contact, provides judges with an array of heterogeneous considerations they can bring to bear when reviewing that judge's decisions. In such cases, the appellate judge, acting as a principal, is able to overcome some of the disabilities associated with information asymmetries due to information regarding the agent's type across various relevant areas of the agent's performance. Sometimes those considerations will suggest reversal when ideological considerations align; other times, the converse will be true. On balance, therefore, the introduction of private information should dilute the effect of ideology on the decision to reverse. Therefore, *when two judges have frequent interpersonal contact, the relationship between the appellate judge's ideological alignment with the lower court decision and reversal should be dampened.*

Data and Research Design

We analyzed all published Fourth Amendment search and seizure decisions made by the numbered U.S. Courts of Appeals from 1953 to 2010. This topic incorporates a discrete

set of legal issues that are routinely raised in litigation within the context of both civil and criminal cases. Though any single-issue study raises concerns about generalizability, we note that search and seizure cases represent a somewhat difficult test case for our theory. The issue is frequently litigated, so most judges have substantial experience with this topic. Consequently, the range of judicial expertise is compressed, limiting the range of “quality” among trial court judges (and the utility of information about such). After excluding all opinions that did not address the merits, resulted in a split decision (i.e., affirmed in part and reversed in part), were en banc, or were not appealed from an identifiable district judge, the resulting dataset contains 11,734 opinions. The unit of analysis is the judge-vote. The outcome variable indicates whether the appellate judge voted to reverse the lower court ruling; 26.5% of votes in the dataset were to reverse. We estimate a probit model with circuit fixed effects and standard errors clustered on the case, though the results are robust to the omission of fixed effects. We further provide results from both the full dataset and a dataset preprocessed with Coarsened Exact Matching (Iacus, King and Porro 2012). Appendix A provides more details on case selection, the matching procedure, and summary statistics.

Our main explanatory variables are the appellate judge’s ideological preferences with respect to the lower court ruling, the level of interpersonal contact between the trial and appellate judges, and the interaction of these two concepts. First, we measure the ideological preferences of the appellate judge using Judicial Common Space (“JCS”) scores (Boyd 2015; Giles, Hettinger and Peppers 2001; Epstein et al. 2007), which range from -1 (liberal) to 1 (conservative). When the lower court ruling is conservative, the appellate judge’s JCS score reflects how well they are aligned with the ruling below. When the lower court ruling is liberal, we multiply the appellate judge’s JCS score by -1. Therefore, this variable measures how well the appellate court judge is *Aligned* with the lower court ruling.

Second, because courts do not generate publicly available data about the level of interpersonal contact between judges, we utilize an observable indicator of frequency of interaction: whether the appellate court judge and lower court judge have their home chambers in the

same courthouse. These judges are more likely to see each other on a frequent basis, professionally and/or socially. Appendix A provides qualitative evidence on this point, and Table A.2 provides further information about the distribution of judges across circuits. *Same Courthouse* equals one if the appellate judge and trial judge have their chambers in the same courthouse and zero otherwise; 10.5% of votes in our dataset were cast by an appellate judge reviewing a trial judge with chambers in the same building.

The extent to which the lower court decision is consistent with the ideological preferences of the trial judge is potentially important (Cameron, Segal and Songer 2000). Such ideological preferences help appellate judges interpret the lower court ruling (Black and Owens 2012). Counterideological lower court decisions might be particularly strong signals of a quality fact pattern or determinate precedent (Black and Owens 2012; Cameron, Segal and Songer 2000), so we account for whether the trial judge decided the case in line with their partisanship. When a district judge who was appointed by a Republican president rules in favor of the prosecution in a criminal case or the defendant in a civil case, *Ideological Lower Court Ruling* equals one. The same is true when a district judge appointed by a Democratic president rules in favor of the defendant in a criminal case or the plaintiff in a civil case. We use party of the appointing president because JCS scores are not available for some of the district judges in the early years of our dataset. We also include a variety of control variables described in Appendix A.

Results

Figure 1 provides both predicted probabilities and marginal effects necessary to assess the conditional relationship between levels of interpersonal contact and ideological alignment. Recall that we expect appellate judges to be less likely to reverse as their alignment with the ruling increases, but that relationship should be dampened when the two judges have frequent interpersonal contact. This pattern is evident in Figure 1 for both the full data and the matched data. In the full dataset when two judges do not have frequent contact, the

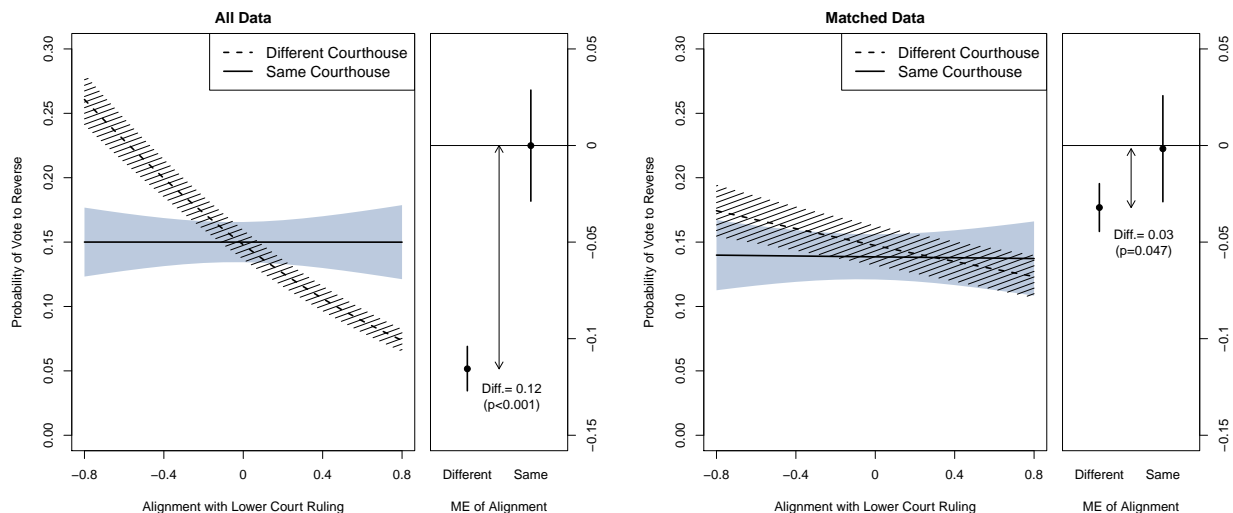


Figure 1: Ideology, Contact, and Reversal: The left-hand panel in each block shows the predicted probability that an appellate judge in a different courthouse or the same courthouse as the lower court judge votes to reverse across all values of *Aligned*. The right-hand panel in each block shows the marginal effect of *Aligned* for same and different courthouse and the difference in marginal effects. 95% confidence intervals are included. Model results provided in Appendix B.

probability of voting to reverse drops from one-in-four to less than one-in-ten over the range of *Aligned*. So, when the trial judge works in another courthouse, the most conservative or liberal appellate judges are three times more likely to vote to reverse a lower court outcome that does not match their preferences compared to one that does. This effect is less dramatic in the matched data, but even there reversal rates drop from 17.4% to 12.3%. When the appellate judge has her chambers in the same building as the trial judge, the probability of reversal remains remarkably stable in both analyses, moving no more than .003 over the *entire* range of *Aligned*. Thus, these results suggest that frequent interpersonal contact can dampen the effect of ideology on an appellate judge’s decision to reverse.

Our findings are robust to a variety of alternative specifications. Judges might give more deference to lower court rulings that are ideologically incongruous with the lower court judge’s preferences, especially when they have frequent interactions with the lower court judge. In Appendix C, we therefore allow the effects of *Same Courthouse* and *Aligned* to vary by the ideological congruence of the lower court decision. We do not find evidence that

the relationship between interpersonal contact and ideological alignment is significantly conditioned by the ideological congruence of the lower court judge and his ruling. Moreover, the pattern shown in Figure 1 persists: the increase in contact associated with shared buildings dampens ideological behavior by appellate judges. In Appendices D and E, we show that alternative measures for ideological alignment and frequent contact, respectively, do not lead to substantially different conclusions.

Discussion and Conclusions

We suggested that appellate judges carry out oversight differently based on the extent to which they have private information regarding the trial court judge. This theory stands in contrast to previous theories of appellate review that prioritize ideological divergence and leave interpersonal relationships unexamined. Given that federal judges work together for years, understanding how relationships shape the development of the law is essential to understanding judicial behavior.

We draw two conclusions. First, the appellate judge's ideological alignment with the lower court decision is a predominant factor in the reversal decision when the trial and appellate court judges do not have frequent contact. This finding is somewhat rote: ideology drives appellate behavior. Still, that ideologically extreme judges are particularly likely to reverse lower court rulings emphasizes the importance of variation in appellate court ideology (Giles, Hettinger and Peppers 2001). Second, when trial and appellate court judges have frequent contact, the relationship between ideology and the decision to reverse is dampened. Segal and Spaeth (2002) suggested that the effect of ideology on judicial behavior is attenuated on lower courts; while legal considerations are one source of constraint (e.g., Bowie, Songer and Szmer 2014), these findings suggest that interpersonal relationships provide another.

We are upfront that ours is a single-issue study with the strengths and weaknesses that derive from that research design. On the one hand, this design limits heterogeneity in case type thereby making comparisons cleaner. As to our choice of issue, we are heartened by

the fact that the conclusions drawn from the many studies of judicial decisionmaking based on search and seizure decisions have proven to generalize across the range of federal judicial decisionmaking (e.g., Segal and Spaeth 2002). At the same time, we recognize that future work should probe whether these effects might vary by issue. Search and seizure cases present a difficult test case for our theory; because every judge has some familiarity with this topic, the extent to which a trial judge could provide a particularly powerful heuristic is limited. In other cases, like tax or antitrust, judicial expertise is more important and the size of our effects might be expected to increase (Klein 2002). Further work should also examine the effects of interpersonal contact on other types of principal-agent relationships to establish how these effects vary beyond the courtroom.

Still, that an admittedly noisy measure of interpersonal contact—since pairs of judges working in the same place surely vary in the frequency of their interaction—is enough to render the effect of ideology essentially imperceptible points to the importance of further research probing interpersonal relationships among judges. Interpersonal contact may affect judicial behavior in other ways, perhaps by dampening the effect of ideology on a judge’s decision to dissent or by encouraging judges to soften the language they use in dissent. Furthering our understanding of this topic will help us explain legal development, both in particular cases and over time.

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Online Appendices

Appendix A: Supplemental Data Information

Data Preparation. We excluded unpublished opinions because they are not readily available for the time-span covered here (Sunstein et al. 2006). We excluded the D.C. Circuit from the analysis because the confined geographical reach of that circuit eliminates variation in chambers location. After excluding all opinions that did not address the merits, resulted in a split decision (i.e., affirmed in part and reversed in part), were en banc, or were not appealed from an identifiable district judge, the resulting dataset contains 11,734 opinions. A dissent in a case that affirmed the lower court ruling is coded as a vote to reverse (and vice versa).

Coding *Same Courthouse*. The front matter of physical volumes of the *Federal Reporter* and *Federal Supplement* contain information on the city in which each U.S. District Court Judge and U.S. Court of Appeals judge has his or her chambers. Twelve percent of votes in the dataset were cast by an appellate judge reviewing a trial judge with chambers in the same city. While most cities contain only a single federal courthouse, some cities do contain multiple courthouses, and courthouses were constructed and remodeled during the nearly 60-year period that our data span. We consulted the Federal Judicial Center’s electronic database of Historic Federal Courthouses¹ to identify which cities had multiple federal courthouses (and in which years). Next, we obtained the chambers address for every trial and appellate judge in our data located in the same city in a year when that city had multiple courthouses. To find these addresses, we consulted print volumes including the *Judicial Staff Directory* (published by Congressional Staff Directory, Ltd. in earlier years and CQ Staff Directories more recently), the *Judicial Yellow Book* (published by Leadership Directories), *BNA’s Directory of State and Federal Courts, Judges, and Clerks* (published by the Bureau Of National Affairs), and *Want’s Federal-State Court Directory* (published by WANT Publishing Co.) as necessary. We also consulted publicly available information available on the internet including court directories, financial disclosure forms available from Judicial Watch, obituaries, and newspaper articles. Because print directories and electronic information are generally only available beginning in the mid-1980s, we had to initiate personal communications to locate chambers locations for many judges who served during the early years of our data. We contacted former law clerks, court staff, historians, and circuit librarians to obtain this information. Ultimately, we dropped four observations from our data due to the inability to identify relevant judges’ chambers location. Appendix E provides a robustness check using the variable *Same City* which does not require dropping these observations. The results do not change.

Validating the Assumption: Proximity and Contact. The empirical analysis relies on the assumption that when two judges’ chambers are located in the same location, frequency of contact and overlapping social networks are increased. This contact, by extension, reduces

¹Available at: <https://www.fjc.gov/history/courthouses>

the effect of ideology on an appellate judge's decision to reverse a trial court judge. This hypothesis is in line with Baum's (2006) suggestion that "[a] judge will be more open to influence from colleagues who are more relevant at a personal level."

Scholars have long noted that close chambers locations foster frequent (and substantively important) collegial contact. Wasby's studies of judicial communication repeatedly make this point. For example, Wasby (1980), studying the Eighth Circuit, reports that "Two judges sitting in the same city tend to contact each other more frequently than judges who sit in different cities" (590). Wasby (1987), studying the geographically massive Ninth Circuit, writes that "[g]eography's principal negative effect was that face-to-face contact was more difficult" (133). Indeed, "[j]udges in the same courthouse with others, in addition to having casual elevator or parking area encounters with their colleagues, found it 'much easier' to call and get together with another judge in the courthouse with their clerks present." (134). Other judges concur. Wilkinson (1994, 1173) writes that "I believe nonetheless that at heart the appellate process is a deliberative process, and that one engages in more fruitful interchanges with colleagues whom one deals with day after day than with judges who are simply faces in the crowd."

To understand how frequent contact can affect appellate-trial judge relationships, consider the reflections of a former D.C. circuit judge, Chief Justice John Roberts (2006), discussing how the fact that all of the D.C. Circuit's district and circuit court judges work in the same building affects their deliberations.

When I joined the D.C. Circuit three years ago, I began to appreciate that the court was different in significant respects from the other courts of appeals with which I was familiar around the country. Some of these differences are very obvious. For example, all the D.C. Circuit judges are in the same building, along with all the district court judges. This allows the circuit judges the unique opportunity of sitting down to lunch right next to a judge who, moments before, they had announced was guilty of abuse of discretion or clear error. It can make for a very short lunch (376).

Now-Chief Justice Roberts is not the only D.C. Circuit judge to note the positive externalities of that circuit's single building set-up. Former Chief Judge Harry Edwards (2003) also praised the practice: "[h]aving the entire circuit's chambers in the same building, as with the D.C. Circuit and the federal Circuit, can also be immensely helpful. The ease of face-to-face interactions outside the context of hearings and conferences makes a difference" (1675).

Others have come to similar conclusions. Cohen (2002) writes that "Although advances in technology have enabled judges to communicate instantly over large geographic areas, such advances have not enabled the judges to communicate informally as judges can do when they work in the same building" (157). Cohen (2002) quotes a Ninth Circuit judge as stating the following about the effects of geography: "I think that if you are in proximity to the other judges, I think it does encourage a bit more of an interaction" (157). Another Ninth Circuit judge, again recounted by Cohen (2002), makes the point even more colorfully: "It is very fine to have them all staying in the same place, but it is like a college dormitory. It is because of the relationships that you develop" (158). As another example, Tenth Circuit judge Michael R. Murphy (2000) writes: "Collegiality requires a familiarity with other judges, which occurs only with regular face-to-face contact... On an appellate court, absence does

not make the heart grow fonder, and familiarity does not breed contempt. Absence makes the heart unfamiliar, and it does not breed collegiality” (458-9).

Scholars of U.S. District Courts have come to similar conclusions about the importance of geographic location for frequency of contact. Carp and Wheeler (1972) note increased frequency of interaction among District Court judges who office in the same city:

For judges in a multi-judge city the source of advice is frequently one or more judges in the same building. “My prime sources of help were the two judges here in [X city]. They sent me various things even before I was appointed, and I was glad to get them,” said one judge. Another recalled, “I had the help I needed right down here in the corner of this building on this floor,” pointing in the general direction of another judge’s chambers (377).

Carp (1972), summarizing interviews with Federal District Judges, argued that “informal social contact among district judges is a current and ever-increasing phenomenon among judges within the same circuit” (413). Carp goes on to note that this contact was particularly important for judges who worked in the same city. Carp writes:

Because Judge Riley [a judge in the Southern District of Iowa] presided over his own separate judicial district he very rarely enjoyed daily personal contact with other federal district judges. Nevertheless, this was a privilege experienced by a number of district judges within the Eighth Circuit who resided in states where the judges sat at large and where they often had joint offices with other judges in the same city. Such was the case, for example, in the state of Minnesota, where several district judges held court jointly in Minneapolis, and in the state of Missouri, where several such judges operated out of St. Louis. Therefore a third of the Eighth Circuit trial judges were able to see one another on almost a day-to-day basis and were thus able to exchange information and advice with considerable ease (414).

Recognizing the positive benefits of this close proximity, Carp notes that Judge Riley was “env[ious] of those Eighth Circuit judges who could enjoy daily contact with one another” (414).

In short, our study builds on a deep body of empirical, anecdotal, and historical evidence in its assumption that close professional physical proximity translates into more frequent interpersonal contact.

Matching Procedure. Throughout our analyses we present models incorporating matched data alongside the analysis of our full dataset. For the models with matched data we use only matched data (with appropriate weights) after using exact matching for the appellate judge and coarsened exact matching on *Aligned* to match observations where the appellate and trial judges are in the same courthouse and are in different courthouses (Iacus, King and Porro 2012).² The result of this matching process was a dataset that includes 69% of our

²We use a modest matching specification because one that includes all pretreatment covariates suggests that more than 90% of our data should be discarded, leaving only 3,361 observations. Still, even with such a specification, our key results still hold.

full dataset and an overall \mathcal{L}_1 distance of 0.027. Figure A.1 illustrates that the distribution of the *Aligned* variable is quite similar for both our full and matched datasets.

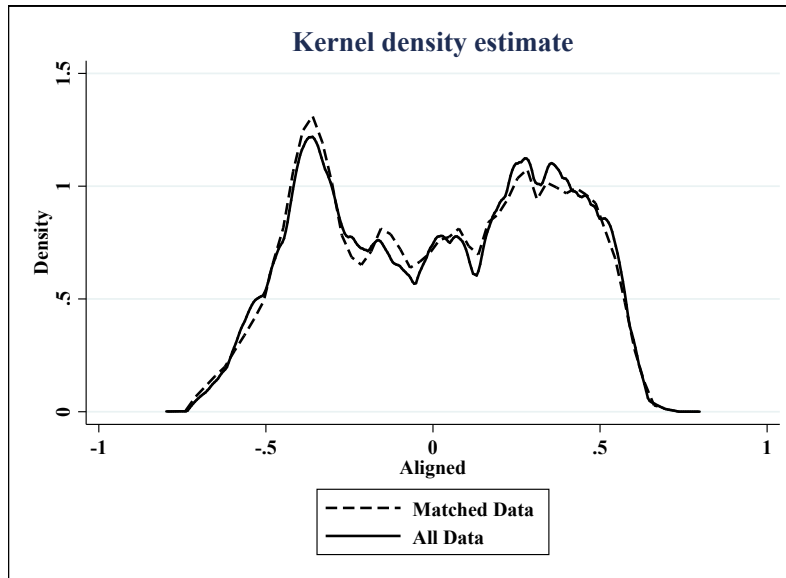


Figure A.1: Distribution of *Aligned*. This figure provides a density plot of the distribution of *Aligned* in both our full dataset and for our matched data. For the full dataset the variable ranges from -0.80 to 0.80 with a median of 0.03, a mean of 0.02, and a standard deviation of 0.35.

Control Variables and Summary Statistics. Our model specification includes a number of control variables. A substantial literature on circuit court decisionmaking shows the persistent effect of panel composition on an individual judge’s votes (Sunstein et al. 2006); these panel effects might complicate the influence of the appellate judge’s private information. We therefore control for the panel type, using the party of the appointing president to classify each judge vote as occurring in one of three situations: on a unified panel, on a split panel where the judge is in the majority, or on a split panel where the judge is in the minority.

We further account for a trial judge’s credentials by summing binary variables indicating whether a lower court judge graduated from a top-14 law school (Sen 2015), received an ABA rating of “Well Qualified” or better, clerked for a judge, or had prior experience as a judge, professor, solicitor general, prosecutor, public defender, or attorney general before ascending to the federal bench. The latter categories indicate subject expertise in search and seizure cases. We also control for whether the appellate judge and trial judge have a shared legal education background. We further account for whether the appellate judge is a district judge sitting on the panel by designation and whether they had judicial experience before their current job. Finally, we control for case type, trial judge demographics (to account for differential reversal rates (Sen 2015)), caseload, and year. *Caseload* is the number of cases (in 100s) terminated in the circuit-year divided by the number of active circuit judges that year.

The summary statistics for these control variables and all of our other variables are provided in Table A.1. Table A.2 provides a snapshot of the distribution of federal judges

in 2010, the most recent time point in our data, in order to provide an overview of the concentration of federal district and circuit judges in each federal courthouse where there is at least one circuit judge.

	25%	50%	75%
Continuous Variables			
Aligned	-.30	.03	.33
Caseload	1.98	2.64	3.33
Year	1980	1991	2001
Lower Ct. Judge Credentials	1	2	3
Dichotomous Variables			
	0	1	
Reverse	73.53%	26.47%	
Same Courthouse	89.54	10.46%	
Ideologically Congruent Lower Court Ruling	46.01%	53.99%	
Split Panel, Majority	53.07%	46.93%	
Split Panel, Minority	76.52%	23.48%	
Same Law School	92.32%	7.68%	
DJ Sitting by Designation (Appellate)	90.72%	9.28%	
Prior Judicial Experience (Appellate)	74.08%	25.92%	
Civil Rights	74.99%	25.01%	
Habeas	93.41%	6.59%	
Female Lower Ct. Judge	91.16%	8.84%	
Minority Lower Ct. Judge	90.07%	9.93%	

Table A.1: Summary Statistics

Circuit	# of Courthouses with CJ	# of DJs per Courthouse
1	6	4.8
2	7	2.3
3	8	8.5
4	10	2.1
5	8	5.1
6	16	4.3
7	6	6.8
8	11	4.1
9	19	5.1
10	10	2.8
11	8	4.9
Total	109	5.8

Table A.2: Distribution of District Judges in 2010 across all courthouses with at least one Circuit Judge.

Appendix B: Full Model Results

Figure B.1 shows the effect of changing each binary variable from zero to one and the effect of moving each continuous variable from its 25% to 75% value. Table B.1 provides full regression results. On average (and as expected), appellate judges who are more aligned with a lower court ruling are significantly less likely to vote to reverse.³ At first glance, neither model provides evidence that frequent interpersonal contact leads to a “reversal aversion” akin to the dissent aversion well-documented in other contexts (Epstein, Landes and Posner 2013). However, this null finding is produced by setting *Aligned* at its median value. When two judges are even moderately unaligned (i.e., $Aligned \leq 0.12$) such a pattern does emerge to a significant extent in both models. The hypothesized pattern exists in 39% of the data.

	(1)		(2)	
	All Data		Matched Data	
	Coef.	S.E.	Coef.	S.E.
Aligned	-0.505*	(0.023)	-0.139*	(0.027)
Same Courthouse	0.009	(0.025)	-0.039	(0.025)
Aligned × Same Courthouse	0.505*	(0.067)	0.131	(0.068)
Ideologically Congruent Lower Court Ruling	-0.005	(0.015)	-0.003	(0.018)
Split Panel, Majority	0.010	(0.018)	-0.017	(0.021)
Split Panel, Minority	0.017	(0.020)	-0.026	(0.024)
Lower Court Judge Credentials	0.012	(0.006)	0.017*	(0.007)
Same Law School	-0.028	(0.029)	0.021	(0.030)
DJ Sitting by Designation (Appellate)	-0.025	(0.025)	0.061	(0.052)
Prior Judicial Experience (Appellate)	-0.044*	(0.017)	-0.103*	(0.023)
Civil Rights	0.415*	(0.017)	0.386*	(0.021)
Habeas	0.080*	(0.030)	0.183*	(0.033)
Female Lower Court Judge	-0.069*	(0.027)	-0.000	(0.035)
Minority Lower Court Judge	0.171*	(0.024)	0.131*	(0.030)
Caseload	0.037*	(0.012)	0.082*	(0.017)
Year	-0.006*	(0.001)	-0.012*	(0.001)
Intercept	11.517*	(2.216)	22.397*	(2.760)
AIC	38615.0		27036.2	
BIC	38843.7		27255.0	
N	35,175		24,370	

Table B.1: Probit regression estimates of the effect of having chambers in the same courthouse, ideological factors, their interaction, and a range of control variables on the decision of whether to vote to reverse a lower court ruling. The models include fixed effects for circuit (not shown). The reported standard errors are robust standard errors that are clustered on the case and * denotes a p-value less than 0.05.

³All discussion of statistical significance is at the 0.05 level.

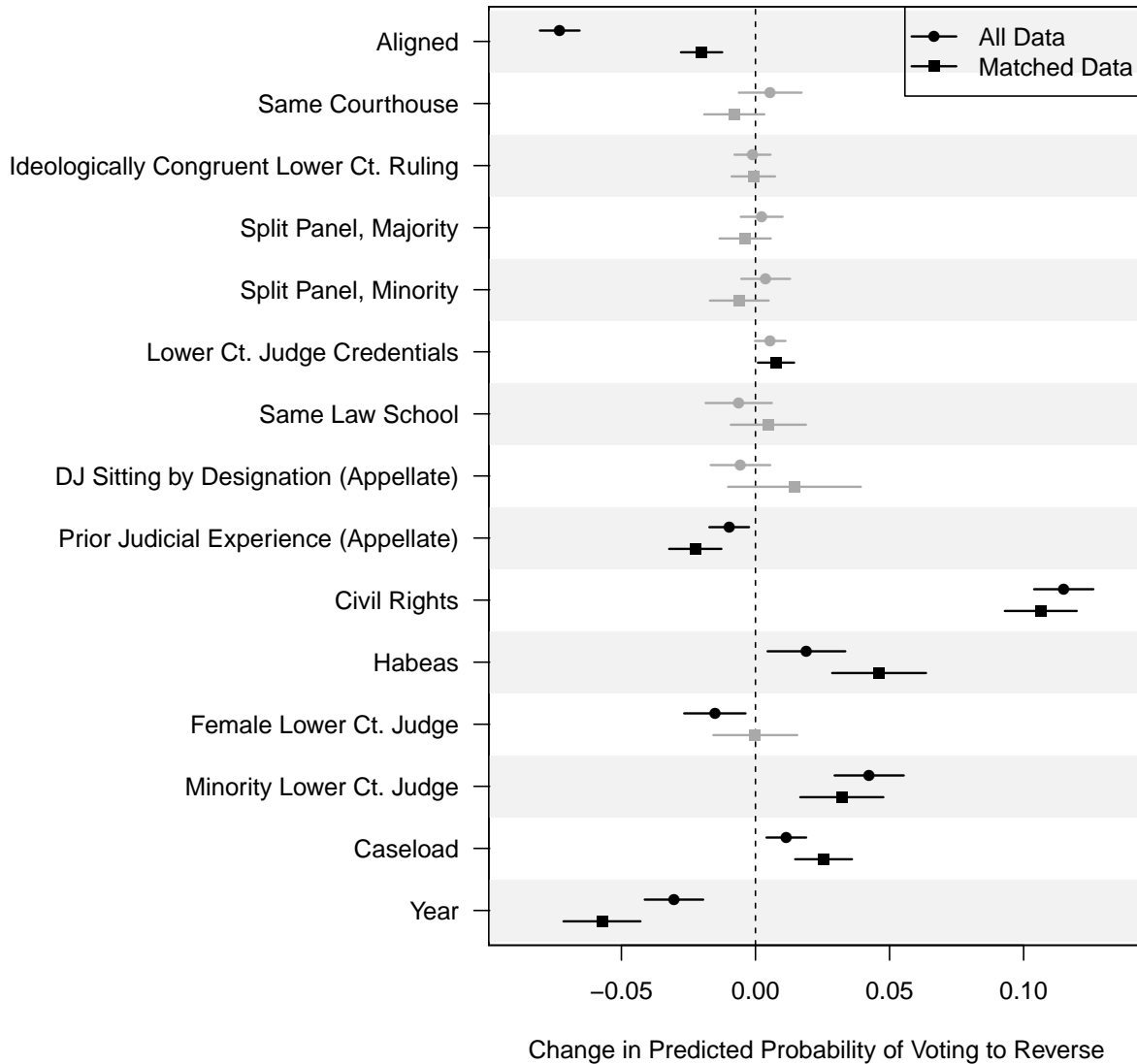


Figure B.1: Changes in predicted probabilities when moving each variable from 0 to 1 or from its 25% to 75% value in the data. Estimates are from a probit regression model that estimates the effect of having chambers in the same courthouse, ideological alignment, their interaction, and a range of control variables on the decision to vote to reverse a lower court ruling. Dots and bars in black (instead of gray) indicate that the confidence interval does not include zero. Full model results are available in Table B.1.

Appendix C: Accounting for the Lower Court Decision

Our main models account for the ideological congruence of the lower court decision and the lower court judge, but, for the sake of parsimony, hold that variable constant. However, it may be the case that the effect of this variable is conditional, rather than additive, because judges give more deference to lower court rulings that are ideologically incongruous with the lower court judge’s preferences. In this appendix, we test this possibility. The key conclusions are twofold. First, we find no evidence that the relationship between interpersonal contact and ideological alignment is significantly conditioned by the ideological congruence of the lower court judge and his ruling. Second, even accounting for such a conditional influence, the conclusions from the body of the manuscript hold: interpersonal contact dampens the extent to which appellate judges reverse lower court decisions with which they are not ideologically aligned.

The ideological congruence of a decisionmaker (here, the lower court judge) and his decision can provide another powerful cue that could factor into the appellate judge’s calculus. Calvert (1985) shows that information from an ideologically biased source can be particularly informative to a decisionmaker; if the source makes a decision contrary to expectations, that action is likely to be particularly influential to the receiver of that message (see also Slothuus and de Vreese 2010). Scholars have found evidence of this “Nixon goes to China” effect in appellate review of district courts (Haire, Lindquist and Songer 2003), as well as other types of appellate review (e.g. Cameron, Segal and Songer 2000; Beim, Hirsch and Kastellec 2014).

When a trial judge decides a case in line with his ideological predisposition (e.g., a liberal trial court judge decides a case in a liberal direction), we expect the appellate judge to generally view the case as typical and not requiring much effort to review. Under these circumstances, the appellate court judge’s ideology should dominate her decision to reverse ideologically congruent trial court decisions. This is consistent with evidence that ideologically expected decisions tend to be less secure on appeal (e.g. Beim, Hirsch and Kastellec 2014; Haire, Lindquist and Songer 2003).

At the same time, a conservative ruling from a liberal trial judge (or vice versa) sends an informative signal that the case below may well have been decided correctly. After giving the case its due, the appellate judge is more likely to find that the law or facts are strong enough to require a particular legal outcome. To this end, we expect the role of an appellate judge’s own ideology to be dampened—or even eliminated—when she reviews a trial court ruling that is incongruous with the trial judge’s ideological preferences.

Both effects of ideological incongruency are dependent upon the appellate judge knowing about the trial judge’s ideological preferences. Awareness of a trial judge is likely to be enhanced by regular interactions in a shared work environment. Because more frequent contact enables appellate judges to have more information about a trial judge, we expect that the effect of an ideologically incongruent lower court ruling will be largest when the appellate and trial judges have frequent contact. In particular, *the relationship between an appellate court judge’s ideology and reversal should be particularly weak when she is reviewing an ideologically incongruent decision by a judge with whom she has frequent contact*. Figure C.1 illustrates these expectations.

We assess these expectations by adding the ideological congruence of the lower court decision to the two-way interaction between *Same Courthouse* and *Aligned* discussed in the

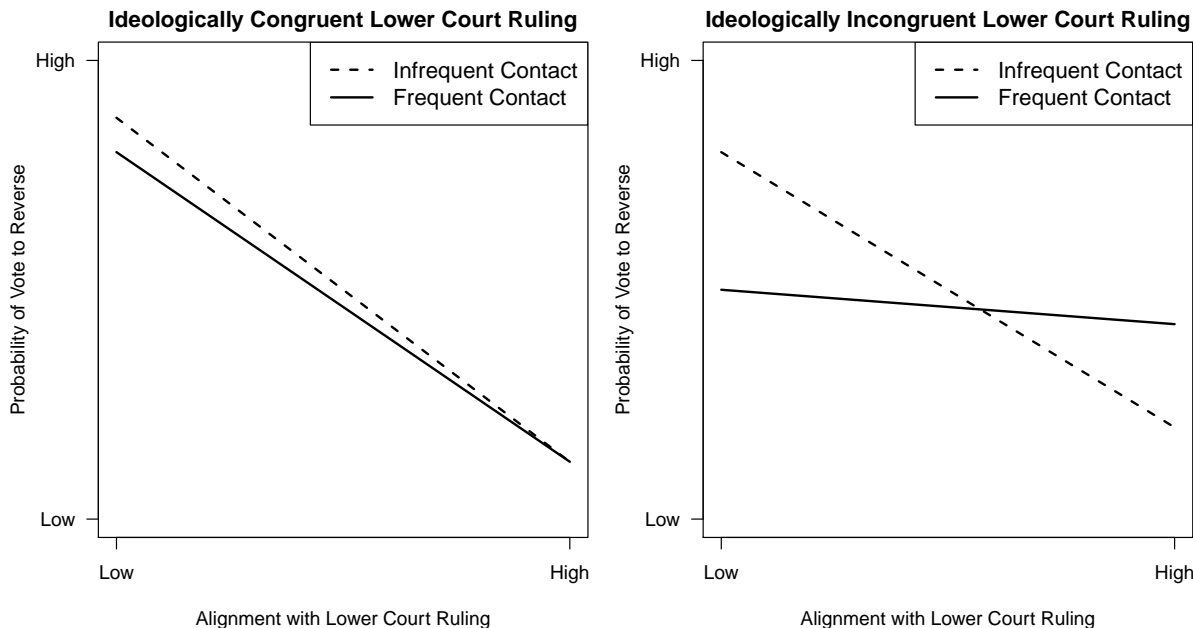


Figure C.1: Summary of Hypotheses.

body of the paper. Again, we estimate probit models, including fixed effects for circuit. We cluster the standard errors on the case.

Table C.1 provides our full regression results. The most telling conclusion from that table is the lack of statistical significance for the three-way interaction for either the full dataset or the matched dataset. While, in both datasets, frequent contact conditions the effect of *Aligned* and *Ideologically Congruent Lower Court Ruling*, there is no evidence from the table that *Ideologically Congruent Lower Court Ruling* conditions the relationship between *Aligned* and *Same Courthouse*.

Still, with any interaction term, further analysis is both helpful and necessary to fully understand the estimated effects. To assess the relationship between levels of interpersonal contact, ideological alignment, and the ideological congruence of the lower court ruling, Figure C.2 shows the predicted probabilities that correspond to Figure C.1 as well as the marginal effects that are included in our main analysis in Figure 1. When we limit our focus to the matched data, the hypothesized pattern illustrated above in Figure C.1 emerges. Recall that we expected the effect of *Aligned* to be weakest among judges who are reviewing ideologically incongruent lower court rulings made by judges with whom they have frequent contact. This corresponds to the solid line in the right-hand panels of Figure C.2. This estimated effect—while surprisingly having a positive point estimate—crosses zero, as expected. However, comparing that estimate with the corresponding estimate in the left-hand panel, it appears that the two estimates are not statistically different from one another. And, indeed, this is the case. In the matched data, for example, the differences in the marginal effect of *Aligned* between ideologically consistent and incongruent lower court rulings is not statistically significant either when two judges are in the same courthouse ($p=0.17$) or when they are in different courthouses ($p=0.14$).

This provides more evidence that the ideological congruence of the lower court judge and

her decision does not further condition our effect of interest. However, even when accounting for this possibility, Figure C.2 shows patterns markedly similar to those discussed in the main text of the paper. The marginal effect of *Aligned* is not statistically significant for judge dyads located in the same courthouse for any of the four situations presented in Figure C.2. All four panels also reveal that the marginal effect of *Aligned* is negative and statistically significant when the appellate and trial judges do not share the same courthouse. The key difference that emerges here is that the difference in the marginal effects of *Aligned* for same and different courthouse is not statistically significant in the model with the matched data when the lower court ruling was ideologically consistent.

	(3)		(4)	
	All Data		Matched Data	
	Coef.	S.E.	Coef.	S.E.
Aligned	-0.493*	(0.034)	-0.184*	(0.039)
Same Courthouse	0.086*	(0.036)	0.043	(0.036)
Aligned × Same Courthouse	0.598*	(0.097)	0.281*	(0.099)
Ideologically Congruent Lower Court Ruling	0.010	(0.016)	0.022	(0.020)
Aligned × Ideo. Cong. Lower Ct. Ruling	-0.023	(0.046)	0.082	(0.053)
Same Courthouse × Ideo. Cong. Lower Ct. Ruling	-0.146*	(0.048)	-0.153*	(0.050)
Aligned × Same Courthouse × Ideo. Cong. Lower Ct.	-0.153	(0.134)	-0.258	(0.137)
Split Panel, Majority	0.010	(0.018)	-0.016	(0.021)
Split Panel, Minority	0.015	(0.021)	-0.025	(0.024)
Lower Court Judge Credentials	0.012	(0.006)	0.017*	(0.007)
Same Law School	-0.029	(0.029)	0.020	(0.031)
DJ Sitting by Designation (Appellate)	-0.025	(0.025)	0.060	(0.052)
Prior Judicial Experience (Appellate)	-0.044*	(0.017)	-0.103*	(0.023)
Civil Rights	0.415*	(0.017)	0.386*	(0.021)
Habeas	0.081*	(0.030)	0.184*	(0.033)
Female Lower Court Judge	-0.070*	(0.027)	-0.000	(0.035)
Minority Lower Court Judge	0.171*	(0.024)	0.132*	(0.030)
Caseload	0.038*	(0.012)	0.082*	(0.017)
Year	-0.006*	(0.001)	-0.012*	(0.001)
Intercept	11.542*	(2.216)	22.452*	(2.761)
AIC	38610.5		27029.8	
BIC	38864.6		27272.8	
N	35,175		24,370	

Table C.1: Probit regression estimates of the effect of having chambers in the same courthouse, ideological factors, their interaction, and a range of control variables on the decision of whether to vote to reverse a lower court ruling. The models include fixed effects for circuit (not shown). The reported standard errors are robust standard errors that are clustered on the case and * denotes a p-value less than 0.05.

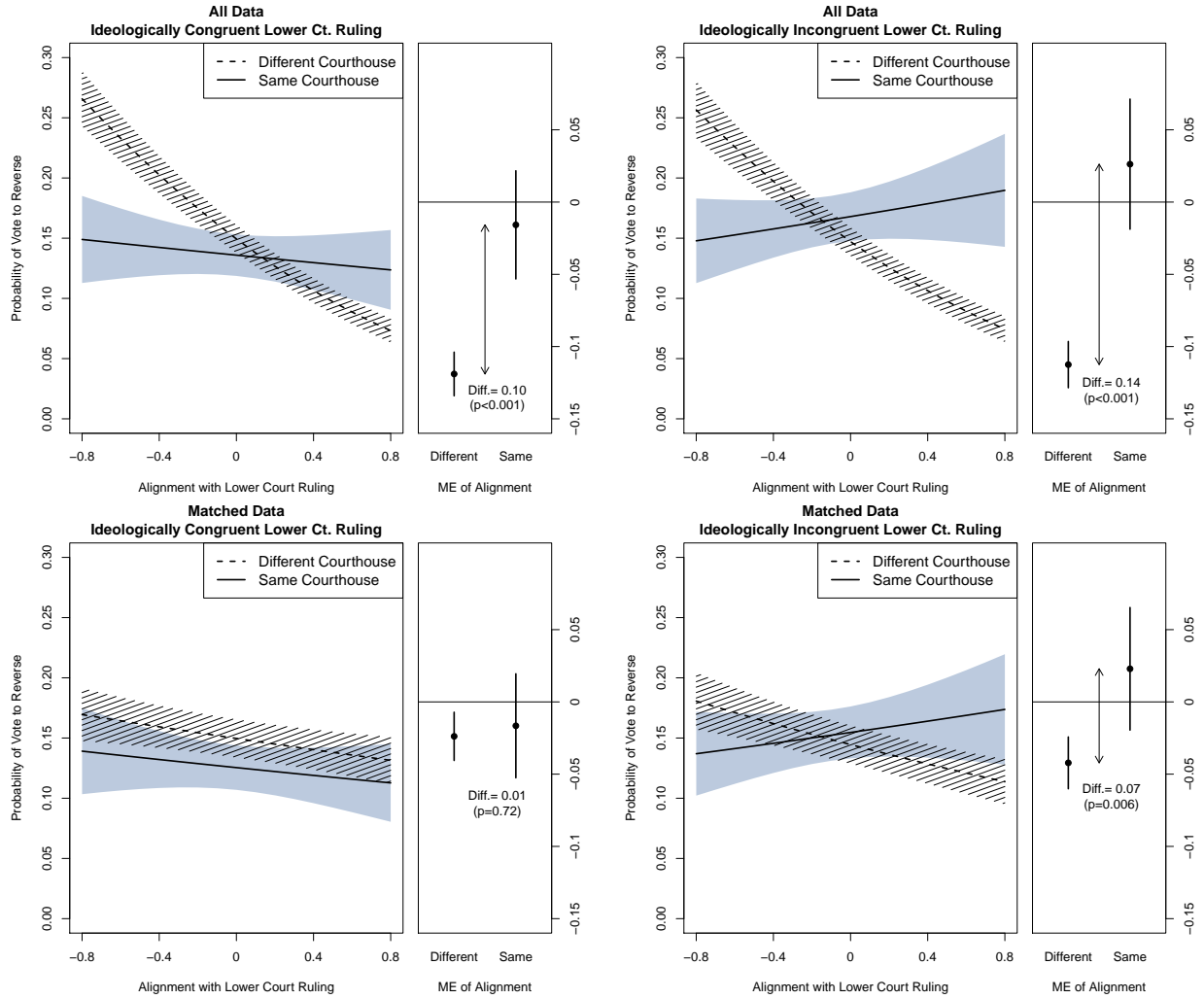


Figure C.2: Ideology, Contact, and Reversal: The left-hand panel in each block shows the predicted probability that an appellate judge in a different courthouse or the same courthouse as the lower court judge votes to reverse across all values of *Aligned*. The right-hand panel in each block shows the marginal effect of *Aligned* for same and different courthouse and the difference in those marginal effects (as well as the p-value of that difference). 95% confidence intervals are included.

Appendix D: Alternative Measurement of Ideology

In order to assess the robustness of our results to an alternative measure of judicial ideology, we re-estimated our model with a measure of *Aligned* calculated using Campaign Finance scores (Bonica and Sen 2017) in place of Judicial Common Space scores. Bonica and Sen (2017) generate ideological scores for federal judges based on campaign contributions made to and by those individuals prior to their ascension to the federal bench. When such data are unavailable, Bonica and Sen impute CF scores for a judge using a sophisticated model incorporating over a dozen variables (including JCS scores) (Bonica and Sen 2017).

Our main analyses rely on Judicial Common Space scores because availability presents limitations for the utility of CF Scores for our purposes. First, our dataset goes back to 1953, a date prior to the availability of CF Scores. Second, many federal judges never donated to political campaigns before ascending to the federal bench. As a result of these two factors, actual (non-imputed) CF Scores are only available for 24% of our data. Even using the imputed CF Scores, we are forced to drop 15% of our observations because no CF Score is available.

With these caveats, Table D.1 shows the results of regression models replacing our *Aligned* measure with one that is calculated the same way except that CF scores are used in place of JCS scores. The matched data used for Model 6 were matched using the same process described above in Appendix A with the exception that matching was done with our measure of *Aligned* that is based on CF scores in place of the measure used based on JCS scores.

Figure D.1 shows how the results of Models 5 and 6 compare to the results of Models 1 and 2. Our main conclusion that *Same Courthouse* significantly dampens the effect of ideology on the probability of reversal is borne out by these models. For both the full dataset and the matched data the marginal effect of *Aligned* is significantly larger (and negative) when two judges do not have chambers in the same courthouse compared to when they are in the same courthouse. The one key difference in these models is that, while dampened, ideology still appears to significantly affect the decision to reverse when the appellate judge is in the same courthouse as the lower court judge.

	(5)		(6)	
	All Data		Matched Data	
	Coef.	S.E.	Coef.	S.E.
Aligned (CF scores)	-0.286*	(0.013)	-0.240*	(0.017)
Same Courthouse	-0.007	(0.028)	-0.040	(0.028)
Aligned (CF scores) \times Same Courthouse	0.180*	(0.040)	0.125*	(0.042)
Ideologically Congruent Lower Court Ruling	-0.020	(0.016)	-0.004	(0.020)
Split Panel, Majority	0.012	(0.019)	-0.009	(0.024)
Split Panel, Minority	0.006	(0.022)	-0.065*	(0.027)
Lower Court Judge Credentials	0.011	(0.007)	0.012	(0.008)
Same Law School	-0.026	(0.031)	0.020	(0.035)
DJ Sitting by Designation (Appellate)	-0.018	(0.029)	-0.039	(0.061)
Prior Judicial Experience (Appellate)	-0.057*	(0.019)	-0.140*	(0.026)
Civil Rights	0.408*	(0.018)	0.399*	(0.022)
Habeas	0.029	(0.036)	0.158*	(0.041)
Female Lower Court Judge	-0.065*	(0.028)	0.021	(0.035)
Minority Lower Court Judge	0.167*	(0.025)	0.140*	(0.031)
Caseload	0.032*	(0.013)	0.048*	(0.018)
Year	-0.008*	(0.001)	-0.013*	(0.002)
Intercept	14.894*	(2.493)	24.789*	(3.123)
AIC	32680.5		21180.7	
BIC	32904.8		21395.7	
N	30,028		21,260	

Table D.1: Probit regression estimates of the effect of having chambers in the same courthouse, ideological factors, their interaction, and a range of control variables on the decision of whether to vote to reverse a lower court ruling. The models include fixed effects for circuit (not shown). The reported standard errors are robust standard errors that are clustered on the case and * denotes a p-value less than 0.05.

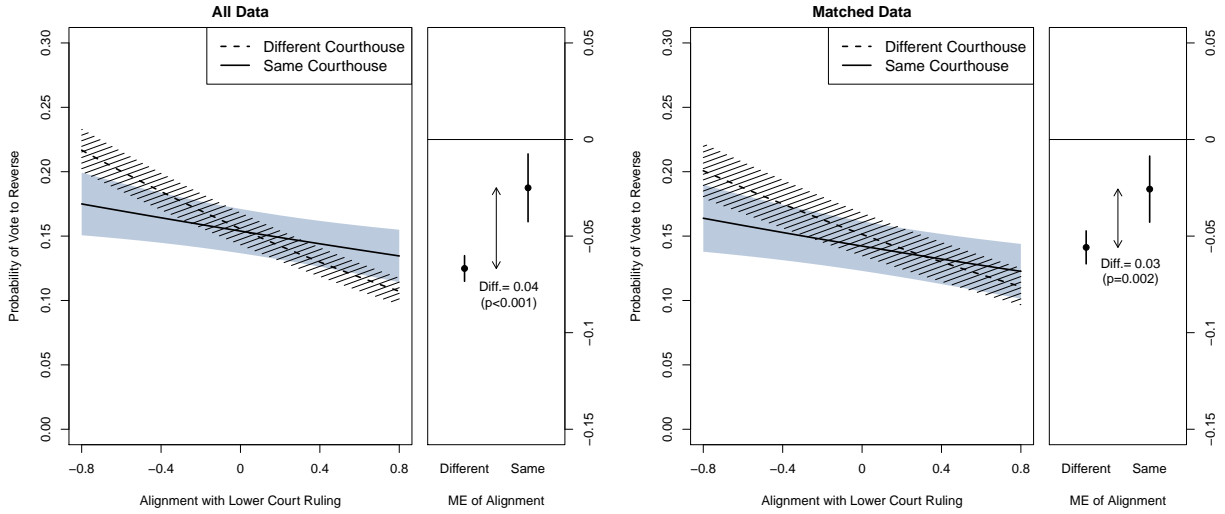


Figure D.1: Ideology, Contact, and Reversal using CF scores to calculate *Aligned*: The left-hand panel in each block shows the predicted probability that an appellate judge in a different courthouse or the same courthouse as the lower court judge votes to reverse across all values of *Aligned*. The right-hand panel in each block shows the marginal effect of *Aligned* for same and different courthouse and the difference in those marginal effects (as well as the p-value of that difference). 95% confidence intervals are included.

Appendix E: Alternative Measure of Frequent Contact

The analyses in the previous appendices rely on a measure of frequent contact that indicates whether or not an appellate-trial judge pair work in the same building. Some might argue that operationalization is too stringent because judges who work in the same city are also likely to have frequent interactions. A little over one percent of the observations in our data consist of appellate-trial judge pairings that work in the same city but not the same courthouse. Put differently, of the judge pairs who are located in the same city, 10.5% of them are not located in the same courthouse and, therefore, are zeros for the *Same Courthouse* measure. To evaluate the effect of thinking about frequent contact in a somewhat broader sense, we re-estimated the models using a measure of *Same City* rather than *Same Courthouse*. The results presented in Table E.1 and Figure E.1 demonstrate that we reach the same conclusions.

	(7)		(8)	
	All Data		Matched Data	
	Coef.	S.E.	Coef.	S.E.
Aligned	-0.509*	(0.023)	-0.174*	(0.026)
Same City	0.006	(0.024)	-0.047	(0.024)
Aligned × Same City	0.479*	(0.064)	0.135*	(0.065)
Ideologically Congruent Lower Court Ruling	-0.004	(0.015)	0.005	(0.017)
Split Panel, Majority	0.010	(0.018)	-0.025	(0.020)
Split Panel, Minority	0.016	(0.020)	-0.023	(0.024)
Lower Court Judge Credentials	0.012	(0.006)	0.024*	(0.007)
Same Law School	-0.028	(0.029)	0.010	(0.030)
DJ Sitting by Designation (Appellate)	-0.025	(0.025)	-0.020	(0.049)
Prior Judicial Experience (Appellate)	-0.044*	(0.017)	-0.099*	(0.022)
Civil Rights	0.415*	(0.017)	0.377*	(0.020)
Habeas	0.080*	(0.030)	0.144*	(0.032)
Female Lower Court Judge	-0.069*	(0.027)	-0.028	(0.034)
Minority Lower Court Judge	0.171*	(0.024)	0.141*	(0.029)
Caseload	0.037*	(0.012)	0.065*	(0.015)
Year	-0.006*	(0.001)	-0.010*	(0.001)
Intercept	11.480*	(2.215)	19.795*	(2.586)
AIC	38624.6		28800.5	
BIC	38850.3		29020.6	
N	35,179		25,679	

Table E.1: Probit regression estimates of the effect of having chambers in the same city, ideological factors, their interaction, and a range of control variables on the decision of whether to vote to reverse a lower court ruling. The models include fixed effects for circuit (not shown). The reported standard errors are robust standard errors that are clustered on the case and * denotes a p-value less than 0.05.

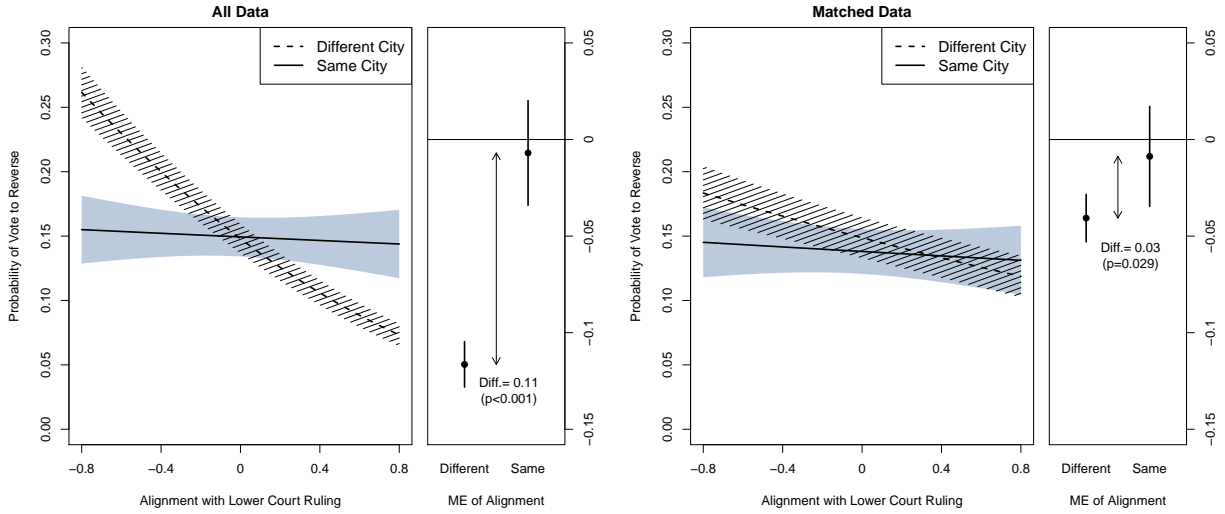


Figure E.1: Ideology, Contact, and Reversal using *Same City*: The left-hand panel in each block shows the predicted probability that an appellate judge in a different city or the same city as the lower court judge votes to reverse across all values of *Aligned*. The right-hand panel in each block shows the marginal effect of *Aligned* for same and different city and the difference in those marginal effects (as well as the p-value of that difference). 95% confidence intervals are included.

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