How Interpersonal Contact Affects Appellate Review
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 We excluded unpublished opinions because they are not readily available for the time-span covered here (Sunstein et al. 2006).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 Courthouses[1] 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

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[1]Available 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

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[2]We 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.
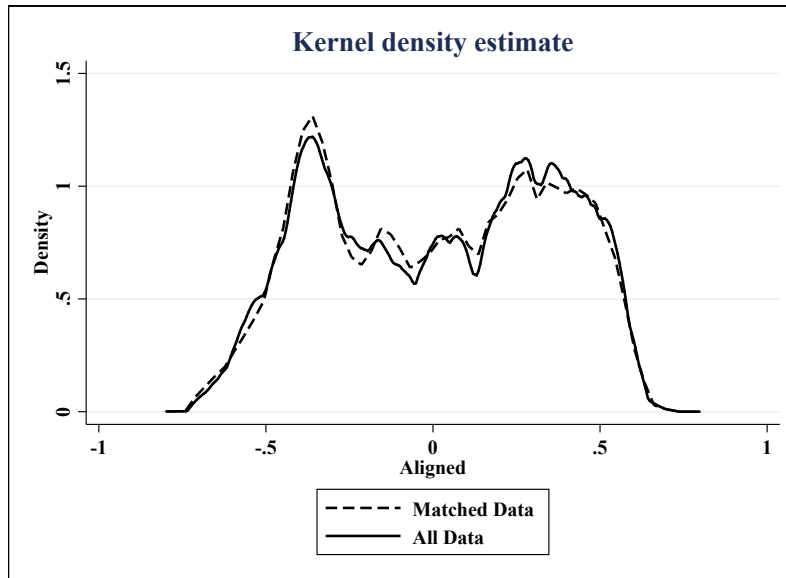

Kernel density estimate

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	4.5

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* ≤ 0.12) such a pattern does emerge to a significant extent in both models. The hypothesized pattern exists in 39% of the data.

	(1) All Data		(2) 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.

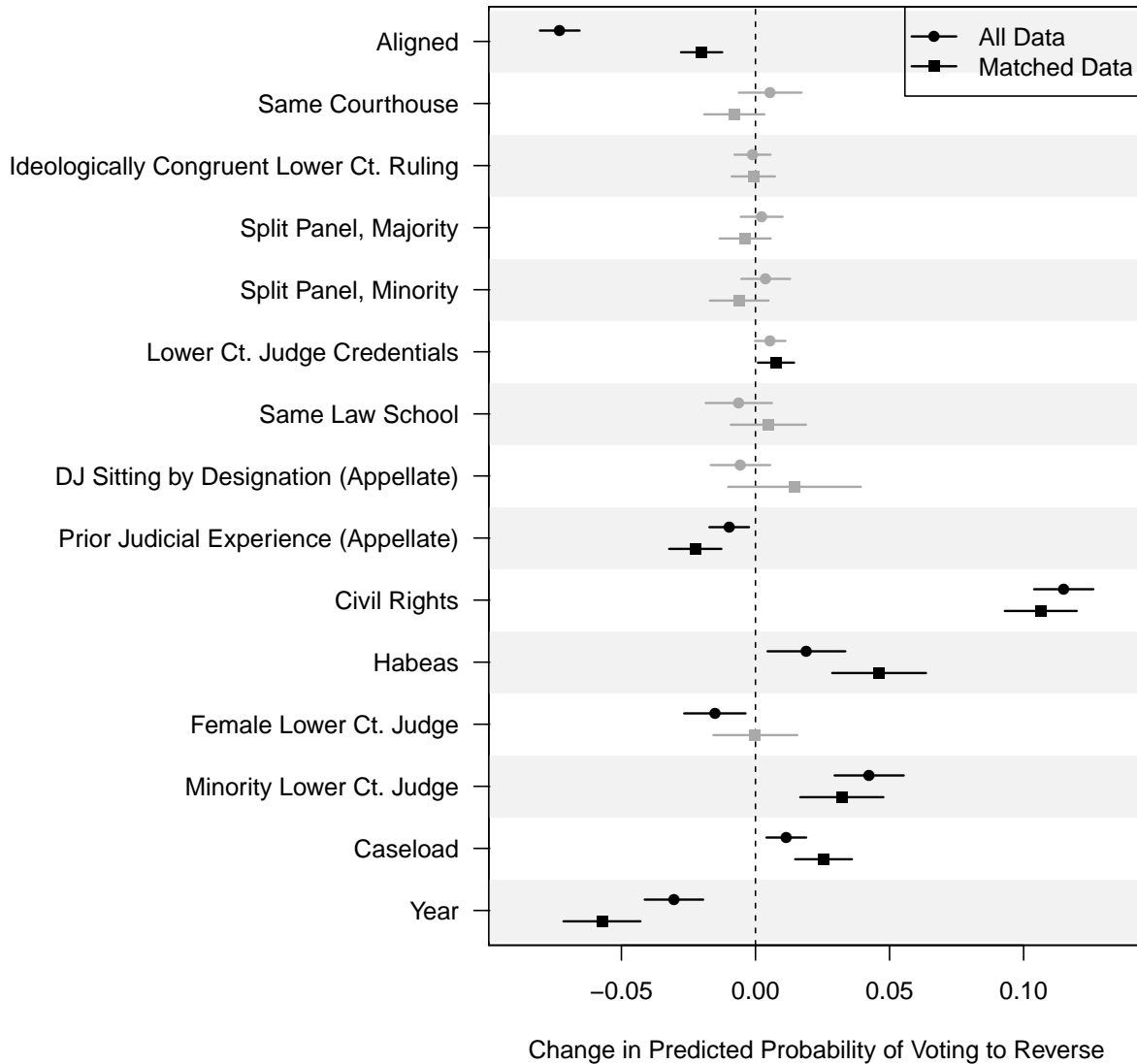[3] All discussion of statistical significance is at the 0.05 level.

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
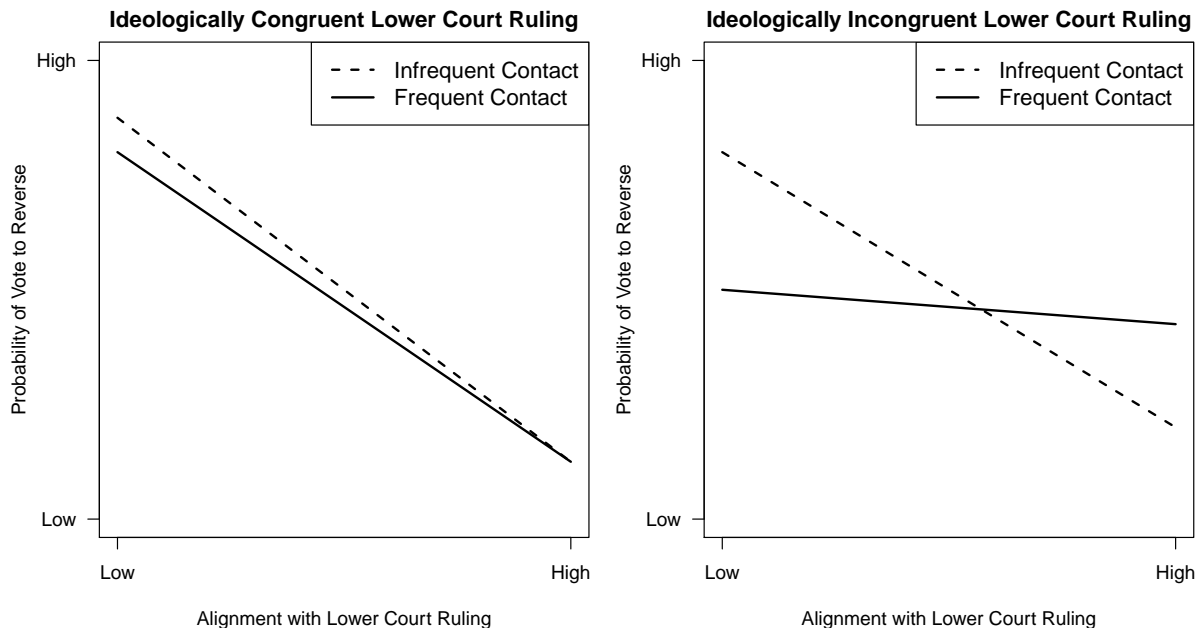
Figure C.1: Summary of Hypotheses.

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

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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) All Data		(4) 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.
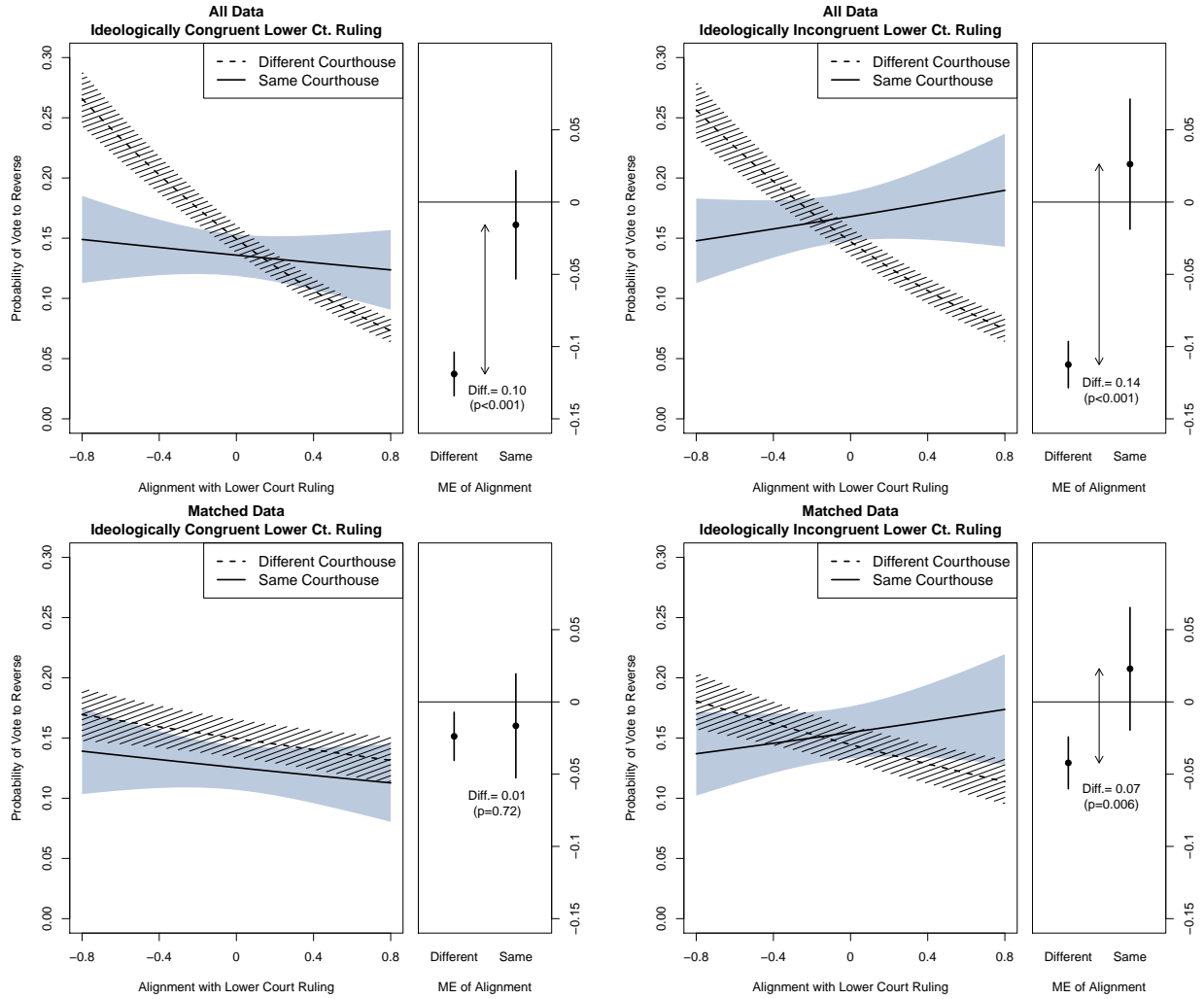
Figure C.2: Ideology, Contact, and Reversal: The left-hand panel in each block shows the predicted probability that an appellate judge in a different courthouse or the same courthouse as the lower court judge votes to reverse across all values of *Aligned*. The right-hand panel in each block shows the 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) × 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.
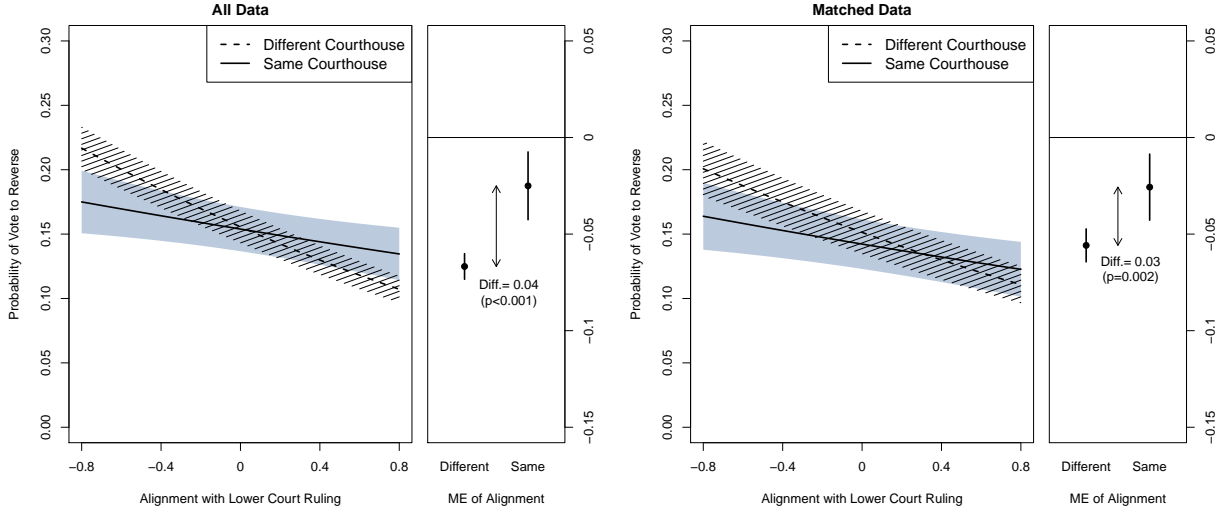
Figure D.1: Ideology, Contact, and Reversal using CF scores to calculate *Aligned*: The left-hand panel in each block shows the predicted probability that an appellate judge in a different courthouse or the same courthouse as the lower court judge votes to reverse across all values of *Aligned*. The 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) All Data		(8) Matched Data	
	Coef.	S.E.	Coef.	S.E.
Aligned	−0.509*	(0.023)	−0.174*	(0.026)
Same City	0.005	(0.024)	−0.048*	(0.024)
Aligned × Same City	0.481*	(0.064)	0.136*	(0.065)
Ideologically Congruent Lower Court Ruling	−0.005	(0.015)	0.004	(0.017)
Split Panel, Majority	0.010	(0.018)	−0.024	(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)	−0.019	(0.049)
Prior Judicial Experience (Appellate)	−0.044*	(0.017)	−0.098*	(0.022)
Civil Rights	0.415*	(0.017)	0.377*	(0.020)
Habeas	0.081*	(0.030)	0.143*	(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	0.038*	(0.012)	0.065*	(0.015)
Year	−0.006*	(0.001)	−0.010*	(0.001)
Intercept	11.505*	(2.216)	19.817*	(2.587)
AIC	38615.3		28792.4	
BIC	38843.9		29012.5	
N	35,175		25,675	

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.
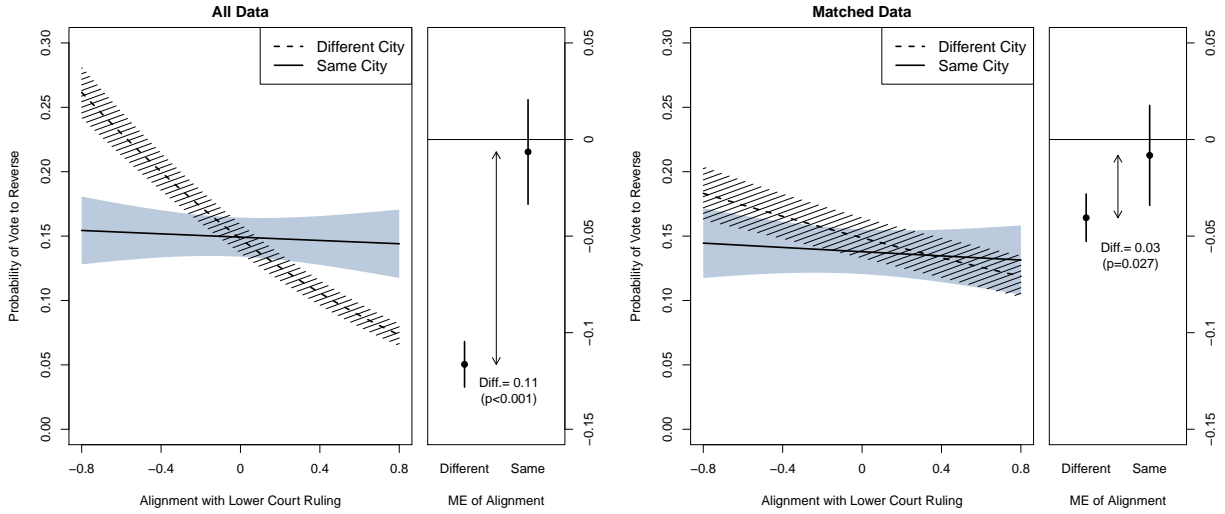
Figure E.1: Ideology, Contact, and Reversal using *Same City*: The left-hand panel in each block shows the predicted probability that an appellate judge in a different city or the same city as the lower court judge votes to reverse across all values of *Aligned*. The right-hand panel in each block shows 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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