

# Supplementary/Online Appendix for “The Swing Justice”

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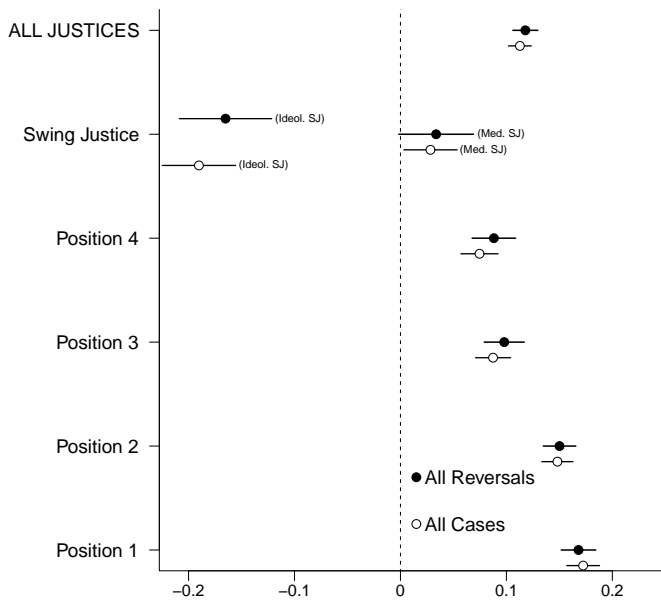
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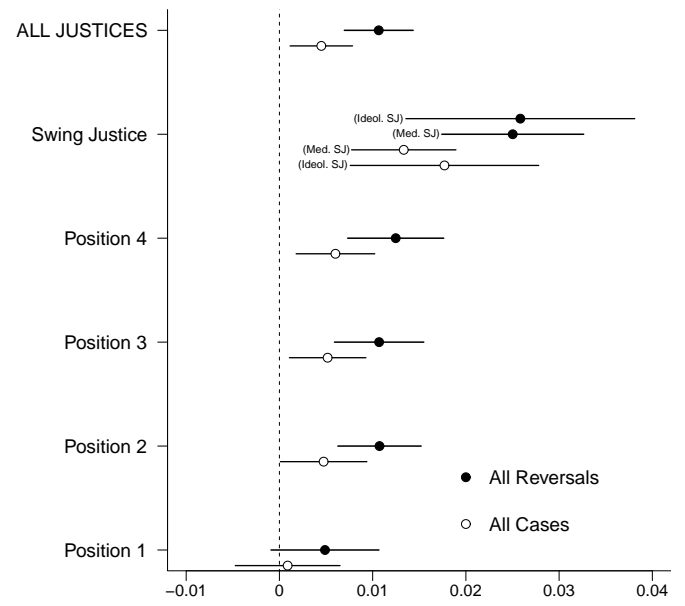
# 1 Appendix 1: All Cases Versus Reversals

The results reported in the text analyze all cases that reversed the lower court's ruling and all 5-4 decisions that reversed the lower court's ruling. McGuire, Vanberg, Smith & Caldeira (2009) show that because strategic litigants consider the costs of litigation and the probability of success (Songer, Cameron & Segal 1995), the cases appealed to the Supreme Court are endogenous to the Court's ideological composition. Particularly for the most moderate justices, this endogeneity produces an affirmation bias where the ideological direction of the Court's affirmances will be disproportionately more liberal (conservative) as the Court itself becomes more conservative (liberal). To avoid this bias, the analyses follow previous research (e.g. Casillas, Enns & Wohlfarth 2011, McGuire & Stimson 2004, McGuire et al. 2009) and only analyze reversals.

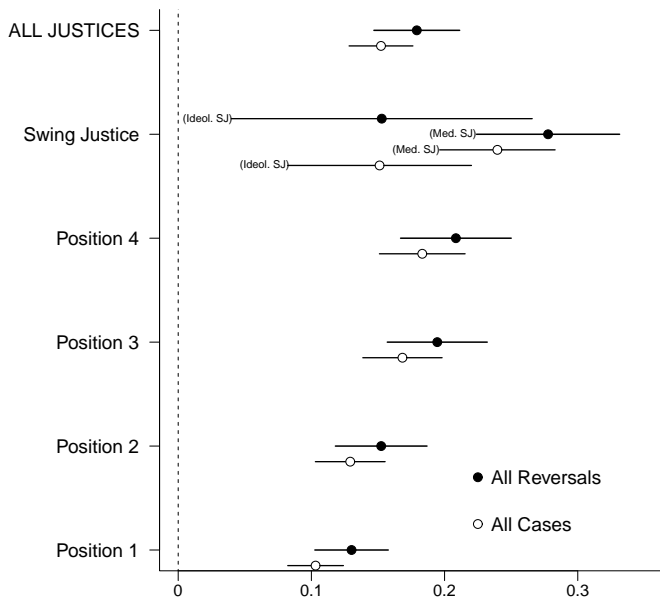
Of course, reducing affirmation bias comes at a cost of information, because we are forced to drop observations. To evaluate whether this decision affects our results, Figure A-1 presents the results of the analysis reported in the text (all cases that reversed the lower court's ruling) alongside an analysis that relies on *all* cases (i.e., reversals and affirmances). Figure A-2 reports the results reported in the text for 5-4 decisions (all 5-4 decisions that reversed the lower court's ruling) and *all* 5-4 decisions. In both figures, the results are remarkably similar and indicate that analyzing only reversals (which we believe is the theoretically appropriate decision) does not lead to markedly different results than when analyzing all cases. The most important difference, which we note in the text, is that when we analyze all cases, the confidence interval for the average marginal effect of justice ideology for the median swing justice no longer crosses the zero line. Consistent with expectations, however, this coefficient is significantly smaller than the estimated relationship for the all justices analysis, and we continue to observe the increasing monotonic pattern across justice position.



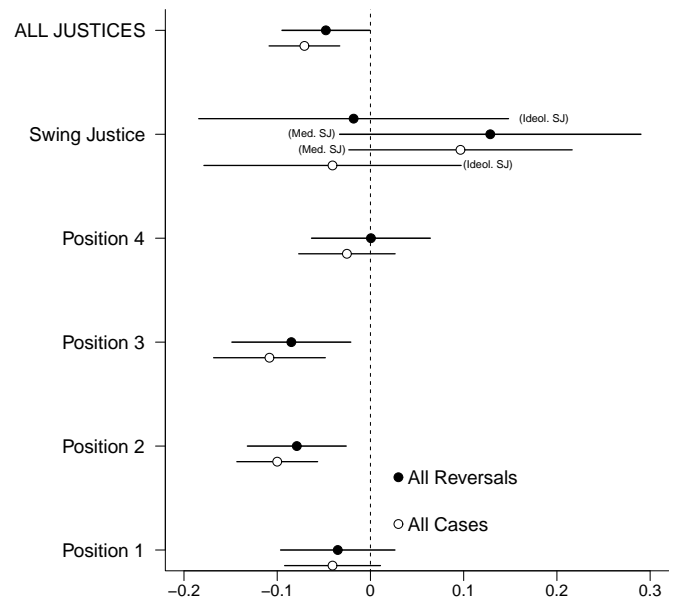
(a) Justice Ideology



(b) Policy Mood



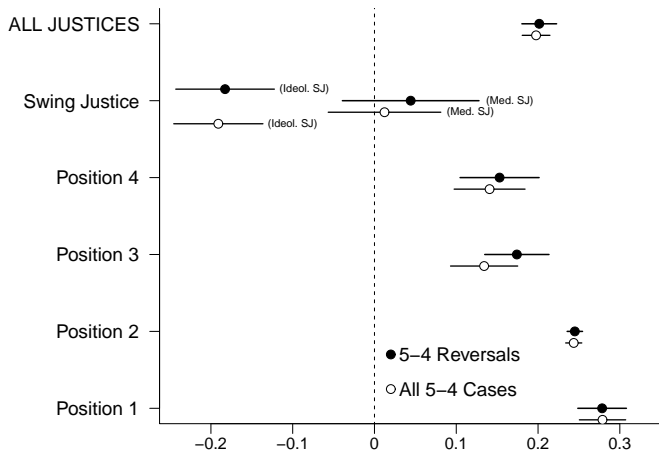
(c) Solicitor General



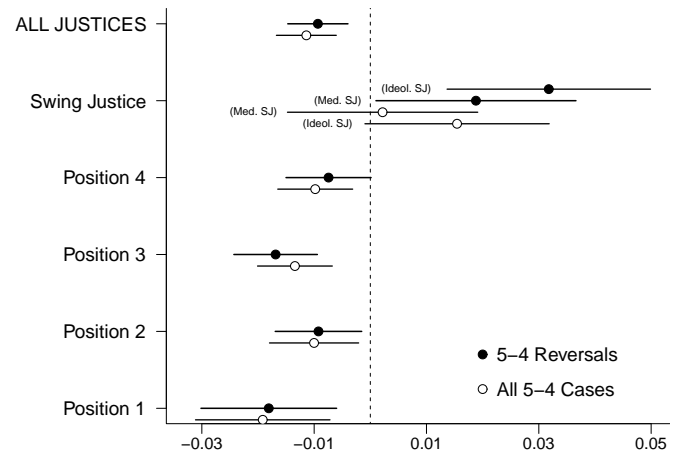
(d) Separation of Powers

Figure A-1: The Average Marginal Effect (with 95% Confidence Intervals) for ALL JUSTICES and by Justice Position, among all cases (hollow dots) and all cases that reversed the lower court's decision (solid dots)

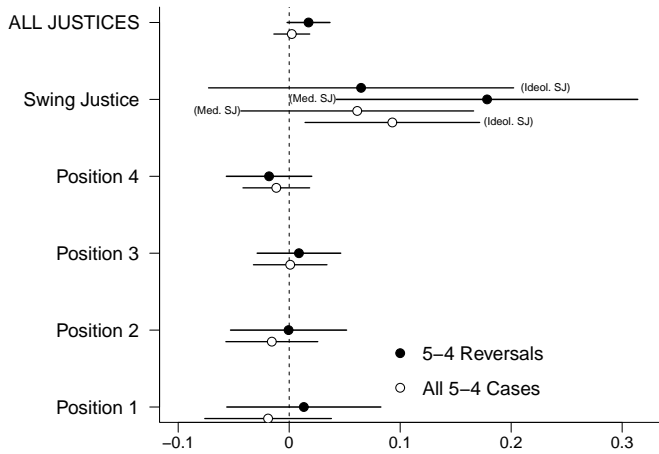
*Note:* The average marginal effect is based on separate logistic regressions. The top row (ALL JUSTICES) reports the average marginal effect based on a regression that includes all justices participating in each case. Each subsequent row reports the results for specific justice positions, based on proximity to the median position. Position 4 corresponds with the two justices that are the closest ideologically to the Court's median. Position 1, by contrast, corresponds with the justices that are farthest ideologically.



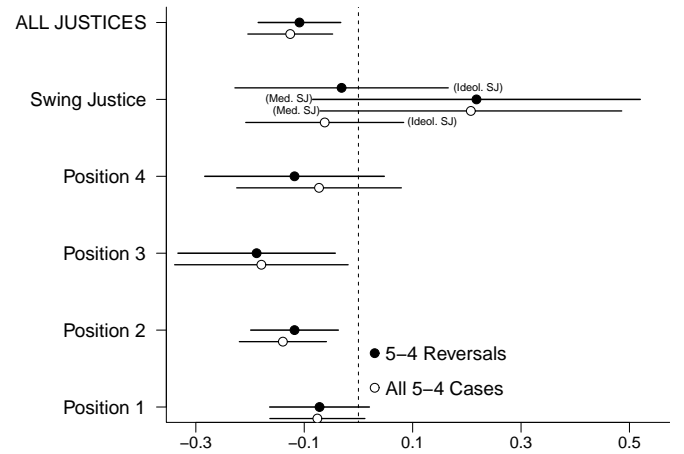
(a) Justice Ideology



(b) Policy Mood



(c) Solicitor General



(d) Separation of Powers

Figure A-2: The Average Marginal Effect (with 95% Confidence Intervals) for ALL JUSTICES and by Justice Position, among all 5-4 cases (hollow dots) and all 5-4 cases that reversed the lower court's decision (solid dots)

*Note:* The average marginal effect is based on separate logistic regressions. The top row (ALL JUSTICES) reports the average marginal effect based on a regression that includes all justices participating in each case. Each subsequent row reports the results for specific justice positions, based on proximity to the median position. Position 4 corresponds with the two justices that are the closest ideologically to the Court's median. Position 1, by contrast, corresponds with the justices that are farthest ideologically.

## 2 Appendix 2: Complete Regression Results

The Figures in the text report the Average Marginal Effect (AME) based on logistic regressions. We report the AME because this offers a summary measure of each predictor’s expected influence on the probability of a liberal vote that is suitable for intergroup comparisons (Mood 2010). Below, we report the full regression results. Table A-1 corresponds with all cases that reversed the lower court’s decision (Figure 1) and Table A-2 corresponds with all 5-4 decisions that reversed the lower court’s decision (Figure 2).<sup>1</sup> As noted in the text, the models control for potential differences across issue areas on the Court’s docket. Cases primarily involving issues of federalism represent the baseline category. We code issues based on the “issueArea” variable in the Supreme Court Database. Civil Liberties cases involve Criminal Procedure, Civil Rights, First Amendment, Due Process, Privacy, and Attorneys. The Economic Activity category includes Unions, Economic Activity, and Taxation.<sup>2</sup> Importantly, the coefficients for the SG, public opinion, ideology, and SOP that are reported below reveal the same heterogeneity across justice positions that we observe in the figures in the text.<sup>3</sup>

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<sup>1</sup>The variation in sample sizes across regressions results because when a justice who is not the term-specific median (i.e., Position1 through 4) casts the swing vote, we code this as the ideological swing justice, which reduces the total number of observations for each ideological position. This decision reflects our theoretical argument that justices that typically vote in an ideological manner incorporate different considerations when they cast the pivotal swing vote. However, to ensure this decision did not influence our conclusions, we re-estimated each regression for positions 1 through 4 including the ideological swing justices in their original ideological positions. Because a small proportion of the justice votes at each ideological position are coded as an ideological swing vote, not surprisingly, this change made no substantive difference in the results for the various justice positions.

<sup>2</sup>The results do not change when substituting fixed effects to account for each separate “issueArea” category. We do not include justice fixed effects because a justice’s Cameron-Park score is constant across his or her tenure, and is thus redundant with justice fixed effects.

<sup>3</sup>Consistent with Edelman, Klein & Lindquist’s (2008) conclusion that different issue areas do not explain different voting coalitions, the coefficients for each issue area do not differ systematically across justice position.

Table A-1: The Determinants of the Probability of a Liberal Supreme Court Vote for All Justices and by Justice Position, 1953–2007

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	All	Median	Ideol.	Justice	Justice	Justice	Justice
	Justices	Swing	Swing	Pos.	Pos.	Pos.	Pos.
		Justice	Justice	4	3	2	1
Solicitor General	0.81*	1.29*	0.73*	0.93*	0.87*	0.73*	0.63*
	(0.08)	(0.14)	(0.28)	(0.10)	(0.09)	(0.09)	(0.07)
Public Mood	0.05*	0.12*	0.12*	0.06*	0.05*	0.05*	0.02
	(0.01)	(0.02)	(0.03)	(0.01)	(0.01)	(0.01)	(0.01)
Justice Ideology	0.53*	0.19	-0.79*	0.39*	0.44*	0.72*	0.81*
	(0.03)	(0.10)	(0.13)	(0.05)	(0.05)	(0.05)	(0.05)
SOP Constraint	-0.22*	0.60	-0.05	0.00	-0.38*	-0.38*	-0.17
	(0.11)	(0.39)	(0.40)	(0.14)	(0.15)	(0.13)	(0.15)
Judicial Power	-0.85*	-1.04*	-0.52	-0.84*	-0.90*	-0.92*	-0.74*
	(0.18)	(0.29)	(0.52)	(0.21)	(0.20)	(0.19)	(0.21)
Economics	-0.41*	-0.41	-0.12	-0.38	-0.43*	-0.48*	-0.44*
	(0.17)	(0.30)	(0.41)	(0.21)	(0.19)	(0.17)	(0.20)
Civil Liberties	-0.28	-0.38	0.08	-0.41*	-0.27	-0.25	-0.16
	(0.17)	(0.30)	(0.42)	(0.20)	(0.19)	(0.17)	(0.19)
Constant	-2.27*	-6.35*	-6.84*	-2.68*	-2.21*	-2.40*	-0.87
	(0.53)	(1.19)	(2.00)	(0.76)	(0.70)	(0.67)	(0.82)
Pseudo R <sup>2</sup>	0.08	0.11	0.11	0.07	0.07	0.12	0.13
N	31,186	2,734	733	6,508	6,770	6,834	6,871

*Note:* The dependent variable reflects decisions by individual justices to support a liberal outcome among cases that reversed the lower court's decision. Two-Tailed Significance Levels: \*  $p < .05$ . Standard errors (clustered by term) in parentheses.

Table A-2: The Determinants of the Probability of a Liberal Supreme Court Vote for All Justices and by Justice Position among 5 to 4 Decisions, 1953–2007

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	All	Median	Ideol.	Justice	Justice	Justice	Justice
	Justices	Swing	Swing	Pos.	Pos.	Pos.	Pos.
		Justice	Justice	4	3	2	1
Solicitor General	0.09 (0.05)	0.83* (0.33)	0.32 (0.34)	-0.08 (0.09)	0.04 (0.09)	-0.00 (0.18)	0.09 (0.25)
Public Mood	-0.05* (0.01)	0.09* (0.04)	0.16* (0.05)	-0.03 (0.02)	-0.08* (0.02)	-0.06* (0.03)	-0.13* (0.05)
Justice Ideology	0.98* (0.08)	0.21 (0.20)	-0.90* (0.20)	0.67* (0.13)	0.86* (0.16)	1.68* (0.16)	1.99* (0.24)
SOP Constraint	-0.53* (0.19)	1.02 (0.74)	-0.15 (0.50)	-0.52 (0.38)	-0.87* (0.36)	-0.81* (0.28)	-0.51 (0.35)
Judicial Power	-0.18 (0.12)	0.25 (1.09)	0.07 (0.71)	-0.45 (0.23)	0.29 (0.29)	-0.87 (0.56)	0.66 (0.80)
Economics	-0.33* (0.11)	1.09 (0.98)	-0.31 (0.72)	-0.35 (0.23)	-0.03 (0.20)	-0.93 (0.53)	0.42 (0.59)
Civil Liberties	-0.17 (0.11)	0.92 (0.90)	-0.11 (0.68)	-0.51* (0.21)	0.16 (0.21)	-0.60 (0.49)	1.16 (0.60)
Constant	2.88* (0.81)	-6.49* (2.66)	-8.63* (3.04)	2.33* (1.05)	4.55* (1.17)	4.41* (1.76)	6.73* (2.72)
Pseudo R <sup>2</sup>	0.14	0.09	0.13	0.07	0.10	0.34	0.37
N	5,783	353	290	1,174	1,202	1,254	1,220

*Note:* The dependent variable reflects decisions by individual justices to support a liberal outcome among 5 to 4 decisions that reversed the lower court's decision. Two-Tailed Significance Levels: \*  $p < .05$ . Standard errors (clustered by term) in parentheses.

### 3 Appendix 3: Separation of Powers Measures

For the analyses reported in the text, we measured the degree of separation of powers constraint as the ideological distance between each justice and the nearest institutional pivot when that justice is either more conservative or more liberal than the president and both chambers of Congress.<sup>4</sup> Debate exists, however, regarding how to measure the pivotal actor in each chamber of Congress. Thus, we also estimated our models with two alternate SOP measures, substituting the filibuster pivot and then majority party medians for the chamber medians (as used to construct the measure reported in the text). The results are reported below in Figure A-3.

This figure reports the results for these three measures of SOP constraint in addition to results for Justice Ideology, Mood, and SG based on a regression including all justices as well as separate regressions for the median swing justice and the ideological swing justice. The analysis corresponds with all cases that reversed the lower court (Figure 3(a)) and all 5-4 decisions that reversed to lower court's ruling (Figure 3(b)). Whether we look at all cases or 5-4 decisions, two patterns stand out. First, looking at the results for Justice Ideology, Policy Mood, and Solicitor General, we see that our estimates are remarkably robust to the specification used to measure the pivotal actor in Congress. Second, we see that our estimates of the relationship between SOP constraint and the probability of casting a liberal vote are somewhat sensitive to the different specifications of Congress's pivotal actor. However, the general pattern of results for SOP constraint is consistent across measures. For the analysis of all justices, the coefficient for SOP constraint is always negative. Despite this sensitivity, looking at all cases and 5-4 decisions, five of the six coefficients for the median swing justice are positive. Thus, while the specific results for SOP constraint depend on the measure being used, the finding that the relationship between SOP considerations and vote is different for the median swing justice appears to be relatively consistent.

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<sup>4</sup>We also standardized each justice's SOP score to a mean of zero (and retain the original variance). This step is necessary because justice ideology is confounded with distance from the institutional pivot. Standardization in this way assigns the same mean SOP score to all justices while preserving the over time variation in the distance between each justice and the nearest institutional pivot.



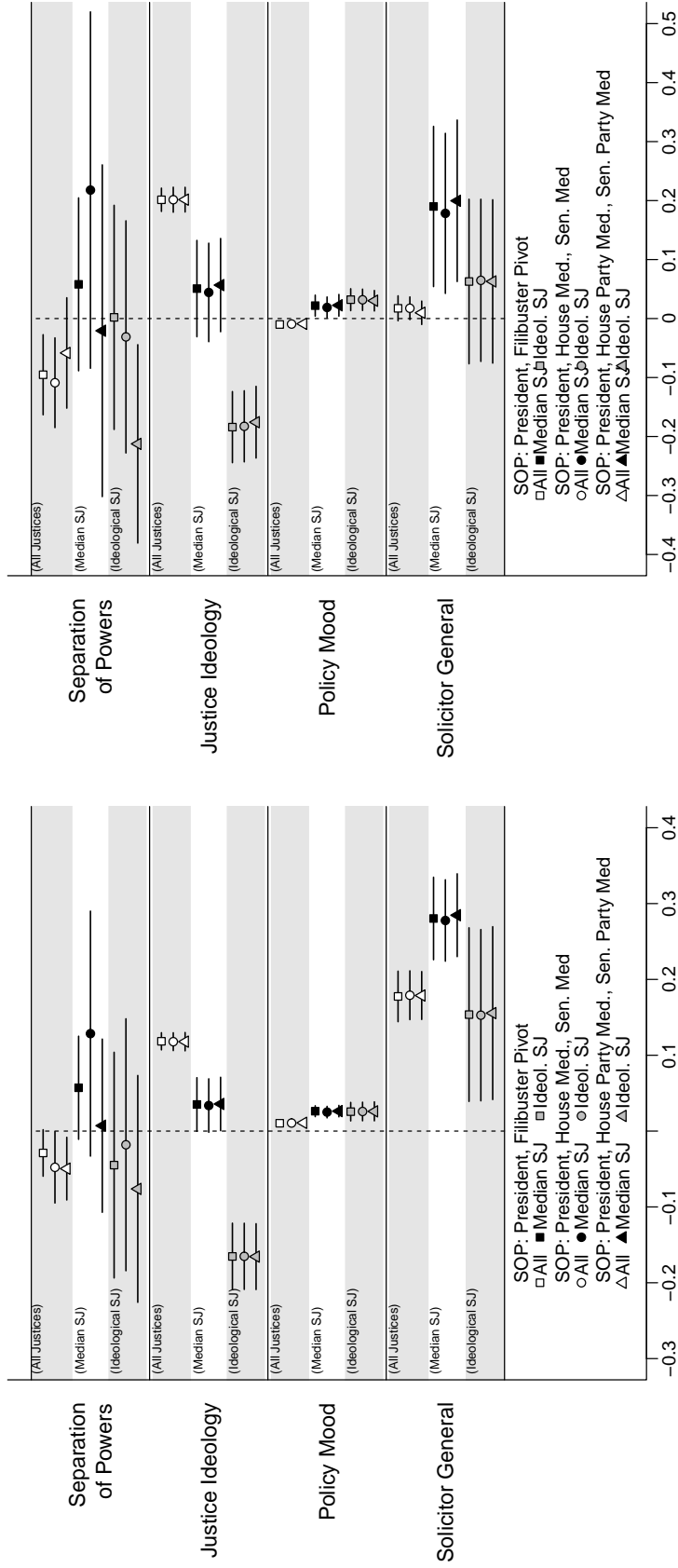


Figure A-3: The Average Marginal Effect (with 95% Confidence Intervals) for ALL JUSTICES, the Median Swing Justice, and the Ideological Swing Justice among all cases and 5-4 cases that reversed the lower court's decision, using three different measures of Separation of Powers

Note: Each figure presents the results for three justice types (all justices, median swing justice, and ideological swing justice) with three different measures of SOP. The y-axis corresponds with the predictor variable of interest.

## 4 Appendix 4: Justice Ideology Measures

### 4.1 Segal-Cover versus Cameron-Park Scores

Similar to the exogenous Segal & Cover (1989) scores, the Cameron-Park ideology measure is largely exogenous to justice votes (Cameron & Park 2009). We relied on the Cameron-Park scores because they incorporate more information than the Segal-Cover scores and do a better job predicting future justice votes (Cameron & Park 2009). Thus, the Cameron-Park scores offer the most valid available indicator of justice ideology that is—for the most part—exogenous to justice vote.<sup>5</sup> Despite the desirable properties of this measure, we want to be sure that our results are not sensitive to this decision.

For this reason, we replicated our analyses using the updated Segal-Cover scores (Segal & Cover 1989, Segal, Epstein, Cameron & Spaeth 1995) as our measure of ideology. For these analyses, we analyzed only civil liberties cases.<sup>6</sup> The Segal-Cover scores have been shown to perform best in this issue area (Segal & Cover 1989, Epstein & Mershon 1996). Thus, by limiting our analysis to this subset of cases, we are offering a *best* case scenario for this measure (and for the attitudinal model). Figure A-4 reports the average marginal effect (and 95-percent confidence interval) for ideology, mood, solicitor general, and SOP considerations for separate regression models of all justices, the median swing justice, the ideological swing justice, and the four remaining justice positions (where position 4 is adjacent to the swing justice and position 1 is the most ideologically distant from the swing justice). In half of the models, we used the Cameron-Park scores as our measure of ideology (solid dots) and in the other half we used the Segal-Cover scores (hollow dots).

Two results stand out. First, regardless of whether we use the Cameron-Park or Segal-Cover scores, the estimated relationship between the predictors and the probability of casting a liberal vote is very similar. Given the strong correlation between these two measures ( $r=0.87$ ), these similarities are not surprising. Second, the results are consistent with our two hypotheses that: 1.) the relationship between predictor variables and vote choice will differ for the swing justice and 2.) the differences across justices will shift monotonically. It is important to note that the results for ideology when using the Segal-Cover scores (Panel a) indicate that the relationship for justice ideology and the probability of casting a liberal vote is statistically significant for the median swing justice. Although the focus on only civil liberties cases presents the most likely scenario for observing an effect of ideology, this is still an important result. Nevertheless, we still see that the expected influence of ideology is *weakest* for the median swing justice and increases monotonically as we move toward the most ideologically extreme justices. Furthermore, the estimated magnitude of the relationship between ideology and the probability of a liberal vote for the median swing justice is almost half the magnitude of the observed relationship for all justices and the estimates are statistically different. Even when using the Segal-Cover scores and analyzing just civil liberties cases, we observe a substantially diminished influence of ideology when we analyze the vote of the pivotal justice. These findings offer strong support for our predictions of both heterogeneity across justices and

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<sup>5</sup>When a justice sitting on the Supreme Court is elevated to the Chief Justice, the Cameron-Park measure incorporates information from the period as associate justice to estimate the ideological position for the period as Chief Justice.

<sup>6</sup>Following Segal & Cover (1989, 561), we define civil liberties cases as those involving criminal procedure, civil rights, the First Amendment, due process, and privacy. Because we analyze only civil liberties, we do not analyze smaller subsets of the data, such as 5-4 decisions.

reduced reliance on attitudinal considerations by the median swing justice.

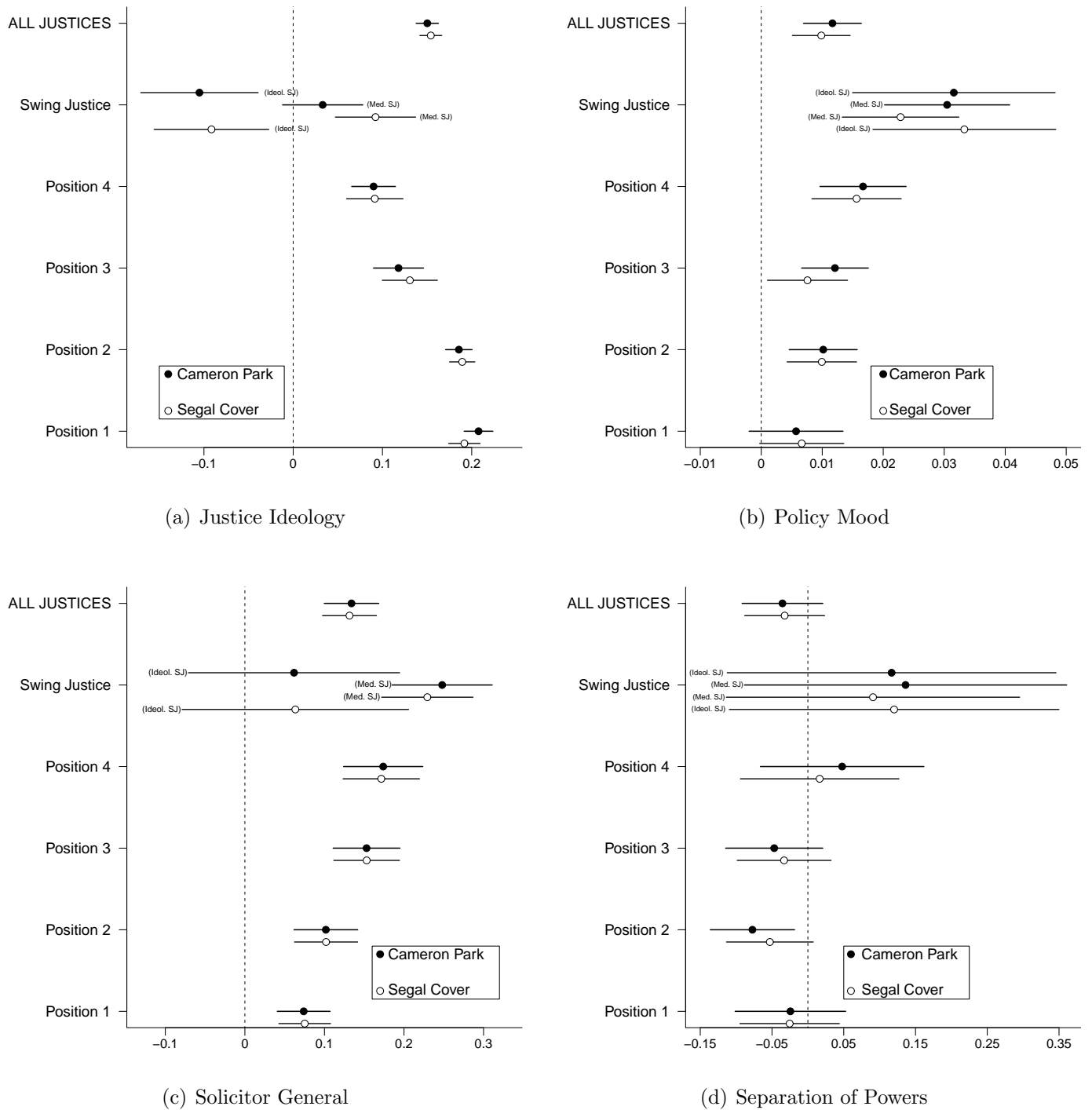


Figure A-4: The Average Marginal Effect (with 95% Confidence Intervals) for ALL JUSTICES and by Justice Position, Using Two Different Measures of Justice Ideology (Cameron Park Scores and Segal Cover Scores). The analysis includes all civil liberties cases that reversed the lower court's decision.

## 4.2 Martin-Quinn Scores

The Martin & Quinn (2002) Scores offer another prominent measure of justice ideology. These measures are advantageous because they estimate the justices' revealed preferences each term, producing dynamic measures of justices' latent ideological predispositions. In the current context, a limiting factor of these measures is that they are based on justices' votes. Thus, these measures of ideology are endogenous to our dependent variable. Martin & Quinn (2005) offer a potential solution to this problem that we employ here to further test that our results are robust to alternate measures of justice ideology. Martin & Quinn (2005) propose estimating Martin-Quinn scores for a subset of cases. They then use this estimate of justice ideology in an analysis of the remaining cases. Estimating justice ideology based on cases that are not included in the dependent variable alleviates the circularity problem that would occur with the standard Martin-Quinn scores.

The results reported below in Figure A-5 follow this intuition. We estimate four different logistic regressions for all justices, the median swing justice, and the ideological swing justice. Each regression uses a different measure of the Martin-Quinn scores. Specifically, we regress all cases *except* civil liberties cases on the civil liberties Martin-Quinn scores. Similarly, we regress all cases *except* Criminal Procedure on the Criminal Procedure Martin-Quinn scores. We do this for each of the four separate Martin-Quinn scores. This strategy allows us to include dynamic measures of justice ideology while ensuring these measures are not based on the votes we are analyzing. To save space, we only report the average marginal effect for justice ideology. These AMEs allow us to assess whether our conclusions regarding the heterogeneous influence ideology are robust to the use of the Martin Quinn scores as a measure of ideology.

When reading the figure, estimates for specific issues (e.g., MQ: Civil Rights) should be compared across justice categories. The results in Figure A-5 strongly support expectations. We observe distinct differences in the relationship between ideology and the probability of a liberal vote across all justices and both types of swing justice. These differences emerge regardless of the subset of cases we analyze and regardless of which measure of the Martin-Quinn scores we analyze.

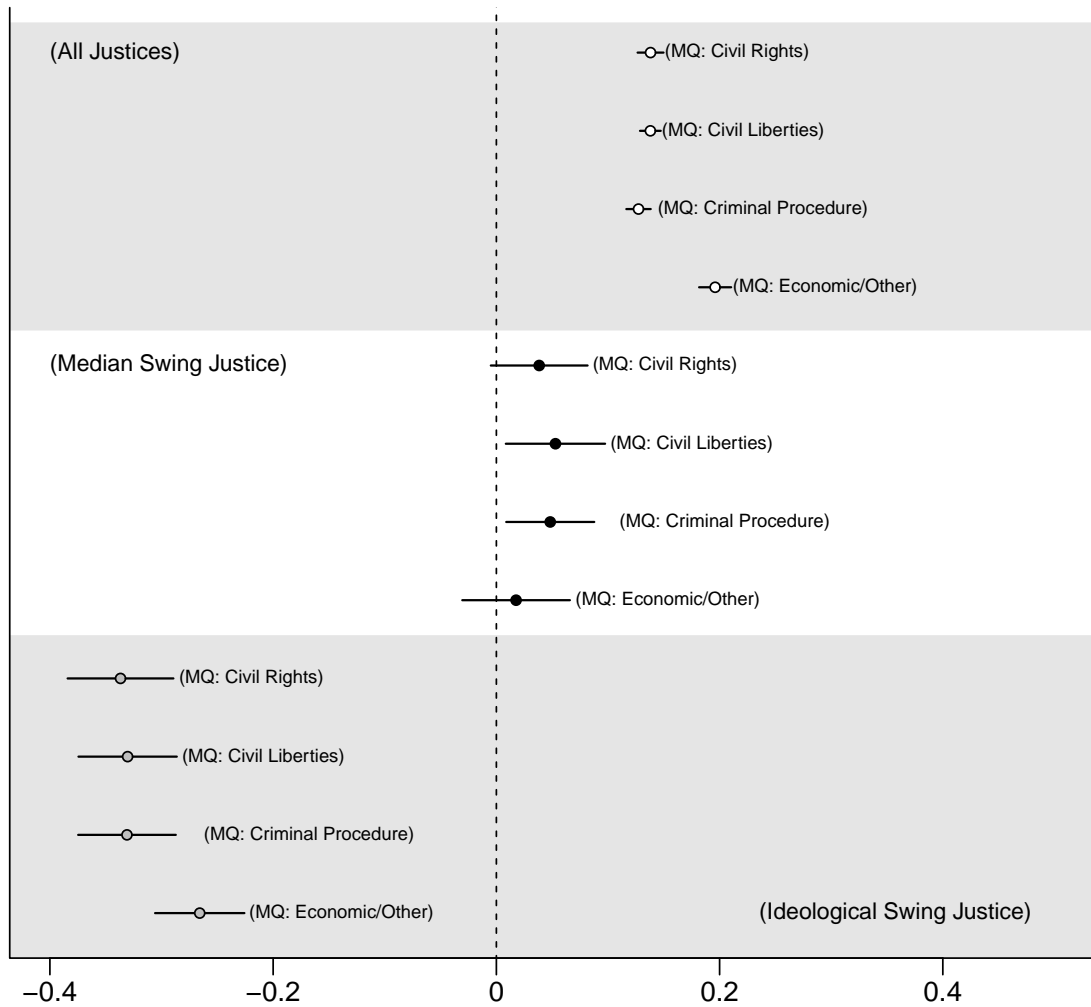


Figure A-5: The Average Marginal Effect (with 95% Confidence Intervals) for ALL JUSTICES, the Median Swing Justice, and the Ideological Swing Justice among all cases that reversed the lower court’s decision, using dynamic measures of Justice Ideology

*Note:* The top section corresponds with all justices, the middle section with the median swing justice, and the bottom section with the ideological swing justice. Estimates for specific issues (e.g., MQ: Civil Rights) should be compared across these terciles.

### 4.3 Cross-Sectional versus Over Time Variance

One goal of our analysis is to assess whether the relationship between justice ideology and the probability of casting a liberal vote differs across justices. We consistently observe a weak and oftentimes statistically insignificant relationship for the median swing justice and a substantively large, negative, and significant relationship for the most ideologically extreme justices. Not surprisingly, for much of the Court's history, there is more cross-sectional variance in the ideological positions (i.e., Cameron-Park scores) of the most extreme justices than the median swing justice. This is true even though we do observe meaningful cross-sectional variance for the median justice position (because we identify the median based on 8 issue categories, we could theoretically observe 8 different median justices each term). In order to guard against the possibility that greater variance in ideology scores for the most extreme justices influenced our findings, we standardized the variance for ideology for each regression analysis reported in the text.

Here, we perform an additional analysis to further ensure that different cross-sectional variances are not responsible for our results. During the period of our analysis, the Cameron-Park scores range from -0.582 (Scalia) to 0.603 (Marshall) (we recoded the scores so higher values indicate more liberal justices). However, in the early 1960s, the Cameron-Park scores suggest a much more moderate Court, with the most conservative member receiving a C-P score of -.113 (Stewart) and the most liberal member receiving a C-P score of .482 (Black). The more moderate Court during this period affects the cross-sectional variance of ideology in two ways. First, the maximum variance of ideological voting was diminished because the range of ideology values was smaller. Second, the relatively moderate Court resulted in increased variation in the identity of the median swing justice because different issue categories often corresponded with a different median justice. The result is that from the 1956 to 1970 terms, the cross-sectional (i.e., within term) variance is roughly equivalent for the median swing justice and the most ideologically extreme position.<sup>7</sup> The roughly equivalent variances during this period offers a unique opportunity to evaluate whether the previous results are a product of the greater cross-sectional variance of more ideologically extreme justices. If so, when we analyze this subset of terms where the cross-sectional variance is roughly equivalent for the median swing justice and most polarized justices, the differences should disappear.

As we see in Table A-3, this is decidedly not the case. Despite the fact that the within term variance was nearly equivalent for the median swing justice position and the most ideologically extreme justice position, the relationship between ideology and the probability of a liberal vote is significantly greater for the most ideologically extreme justices than the median swing justice. In fact, the average marginal effect for the two justices who are typically most distant from the median swing justice is 22 times the size of the median swing justice. This result offers powerful evidence that the findings of heterogeneity across justices presented in the text is *not* a result of different variances in ideological predisposition across justice position. The results for the other predictors also confirm expectations.

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<sup>7</sup>Specifically, the average term-variance (weighted by the number of observations each term) from 1956 through 1970 is 0.019 for the median swing justice and 0.015 for the justices in position 1 (i.e., the most ideologically distant justices)

Table A-3: Comparison of Median Swing Justice and Most Ideologically Distant Justices when the cross-sectional variation of ideology is equivalent

	Coefficient		AME	
	Median S.J.	Ideologue Pos. 1	Median S.J.	Ideologue Pos. 1
Justice Ideology	0.35 (0.68)	5.63* (1.24)	0.05 (0.10)	1.10* (0.21)
Solicitor General	2.51* (0.52)	0.82* (0.24)	0.36* (0.08)	0.16* (0.05)
Public Mood	0.144* (0.071)	-0.07 (0.04)	0.021 (0.011)	-0.01 (0.01)
SOP Constraint	0.62 (0.48)	-0.08 (0.44)	0.09 (0.07)	-0.02 (0.09)
Judicial Power	-1.13 (0.61)	-1.13* (0.50)	-0.20 (0.13)	-0.24* (0.11)
Economics	0.27 (0.65)	-0.69 (0.50)	0.04 (0.09)	-0.14 (0.10)
Civil Liberties	0.81 (0.58)	0.05 (0.43)	0.12 (0.09)	0.01 (0.08)
Constant	-8.43 (4.57)	3.91 (2.47)		
Log Pseudolikelihood	-316.58	-1021.80		
McFadden's Pseudo R <sup>2</sup>	0.14	0.11		
N	703	1,773		

The dependent variable reflects decisions by individual justices to support a liberal outcome among cases that reversed the lower court's decision from 1956 to 1970. Two-Tailed Significance Levels: \*  $p < .05$ . Standard errors (clustered by term) in parentheses.

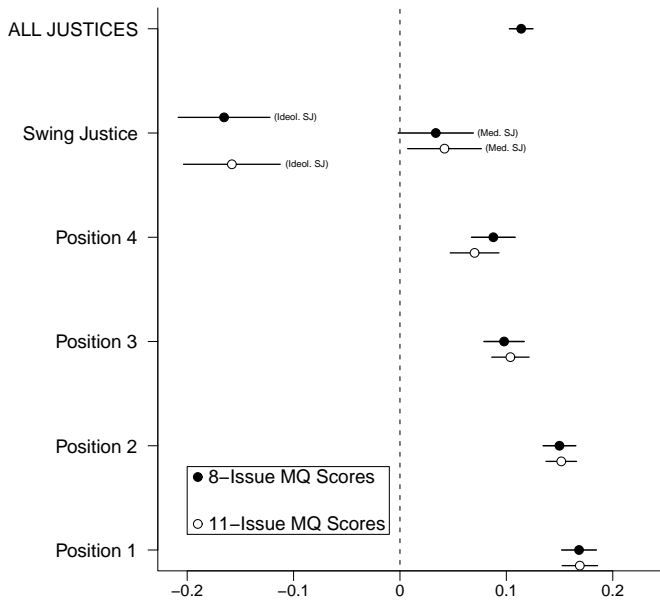
## 5 Appendix 5: Issue Categories Used to Estimate Martin-Quinn Scores

The measurement strategy we use to identify the swing justice allows justices' ideological preferences to vary across issue category. We allow for preferences to vary across type of case by estimating issue-specific Martin-Quinn scores (Martin & Quinn 2002, Martin & Quinn 2005). The results in the text allowed preferences to vary across eight separate issue areas on the Court's docket: Criminal Procedure, Civil Rights, First Amendment, Other (Non-First Amendment) Civil Liberties, Unions, Economic Activity, Judicial Power, and Federalism/Other.<sup>8</sup> The decision to use eight separate categories was based on two considerations. First, we want to allow preferences to vary across meaningfully distinct categories. Second, we wanted to maintain enough cases within each category so we had enough information to generate estimates without substantial measurement error. Although analyses of Supreme Court decision making typically consider eight or fewer categories (e.g., Cameron & Park 2009, Epstein & Martin 2011, McGuire & Stimson 2004), we want to be sure that our results are not sensitive to this decision. Below, Figures A-6 and A-7 report the results when we identify the swing justice based on 11-category, issue-specific Martin-Quinn scores. Because the finer categories have fewer cases, there is some concern that the flexibility of these estimates comes at a cost of information. However, the results are very similar to, and reinforce, our previous findings. Although not reported here, we have also analyzed the data based on 4-category and 6-category Martin-Quinn scores, and again, the results reinforce all conclusions.

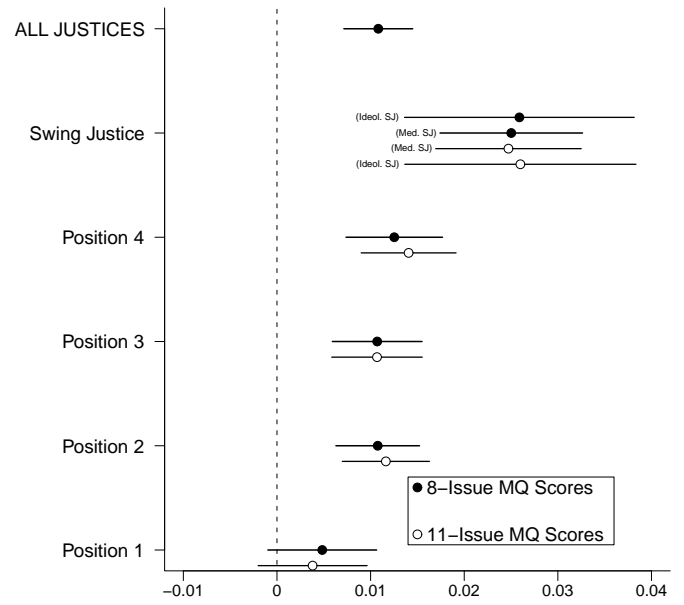
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<sup>8</sup>Using the Supreme Court Database (<http://supremecourtdatabase.org/>), we rely on the "issueArea" variable to split the data into the eight categories (Criminal Procedure: "issueArea"=1; Civil Rights= 2; First Amendment=3; Other Civil Liberties=4-6; Unions=7; Economic Activity=8; Judicial Power=9; Federalism/Other=10-14). These issues represent substantively distinct issue areas and include sufficient cases to generate meaningful and reliable estimates of the justices' issue-specific preferences.

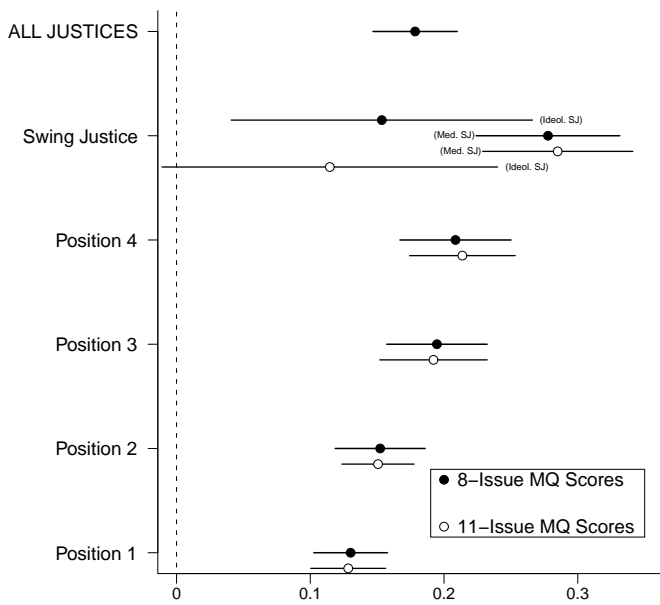




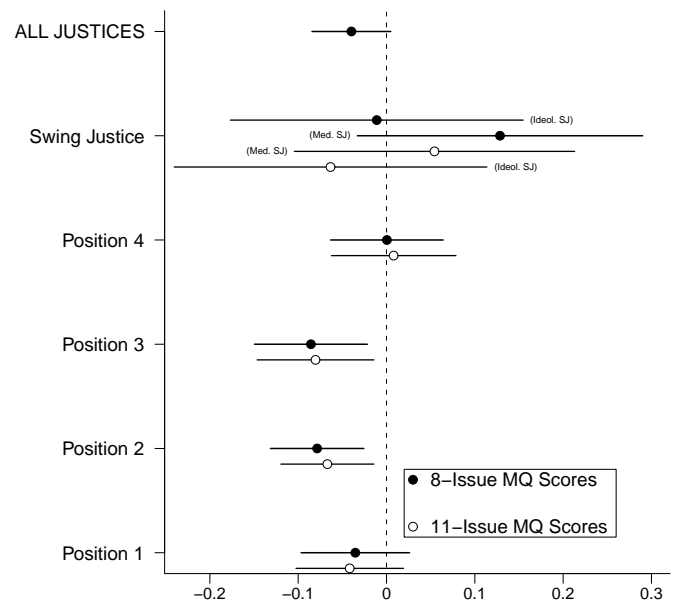
(a) Justice Ideology (All)



(b) Policy Mood (All)

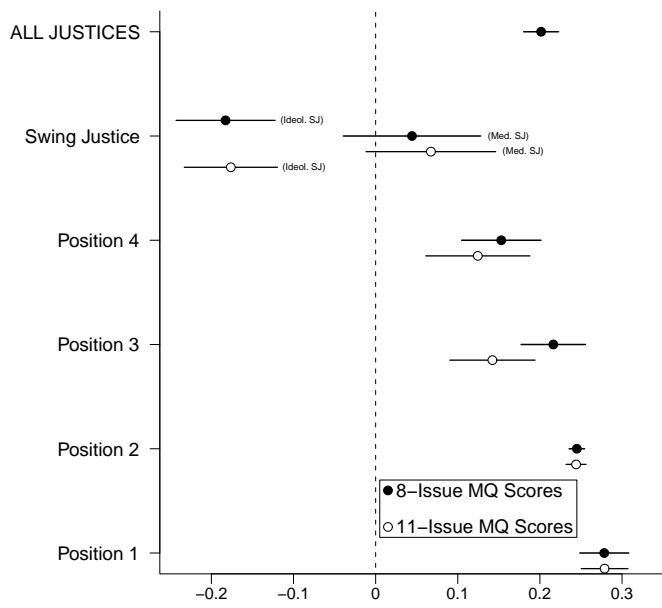


(c) Solicitor General (All)

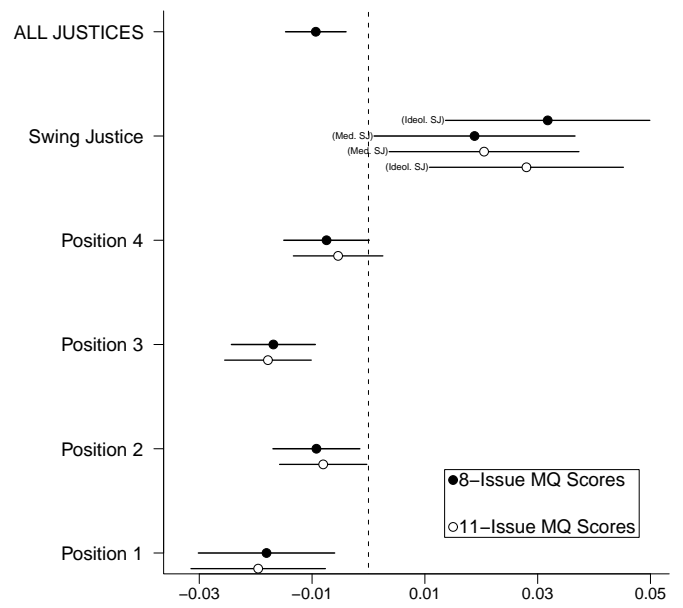


(d) Separation of Powers (All)

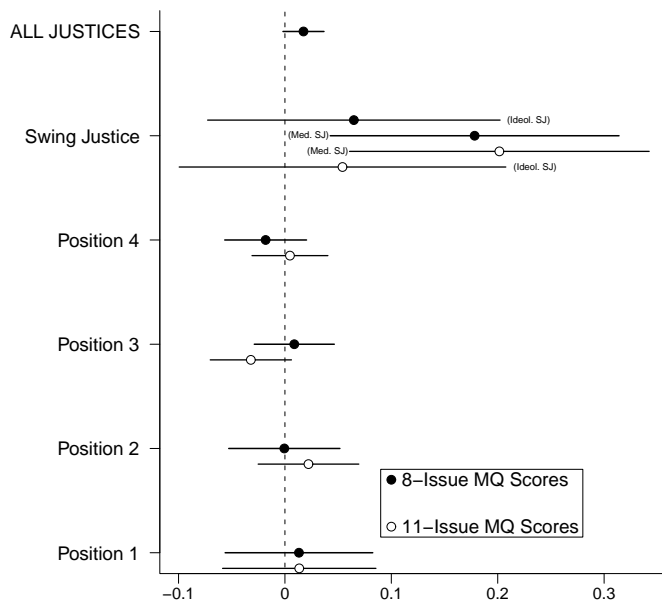
Figure A-6: The Average Marginal Effect (with 95% Confidence Intervals) when the swing justice is identified with 8-issue MQ scores (solid dots) and 11-issue MQ scores (hollow dots).



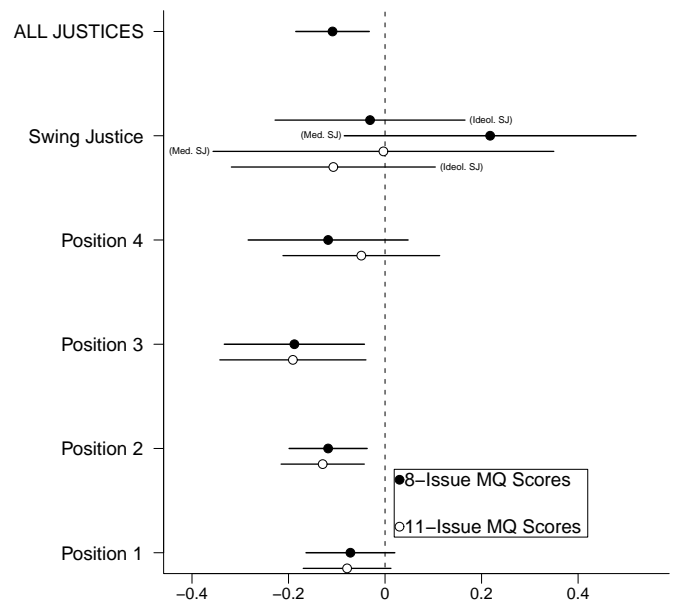
(a) Justice Ideology (5-4 Decisions)



(b) Policy Mood (5-4 Decisions)



(c) Solicitor General (5-4 Decisions)



(d) Separation of Powers (5-4 Decisions)

Figure A-7: The Average Marginal Effect (with 95% Confidence Intervals) when the swing justice is identified with 8-issue MQ scores (solid dots) and 11-issue MQ scores (hollow dots). (Among 5-4 Decisions)

## 6 Appendix 6: Majority Opinion Coalition Formation

As indicated in the text, the length of time it takes members of the majority coalition to join the majority opinion holds two implications for our argument. First, we might expect that, on average, the swing justice takes longer to join the majority opinion than other justices. We do not propose that the swing justice *always* joins the majority opinion later than other justices. An ideologically extreme justice could threaten a concurrence and hold out from joining the majority with the hope of pulling the majority opinion in a more ideologically extreme direction. However, given our argument that the swing justice relies on more considerations than other justices, we think it is plausible that this justice takes longer to join the majority opinion than other justices. Justices relying more on ideology, after all, have a clear guide as to the direction of their vote. The longer time could also reflect the pivotal nature of this justice. That is, the swing justice may delay signing onto the majority opinion because this justice is still contemplating voting with the other side. Second, we believe the ideological swing justice, in particular, is likely to take more time. This prediction results from our suggested mechanism of ambivalence. We propose that instead of clarifying the ideological swing justice's decision, case-specific factors that compete with this justice's ideological predisposition may lead to ambivalence. Although we cannot observe ambivalence, one implication of facing competing considerations would be taking longer to make a decision to join the majority opinion.

To evaluate these predictions, we followed Maltzman, Spriggs & Wahlbeck (2000, 137-138) and utilized the *Burger Court Opinion-Writing Database* (Wahlbeck, Spriggs & Maltzman 2009) to determine the number of days between the first-draft circulation of the majority opinion and when each justice joined the opinion. We contemplated generating a measure of the order that justices join that majority opinion, but this proved problematic for two reasons. First, it is common for multiple justices to join the opinion on the same day, rendering the order uninformative. Second, ordering the justices ignores the amount of time between when justices join the opinion. For example, if one day separates the fourth and fifth justice to join the majority, the ordering might be inconsequential (i.e., idiosyncratic factors might explain which justice joined first). However, if 20 days separate these justices, this delay is more likely to reflect an active decision to withhold support from the opinion. Thus, like Maltzman, Spriggs & Wahlbeck (2000), we believe the number of days is the preferred indicator.

Our analysis controls for both case and justice fixed effects. Case fixed effects ensure that we are analyzing *within* case variance in the number of days between the first-draft opinion circulation and when each justice in the majority joined the opinion. It is crucial that we do not compare across cases. In one case, the first justice to join the majority opinion might do so after five days. Yet, in a different case, the fifth (and pivotal) justice might join the majority after five days. Clearly, the span of five days represents substantively different information about joining the majority in these hypothetical examples. Using fixed effects to analyze *within* case variance should address this concern. Our use of justice fixed effects controls for the possibility that some justices systematically take more or less time to join the majority.<sup>9</sup> Finally, we only analyze 5

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<sup>9</sup>Consider, for example, Chief Justice Burger, who typically joined the majority opinion later than other justices. Because the Chief Justice assigns the majority opinion writer (when he or she is in the majority), we have little reason to suspect that the Chief Justice's delay signaled a threat to switch to

to 4 decisions. This decision reflects our concern that when the majority is larger than five justices, assumptions about the reasons for joining the majority early or late become more complicated. For example, after five justices join the majority coalition, and the direction of the decision is determined, other justices may consider joining the opinion to try to influence the legal rule. Of course, we never know with certainty why justices join the majority when they do, but we believe the assumption that the timing of joining the majority corresponds with likelihood of being pivotal is most likely to hold for 5 to 4 decisions.

Since we are analyzing count data (i.e., the number of days between the first opinion circulation and when each justice in the majority signed on to the opinion), we estimate a negative binomial regression model. Recall, our goal is to determine whether the median swing justice and the ideological swing justice take longer, on average, to join the the majority opinion than other justices. The results decidedly support this conclusion. Using fixed effects to examine *within* case differences (in terms of the time it takes to join) and controlling for average differences across the justices, we see that both the median and ideological swing justices are more likely to take additional time to join the majority. The results, reported in Table A-4, correspond with an expected additional 2.7 days for the median swing justice and an expected additional 4.4 days for the ideological swing justice. These values are substantively important. The average number of days between the fourth and fifth justices to join the majority in 5 to 4 decisions is approximately 10 days and the median difference is just four days.

In addition to supporting our measurement strategy, the results for the ideological swing justice are consistent with our discussion of the role of ambivalence in this justice's decision making. If the ideological swing justice faces competing considerations between his or her ideology and case-level considerations, we might expect the resulting ambivalence to lead this justice to take longer to make up his or her mind and join the majority. This is precisely what we observe.

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the minority. Our use of justice fixed effects controls for this type of systematic difference across justices.

Table A-4: The Expected Influence of Being the Swing Justice on the Number of Days to Joining the Majority Opinion

Median Swing Justice	0.25*
	(0.10)
Ideological Swing Justice	0.39*
	(0.09)
Constant	5.00*
	(0.71)
Pseudo R <sup>2</sup>	0.15
N	1,370

*Note:* Negative binomial regression coefficients with standard errors in parentheses. \* p<0.05, two-tailed test. Fixed effects for case and justice not shown.

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