SUPPLEMENTAL WEB APPENDIX

SUPPLEMENTAL APPENDIX A – OPINION AND JUSTICE VOTE MEASUREMENT

This analysis's movement toward a new unit of analysis consequently introduces questions about which measurement strategy is the best possible to evaluate temporal variance in consensusual decision-making norms. This question regarding the relative strength of justice-level rates versus opinion-based rates of dissent and concurrence must be understood from three different perspectives: 1) the theoretical differences in measurement strategy; 2) the reliability of the new measures versus existing measures; and, 3) the aspects of content validity found within different estimation results. This supplemental appendix considers each of these criteria and shows a justice-level unit of analysis to be the preferred approach.

First, from a theoretical standpoint, opinion-based data on dissensus are unnecessarily blunt measures when the justice-level data are available. Opinion-based measurement simply identifies whether a dissenting or concurring opinion is associated with the majority opinion, but it does not address the magnitude of coalitional support for separate opinions and, therefore, comparatively is weaker in terms of generating estimable variance. A hypothetical Court that decided all cases by a 8-1, 7-2, 6-3, or 5-4 split would each result in a singular 100% rate of dissent or concurrence for the opinion level. Whereas the justice-level rates respectively would be 11%; 29%; 33%; or 44%. By moving to a unit of analysis that captures coalitional support, we now have a more finely tailored measure that engenders greater variance within the dependent variables of interest.

Justice-level data also is a much more flexible in terms of constructing alternative dependent variables to evaluate dissensus. The extant literature on the Court's norm of consensus emphasizes that the chief justice plays an instrumental role in leading the associate justices (*i.e.*, the chief justice may marshal the rest of the Court). Opinion-based measures cannot speak to differences in decision-making between associate justices and the chief justice as they consider dissents or concurrences as originating from an anonymous justice of either level. Data on individual justice votes, on the other hand, allow us to aggregate dissent and concurrence rates for the Court as a whole, the collective of associate justices, and a separate series for the chief justice. Estimation results show that the best performing specification always is related to the group of associate justices, meaning that there are institutional differences between the associate justice and chief justice decision-making calculus. In this sense, the ability to segregate the associate justices from the chief justice offers an improvement over simply looking at aggregate behavior of the Court as a whole.

In addition to the theoretical advantages of a justice-level unit of analysis, the calculation of these new justice-level series point to problems of reliability within the existing opinion-based measures. The existing opinion measures are constructed by aggregating two unique sets of data (Blaustein and Mersky 1978; Spaeth 2009) and because other norms of the Court, such as the identification of oral argument dates and the act of signing opinions, have changed over time the aggregation of these different data sources lead to inconsistent measures of dissent and concurrence across time. To exhibit these inconsistencies, we present the existing opinion-based and new justice-level rates of dissent and concurrence for the Court in Supplemental Figure 1. This figure also presents a revised opinion-based measure created with our individual justice vote data.

The issue of reliability present here is that the existing opinion-based and justice level rates are comparable in the period beginning with the Stone Court, but the preceding period from 1900 to 1940 shows a significant divergence. Due to the different units of analysis, an opinion-based measure should always be greater than the corresponding justice-level rate. This is the case beginning with the Stone Court, but the data from the Fuller, White, Taft and Hughes Courts show that existing opinion measures actually are less than the justice-level rate.

The source of the diminished measurement of dissent and concurrence is related to the types of cases found within annual samples of decisions. The early part of the series tends to capture numerous cases that did not receive an oral argument before the Court; the majority of which would be considered *per curiam* decisions within the modern era. Epstein et al. (2007, 231) suggest that there may be problems associated with the comparison of these two different data sources across time and the evidence here suggests that is the case.

Alternatively, the revised opinion-based measures calculated for orally argued cases presents the expected relationships. From a theoretical measurement perspective, the opinion-based measures should be more blunt and inflated with respect to justice rate measures. The plots of the revised opinion measures and justice rates for dissent and concurrence show this to be the case.

The substantive implications of an unreliable measurement strategy are clear in this instance. Existing accounts (Walker, Epstein, and Dixon 1988) emphasize the lacking leadership traits of Chief Justice Stone within the demise of consensual norms upon the Court versus a broad array of alternative explanations. But the analysis is built upon measures that not only are unreliable over time and but that also show a distinct change in reliability during the transition from the Hughes Court to the Stone Court. While our new results find that Chief Justice Stone's leadership was a factor in changing the norms of the Court, we also find clear support for alternative hypotheses, such as agenda transformation and the demographic and ideological composition of the Court.

With the calculation of newly revised opinion-based measures of dissent and concurrence, we are further able to evaluate their potential to explain changes in dissensus versus corresponding justice level rates. Although the lack of data on issue types prevented the evaluation of agenda composition hypotheses with existing opinion measures, we are now able to make a direct comparison between the two measurement strategies. These results are presented in Supplemental Tables 1 and 2, where the justice-level rates yield a broader and more nuanced set of results. On whole, a reliable opinion-based measure performs reasonably well, but the justice-level rates are better able to parse and distribute the additional variance that the alternative measurement strategy generates.

In terms of levels of dissent upon the Court, these differences can be substantively important to our knowledge of dissensual norms. If we only look at the effects of the Judiciary Act of 1925, or changes in agenda composition, upon the level of dissent, then an opinion measure would indicate that these effects are limited to criminal, economic and institutional power issue types. Justice-level rates, however, find that the most robust parameter relationship actually is related to the Court's attention to the collection of civil liberties cases that it incrementally moved toward after gaining a largely discretionary docket. Similarly, the opinion level measure does not tap into effects of varying levels of justice experience upon the Court. Instead, the more blunt opinion-based measure is more susceptible to allocate variance to variables such as a chief justice freshman effect, which is found to be less robust with the justice-level rate.

In terms of levels of levels of consensus, the differences are of a more minor scale due mostly to the idiosyncratic nature of this form of separate opinion. The primary difference between these models is that an opinion measure would find a stronger effect related to growth in the Court's docket and demand for litigation. For the associate justice model, this relationship exhibits moderate strength, but remains outside traditional significance levels. One final area, where the greater variance of justice rates does appear, however, is in evaluating the different effects of chief justice leadership. With opinion measures, we would show an substantial increase under Chief Justice Stone and incremental growth thereafter. By moving to the justice-level unit of analysis, we are better able to discern leadership aspects related to Chief Justice Burger and empirically verify that he, like Stone, was unique in his leadership of the Court.

Therefore, on the criteria of theoretical measurement design, reliability of existing measures, and aspects of content validity, the justice level unit of analysis offers distinct advantages over opinion-based measures of dissent and concurrence.

SUPPLEMENTAL APPENDIX B – FRACTIONAL DIFFERENCING

Long-memoried series that can be made into stationary, or short-term, series by taking the first differences of the series are said to be integrated of order one, denoted *I(1)*. They are, therefore, described as having a unit root in that the coefficient estimate for rho (ρ), describing the relationship between the estimated error term and its value lagged one period, is one: $\mathrm{Y}_t = \beta \mathrm{X}_t + \hat{\varepsilon}_t$ where $\hat{\varepsilon}_t = \rho \hat{\varepsilon}_{t-1} + \upsilon_t$. Thus, as the equation indicates, any disturbances or "shocks" to the series (represented by upsilon, υ) across time are accumulated rather than being dissipated gradually, serving to make the value of the variable progressively more volatile through time (*e.g.*, Dickey and Fuller 1979, 1981; Engle and Granger 1987). Taking the first difference of a series entails subtracting a series' value lagged one period from the value for the series' current period (*i.e.*, Y_t-Y_{t-1}). Differencing, while common, may not always be a benign transformation of the data. In particular, overdifferencing the data may upset the inferential process that is at the heart of empirical research as it may build patterns into the data that are not within the untransformed data-generating process. Issues of the non-integer, or fractional, integration of the data are also of concern in this context (Box-Steffensmeier and Smith 1998; De Boef and Granato 1997; Hurwitz and Lanier 2004; Lebo, Walker and Clarke, 2000).

When working with time serial data, as is done here, the analyst must first insure that the series in question are stationary; otherwise spurious regressions may result (*e.g.,* Engle and Granger 1987; Granger and Newbold 1974). Traditionally, analysts have dealt with this problem by first differencing the data (Bowerman, O'Connell, and Koehler 2005). More generally, a series is *integrated* to the order of *d* (I(*d*)), where *d* is the number of differences needed to make the series stationary. This general form implies that the value of the differencing parameter need not be constrained to integers (*e.g.*, 0 *or* 1). That is, a series' *d* value may take on noninteger values (*e.g.*, $0 \leq d \leq 1$), relaxing the traditional assumption that time series variables are completely stationary or wholly integrated. If so, then the series is *fractionally integrated* (FI) (e.g., Box-Steffensmeier and Smith 1998; Clarke and Lebo 2003; Dickinson and Lebo 2007; Granger 1980; Hosking 1981; Hurwitz and Lanier 2004; Lanier 2003; Lebo and Box-Steffensmeier 2008; Lebo, McGlynn and Koger 2007). Granger (1980) asserts that when heterogeneous individual-level data are aggregated, a FI series may result since the aggregate series is being produced by divergent autoregressive components that describe the individuals' behavior that is being combined. Under Granger's Aggregation theorem (Granger 1980), the dependent variable series Y, composed of the justices j = 1 … n, each exhibiting a unique autoregressive structure yields $Y\ j,t = \alpha_j\ Y_{j,t-1} + \varepsilon_t, where\ \alpha_j \sim \beta\ (0,1)$ (*see also* Caldeira and Zorn 1998; Hurwitz and Lanier 2004; Lanier 2003; Lebo, McGlynn and Koger 2007, 472).

These characteristics have profound implications for researchers who seek to accurately model their underlying data-generating process. First, by fractionally differencing variables, researchers are able to more accurately capture the data-generating process underlying their data (*e.g.*, Box-Steffensmeier and Smith 1998; Clarke and Lebo 2003; Hurwitz and Lanier 2004; Lanier 2003; Lebo Walker and Clarke 2000). Imposing the restriction that their data exhibit the perfect memory of a unit root (d = 1), or no memory entirely (d = 0), implies a profound and perhaps erroneous implication about the nature of the data, which can be avoided if an analyst is open to the potential for the fractional integration of the data (*see* Lebo and Box-Steffensmeier 2008; Lebo Walker and Clarke 2000). Second, if researchers were to take the first difference of a data series when in fact the series is FI, that transformation may serve to create patterns in the data that are not naturally present. By measuring the degree of a series' fractional integration, researchers can avoid the "knife-edge" decision that they otherwise would have had to make as whether their data are either exclusively stationary (I(*0*)) or described by a unit root (I(*1*)) and, thus, have greater confidence in the analytical results obtained with a fractionally-differenced variable (Box-Steffensmeier and Tomlinson 2000; DeBoef and Granato 1997, 619; Hurwitz and Lanier 2004).

An understanding of the origin of fractional dynamics of one's data may assist researchers in identifying the underlying data-generating process of the series at hand. Such series may be fractionally integrated (FI) and can be made stationary by fractional differencing. Our series here are likely FI as the justices' behavior (*j*) will likely vary in terms of its autoregressiveness (reflected in α) across varying individuals, issue dimensions, majority coalitions and time (*t*), among other differences. Hence, we will proceed in this analysis estimating the order of fractional integration of our data, filtering them according to their specific level of fractional integration, estimated by Robinson's *d*.

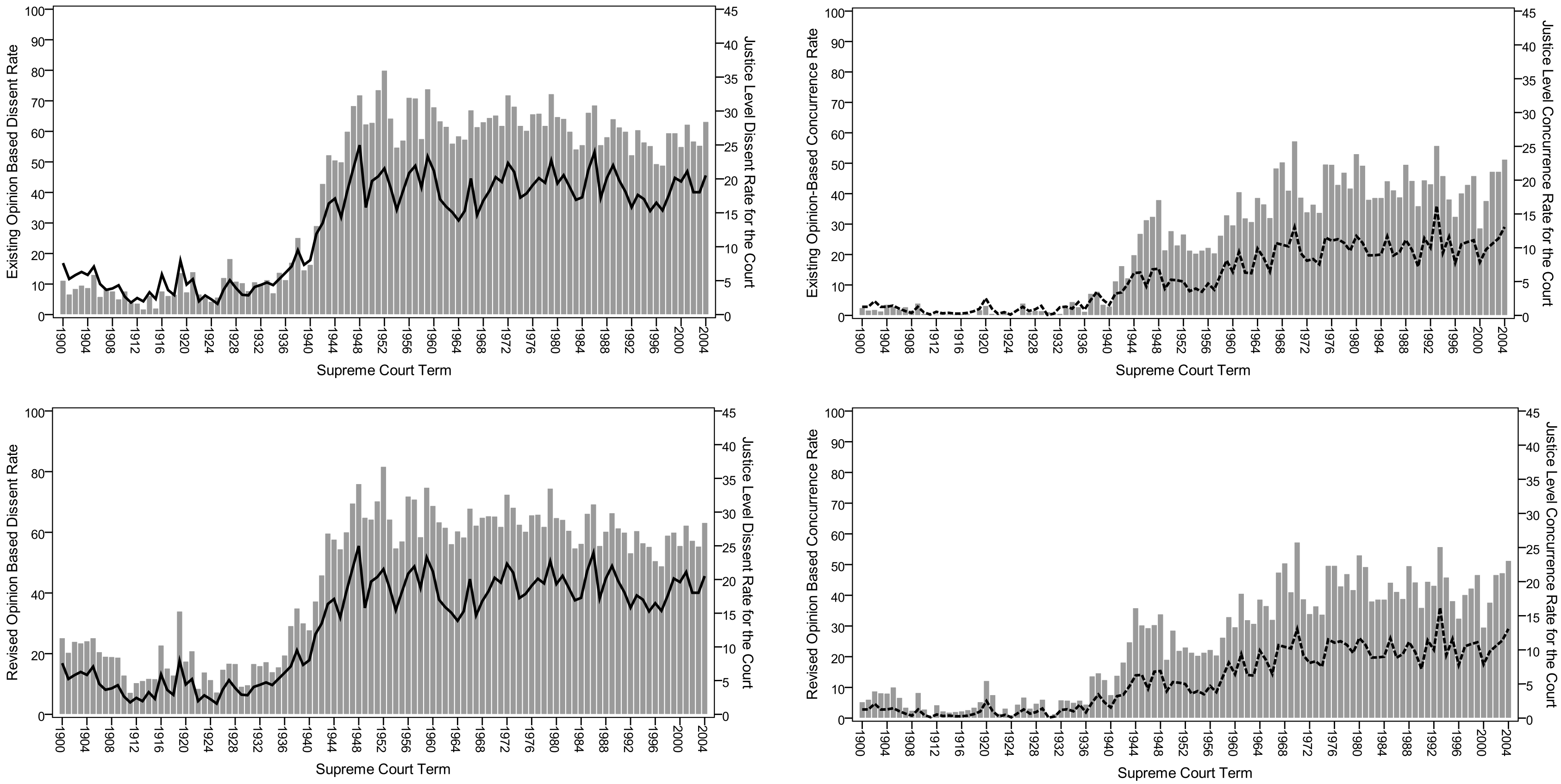
We first test each of our series to determine if they are stationary, that is characterized by a unit root. The Kwiatkowski, Phillips, Schmidt, and Shinn (1992) ("KPSS") Test is the most appropriate measure of stationarity in the current context as it is more robust to FI series than are other tests of stationarity.[1] The KPSS test statistic's null hypothesis is that the series is stationary. Following KPSS, we use a lag of four to determine stationarity. Supplemental Table 1 reports these results. The series nearly all reject the null hypothesis of stationarity at conventional levels of statistical significance. Taken together, these findings allow one to provisionally conclude that not only are the series in question nonstationary, but they are most likely FI.

[1] These include the Dickey-Fuller (1979) or Augmented Dickey-Fuller tests. We also conducted, but do not report here, stationarity tests with a constant and a trend term. The results are consistent with those reported in Supplemental Table 1.

Accordingly, we next test each of the present variables to determine their respective integrative order. To do so, we employ Robinson's (1995) procedure.[2] Supplemental Table 2 presents the results of these tests. As one can see, nearly all of the variables are FI, with their *d* estimates midway between zero and one, but they do vary significantly among each other. As such, if one were to have wholly differenced nearly all of these series, one most likely would have obtained spurious results. More specifically, many of the series have *d* estimates near 0.5; thus, using the much more precise fractional differencing is critical in order to obtain valid results. These findings imply that there may be substantively different data-generating processes undergirding the chief justices' separate voting and writing behaviors. With these more precise estimates, the variables were then individually made stationary (I(*0*)) based on their *d* estimates.

[2] When implementing this procedure, one must difference the data first to avoid the estimation of the "troublesome intercept parameter" (Baillie 1996, 39). Hence, the test statistic arises from the estimates of (0, 1+*d*, 0) on first-differenced data due to the constrained parameter space (*i.e.*, $-1.5 \leq d \leq 0.5$) and to ensure that the data are stationary (*see e.g.*, Lebo, McGlynn and Koger 2007; Lebo, Walker and Clarke 2000; Box-Steffensmeier and Smith 1998). Thus, to determine the order of integration of the level-form data, one must add 1.0 to the estimate of *d* obtained using this procedure. The code necessary to implement Robinson's (1995) procedure can downloaded from the RATS website (*http://www.estima.com*).

Supplemental Figure 1: Temporal Biases Within Existing Opinion-Based Measures



Note: The existing opinion-based measures are derived from the *Supreme Court Compendium* (Epstein *et al.* 2007) and represent the proportion of cases with at least one dissenting, or concurring, opinion in each term. The revised opinion-based measures are the proportion of cases with at least one dissenting, or concurring, opinion for orally argued cases. The justice-level measures are the number of dissenting votes, or concurring votes, in orally argued cases expressed as a proportion of all votes cast in each term.

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Supplemental Table 1: Box-Jenkins Estimates of Dissent Rate Alternatives
Orally Argued Cases 1899–2004

	Opinion-level Estimates			Court-level Estimates			Associate Justice Estimates		
	β	(s.e.)	ρ	β	(s.e.)	ρ	β	(s.e.)	ρ
Criminal Cases	.12	.063	.06	.06	.036	.10	.07	.030	.03
Civil Liberties Cases	.05	.044	.23	.05	.015	.001	.07	.018	.000
Economic Cases	-.18	.079	.02	-.04	.021	.05	-.05	.021	.02
Institutional Power Cases	-.10	.047	.03	-.04	.022	.10	-.05	.026	.09
Original & Misc. Cases	-.01	.246	.97	.12	.134	.38	.15	.123	.22
Cases on Docket[a]	1.09	1.423	.45	1.07	.892	.24	1.18	.832	.16
AJ Ideology $_{(std\ dev)}$[b]	.34	.041	.000	.11	.018	.000	.12	.015	.000
AJ Experience $_{(mean)}$	.06	.386	.88	.16	.138	.24	.20	.109	.07
CJ Freshman Effect	-8.87	3.981	.03	-2.11	1.211	.09	-1.79	1.184	.14
CJ White	17.18	5.272	.002	5.16	1.433	.001	5.48	1.544	.001
CJ Taft	24.84	8.042	.003	6.36	2.348	.01	6.36	2.420	.01
CJ Hughes	35.86	12.03	.004	8.48	3.054	.01	8.23	3.040	.01
CJ Stone	59.44	14.45	.000	16.64	4.470	.000	16.69	4.244	.000
CJ Vinson	72.46	15.79	.000	21.74	5.026	.000	22.61	4.628	.000
CJ Warren	68.04	19.01	.000	21.80	6.622	.001	22.72	6.042	.000
CJ Burger	75.51	22.67	.001	24.75	7.553	.002	23.53	6.947	.001
CJ Rehnquist	87.74	26.16	.001	29.60	8.898	.001	27.32	8.217	.001
Constant Value	-.63	.487	.20	-.31	.282	.28	-.38	.282	.18
MA(1)	-.36	.111	.002	-.35	.126	.01	-.40	.117	.001
N observations		104			104			104	
R^2 value		.32			.30			.35	
Durbin Watson Statistic		1.95			1.96			1.97	
Breusch-Godfrey Test		.51	.89		.39	.96		.32	.98
White Heteroskedasticity		.59	.91		.68	.83		.61	.89

Note: Estimations conducted after fractionally differencing of the series. Newey-West HAC consistent/pre-whitened standard errors (AIC; 4 lags) are presented along with two-tailed probability tests. [a] Series divided by 1000 for parameter scale. [b] Series multiplied by 100 for parameter scale. Breusch-Godfrey serial correlation test with 11 lags (H_0: residuals do not exhibit serial correlation). White Heteroskedasticity test (H_0: residuals are homoskedastic).

Supplemental Table 2: Box-Jenkins Estimates of Concurrence Rate Alternatives Orally Argued Cases 1899–2004

	Opinion-level Estimates			Court-level Estimates			Associate Justice Estimates		
	β	(s.e.)	ρ	β	(s.e.)	ρ	β	(s.e.)	ρ
Criminal Cases	.01	.105	.94	.01	.028	.84	-.00	.031	.97
Civil Liberties Cases	.08	.106	.46	.00	.032	.96	-.01	.026	.79
Economic Cases	-.04	.038	.35	-.01	.011	.21	-.02	.011	.11
Institutional Power Cases	-.07	.039	.06	-.03	.021	.11	-.04	.018	.02
Original & Misc. Cases	-.30	.330	.36	-.18	.143	.22	-.22	.171	.19
Cases on Docket[a]	5.36	2.436	.03	.79	1.91	.68	1.07	.948	.26
AJ Ideology (std dev)[b]	.05	.056	.40	-.01	.026	.61	-.00	.020	.90
AJ Experience (mean)	.57	.214	.01	.14	.054	.01	.16	.075	.03
CJ Freshman Effect	-9.66	3.693	.01	-2.45	.501	.000	-2.53	.625	.000
CJ White	8.57	4.370	.05	1.42	.913	.12	1.85	1.020	.07
CJ Taft	15.38	7.072	.03	2.93	1.361	.03	3.54	1.431	.02
CJ Hughes	18.03	9.780	.07	3.21	1.825	.08	3.71	1.924	.06
CJ Stone	32.41	13.11	.02	6.23	2.199	.01	6.69	2.440	.01
CJ Vinson	41.37	15.17	.01	7.65	2.677	.01	8.75	2.889	.003
CJ Warren	45.77	19.20	.02	7.19	3.316	.03	7.93	3.818	.04
CJ Burger	49.78	23.19	.04	11.22	3.908	.01	11.95	4.566	.01
CJ Rehnquist	59.14	26.61	.03	13.14	4.630	.01	15.22	5.382	.01
Constant Value	2.29	.731	.002	.85	.252	.001	.97	.316	.003
MA(1)	-.01	.044	.79	-.23	.241	.35	-.24	.184	.19
N observations		104			104			104	
R^2 value		.12			.16			.16	
Durbin Watson Statistic		1.99			1.98			2.00	
Breusch-Godfrey Test		1.16	.33		1.14	.35		1.22	.29
White Heteroskedasticity		.53	.94		.47	.97		.46	.97

Note: Estimations conducted after fractionally differencing of the series. Newey-West HAC consistent/pre-whitened standard errors (AIC; 4 lags) are presented along with two-tailed probability tests. [a] Series divided by 1000 for parameter scale. [b] Series multiplied by 100 for parameter scale. Breusch-Godfrey serial correlation test with 11 lags (H_0: residuals do not exhibit serial correlation). White Heteroskedasticity test (H_0: residuals are homoskedastic).

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Supplemental Table 3: KPSS Unit Root Tests on Indicated Variables

Series	Lag Truncation Parameter (*l*)				
	l=0	*l*=2	<u>*l*=4</u>	*l*=6	*l*=8
Opinion-level Dissent Rate	7.619	2.651	1.622	1.178	0.933
Court-level Dissent Rate	8.037	2.824	1.736	1.267	1.007
Associate Justice Dissent Rate	7.988	2.805	1.724	1.257	0.998
Chief Justice Dissent Rate	5.855	2.504	1.650	1.256	1.028
Opinion-level Concurrence Rate	9.230	3.287	2.035	1.484	1.176
Court-level Concurrence Rate	9.352	3.381	2.091	1.523	1.205
Associate Justice Concurrence Rate	9.342	3.390	2.100	1.532	1.215
Chief Justice Concurrence Rate	3.390	1.665	1.307	0.812	0.661
Criminal Cases	5.174	2.208	1.452	1.099	0.901
Civil Liberties Cases	6.150	2.207	1.356	0.990	0.788
Economic Cases	9.015	3.170	1.962	1.439	1.146
Institutional Power Cases	1.959	1.269	1.008	0.842	0.715
Original and Misc. Cases	1.917	1.338	0.905	0.693	0.567
Cases on Docket	9.003	3.087	1.903	1.397	1.117
Associate Justice Ideology (std dev)	1.327	0.490	0.328	0.261	0.225
Associate Justice Experience (mean)	1.946	0.708	0.459	0.353	0.296
Chief Justice Freshman Effect	0.217	0.166	0.178	0.195	0.214

Note: The indicated results are for a constant and no trend. Critical values for KPSS tests come from Kwiatkowski, *et al.* (1992). The critical value (p<.05) is 0.463; at p<.01, 0.739. The KPSS test proposes a null hypothesis that the series is characterized by a strong mixing process and, thus, stationary or short-memoried. The recommended lag truncation (*l*) is given by KPSS (1992, 69-73): *l*=integer[4(T/100)] = 4 for these series.

Supplemental Table 4: Point Estimates of the Order of Integration (*d*) of Indicated Variables

Series	Estimate of *d*[a]	H_o: d=0[b]	H_o: d=1[b]
Opinion-level Dissent Rate	0.81 (0.078)	-2.43	10.37
Court-level Dissent Rate	0.76 (0.078)	-3.07	9.73
Associate Justice Dissent Rate	0.75 (0.078)	-3.20	9.60
Chief Justice Dissent Rate	0.40 (0.078)	-7.68	5.12
Opinion-level Concurrence Rate	0.54 (0.078)	-5.89	6.91
Court-level Concurrence Rate	0.46 (0.078)	-6.91	5.89
Associate Justice Concurrence Rate	0.44 (0.078)	-7.17	5.63
Chief Justice Concurrence Rate	0.48 (0.078)	-6.66	6.14
Criminal Cases	0.43 (0.078)	-7.30	5.51
Civil Liberties Cases	0.67 (0.078)	-4.23	8.58
Economic Cases	0.74 (0.078)	-3.33	9.48
Institutional Power Cases	0.16 (0.078)	-10.76	2.05
Original or Misc. Cases	0.26 (0.078)	-9.48	3.33
Cases on Docket	1.25 (0.078)	3.20	16.01
Associate Justice Ideology $_{\text{(std dev)}}$	1.28 (0.078)	3.58	16.39
Associate Justice Experience $_{\text{(mean)}}$	1.07 (0.078)	0.90	13.70
Chief Justice Freshman Effect	0.15 (0.078)	-10.88	1.92

Note: Robinson's (1995) Gaussian Semiparametric Estimate of *d* is presented. [a] Because of the constrained parameter space (i.e., $-1.5 \le d \le 0.5$), estimates were completed on first-differenced data (0, 1, 0). Thus, the results reflect the estimates of d + 1. The numbers in parentheses are the standard error of the estimate of *d*. The numbers in parentheses are the standard error of the estimate of *d*. [b] These are the *t*-ratios of the null hypothesis that d = 0 and d = 1 for the level-form, undifferenced data.

Supplemental Table 5: Changepoint Estimation of Concurrence and Dissent Rates

Supreme Court Dissent Rate

Matrix of Natural Log Bayes Factors

	1 Changepoint	2 Changepoints	3 Changepoints	4 Changepoints
1 Changepoint	.00	-1.91	.53	4.14
2 Changepoints	1.91	.00	2.44	6.05
3 Changepoints	-.53	-2.44	.00	3.61
4 Changepoints	-4.14	-6.05	-3.61	.00
Log Marginal Likelihood	-271.45	-269.54	-271.98	-275.59

Supreme Court Concurrence Rate

Matrix of Natural Log Bayes Factors

	1 Changepoint	2 Changepoints	3 Changepoints	4 Changepoints
1 Changepoint	.00	-25.82	-25.41	-24.28
2 Changepoints	25.80	.00	.41	1.54
3 Changepoints	25.40	-0.41	.00	1.13
4 Changepoints	24.30	-1.54	-1.13	.00
Log Marginal Likelihood	-232.58	-206.76	-207.17	-208.31

Note: Poisson changepoint estimates in MCMC Pack (Martin, Quinn and Park 2010) with *c0* shape parameter set to the mean concurrence or dissent rate (5.5 and 13.0 respectively), the *d0* scale parameter set to 1, and 100K burn in and mcmc iterations. Per Jeffrey's rule, support for the existence of 2 changepoints in each series can be found. For the dissent rate, the changepoints are the 1938 and 1943 terms. For the concurrence rate the changepoints are the 1942 and 1964 terms.

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