

On Separation of Powers and Obfuscation in U.S. Supreme Court Opinions*

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Abstract

A longstanding debate in American judicial politics concerns whether the U.S. Supreme Court anticipates or responds to the possibility that Congress will override its decisions. A recent theory proposes that opinions that are relatively hard to read are more costly for Congress to review—and that as a result, the Court can decrease the likelihood of override from a hostile Congress by *obfuscating* its opinions: writing opinions that are less readable when congressional review is a threat. I derive a straightforward but novel empirical implication of this theory; I then show that the implication does not in fact hold. This casts serious doubt on the claim that justices strategically obfuscate opinion language to avoid congressional override. I also discuss sentence tokenization as a source of measurement error for readability statistics for judicial opinions.

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A longstanding debate in American judicial politics concerns whether the U.S. Supreme Court anticipates or responds to the possibility that Congress will override its decisions. A widely-cited paper, Owens, Wedeking and Wohlfarth (2013, 38–40) theorizes that opinions that are relatively hard to read are more costly for Congress to review.¹ Thus, the paper argues, the Court can decrease the likelihood of override from a hostile Congress by *obfuscating* its opinions: writing opinions that are less readable when congressional review is a threat. Owens, Wedeking and Wohlfarth (2013, 48) provides evidence that the Court’s majority opinions are relatively less readable when the Court is constrained by Congress (in a sense to be made precise below).

On the one hand, as Owens, Wedeking and Wohlfarth (2013, 52) acknowledges, this result is, broadly speaking, inconsistent with a fair bit of empirical research about congressional influence on the Court (see e.g., Owens 2010; Owens 2011; Sala and Spriggs 2004; Segal 1997; Segal, Westerland and Lindquist 2011).² On the other hand, the theory in Owens, Wedeking and Wohlfarth (2013)—which I will refer to as *strategic obfuscation theory*—has an important strength: it is *elaborate*, in the sense used by, for example, Rosenbaum (2010, Ch. 19). This is to say, in short, that strategic obfuscation theory’s proposed causal mechanism has several different testable implications.³ In this research note, I propose and test one straightforward implication of the theory’s causal mechanism.

Theory

Strategic obfuscation theory draws loosely on literature formally modeling how courts or other agencies can raise the costs of review for supervisory bodies (e.g., Staton and Vanberg 2008). The initial premise of strategic obfuscation theory is intuitive and well-supported: Congress has limited resources and time (e.g., Cox and McCubbins 2005; Lee 2010). As such, the costs of taking a given action are always relevant for Congress. Owens, Wedeking and Wohlfarth

¹As of 7/5/21, the paper has been cited 71 times according to Google Scholar, including by at least three textbooks.

²But see King (2007) and, somewhat more generally, Clark (2009).

³The terminology dates back to R.A. Fisher and William G. Cochran (Rosenbaum 2015). The perspective is of course consistent with mainstream philosophy of science, which prefers theories that make relatively more falsifiable predictions; see Rosenbaum (2017, Ch. 7) discussing, among others, Popper (2002) [1959].

(2013, 39) cites, for example, collective action problems, the need to regularly credit-claim and produce benefits for constituents, and the shrinking size of staffs as factors limiting congressional capacity to act on issues, particularly those that are complex.

The novel proposal in Owens, Wedeking and Wohlfarth (2013) is that the Court can raise the costs for Congress to review and potentially override its opinions by obfuscating the language therein. The argument is as follows:

Obfuscated Court opinions can generate heightened review costs and thereby deter congressional responses. To understand complex and obscure Court decisions, Congress must expend additional—and scarce—resources. A member who wishes to alter the Court’s policies or otherwise punish the Court must examine the central logic and tenets of the Court’s opinions and may even need to examine how the opinion compares to others written in the past by the Court. In some cases, the Court’s opinion may be clear. In those cases, members may easily internalize the degree to which they favor the political content of the majority opinion. Yet the Court also has the ability to obfuscate opinions by making them less readable. In those instances, the heightened legislative costs required to address the opinion may increase. By writing a less readable opinion, justices might craft a desired judicial policy while simultaneously deterring a legislative response by making it more difficult for Congress to address it (Owens, Wedeking and Wohlfarth 2013, 39).

Owens, Wedeking and Wohlfarth (2013, 39–40) recognizes that making opinions less readable will not absolutely bar review, and that obfuscation has costs; for example, it may cause lower courts or relevant agencies to implement Court policies inaccurately (see also Black, Owens, Wedeking and Wohlfarth 2016). Nonetheless, strategic obfuscation theory proposes that when the threat of congressional override is great—in particular, when the Court is constrained—the Court can reduce the chances of review by obfuscating the language in the majority opinion. To this end, under the proposed causal mechanism, the majority opinion author intentionally obfuscates when facing a hostile Congress. Owens, Wedeking and Wohlfarth (2013) presents

results indicating that majority opinions are written less readably when the Court is constrained; the magnitude of the effect is as much as one full grade level.⁴

Owens, Wedeking and Wohlfarth (2013) does not discuss dissenting opinions or the theoretically-predicted behavior of dissenters. Nonetheless, as I argue here, strategic obfuscation theory has clear implications for how dissenting opinions should be written. Consider the incentives of the dissenters. When should dissenters seek to increase the probability of review? Assuming policy-motivated actors (as does strategic obfuscation theory), they should do so when they prefer the policy that would result from an override to the policy announced in the majority opinion. Let C be the policy announced in the Court’s majority opinion, D the dissenters’ most preferred policy, and R the policy that results from congressional review and override. Given a very basic spatial model, the dissenters prefer an override whenever $|D - R| < |D - C|$ —whenever the dissenters are closer to Congress than to the Court majority.⁵ Since, under strategic obfuscation theory, the probability of review increases as obfuscation decreases, dissenters should write particularly *readably* when the majority is constrained but the dissenters prefer an override on policy grounds. The benefit to dissenters from writing more clearly when the Court is constrained is in making an override—and thus, a policy outcome they prefer to that resulting from the majority opinion—more likely.⁶

To summarize, under strategic obfuscation theory, while the Court majority has incentive to obfuscate when the Court is constrained, the dissent does not. Rather, the dissenters have the opposite incentive insofar as they prefer the policy that would result from an override: they should write particularly readably.⁷ By making it easier for Congress to understand what the

⁴This is as measured by the Coleman-Liau Index of readability. I describe the measure and detail other empirical specifics below.

⁵I discuss operationalization of these policy locations below.

⁶As stated just above, this is conditional on $|D - R| < |D - C|$; my empirical tests below account for this caveat.

⁷One might object that dissenters may wish to avoid overrides from Congress even if they favor Congress’ policy preferences over those of the majority, perhaps for institutional reasons. There are two answers to this point. First, we know that, on a regular basis, justices do explicitly invite overrides from Congress (e.g., Hausegger and Baum 1999; Rice 2019); thus, if explicit requests are normative, surely so are implicit actions that increase the chances of review. Second, even granting for the moment that dissenting justices do not intentionally seek to increase the readability of their opinions when Congress is hostile to the majority, they—at the very least—have no reason to affirmatively obfuscate under strategic obfuscation theory. This is a slightly different, in a sense weaker, implication; but given the results I present below, the distinction turns out to be

majority opinion implies, and where the majority opinion has erred, the dissenters can reduce the costs of review for Congress, and make it more likely that an opinion they disagree with is overridden. I test this implication of strategic obfuscation theory below.

Measurement, Sample, Hypothesis

Measurement of Variables

Constructing a dependent variable requires a measure of obfuscation, that is, (lack of) readability.⁸ Following Owens, Wedeking and Wohlfarth (2013, 42–44), I use the Coleman-Liau Readability Index (CLI). This index is a function of word and sentence length. A key advantage of the measure is ease of interpretation, since it is scaled to approximate the (U.S.) grade levels of education needed to understand a text. CLI is defined as:

$$\text{CLI} \equiv 5.88 \left(\frac{\text{Number of Letters}}{\text{Number of Words}} \right) - 29.6 \left(\frac{\text{Number of Sentences}}{\text{Number of Words}} \right) - 15.8. \quad (1)$$

Thus, as intuitive, a text becomes more readable (or less obfuscated) as average word length and average sentence length decrease. Later I discuss specifics regarding implementation, and discuss of the non-trivial challenges associated with measuring CLI’s constituent terms.

The most straightforward way to construct an appropriate dependent variable is to use the average CLI for dissents in a given case; I call this variable *Dissent CLI*.⁹ Under strategic obfuscation theory, the expectation is that *Dissent CLI* decreases when the Court (i.e., the majority) becomes constrained, since the dissenters then have incentive to make the opinions more readable.

The key independent variables are measures of congressional constraint. In the results I present in the main text, I follow Owens, Wedeking and Wohlfarth’s (2013) operationalization

irrelevant.

⁸In the relevant literature, readability is also referred to as (rhetorical) clarity.

⁹In practice, this involves combining all dissents for a given case into a single text file, and calculating a CLI for the combined text. Thus, this is a weighted average of all dissents in a case, where the weights are a function of individual opinion length.

in all respects.¹⁰ Conceptually, the measures are set to 0 when the Court is unconstrained—that is, when it is located ideologically between the most extreme congressional pivots (very generally, see Krehbiel 1998). When the Court is constrained—that is, when it is to the right of the rightmost pivot or left of the leftmost pivot—it takes on the value of the ideological distance between the Court and the pivot closest to it. Figure 1 illustrates. If the scenario shown on the top axis obtains, the measure of constraint equals 0. If the scenario on the bottom holds, the measure equals the euclidean distance between C and P_L .

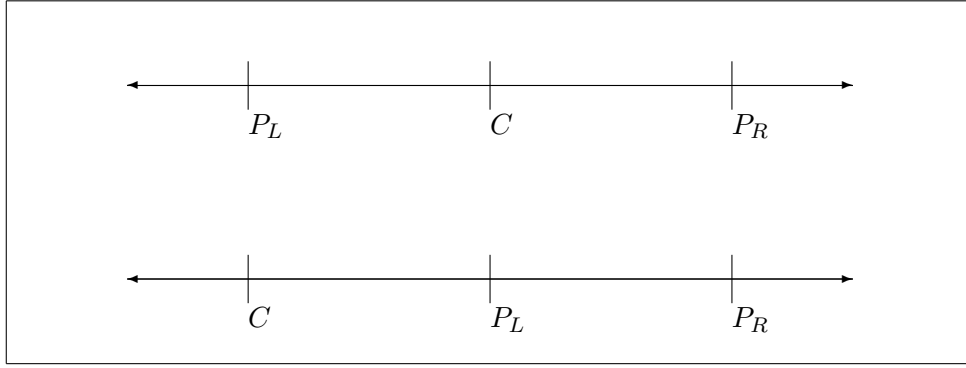


Figure 1. The Court (C) is unconstrained on the top axis, since it is between the leftmost congressional pivot (P_L) and the rightmost pivot (P_R). It is constrained on the bottom axis, because it is to the left of the leftmost pivot. Were the Court to the right of the rightmost pivot, it would also be constrained.

It remains to locate the Court and relevant pivots in ideological space. My approach is exactly that of Owens, Wedeking and Wohlfarth (2013). I locate actors using Judicial Common Space (JCS) scores (Epstein, Segal, Spaeth and Walker 2007); that is, 1st Dimension DW-NOMINATE scores for legislators (Lewis, Poole, Rosenthal, Boche, Rudkin and Sonnet 2021) and Martin-Quinn (2002) scores transformed into DW-NOMINATE space for justices. JCS scores thus range from -1 (most liberal) to 1 (most conservative). The Court’s ideal point in a given case is identified as that of the median in the majority coalition. There are four different ways of locating relevant pivots, each motivated by a different model of congressional policymaking (Owens, Wedeking and Wohlfarth 2013, 45). For each of the four models, the relevant pivots are the leftmost and the rightmost of the following actors:

¹⁰I do this to limit researcher degrees of freedom (Simmons, Nelson and Simonsohn 2011). But there are any number of reasonable alternative variable specifications; I discuss some of these in the Appendix.

1. *Filibuster Pivot Model*. For and after the 94th Congress: the House median, the 40th most conservative Senator (i.e., the Senator with the 40th greatest JCS score), and the 60th most conservative Senator. Before the 94th Congress: the House median, the Senator at the 33rd percentile of conservatism, and the Senator at the 67th percentile of conservatism.¹¹
2. *Chamber Median Model*. The median member of the Senate and the median member of the House.
3. *Committee Median Model*. The median member of the Senate Judiciary Committee and the median member of the House Judiciary Committee.¹²
4. *Majority Party Median Model*. The median member of the majority party in the Senate, and the median member of the majority party in the House.

There are thus four variants of the key independent variable measuring the degree to which the Court majority is constrained: *Distance To Filibuster Pivot*, *Distance To Chamber Median*, *Distance To Committee Median*, and *Distance To Majority Party Median*. Each is defined as the absolute difference between the ideal point of the Court (i.e., the Judicial Common Space score for the median of the majority coalition in a case) and the ideal point of the closest pivot, as defined just above, if the Court is constrained, and 0 otherwise. Thus, as the Court becomes “more constrained,” these variables increase. Below, I refer to these variables collectively as the *constraint variables*.

I include the same control variables as Owens, Wedeking and Wohlfarth (2013): *Lower Court Conflict*, *Case Complexity*, *Precedent Alteration*, *Judicial Review*, and *Coalition Heterogeneity*. The definitions follow those given in Owens, Wedeking and Wohlfarth (2013, 46–47). Unless noted, the variables are based on information in Spaeth, Epstein, Segal, Ruger, Martin and Benesh (2017).

¹¹This is of course because before the 94th Congress, two thirds of Senators were required to vote for cloture to end a filibuster, while starting with the 94th Congress, only 60 Senators were required. I refer to percentiles since in the earliest years of my sample there were only 96 senators.

¹²I identified committee members using two datasets: Swift, Brookshire, Canon, Fink, Hibbing, Humes, Malbin and Martis (2009) and Stewart III and Woon (2017).

Lower Court Conflict equals 1 if the Court notes that the sole reason it granted a case is to resolve a conflict in the lower federal or state courts, and 0 otherwise. Case Complexity is the number of amicus briefs in a case. These data are from Collins (2008) and Box-Steffensmeier and Christenson (2012). Precedent Alteration equals 1 if the majority opinion alters existing Court precedent, and 0 otherwise. Judicial Review equals 1 if the majority struck down a federal law, and 0 otherwise. Coalition Heterogeneity is the standard deviation of Martin-Quinn (2002) scores for justices voting in a given majority or minority coalition.¹³

Sample

Building on the Supreme Court Database (Spaeth et al. 2017), I construct a dataset of all signed Supreme Court majority opinions from October Terms 1947-2012. This is altogether 6699 majority opinions, 6690 once observations with data missing on covariates are dropped. I am chiefly concerned with those cases where at least one dissent was authored.¹⁴ There are 3374 such cases, 3372 once observations with data missing on covariates are dropped.

I exclude one (small) subset of cases for reasons discussed above: those where the majority is constrained, but the dissenters prefer the policy in the majority opinion to that which would result from a congressional override. (I.e., where $|D - R| > |D - C|$.) To locate the policy from an override (R), I take the midpoint between the House and Senate chamber medians for the Filibuster Pivot, Chamber Median, and Committee Median models, and the midpoint between the House and Senate majority party medians for the Majority Party Median Model (for detailed discussion, see e.g. Harvey and Friedman 2006, 540–542). I locate the dissent (D) at the median of dissenting justices’ ideal points. (And as stated above, I follow Owens, Wedeking and Wohlfarth (2013) in locating the Court majority opinion (C) at the median of the majority coalition.) This leaves between 2755 and 2964 cases in the estimation sample, depending on the theoretical model used to locate congressional pivots. Notably, this sample is significantly larger

¹³In other words, if a given observation is a majority opinion, Coalition Heterogeneity is the standard deviation of the Martin-Quinn scores for the majority justices, and if the observation is a dissenting opinion (or opinions), Coalition Heterogeneity is the standard deviation of the Martin-Quinn scores for the dissenting justices). I define dissenting opinion Coalition Heterogeneity for a single dissenting justice as 0.

¹⁴I exclude from the definition of *dissent* those dissents in part where the “dissenters” were coded as agreeing with the majority disposition by Spaeth et al. (2017).

(and slightly broader in temporal scope) than the sample in Owens, Wedeking and Wohlfarth (2013), which is a random sample of 529 majority opinions 1953–2008; thus, a lack relative lack of power is not a concern.

Hypothesis

As discussed above, strategic obfuscation theory implies that dissent authors will write more readably when the Court majority is constrained. Thus, the hypothesis is that if the Court majority’s distance to a relevant pivot increases, readability of dissenting opinions increases. Precisely, as Distance to Filibuster Pivot, Distance to Chamber Median, Distance to Committee Median, and Distance to Majority Party Median, respectively, increase, Dissent CLI decreases.

Analysis

For each of the four constraint variables (Distance To Filibuster Pivot, Distance To Chamber Median, Distance To Committee Median, and Distance To Majority Party Median), I estimate three models predicting a dissenting opinion’s CLI. The Baseline model includes no controls, the Add Controls model includes the controls mentioned above (Lower Court Conflict, Case Complexity, Precedent Alteration, Judicial Review, Coalition Heterogeneity), while the Add FEs model includes these controls and also fixed effects for dissenting opinion author and primary issue area. The models are OLS regressions, with standard errors clustered by term.¹⁵

Table 1 presents regression coefficients and standard errors for the twelve models. For none of the specifications is the coefficient on the constraint variable statistically significant and negative; this is clearly contrary to theoretical predictions. In fact, all of the coefficients are positive, and statistically significant in seven specifications, including all three specifications where Distance to Filibuster Pivot is the variable measuring constraint. Thus, there is no

¹⁵Again, this follows the procedure in Owens, Wedeking and Wohlfarth (2013), though there, of course, the fixed effects are for the majority opinion author, since the outcome is majority opinion CLI. I combine all justices writing fewer than 30 opinions into a single “Other Justice” category, to allow for valid estimation of the clustered standard errors.

indication that dissenters write more readably when the majority is constrained, contrary to the expectation derived from strategic obfuscation theory.

Key IV	Baseline	Add Controls	Add FEs
Distance to Filibuster Pivot	1.230* (.20)	1.221* (.18)	0.514* (.16)
Distance to Chamber Median	0.760* (.18)	0.779* (.15)	0.215 (.12)
Distance to Committee Median	0.465* (.18)	0.510* (.15)	0.119 (.11)
Distance to Majority Party Median	0.208 (.17)	0.082 (.16)	0.069 (.09)

Table 1. Dissenting opinion readability as a function of Court majority constraint. DV: Dissenting opinion CLI. OLS coefficients and standard errors (clustered by term), for twelve models: four variants of a distance to relevant pivot, and three model specifications. See text for details. (*: $p < 0.05$.)

Discussion

Can these results be made consistent with strategic obfuscation theory? One argument, based on a certain form of unobserved confounding, is as follows. Suppose that there is a case-level confounder that happens to be positively associated with an opinion’s CLI, and also (by unfortunate chance) with the constraint variables. On the face of it, at least the first association is not unreasonable: the fixed effects for primary issue are relatively crude, consisting of 13 issue categories (Spaeth et al. 2017). If such a confounder exists, the results in Table 1 could still hold if this confounding overwhelms dissenters’ efforts to write readably; in other words, one might propose that the coefficients in Table 1 would be *even greater* (due to the confounding) if dissenters did not make a particular effort to write readably when strategically warranted.¹⁶

One way to rule out this out is to examine how the putative effects of the constraint variables vary between majority opinions and dissents. Even under the proposed confounding, the positive association between the constraint variables and opinion CLI should be greater for

¹⁶But note that such confounding would imply that the results in Owens, Wedeking and Wohlfarth (2013) are also overestimates.

majority opinions, whose authors are trying to obfuscate, than for dissenting opinions, whose authors are trying to write readably.

I test for this possibility by analyzing both majority opinions and dissents in a single model; specifically, I add the majority opinion associated with each case whose dissent(s) were analyzed in Table 1. I estimate the same models shown in Table 1, except I include a binary variable indicating whether a given opinion is (=1) or is not (=0) a majority opinion, which I interact with the constraint variable in the model. If there is a stronger positive association between a constraint variable and CLI for majority opinions, the coefficient on the interaction term should be positive.

Key Interaction	Baseline	Add Controls	Add FEs
Distance to Filibuster Pivot × Majority Opinion	−0.829* (.16)	−0.833* (.17)	−0.526* (.19)
Distance to Chamber Median × Majority Opinion	−0.525* (.13)	−0.560* (.14)	−0.325* (.14)
Distance to Committee Median × Majority Opinion	−0.487* (.12)	−0.487* (.12)	−0.266* (.11)
Distance to Majority Party Median × Majority Opinion	−0.093 (.12)	−0.103 (.12)	0.023 (.09)

Table 2. Opinion readability—conditional on opinion status (majority vs. dissent)—as a function of Court majority constraint. DV: Opinion CLI. OLS coefficients and standard errors (clustered by term), for twelve models: four variants of a distance to relevant pivot, and three model specifications. The table presents the coefficients and standard errors for the key interaction, which should be positive if majority opinions, more so than dissenting opinions, are written as majority opinions are predicted to be written by strategic obfuscation theory. See text for details. (*: $p < 0.05$.)

The results, given in Table 2, are contrary to the prediction derived from strategic obfuscation theory. For none of the 12 specifications is the coefficient attending the interaction term positive and statistically significant. In 11 of 12 specifications, the coefficient is negative; in nine of those 11—in all but those where Distance to Majority Party Median is the measure of constraint—the coefficient is statistically significant. Thus, the weight of the evidence indicates that the effect of majority constraint is greater for dissents than for majority opinions. That is, dissenters apparently increase their level of obfuscation *more* than the majority, as the threat

of congressional override increases.¹⁷

These results are robust. A similar pattern of results obtains if I modify the sample used in Table 2 by adding unanimous majority opinions. The same is true for a slightly different analytical approach: setting the case as the unit of analysis, and the difference between the majority and dissent CLIs as the dependent variable. These results are in the Appendix, along with other specifications involving an alternative measure of ideology, definition of constraint, and location of Court majority opinion. In each of those specifications, as in all but one specification above, the dissenters' response to constraint is closer to the response theoretically predicted for the majority, *than the response of the majority itself*.

In short, not only is there no evidence that dissenters write more readably when the majority is constrained, but there is not even evidence that majority opinion authors obfuscate as a function of Court constraint to a greater degree than dissenters. This is incompatible with strategic obfuscation theory, since majority obfuscation is strategically warranted when the Court is constrained, and dissenting opinion obfuscation is strategically unwarranted.

In sum, I have shown that a straightforward empirical implication derived from the theory of strategic obfuscation receives no support. This casts serious doubt on the theory—specifically, its proposed causal mechanism: that justices strategically manipulate writing style to avoid review from Congress. To be explicit, strategic obfuscation theory has no straightforward theoretical explanation for why justices would seek to affect the probability of override when in the majority, but *those same justices* would not seek to do so when in dissent. (Even worse for the theory, justices in dissent tend to write in ways that are not just nonstrategic, but strategically counterproductive.) Turning now from the theoretical to the practical, I consider the potential role of measurement error in the original result supporting strategic obfuscation theory.

¹⁷The alert reader may note that the coefficients from the analysis in Owens, Wedeking and Wohlfarth (2013) are generally greater than those in Table 1 above. Below, I explain why, despite this apparent discrepancy, the results in Table 2 and the associated discussion should be credited.

On Calculating Accurate Sentence Counts

In this section, I sketch my approach to calculating the number of sentences in a legal text and show that it is a major improvement on the approach used by Owens, Wedeking and Wohlfarth (2013). Then I replicate the original analysis in Owens, Wedeking and Wohlfarth (2013) on my larger sample with an improved sentence counter, showing that the coefficients on the constraint variables are several times smaller than those originally reported; the discrepancy is likely due in large part to measurement error in the original study.

Recall the definition of that the definition of CLI (given in Eq. 1) requires counting the number of letters, words, and sentences in a text. Counting the number of letters and words in a legal opinion is relatively straightforward. The challenge is accurately counting the number of sentences in a text.

A naive approach is to count as a sentence any segment of text that ends in an end-of-sentence punctuation mark like ., ?, or !. This is not satisfactory, however, since abbreviations within sentences can also contain periods. (See also initials and ellipses.) And legal opinions are full of abbreviations—most notably, but not only, as part of citations: U.S., L.Ed, F.2d, and so on.

I implement my approach in the Python programming language, relying on tools in Bird, Loper and Klein (2009). The first step is to use the unsupervised sentence boundary detection method (or sentence *tokenizer*) in Kiss and Strunk (2006). Specifically, I train the tokenizer on a corpus of appellate opinions. Essentially, the tokenizer looks for collocations: pairs (more precisely, n-tuples) of text strings with a period between them that are likely to be abbreviations. I also add a set of abbreviations from the pre-trained English language sentence tokenizer from Kiss and Strunk (2006). In my application, this only gives a slight improvement over the naive approach.

Thus, I manually augment the abbreviations and collocations detected by the Kiss and Strunk (2006) tokenizer with various “legal” abbreviations and collocations (e.g., `civ.p.`, `id.at`, `u.s.c.`, and many more). This step gives the greatest improvement over the naive

method. I then take sentences tokenized by the augmented tokenizer and disallow certain “sentences” that are unlikely to actually be sentences: for example, those that start with a lower-case letter, those that contain fewer than 3 words, and those that end with certain abbreviations. This gives a further slight improvement.

Owens, Wedeking and Wohlfarth (2013, 43) gives a sentence count for one of the opinions in its sample: *Washington v. Recueno* (548 U.S. 212), which is classified as having 400 sentences. (Indeed, the opinion includes 400 periods.) My method counts 99 sentences. The opinion is reproduced in the Appendix. An exact manual count of sentences is not entirely straightforward because readers might have the occasional disagreement on what constitutes a sentence, but in any case, it is clear that 99 is at worst a slight overestimate, and 400 overstates the number of sentences by at least a factor of 4.¹⁸ This overestimate of course distorts the CLI: while Owens, Wedeking and Wohlfarth (2013) gives a CLI score of 6.1 (implying that the opinion is comprehensible to a sixth-grader), my estimate of the CLI is 13.3.

The face validity of some other scores cited in Owens, Wedeking and Wohlfarth (2013, 43, fn. 8) is also open to doubt; for example, *Crane v. Cedar Rapids and Iowa City Railway Co.* (395 U.S. 164) starts with these two (not atypical) sentences:

The Federal Safety Appliance Act of 1833 requires interstate railroads to equip freight cars “with couplers coupling automatically by impact,” but does not create a federal cause of action for employees or nonemployees seeking damages for injuries resulting from a railroad’s violation of the Act. The Federal Employers’ Liability Act of 1908 provides a cause of action for a railroad employee based on a violation of the Safety Appliance Act, in which he is required to prove only the statutory violation and the carrier is deprived of the defenses of contributory negligence and assumption of risk.

Owens, Wedeking and Wohlfarth (2013, 43) score this opinion’s CLI as a 4.3, while my estimate

¹⁸Other off-the-shelf methods are only somewhat better. The **quanteda** package (Benoit, Watanabe, Wang, Nulty, Obeng, Muller, Matsuo and Lowe 2021) in R counts 268 sentences. NLTK’s (Bird, Loper and Klein 2009) **sent_tokenize**—which is in fact an unmodified version of **punkt** trained on English language texts—gives 160 sentences; better than **quanteda** but still a significant overestimate.

is 12.6. In short, it is very likely that measurement error due to a naive sentence count affects the dependent variable in the original analysis.

Ex ante, it is not obvious how much this biases the central results in Owens, Wedeking and Wohlfarth (2013): the rank ordering of opinions' CLI could be more-or-less preserved if there is a global overestimation of the number of sentences. I thus replicate the main analyses, using the larger sample I describe above (and the improved sentence counter/CLI score). Table 3, analogous to Table 1 in Owens, Wedeking and Wohlfarth (2013), presents these results.

Key IV	(1)	(2)	(3)	(4)
Distance to Filibuster Pivot	0.216 (0.11)			
Distance to Chamber Median		0.114 (0.08)		
Distance to Committee Median			0.072 (0.08)	
Distance to Majority Median				0.010 (0.05)
Lower Court Conflict	0.029 (0.03)	0.029 (0.03)	0.029 (0.03)	0.029 (0.03)
Case Complexity	0.024* (0.00)	0.023* (0.00)	0.023* (0.00)	0.023* (0.00)
Precedent Alteration	0.021 (0.06)	0.020 (0.06)	0.019 (0.06)	0.021 (0.06)
Judicial Review	0.260* (0.08)	0.261* (0.08)	0.261* (0.08)	0.258* (0.08)
Coalition Heterogeneity	0.005 (0.02)	-0.002 (0.02)	-0.006 (0.02)	-0.012 (0.02)

Table 3. Majority opinion readability as a function of Court majority constraint. $N = 6690$. DV: Majority opinion CLI. OLS coefficients; standard errors clustered by term. Constant and fixed effects for primary issue area and opinion author not shown. See text for details. (*: $p < 0.05$.)

The coefficient on each of the key independent variables (Distance To Filibuster Pivot, Distance To Chamber Median, Distance To Committee Median, and Distance To Majority Party Median) remain positive, as strategic obfuscation theory predicts. But, whereas in the original analysis, all except one of the key independent variables (Distance to Majority Party Median) had a statistically significant attending coefficient, now none of them remain significant. Even more to the point, the coefficients are much smaller than those reported in the original analysis. A one-unit increase in Distance to Filibuster Pivot increases CLI by less than a quarter grade

level; in fact, Distance to Filibuster Pivot ranges only from 0 to about .55, so the maximum in-sample effect is approximately an eighth of a grade level.¹⁹

This, then, explains why Table 1 and Table 2 are correct despite the results in Owens, Wedeking and Wohlfarth (2013). The results in the original article vastly overestimate, in all likelihood, the coefficients on the key independent variables. Once those results are corrected, by using a more accurate sentence tokenizer to count sentences, it is clear that the coefficients in Table 1 are larger than those in Table 3, which effectively implies the results in Table 2.²⁰

Conclusion

This research note has presented two central results. First, I have shown that a straightforward empirical implication derived from the theory of strategic obfuscation receives no support. Specifically, dissenting justices who prefer a congressional override on policy grounds do not write more readably to an increase the probability of an override when majority is constrained (and thus subject to potential override). This casts serious doubt on the theory—specifically, its proposed causal mechanism: that justices strategically manipulate writing style to affect the probability of review from Congress.

Still, there are positive lessons to be learned from this result. Because strategic obfuscation theory was elaborate (in the sense I discussed above), it allowed for testing of multiple implications. This must be acknowledged as a strength of the theory, even though it did not ultimately find empirical support. As Rosenbaum (2015) eloquently points out, scholars should

¹⁹This difference from the original result cannot be attributed to the different samples. I do not have access to the particular sample of 529 cases used in Owens, Wedeking and Wohlfarth (2013), but I investigate the role of sampling by re-running the analysis in Column 1 of Table 3 on 500 random samples of 529 cases. The median coefficient attending Distance to Filibuster Pivot across the 500 sample draws is .19 (standard deviation: .32); the largest across 500 draws is 1.20—less than the 1.81 in Owens, Wedeking and Wohlfarth (2013, 48). It is possible of course that *some* of the difference in our results is due to an (un)lucky sample draw in Owens, Wedeking and Wohlfarth (2013); it is very unlikely that this accounts for any meaningful part of the difference. In any case, since my sample—which is effectively the population of relevant cases from 1947–2012—subsumes the sample in Owens, Wedeking and Wohlfarth (2013), it is the results here that should be credited, at least on sampling grounds.

²⁰I use the qualifier “effectively” because the results in Table 2 exclude unanimous opinions and (as discussed) a small set of non-unanimous opinions, whereas the results in Table 3 above include both unanimous and non-unanimous majority opinions (as do the results in Owens, Wedeking and Wohlfarth (2013)).

hesitate to accept theories that make only a single prediction or a few related predictions; an elaborate theory, which makes several independent predictions, is to be preferred.²¹ True, the evidence from testing several implications of an elaborate theory may be ambiguous, or even disappointing. But “inconsistency and uncertainty [are] necessary stepping stones on a path to greater consistency and greater certainty (Rosenbaum 2015, 209).”

The second central result in the note is that the initial analysis supporting strategic obfuscation theory, in Owens, Wedeking and Wohlfarth (2013), was likely affected by measurement error in the outcome variable, CLI. Specifically, overestimation of the number of sentences in the opinions led to underestimation of CLI scores, which in turn appears to have inflated the effect estimates by a factor of at least four.

This has implications not just for strategic obfuscation theory, but more generally for researchers who seek to calculate readability metrics for legal texts. In particular, researchers should ensure that the sentence tokenizer (segmenter) used to calculate the number of sentences is adapted to the peculiarities of judicial opinions. Most importantly, the tokenizer should account for the abbreviations that are common in legal texts but not other English-language texts. The customized tokenizer I use here, which outperforms several off-the-shelf solutions, is included with the replication materials.²²

²¹There is a statistical literature that makes these points more precise. For citations, see Rosenbaum (2015, 209) and Cook (2015, 145).

²²The tokenizer is most appropriate for federal appellate opinions (Courts of Appeals and Supreme Court). Some caution should be used if applied to legal texts other than opinions, if applied to state court or federal trial court opinions, or opinions distant from the time period considered here. It should not be used for opinions from non-U.S. courts.

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