

## Reviewer #1

### Empirical approach

**In Section 4, there is no clear justification provided for the choice of sample. What is the conceptual reason behind the choice to “narrow” the cases as described at the bottom of p. 15?**

Regarding leaving out the first decade of the CCC, the reason is the great imprecision of information: we do not sometimes even know who attached a certain separate opinion, what type of it was, the composition of the benches is mostly unknown – even despite using the official CCC website + contacting the unit responsible for keeping it to obtain the data.

Regarding leaving out the non-merit decisions 3-chamber decisions, the law stipulates that those decisions have to be made unanimously. Therefore, there is only very limited room for concurring opinions that has barely been used, most of them are just two concurring opinions from two judges that they had copied verbatim into similar decisions. In contrast with that, there are tens of thousands of decisions of this type. To us, it makes sense to limit the analysis only to those decisions, in which disagreement on the bench in the form of potential vote against the majority may arise.

**Why exactly would number of cited acts in a given case be a measure of case complexity? There is no clear justification offered in the paper.**

We have reworked the paragraph on complexity to make it clearer why we deem references to caselaw/legal provisions relevant for legal complexity and included further references to papers that operationalized legal complexity similarly.

**On what basis were specific subjects chosen as controversial? The authors never explain this.**

We tried to explain the research choice further. We are aware that the way to capture the variable via to topics is far from perfect, it's at least somewhat feasible in the Czech context and we tried to provide a robustness check with the reworked placebo test.

**The identification strategy section (4.3) is unclear and lacks substance. For example, the very first paragraph suggests that the authors will be working with “a generalized linear model estimating the probability of a judge attaching a separate opinion with the dependent variable following a binomial distribution”. But then, the results in Table 5 are based on a logit.**

We have rewritten the whole identification part so that it becomes clearer why we make the choices we make and that they are better justified.

We made it clear that the dependent variables follows a Bernoulli distribution, which is a special case of the Binomial distribution with a single trial (in other words, we were not wrong, it was only slightly misleading).

**The discussion of the use of FE on pp. 23-24 is unclear and does not make a clear case for why specific FE are, or are not, included. In particular, if the reader studies Table 5, no FE seem to be included. Yet the authors note on p. 24 that some notion of panel FE is included. But the underlying logic for why these FE are the appropriate ones is not made clear. The point of articulating an identification strategy is to explain how doing something (here, including FE) helps one ascertain a specific ceteris-paribus effect, or set of effects, of interest. It is not about digressing on who included, and who did not include, some FE.**

We have completely rewritten the identification part on FEs to fully justify the choice of fixed effects.

**In the same spirit, the authors note that they use term FE, not year FE. But they do not offer any explanation as to why this is an appropriate choice.**

In the end we opted for the year FE as they would in any case also capture the change in the composition of the CCC + other shocks and the choice is clearer.

**More generally, once one settles on and justifies a particular identification strategy, one should not be engaging in testing of “the model under different specifications by excluding different variables” and then presenting the model based on “the best fit”. This is antithetical to the very logic of hypothetico-deductive reasoning. The authors would benefit from studying the notion of pretest bias.**

The model has been always based on the theory. Testing whether excluding or including the norm-identification and disagreement groups of variables reduces the fit is also motivated by theory. In any case, we have included only the whole model that fully corresponds to the theory and omitted any references to other specifications.

**The placebo tests in Table 7 do not make much sense. If anything, such tests would have to be performed using the full estimated model, as opposed to bivariate specifications (which is how I interpret Table 7).**

The placebo test has been fitted using the same covariates as with the full model even in the old manuscript. The presentation of it has been improved so that now it includes the information that it has been fit with the same specification as the full model both in the text of the article as well as in the description of the table.

We reworked the placebo test even further. Heeding the (Hartman and Hidalgo 2018) paper on equivalence, we instead now tested the placebo against the null that the effects of placebo fall within 0.36 standard deviations of the actual treatment effect of the controversial variable. This way, the null is that the effects of the placebo and the

actual treatment are roughly equal (instead of the placebo failing to reject the null that it is different from 0).

**In generating the results in Table 5, it is not clear what the unit of observation is. A remark on p. 24 seems to suggest it is not “a decision level”. Then what is it? More generally, it would be beneficial to articulate the full empirical model with an equation and using notation.**

A description has been added to the table. A formal specification of the full empirical model too.

**There is no discussion of how statistical inference is conducted. Should we worry about correlation of unobservable factors over time? If so, clustering of standard errors at some (appropriate) level would seem important. But there is no discussion of this in the paper.**

Clustered standard errors have been computed now at the formation level (i.e. the justices deciding cases with the same justices over time).

**In Section 5, the presentation and discussion of the results is very confusing and unfocused. As a reader, I simply could not tell what variable(s) in Table 5 I should be looking at for purposes of ascertaining the validity of specific hypotheses.**

The presentation as well as the discussion has been changed.

**What is reported in Table 5? I presume they logit coefficients. But this is not made clear. Moreover, in a logit, coefficients are uninformative of the magnitude of the effects, which would be important to grasp.**

We exponentiated the logit coefficients and we have rewritten the whole discussion of results in terms of percentage change of odds.

**What exactly is the conceptual purpose of including interaction effects in the model, as shown in Table 5? P. 25 does not offer an answer.**

The now section 2.2.1 does and 2.3.1. in the old manuscript did offer an answer. We now made it clearer in the discussion of the results.

## Theory

**In the exposition of theory, there is unusually heavy emphasis on the so-called identification-disagreement model proposed by Wittig (2016). I should note that I have done work on judicial dissent, but this is the first time I have come across Wittig’s theory. This is not to say that Wittig’s work is irrelevant, but I do not see a clear reason why so much emphasis is placed on it in the paper – in particular in light of the fact that the empirical results in Section 5 do not seem to find strong support in favor of Wittig’s arguments.**

With all respect to the reviewer, the fact that they are not aware of the book does not undermine Wittig's study in any way. The book presents a full-fledged theory of dissenting behavior in the civil law context that is more than just adoption of the general accounts (attitudinal, strategic...) like in the common context. That makes it immediately more appropriate for the CCC than all the SCOTUS based theories and studies.

Legal scholarship does not discard the strategic or attitudinal accounts just because at SCOTUS, point estimation of dissenting opinion corresponds to judges' attitudes (Clark and Lauderdale 2010) and at another court, it does not (Hanretty 2015). In a similar vein, we do not see a reason to discard it if our results partially do not fit her theory, while her results from the GFCC do fit it.

Lastly, the theoretical section comes logically before our empirical results. At the time of exposing the theory, Wittig's theory seems to be appropriate for the CCC. Only after conducting the empirics, we can say something about its appropriateness.

In any case, since the whole theoretical part has been made considerably more concise, the emphasis placed on Wittig should now be weaker.

## Exposition

**The paper is very long-winded, especially given how short and sketchy the actual empirical execution is. To illustrate this point, in the Introduction, it takes the authors more than three full pages to get to the point of articulating for the reader what the paper is actually about.**

The introduction as well as the literature review has been made considerably more concise. The length is almost halved.

**The authors engage in unnecessary digressions. On p. 3, "The Smekal et al. (2021) book goes so far to coin the first coalition as a more left-leaning and the second as a more right-leaning. We are not convinced by this label." Why is this a relevant comment in light of the present paper's aims? If it is relevant, why are you not convinced?**

The digressions have been left out completely.

**Similarly, in discussing work by Garoupa and Grajzl (2020), the authors argue that the original authors' choices lead to "selection bias that accentuates rather than attenuates the main effect of interest". I again do not see how and why this discussion is relevant to the present paper's pursuits. More generally, since I happen to know this particular contribution, I do not think that the authors of the present paper actually understood the article they are criticizing. In such situations, it is probably better to refrain from saying too much.**

We have refrained from the comment.

**The paper would greatly benefit from proof-reading and language editing by a native English speaker. There are many instances of awkward phrasing in the manuscript. For example, see the following selection of statements appearing only on p. 2:**

**“The judges enjoy a life time tenure, the nomination procedure enjoy attention and is conducted in a more political manner...”;**

**“To the extent that other influences other than simple black letter law...”;**

**“...Epstein, Landes, and Posner (2011) have come up with a theoretical model of dissenting behavior and dissent aversion and they tested to what extent do US judges behave strategically when attaching separate opinions.”**

**On p. 9, “Both of research choices are inductive to selection bias that accentuates rather than attenuates the main effect of interest in the former case”.**

**None of the above is correct English. There are many more instances of poor use of language in the draft.**

We have proofread the paper once more.

**There are instances of unusual citations, e.g., p. 5: “a potential dissenter balances the costs and benefits of issuing a dissenting opinion.” (Garoupa and Botelho 2022; Garoupa, Salamero-Teixidó, and Segura 2022)”.**

## Reviewer #2

**This paper examines the context of the judicial dissenting opinions at the Czech Constitutional Court and argues against the existence of a strong norm of consensus operating the court. To this end, the authors assemble a dataset of cases the constitutional court and deploy a series of simple limited dependent variable-based specification to bolster their claims. The centerpiece of the scholarly analysis is an innate critique of Garoupa and Grajzl's pivotal comparative study of Slovenia and Croatia where no evidence to support the effect of judges background. Consistent with the theoretical narrative and empirical operationalization, Garoupa-Grajzl study narrowed down the cases to the sample which included a political petitioner and deployed a battery of control variable that might pose a source of omitted variable bias. This research spends a great of deal criticizing this study on the grounds of selection and attenuation bias which to me is very unclear, hyperbolic at best and misleading for the most part.**

While we have so far attempted to take every comment seriously and honestly incorporate it, we find it difficult with reviewer 2's suggestions. While at no point would we suggest that our manuscript was anywhere near being flawless, it is a bit of an overstatement to claim that barely a half of a paragraph of 20+ pages long paper is "the centerpiece of scholarly analysis". But maybe we were simply taught different language than reviewer 2 and we completely misunderstand the term "centerpiece". Or reviewer #2 is overexaggerating on purpose, which seems even worse.

Moreover, we would expect constructive feedback from a peer review process. We are aware that we are working with limited data and limited specifications. After all, we are working with observational data in a very static environment that does not lend itself to naturally occurring experiments. Despite that we have tried our best to work within the constraints we face. If the reviewer 2 indeed believes that the series of specifications of independent (we assume that's what reviewer 2 had in mind and not dependent variables) variables was limited, they could have at least been more specific or even suggested better practices (as the first and third reviewer did). At no point in the manuscript do we pretend to make causal claims about the features of the CCC and we have been 100 % transparent regarding about it.

Moreover, while we omitted the critique from the manuscript, we explain the critique in greater detail here in an attempt to lead an honest academic discussion without any personal barbs. The critique is twofold.

Firstly, and more fundamentally, while we are aware that the authors find a quasi-DiD design with two countries that up to a certain point followed a similar trend and then diverged regarding the political fragmentation, we are still convinced that there is either no justification for narrowing the sample only to the cases including a political actor (p. 5) or the conclusion that the authors reach is misleading. The authors conclude: "we have then argued that, within any given legal tradition, greater party fragmentation should increase incentives of the justices to engage in dissenting behavior." The conclusion does not take into account that this is true only for the political cases – in those, there is indeed no doubt given the smart research design that all things equal, higher political fragmentation affects dissenting behavior. However, what is missing is the information that this conclusion does not necessarily extent to other cases. What's more, it may be theoretically expected that the effect would be lower in run-of-the-mill cases as it is hard to see how political fragmentation would affect, say, fair trial cases. The authors even admit to this: "The additional criterion to investigate only cases involving political actors is predicated on a tenable premise that the role of political dynamics for judicial dissent, if any, is likely **especially important in the corresponding deliberations.**" (p. 5). If it is not important in deliberation in other cases, then the conclusion shouldn't be anywhere near as broad. Therefore, there is positive selection bias in play.

Secondly, including “a battery of control variables” is simply not sufficient at best, it increases the estimated standard errors of the estimates at worst (Wooldridge 2019, 92–95). There is no discussion regarding the potential confounding potential of the “battery of control variables” in the (Garoupa and Grajzl 2020) paper. How is for example length of a decision or number of justices correlated with political fragmentation?

In any case, I (the lead author) would happily engage in a debate outside the review for our paper. If the editors are OK with that, the anonymous reviewer 2 can reach out to the lead author after the review process is done.

**Moreover, the authors never write down a formal model to be tested and rush through the methodological section in a brief digression consistent of ad hoc variable description. Any standard specification of case-outcome-decision level would have to include at least time-fixed effects that absorb common judicial technology shocks into the model as well as case- or judge-fixed effects that control for level of idiosyncracies. None of the unobserved effects appears into the model which questions the validity especially in the light of the fact that the model specification is assumed to be complete which is highly doubtful. In addition, standard errors are incorrectly estimated given the presence of the interaction term in the model specification. Norton-Ai-Wang derived asymptotically valid standard errors for limited dependent variable models in the presence of many possible interaction terms which the authors should heed without hesitation. There also appears to be very little discussion of results. I expected a discussion where results are linked to the previous findings in the literature but to no avail.**

On the one hand, we very much appreciate the reviewer #2's remarks and we have now included clustered standard errors, a formal model a clearer identification of the model. All in all, some of the reviewer #2's remarks have helped the article become way better and we are by any means not claiming that the article has been perfect, quite on the contrary.

On the other hand, we are not sure whether reviewer #2 had not missed some key parts of the text. Take for example the following statement: “Any standard specification of case-outcome-decision level would have to include at least time-fixed effects that absorb common judicial technology shocks into the model as well as case- or judge-fixed effects that control for level of idiosyncracies.”

First, the model is not specified at a case-outcome-decision level but rather on a separate-opinion-level, which is explained in sections 4.2.1. and 4.3. Therefore, the statement starts off with a wrong premise. Secondly, regarding the time FE, those were included under the term variable as had been explained in the section 4.3. In the revised manuscript, we have included year FE instead of term FE. Thirdly, as we explained in

section 4.3. in detail, we included the panel fixed effects to capture the “level of idiosyncrasies” at the logical clustering variable – the panel. Lastly, in our revised model, we include even the judge fixed effects at the cost of them absorbing the unit-specific time-invariant profession of the judges. To alleviate this issue, we fitted two models to obtain the estimates for the effects of the professional background of judges.

## Reviewer #3

### Institutional Setting:

**At page 14, the authors mention the “special disciplinary chamber”. Could you please provide more information on this?**

We provide further information here as elaborating on it in the article would be unnecessary as it has no relevance for the dissenting behavior judges. Basically, as the CCC is an apex court within the Czech judiciary, the argument goes, that there is no one that would carry out disciplinary proceedings against the CCC judges. Therefore, the disciplinary proceedings are held by a 5-member disciplinary chamber. The “ad hoc” disciplinary chamber goes into a session only on a disciplinary proposal of the President of the CCC that then gets discussed in the plenary session. If the plenum does not reject the proposal, the disciplinary proceedings proceed to the 5-member disciplinary chamber that gets established during these proceedings (i.e., if the proposal does not get rejected, the plenary session elects 5 members out of itself). The only possible “punishment” of the proceedings is a reproach/reprehension against the judge for the deeds that they’d committed. There is no monetary, no functional and alike punishment. The relevant §§ 132-144 are to be found here

[https://www.usoud.cz/fileadmin/user\\_upload/ustavni\\_soud/www/Pravni\\_uprava/AJ/Constitutional\\_court\\_act\\_182\\_1993.pdf](https://www.usoud.cz/fileadmin/user_upload/ustavni_soud/www/Pravni_uprava/AJ/Constitutional_court_act_182_1993.pdf)

### Data & Explanatory Variables

**1. The *time in office* variable is computed as the number of months left till the termination of the judge’s mandate at the date of the decision. This is said to account for both the collegiality costs hypothesis and the profession hypothesis. One could question if this is actually the case with respect to the latter hypothesis. The effect of judges’ former profession is expected to be stronger the closer a judge is to the beginning of her office. A judge’s mandate last 10 years. However, re-election is possible with no restrictions. Then, if a judge is not at her first mandate, the *time in office* (as constructed) would not properly capture the time since she left her previous profession. To properly account for the profession hypothesis, the authors should include the number of months since the beginning of the first mandate of a judge (unless judges in the dataset are never re-elected).**



We corrected that as we missed this piece of puzzle. Now the time in office variable is calculated as the number of months from the beginning of the first term of the judge, notwithstanding how many terms they have served.

**Norm-identification is proxied looking at judges' last profession before entering the CCC. This is done as, according to the authors, the vast majority of judges would retire after their terms. Hence, career concerns would not be at play. I believe it is still worth to check if such concerns are or are not at play. Rather than reconstruct the judges' profession after their term, the authors could easily control for some judges' characteristics commonly used to capture the effect of possible career concerns such as age at the time of the appointment, age at the time of the decision, gender (La Pellegrina et al., 2017).**

We are not sure whether we follow this comment. The norm-identification is proxied by the profession because we can theoretically expect it to vary across different professions: judges coming from a judiciary had already been socialized into the norms of the judiciary whereas lawyers had not been. Therefore, the variable is included in the model because we are interested in measuring its effect, not because it's supposed to control for a potential confounder as the reviewer suggests ("the authors could easily control for"). We do not see a reason to account for the age of the CCC judges, neither as a variable of interest nor as a confounder – could the reviewer maybe elaborate on that?

Potentially, gender could be a confounding variable. From our previous research we know that women are disproportionally represented at the lower courts, whereas courts occupy the positions at the higher and apex courts (for a more detailed discussion from a different set of authors see Urbáníková, Havelková, and Kosař 2023).

#### **Section 4.2.2.1 on mixed coalition: how coalitions have been identified?**

The coalitions have been identified based on the literature we build on.

## **Empirical Analysis & Results**

**At page 24, the authors claim that the court composition is rather stable within the two observed terms. Still, they construct the variable *term* depending on the number of judges (who sit in a given panel) coming from a same term, so there is some variability in the court's composition. How much?**

The variable *term* was supposed to capture the shock in the form of complete personal overhaul that happens roughly every ten years. The CCC was founded in 1993, the term of a judge lasts for 10 years, and there has been only one premature retirement. Therefore all 15 but 1 judges finish their terms within a couple of months apart from each other. Therefore, instead of including year FEs, we'd included term FEs in the form of a dummy for the CCC "generation" to absorb this shock – the complete overhaul. In

the end, we opted for year FEs as they would still absorb the complete personal overhaul of the CCC + other potential time-variant constant across entities variables.

**Related to my previous comment, year FE and term FE capture different aspects. At least as a robustness check, the authors should include year FE.**

See our answer to the previous comment.

**Why to test the fourth hypothesis the model is run only on 3-member panel decisions belonging to the third term (i.e, 2014-2023)?**

Because that's the term for which the theory was generated, i.e. it concerns a specific term of the CCC for which it applies. The voting bloc/coalition theory was developed only for the 3rd term of the CCC (which is currently entering its fourth term/decade).

**The authors show and discuss the results of the logit specification without commenting on the magnitude of the effects. It would be interesting and relevant to also comment on the effects' size.**

We have exponentiated the estimates and discuss their magnitude in terms of change of the odds of the dependent variable.

## Minor comments

**Tables and figures should be complemented with proper footnotes as to make them readily understandable to readers.**

The footnotes have been added and edited so that they are more proper.

**Rather than individual tables for some selected variables (e.g., Tables 3 and 4), the authors should provide a comprehensive table reporting the descriptive statistics for all the variables included in their models.**

This has been redone. Now there are comprehensive tables divided by the type of variables (discrete categorical vs continuous variables).

**At pag.19, the authors explain how they construct the variable capturing the salience of a case. In particular, they specify the subject matters identified as controversial. It would be good to know what is then left out as uncontroversial.**

We added a couple of examples of uncontroversial topics.

## Editor

**Additionally, there should be a covariate for partisanship, ideally the standard ideological distance between the rapporteur and the voting justice (resulting, for example, from ideal point estimation). Alternatively, party affiliation of the appointer or some equivalent measure.**

Here we present a lengthy answer to the editor's suggestion. We summarize the ensuing discussion in the Identification section of our article. Unfortunately, we believe it to be impossible theoretically as well as practically to control for partisanship.

Theoretically, we are doubtful that partisanship presents a potential confounding potential. The CCC judges are nominated and appointed by the President of the Republic, who traditionally plays an apolitical role in the Czech political system. The CCC judges are not political candidates of Czech political parties [as in the case of the US or of Spain (Hanretty 2012)]. (Šipulová 2018) has argued that the CCC has been relative spared of political influence. If anything, the role of political preferences would be much more complex than a simple uni-dimensional conservative/liberal or left-right split and would vary across areas of law as the point estimation techniques usually place the justices on an unidimensional space.

For example, one could argue that different aspects of political preferences come into play into cases concerning fundamental human rights (therein a traditional conservative/progressive split would be relevant), procedural issues (therein a preference regarding the role of the judiciary/the CCC – i.e. for example the degree of judicial activism and fairness would be important), or issues of taxes (therein a traditional left-right wing split would come into play. While in the US, most of such differences may be mapped into a bipolar unidimensional space as the whole political system is divided into two parties, in Czechia the political landscape consists of many small parties shaped around different issues and different preferences.

Moreover, theoretically, only variables that cause omitted variable biases as a result of confounding, i.e. being correlated both with the X of interest as well of Y of interest, are to be controlled for. While it is always possible to “include a battery of control variables” (as Reviewer #2 writes), the price the researcher pays is the increased variance of the parameter estimates as the variance of estimates of parameters of the X covariates increases with the degree of correlation of x with other covariates (Wooldridge 2019) – an effect known as the bias variance tradeoff. Theoretically, we can reasonably expect political preferences to be correlated with the decision whether to dissent or not, the Y. Are political preferences correlated with workload, with the career choices of lawyers, with the time of their appointment as a CCC judge, with number of citations that a case contains? We are doubtful, especially given that we do not expect the role of political preferences to be strong in the first place.

Practically, we can neither directly estimate it using the voting behavior of justices (Segal and Spaeth 2002), because the votes of the CCC judges are not published, nor can we rely on the separate opinions themselves to produce point estimates of the political positions of judges. We are very well aware of the research on employment of IRT models to estimate the political position of judges (Hanretty 2012; Varol, Dalla Pellegrina, and Garoupa 2017). However, at the CCC a vote against the majority

resolution does not necessitate a separate opinion, therefore, there is no clear link between a separate opinion and a vote of the judge. What's more, estimating political preferences via dissenting behavior of the judges would introduce endogeneity into our model as it would be determined and measured through the dependent variable, the dissenting behavior.

Finally, one of the authors of the articles permits themselves to engage with the two articles and would be happy to hear a response from the editor/anyone interested in an academic discussion.

As I wrote above, I am not convinced by simply estimating the position of a judge based on their dissenting behavior, inasmuch the assumption that dissent = vote against a certain decision, lack of dissent = vote for the decision does not hold. If the assumption does not hold and if the "propensity"<sup>1</sup> to dissent given a vote against a majority systematically differs among judges, then the estimates of the political position simply based on the dissenting behavior must be logically biased.

Based on the interviews we conducted with the CCC judges, we know that the propensity to dissent varies strongly depending, for example, on the judges' perception of the role of the judiciary (i.e. whether courts should be viewed as unanimous bodies otherwise their legitimacy would be damaged or whether, on the contrary, showing disagreement increases the legitimacy in public's eyes).

Out of the two studies (Hanretty 2012; Varol, Dalla Pellegrina, and Garoupa 2017), this issue gets discussed only once. Hanretty writes:

- (1) Regarding Spain: "Finally, both courts follow the practice of the German Federal Constitutional Court in permitting concurring and dissenting opinions. Article 90(2) of the Spanish Law on the Constitutional Tribunal allows judges on the tribunal to issue a 'particular vote' (voto particular) on a point of disagreement (su opinión discrepante), on the condition that the issue has been raised during the course of the tribunal's deliberation. Such opinions are required to be published alongside the majority opinion of the court." If indeed the Spanish court is modelled after the GFCC, then the dissent is merely a right of the judge, not an obligation, therefore the assumption does not hold.
- (2) Regarding Portugal: "Article 42 of the Portuguese Law on the Constitutional Tribunal states that decisions shall be taken 'on the majority vote of members present', and that judges 'have the right to table their reasons for a dissenting vote' (court's translation), though in practice the court has noted whether a particular justice voted in the minority (i.e., whether she or he was vencido – literally, 'defeated'), **even where she or he has not tabled reasons for a**

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<sup>1</sup> By that I do not refer to the dissenting rate by specific judges. By that I understand the likelihood that a given judge attaches a separate opinion if they voted against majority.

**dissenting vote.**” Here, it becomes clear that the Portuguese CT notes that a judge voted in a minority even if they did not attach a separate opinion. (Hanretty 2012, 5)

- (3) The (Varol, Dalla Pellegrina, and Garoupa 2017) is clearly aware of this distinction without ever mentioning it explicitly. At points, it uses almost intentionally unclear language to muddy up this distinction: “Our dataset also includes **individual votes**. (...) Table 1 summarizes the statistics concerning individual **votes and dissents**, before and after the 2010 reforms. Overall, there is a relatively **low percentage of dissents** (around 11% in total).” But to the extent I understand the table correctly, one column is the total number of votes a judge casted and another column is the number of separate opinions out of this total number. However, when discussing the model “As for the IRT implementation, we assume that  $x_{ijt}$  is the vote of each judge  $j$  ( $j = 1, \dots, J$ ) for decision  $i$  ( $i = 1, \dots, N$ ) at time of decision  $t$  ( $t = 1, \dots, T$ ).”, wherein the  $x_{ijt}$  vote of the judge = 0, when they dissented.

Inasmuch a vote against the majority cannot be equated with a dissent and inasmuch the propensity to dissent is not distributed randomly/equally among the judges (i.e. certain sub-group is likely to dissent more or less given their vote against the majority), I don’t see how using the dissents can be used to estimate the political position of judges. If, as is the case with the CCC, judges that perceive the legitimacy of the judiciary to hinge on the unanimity of the court are less likely to dissent whenever they voted against the majority than the rest, then, I argue, the presence of a dissent used as a proxy for negative vote cannot be employed to estimate political positions of judges using the IRT model. And I also do not see how employing the anchors of the two extreme judges can alleviate my concern.