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Media Content and Political Behavior in Observational Research: A Critical Assessment

ZOLTÁN FAZEKAS AND ERIK GAHNER LARSEN*

This article discusses possible issues of how media content and exposure were linked in previous research. It argues that the original conclusions of the article 'Who's Afraid of Conflict? The Mobilizing Effect of Conflict Framing in Campaign News' do not hold due to the chosen operationalization. It also demonstrates that using the proposed methodology, both exposure to conflictual and non-conflictual news yield the same substantive conclusion. In addition to re-evaluating the role of conflict, the article contributes to the discussion on how to integrate media and individual-level measures in the study of electoral behavior.

The media informs and influences individual political decisions.[1] Rather than drawing inferences based on the amount of general media consumption,[2] the goal in recent research concerning media effects on political behavior is to jointly assess the effects of specific media content consumed by citizens at a specific rate on their political behavior.[3] Hence, a growing body of research uses composite indicators to link analyses of media content to individual-level political attitudes and behaviors.[4] In addition to relying on the validity of self-reported news exposure,[5] the additional assumptions imply that information about specific media content (1) provides better measures of citizens' news exposure, which (2) provides a better estimate of the impact of media content on political behavior.

The methodological challenge is to link media consumption and behavior in such a manner that all information is preserved and content features are accurately integrated with the media consumption measurement. In many such analyses, media content features are measured as the proportion of news items that falls into a specific coding category (for example, is it an EU-related topic, does it contain a conflict frame, does it contain a strategic frame, etc.). Media content information is then combined with the individual-level frequency of exposure, resulting in a measurement of the extent of exposure to a particular type of news rather than general media consumption.

Thus, the ambition of these media effects studies is to make inferences about how specific media content features affect political behavior. To do so, it is crucial to separate exposure to specific media content from general media exposure. If the operationalization of specific media content consumption is merely a proxy for general media exposure, we are making

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[1] Chong and Druckman 2007; Ladd and Lenz 2009; Newton 1999.

[2] Bartels 1993.

[3] Barabas and Jerit 2009; Barabas et al. 2014; Dilliplane 2014.

[4] Schuck, Boomgaarden, and Vreese 2013; Schuck, Vliegenthart, and Vreese 2014; Vreese and Semetko 2002.

[5] Dilliplane, Goldman, and Mutz 2013; LaCour and Vavreck 2014; Prior 2009.

unsubstantiated conclusions about the effects of specific media consumption on political outcomes. In this article we address this problem, which has so far been given little attention.

We do so by examining the validity of the main results of an article recently published by Schuck, Vliegenthart and de Vreese (abbreviated SVdV from now on)[6] on the impact of news content framed in a conflictual manner on the propensity to vote. Their article is a state-of-the art analysis of the impact of specific news consumption on political behavior, and we sympathize with the authors' research ambitions. It combines media information that was content coded for the whole 2009 European Parliament (EP) election campaign period and links this information to the frequency of news consumption. It then draws conclusions that are heavily based on the content features interacting with specific news consumption rather than the effects associated with general news consumption. Since it is a multi-country analysis, its results are even more relevant: the validity of the inferences is increased because the study accounts for systematic cross-country variation in individual behavior as well as media landscape features in each country.

We use two approaches to show that SVdV's conclusion – that exposure to news conflict frames (rather than mere news exposure) in the 2009 EP elections affects voter turnout – does not hold. First, we show that the effects of specific news exposure (conflict framing) and general news exposure on voter turnout are impossible to distinguish. Secondly, through an extension, we show that the effects of exposure to conflict and non-conflict news frames are indistinguishable. In short, SVdV's conclusions do not hold due to fundamental research design issues and the operationalization they employed. The heterogeneous effect of exposure to different news content features is crucial in such research, and also in their article, as SVdV argue that exposure to specific content – that is, the news presented in a conflictual manner – and not simply the level of news consumption, affects turnout. This differentiation also arbitrates between previous contradictory findings on the role of the media. We suggest that scholars in future studies explicitly address whether it is general self-reported news exposure that correlates with the outcome of interest rather than specific news content exposure, and pursue better designs to obtain reliable estimates of the effects of specific media coverage on political behavior.

The methodological issues discussed in this article are of particular relevance for studies interested in the impact of news frames. We also point to methodological difficulties and challenges for observational media studies in general, pertaining to the joint use of individual news consumption rate and media content as key predictors of political attitudes and behaviors.

CONFLICTUAL EU NEWS: AN EMPIRICAL ILLUSTRATION

In the article 'Who's Afraid of Conflict? The Mobilizing Effect of Conflict Framing in Campaign News', SVdV theorize that exposure to EU- or EP-related news focusing on conflict will increase the likelihood of turning out in the EP elections, and that this effect is conditional on the general information environment – that is, the degree of positive vs. negative polity evaluations of the EU in the news.

SVdV use survey and media content data from twenty-one EU member states. The individual-level data contain turnout intention before the 2009 EP elections and self-reported turnout from a post-election survey. Media content data stems from the Providing an Infrastructure for Research on Electoral Democracy in the European Union project.[7] For both

[6] Schuck, Vliegenthart, and Vreese 2014.
[7] Schuck et al. 2010.

expectations put forward, SVdV report positive findings. We will show below that their approach is insufficient to derive such conclusions.

During our step-by-step reproduction and replication of SVdV's presented empirical analysis, we discovered data and methods-related issues that required corrections.[8] On the one hand, these corrections help with the validity, transparency and interpretation of the results. On the other hand, they confirm that our broader concerns are not simply rooted in the particular data or analysis reported in the original article.

HOW 'SPECIFIC' IS EXPOSURE TO THE SPECIFIC TYPE OF NEWS?

While *News Exposure* is a simple summation of the weekly frequency with which an individual follows the specific media outlets (from 0–7 days, for five outlets, on average, per country), incorporating content-related information requires additional adjustment. The core operationalization of *News Conflict* combines both individual and content-related features, and SVdV[9] use the following formula to calculate this individual-level predictor:

$$\begin{aligned} \textit{News Conflict} = {} & [(\text{exposure medium } 1 \times \text{conflict frame index medium } 1) \\ & + (\ldots) + (\text{exposure medium } n \times \text{conflict frame index medium } n)] \end{aligned}$$

From the media content data, each outlet has a conflict frame index. This is the proportion of news items with a conflict frame out of the total news items during the campaign that were about the EU/EP elections.

The first step carried out by SVdV includes a hierarchical model, with observations nested in countries, in which the probability to turn out in the EP elections is a function of both *News Exposure* and *News Conflict*. They also control for other variables. Next SVdV use models including only *News Conflict*: '[…] however, the model in Table 2 includes conflict news [but not mere news exposure] in light of previous findings'.[10] This may be because *News Exposure* had a substantively small effect, a negative effect and/or that adding *News Conflict* to the full model (in Table 1, SVdV) resulted in a significant improvement in model fit. We assess these decisions below, focusing on the substantive features.[11] In the subsequent steps of the analysis, SVdV evaluate whether the effect of *News Conflict* on turnout varies across countries, and whether that variation can be explained by country-level features of the media content, modeled as cross-level interaction between polity evaluations and *News Conflict*.

The authors note that there is a substantial correlation between *News Conflict* and *News Exposure*, $r = 0.89$. Ideally, the inclusion of *News Exposure* would be required to tap into the specific effects of conflict. Using models with only one media predictor appears to be an

[8] In short, the analysis and results are based on twenty countries instead of the intended and reported twenty-one countries. After identifying a data-related issue, with the necessary adjustments we reach a sample size similar to that reported by Schuck, Boomgaarden, and de Vreese (2013) on the same dataset. The use of the incomplete data has direct implications for SVdV's support for Hypothesis 2: it fails to reach statistical significance after corrections. First, we see this as a minor issue, as substantial support for Hypothesis 2 was absent in the first place. Secondly, coding errors can happen, and the more general claims we make in this article are not strictly related to these deficiencies. However, we urge those interested in the role of the polity evaluation at a macro level, or general considerations regarding cross-level variation in hierarchical models, to consult the detailed reproduction and corrections described in Online Appendix A.

[9] Schuck, Vliegenthart, and Vreese 2014, 9, footnote 45.

[10] Schuck, Vliegenthart, and Vreese 2014, 11.

[11] An analysis of the 'model-fit improvement argument' is presented in Online Appendix B.

TABLE 1 *Multivariate Model Results:* News Exposure *vs.* News Conflict

	News Exposure only	News Conflict only	News Exposure interaction	News Conflict interaction
News Exposure	0.038**	–	0.051*	–
	(0.018)		(0.026)	
News Conflict	–	0.048***	–	0.061**
	–	(0.018)		(0.025)
News Exposure × Polity evaluations	–	–	0.293	–
			(0.224)	
News Conflict × Polity evaluations	–	–	–	0.313
				(0.212)
Polity evaluations	0.167	0.170	0.064	0.065
	(1.148)	(1.148)	(1.157)	(1.157)
All other controls	Yes	Yes	Yes	Yes
N	22,792	22,792	22,792	22,792
Countries	21	21	21	21
Log likelihood	– 11,571.350	– 11,570.050	– 11,567.650	– 11,567.090
AIC	23,166.700	23,164.100	23,163.290	23,162.170

Note: $^*p < 0.1$; $^{**}p < 0.05$; $^{***}p < 0.01$ (two-tailed).

empirical necessity, given the multicollinearity issue. Thus the decision of which predictor to use, and how it relates substantively to the excluded operationalization, becomes the most important step of the analysis – and basing this decision solely on model results with serious multicollinearity issues can be problematic.

SVdV do not discuss in detail the properties of the scenario with both predictors included in one model, but this model (Model 1 in SVdV) raises several interpretation difficulties. Within a single country, two individuals can have the same summed *News Exposure* score with different per-outlet consumption frequency, and this will translate into potential differences in their *News Conflict* scores. This difference is a function of both the variation in outlet conflict scores and how much their media consumption vector differs. The strict interpretation of coefficients would imply the classic formulation of: 'everything else held constant', the effect of *News Exposure* or *News Conflict* is the associated β coefficient. First, we know – given the correlation between the two predictors – that in most cases, *News Conflict* and *News Exposure* will change together. *News Exposure* has a small negative effect – we discuss below in detail why the magnitude is problematic – when *News Conflict* is held constant. This would entail at least some variation in *News Exposure* at each level of the *News Conflict*. In 65 per cent of the cases in the corrected dataset, one *News Conflict* value within a country is associated with one *News Exposure* value. More strikingly, in 97 per cent of the cases, the number of unique values of *News Exposure* for a fixed *News Conflict* value ranges between 1 and 3, which calls into question how meaningful the *News Exposure* coefficient (or that of *News Conflict*, for that matter) can be in the initial model reported by SVdV.

To address how serious the multicollinearity issue is, we estimated and report correlations for each country separately. As displayed in Panel a of Figure 1, these correlations are close to 1.0 in most countries, except Lithuania, which has the lowest degree of country-level conflict. The second-lowest correlation is for Belgium, but as the value is for country and not political system (Walloon vs. Flanders), this comes as no surprise. In short, this suggests that *News Conflict* is merely a rescaling of the *News Exposure* variable.

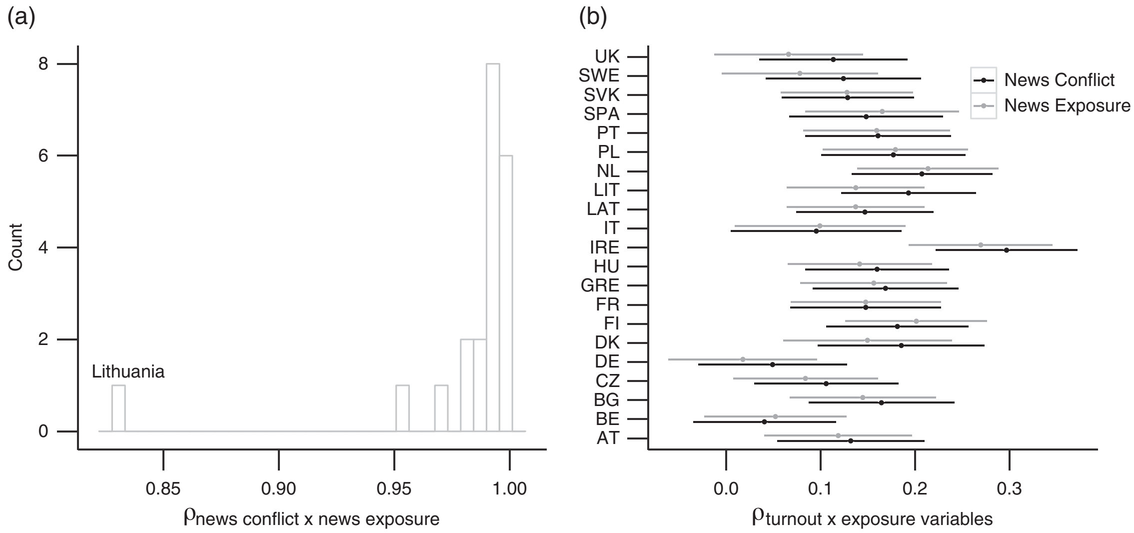

Fig. 1. News Exposure *and* News Conflict, *correlations*
Note: Panel (a): Country-level Pearson's correlations between *News Conflict* and *News Exposure*, twenty-one countries, corrected measures. Panel (b): Country-level polyserial correlations between *News Conflict* and turnout (black) and *News Exposure* and turnout (gray), twenty-one countries, corrected measures. Error bars: 95% confidence intervals.

The 'weighted' index nature of *News Conflict* results in substantially different ranges for *News Exposure* and *News Conflict*, but SVdV present, discuss and base their decisions on unstandardized coefficients. We show that when standardized (and hence comparable) coefficients are used, the dismissal of *News Exposure* in favor of *News Conflict* becomes less straightforward. The first indication that this choice of reporting is problematic is displayed in Panel b of Figure 1. When looking at the zero-order correlations in each country between turnout and the two exposure variables, we find – unsurprisingly, as there is close-to-perfect collinearity in most countries – that the bivariate relationships are very similar.

For the multivariate setting, we recoded the two predictors to account for the difference in scales, which convinced us that the decision made by SVdV on the basis of the unstandardized coefficients is erroneous. We group-mean centered the two predictors[12] and divided them by their standard deviation. In Table 1 we report four models with the newly recoded variables.

All these models indicate that, when modeled separately, both *News Exposure* and *News Conflict* have very similar influences on the outcome variable. When we assess the standardized coefficients and their standard errors, we can see that they overlap. Furthermore, the aim of Figure 1, Panel B is to stipulate this at the bivariate level. Accordingly, the major difference in the magnitude of the effects was a simple artifact of the reporting chosen by the authors.[13] When the cross-level interaction is modeled, *News Exposure* is not statistically significant at the $p < 0.05$ level, but absence of evidence is not evidence of absence, and using the thresholds proposed by SVdV, this effect is still statistically significant.

If the model comparisons or p-values larger than 0.05 leave room for skepticism, we can always look at substantive differences and implications. Figure 2 presents the slopes for Spain,

[12] As the goal is to get *cleaner* estimates for the slopes that vary across groups, we use group-mean centering following the recommendations in the literature (Enders and Tofighi 2007).

[13] The results from a model including both exposure variables rescaled (as in Table 1, original article) are: *News Exposure* $\beta = -0.144$ (SE = 0.074), *News Conflict* $\beta = 0.188$ (SE = 0.075).

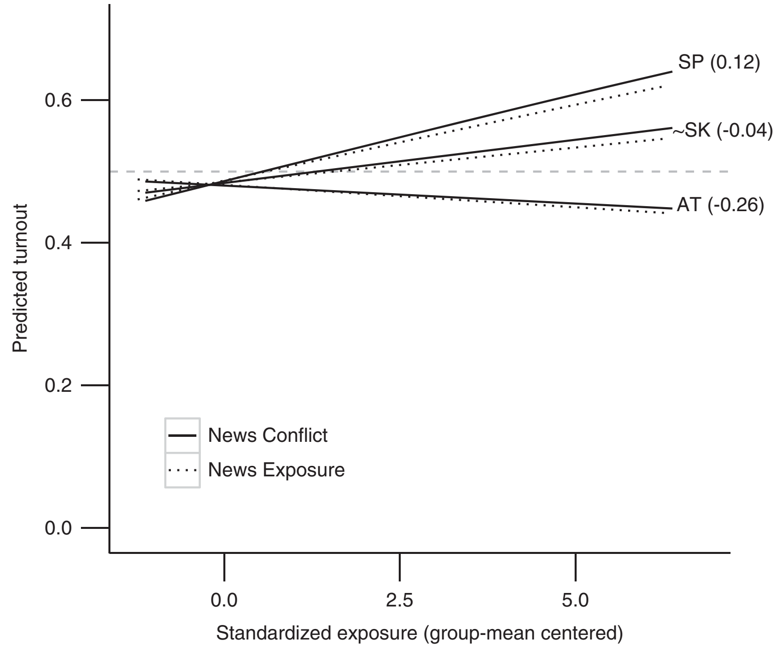

Fig. 2. News Conflict *vs.* News Exposure*, cross-level interaction*
Note: predicted turnout as a function of *News Conflict* (solid lines, based on *News Conflict* only cross-level interaction model, Table 2) and *News Exposure* (dotted lines, based on *News Conflict* only cross-level interaction model, Table 2) for different polity evaluation levels (SP = Spain, SK = Slovakia, AT = Austria). All other variables fixed at values described in the text and by SVdV, predictions based on fixed effects only. Predictors were group-mean centered and standardized (one SD, within country).

Slovakia and Austria (as reported by SVdV in Figure 2) for *News Conflict* and *News Exposure*, estimated in separate models, using the full sample (twenty-one countries) and corrected *News Conflict* (see Online Appendix A). We fail to see support for their conclusion that exposure to conflict, instead of mere news exposure, increases the likelihood of participating in the EP elections.

THE MOBILIZING EFFECT OF NON-CONFLICTUAL NEWS: AN ILLUSTRATION

While the high correlation between specific and general news exposure shows the problem related to estimating the impact of specific content on political behavior, this does not necessarily mean that the proposed approach of linking media content features with individual exposure is necessarily wrong. However, we show through an extension of SVdV's work that the methodological contribution is unlikely to benefit the study of media effects on individual political behavior. Even if we assume that *News Conflict* measures both exposure and exposure to content, our example below – which focuses on non-conflictual news – is intended to show that substantively, we could easily formulate the same conclusions about two opposite contextual situations. We use this section to show that with such a proposed operationalization, even if we disregard all the discussion about multicollinearity or results reporting, we are losing most information in substantive terms that is about context (or media content). Hence, evaluating what news conflict operationalized as such actually measures is very problematic. In our reading of the claims made in the article, we should perhaps expect no relationship (or a negative relationship) when it comes to news non-conflict.

We use the original media data[14] to recreate the conflict scores at the outlet level and then merge it with the European Election Study 2009 individual-level data. Using SVdV's operationalization and the 2009 PIREDEU Media Study,[15] we calculate the outlet-specific conflict score, using all four content variables indicated by the authors. Note that these are dichotomous indicators, 0 for no conflict, 1 for conflict. Each outlet can be described by an average level of conflict in its EU reporting throughout the coding period. Similarly, each outlet can be described by the average ratio of non-conflictual news. The degree of *News Non-conflict* will be 1 − *News Conflict*. This means there should be a perfect negative correlation between these two outlet features. That is indeed the case in the Media Study, at both the outlet and country levels.[16]

Next, using the formula provided by SVdV and the EES data, we created the individual-level *News Conflict* variable, as well as a *News Non-conflict* variable, following the same logic, but multiplying outlet-specific exposure by the outlet-specific *News Non-conflict* score. The correlation between these two variables is 0.85 on the full sample, with country-specific correlation coefficients for these two variables and mere news exposure reported in the last three columns of Table 1, Online Appendix C. From the theoretically grounded perfect negative correlation between the two variables at the outlet level, we have ended up with an extremely strong positive correlation in the individual-level data. This is a joint effect of both the proposed method of linking the two levels together and the measurement of conflict framing at the media-item level.

While we do not have turnout intention measured in an earlier wave, we expect that the only implication is that the media coefficients will be larger than those reported in the previous section or the original study. However, how they compare to each other – both effects and variables – is independent of the turnout intention variable.[17] Otherwise, we used and coded all individual- and country-level variables employed in the original analysis, following the coding description from SVdV.[18]

In Table 2 we present the results of interest from three turnout models on the EES data that mimic SVdV's 'fixed-effects' model. Each model contains all individual- and country-level controls, with a varying intercept. Full model results are reported in Table 3, Online Appendix C. The row 'Exposure' contains the coefficients for the media variables, and each column head stipulates which exposure measure was used in the model. All three models indicate a positive effect of the exposure variables, independent of whether exposure is multiplied by conflict or non-conflict (see Online Appendix C for model formula and descriptive statistics). Again, this result is due to the fact that even after applying the proposed transformations, we are essentially looking at a simple news exposure measure. Furthermore, this also indicates that, given the coding at the news-item level, the proposed approach is unable to differentiate between qualitatively and quantitatively opposite media features; it supplies coefficients that are heavily conflated by mere exposure.

[14] Schuck et al. 2010.

[15] Schuck et al. 2010.

[16] Country-level values for both variables, which indicate that the conflict scores are in line with those reported by SVdV in Figure 1, are displayed in the first two columns of Table 1, Online Appendix C.

[17] The effect of group-mean centered *News Conflict* in the corrected twenty-one country SVdV dataset for the Table 2 'fixed-effects model' without turnout intention is $\beta = 0.181$, SE $= 0.017$. As seen below, the results for the full EES 2009 sample are comparable.

[18] We were unable to get polity evaluation scores identical to the SVdV data by simply following the coding description. However, the correlation between the measure included in this set of analyses and those used by SVdV is 0.98.

TABLE 2 *The Impact of Mere Exposure, Conflict and No Conflict, EES 2009*

	Turnout		
	Mere exposure	Conflict	No conflict
Exposure	0.259***	0.252***	0.261***
	(0.016)	(0.016)	(0.016)
Constant	–0.946***	–0.950***	– 0.948***
	(0.138)	(0.138)	(0.138)
All controls	Yes	Yes	Yes
N	25,841	25,841	25,841
Political systems	28	28	28
σ^2 country level	0.289	0.288	0.289
Log likelihood	– 13,502.110	– 13,507.600	– 13,499.410
AIC	27,026.220	27,037.190	27,020.810

Note: Best-fitting model based on model comparison: news non-conflict. *p < 0.1; **p < 0.05; ***p < 0.01 (two-tailed).

CONCLUSION

A decade ago, Slater argued that a weakness of using self-reported measures of news exposure is the 'uncertainty about the content of the media, but that this uncertainty can largely be removed when content analyses of those media are conducted in conjunction with the survey of exposure self-reports'.[19] Over the years, a large body of literature has used this approach with great optimism, employing different solutions for linking these sources of information. In this article we question fundamental – but thus far unaddressed – properties of one approach, showing the deficiencies resulting from important methodological decisions.

Theories of how the media presentation of political news can affect individual attitudes and behavior have become more fine grained over time. Testing these theories requires the measurement and operationalization to accurately capture such fine-grained differences. Summarizing rich content data using features that reflect the prevalence of some frames marks the start of the groundwork for testing content-related heterogeneity. However, as we have shown, relevant and meaningful differences at the content level can get lost through the attempted linking of these features with individual exposure measures. While an explicit focus on content is both desirable and assures that we put meaning behind media consumption, the methodological steps to maintain the added content-related features deserve future attention. Otherwise, the rich content information is lost, and the gap between the refined theory and its empirical test widens to an extent that the added value disappears.

This was the case in the article chosen for illustration purposes. We had to realize that with the available measurement and proposed method we do not get very far, which calls into question the substantive contribution of the original article. To be more precise, we get to the same place when we focus on the effects of exposure to conflict vs. the effects of exposure to non-conflict. Using SVdV's approach to integrating media and individual specific features, we are still basing most of our conclusions on mere media exposure. In substantive terms, we find that individuals with average media exposure are more likely to turn out to vote. This is hardly as impressive as being able to settle the cited debate over whether exposure to conflict has

[19] Slater 2004, 169.

positive or negative effects. Thus we are left with a situation in which media and individual characteristics are both important separately, but how they were linked to create a more meaningful measure backfires, losing the added value on both levels.

The broader methodological implications of the original piece are important for researchers in terms of how one should combine media content features and individual exposure to such content. However, the proposed approach comes with problems. As in most cases, there are no easy solutions. However, the first – and arguably the most important – step in such analyses should be an extensive discussion of the measurement and empirical properties of how the content information is merged with individual exposure. As presented above, many of the detailed checks on the correlation patterns and the detailed comparison of the substantive implications already flag the issues of the operationalization. Once this step is carried out, it is clear that the operationalization needs enhancement, or the limitations should be explicitly stipulated.

Overall, based on the work that we have carried out so far, there are straightforward quantifiable and serious issues whenever the outlet-level features are thought of in terms of proportion or composition. While not completely independent of the distribution of these outlet scores, this issue will be serious even in the best and very unlikely scenarios. When outlet-level features are coded differently, a joint estimation of the weighted and unweighted measures might be possible (further simulations would be needed to address this issue), but in that case, the researchers should discuss the characteristics of the weighted index in precise detail. A beneficial next step for future research is to extend the analysis to such cases in order to establish whether the composite measures are indeed able to capture the desired content-related features. Through these extensions and further reliability and validity assessments of the linking approaches, researchers can pave the road toward a unified methodological approach to incorporating, and then comparatively assessing, the role of content conditional on exposure – or the role of exposure, conditional on content – in observational media studies. We believe such analyses will also inform where and how data collection, coding and study designs can be improved to assure that we are testing the theories we intend to.

To conclude, news exposure is not an empty shell, and there are profound reasons to believe that specific content in the news media matters for political outcomes in predictable ways. In most cases, mere media exposure is of little interest by itself and is useful due to its function as a proxy for exposure to specific content, for example exposure to news as a proxy for exposure to political content. Hence, there are important reasons for scholars to examine how specific content – in experimental as well as observational research – affects citizens' political attitudes and behavior. For observational research, the challenge is to disentangle how specific content matters vis-à-vis other types of content.

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Supplementary Information for Media content and political behavior in observational research: A critical assessment*

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*Research note accepted in the British Journal of Political Science. Both authors contributed equally. Please address correspondence to both authors. Replication materials on https://github.com/zfazekas/bjps-conflict and the journal website.

Supplementary Information A

Replication of Schuck, Vliegenthart, and Vreese (2014)

The results reported by SVdV are based on data from twenty countries instead of the intended twenty-one. Most likely due to between-wave merging problems, all entries from Bulgaria appeared as duplicates, in which the data rows were shifted. Ultimately, this generated a data file that – published along the original article, for Bulgaria, contained no single observation for which all of the covariates and the response variable were *complete cases*. Given the default listwise deletion employed by many statistical software packages, this means that all results reported by SVdV in the multivariate section are based on data from twenty, instead of the intended twenty-one countries, Bulgaria being completely excluded from the analysis.

The *N* of 21,776 reported by SVdV does not include a single respondent from Bulgaria. After the necessary corrections, we have 22,792 valid observations, in twenty-one countries now. This sample size is also in line with what Schuck, Boomgaarden, and Vreese (2013) report (22,791) when they analyze a different dependent variable – cynicism – using data from the same study with a very similar methodological approach. Thus, we re-specified the models reported by SVdV using the originally intended sample of twenty-one countries. We re-estimated the models using multiple statistical softwares (results from R reported here). Table SI.A1 displays results from all four models reported by SVdV (pages 11-12) tagged as *Published* and, in parallel, the results from the models on the complete data, tagged as *Revised*.

The results reported in the *Published* columns are identical to the ones originally reported by the authors. When comparing the revised model outputs on the full data, few, but important results change. SVdV report statistical significance based one-tailed tests, so we note that $\star$ in our tables ($p < 0.1$, two-tailed) is identical to the $\star$ ($p < 0.05$, one-tailed significance levels) reported by SVdV. Independent of the threshold chosen, when Bulgaria is included, the cross-level interaction supporting SVdV's second hypothesis fails to reach statistical significance. While we do not necessarily contest the idea that the same media content can have different effects in different contexts, we do argue that there is no empirical support for contextual effects as proposed by SVdV.

The authors themselves point out that the amount of between-country variation of the *news conflict* slope is rather small, but they also emphasize that the variation is statistically significant. Beside the point that just because something is statistically significant it does not mean that it is substantively relevant, we investigate the "proclaimed significance" of the slope variation. Naturally, variation is bound to be ≥ 0, so if the decision to consider it statistically significant was based on the fact that the lower bound of the 95% confidence interval for the slope variation was above zero, there might be an issue. Also, just by looking at the log-likelihoods reported by SVdV

Table SI.A1: Model results

	Table 1 model		Fixed-effects model		Random-effects model		Cross-level interaction	
	Published	Revised	Published	Revised	Published	Revised	Published	Revised
Turnout intention (t-1)	0.514***	0.512***	0.514***	0.511***	0.514***	0.511***	0.514***	0.511***
	(0.009)	(0.009)	(0.009)	(0.009)	(0.009)	(0.009)	(0.009)	(0.009)
Education	0.139***	0.145***	0.140***	0.146***	0.140***	0.146***	0.140***	0.146***
	(0.018)	(0.018)	(0.018)	(0.018)	(0.018)	(0.018)	(0.018)	(0.018)
Female	−0.186***	−0.171***	−0.187***	−0.172***	−0.187***	−0.172***	−0.188***	−0.174***
	(0.034)	(0.033)	(0.034)	(0.033)	(0.034)	(0.033)	(0.034)	(0.033)
Age	0.016***	0.017***	0.016***	0.017***	0.016***	0.017***	0.016***	0.017***
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Direct campaign contact	0.162***	0.195***	0.159***	0.191***	0.159***	0.191***	0.159***	0.191***
	(0.051)	(0.050)	(0.051)	(0.050)	(0.051)	(0.050)	(0.051)	(0.050)
Mediated campaign contact	0.180***	0.180***	0.180***	0.179***	0.180***	0.180***	0.179***	0.179***
	(0.025)	(0.024)	(0.025)	(0.025)	(0.025)	(0.025)	(0.025)	(0.025)
News exposure	−0.014*	−0.016**						
	(0.008)	(0.007)						
News conflict	0.073***	0.078***	0.030***	0.027***	0.030***	0.027***	0.037***	0.033***
	(0.025)	(0.025)	(0.010)	(0.009)	(0.010)	(0.010)	(0.010)	(0.010)
News conflict × Polity evaluations							0.151*	0.133
							(0.086)	(0.085)
Polity evaluations	0.312	0.382	0.181	0.215	0.146	0.187	−0.204	−0.123
	(1.168)	(1.128)	(1.181)	(1.141)	(1.187)	(1.147)	(1.198)	(1.158)
Compulsory voting	0.817**	0.821**	0.801*	0.801**	0.799*	0.800**	0.788*	0.789**
	(0.406)	(0.396)	(0.411)	(0.402)	(0.413)	(0.404)	(0.410)	(0.401)
Simultaneous elections	0.657**	0.643**	0.636**	0.619**	0.640**	0.623**	0.631**	0.615**
	(0.274)	(0.267)	(0.277)	(0.270)	(0.279)	(0.272)	(0.277)	(0.270)
Constant	−3.274***	−3.288***	−3.276***	−3.292***	−3.279***	−3.294***	−3.292***	−3.304***
	(0.157)	(0.149)	(0.159)	(0.150)	(0.159)	(0.151)	(0.159)	(0.150)
N	21,776	22,792	21,776	22,792	21,776	22,792	21,776	22,792
Countries	20	21	20	21	20	21	20	21
σ^2 country level	0.252	0.240	0.259	0.247	0.260	0.249	0.257	0.246
σ^2 News conflict					0.0002	0.0002	< 0.0001	< 0.0001
Log Likelihood	-11,013.380	-11,566.840	-11,015.070	-11,569.260	-11,015.030	-11,569.240	-11,013.530	-11,568.050

Note: *p<0.1; **p<0.05; ***p<0.01. In line with SVdV, unstandardized coefficients reported.

in Table 2 (original piece), we are hard-pressed to believe that letting the slope of *news conflict* vary across countries yields significantly better fitting models compared to the "fixed-effects" model. We use quotation marks here, because we consider this to be better described as a varying-intercept model, but for the ease of comparability, we follow the names employed by SVdV. There are many ways to assess whether this variation is significant, and here we choose to look at comparative model fit (χ^2 difference test) for nested models: the "fixed-effects" model is nested within the random-effects model, which is nested within the cross-level interaction model (and subsequently the "fixed-effects model" is nested in the cross-level interaction model).

Table SI.A2: Model fit comparison

Compared to	Model	Df	AIC	BIC	logLik	χ^2	Diff. Df	Pr(> χ^2)
Published								
-	Fixed-effects	12	22054.13	22149.99	-11015.07			
Fixed-effects	Random-effects	13	22056.06	22159.91	-11015.03	0.07	1	0.7945
Fixed-effects	Cross-level int.	14	22055.06	22166.90	-11013.53	3.07	2	0.2153
Random-effects	Cross-level int.	14	22055.06	22166.90	-11013.53	3.00	1	0.0831
Revised								
-	Fixed-effects	12	23162.53	23258.94	-11569.27			
Fixed-effects	Random-effects	13	23164.47	23268.91	-11569.23	0.06	1	0.8058
Fixed-effects	Cross-level int.	14	23164.10	23276.58	-11568.05	2.43	2	0.2964
Random-effects	Cross-level int.	14	23164.10	23276.58	-11568.05	2.37	1	0.1235

Model fit comparisons for both *Published* and *Revised* models are reported in Table SI.A2. These results indicate that estimating an extra parameter for the between-country variance of the *news conflict* slope brings no added value to the model. While one could argue that including the cross-level interaction and fitting that model to the original (20 country) data results in a better fit compared to the random-effects model, it still does not fit significantly better than the "fixed-effects" model. And in-between, the random-effects model does not fit better than the "fixed-effects" model. Both the Akaike Information Criterion and the Bayesian Information Criterion point to the same substantive conclusion. It should be noted that with each step between the models we are losing only one degree of freedom. This is due to the fact that the models employed by SVdV assumed that the random effects for the intercept and the slope are uncorrelated.

We re-specified the last two models including the estimation of a covariance parameter between the random effects. The results do not change substantively, and the correlation between the intercept and slope random effects is -0.80 for the random effects model. This correlation is substantially reduced, when we employed the necessary centering of the predictors (an issue discussed in the next section). Model fit comparisons are even less friendly towards the original claims, if we estimate the random effects covariance parameter.

Figure 2 of the original piece offers a visual display of the different *news conflict* slopes, depending on polity evaluation (negative, neutral, positive). This figure is intended to convey the substantive importance of the information environment. The figure is based only on the "fixed part of the analysis" (SVdV, 12) and does not display any uncertainty measures. Accordingly, when reproducing this figure, no substantial differences should be expected compared to those reported in the original paper. We have followed the description by SVdV of Figure 2. It was not perfectly clear what SVdV mean by "fixed part of the analysis", so we used the fixed-effects from the cross-level interaction. All continuous variables were set to closest existing value of their mean, and as the mean polity evaluations (−0.04) are closest to Slovakia, the country level predictors were fixed to the values found for each country – Austria, Slovakia, and Spain – and in all cases these country predictors had the value of 0.

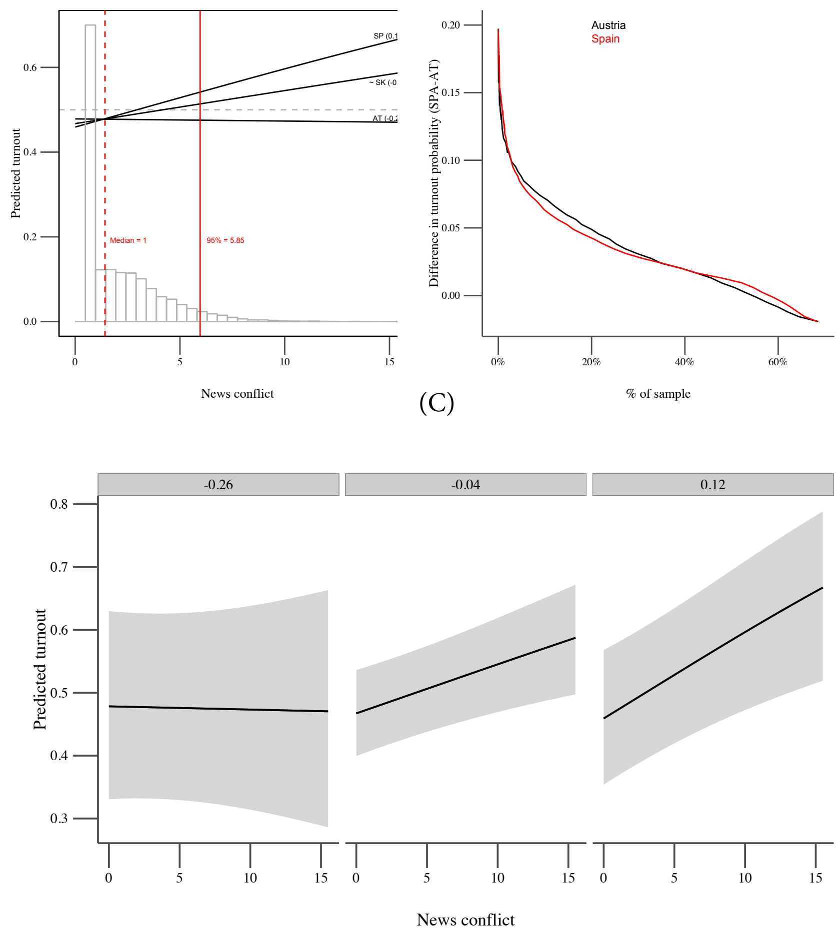
However, confidence intervals and the distribution of the key variable help us to understand the conditions under which *news conflict* has a statistically and substantially significant effect on turnout (Brambor, Clark, and Golder 2006; Kam and Franzese 2007). As the point we are making does not necessarily depend on whether the cross-level interaction is significant or not, we decided to reproduce Figure 2 using the original data (20 countries), displayed in Panel A of Figure SI.A1.

The distribution of the predictor (full sample, 20 countries) and sample moments of interest indicate that sizable between-country differences in predicted turnout appear only at unlikely values of the *news conflict*. Panel B in Figure SI.A1 substantiates this claim. Again, we see that substantive between-country differences in predicted turnout appear at very small proportions of the samples. While we do not deem the chosen method of presentation or discussion used by SVdV as an issue, these further details help in better grasping the substantial role between-system differences can play.

However, we can also see that in some countries, given the same level of *news conflict*, the predicted probability of turnout is above 0.5, whereas in others is below 0.5. This can also be regarded as a substantively important effect, as it differentiates between individuals for whom the predicted behavior would be turnout vs. those below 0.5 are expected to stay home. Panel C in Figure 1 includes 95% confidence bounds calculated using only the fixed effects from the cross-level interaction model (original data), with all other covariates fixed at values discussed above. We note that bounds do not take the distribution of the predictor in the data into consideration. When uncertainty is factored in, we see that even with the selection of the minimum and maximum (positive) polity evaluation country values, these slopes are not significantly different from each other, or the 0.5 probability value. Using the full sample, we find no statistical or substantial cross-level interaction between the information

Figure SI.A1: Polity evaluation and news conflict

(A) (B)



Panel (A): predicted turnout as a function of news conflict for different polity evaluation levels (reproducing Figure 2 by SVdV). All other variables fixed at values described in the text and by SVdV, predictions based on fixed-effects only. Distribution of news conflict on the 20 country original sample. Panel (B): difference in predicted turnout between most positive and most negative polity evaluation countries plotted against the % of the sample in a country with news conflict values for which the δ turnout probability was calculated. Panel (C): identical to Panel (A) with 95% confidence intervals calculated based on fixed-effects (and their standard errors) only. Random effects are averaged over, news conflict values range from its minimum (0) to its maximum (15.5), with increments of 0.25 for the prediction.

environment and *news conflict*.

In order to complete the process of replication/reproduction process two additional features have to be noted. First, the operationalization of news conflict implies that for an individual who reports not following at all any of the media outlets, both news exposure (a simple summation) and *news conflict* should take the value 0: people with no media exposure could not have been exposed to any *news conflict*. However, in the replication data 1208 respondents were not exposed to any medium but have scores above zero on the *news conflict* variable. The *news conflict* scores were very low for all these cases, and accordingly it does not change any of the previously reported results or the results reported in the original piece. For the analyses reported in the article, we followed the operationalization coding, and we re-coded all cases with 0 exposure to 0 *news conflict* value.

Second, as per page 6 or the original article, four features of the news item indicate conflict, and each of them can either present (1) or not (0). One of these items is listed in Appendix B, SVdV as "Explicitly (only if the story or somebody in the story says depicts so): Does the story (or somebody in the story) mention two or more sides of (i.e., not two separate arguments of but two or more distinct perspectives on) a problem or issue?", and in the Media Study codebook as "**V33** Explicitly (only if the story or somebody in the story says depicts so): Does the story (or somebody in the story) mention two or more sides of (i.e., not two separate arguments of but two or more distinct perspectives on) a problem or issue? Note: These 'sides' do not necessarily indicate a conflict or disagreement. Example: The tax increase might look good on the budget but it might slow down demand as citizens will be left with less money to spend. Coding: 1 = no 2 = yes" Schuck et al. (2010, 54). We have checked whether including this item in the index has any influence on the original or our findings and that is **not** the case, and we use the four items as SVdV.

Supplementary Information B

Model fit comparison

SVdV present an additional, statistical, reason behind the dismissal of news exposure in favor of *news conflict*. As the collinearity between the two variables is not perfect, the authors also report that adding *news conflict* to a model that has all other predictors (including exposure) yields a significant gain in fit. This is correct, but some other model comparisons are missing from the discussion. Table SI.B1 reports these model comparisons. The sequence of models based on SVdV's Table 1 is intuitive: the "no media" model contains all predictors used by the authors, except the two exposure predictors; the "exposure only" model includes the mere news exposure predictor; the "conflict only" model includes the *news conflict* measure; and finally, the "full model" includes the two predictors simultaneously, as the model reported by SVdV in Table 1. We use the full sample (21 countries), with the corrected *news conflict* measure (see discussion in the main text and *Supplementary Information* **A**).

Table SI.B1: Model fit comparison: exposure vs. news conflict

Compared to	Model	Df	AIC	BIC	logLik	χ^2	Diff. Df	Pr(> χ^2)
No media	Conflict only	12	23164.10	23260.51	-11570.05	6.90	1	0.0086
No media	Exposure only	12	23166.70	23263.11	-11571.35	4.29	1	0.0383
Exposure only	Full model	13	23162.36	23266.81	-11568.18	6.34	1	0.0118
Conflict only	Full model	13	23162.36	23266.81	-11568.18	3.74	1	0.0532

As Table SI.B1 shows, adding either one of the exposure related predictors produces a model with significantly better fit (first two rows). Next, adding *news conflict* to a model that already includes news exposure further enhances the fit. Adding exposure to a model with *news conflict* already present results in a better fitting model, but this difference is not statistically significant at conventional level (p = 0.0532). But the differences are minimal, and the the AIC and BIC fit statistics point into opposite directions.

Supplementary Information C

Data and model for "The mobilizing effect of non-conflictual news"

In the first two columns of Table SI.C1 we report the country average scores for news conflict and news non-conflict. These are based on the conflict and non-conflict scores for each outlet in the respective country. The last three columns report correlations between the individual level exposure variables, after using the operationalization proposed by SVdV.

Table SI.C1: Exposure, news conflict, new non-conflict

	News conflict	News non-conflict	ρ (conf, no-conf)	ρ (conf, exposure)	ρ (no-conf, exposure)
AT	0.456	0.544	0.992	0.998	0.998
BE-F	0.172	0.828	0.978	0.984	0.999
BE-W	0.279	0.721	0.961	0.980	0.997
BG	0.231	0.769	0.982	0.989	0.999
CYP	0.246	0.754	0.957	0.976	0.997
CZ	0.324	0.676	0.974	0.988	0.997
DE	0.146	0.854	0.984	0.988	1.000
DK	0.263	0.737	0.952	0.975	0.996
EE	0.195	0.805	0.982	0.988	0.999
FI	0.314	0.686	0.976	0.989	0.998
FR	0.438	0.562	0.986	0.995	0.998
GRE	0.286	0.714	0.975	0.988	0.998
HU	0.302	0.698	0.984	0.993	0.998
IRE	0.169	0.831	0.985	0.990	1.000
IT	0.334	0.666	0.980	0.991	0.998
LAT	0.358	0.642	0.993	0.997	0.999
LIT	0.040	0.960	0.716	0.740	0.999
LUX	0.190	0.810	0.997	0.998	1.000
MT	0.455	0.545	0.992	0.998	0.998
NL	0.329	0.671	0.955	0.980	0.995
PL	0.383	0.617	0.939	0.980	0.988
PT	0.348	0.652	0.919	0.969	0.988
RO	0.371	0.629	0.976	0.991	0.996
SLO	0.255	0.745	0.986	0.942	0.932
SPA	0.332	0.668	0.981	0.992	0.998
SVK	0.283	0.717	0.983	0.991	0.999
SWE	0.165	0.835	0.904	0.935	0.997
UK	0.214	0.786	0.960	0.977	0.998

Descriptive statistics for the variables used in this section are displayed in Table SI.C2. All models were specified to mimic those reported by SVdV, Table 1. Media predictor slopes are fixed across countries and no-cross level interaction was specified. General formula for n individuals in J countries:

$$
\begin{aligned}
\Pr(y_i = 1) = \text{logit}^{-1}(\alpha_{j[i]} + \beta_1 \text{age}_i + +\beta_2 \text{female}_i + \beta_3 \text{education}_i + \\
\beta_4 \text{directContact}_i + \beta_5 \text{indirectContact}_i + \beta_6 \text{mediaVariable}_i), \text{ for i} = 1, \ldots, \text{n} \\
\alpha_j = \gamma_0^\alpha + \gamma_1^\alpha \text{simElect} + \gamma_2^\alpha \text{compVote} + \gamma_3^\alpha \text{polityEval} + \eta_j^\alpha, \text{ for j} = 1, \ldots \text{J}.
\end{aligned}
$$

where the second level errors are normally distributed, $\eta_j^\alpha \sim N(0, \sigma_\alpha^2)$. Full model results are reported in Table SI.C3.

Table SI.C2: EES summary statistics

Statistic	N	Mean	SD	Min	Max
Turnout	26,908	0.710	0.454	0	1
Age	26,763	50.291	16.911	18	99
Sex (female = 1)	27,068	0.559	0.497	0	1
Education	26,206	0.771	0.896	0	2
Direct contact	27,069	0.161	0.432	0	2
Indirect contact	27,069	0.824	0.982	0	5
News conflict (uncentered)	27,069	2.579	2.034	0.000	16.593
News non-conflict (uncentered)	27,069	6.187	4.417	0.000	33.587
News exposure (uncentered)	27,069	8.839	6.037	0	42

Table SI.C3: The impact of mere exposure, EES 2009

	Turnout		
	Mere exposure	Conflict	No-conflict
Exposure	0.259^{***}	0.252^{***}	0.261^{***}
	(0.016)	(0.016)	(0.016)
Age	0.026^{***}	0.026^{***}	0.026^{***}
	(0.001)	(0.001)	(0.001)
Female	-0.062^{**}	-0.065^{**}	-0.061^{**}
	(0.030)	(0.030)	(0.030)
Education	0.248^{***}	0.248^{***}	0.246^{***}
	(0.018)	(0.018)	(0.018)
Direct campaign contact	0.210^{***}	0.212^{***}	0.211^{***}
	(0.046)	(0.046)	(0.046)
Mediated campaign contact	0.263^{***}	0.263^{***}	0.263^{***}
	(0.020)	(0.020)	(0.020)
Polity evaluations	0.201	0.201	0.201
	(0.932)	(0.936)	(0.937)
Compulsory voting	1.003^{***}	1.002^{***}	1.004^{***}
	(0.385)	(0.385)	(0.385)
Simultaneous elections	0.951^{***}	0.950^{***}	0.952^{***}
	(0.233)	(0.232)	(0.233)
Constant	-0.946^{***}	-0.950^{***}	-0.948^{***}
	(0.138)	(0.138)	(0.138)
N	25,841	25,841	25,841
Political systems	28	28	28
σ^2 country level	0.289	0.288	0.289
Log Likelihood	-13,502.110	-13,507.600	-13,499.410
AIC	27,026.220	27,037.190	27,020.810

Note: *p<0.1; **p<0.05; ***p<0.01
Best fitting model based on model comparison: news non-conflict.

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