**Supplementary Information for Matching Theory and Data:**

**Why Combining Media Content with Survey Data Matters**

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**Supplementary material:**

This supplementary file consists of four sections. Sections A-C respond to sections A-C in F&L. Section D provides the analyses referred to in our Response.

For all the analyses presented here, we rely on the adjusted dataset, including 21 countries and a marginal correction of the conflict variable (see under A). We rely on unstandardized coefficients here, since we believe they are most insightful when wanting to understand substantive impacts of variables (Hayes, 2005). Only when we are explicitly interested in comparing effects across variables/models (as in Table S5 below), we use standardized coefficients.

**SECTION A**

***Cross-level interaction***

As acknowledged in our Response, the original article was erroneously based on 20 countries (due to data file merging). The exclusion of Bulgaria from the analyses does not affect our substantial interpretations of our findings, as we discuss below. However, it does result in one change. As F&L correctly note, when including Bulgaria in the analyses the p-value changes and falls just above the .05-threshold (one-tailed test). F&L note that this means that the cross-level interaction hereby “fails to reach statistical significance”. We cannot disagree with this observation. However, to paraphrase Gelman and Stern (2006, p. 328) ‘the difference between “significant” and “not significant” is not itself statistically significant’. Indeed, if one compares the result from the analysis with and without Bulgaria, the difference is small. To test whether this difference is significant, one cannot rely on a common test for comparison of effects sizes (Gelman & Stern, 2006), since they assume studies to be independent – which is clearly not the case here. Instead, we employ a bootstrap procedure (Mooney & Duval, 1993): for both analyses we bootstrap 100 samples with replacement, all with the original number of observations and compare the estimations of the cross-level interaction. This comparison shows *no* significant difference between the bootstrap estimates for twenty and twenty-one countries (MD=.0099, t-value=.871, p=.385).

Moreover, there are several reasons that lead us to consider the effect to be meaningful anyway.

First of all, while our study is unique in the combination of content analysis and panel surveys in 21 countries, for multilevel modeling the number of cases at the second (country) level is relatively low. The most important consequence of this is that analyses considering explanatory variables at that level, as well as cross-level interactions, are likely to suffer from a lack of statistical power (Hox, 2010) and will not easily reach statistical significance. Dismissing potentially interesting and substantially meaningful results that are marginally significant, as is the case for the cross level interaction in the current case (in the revised version of the final model the p-value in a one-tailed test is .064), would run counter to insights about statistical power in multi-level analyses with limited numbers of cases. Earlier studies on campaign effects on political knowledge and participation also considered cross-level interactions that are marginally significant, but substantially interesting (Sides & Karch, 2008; Liu et al., 2013).

Second of all, further analyses (reported below) reveal significant differences in the slope for the conflict variable between several countries – indicating that the effect of news conflict *does* differ in a significant way depending on the level of polity evaluations.

Third of all, as reported in footnote 47 of the original article, we also tested an alternative measure for polity evaluations, based on the net-contribution/benefit a country receives from the EU. In Table 1, we report the final model that includes this variable and its interaction with news conflict. Indeed, the results of this analysis reveal that the effect is, also with the 21 country dataset, significant, even if one considers a two-tailed test necessary here (see panel 3 in Table S4 below). We thus refrain from dismissing the substantive interpretation of these findings.

In their supplementary material F&L also discuss two other issues with our analysis of the cross-level interaction. First of all, they question our decision to consider a cross-level interaction at all, given the limited variance around the slope of news conflict. Here, they contend that if one uses model fit statistics, such as the (change in) log-likelihood, a model that includes a random slope for news conflict is not significantly better than a model that only includes a random intercept (which we labeled ‘fixed effects’, an appropriate term in our view and one that is commonly used in multilevel analysis as well – e.g., Snijders, 2005). F&L’s systematic comparison of model fit statistics indeed reveal that we are talking about small effects of the interaction term here. This is something that we explicitly stated in the original manuscript as well (p.11). One might even argue that, given the small variance in the effects of news conflict across countries, it is remarkable that an interaction that is (marginally) significant is present.

What is even more important here, however, is that there actually *is* slope variation and the variance differs significantly from 0. There is no agreed upon way to assess whether it is worthwhile to consider the variation in the slope of a variable, as F&L also admit. Also here, both theoretical and empirical considerations are of importance. If we focus on the empirical side, model fit statistics can be consulted, but also the presence of (significant) slope variation can be used (see Aguinis et al., 2013). We relied on the latter, since we consider it is the most direct way to assess whether it is worthwhile to investigate cross-level interactions (see Hakhverdian & Mayne, 2012 for a similar approach). F&L argue that there might be an issue here, since variance is per definition positive and bound to scores equal to or higher than 0. The same argument is made by Snijders and Bosker (2012, p. 100) who argue that a test that assumes a normal distribution might be inappropriate especially because the variance estimate is necessarily nonnegative. As the results of the final model in our original manuscript show, this does not mean that the lower bound of the variance cannot equal 0. When including the interaction term, all slope variation has been captured and the remaining variance is not statistically different from zero. This means that the cross-level interaction captures all variation in the effect of conflict framing that exists across countries.

As a supplementary test, we re-estimated the “random effects” model applying a bootstrapping method (as recommended by Aguinis et al., 2013) (100 samples with replacement, all with the original number of observations). Results reveal that also using this method, we find that the slope variation is statistically significant (.0002 – 95% CI lower bound: 4.687\*10-8 ). Again, we do not argue that this is a large amount of variance, but as the results from the final model indicate – it is worthwhile to consider.

In passing, F&L seem to suggest that a model including a covariance parameter between the random intercept and the random slope is more appropriate and that centering of the predictors in the interaction model is necessary (p. 4). Here we can be short: first, a model including a covariance parameter is virtually identical and does not yield any substantively different conclusions (see second panel in Table S4 below). The (negative) correlation has such a large standard error that it does not differ statistically from zero. Perhaps more importantly, we do not see any substantive argument to prefer such a model. Second, while mean centering can be argued to increase the interpretability of the interaction effect (but see e.g. Hayes, 2005 for an alternative view), it does not alter the substantial findings from the model in any way. The fact that it substantially reduces the correlation between the random slope and random intercept (p. 4 in supplementary material F&L) is a statistical artifact.

F&L discuss the *substantive* interpretation of theeffect at length. Their analyses go a step further than what we presented in the original paper. Simply put, they argue that the interaction effect, even when considered significant, has little substantial value. We disagree with this. Figure SI.A1 in F&L’s Supplementary Material – most notably panel C – demonstrates that news conflict has substantially different effects for the three countries highlighted (Austria, Slovakia and Spain). However, due to the very large variance around the predicted turnout (and consequently large confidence intervals), these differences are not significant – a criterion F&L deem important. In our view, this is rather a consequence of the fact that the overall models are only to some extent able to predict turnout. As we know from previous literature (see e.g. Matsusaka & Palda, 1999), this type of political behavior is very hard to explain. Predictions in all instances thus have a high level of uncertainty and in such a situation, it will be very hard for any kind of interaction effect to meet this criterion. If we focus more straightforward on the question whether the slopes for Austria, Slovakia and Spain differ (the examples used in our original article and in F&L’s response as well), we find that, also in the 21-country analysis, this is the case for Austria and Slovakia and Austria and Spain. The slopes are -.0044 (95% CI: -.0447;.0358) for Austria, .0286 (95% CI: .0088;.0484) for Slovakia and .0442 for Spain (95% CI: .0114;.0770). The coefficients indicate that the impact on predicted turnout is not huge, but still substantially different. We find this to be the case for a substantial amount of country-by-country comparisons (see Table S1 for the slopes for each of the countries). We consider this an indication that we deal with an interaction effect that in fact *is* substantially interesting and meaningful.

Table S1: Estimated slopes for individual countries

|  |  |  |  |  |
| --- | --- | --- | --- | --- |
| **country** | **polity evaluation** | **estimated slope** | **SE** | **z-value** |
| UK | -0.219 | 0.001 | 0.018 | 0.04 |
| France | 0.011 | 0.031 | 0.011 | 2.88 |
| Italy | -0.092 | 0.017 | 0.010 | 1.67 |
| Germany | 0.113 | 0.044 | 0.017 | 2.65 |
| Spain | 0.116 | 0.044 | 0.017 | 2.64 |
| Sweden | -0.118 | 0.014 | 0.011 | 1.21 |
| Denmark | -0.088 | 0.018 | 0.010 | 1.74 |
| Greece | -0.172 | 0.007 | 0.014 | 0.47 |
| Hungary | 0.028 | 0.033 | 0.011 | 2.89 |
| Czech Republic | -0.152 | 0.009 | 0.013 | 0.71 |
| Poland | 0.043 | 0.035 | 0.012 | 2.87 |
| Ireland | 0.065 | 0.038 | 0.013 | 2.81 |
| Austria | -0.258 | -0.004 | 0.021 | -0.22 |
| Portugal | 0.017 | 0.031 | 0.011 | 2.88 |
| Belgium | -0.141 | 0.011 | 0.013 | 0.86 |
| Netherlands | -0.101 | 0.016 | 0.011 | 1.50 |
| Finland | -0.075 | 0.019 | 0.010 | 1.97 |
| Lithuania | 0.047 | 0.035 | 0.012 | 2.86 |
| Latvia | 0.033 | 0.033 | 0.012 | 2.88 |
| Slovakia | -0.004 | 0.029 | 0.010 | 2.83 |
| Bulgaria | 0.045 | 0.035 | 0.012 | 2.86 |

*Note*: Based on cross-level interaction model in Table S3.

***Additional issues***

F&L note in their supplementary material two additional data issues. The first one relates to the non-zero values of the conflict variable for respondents with no news exposure. Indeed, there was a marginal error in the construction of the variable that resulted in slight deviations from the score that respondents should have received. In the analyses here, we use the corrected variable. This variable correlates .9998 with the one we used in the original article. As F&L also note the difference has no impact on any of the analyses presented. They also tested a conflict index consisting of three items (as opposed to the original four) and found the same results. The measurement and construction of news frames based on content data in a cross-national setting and the inherent challenges to this effort are also subject to work we are currently involved in (see de Vreese et al., 2001; Schuck et al., 2015).

**SECTION B**

Regarding section B, we can be short: the model comparisons provided by F&L offer additional information, but not of the kind that would have led to different decisions about the analysis from our side. As argued in our response, we prefer news conflict over news exposure first and foremost because of theoretical reasons. Furthermore, as their comparisons (in line with Table 1 of their article) indicate as well, the news conflict variable seems to perform better than the exposure variable (and the non-conflict variable as well) – but due to multicollinearity issues they are hard to compare. We noted this in our original article as well. In section D, we offer a first attempt to further disentangle news conflict and news exposure.

**SECTION C**

In our rebuttal we explained why we consider the EES analysis irrelevant and we do not respond additionally to this section here.

**SECTION D**

In this section, we present the following models:

* A replication of Table 1 and 2 (Table S2 and table S3) of our original manuscript with the adjusted 21-country dataset. We do not discuss the results in detail, since they are largely similar to the original results and we have responded to specific points coined by F&L in our rebuttal and in section A of this document. We argued that our choice of the news conflict variable over the news exposure variable is a valid one and that the interaction between news conflict and polity evaluations is substantively relevant.
* Additional models: (a) random effects model based on bootstrapping; (b) random effects model including a covariance parameter for random intercept and random slope; (c) final interaction model including the alternative measure of polity evaluations. We have referred to those models in section A of this document (Table S4). The models show that (a) the slope variance differs significantly from 0; (b) the model including a covariance parameter yields similar outcomes as the original model and (c) the interaction effect using an alternative measure for polity evaluations as cited in our original article is statistically significant.
* An additional analysis that offers a first attempt to disentangle the effects of news exposure and news conflict.

Table S2: Multilevel logistic regression explaining turnout in 2009 EP elections (wave2)

|  |  |
| --- | --- |
|  | Turnout model |
|  | B | SE |
| Vote intention (t-1) | 0.522\*\*\* | 0.009 |
| Education | 0.145\*\*\* | 0.018 |
| Female | -0.172\*\*\* | 0.033 |
| Age | 0.017\*\*\* | 0.001 |
| Direct campaign contact | 0.195\*\*\* | 0.050 |
| Mediated campaign contact | 0.180\*\*\* | 0.025 |
| News exposure | -0.008 | 0.007 |
| News conflict | 0.050\* | 0.025 |
| Polity evaluations | 0.294 | 1.136 |
| Compulsory voting | 0.808\* | 0.399 |
| Simultaneous elections | 0.629\*\*\* | 0.269 |
| Constant | -3.284\*\*\* | 0.150 |
|  |  |  |
| Variance country level | 0.243 |  |
|  |  |  |
| Log restricted-likelihood | -11569.804 |  |

*Note.* Bs are unstandardized coefficients from fixed-effects multilevel models. \* p<.05; \*\* p<.01; \*\*\* p<.001 (one-tailed); N= 22,792

Table S3: Multilevel logistic regression explaining turnout in 2009 EP elections (wave2)

|  |  |  |  |
| --- | --- | --- | --- |
|  | Fixed effects model | Random effects model | Cross-level interaction |
|  | B | SE | B | SE | B | SE |
| Vote intention (t-1) | 0.512\*\*\* | 0.009 | 0.512\*\*\* | 0.009 | 0.512\*\*\* | 0.009 |
| Education | 0.146\*\*\* | 0.018 | 0.146\*\*\* | 0.018 | 0.146\*\*\* | 0.018 |
| Female | -0.172\*\*\* | 0.033 | -0.172\*\*\* | 0.033 | -0.174\*\*\* | 0.033 |
| Age | 0.017\*\*\* | 0.001 | 0.017\*\*\* | 0.001 | 0.017\*\*\* | 0.001 |
| Direct campaign contact | 0.193\*\*\* | 0.050 | 0.193\*\*\* | 0.050 | 0.192\*\*\* | 0.050 |
| Mediated campaign contact | 0.180\*\*\* | 0.025 | 0.180\*\*\* | 0.025 | 0.180\*\*\* | 0.025 |
| News conflict | 0.023\*\* | 0.009 | 0.023\* | 0.010 | 0.029\*\* | 0.010 |
| Polity evaluations  | 0.207 | 1.142 | 0.174 | 1.153 | -0.115 | 1.159 |
| News\*Polity evaluations |  |  |  |  | 0.130+ | 0.086 |
| Compulsory voting | 0.798\* | 0.402 | 0.796\* | 0.404 | 0.786\* | 0.401 |
| Simultaneous elections | 0.616\* | 0.271 | 0.622\* | 0.273 | 0.612\* | 0.270 |
| Constant | -3.287\*\*\* | 0.151 | -3.290\*\*\* | 0.151 | -3.298\*\*\* | 0.150 |
|  |  |  |  |  |  |  |
| Variance country level | 0.248 |  | 0.250 |  | 0.246 |  |
|  |  |  |  |  |  |  |
| Variance news conflict |  |  | 0.0002 |  | 0.0000 |  |
|  |  |  |  |  |  |  |
| Log restricted-likelihood | -11570.445 |  | -11570.398 |  | -11569.291 |  |

*Note.* Bs are unstandardized coefficients from multilevel models. + p<.10; \* p<.05; \*\* p<.01; \*\*\* p<.001 (one-tailed); N= 22,792

Table S4: Multilevel logistic regression explaining turnout in 2009 EP elections (wave2)

|  |  |  |  |
| --- | --- | --- | --- |
|  | Random effects modelbootstrapping | Random effects modelcovariance | Cross-level interactionAlternative variable |
|  | B | SE | B | SE | B | SE |
| Vote intention (t-1) | 0.512\*\*\* | 0.010 | 0.512\*\*\* | 0.009 | 0.512\*\*\* | 0.009 |
| Education | 0.146\*\*\* | 0.018 | 0.146\*\*\* | 0.018 | 0.146\*\*\* | 0.018 |
| Female | -0.172\*\*\* | 0.033 | -0.172\*\*\* | 0.033 | -0.173\*\*\* | 0.033 |
| Age | 0.017\*\*\* | 0.001 | 0.017\*\*\* | 0.001 | 0.017\*\*\* | 0.001 |
| Direct campaign contact | 0.193\*\*\* | 0.040 | 0.194\*\*\* | 0.050 | 0.195\*\*\* | 0.050 |
| Mediated campaign contact | 0.180\*\*\* | 0.022 | 0.179\*\*\* | 0.025 | 0.180\*\*\* | 0.025 |
| News conflict | 0.023\* | 0.011 | 0.023\* | 0.012 | 0.035 | 0.026 |
| Polity evaluations  | 0.174 | 0.228 | 0.730 | 1.061 | -0.232\* | 0.120 |
| News\*Polity evaluations |  |  |  |  | 0.021\* | 0.011 |
| Compulsory voting | 0.796\*\*\* | 0.074 | 0.772\* | 0.378 | 0.953\* | 0.372 |
| Simultaneous elections | 0.622\*\*\* | 0.057 | 0.577\* | 0.257 | 0.404 | 0.256 |
| Constant | -3.290\*\*\* | 0.081 | -3.257\*\*\* | 0.157 | -3.109\*\*\* | 0.170 |
|  |  |  |  |  |  |  |
| Variance country level | 0.250 |  | 0.292 |  | 0.213 |  |
|  |  |  |  |  |  |  |
| Variance news conflict | 0.0002 |  | 0.0008 |  | 0.0000 |  |
|  |  |  |  |  |  |  |
| Covariance |  |  | -0.771 |  |  |  |
| Log restricted-likelihood | -11570.398 |  | -11570.398 |  | -11566.583 |  |

*Note.* Bs are unstandardized coefficients from multilevel models. + p<.10; \* p<.05; \*\* p<.01; \*\*\* p<.001 (one-tailed); N= 22,792

 *Alternative operationalization*

F&L note that our conflict measure and media exposure are strongly correlated, as we also state in the original manuscript (p. 11) and repeat in our rebuttal. This makes it challenging to disentangle the two. Models that include both suffer from problems with multicollinearity. This comes as no surprise, since the news conflict measure explicitly weighs in exposure. In the original article, we do *not* (as F&L suggest) make the claim that our conflict measure outperforms the exposure measure empirically. Theoretically, we have good reasons to prefer conflict framing over media exposure (see main text). Empirically, we demonstrate that the effect of conflict framing holds also if we include the media exposure measure (first model in Table S2). We acknowledge, however, that it indeed is important to try to further disentangle the two and understand what part of the effect of our variable is driven by exposure and what part by content.

 Table S5 presents the results of analyses that shed some first light on this issue. Our analyses are based on the 21-country dataset, but findings are comparable if based on 20 countries, as the analysis reported in our original article. It presents the media effects of different models, each including different, alternative media variables, some of which are purposefully chosen to reflect theoretically less relevant or even counterintuitive operationalizations, such as e.g. the non-conflict variable F&L developed. We look at conflict framing, mere exposure, no conflict framing (1-conflict, as F&L do in their analyses of EES data), and some other commonly used frames in media effects research (strategy and horse race – all constructed according to the same procedure as the conflict framing variable and all relying on the PIREDEU data set). Since we are interested in a comparison of effects here, we use standardized (based on grand mean) coefficients here. The results show that the model including conflict framing *is* overall the best one: all fit statistics point in that direction and also the size of the standardized coefficient (beta) is highest for the conflict variable.

Table S5. Random intercept models with different media variables included

|  |  |  |  |  |  |  |  |
| --- | --- | --- | --- | --- | --- | --- | --- |
| Variable | Beta | SE | z-value | significance one tailed | log likelihood | AIC | BIC |
| Conflict | .0478 | .0194 | 2.46 | <.01 |  -11570.45 |  23164.89 |  23261.3 |
| no conflict | .0285 | .0186 | 1.53 | <.10 | -11572.33 |  23168.65 |  23265.06 |
| exposure | .0344 | .0185 | 1.86 | <.05 |  -11571.77 | 23167.54 | 23263.95 |
| strategy | .0315 | .0197 | 1.60 | <.10 | -11572.21 |  23168.43 | 23264.84 |
| horserace | .0046 | .0191 | 0.24 | Ns | -11573.47  |  23170.93  |  23267.34 |

N=22,792. Controls as in other models are included, but not reported.

That differences are relatively small is not surprising given the high correlations between the variables (all in the .8-.9 range). To further unpack the differences, we again use a bootstrap method with 100 samples with replacement, all with the original number of observations and compare the estimated coefficients for conflict, exposure and non-conflict. A mean comparison shows that the estimated coefficients for conflict are significantly larger than those of exposure (MD=.0141; t-value=5.004; p<.001) and non-conflict (MD=.0242; t-value=9.466; p<.001). Combined with the theoretical argumentation provided in our original contribution, we are confident we made the right choice by focusing our analyses on the effects of conflict.

Although we think there are very convincing theoretical arguments to prefer conflict above an ‘empty’ exposure measure and that we do not claim that a weighted content variable would have to empirically outperform an ‘empty exposure’ variable per se, we do find it an interesting and relevant exercise to further disentangle the effects of ‘empty’ exposure and weighted exposure. To empirically distinguish the exposure and the conflict components of our weighted conflict measure, we computed a variable that specifically captures the conflict part. This variable measures the deviation between a respondents’ conflict score (based on the initial formula weighing in media exposure) on the one hand and the conflict score of a respondent with similar exposure multiplied by the average conflict scores across all outlets in the analyses. It thus offers a comparison between the conflict score of the respondent and a (virtual) respondent that has the same media exposure to outlets with average conflict framing. If the outlets that the respondent has been exposed to report in a more conflictual way than average, the score will be positive. If the outlets are less conflictual, the score will be negative. In that way, the conflict framing in the outlets a respondent consumes determines the score here, not exposure. In a formula:

transformed news conflict variable = news conflict framing - (.2812\*news\_exposure)

We expect that the higher the score here (thus the more conflictual the coverage in the outlets the respondent consumed), the more likely (s)he will be mobilized to vote. The new variable has a mean close to zero (.03) and hardly correlates with exposure (*r=*-.02). In Table S6 (panel 1) we summarize a random intercept model that looks at the effects of this new conflict variable in a model that also includes news exposure.

Table S6. Random intercept models predicting turnout

|  |  |  |
| --- | --- | --- |
|  | Conflict model | NO conflict model |
|  | B | SE | B | SE |
| Vote intention (t-1) | 0.512\*\*\* | 0.009 | 0.512\*\*\* | 0.009 |
| Education | 0.145\*\*\* | 0.018 | 0.145\*\*\* | 0.018 |
| Female | -0.172\*\*\* | 0.033 | -0.172\*\*\* | 0.033 |
| Age | 0.017\*\*\* | 0.001 | 0.017\*\*\* | 0.001 |
| Direct campaign contact | 0.195\*\*\* | 0.050 | 0.195\*\*\* | 0.050 |
| Mediated campaign contact | 0.180\*\*\* | 0.025 | 0.180\*\*\* | 0.025 |
| News conflict (transformed) | 0.050\* | 0.025 |  |  |
| News no conflict (transformed) |  |  | -0.050\* | 0.025 |
| News exposure | 0.005\* | 0.003 | 0.005\* | 0.003 |
| Polity evaluations  | 0.294 | 1.136 | 0.294 | 1.136 |
| Compulsory voting | 0.808\* | 0.399 | 0.808\* | 0.399 |
| Simultaneous elections | 0.629\*\* | 0.269 | 0.629\*\* | 0.269 |
| Constant | -3.284\*\*\* | 0.150 | -3.284\*\*\* | 0.150 |
|  |  |  |  |  |
| Variance country level | 0.243 |  | 0.243 |  |
| Log restricted-likelihood | -11569.804 |  | -11569.804 |  |

*Note.* Bs are unstandardized coefficients from multilevel models. \* p<.05; \*\* p<.01; \*\*\* p<.001 (one-tailed); N= 22,792

It shows that *both* exposure and conflict have a positive and significant effect (p<.05, one-tailed). This confirms the idea that both exposure and content are driving the conflict framing effect on mobilization – which is in line with our initial argumentation in the original article.

For illustrative purposes, we run the same model for a variable that focuses on the share of coverage that has no conflict framing, as F&L do in their study based on cross-sectional EES data. Here, we computed the variable in a similar manner, this time subtracting .7188\*exposure from the initial NO conflict variable. The results of a random intercept model are summarized in Table S6, second panel and actual resemble those of the previous model, which makes sense because of the perfect correlation between conflict and no conflict. Thus, we find exactly the opposite effect of the no conflict variable– with the same effect size: in this case non-conflictual news demobilizes. The exposure variable has a positive effect. Again, this finding is in line with our initial theoretical argument.

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