The Effect of Terrorism on Judicial Confidence: Supplemental Appendix

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Contents

Introduction

This is the supplemental appendix to "The Effect of Terrorism on Judicial Confidence," a manuscript set to appear in *Political Research Quarterly*. The supplemental appendix, like the manuscript, is a dynamic document that automatically generates the code presenting the results within the document itself (Xie, 2013). This approach to document preparation has multiple benefits, namely in the ability to drive the incidence of transcription error to zero while calling specific results into the document. I will make some references in this document to specific statistics that the raw markup will show is a direct extrapolation from code into presentation. The raw markup is available upon request and will be available on my personal Github account upon publication. This will facilitate transparency in published statistical analysis, consistent with the Data Access and Research Transparency Initiative (DA-RT) by the American Political Science Association.

Descriptive Statistics

I start the supplemental appendix with basic descriptive statistics for the variables I present in the manuscript. I choose to be brief in this section of the appendix since this information is descriptive and provides basic background information about the analyses I present in the manuscript. Table A.1 starts this section with a summary of the countries and years that appear in the analysis. The reader should note that despite appearing in the first two waves of European Values Survey data, Canada and the United States do not appear in the analyses I conduct.

Table A.1: Average Judicial Confidence, Terror Level, and Judicial Independence by Country-Year

Country	Year	Average Confidence in Judiciary	Level of Terror Threat	Judicial Independence
Norway	1982	0.839	3.045	Yes
Sweden	1982	0.732	2.944	Yes
Malta	1983	0.483	0.000	Yes
Iceland	1984	0.689	2.833	Yes
Austria	1990	0.584	5.591	Yes
Belgium	1990	0.465	6.380	Yes
Denmark	1990	0.794	3.892	Yes
Finland	1990	0.663	1.386	Yes
France	1990	0.575	7.929	Yes
Iceland	1990	0.666	1.099	Yes
Ireland	1990	0.472	5.976	Yes
Italy	1990	0.318	7.576	Yes
Netherlands	1990	0.629	6.240	Yes
Norway	1990	0.752	3.664	Yes
Poland	1990	0.525	0.000	Yes
Portugal	1990	0.442	5.578	Yes
Spain	1990	0.440	9.545	Yes
Sweden	1990	0.559	4.127	Yes
United Kingdom	1990	0.525	9.105	Yes
Bulgaria	1991	0.455	4.394	No

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Table A.1: Average Judicial Confidence, Terror Level, and Judicial Independence by Country-Year (continued)

Country	Year	Average Confidence in Judiciary	Level of Terror Threat	Judicial Independence
Czechoslovakia	1991	0.443	5.342	Yes
Hungary	1991	0.596	2.197	Yes
Malta	1991	0.398	3.178	Yes
Romania	1993	0.476	4.111	No
Austria	1999	0.681	4.749	Yes
Belgium	1999	0.366	5.944	Yes
Bulgaria	1999	0.269	5.935	No
Croatia	1999	0.306	5.063	No
Czech Republic	1999	0.230	4.718	Yes
Denmark	1999	0.785	2.565	Yes
Estonia	1999	0.327	3.850	Yes
France	1999	0.462	7.340	Yes
Germany	1999	0.574	7.402	Yes
Greece	1999	0.437	8.102	Yes
Hungary	1999	0.442	6.236	Yes
Iceland	1999	0.736	0.000	Yes
Ireland	1999	0.556	5.384	Yes
Italy	1999	0.315	6.332	Yes
Latvia	1999	0.472	5.951	No
Lithuania	1999	0.167	3.932	Yes
Luxembourg	1999	0.598	1.386	Yes
Malta	1999	0.450	2.996	Yes
Netherlands	1999	0.475	5.449	Yes
Poland	1999	0.431	5.981	Yes
Portugal	1999	0.423	3.367	Yes
Romania	1999	0.401	2.079	No
Russia	1999	0.366	9.277	No
Slovakia	1999	0.355	6.084	No
Slovenia	1999	0.437	3.314	Yes
Spain	1999	0.423	8.233	Yes
Sweden	1999	0.608	4.227	Yes
Ukraine	1999	0.327	4.816	No
United Kingdom	1999	0.471	6.968	Yes
Belarus	2000	0.464	4.890	No
Finland	2000	0.667	0.916	Yes
Turkey	2001	0.573	9.582	No
Albania	2008	0.252	0.693	No
Armenia	2008	0.418	4.727	No
Austria	2008	0.636	5.220	Yes
Azerbaijan	2008	0.686	4.727	No
Belarus	2008	0.617	2.565	No
Bosnia and Herzegovina	2008	0.389	5.472	No

Continued on next page...

Table A.1: Average Judicial Confidence, Terror Level, and Judicial Independence by Country-Year (continued)

Country	Year	Average Confidence in Judiciary	Level of Terror Threat	Judicial Independence
Bulgaria	2008	0.176	0.000	No
Croatia	2008	0.195	1.946	No
Cyprus	2008	0.670	4.554	Yes
Czech Republic	2008	0.350	0.693	No
Denmark	2008	0.871	0.000	Yes
Estonia	2008	0.547	0.000	Yes
France	2008	0.555	6.625	Yes
Georgia	2008	0.314	6.297	No
Germany	2008	0.525	5.649	Yes
Greece	2008	0.508	7.780	No
Hungary	2008	0.383	0.000	Yes
Ireland	2008	0.505	5.421	Yes
Latvia	2008	0.437	5.030	No
Lithuania	2008	0.253	0.000	Yes
Luxembourg	2008	0.692	0.000	Yes
Macedonia	2008	0.378	5.466	No
Malta	2008	0.472	0.000	Yes
Moldova	2008	0.420	4.575	No
Montenegro	2008	0.439	4.977	No
Netherlands	2008	0.538	2.996	Yes
Norway	2008	0.783	4.060	Yes
Poland	2008	0.429	0.000	Yes
Portugal	2008	0.476	0.000	Yes
Romania	2008	0.415	0.000	No
Russia	2008	0.414	9.859	No
Serbia	2008	0.245	0.000	No
Slovakia	2008	0.349	0.000	No
Slovenia	2008	0.473	0.000	Yes
Spain	2008	0.427	8.760	Yes
Switzerland	2008	0.739	4.970	Yes
Ukraine	2008	0.251	2.918	No
Belgium	2009	0.510	4.043	Yes
Finland	2009	0.724	6.023	Yes
Iceland	2009	0.674	0.000	Yes
Italy	2009	0.361	5.717	No
Sweden	2009	0.684	5.069	Yes
Turkey	2009	0.810	9.386	No
United Kingdom	2009	0.513	8.008	Yes

Table A.2 is basic table of descriptive statistics for all variables used in Table 1 of the manuscript. Recall that all macro-level variables were group-centered (on the survey wave) and standardized by two standard deviations. This scaling was done on a macro-level data frame of the country-years used in the analysis and prior to a merge into the micro-level data frame (of individual-

level survey respondents). This is why the scaled micro-level variables all have means of zero and standard deviations of .5 while the macro-level variables slightly deviate from this set mean and standard deviation.

Table A.2: Descriptive Statistics for Variables Used in the Analysis

Statistic	N	Mean	St. Dev.	Min	Max
Judicial Confidence	151,235	0.495	0.500	0	1
Age	156,190	0.000	0.500	−1.090	1.742
Female	156,683	0.542	0.498	0	1
College Education	106,016	0.101	0.301	0	1
Income Groups	130,681	−0.000	0.500	−0.889	0.979
Unemployed	155,219	0.075	0.263	0	1
Confidence in Parliament	147,560	−0.000	0.500	−1.421	1.773
Level of Democracy	152,226	0.000	0.500	−1.444	0.807
Real GDP per Capita	152,522	0.000	0.500	−1.267	0.927
Level of Terrorism	146,449	0.000	0.500	−1.182	1.021
Judicial Independence	139,103	0.666	0.472	0	1
Judicial Inefficiency	62,685	0.000	0.500	−0.611	1.895
Post-9/11 Dummy	156,738	0.426	0.495	0	1
Western Europe Dummy	156,738	0.583	0.493	0	1

Figure A.1 provides a correlation matrix of the variables used in the analysis to address preliminary concerns of multicollinearity.[1] A correlation matrix presented akin to a "heat map" has multiple benefits over a standard correlation matrix. It weights the visibility of the correlation coefficient by how high the absolute value of Pearson's r, drawing the reader's attention to only the important potential sources of collinearity. A standard correlation matrix, by contrast, presents low correlation coefficients visually equivalent to high correlation coefficients, which places an extra burden on the reader to gleam the relevant information. For example, it is of no interest that age and confidence in parliament correlate at 0.069 while the correlation coefficient between the Western Europe dummy and GDP per capita (r = 0.674) is more important to know for the reader and the researcher. A correlation matrix as "heat map" downplays irrelevant information and emphasizes important information.

There are a few conspicuous correlation coefficients for variables for which there is an intuitive relationship. Western Europe scores high in democracy relative to other countries in Europe (r = 0.607) and Western Europe is wealthier than other countries in Europe (r = 0.674). Democracy and economic wealth travel together (r = 0.773), an observation well-known to scholars interested in the exact relationship between economic development and democracy (e.g. Przeworski et al., 2000). Likewise, judicial independence is prominent in Western Europe (r = 0.683) and among countries scoring higher in democracy. (r = 0.665). This does not surprise those who see Western Europe as more democratic than its other peers in Europe and for scholars who think that judicial independence could be a "necessary condition" for the observation of democracy (e.g. Howard and Casey, 2003). However, it does suggest a concern of collinearity in the coefficients. I address this concern with robustness checks in the next section.

[1] There is no correlation coefficient for the post-9/11 dummy and judicial ineffiency because the *Doing Business* project, which is responsible for the judicial ineffiency data, starts after 9/11.

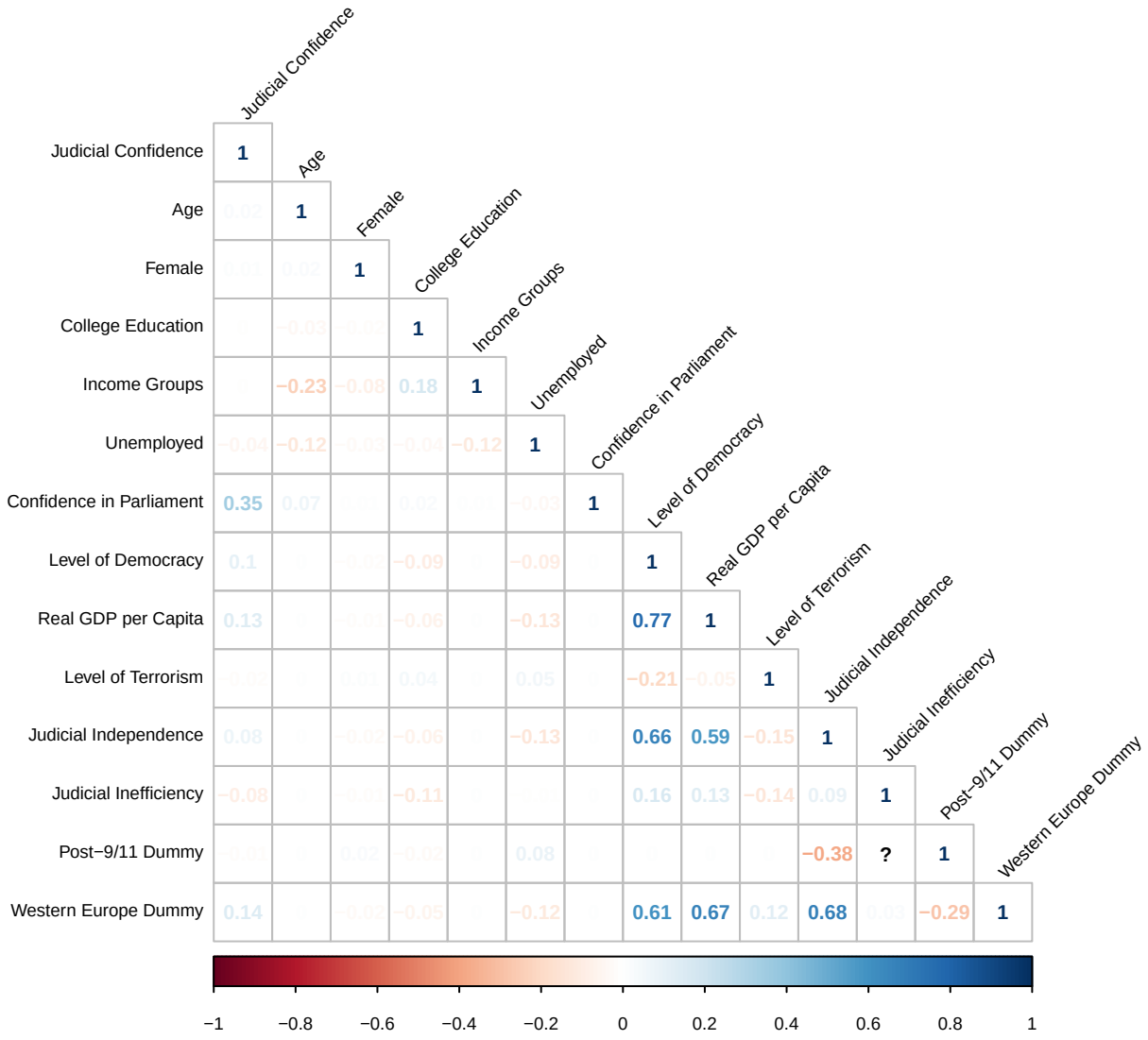

Figure A.1: A Correlation Matrix of the Items Used in the Analysis

Additional Models and Robustness Checks

This section of the appendix discusses a multitude of robustness checks I conducted in addition to the main findings I report in the manuscript. For simplicity, I choose to present the results of almost all models as coefficient plots rather than regression tables. The rationale here is both aesthetic and practical. Coefficient plots are visually appealing and allow for more regression summaries in a smaller space.

I further focus most of the robustness tests on what I present as Model 2 in Table 1 in the manuscript. All robustness concerns raised by anonymous reviewers focused on macro-level variables. Micro-level coefficients typically have little bearing on macro-level coefficients (and vice-versa) in multi-level models (beyond concerns of non-random data loss).

Figure A.2, a correlation of fixed effects of Model 4 from the manuscript, highlights this negligible effect of micro-level fixed effects on macro-level fixed effects. A correlation matrix of fixed effects in a mixed effects model differs in interpretation from a correlation matrix of raw variables. In this application, they give an approximation of the correlation of the regression coefficients. Put another way, they give us an estimate of what would happen to other regression coefficients in the model if a particular regression coefficient increased or decreased in magnitude.[2] Notice the bottom-left quadrant of this is almost completely white. This indicates there is practically no discernible effect of a macro-level fixed effect on a micro-level fixed effect. This is true even for the parliamentary confidence fixed effect, which is the most robust and precise fixed effect in any model I estimate.

Assessing the Effects of Western Europe, Democracy, and Real GDP per Capita

I start the robustness tests with a series of regressions that rotate out some variables that may cause collinearity concerns indicated in Figure A.1. These are the real GDP per capita variable, the Western Europe dummy, and the level of democracy variable. Figure A.3 summarizes the results of a series of different specifications for Model 2 in the manuscript in which I either omit the democracy variable, Western Europe dummy, or real GDP per capita variable or make that variable the only specified fixed effect among the three variables that all correlate at high levels.

The results I summarize in Figure A.3 suggest few meaningful changes to these parameters contingent on the model specification. The Western Europe dummy is positive and significant in all models in which it is estimated. The democracy variable does not obtain statistical significance in any model in which it is estimated. Its inclusion in a particular model has no bearing on the parameter for judicial independence. The only meaningful change concerns the effect of real GDP per capita on judicial confidence, which we observe contingent on the model specification. Its parameter yields a p value we can discern from zero when 1) it is the only one of three correlated variables included in the model or 2) the Western Europe dummy is excluded. It is not significant when Western Europe is included and democracy is excluded or when all three are estimated in the same model (as Model 2 in the manuscript).

Importantly, none of the macro-level parameters of interest in the manuscript (i.e. terror threats, judicial independence, and their interaction) are affected by these different model specifications. The concern of correlation among these three variables has implications *only* for the regression parameters for these three variables, not the variables of interest in the manuscript.

[2]For further discussion: https://stat.ethz.ch/pipermail/r-sig-mixed-models/2009q1/001941.html

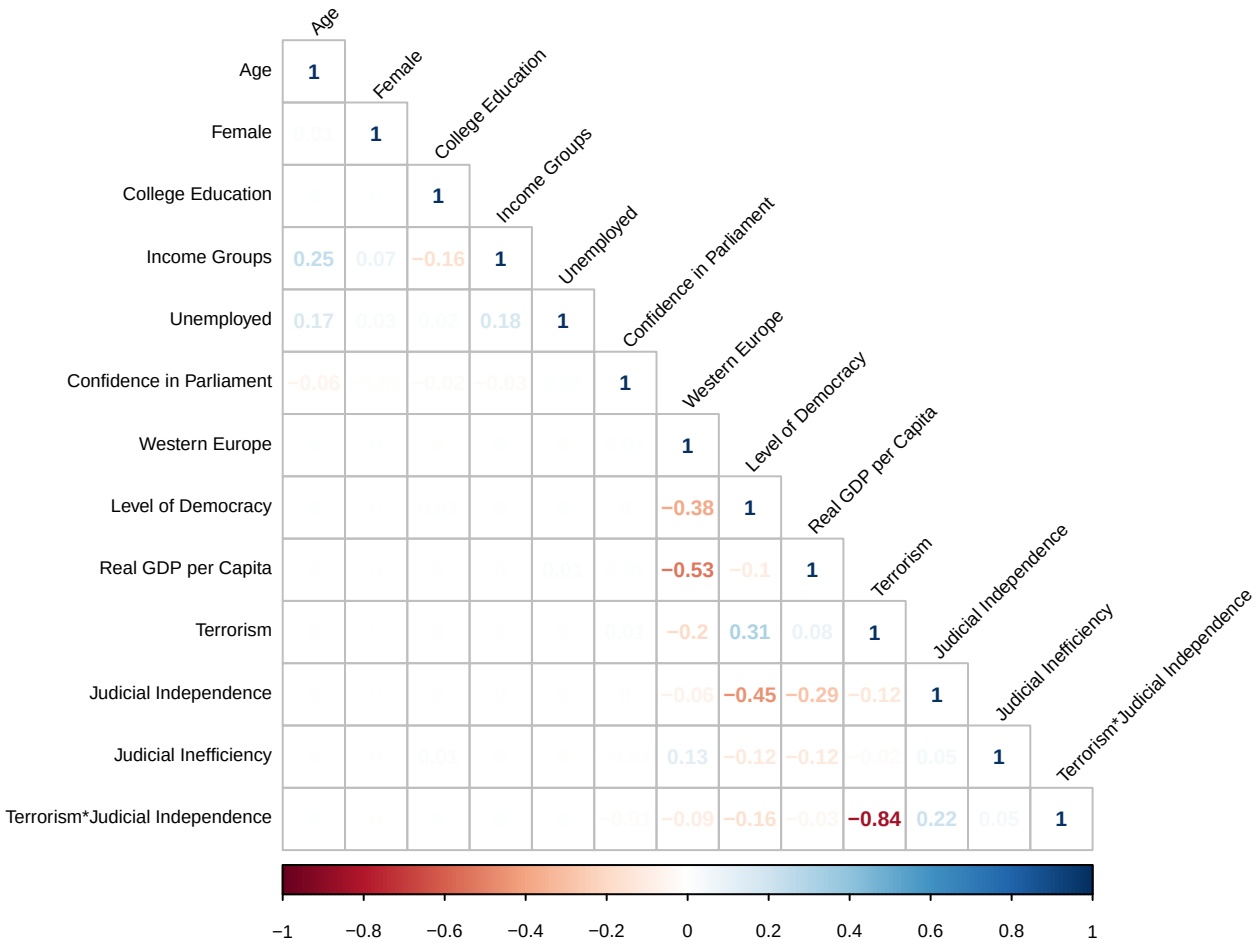

Figure A.2: A Correlation Matrix of the Fixed Effects in Model 4 in the Manuscript

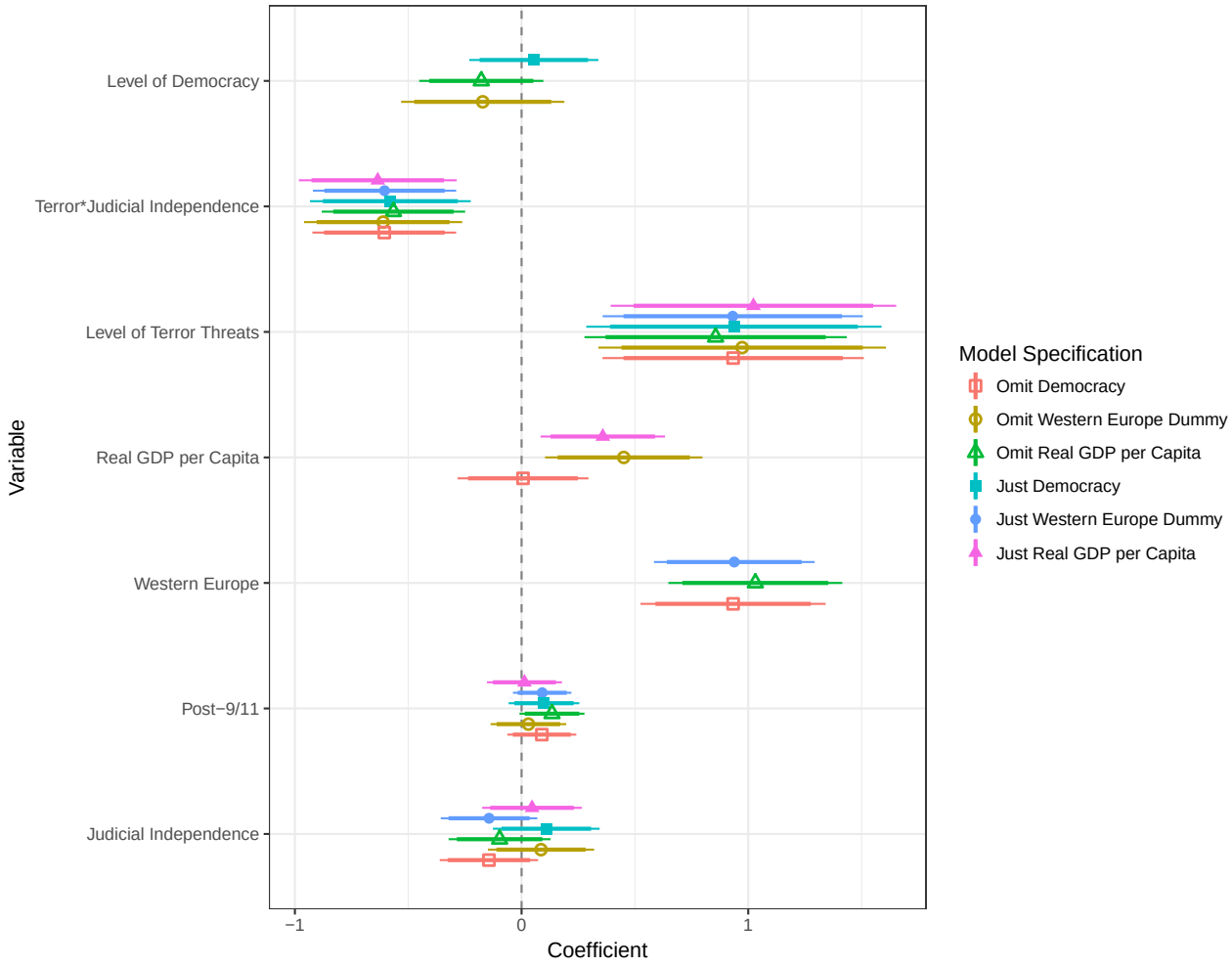

Figure A.3: Coefficient Plot of Different Specifications for Model 2

A Replication with an Ordinal Logistic Mixed Effects Model

The next robustness tests maintains the ordinal level of the dependent variable and re-estimates the models in Table 1 of the manuscript as ordinal logistic mixed effects models. Table A.3 contains the results of these regressions.

There are a few differences between Table A.3 and Table 1 in the manuscript. The effect of unemployment had no discernible effect on judicial confidence in Table 1 when I condense the dependent variable to a dichotomous measure of judicial confidence. The parameter for unemployed status of the respondent is negative and significant in Model 3 in Table A.3; individual-level unemployment decreases individual-level judicial confidence in the third and fourth waves. Similarly, college education is positive and significant in Model 3 in Table A.3 though not in Model 4. The effect of the Western Europe dummy fails to reach statistical significance in Model 4 in Table A.3. The nature of data loss affected the parameter estimate for the Western Europe dummy in the analyses on the fourth wave when the ordinal information of the dependent variable is preserved and modeled, though the effect of the Western Europe dummy was robust to the data loss and subsetting of survey waves in Table 1 in the manuscript. The final changes concern the level of democracy in Model 1 and real GDP per capita variable in Model 4. Both attain statistical significance in Table A.3 though are insignificant across all estimations in Table 1 in the manuscript. The astute reader will see this is a chance fluctuation in the parameter estimates. Both parameters were actually significant at the .10 level with p values approaching .05, but below the hard cut-off I institute in the analyses. Informed readers would know not to make too much into this change in p value contingent on the different model specification (e.g. Gelman and Stern, 2006).

This, however, is the full extent of changes between Table 1 in the manuscript and Table A.3. The parameter estimates for the level of terror threats, judicial independence, and the interaction between them, are unchanged when the ordinal nature of the dependent variable is modeled rather than condensed to a binary measure.

A Myriad of Different Ways to Model Terror Threats

I next consider the possibility that the relationships I observe for the macro-level parameters of interest in Table 1 are a function of the way in which I code terrorism. I justify my decision in the manuscript by noting my procedure is identical to how the *Global Terrorism Index* (GTI) (2014) codes terrorism in its database. The benefits are multiple, namely in its ability to model and weight terror incidents in the years prior to the observation year. However, this is just one way to code terror threats. The robustness of my results may depend on the ability to replicate them with different methods of modeling terror threats.

Figure A.4 summarizes a replication of Model 2 using ten different specifications for terror threat levels. The first specification is a latent terror threat specification from an item response model (Samejima, 1969) that produces a continuous estimate of terror threats from the number of terror incidents, the number of successful terror incidents, the number of people killed, the number of people wounded, and the extent of property damage for each of the five years prior to the observation year. The next two terror threat estimates focus on terror incidents that the *Global Terrorism Database* (GTD) (2014) codes as having an international element (i.e. for which the perpretators or logistics came from outside the country) or were purely domestic terror incidents. The next terror threat specification subsets terror attacks to those targeting the public rather than the government or military.[3] The next five terror threat specifications are the sum

[3]A terrorist attack that targets the public includes those targeting businesses, abortion-related services, aircraft

Table A.3: Ordinal Mixed Effects Models of Judicial Confidence in European Values Survey

	Model 1	Model 2	Model 3	Model 4
Age			-0.029^{*}	-0.026
			(0.014)	(0.019)
Female			0.093^{***}	0.078^{***}
			(0.014)	(0.018)
College Educated			-0.065^{**}	-0.031
			(0.022)	(0.030)
Income Group			-0.027	-0.015
			(0.014)	(0.019)
Unemployed			-0.069^{**}	-0.046
			(0.026)	(0.032)
Confidence in Parliament			1.924^{***}	1.972^{***}
			(0.015)	(0.020)
Post-9/11 Dummy	0.102	0.094	0.145	
	(0.083)	(0.079)	(0.100)	
Western Europe Dummy	0.998^{***}	0.936^{***}	0.888^{**}	0.717
	(0.234)	(0.213)	(0.306)	(0.379)
Level of Democracy	-0.352^{*}	-0.273	-0.151	-0.196
	(0.175)	(0.164)	(0.252)	(0.348)
Real GDP per Capita	0.003	0.039	0.180	0.717^{*}
	(0.199)	(0.185)	(0.267)	(0.361)
Level of Terrorism	-0.115	1.016^{***}	1.204^{***}	1.441^{***}
	(0.108)	(0.289)	(0.326)	(0.403)
Judicial Independence	-0.025	-0.018	-0.054	-0.113
	(0.123)	(0.113)	(0.134)	(0.207)
Level of Terrorism*Judicial Independence		-0.664^{***}	-0.711^{***}	-1.006^{***}
		(0.160)	(0.185)	(0.260)
Judicial Inefficiency				-0.424^{*}
				(0.180)
Num. obs.	130530	130530	80635	47870
Groups (countryyear)	100	100	76	
Groups (country)	45	45	44	42
Variance: countryyear: (Intercept)	0.082	0.075	0.088	
Variance: country: (Intercept)	0.242	0.185	0.243	0.327
1 | 2	-1.343^{***}	-1.339^{***}	-1.575^{***}	-1.839^{***}
	(0.222)	(0.207)	(0.290)	(0.313)
2 | 3	0.610^{**}	0.614^{**}	0.622^{*}	0.346
	(0.222)	(0.207)	(0.290)	(0.313)
3 | 4	2.895^{***}	2.899^{***}	3.274^{***}	3.081^{***}
	(0.222)	(0.207)	(0.290)	(0.314)

$^{***}p < 0.001$, $^{**}p < 0.01$, $^{*}p < 0.05$

of the constituent elements of the terror threat index. These are just the sum of the number of incidents in the year prior to the observation year, the sum of the number of successful incidents, the sum of the number of people killed, the sum of the number of people wounded, and the sum of the property damage on GTD's ordinal scale. I finally include the GTI terror score that I coded and present in the manuscript for comparison.

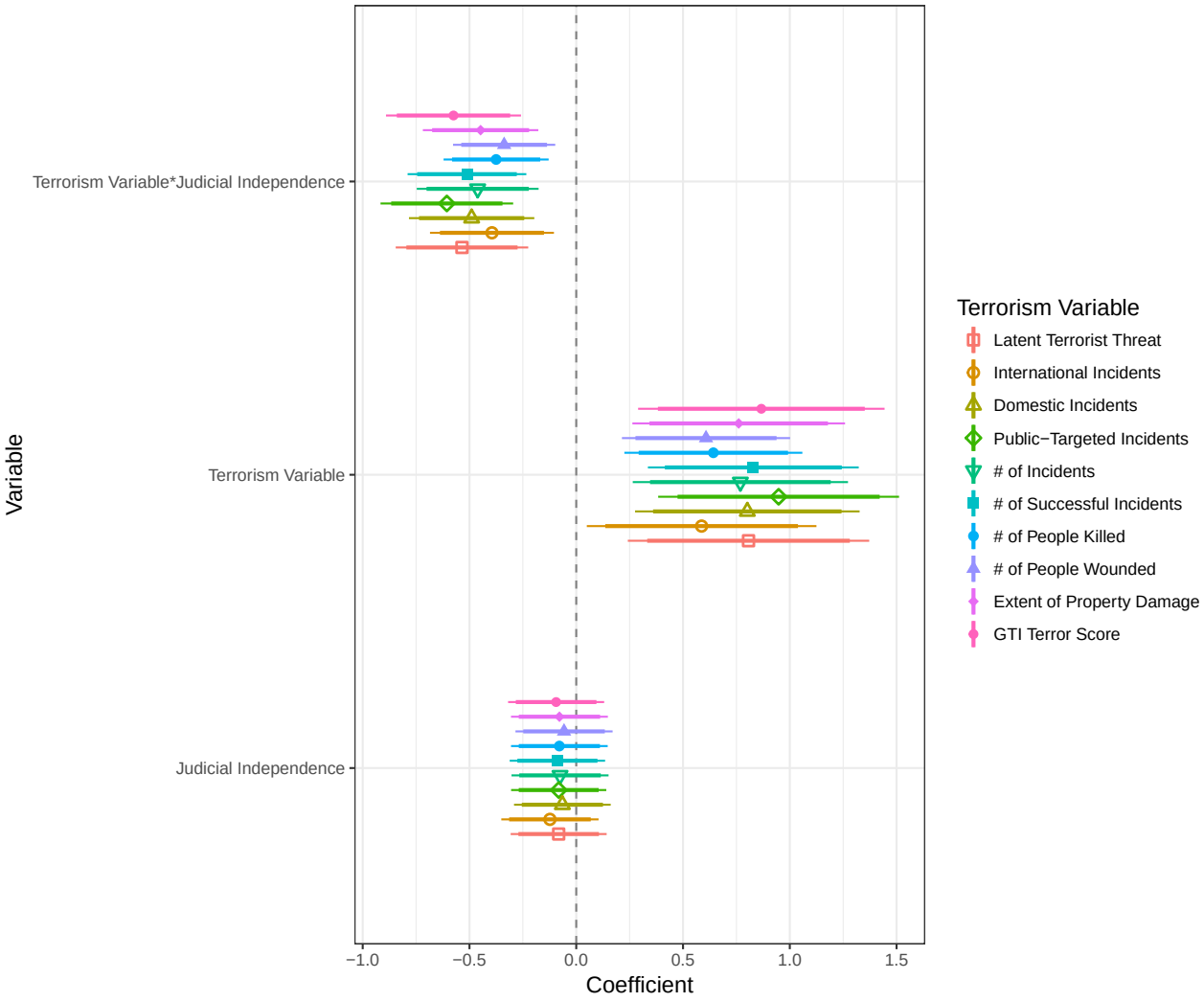

Figure A.4: Coefficient Plot of Different Terror Threat Specifications for Model 2 in the Manuscript

Figure A.4 summarizes these results of these ten different terror threat specifications, though I exclude a presentation of the controls for democracy, Western Europe, post-9/11 observations, and real GDP per capita to save space. The results of these regressions summarized in Figure

(which includes airline employees), educational institutions, food and water supplies, private property, religious figures and institutions, telecommunications, tourists, and public transportation. This measure omits attacks against the government, the police, the military, diplomats, journalists, maritime ports/facilities, non-governmental organizations, terrorists, utilities (e.g. oil pipelines), unknown targets, and violent political parties.

A.4 suggest that the findings consistent with my hypothesis are robust to a myriad of different specifications of terror threats. The effect of the terrorism variable is positive and statistically significant in each application. This is consistent with my contention that high terror threats increase judicial confidence in states without judicial independence. The effect of the interaction is negative and significant in each application. This is consistent with my contention that increasing terror threats decrease judicial confidence in states with judicial independence. Critically, my results do not depend on the measure of terrorism I use. The findings are the same even subsetting to just international terror incidents, domestic terror incidents, or terror incidents that target the public.

Potential Imbalances in the Data

The manuscript includes partial effects for democracy, judicial independence, and a Western Europe dummy for data I draw from the European Values Survey. This leads to credible concerns for an imbalance in the data as democracy conditions almost all macro-level indicators I employ in the analysis. There are also more democracies in Europe than non-democracies. A simple democracy control variable that I use may be insufficient. I account for this issue with a coarsened exact matching on democracy (Iacus, King and Porro, 2012) in the data. I describe the results below.

The concern for imbalance rests on the macro-level variables in the analysis, which is why I restrict an imbalance check to a re-consideration of the results I report in Model 2 in Table 1 in the manuscript. For simplicity's sake, I condense a "treatment" variable of democracy that equals a 1 if the country has a `polity2` score (c.f. Marshall and Jaggers, 2002) of 7 or greater in the given survey year. Thereafter, I assess balance on the possible variables in the model that democracy is likely to co-determine: the individual-level judicial confidence variable, the macro-level judicial independence variable, and the terror threat variable.

A simple imbalance test of the raw data produces a multivariate imbalance measure ($\mathcal{L}_1$) of 0.594. Whereas $\mathcal{L}_1$ is hard-bound between 0 (perfect balance, complete overlap between treatment and control) and 1 (perfect imbalance, no overlap between treatment and control), the $\mathcal{L}_1$ statistic indicates a some important imbalance in the data. Coarsened exact matching on democracy yields more balance in the data. The multivariate imbalance measure ($\mathcal{L}_1$) decreases from 0.594 to 0.438 after the coarsened exact matching procedure. However, Table A.4 shows that more balance came at the expense of sample size. Those familiar with matching know this as the balance-sample size frontier (King, Lucas and Nielsen, Forthcoming). Pruning observations from the original data frame provides more balance between treatment and control, but this balance comes at the expense of more precise parameter estimates in the statistical model. Likewise, a failure to prune anything from an original data set maximizes precision in a large-n data frame, but leaves aside important questions of potential imbalance. The matching procedure I chose here ultimately prunes 29% of the original data set for an increase in balance.

Table A.4: Summary of Matched and Unmatched Observations

	Control	Treatment
All	16,396	114,134
Matched	16,396	75,961
Unmatched	0	38,173

$\mathcal{L}_1$ (unbalanced): 0.594; $\mathcal{L}_1$ (balanced): 0.438

Figure A.5 shows my re-estimation of Model 2 with the balanced data, minus the democracy variable on which the data were balanced.[4] It also includes the number of observations (92,357), and parameters of interest for the country-year random effect (number of country-years: 68, standard deviation: 0.320) and the country random effect (number of countries: 40, standard deviation: 0.416). It suggests that the potential loss of efficiency that follows the pruning of 29% of the original data does not lead to a substantive change in the parameter estimates of importance to my hypotheses. There is still the significant interaction effect. Further, the parameter estimate for the constituent term of terror threats is still positive and significant as well. One somewhat interesting change is the judicial independence measure, which is still negative but now has a z-value with more precision (z = -1.482) albeit one that is still indiscernible from zero. Recall this is a constituent term in an interaction; the coefficient communicates a discernible decrease in individual-level judicial confidence for increasing judicial independence for citizens in states with the mean level of terror threats. A plot of predicted probabilities in Figure A.6, similar to what I present in Figure 1 in the manuscript, does not suggest a substantively different interpretation of the interactive relationship between judicial independence and terror threats on individual-level judicial confidence with this coefficient for judicial independence. The results appear "starker" but the interpretation is ultimately the same.

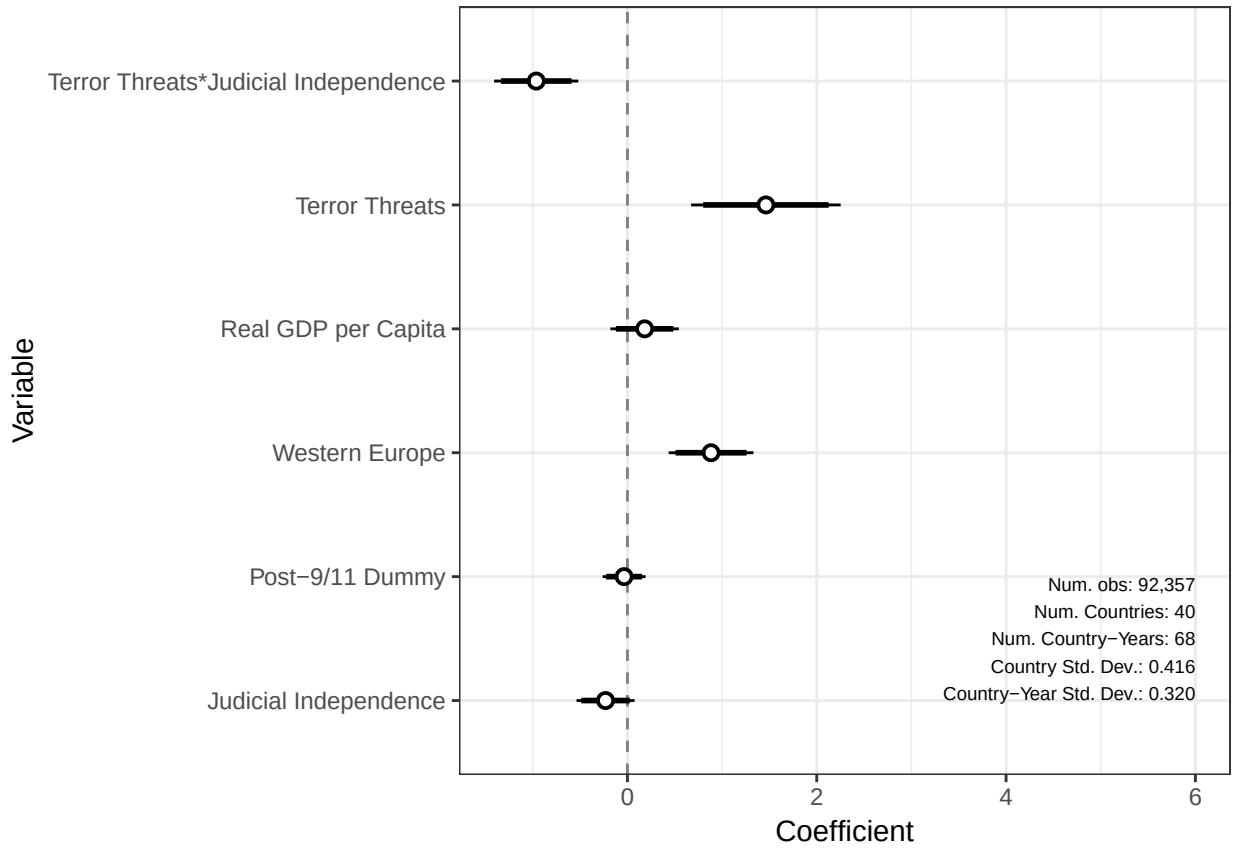

Figure A.5: Coefficient Plot of Macro-level Model with Matched Data

Ultimately, matching, and pruning 29% of the original data, do not change the important results I present in the manuscript.

[4]Figure A.3 suggests the level of democracy variable ultimately has no bearing on individual-level judicial confi-

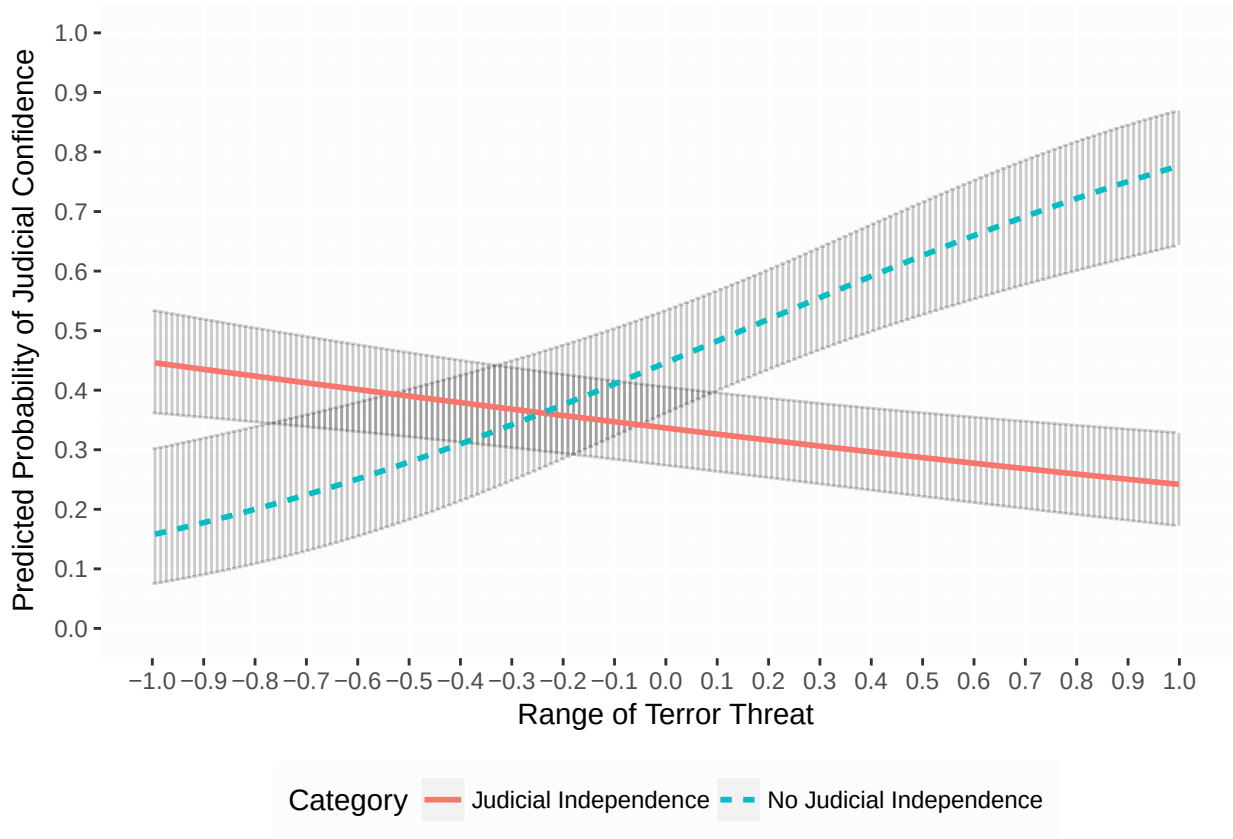

Figure A.6: The Interaction between Judicial Independence and Terror Threats on Judicial Confidence from Figure A.5

Domestic Terror Threats and Varying Effects by Income

Reviewer #3 expressed an interest in possible heterogeneity of domestic terror threats by an individual-level attribute like income. This is difficult to evaluate with European Values Survey (EVS) data for a variety of reasons. Namely, EVS does not consistently ask sociodemographic questions across countries or waves. In fact, several important sociodemographic variables, like education, do not appear until the third wave of data in the late 1990s. Further, income questions vary considerably across countries and waves. The three-part ordinal income group variable I use in the manuscript represents the best means to standardize these income questions across all four waves, but the type of model for which R3 asked (a varying slope for terror threats across each level of the income group category) would balk at the idea of allowing a slope to vary for just three income groups. The ensuing parameter estimate would not be reliable.

I identify a work-around to accommodate R3. The fourth wave of EVS asks a more granular question of income by reference to self-placement of annual income into income groups denominated in euros. These income categories are 1) less than 1,800 euros, 2) 1,800 euros to less than 3,600 euros, 3) 3,600 euros to less than 6,000 euros, 4) 6,000 euros to less than 12,000 euros, 5) 12,000 euros to less than 18,000 euros, 6) 18,000 euros to less than 24,000 euros, 7) 24,000 euros to less than 30,000 euros, 8) 30,000 euros to less than 36,000 euros, 9) 36,000 euros to less than 60,000 euros, 10), 60,000 euros to less than 90,000 euros, 11) 90,000 euros to less than 120,000 euros, and 12) 120,000 euros or more earned annually. The model that I then estimate is similar to Model 4 in the manuscript, though it substitutes the fixed effect of the three-part income group variable for the random effect of the 12-item income group category and it replaces the terror threat variable with the domestic terror threat variable that I estimate and show in Figure A.4. I summarize the results with the coefficient plot in Figure A.7, which includes other statistics of interest like the number of observations (47,870), and parameters of interest for the income group random effect (number of groups: 12, standard deviation: 0.078) and the country random effect (number of countries: 42, standard deviation: 0.596).

The results summarized in Figure A.7 square well with the results I provide in Model 4 in the manuscript. The gender fixed effect for women is statistically discernible from zero though its estimated effect is substantively small. The effect of confidence in parliament is again precise and substantively large, as it is in all models I estimate. The only change here is small and conceivably a result of chance or a slight change in model specification. The parameter for the Western Europe fixed effect, comfortably discernible from zero at the .05 level in Model 4 in the manuscript, barely misses that threshold for significance in Figure A.7 ($p = 0.073$). Importantly, the parameter for the level of terrorism, judicial independence, and the interaction between them are unchanged from Model 4 in the manuscript and Figure A.4.

Figure A.8 shows a caterpillar plot of the estimates and conditional variances for the intercepts of each income group and the varying slopes for domestic terror threats at each income group. Conditional variances for random effect parameters in the mixed effect modeling framework do not carry with it the same kind of interpretation as a p value for a fixed effect. Discussion in this section is necessarily about the illustrative value that Figure A.8 provides rather than any inferential implication.

Figure A.8 still reveals interesting patterns that suggest important variation in judicial confidence by different income groups and varying effects of domestic terror threats. The caterpillar plot suggests a general overall story or trend. The estimated intercepts for judicial confidence rise with increases in the income category while the effect of domestic terror threats for citizens

dence anyway.

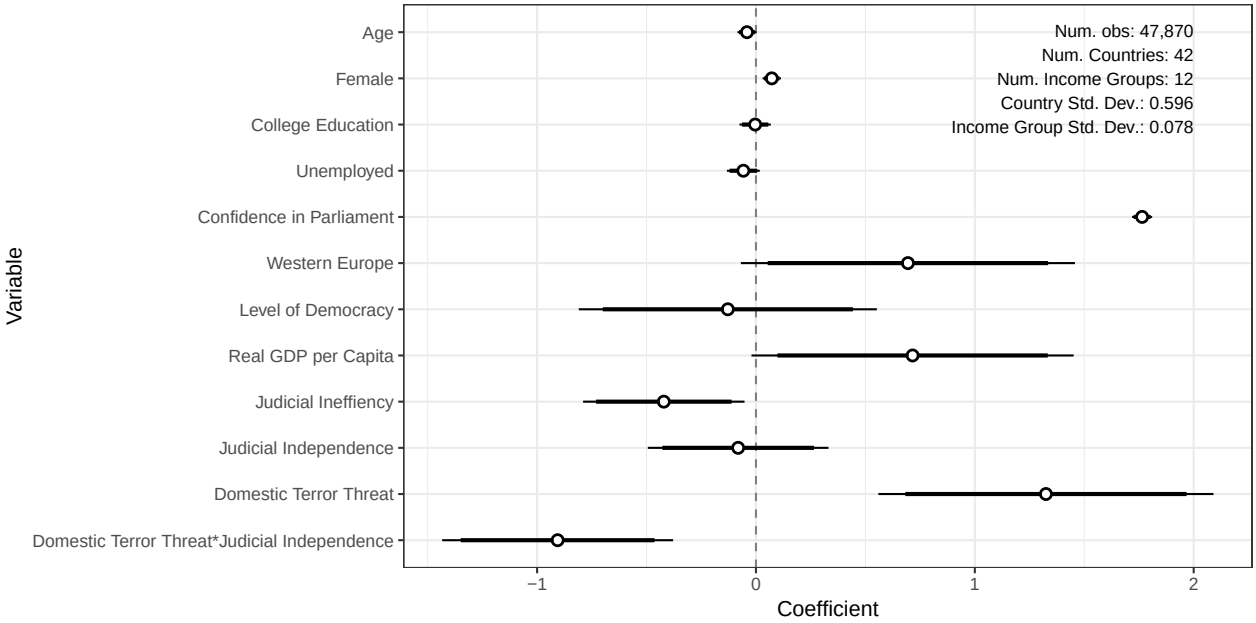

Figure A.7: Coefficient Plot of Correlates of Judicial Confidence in Fourth Wave of EVS

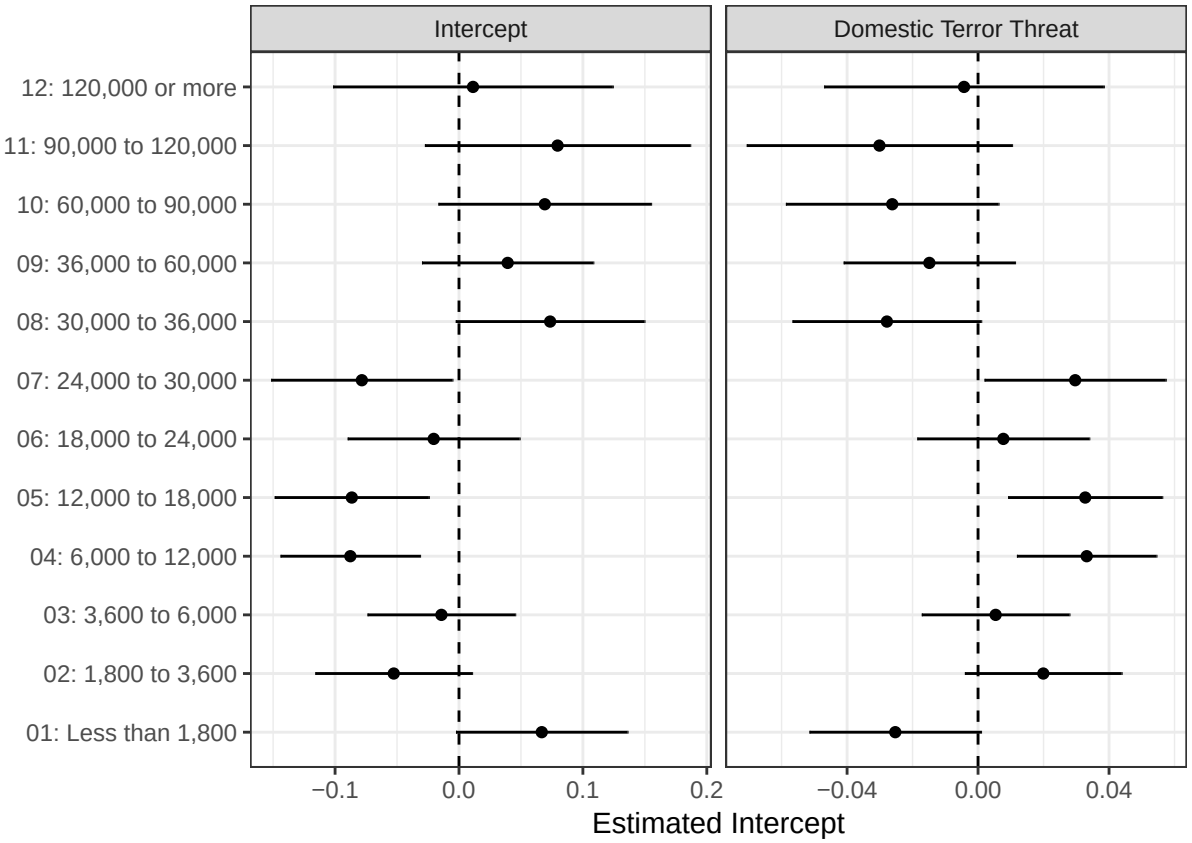

Figure A.8: Caterpillar plot of Varying Intercepts for Income Groups and Slopes for Domestic Terror Threat

in states without judicial independence decreases for increases in income. It is important to reiterate that this an illustrative statement and not an inferential statement given what conditional variances in the mixed effect model communicate. In the case of the varying slopes for domestic terror threats, a negative estimate would not change the sign of the overall fixed effect, just that it suggests a dampening effect.

The lowest income group (less than 1,800 euros in annual income) is conspicuous in this caterpillar plot. The general trend suggests citizens in poorer income groups start lower in their estimated judicial confidence. This would square well with conventional wisdom if not the particular parameter estimate I report in Model 4 in the manuscript. However, the lowest income group appears to start higher in estimated level of judicial confidence. Further, the effect of domestic terror threats appears to be lower than the *overall* estimated effect in the model summarized in Figure A.7, again running counter to a general trend that Figure A.8 illustrates.

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