

Online Appendix B - Interaction effects

Note: this appendix is an expansion of the ‘Results’ section of the main paper. Many parts are also present in the main paper.

Table 2 presents the results of five regression models. The first three include the responsiveness side of the account. They ask whether allocations reflect voters’ preferences (H1) and whether these are conditional upon domestic (H2a and H2b) and international constraints (H3). The fourth model presents the accountability side by analysing how different electoral institutions can provide varying incentives for policy-makers to use subsidies in an electorally pragmatic way (H4). The last model joins both sides to provide a more complete account of State aid politics. All models include a set of controls as discussed in the main document. Further, following Brambor et al. (2006) and Berry et al. (2012), Figures B1, B2 and B3 show how the marginal effects for the significant variables for H2a, H3 and H4 in Table 2 change respectively.

The first hypothesis, on the responsiveness of the government to voters’ preferences (H1), is tested separately because it does not assume higher-order coefficients.¹ Likewise, H2a and H2b, on the effect of coalition partners and veto players respectively, are tested individually to assess the validity of common-pool problems for collective action against veto player theory, whereas H4 is tested without including variables associated with responsiveness. Following King and Roberts (2015), the dependent variable is log-transformed because of its high skewness.² While this transformation generates a normal distribution curve of the dependent variable, diagnostics of the OLS model still show presence of heteroscedasticity and autocorrelation. I employ a Prais-Winsten transformation to model autocorrelation and panel-corrected standard errors (PCSE), as per Beck and Katz (1995), which perform well when the number of years and panels are similar. Further diagnostics reveal that a country-fixed effect model may be preferred over a random effect model due to the potentially high heterogeneity between 27 different political systems, which raises the bar for confirming the theory (Wilson and Butler 2007, 106).

[Table 2 about here.]

The table shows that the full model provides the most promising results, suggesting that an account of State aid politics should indeed look at both sides of the coin. The first model finds no evidence for the responsiveness hypothesis. Parties in government do not seem to act out their policy programmes with regards to correction of market failures. If State aid is granted, it does not, on average, benefit the median voter. While Grossman and Helpman (1996) might suggest that the lack of responsiveness may be due to capture by special interest groups, no such claim can be inferred from this analysis. Another possibility is

that polarisation leads to a government composition that does not reflect the median voter (Powell 2009), which also highlights the limitation of the median voter theorem.

The table, however, suggests that this responsiveness may be conditional on the institutional environment in which policy-makers act. H2a, on the conditional effect of coalition partners on policy preferences, gives weight to the common-pool problem coalitions face (at least in the full model).³ This impinges not only on the partisan strength of the content of the policies as Hartmann (2014) suggests, but also on their output. However, less support in this sense is found for H2b, veto player theory ($p \approx 0.13$). In other words, the element that matters most is not so much the possibility of deadlock, but the necessity to compromise given limited resources. A higher number of effective coalition partners leads to a decline in the marginal effect of the preferred economic policy on subsidisation by approximately 2.6% for each additional coalition partner. The interactive effect can also be understood as a reduction by 2.6% of the marginal effect of the effective number of parties for each one-unit switch towards more positive attitudes for incentives. As the government becomes more in favour of economic incentives such as subsidies, the impact of a higher number of effective coalition partners decreases, suggesting that common pool problems are more severe when parties are unwilling to engage in distributive measures. This may happen, for instance, in times of fiscal retrenchment, where common pool problems may become more severe, as it is unlikely that the government will want to be profligate when there are scarce resources available.

[Figure B1 about here.]

Figure B1 shows that the marginal effect of *Economic Policy* and *Coalition* on aid allocation. The leftmost plot shows a positive marginal effect of *Economic Policy* for all values of *Coalition* until 3.8 (roughly four effective coalition partners), after which the effect becomes negative. However, the effect is significantly different from zero only for values of *Coalition* up to 1, which represent technical and single-party governments.⁴ This suggests, in line with theory, that single-party governments can better reflect the median voter preferences as there is no need for compromise with coalition partners. A different situation arises in the rightmost plot. Here, for the minimum value of *Economic Policy*, *Coalition* is positive but not significantly different from zero. As the government becomes more willing to engage in distributive measures, the effect of *Coalition* decreases and becomes significant for values of *Economic Policy* bigger than 2.2, which makes up roughly 56% of the observations. Hence, common resource pool problems seem to be less serious when there is more willingness on the part of the government as a whole to enact distributive policies.⁵

In terms of international constraints, H3 tests the conditional effect of the impact of Europeanisation on a government's willingness to engage in distributive measures. For each year since the Council Regulation

659/1999 came into force, the marginal effect of the government's policy goal in tackling market failures decreases by approximately 0.8%. The effect is rather small and the hypothesis has little generalisability beyond the EU setting. Nonetheless, its general premise, that international commitments can change the behaviour of responsive governments, holds. This can also be seen in the strong impact of the control variable *EMU*: having to comply with the Maastricht criteria on debt and deficit control reduces State aid allocation by almost 35% in the full model. Governments may be far less willing (or capable) to disburse aid knowing that they face strict limits on their deficit spending and on how much debt they may accumulate. The other side of the hypothesis would be that the marginal effect of State aid control on subsidy spending is lower as the government is more determined to engage in distributive measures. This seems to reflect Zahariadis's (2013, 148) power politics argument that 'aid allocations depend on a government's desire to give aid and its ability to get it past the Commission.'

[Figure B2 about here.]

Figure B2 shows the interactions between *Economic Policy* and *Regulation*. In the leftmost plot, the marginal effect of *Economic Policy* is always positive, though significantly different from zero only for the years where the Regulation is either absent (the 1990s) or still fairly new, for a total of 37% of the observations. The downward slope suggests that the marginal effect of *Economic Policy* is strongest when there is a low level of regulation, suggesting that State aid control does have a negative effect on the policy goals of governments.⁶ The majority of the observations, however, fall within a non-significant confidence interval, which impinges on the strength of the results as presented in the regression table. A similar situation obtains in the rightmost plot. The marginal effect of the Regulation on aid allocation is positive and statistically significant for values of *Economic Policy* lower than 3 (roughly 60% of the observations). As these values increase, the Regulation will negatively impact aid disbursement. The marginal effect of *Regulation*, then, is strongest for lower values of *Economic Policy*. This suggests, in line with Zahariadis (2013), that the willingness of a government to provide aid to domestic producers matters. However, the negative effect of *Regulation* cannot be likely distinguished from zero as the values of *Economic Policy* increase.⁷ The finding must therefore be interpreted cautiously, as we cannot say with enough confidence that the power politics argument always applies.

Finally, strong substantive correlation in H4, on the effect of electoral institutions, gives much weight to the accountability side of the coin. When district magnitude increases, the marginal effect of casting personal votes on aid allocation becomes positive, as there are more *incentives* for the incumbent to distinguish herself from competitors and therefore engage in particularistic spending. Likewise, politicians in bigger district will disburse more aid as the system shifts from party- to candidate-centred, as there is a *need* for the incumbent to distinguish herself from competitors. This effect also seems to be strong: in the full model, the presence

of incentives to cultivate a personal vote (higher district magnitude) leads to an increase in the marginal effect of district magnitude (personal reputation) on aid allocation by 51%. The finding is in line with the literature (Edwards and Thames 2007; Franchino and Mainenti 2013) and provides strong evidence that aid allocation may be driven by electoral pragmatism. However, no support is found regarding the existence of a PBC: politicians do not necessarily engage in subsidy spending to show commitment to their constituency in order to obtain pre-electoral surges.⁸

[Figure B3 about here.]

Figure B3 shows the marginal effects of *Personal vote* when *District Magnitude* changes and vice-versa. In particular, the leftmost plot shows that, as districts become bigger, if there are incentives to cultivate a personal reputation, this effect leads to more spending in State aid. The effect becomes positive only for multi-member districts, as single-member districts, stacked on the value of 0, are by definition candidate-centred, and the effect is therefore negative. The marginal effect is also significantly different from zero for all values that *District Magnitude* can assume, providing strong support to the second half of H4. Likewise, the rightmost plot shows that when systems shift from party- to candidate-centred, for higher levels of district magnitude, State aid is likely to be higher as politicians will have to distinguish themselves from other candidates and have more incentives to lobby the central government for particularistic transfers. The effect, however, is only statistically significant for low values of *Personal Vote*, roughly 23% of the observations. For high values of *Personal Vote*, the marginal effect of *District Magnitude* cannot likely be differentiated from zero. Hence, we can have higher confidence that in party-centred system the marginal effect of *District Magnitude* will be negative (as incumbents will curry favour to the party leadership rather than the constituents) than we could in the positive marginal effect of *District Magnitude* for candidate-centred systems. In sum, although the regression table shows significant effects in the interaction terms, the plots reveal a more ambiguous account than theory suggests.

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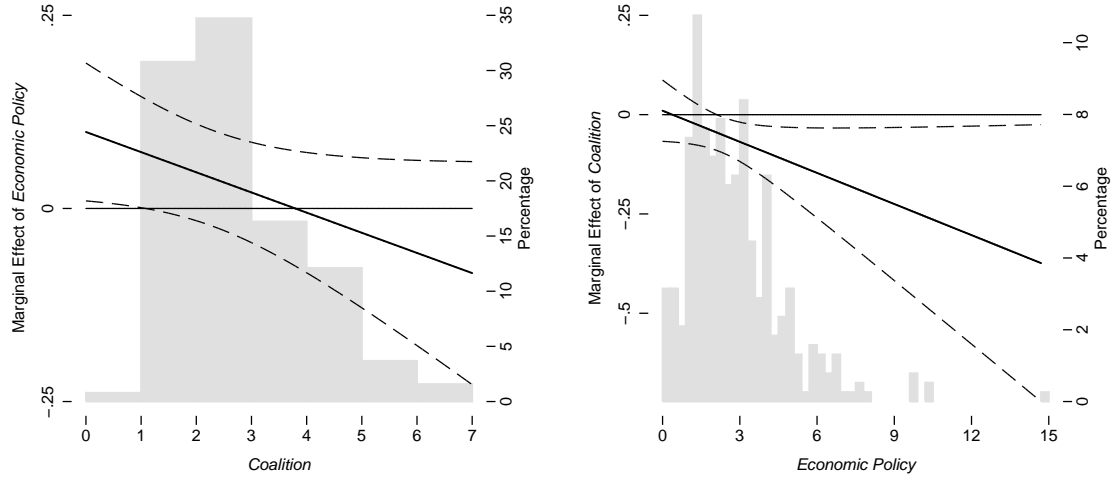


Figure B1: Marginal effect plots for H2a variables on State aid allocation

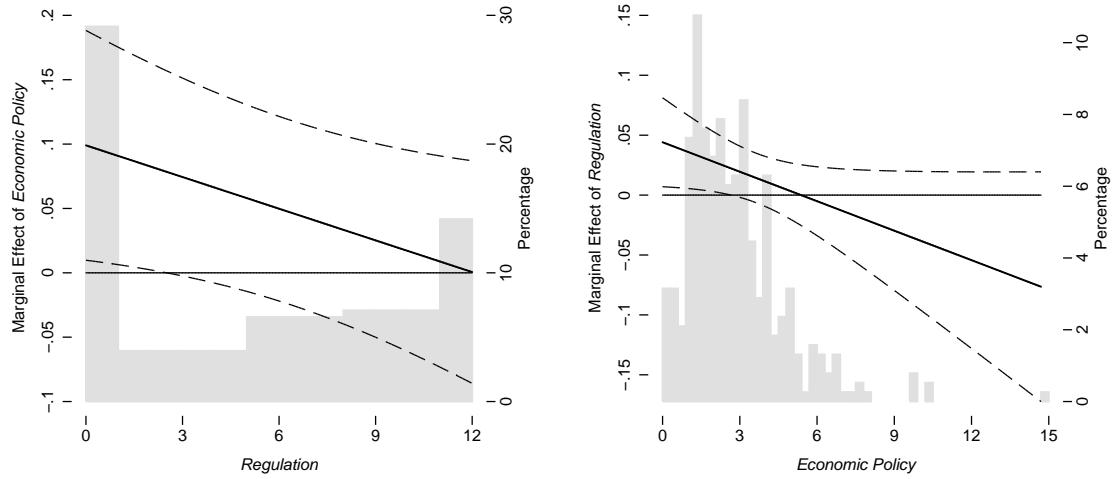


Figure B2: Marginal effect plots for H3 variables on State aid allocation

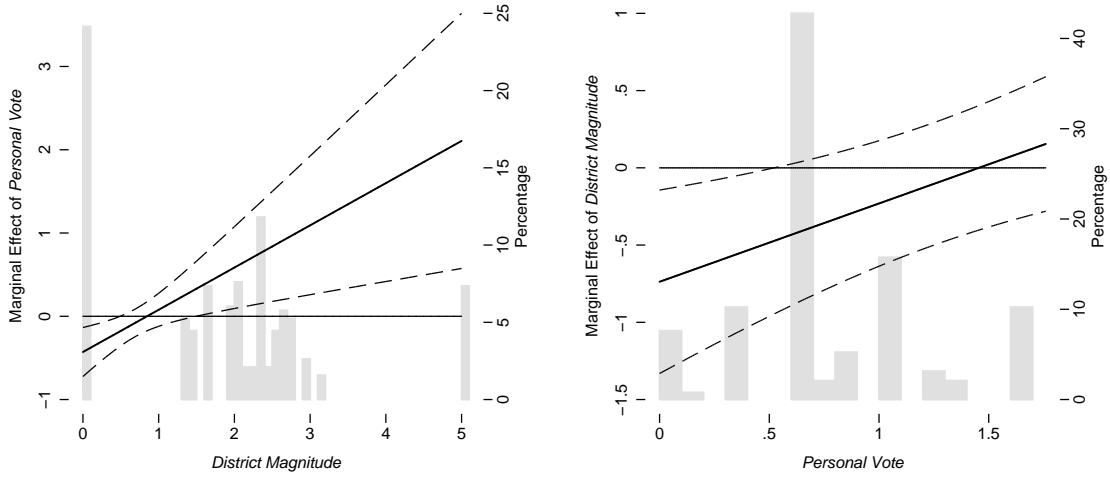


Figure B3: Marginal effect plots for H4 variables on State aid allocation

Table 2: State aid in the EU27 (1992-2011)

	(1)	(2)	(3)	(4)	(5)
	H1	H2a	H2b	H4	Full Model
Economic Policy	-0.027 (0.018)	0.053 (0.037)	0.031 (0.031)		0.099** (0.046)
Coalition	-0.040 [†] (0.025)	0.000 (0.040)			0.010 (0.039)
Veto Players	-0.007 (0.005)		-0.004 (0.007)		-0.002 (0.008)
Economic Policy x Coalition		-0.023 (0.015)			-0.026* (0.014)
Economic Policy x Veto Players			-0.002 (0.002)		-0.004 (0.002)
Regulation	0.013 (0.011)	0.041** (0.017)	0.038** (0.016)		0.044** (0.019)
Economic Policy x Regulation		-0.007* (0.004)	-0.007* (0.004)		-0.008* (0.004)
log(District Magnitude)				-0.413*** (0.143)	-0.429*** (0.150)
Personal Vote				-0.686** (0.297)	-0.738** (0.303)
log(District Magnitude) x Personal Vote				0.588*** (0.169)	0.507*** (0.180)
Real Economic Growth	-0.007 (0.006)	-0.009 [†] (0.006)	-0.008 (0.006)	-0.011* (0.006)	-0.008 (0.006)
Trade Globalisation	0.003 (0.006)	0.002 (0.006)	0.001 (0.006)	0.007 (0.006)	0.003 (0.006)
Financial Globalisation	-0.005 (0.005)	-0.006 (0.005)	-0.007 (0.005)	-0.001 (0.005)	-0.005 (0.005)
Timing of Election	0.002 (0.036)	-0.000 (0.036)	-0.002 (0.037)	-0.002 (0.037)	-0.005 (0.036)
EMU	-0.285*** (0.078)	-0.277*** (0.076)	-0.287*** (0.077)	-0.295*** (0.080)	-0.345*** (0.081)
Debt/GDP	-0.001 (0.002)	-0.002 (0.002)	-0.002 (0.002)	-0.001 (0.002)	-0.001 (0.002)
Unemployment	-0.004 (0.012)	-0.007 (0.012)	-0.006 (0.012)	-0.008 (0.012)	-0.008 (0.012)
Constant	-0.317 (0.435)	-0.342 (0.448)	-0.227 (0.443)	-0.521 (0.415)	0.100 (0.536)
Observations	380	380	381	381	380
R-squared	0.599	0.603	0.607	0.609	0.635
Country-fixed effects	Yes	Yes	Yes	Yes	Yes
Wald χ^2	43202***	14122***	39710***	43118***	6584***
ρ	0.439	0.443	0.430	0.424	0.412

Note: Prais-Winsten regressions with PCSE and pairwise selection; PCSE in parentheses;
*** p<0.01, ** p<0.05, * p<0.1, [†] p≈0.11.

Notes

¹When both lower- and higher-order coefficients are included, the statistical interpretation of the lower-order one(s) becomes meaningless (Braumoeller 2004).

²A generalised linear model with gamma distribution and the log link and a Poisson model, both with a non-transformed dependent variable and clustered standard errors, bear largely similar results (not reported here).

³In the second model, $p \approx 0.12$.

⁴The lower confidence interval crosses the zero line exactly for values of *Coalition* equalling 1. Even excluding technical governments in the regression analysis, the situation does not change.

⁵This could also be an effect of the distribution of the variable within the sample, which explains the large confidence intervals.

⁶The zero-line is reached for values of *Regulation* = 12.38, which is just outside the maximum value the variable can assume.

⁷Again, this could be an effect of the distribution of the variable within the sample, which explains the large confidence intervals.

⁸On the one hand, this could be a limitation of the data, which only includes aid allowed by the Commission, which could take months to process. On the other hand, Table A7 in Online Appendix 1, which uses OECD subsidies as the response variable, confirms this non-finding.