Online Appendix

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I undertake several steps to further check the robustness of the results.

First, I repeat the analysis using fiscal plans instead of episodes as the dependent variable (Appendix B).

Second, fiscal years begin and end at different times across countries (see the documentation in Alesina et al. 2020). In some cases, fiscal adjustments are packaged within the budget laws prepared in the Fall for the next year. Thus, I rerun the main models without lagging the independent variable (Appendix C).

In the reminder of the Online Appendix, I explore the issues of endogeneity and self-selection described in the paper (Section 7).

Appendix D further probes the robustness of the results using the recently proposed counterfactual estimators described in Liu et al. (2022). These estimators - and the associated *fect* R package - have three main advantages: first, early treatment adopters never serve as controls for late treatment adopters, thus avoiding the negative weights problem that affects fixed-effects and Diff-in-Diff standard estimation. This is important in light of the weaknesses of two-way fixed effects as underlined in the recent econometric literature;[1] second, it uses most available data without imposing stronger functional form assumptions, thus resulting in more efficient methods relative to alternative estimators that allow for the treatment to switch on and off multiple times, such as those in Imai et al. (2019) and De Chaisemartin and d'Haultfoeuille (2020); third, they allow for clear statistical routines to check the underlying assumptions. This latter point is particularly relevant since a violation of the underlying assumptions may be indicative of self-selection. Indeed, one may worry that the deterioration to public finances may have two effects: first, it would cause countries to implement fiscal consolidation packages in the periods $T-n$; and, then, it would cause the electorate to select a businessperson at time T. This, in turn, would constitute a violation of the parallel trend assumption and bias the estimates upward. As Tables 7-8 show, the results are robust to these more conservative estimators. Importantly, the placebo tests do not detect major violations of the underlying assumptions. The fundamental aspect of these tests relies on concealing certain periods of observations just before the treatment begins for the units being treated. Then, a model trained on the remaining untreated observations is used to anticipate the untreated outcomes for those withheld periods. If the assumptions for identification are valid, the average disparities between the observed and predicted outcomes during those periods should be minimal. The placebo F test is the usual test common to the diff-in-diff literature, with the null that the placebo effect is zero. All six models pass the placebo F test. As suggested in Liu et al. (2022), I also rely on a more

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[1] Kropko and Kubinec (2020); Goodman-Bacon (2021).

conservative placebo equivalence tests, which reverses the null hypothesis and checks whether the placebo effects is larger than a pre-specified equivalence threshold (I use the default threshold in the *Fect* R package). All three models where fiscal consolidation is the dependent variable reject the null. By contrast, we cannot reject the null that the effect is outside the equivalence range in the case of EB fiscal consolidation, although the *p*-values are not particularly large. At any rate, visual inspection of the dynamic treatment effects graphs (Figure 1-2-3) for the expenditure-based outcome does not suggest any serious parallel trend violation.[2]

In Appendix E, I follow Dreher et al. (2009) and investigate whether the two lagged fiscal variables - the debt-to-GDP and deficit-to-GDP ratios - help predicting when businesspoliticians will gain and remain in office. Evidence of a statistically significant association may suggest that businesspoliticians tend to be selected in office at the time when fiscal consolidation is most likely to occur. I control for all macroeconomic variables (also lagged) as in the main models, but do not control for the political variables since I am not aware of any argument suggesting that they should influence the type of politicians running for office. Since most countries in the dataset are parliamentary democracies with possibly endogenous elections, restricting the sample only to the election years would overlook the possibility that the macro-economy may affect businessleader's probability to *remain* in office. For this reason, Model 1 is run on the full country-year sample. Nevertheless, concerns about fiscal policy might be more salient during the election year. Hence, Model 2 is run only on the election year (hence, I do not include year fixed effects). All coefficients are statistically indistinguishable from zero. These results are in line with previous research at the country-level. For example, Dreher et al. (2009) show that profession and education of politicians is "almost idiosyncratic". The authors rationalize the results – somehow in contradiction with the literature on politicians selections at the legislative and sub-national level – pointing out that "voters can usually choose between only few candidates running for office only. They can thus not freely select a candidate with a certain profession and education, but only between profession and education of few opponents"(p.179). An alternative explanation might be that voters rely on occupational cues in the case of low-information (e.g. municipal), but not in high-information environment (e.g. national elections)[3]

Appendix F deals with the possibility that the effects of electing a business executive reflect the leader's political party affiliations rather than their business experience. First, I re-run the main models including an alternative indicator of leader's ideology borrowed from the recently published Global Leader Ideology dataset.[4] The author's coding of ideology has three distinct advantages over the standard DPI ideology score. First, it directly codes the leader's rather than the government's ideology - although, unsurprisingly, there is much overlapping in the two scores. Second, it contains fewer missing values. Third, it explicitly codes economic ideology (i.e. defined as the desired degree of state intervention in the economy). As it was the case of the DPI ideology score, the effect of business experience holds even after explicitly controlling for the leader's ideology. Second, I formally test for the relationship between ideology and business experience using a simple test of equality of proportions across ideological groups, which yields statistically

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[2]I do not show the equivalent graphs for fiscal consolidation since all models passed all tests. Notice that the first year of treatment does not yield a statistically significant positive effect in Figure 1-2-3. Indeed, a small cross-sectional dataset is unlikely to estimate the period-by-period ATT precisely enough. The coefficients in Table 7 and 8 are the period-by-period ATT averaged over all periods.

[3]Mechtel (2014).

[4]Herre (2022).

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insignificant results in both cases (p=0.6423 when using the DPI score; p=0.8899 when using the GLI ideology score). Appendix F shows the main models (Table 1 and 2 of the paper) using the GLI ideology score as well as the proportion of leaders with business experience across ideological groups (leftist, centrist, rightist). Across all specification, the results are substantively similar.

Appendix G further investigates the possibility that leadership change (and tenure duration) is endogenous to voters' fiscal preferences in the US case. Since the Second World War, polling companies have been asking a representative sample of Americans to identify the importance of dozens of potential issues, including "budget deficit". Unlike for most other countries, such surveys are available for long historical periods. The responses are commonly used to measures individuals' attentiveness to an issue - and, therefore, priorities for government action — in relation to all other issues.[5] If citizens' deficit-aversion incentivizes former businesspeople to run for office, their fiscal policy once in office may be due, at least in part, to the principal's rather than the agent's preferences. To investigate this possibility, I rely on the Most Important Problem Dataset (MIPD) put together by Heffington et al. (2019) for the 1940-2012 election cycles. I subset the sample to the year before the elections to explore whether the percentage of respondents identifying the budget deficit as the most important problem is correlated with a business-candidate running and/or winning the Presidency.[6] Given the abundance of biographical data in the US case, it is relatively straightforward to code the occupational experience of the top two US candidates, including the loser. In addition to the former US presidents with private sector experience, the following three losing candidates are coded as having business experience: Goldwater in 1964 (who run the family business in clothing and merchandising), Humphrey in 1968 (pharmacy owner), and Romney in 2012 (management and consulting industry).[7] Model 1 and Model 2 in Table 11 show the results at the survey-level (N=154). The dependent variable is a binary indicator capturing whether a business candidate runs for (Model 1) or wins (Model 2) the election cycle, while the independent variable captures the percentage of respondents who mentioned the budget deficit as the most important problem the country faces. I control for survey-specific (i.e. polling companies) fixed effects to account for the slightly different wording across surveys.[8] There is some evidence that an increase in societal worries about budget deficit incentivizes businesspeople to run for the Presidency. Nevertheless, the results are not very robust and mostly driven by Mitt Romney's decision to run for office in 2012, when worries about the budget deficit increases due to Obama's stimulus plan were at an all-time high. Indeed, re-running the models only on the winners (thus excluding also Romney) yields a null result.

Finally, a further implicit assumption of my theory concerns the leader of the executive's ability to influence fiscal policy in an effective manner. While Presidents/Prime Ministers are clearly in a prominent position relative to other actors, institutional and political contexts vary in the extent to which they can individually exert influence in fiscal policy making. Broadly speaking, an ancillary hypothesis of my theory can be derived and tested: businesspoliticians should be more likely to implement fiscal consolidation when they are less constrained by other actors. To test this hypoth-

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[5]Jennings and Wlezien (2015)

[6]Presidential candidates tend to announce their campaign one year and half before election day. See https://www.washingtonpost.com/politics/interactive/2022/president-candidate-announcement-timing/. Sub-setting by the election year does not substantively change the results.

[7]I focus on the top two candidates, hence Ross Perrot is not included. Including him among the business candidates in 1992 and 1996 does not substantively change the results.

[8]Results without survey fixed effects are substantially similar.

esis I re-run the full fixed-effects logit models interacting the business experience indicator with the level of government fractionalization. The fractionalization index comes from the *Database of Political Institutions* and captures the probability that two random policymakers from among the government parties will be of different parties. In principle, the index may take any value between 0 (single-party government) and 1 (every policymaker comes from a different party), although the maximum value in the sample is 0.83. As shown in Appendix H, the results by and large comport with the theoretical expectations. As the ability to influence the fiscal policymaking process is higher - for example, under a single-party government - the leader of the executive's preference in favor of fiscal consolidation is more likely to materialize. As the degree of government fractionalization increases, though, other political actors are likely to play a constraining role and the direct influence of the leader of the executive subdues. Somehow surprisingly, the point estimates probability of implementing an expenditure-based fiscal consolidation does not decrease as the level of fractionalization increases. Nevertheless, these estimates also become statistically indistinguishable from zero at higher levels of fractionalization. Overall, the pattern observed here is consistent with the idea that individual leaders can control the fiscal policymaking agenda.

Appendix A: Fiscal Consolidation Sizes

Table 1: Fixed Effects Models - Total Size

	Model 1	Model 2	Model 3	Model 4
Business (t-1)	0.045	0.092	0.009	0.027
	(0.079)	(0.083)	(0.085)	(0.097)
Macroeconomic controls	No	Yes	Yes	Yes
Political controls	No	No	Yes	Yes
Country FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	No	Yes
N	591.000	539.000	517.000	517.000
R^2	0.222	0.437	0.364	0.464

Clustered Standard Errors in parenthesis. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 2: Fixed Effects Models - Spending Cuts

	Model 1	Model 2	Model 3	Model 4
Business (t-1)	-0.004	0.045	0.014	0.018
	(0.049)	(0.047)	(0.049)	(0.055)
Macroeconomic controls	No	Yes	Yes	Yes
Political controls	No	No	Yes	Yes
Country FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	No	Yes
N	591.000	539.000	517.000	517.000
R^2	0.192	0.426	0.358	0.461

Clustered Standard Errors in parenthesis. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Appendix B: Fiscal Plans

Table 3: Logit Fixed Effects Models - Fiscal Plans

	Model 1	Model 2	Model 3	Model 4
Business (t-1)	0.550	0.942**	0.984***	1.308***
	(0.348)	(0.417)	(0.346)	(0.480)
Macroeconomic controls	No	Yes	Yes	Yes
Political controls	No	No	Yes	Yes
t, t^2, t^3	Yes	Yes	Yes	Yes
Country FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	No	Yes
N	623.000	571.000	549.000	538.000
Log Likelihood	-255.132	-195.951	-198.640	-165.710
AIC	542.264	423.901	429.280	363.421

Clustered Standard Errors in parenthesis. $^*\ p < 0.10$, $^{**}\ p < 0.05$, $^{***}\ p < 0.01$

Table 4: Logit Fixed Effects Models - EB Fiscal Plans

	Model 1	Model 2	Model 3	Model 4
Business (t-1)	0.420	0.963**	1.108***	1.342***
	(0.268)	(0.436)	(0.387)	(0.454)
Macroeconomic controls	No	Yes	Yes	Yes
Political controls	No	No	Yes	Yes
t, t^2, t^3	Yes	Yes	Yes	Yes
Country FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	No	Yes
N	528.000	482.000	517.000	462.000
Log Likelihood	-206.442	-155.422	-165.577	-138.958
AIC	442.884	340.845	361.154	307.917

Clustered Standard Errors in parenthesis. $^*\ p < 0.10$, $^{**}\ p < 0.05$, $^{***}\ p < 0.01$

Appendix C: Contemporaneous Independent Variable

Table 5: Logit Fixed Effects - Fiscal Consolidation - Business not lagged

	Model 1	Model 2	Model 3	Model 4
Business	0.785**	0.781**	0.826***	1.049*
	(0.316)	(0.391)	(0.305)	(0.631)
Macroeconomic controls	No	Yes	Yes	Yes
Political controls	No	No	Yes	Yes
t, t^2, t^3	Yes	Yes	Yes	Yes
Country FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	No	Yes
N	624.000	571.000	549.000	455.000
Log Likelihood	-285.195	-247.586	-221.512	-155.626
AIC	602.390	519.172	475.025	343.252

Clustered Standard Errors in parenthesis. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 6: Logit Fixed Effects - EB Fiscal Consolidation - Business not lagged

	Model 1	Model 2	Model 3	Model 4
Business	0.503**	0.629***	0.712***	0.996***
	(0.235)	(0.236)	(0.163)	(0.243)
Macroeconomic controls	No	Yes	Yes	Yes
Political controls	No	No	Yes	Yes
t, t^2, t^3	Yes	Yes	Yes	Yes
Country FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	No	Yes
N	573.000	527.000	549.000	507.000
Log Likelihood	-227.092	-177.894	-192.233	-160.569
AIC	486.183	387.789	416.466	353.139

Clustered Standard Errors in parenthesis. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Appendix D: Counterfactual Estimators and Placebo Tests

Table 7: Counterfactual Estimators - Fiscal Consolidation

	FEct	IFEct	MC
Business (t-1)	0.145**	0.111**	0.092**
	(0.069)	(0.044)	(0.043)
All controls	Yes	Yes	Yes
Country FE	Yes	Yes	Yes
Year FE	Yes	Yes	Yes
Placebo F test	0.463	0.853	0.523
Placebo equivalence test	0.028	0.022	0.051
N	623	623	623

Jackknife bootstrap standard errors. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

The placebo tests are run on the 2 periods before treatment.

Table 8: Counterfactual Estimators - EB Fiscal Consolidation

	FEct	IFEct	MC
Business (t-1)	0.098**	0.067**	0.067**
	(0.038)	(0.027)	(0.028)
All controls	Yes	Yes	Yes
Country FE	Yes	Yes	Yes
Year FE	Yes	Yes	Yes
Placebo F test	0.711	0.692	0.706
Placebo equivalence test	0.163	0.147	0.148
N	623	623	623

Jackknife bootstrap standard errors. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

The placebo tests are run on the 2 periods before treatment.

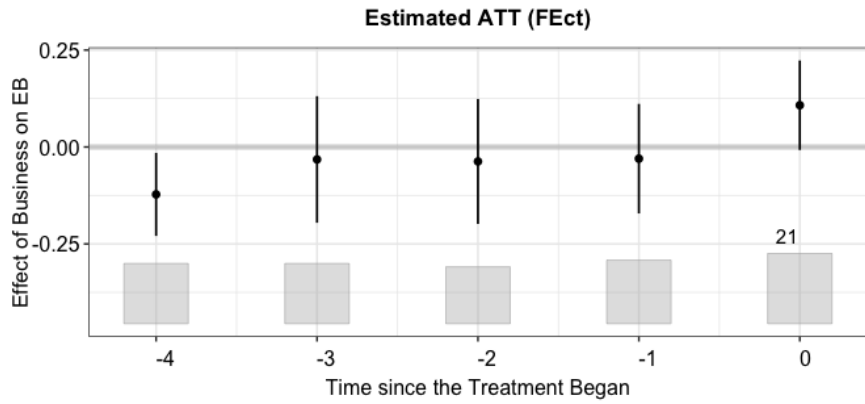Figure 1: Pre-treatment Visual Inspection - FEct

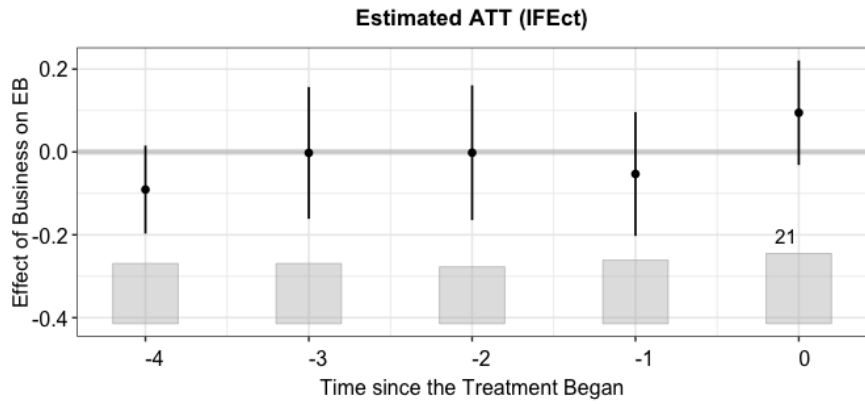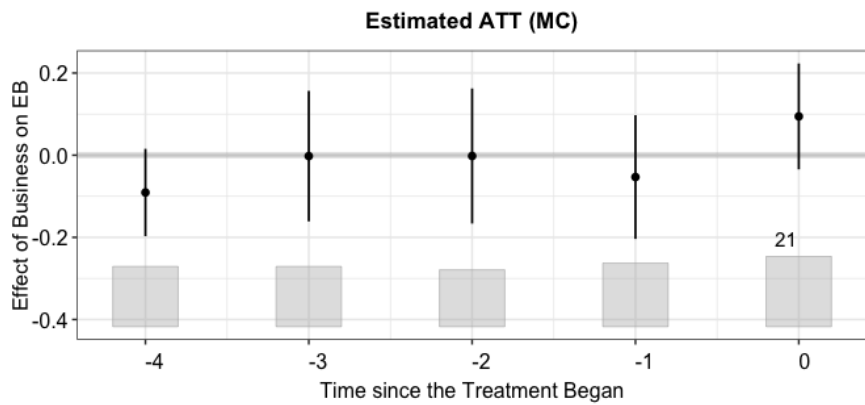Figure 2: Pre-treatment Visual Inspection - IFEct

Figure 3: Pre-treatment Visual Inspection - MC

Appendix E: Reverse Causality

Table 9: Logit Fixed Effects Models

	Model 1	Model 2
Deficit (% GDP) (t-1)	-0.006	-0.103
	(0.077)	(0.082)
Total debt (% GDP) (t-1)	0.003	0.013
	(0.012)	(0.008)
Macroeconomic controls	Yes	Yes
Country FE	Yes	Yes
Year FE	Yes	No
N	660	167
Log Likelihood	-330.549	-93.974
AIC	693.098	203.948

Clustered Standard Errors in parenthesis * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Appendix F: Ideology and Business Experience

Table 10: Logit Fixed Effects Models - Global Leader Ideology Score

	FC	FC	EB FC	EB FC
Business (t-1)	1.049***	1.327**	0.786***	0.960**
	(0.335)	(0.552)	(0.294)	(0.420)
All controls	Yes	Yes	Yes	Yes
Country FE	Yes	Yes	Yes	Yes
Year FE	No	Yes	No	Yes
N	547	547	547	516
Log Likelihood	-222.000	-194.785	-192.395	-161.801
AIC	475.999	421.570	416.790	355.601

Clustered Standard Errors in parenthesis * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Figure 4: Business Experience and Ideology
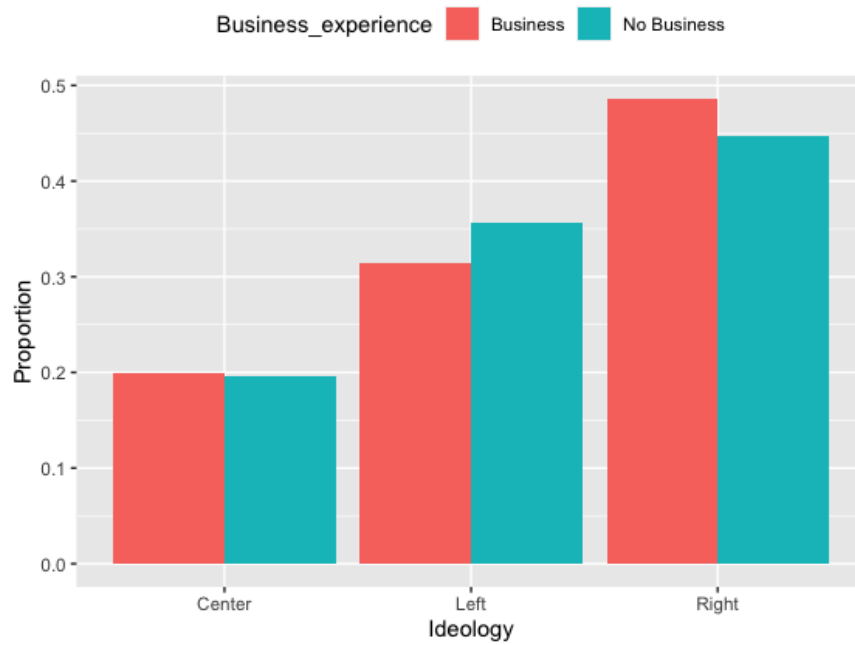
## Appendix G: US Business Candidates and the Electorate's Fiscal Preferences (1940-2012)

Table 11: Businesspoliticians and the Electorate's Fiscal Preferences in the US

	Business Candidate (1)	Business Elected (2)
Budget deficit as MIP	0.056***	0.007
	(0.009)	(0.008)
Survey Fixed Effects	Yes	Yes
R^2	0.304	0.080
Adj. R^2	0.243	0.001
Num. obs.	154	154

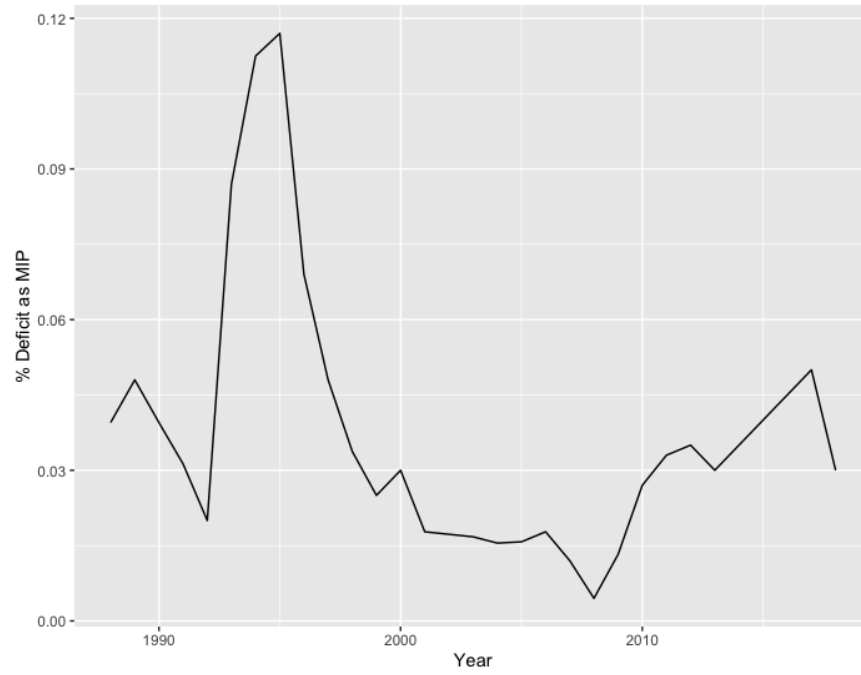Robust Standard Error in parenthesis. $^{***}p < 0.01$; $^{**}p < 0.05$; $^{*}p < 0.1$

Appendix H: Government Fractionalization and Business Leaders

Figure 5: The Conditional Effect of Government Fractionalization
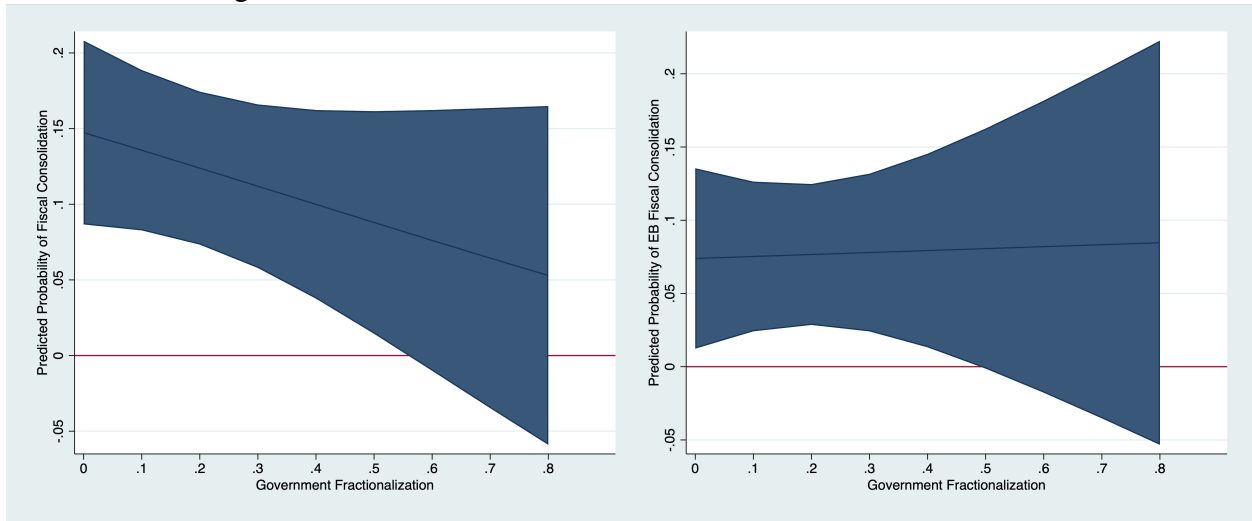
Appendix I: Budget deficit as the MIP in Canada (1988-2018)

Figure 6: Budget deficit as the MIP in Canada (1988-2018)

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