

Aggregate demand shortfalls and economic freedom

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Abstract Political instability is often exacerbated in periods of aggregate demand shortfall. It has been conjectured that inadequate policy responses to recessions may be inimical to free economic institutions. This paper uses the *Economic Freedom of the World* index as its measure of economic institutions, and finds that the change in economic freedom in the following five, ten, and fifteen years is negatively impacted by an aggregate demand shortfall as measured by negative NGDP growth.

Keywords Economic institutions · Voting behavior · Economic freedom · Macroeconomic political economy

JEL classifications D72 · E39 · P16

1 Introduction

Typically, advocates of free markets see inflation as a threat to economic freedom (Hayek 1973; Sennholz 1979). More recently, the reaction following the Great Recession has been a concern about hyperinflation, not insufficient aggregate demand (Laffer 2009; Meltzer 2014). However, informally, it has since been proposed that periods of insufficient aggregate demand may be detrimental to economic freedom (Sumner 2015:

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398; see also Christensen 2013; O'Brien 2013). In this paper, we operationalize this hypothesis and test it empirically.¹

The narrative attached to this hypothesis is that governments with market-oriented politicians err on the side of too little aggregate demand. When this becomes severe, voters react by punishing them and electing populist parties instead. Contrary to many narratives, the election of the Nazi party occurred in conjunction with a deflation, as the Weimar hyperinflation happened years prior. The Smoot-Hawley Tariff and the National Recovery Administration in the United States came about following deflation, not hyperinflation. Syriza seized power in Greece from the center-right party following failed aggregate demand policy under the European Central Bank. Initially, Sweden weathered the Great Recession well. However, after its central bank began to pursue “macroprudential” monetary policy, voters responded by putting the Social Democrats in power. Each of these are anecdotes; we supplement the narrative with data.

Higgs (1987) proposed that crises serve as opportunities for governments to grow above trend (with economic freedom receding), with it never returning to its previous trajectory. A recent literature has appeared exploring the empirical causes of changes in economic freedom, as opposed to using economic freedom as an explanatory variable. Among these are certain crises, including financial crises (de Haan et al. 2009; Bologna and Young 2015), war (O'Reilly and Powell 2015), migration (Clark et al. 2015; Hall 2015), as well as its relationship with several standard international explanatory variables (March et al. 2017). de Haan et al. (2009) is the closest to our analysis, as they find that output gaps have a negative impact on economic freedom; Rode and Revuelta (2015) find negative effects of populist politicians on economic freedom, a secondary component of the hypothesis. Additionally, Caplan (2003) has noted the connection between bad policies, bad growth, and bad ideas, including in the context of the aggregate demand failures of the Great Depression (196–197).

Regarding other relevant literature, our interpretation of our hypothesis and results can be viewed as an application of Entangled Political Economy (Wagner 2014). Instead of viewing the polity and the economy as two separate and distinct objects of study, Entangled Political Economy interprets them as jointly determined. Conventionally in Austrian Economics, this is thought of in terms of the dynamics and theory of intervention

¹ The precise mechanism by which this may occur is not developed in this paper explicitly beyond the description of the narrative. Sumner's description (2015: 398) follows, comparing policies in the Great Recession in America to recent day Argentina:

[Argentina] adopted the dollar peg in the early 1990s as a reaction to its experience with hyperinflation (recall 1920s Europe). And Argentina experienced deflation in the late 1990s and early 2000s as the real value of the dollar rose in the foreign exchange markets (recall the real appreciation of gold after 1929). And Argentina hung onto its dollar peg until the economic pain was so great that a new and more left-wing government decided to devalue its currency and tear up promises that debts would be convertible into dollars (recall FDR revoking the gold clause). And the left-wing government moved away from the neoliberal policies that they wrongly thought also contributed to Argentina's problems (recall the NIRA). And the powerful expansionary effects of the devaluation helped cover up the drag on the economy that normally would have resulted from the adoption of more statist policies (recall the U.S. recovery after 1933). The point is often overlooked; if depressions do encourage statist policy interventions, then deflationary policies may impose costs that are much larger than those predicted by natural rate models of the business cycle.

In a footnote, he continues,

The extraordinarily large and wasteful public works expenditures undertaken during the recent Japanese deflation are another example of this phenomenon. The most dramatic example of nominal shocks leading to harmful policies is World War II, which might have been avoided had the Great Depression not occurred.

(Mises 1929 [Mises 2011]; Ikeda 1997), i.e., how the failure of one intervention leads to calls for still further intervention. A simple narrative may help illustrate the similarities and differences between our hypothesis and this conventional presentation of the dynamics of intervention. The conventional story may posit that the government may fail to perform a function it sets out to perform, for example the incorrect placement of roads, leaving the roads quickly covered in potholes, and charging prices for the roads which do not reflect economic realities. This initial failure in intervention will lead to further calls for intervention in the transportation system of a community; hence continual calls for further intervention. In comparison, the theoretical mechanism here is akin to the failure to fill potholes leading voters to remove relatively competent politicians from office and replacing them with those extremely antagonistic toward the market economy.

As mentioned, the closest earlier work was de Haan et al. (2009), which considered the effects of the output gap on economic freedom. We differ in that we explicitly look at a nominal variable, Nominal Gross Domestic Product, as our indicator for the stance of monetary policy and aggregate demand. For our purposes, we assume that the central bank has complete control over all nominal variables, an assumption which holds so long as the liquidity trap either is not binding or is untrue empirically. We then interpret our results as indications of the effects of central bank policy. Should this assumption not hold, our results may instead be interpreted as the effects of aggregate demand more broadly.

To examine this empirically, we employ the *Economic Freedom of the World* index by the Fraser Institute (Gwartney et al. 2015). In our baseline results, an event of negative NGDP growth has a negative effect on economic freedom five years, ten years, and fifteen years following the crisis period. In the specifications where we employ our control variables, year fixed effects, and country fixed effects, it reduces economic freedom by 0.11–0.19 standard deviations with the effect increasing in time. In these specifications, the effect is statistically significant at the 99% level.

For the purposes of tractability, certain stylizations of Sumner's argument as well as economic reality were implemented. These stylizations will not always be true. Politicians with a favorable view of markets are not the only politicians to have ever governed during a time of negative NGDP growth. Central banks are not always directed to target nominal variables, and when they are, they do so with highly imperfect information. It is also less clear what the consequences of a shortfall would be under purely free monetary arrangements. However, these issues do not concern the econometric tests of the hypothesis, or the hypothesis itself. And while the hypothesis is in need of stylization to be readily applicable to a dataset, it is theoretically coherent. Therefore, while we note these caveats, they do not discredit the purpose or results of the paper.

The structure of this paper follows. In Section II, we discuss our method in greater detail and provide the sources of our data. Section III provides the baseline results. In Section IV, we perform two robustness checks, once by removing the monetary component from our economic freedom measure and secondly by substituting trade openness for economic freedom. Section V concludes.

2 Data and method

Our measure of economic institutions is the *Economic Freedom of the World* (EFW) report published by the Fraser Institution (Gwartney, Lawson, and Hall 2015). It has

been used extensively elsewhere (Hall and Lawson 2014) and is comprised of five components (“areas”), Size of Government, Legal System and Property Rights, Sound Money, Freedom to Trade, and Regulation. The index uses 42 variables from outside sources, each scaled 0–10, with several variables used to measure each of the five areas of economic freedom. Our controls are the Polity IV dataset, which measures the quality of democracy (Marshall et al. 2014), logged real GDP per capita, PPP adjusted from Penn World Tables (Heston et al. 2012), and human capital from the Barro and Lee (2013).

There are numerous ways of operationalizing the hypothesis that NGDP shortfalls harm institutions. We focus on one, given the nature of the EFW dataset. From 1970 to 2000, the data is in five year increments. Following 2000, it is yearly. To make all comparisons truly apples-to-apples, we start with any year we have data available for EFW in the given year t . We then look back to years t to $t-4$. If, for any year in that interval, NGDP growth was negative, we assign the year t a score of one. Otherwise, it gets a zero. This also captures the spirit of the hypothesis, that an aggregate demand shortfall triggers an expulsion of political incumbents down the line.

Subsequently, we look at the *change* in EFW over the following five, ten, and fifteen years. Again, this was done in part due to data constraints, but it also fits with the hypothesis, as the NGDP shortfall triggers political changes that take time. There is also another methodological benefit to looking at these long run changes. The most recent data point we have is NGDP shortfalls in 2008 impacting economic freedom in 2013, the latter being the most recent data. In other words, our test implicitly excludes our two primary motivating examples, the Great Recession and Nazi Germany, from the sample. If the most extreme historical occurrences of the phenomenon are not included and we still achieve results, it adds to the power of the result.

While we may refer to instances of deflation and inflation in passing as shorthand for failures of aggregate demand management, we do not intend, for instance, to argue that every episode of price level deflation would have negative consequences (c.f. Bordo et al. 2009). As operationalized here, in fact, only rather drastic shortfalls in aggregate demand are marked as failures in our dataset. This definition of an aggregate demand shortfall is not a weak one and corresponds to F.A. Hayek’s original position (Hayek 1931; see White 1999); they would result in “bad deflation.” The intention is to test the hypothesis that aggregate demand shortfalls impair economic institutions, not that deflation itself does so. For shorthand, at times we will use “AD shortfall” and “NGDP shortfall” synonymously throughout the paper.

One objection to our method is that one fifth of EFW is “Sound Money.” While one could argue that long differences reduces this concern, Section IV excludes Sound Money to measure to what extent it drives the results. Also in Section IV, we will use Trade Openness, which we calculated by adding Imports and Exports as percentages of GDP from World Development Indicators.

Descriptive statistics for all variables are provided in Table 1. Equations 1 through 3 provide our baseline models. γ_i denotes country fixed effects and δ_t denotes year fixed effects. *ADShortfall* denotes the aforementioned dummy variable for negative NGDP growth between years $t-4$ and t . $\varphi_{i,t}$ is the vector of control variables. For each of these models, this is a final specification; in

providing the results, we provide both these and specifications with and without the controls variables and separately with and without fixed effects.

$$EFW_{i,t+5}-EFW_{i,t} = \beta_0 + \beta_1EFW_{i,t} + \beta_2ADShortfall_{i,t} + \beta_3\varphi_{i,t} + \beta_4\gamma_i + \beta_5\delta_t + \varepsilon \tag{1}$$

$$EFW_{i,t+10}-EFW_{i,t} = \beta_0 + \beta_1EFW_{i,t} + \beta_2ADShortfall_{i,t} + \beta_3\varphi_{i,t} + \beta_4\gamma_i + \beta_5\delta_t + \varepsilon \tag{2}$$

$$EFW_{i,t+15}-EFW_{i,t} = \beta_0 + \beta_1EFW_{i,t} + \beta_2ADShortfall_{i,t} + \beta_3\varphi_{i,t} + \beta_4\gamma_i + \beta_5\delta_t + \varepsilon \tag{3}$$

These three models are the backbone of our methodology. When we later perform robustness checks, we difference and control at the beginning of the period analogously. For all specifications, all possible country-years are employed. Generally, the binding constraint is the EFW index, which limits the time dimension to 1970 and in earlier years has limited country coverage.

3 Results

Tables 2, 3 and 4 provides are baseline results. The tables are sorted by LHS variable, with Table 2 providing results for the five year difference, followed by the ten and fifteen year differences. Regressions 1, 5, and 9 give a simple specification, with only the initial level of EFW controlled for. Regressions 2, 6, and 10 introduce year and country fixed effects. Regressions 3, 7, and 11 include the controls but no fixed effects. Regressions 4, 8, and 12, which are our headline results, include both the controls and the fixed effects.

The results are not always statistically significant without fixed effects. However, upon their inclusion and the inclusion of our control variables, *AdShortfall* achieves statistical significance at the 95% level. The individual

Table 1 Descriptive Statistics

Variable	Obs	Mean	Std. Dev.	Min	Max
5 Yr. Δ Economic Freedom	1662	0.168	0.53	-2.3	3.0
10 Yr. Δ Economic Freedom	1052	0.430	0.80	-2.9	4.0
15 Yr. Δ Economic Freedom	564	0.896	1.05	-2.3	5.0
AD Shortfall	2594	0.207	0.41	0.0	1.0
Initial Economic Freedom	1662	6.339	1.26	1.8	9.2
Economic Freedom Less Area 3	1655	6.075	1.22	2.1	9.1
Polity IV	2024	2.474	7.05	-10.0	10.0
Human Capital	1488	6.703	2.98	0.3	12.7
Ln Real GDP per capita	2681	8.479	1.33	5.1	11.6
Trade Openness	2508	85.223	51.56	0.3	439.7

point estimates are small but not economically insignificant. In the five year difference, an NGDP shortfall reduces EFW by 0.11 standard deviations. This increases up to 0.19 standard deviations in the fifteen year difference. These coefficients are not statistically distinguishable, however.

4 Robustness checks

For our robustness checks, we change our economic freedom variable. First, to answer the question as to whether the measure of economic freedom is simply a function of monetary policy, we remove the “Sound Money” area and recalculate the index. Secondly, we use trade openness as an imprecise proxy for economic freedom. These regressions all employ our control variables, year fixed effects, and country fixed effects. We replicate the previous models closely, replacing the control for economic freedom at the beginning period with economic freedom less sound money and trade openness at the beginning period, respectively.

Table 5 provides these results. Regressions 13–15 correspond to removing Sound Money from economic freedom. The result hold strongly for changes in economic freedom over five years and ten years. It loses significance ($t = -1.32$) but maintains sign and magnitude over fifteen years. This check may be interpreted as providing evidence in favor of the interpretation of we are measuring distantly lagged effects on monetary policy, though this would still be negative for economic freedom. Regardless, this issue only arises at the most distant of the time horizons.

Regressions 16–18 correspond to trade openness and do not support the hypothesis. Coefficients are imprecisely estimated, statistically insignificant, and in any case their point estimates correspond to less than 5% of a standard deviation in trade openness. Charitably, this suggests that the manner in which

Table 2 5 Year Regressions

Regression	1	2	3	4
LHS	5 Yr. Δ	5 Yr. Δ	5 Yr. Δ	5 Yr. Δ
AD Shortfall	-0.031 (0.035)	-0.115** (0.054)	-0.008 (0.054)	-0.141** (0.066)
Economic Freedom	-0.144*** (0.012)	-0.401*** (0.045)	-0.214*** (0.025)	-0.405*** (0.038)
Polity IV			0.014*** (0.004)	0.021*** (0.008)
Human Capital			0.045*** (0.011)	0.069* (0.037)
Ln Real GDP per capita			-0.056** (0.025)	-0.549*** (0.148)
Constant	1.083*** (0.085)	1.835*** (0.240)	1.658*** (0.164)	5.950*** (1.167)
Country + Year Fixed Effects	N	Y	N	Y
R-Squared	0.12	0.20	0.18	0.16
n	1617	1617	985	985

Table 3 10 Year Regressions

Regression	5	6	7	8
LHS	10 Yr. Δ EF	10 Yr. Δ EF	10 Yr. Δ EF	10 Yr. Δ EF
AD Shortfall	-0.117* (0.061)	-0.139 (0.093)	-0.052 (0.080)	-0.213** (0.098)
Economic Freedom	-0.279*** (0.021)	-0.736*** (0.071)	-0.465*** (0.037)	-0.784*** (0.049)
Polity IV			0.020*** (0.005)	0.023*** (0.009)
Human Capital			0.078*** (0.015)	0.109* (0.058)
Ln Real GDP per capita			-0.019 (0.035)	-0.807*** (0.220)
Constant	2.143*** (0.140)	3.946*** (0.350)	2.870*** (0.221)	10.282*** (1.755)
Country + Year Fixed Effects	N	Y	N	Y
R-Squared	0.22	0.28	0.35	0.23
n	1010	1010	793	793

an aggregate demand shortfall harms economic freedom does not do so in a way that fundamentally harms its level of trade. But this too may cut against the narrative discussed earlier in the paper. If populist politicians are to blame, they do not have an economically significant impact on the globalized structure of their nation’s economy.

Appendix A additionally contains analogous regressions for each individual area of the *Economic Freedom of the World* index. The point estimate of the coefficient in all fifteen regressions is actually negative. The coefficient of Area 3, Sound Money, is negative but only even borderline statistically significant in the fifteen year difference.

Table 4 15 Year Regressions

Regression	9	10	11	12
LHS	15 Yr. Δ EF	15 Yr. Δ EF	15 Yr. Δ EF	15 Yr. Δ EF
AD Shortfall	-0.388*** (0.115)	-0.284*** (0.090)	-0.131 (0.122)	-0.245** (0.101)
Economic Freedom	-0.402*** (0.031)	-0.109 (0.076)	-0.705*** (0.047)	-1.106*** (0.053)
Polity IV			0.017** (0.008)	0.018* (0.010)
Human Capital			0.144*** (0.028)	0.079 (0.090)
Ln Real GDP per capita			0.052 (0.065)	-0.934*** (0.261)
Constant	3.200*** (0.200)	5.962*** (0.388)	3.584*** (0.407)	13.327*** (2.138)
Country + Year Fixed Effects	N	Y	N	Y
R-Squared	0.27	0.33	0.49	0.23
n	525	525	412	412

Table 5 Robustness Checks

Regression	13	14	15	16	17	18
LHS	5 Yr. Δ less Area 3	10 Yr. Δ less Area 3	15 Yr. Δ less Area 3	5 Yr. Δ Trade Openness	10 Yr. Δ Trade Openness	15 Yr. Δ Trade Openness
AD Shortfall	-0.164*** (0.051)	-0.239** (0.092)	-0.119 (0.090)	2.482 (2.261)	-2.420 (2.671)	2.010 (3.883)
Economic Freedom less Area 3	-0.526*** (0.039)	-0.861*** (0.050)	-0.995*** (0.052)			
Trade Openness				-0.593*** (0.086)	-0.897*** (0.082)	-0.874*** (0.102)
Polity IV	0.019*** (0.007)	0.023*** (0.008)	0.013 (0.009)	-0.220 (0.167)	-0.178 (0.252)	-0.358 (0.286)
Human Capital	0.060* (0.134)	0.076 (0.051)	0.031 (0.082)	1.598 (2.044)	1.310 (3.411)	1.059 (3.828)
LN Real GDP per capita	-0.429*** (0.134)	-0.404*** (0.202)	-0.427** (0.178)	7.361 (5.207)	10.912 (7.737)	10.608 (10.101)
Constant	5.628*** (1.081)	7.504*** (1.702)	8.655*** (1.543)	-25.757 (45.971)	-27.998 (69.143)	-30.691 (86.476)
Country and Year FE	Y	Y	Y	Y	Y	Y
R-Squared	0.21	0.33	0.35	0.01	0.00	0.00
n	982	790	409	930	499	401

The largest of the calculated point estimates is -0.914 in the ten year difference of Area 5, Regulation, but from the other estimates for Regulation it is not clear that the greatest impact really is on Area 5. Perhaps surprisingly, the negative effects of an aggregate demand shortfall on economic freedom may be general, not at all specific.

5 Conclusion

As a first approximation, serious shortfalls in aggregate demand harm free economic institutions. In our complete specifications with controls, year fixed effects, and country fixed effects, the effect of shortfalls varies from 0.11 standard deviations over five years to 0.19 standard deviations over fifteen years as measured by the *Economic Freedom of the World* index. We find this despite our estimation technique necessarily excluding the historical episodes motivating our research. The effect is fairly robust upon exclusion of monetary variables from the index (with caveats), but is not robust if economic institutions are thought of in terms of trade openness.

Alternative hypotheses may be raised to explain these findings. Our econometric results only operationalize and test the implications of one particular narrative about the interplay between economic liberalization, monetary policy, voters, and practical politics. We did not test the precise narrative and mechanism, only their implications. These implications hold under the baseline approximations. However, the narrative itself sits less well with the uneven results found in the robustness checks. If poor monetary decisions lead to populism and inwardness, it does not show up in the trade data. Perhaps this is merely because levels of trade are dominated by variables besides marginal changes in economic policy, but perhaps not. In any case, the predicted

empirical regularity is present in the data, which calls out for explanation even if our underlying narrative is flawed or incomplete.

APPENDIX A. Regression Results by Area of *Economic Freedom of the World*

Table 6 Area 1

Regression	19	20	21
LHS	5 Yr. Δ Area 1	10 Yr. Δ Area 1	15 Yr. Δ Area 1
AD Shortfall	-0.119 (0.117)	-0.217 (0.140)	-0.075 (0.143)
Initial Area 1	-0.568*** (0.045)	-0.915*** (0.044)	-1.139*** (0.043)
Polity IV	-0.011 (0.010)	0.006 (0.013)	0.015 (0.012)
Human Capital	0.151** (0.063)	0.173* (0.100)	0.149 (0.136)
Ln Real GDP per capita	-0.431** (0.202)	-0.359 (0.302)	-0.675* (0.359)
Constant	5.684*** (1.623)	6.668*** (2.427)	10.504*** (2.850)
Adjusted R-Square	0.22	0.27	0.04
n	990	806	441

Table 7 Area 2

	Column 1	Column 2	Column 3
Regression	22	23	24
LHS	5 Yr. Δ Area 2	10 Yr. Δ Area 2	15 Yr. Δ Area 2
AD Shortfall	-0.156 (0.116)	-0.308** (0.131)	0.006 (0.142)
Initial Area 2	-0.649*** (0.051)	-0.891*** (0.046)	-1.105** (0.522)
Polity IV	0.019 (0.012)	0.006 (0.012)	-0.014 (0.014)
Human Capital	0.029 (0.083)	-0.071 (0.090)	-0.166 (0.125)
Ln Real GDP per capita	0.193 (0.244)	0.054 (0.331)	-0.276 (0.323)
Constant	4.481 (1.992)	4.676* (2.655)	9.091*** (2.507)
Adjusted R-Square	0.21	0.15	0.11
n	925	741	376

Table 8 Area 3

Regression	25	26	27
LHS	5 Yr. Δ Area 3	10 Yr. Δ Area 3	15 Yr. Δ Area 3
AD Shortfall	-0.196 (0.167)	-0.283 (0.211)	-0.602* (0.312)
Initial Area 3	-0.381*** (0.045)	-0.706*** (0.062)	-1.103*** (0.077)
Polity IV	0.041** (0.021)	0.058* (0.030)	0.069** (0.034)
Human Capital	0.110 (0.107)	0.222 (0.175)	0.325 (0.236)
Ln Real GDP per capita	-0.601* (0.314)	-1.239** (0.499)	-1.966*** (0.650)
Constant	6.124** (2.501)	13.202*** (3.863)	21.512*** (4.990)
Adjusted R-Square	0.14	0.20	0.28
n	1005	821	456

Table 9 Area 4

Regression	27	28	29
LHS	5 Yr. Δ Area 4	10 Yr. Δ Area 4	15 Yr. Δ Area 4
AD Shortfall	-0.240** (0.119)	-0.383** (0.185)	-0.361 (0.286)
Initial Area 4	-0.447*** (0.049)	-0.769*** (0.075)	-0.983*** (0.050)
Polity IV	0.035** (0.015)	0.049*** (0.018)	0.026 (0.021)
Human Capital	0.108 (0.068)	0.233** (0.110)	0.236 (0.175)
Ln Real GDP per capita	-0.754*** (0.281)	-1.103*** (0.425)	-1.134** (0.492)
Constant	8.211***	12.204***	15.264***
Adjusted R-Square	0.21	0.34	0.37
n	912	740	399

Table 10 Area 5

Regression	30	31	32
LHS	5 Yr. Δ Area 5	10 Yr. Δ Area 5	15 Yr. Δ Area 5
AD Shortfall	-0.094 (0.077)	-0.914*** (0.116)	-0.082 (0.142)
Initial Area 5	-0.549*** (0.047)	-0.819*** (0.063)	-1.130*** (0.071)
Polity IV	0.008 (0.008)	0.012 (0.010)	-0.023** (0.011)
Human Capital	-0.040 (0.043)	-0.033 (0.066)	-0.132* (0.077)
Ln Real GDP per capita	-0.373 (0.315)	-0.311 (0.421)	-0.337 (0.420)
Constant	6.100** (2.647)	7.239** (3.548)	9.719*** (3.628)
Adjusted R-Square	0.17	0.25	0.22
n	937	753	388

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