

Fiscal Rule Stretching in Europe During Financial Market Stress and Crises

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Abstract

Elected governments have incentives to stretch accounting rules by classifying loss-making and/or indebted endeavors, such as public companies and pension schemes, as off of the public balance sheet. Doing so improves the appearance of the incumbent government to cost-conscious voters and fiscally important international institutions. We expect rule stretching to be especially prevalent during periods of financial market stress and crisis given the large expense of responding to these events. In addition, policy options available to aid troubled financial institutions, such as liquidity assistance, bad banks, and bank nationalizations, are often hard to classify as being inside or outside of the government sector, thus making it more likely that governments will stretch how they are classified. To test these propositions, we examine revisions to government debt and deficit figures made by the European statistical agency—Eurostat. These revisions frequently occur because this politically independent agency re-classifies member state policies and organisations as being within the government sector, when a national government had originally classified them as outside. We find that debt figures are more likely to be revised upwards for years close to national elections, especially when unscheduled. Such election effects are heightened further by financial market stress. Our research underlines the importance of having

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a vigilant and strongly politically independent government statistical agency during periods of financial market stress to ensure reliable government finance statistics.

1 Introduction

This paper examines why governments stretch the rules when they determine how policies impact their debts and deficits, especially during periods of financial market stress and turmoil. By “rule stretching” we mean that when the fiscal implications of a policy are potentially ambiguous, a decision is made to minimise how it affects the public balance sheet. This research question affects other literatures in political science in interesting ways. One strand examines the relationship between the reporting of data and the quality of governance (e.g. Hollyer, Rosendorff and Vreeland, 2014). Alt, Lassen and Wehner (2014) explore the relationship among the transparency of fiscal data, elections, and pressure from the European Union. They argue that European Union member states were more likely to violate the Eurostat statistical agency’s rules for reporting budget data when fiscal transparency was low and when it was an election year in ways that made the government look better to voters. A related strand focuses on the economic vote and whether or not voters notice or incorporate information about these revisions into their decisions. Kayser and Leininger (2015) find that the press, and voters as a result, do not pay attention to revised figures.

Our paper builds on insights on data revisions by Kayser and Leininger and on elections and fiscal transparency by Alt, Lassen, and Wehner to explore the implications of their arguments for government action. If voters care most about reported, rather than actual, figures then governments have an incentive to manipulate them before elections. One would expect greater manipulations in ways that make the government’s stewardship of the public balance sheet look better as elections approach. There is also an indirect electoral channel that runs through international institutions and investors for some countries. Especially in the European Union where members’ budgets are constrained by the Stability and Growth Pact, fiscal rule stretching in one policy area allows governments to not have to cut back as much in others areas voters care about in order to meet internationally agreed commitments. Countries that rely on foreign sovereign bond investors may similarly rule stretch in order to maintain an attractive balance sheet for foreign sovereign bond investors, thus allowing them to fund programs that voters want.

During a financial crisis, governments’ electorally motivated fiscal rule-stretching is potentially even more pronounced. Financial crises substantially strain public budgets as economic activity and so tax revenue decrease while bank bailout policies incur significant expenses. At the same time, many tools commonly

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used to respond to financial crises—guarantees, liquidity assistance, public asset management companies—have potentially ambiguous fiscal implications. So, during a financial crisis politicians have even stronger incentives and potentially capabilities to rule-stretch around elections.

We can measure these activities by looking at revisions of budget figures, and in particular debt.

We also explore two main alternative hypotheses. The first alternative focuses on “shocks” to the economy, which in turn affect government statistics. Countries with more shocks may engage in more revisions, with the bias depending upon the nature of the shocks (positive or negative). One can expect that the shocks are symmetrical, where the greater the shock the more the potential revision, or that one direction requires more revisions, with the relative elasticities of taxes making one direction more volatile than another. This would be the standard “economic” argument. These revisions, however, should be due to “economic” effects and not conscious government manipulation. The forecasting literature in economics includes such measures. The second one considers institutional arguments about who generates the numbers. In some countries, the government does the reporting, while in others an independent body plays this role. Of course, these three arguments may be related in theoretically interesting ways. For example, a country experiencing a positive shock may be more likely to call an election in countries where elections are unscheduled.

Our dataset is composed only of European Union member states. This is useful for several reasons. They face one international organisation that has two sets of rules on economic matters for its members that are often closely related, namely those rules for Member States inside the eurozone and those outside, which are nevertheless the same when it comes to the reporting of the data, though incentives to follow the rules differ. Moreover, there is one body that can provide comparative data that we can analyse. While fiscal figures are initially generated by national agencies, as part of Stability and Growth Pact (SGP) deficit and debt limit enforcement all Member States face one set of accounting rules (the European System of Accounts or ESA) for data they report to the EU and they have one body, Eurostat, that ultimately enforces it. Eurostat regularly revises Member State data so that it conforms to the common rules. This means that differences across countries in initial reporting are not due to different accounting rules, but instead often different interpretations of these rules. Finally, they are all parliamentary or semi-presidential systems where elections are crucial for the formation of governments.

We find that Member State debt figures are more likely to be revised upwards for years close to national elections, especially when these are unscheduled. This suggests that governments are more prone to rule stretch closer to elections. They tend to “low ball” how much policies will cost closer to elections thus leading to an upward Eurostat revision in later years. Such election effects are strengthened further by financial

market stress. Our research underlines the importance of having a vigilant and politically independent government statistical agency during periods of financial market stress to ensure reliable government finance statistics.

How do
we want
to handle the
deficit
findings?

2 Political budget cycles and fiscal gimmicks

The revision of government-reported statistics is of interest for several reasons.

The reporting of statistics, or the lack of it, may tell one about the overall governance of a given country. Hollyer, Rosendorff and Vreeland (2014) argue that the lack of reporting is not random. They record the availability of data for 125 countries over a thirty-year period. They analyse this data with Bayesian Item Response model to create a transparency index. They then use this index to predict the quality of governance in autocracies. In other work, they anticipate that this type of transparency affects government accountability, as well as collective action (Hollyer, Rosendorff and Vreeland, forthcoming).¹

While Hollyer, Rosendorff and Vreeland (2014) consider how voters might react under conditions when they get data and when they do not, there is a separate literature on the economic vote that assumes that voters have no trouble accessing regular data on the state of the economy and they use this to decide about the competence of the government in managing economic policy. One debate considers whether voters are prospective or retrospective. On the latter, the assumption is that voters observe outcomes, and they decide whether to support the incumbent government. In response to the strong assumptions about voter knowledge, there is a growing literature on “real-time fiscal policy”. Kayser and Leininger (2015) argue that the press ignores revisions to data. Moreover, Kayser and Peress (2015) find that voters pay particular attention to data they do not experience themselves. They observe unemployment, for example, but they do not observe economic growth and rely on reported figures when making decisions about which party to support. In our case, we are interested in government fiscal data that only the government can report. If one combines the insights in the two papers, they suggest that voters make decisions on the first set of data they see. This would make manipulation of such data potentially rewarding for governments that seek to remain in office.

We build on this insight for our core argument. Following Nordhaus (1975) as well as Alt, Lassen and Wehner (2014), one would anticipate that there are opportunistic business cycles where governments manipulate macro-economic tools at their disposal in an effort to make the economy look better before an

¹Jerven (2013) tells a similar story for African countries. The quality of data is generally good when governments have state capacity to provide them. When those governments get into trouble, however, so that their capacity declines the quality of the data also drops.

election. Clark (2003) finds the tools a government uses depends upon the logic of the Mundell-Fleming model: if one assumes that capital is mobile, a government relies on monetary policy when the exchange rate is flexible while it leans on fiscal policy when the exchange rate is fixed. Both Nordhaus and Clark, however, focus on actual economic output from the use of these instruments. We are interested in the perceived figures prior to an election. There is a possible spin-off on the Clark argument. One can expect that extra spending in EU member states with flexible exchange rates has no macro-economic payoffs, while it does have payoffs in countries with fixed exchange rates, which will mostly be those in the eurozone.² Governments may, however, want to look good to their voters on these figures even in the flexible exchange rate countries in the dataset. This suggests that there should be cycles in revisions to figures according to elections, but that the revisions should be in all member states. De Castro, Pérez and Rodríguez-Vives (2013) in fact find using pre-crisis data that fiscal data for years closer to elections are revised by Eurostat more.

Moreover, we add two additional parts to the model. First, and separate from Alt, Lassen and Wehner (2014) who focus on all election years, we anticipate that manipulation of figures is greater when the government calls the election—i.e. it is unscheduled—instead of allowing it to go to full term. The reason follows the insights of Clark (2003) on the use of instruments, but with the expectation that elections called quickly—largely for non-fiscal or economic reasons—do not give the government time to affect the economy directly either through monetary or fiscal policy. Kayser (2005) finds that manipulation of the economy before unscheduled elections is too cumbersome and too difficult to time. This suggests that the manipulations are substitutes for actual instrument use.

Second, we are interested in circumstances under which governments more easily can manipulate the figures. Where are the “grey” areas in terms of the classification of assets and liabilities? For Alt, Lassen and Wehner (2014), those “grey” areas are in the budgetary shadows, in places where there is less transparency. We focus on specific types of operations in this paper. In particular, Eurostat has been active clarifying initially ambiguous rules on how to treat assistance to the financial sector (see Gandrud and Hallerberg, 2016). There is a potential endogeneity problem to using actual operations, however; ultimately, a government decides which tools to use, as Gandrud and Hallerberg (2016) explore in more detail. As we explain below, we measure overall financial market stress instead of actual realisations, which are part of what the government is manipulating in the first place.

Domestic voters, however, may not be the actors the government is directly trying to impress with its

²Exceptions would be those countries that fix their exchange rate to the euro or that have very narrow bands. Some central and East European countries like Lithuania, for example, have (or had) currency boards that maintained a de facto fix. Denmark has chosen not to join the eurozone, but it maintains a tight band around the euro for its kronor.

budget statistics. There is evidence that countries intentionally distort statistics so that they receive some sort of payoff from an international organisation. For example, Kerner, Beatty and Jerven (2016) find that some African countries kept their per capita incomes below the eligibility threshold for World Bank's International Development Association (IDA). This is a clear case where governments change their official statistics in response to an expectation from an international organisation.

Reported outcomes in terms of economic and fiscal data have a real impact on policy especially in European Union countries because of the role of the European institutions, and in particular the European Commission and Council of Ministers. GDP figures affect what type of co-financing governments receive from the European Union for a range of activities, such as environmental protection, guarantees for loans that Small and Medium-sized Enterprises (SMEs) take, and the construction of roads.³ Under the Stability and Growth Pact, all Member States are expected to have budget balances no worse than 3 percent of GDP and debt burden no greater than 60 percent of GDP. The European Commission decides each year whether a member state has an “excessive deficit”, with those earning this distinction under the Commission subject to an Excessive Deficit Procedure (EDP). Member states must propose corrective methods. The subset that are also eurozone members face potential penalties, such as fines. Member states that do not adjust their performance also could lose their access to structural and cohesion funds, which are the largest part of the European Union budget.

There are two alternative hypotheses that are relevant here. One could ask whether countries that have clear biases concerning future performance have similar biases concerning current performance. This means looking at the literature on forecasting, which considers why there are biases in forecasting numbers. Some governments seem to be eternal optimists; they report figures that that always appear to be better than turn out to be. Others are chronic pessimists, and they have positive “surprises”. Looking at European Union member states, work from the early years of the euro finds that some types of spending are procyclical, such as public investment spending. There is also a pronounced electoral cycle in the forecasting literature, with more optimistic forecasts for electoral years (Hallerberg and Strauch, 2002). This work, and others like it, usually consider the size of the “shock” that a country experiences, both in terms of the magnitude (usually by taking the absolute value of the deviation) and the direction of the shock. In more recent work, Hallerberg and Strauch (2009) find that European Union member states that fit most closely a “fiscal contracts” approach to budgeting have more conservative forecasts than member states that fit better a

³The amounts can be substantial as a percentage of total funding for a given project, up to 85 percent of the cost of the project in regions judged to be “Less Developed”, which are those with a per capita income below 75 percent of the European Union average.

“delegation to a strong finance minister” approach.

This discussion leads us to a second possible alternative explanation to explore, namely an institutional one. *[undeveloped]* The predictions, however, are not so clean. One can imagine that if there are fiscal contracts in a country like the Netherlands, one might fudge numbers so they more or less comply with internal targets. If it turns out later the government missed the target, this is unlikely to bring down the government. At the same time, precisely because there is a temptation to do this one might have especially competent staff do the numbers. The Dutch largely outsource the production of the figures to the CPB, which, while technically part of the executive, has established a reputation since the end of World War II as an independent body. What may ultimately matter most, then, is the relative independence of the body that reports the data. Delegation to a strong finance minister may suggest that the finance ministry does the numbers. This, too, can be checked, as happened in the United Kingdom, when frustration with habitually optimistic figures led to pressure for an objective body, which was the inspiration for the Office of Budget Responsibility created in 2010. How to measure the independence of the statistical agency, however, is not straight-forward.

3 Rule stretching during financial crises

Governments in countries that experience financial market stress and crises face considerable fiscal difficulties (see Laeven and Valencia, 2012) that heighten politicians’ incentives to rule stretch. At the same time, and an unexplored area in the literature, the policy options available to politicians to respond to financial crises—e.g. buying equity in a failing bank, bad banks, and bank nationalisations—present numerous rule stretching opportunities. As such, we expect politicians to engage in rule stretching even more during periods of financial market stress and crisis.

Gandrud and Hallerberg (2016) argue that many of the policies available to politicians, especially during the 2008-2011 crisis in Europe were both rarely used previously and had potentially ambiguous debt and deficit implications. For example, if a government buys equity in a troubled bank as, for example, Ireland did with Irish Nationwide Building Society and the Educational Building Society in 2010 has the government spent money or does it have a different, but equivalently valued asset? Does the transaction increase the deficit, or is it what European System of Accounts (ESA 95)—the common European fiscal accounting rulebook at the time—termed a “financial transaction”, with no effect on the deficit? If a government owns a stake in an asset management company (AMC) used to clean up a failed bank, as the German government

did with its AMCs, are the AMC’s liabilities government liabilities counted against its debt? Or are they what the ESA 95 terms “contingent liabilities”, where the government is liable only if the AMC cannot repay them?

Given strong incentives for governments to minimise debt and deficit increases around elections and especially during crises, they have strong incentives to classify initially such policies in the best possible light, so as not to increase public debts and deficits. In other words, politicians have strong *incentives* and many *opportunities* to fiscal rule stretch during periods of financial market stress.

4 Hypotheses

We test the following hypotheses regarding fiscal rule stretching—as measured by Eurostat revisions to member states’ debt and deficit figures—around elections and during financial market stress:

H_1 : Debt revisions will be larger for years closer to national government elections.

H_2 : Debt revisions will be greater for years when there are unscheduled elections.

H_3 : The effects predicted by H_1 and H_2 will be stronger when a country also has high financial market stress.

Need deficit hypotheses.

5 Empirical tests: Set up

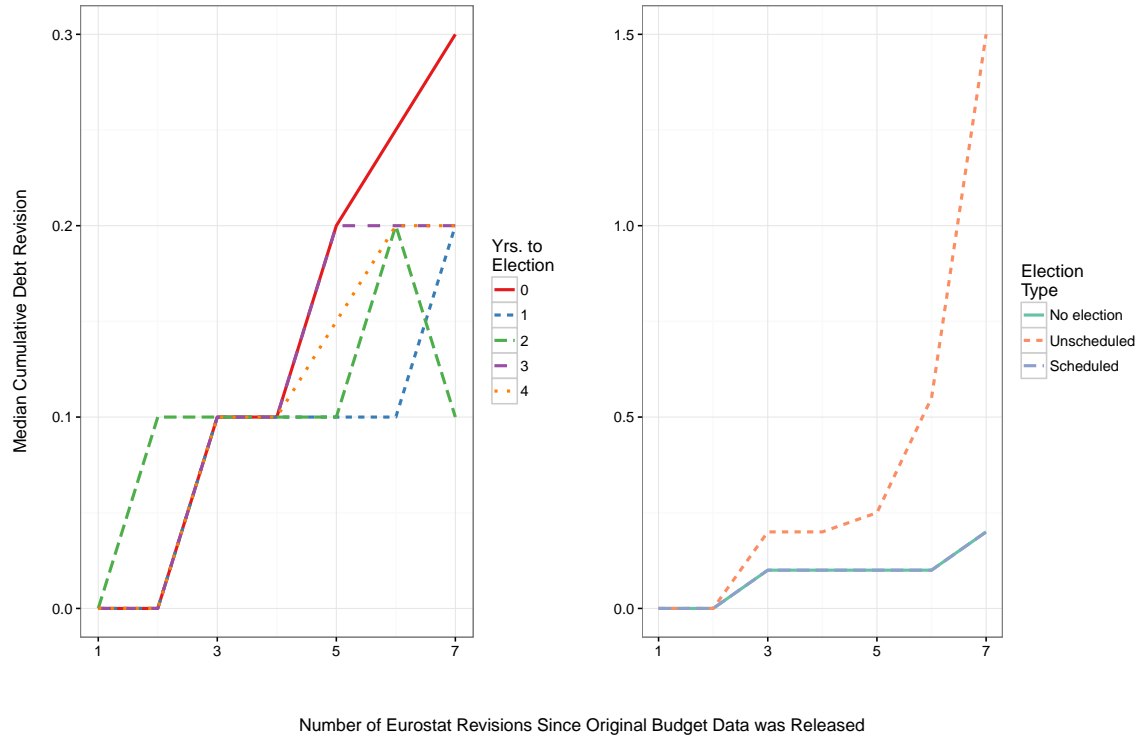
To test our hypotheses, we ran a series of linear regressions with cumulative debt and deficit revisions made by Eurostat as the dependent variable and a number of relevant political and economic indicators on the right-hand side.

5.1 Dependent variable: Eurostat revisions

We gathered a data set of all revisions that Eurostat made to EU member state debt and deficit figures from 2003 through 2013.⁴ Eurostat publishes revisions bi-annually—typically once at the end of April and again in late October. These revisions cover government finance statistics released within the previous four

⁴PDF files with the fiscal figures were downloaded from <http://ec.europa.eu/eurostat/news/news-releases>. Accessed March 2015.

Figure 1: Median **Debt** Revisions for Data Released Under Various Electoral Conditions



years. As such, the unit of analysis in the following regressions is each Eurostat revision. For every year that government statistics are revised, there are up to seven revisions: The first revision occurs in October of the initial reporting year and continue bi-annually for three years thereafter.

We created variables of *cumulative revisions for debts and deficits* up to and including each revision point. We used these as our dependent variables. For cumulative debt revisions, the variable ranged from -1.1 to 12.7 percent of GDP. For cumulative deficit revisions, the variable ranged from -4.5 to 1.1 percent of GDP. It is important to note that all of these revisions were not due to updates made to the denominator, or to GDP. Eurostat reports GDP revisions separately from corrections made to debt and deficit figures. As De Castro, Pérez and Rodríguez-Vives (2013) found for similar Eurostat data in a shorter pre-financial crisis sample, there is a clear tendency for debt statistics to be revised upward and deficit statistics downward, indicating that policies are more expensive than initially reported.

Figures 1 and 2 show the cumulative Eurostat debt and deficit revisions, respectively, in our sample under different electoral conditions. Note: below we discuss the electoral variables in detail.⁵

⁵In figures 1 and 2 we use the original electoral timing variable, not the reversed scale as, in this case, the original scale is easier to interpret.

Figure 2: Median **Deficit** Revisions for Data Released Under Various Electoral Conditions

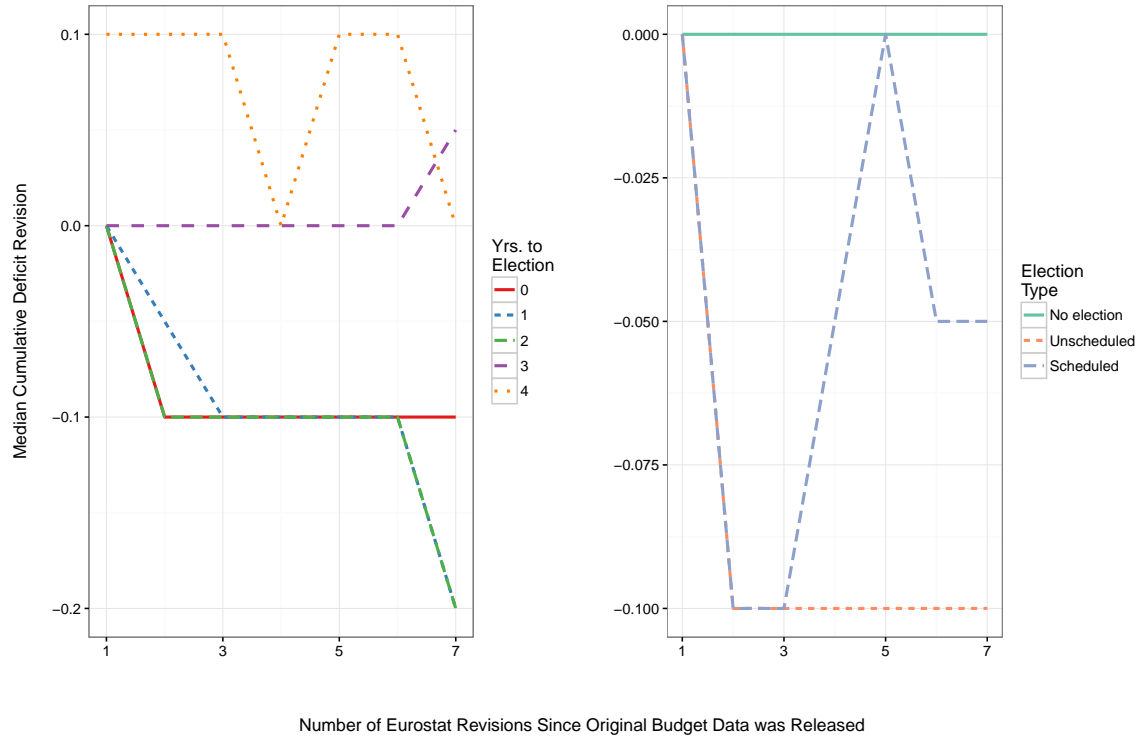


Figure 1 plots clearly indicates that revisions are higher for budget data released in years with elections, especially when they are unscheduled. The difference appear from about five to seven revisions, e.g. from two years after the first data was released.

The pattern is somewhat less clear for deficit revisions, though the possible effect is in the same theoretical direction as we found in the debt revisions. For example, in the left-panel of Figure 2 years far away from a scheduled election—i.e. three and four years from the election—deficits are generally either not revised or slightly revised upwards such that the government ran a slight surplus relative to the original data. Conversely, data for years closer to an election are revised somewhat downward. In other words, governments spent more relative to their income than they had originally reported. The right-panel of Figure 2 indicates that both scheduled and unscheduled election years see larger median downward deficit revisions. There appears to be a small difference between these two types of elections with unscheduled elections having somewhat larger downward revisions by the seventh revision.

5.2 Right-hand variables

To further explore whether governments are more likely to stretch the rules closer to elections and whether this effect interacts with financial market stress, we use Gandrud’s (2015) *years to election* variable in the following regressions. The variable counts down from the year that is the furthest away based on national election requirements. Election years are recorded as zero. To make the following marginal effect estimates more intuitive—i.e. so that a positive coefficient would indicate that being closer to an election was associated with an increase in revisions, we reversed the scale.

Not only do we expect that governments are more likely to use rule stretching as elections approach, but that rule stretching should be more prevalent when governments are not able to choose when the election occurs so as to present themselves in the best fiscal light to voters. As such, we also include a dummy variable that is one for *required elections* (e.g. scheduled elections) and zero for all other years. The variable is from Brender and Drazen (2008), and it was updated and corrected by Hallerberg and Wehner (2015). We expect that the revisions will be greater for years when there is an unscheduled election.

To examine how responding to financial market stress may exacerbate governments’ fiscal accounting rule stretching behavior, we included Gandrud and Hallerberg’s (2015) continuous “FinStress” measure of real-time perceptions of financial market stress. This measure is created by analyzing monthly content from Economist Intelligence Unit texts on banking and financial markets using a statistical method called kernel principal component analysis. Unlike previous post-hoc dichotomous measures of financial crisis (e.g. measures compiled by Laeven and Valencia, 2012; Reinhart and Rogoff, 2010), the authors argue this measure captures what we are most interested in when trying to understand policy choices: what stress policy-makers perceived and so responded to at the time. It also does not rely on ad hoc methods of determining when a crisis ended, but instead charts perceived intensity over time. To make this monthly measure comparable with our other variables, we found yearly averages. FinStress is able to vary between zero and one, with higher values indicating more perceived financial market stress. In the sample it varies between 0.12 and 0.76. Finally, note that actual fiscal costs, which Laeven and Valencia (2012) include as part of their definition and that may be correlated with fiscal revisions, are not used to judge whether there was a “crisis”.

Because we hypothesize that the effect of election timing on rule stretching will increase at higher levels of financial market stress, we focus on interactions between FinStress and the election timing and election endogeneity variables.

So far we have only operationalised the domestic–voter–audience for rule stretching. To examine the “international” audience, we examine whether the level of government gross debts and deficits effects the

propensity of rule stretching and therefore revisions by Eurostat. Perhaps countries that have higher debts and deficits and so are in threat of, or have actually breached, the SGP’s limits will be more likely to rule stretch. If this were the case, we would expect that in our sample, higher deficits will be more strongly associated with revisions than debts. This is because the 3 percent of GDP deficit limit was the focus of EDP enforcement much more so than the 60 percent of GDP debt limit until the advent of the eurozone debt crisis. To test this we gathered general government deficits as a percentage of GDP from Eurostat.⁶ For gross debts, we use information reported by the World Bank’s Development Indicators.⁷ Both sets of numbers were updated by 2015. *Note that in a future version of the paper we can include a dummy for member states facing an “excessive deficit procedure”, which only the European Commission may abrogate once that Council of Ministers (ECOFIN) has agreed that a member state indeed has an excessive deficit.*

In some model specifications we included a dummy variable for eurozone membership. An important consideration in enforcement through the EDP is governments’ good faith in returning to compliance. Fiscal rule stretching may be viewed as a bad faith move. As such countries subject to SGP enforcement may be less likely to rule stretch. All member states are covered under the monitoring procedures of the Stability and Growth Pact. However, non-eurozone members, such as the United Kingdom, face weaker or non-existent enforcement actions if they breach the SGP’s debt and deficit limits. As such, we would expect that higher revisions would be associated with being outside of the eurozone. Conversely, inspired by Clark (2003), perhaps countries in the eurozone having fixed exchange rates need to rely more rule stretching and so eurozone membership is positively associated with revisions. Because of these conflicting predictions, we do not have strong priors on eurozone membership’s effect.

6 Empirical Tests: results

Tables 1 and A-2 show our results from linear regressions with cumulative debt and deficit revisions as the dependent variables, respectively. As Eurostat has more time to examine member state government policies, we would expect on average that the cumulative revisions will grow over the course of the three and a half year period during which they are revised. To account for this we also include a variable counting the years since the original publication year on the right-hand side. All models also include country fixed effects to help account for unobserved country variation.

⁶Available at: <http://ec.europa.eu/eurostat/>. Accessed December 2015.

⁷Available at: <http://data.worldbank.org/data-catalog/world-development-indicators>. Accessed December 2015.

6.1 Debt Revisions

We can see in Table 1, model 3 that years from an election is estimated to have a negative effect on debt revisions in models where it is not interacted with FinStress. Government debt figures tend to be revised less for years further away from the next scheduled election. Or stated another way, debt revisions are larger for years closer to elections. This finding is similar in direction and magnitude to what De Castro, Pérez and Rodríguez-Vives (2013) previously uncovered with similar data over a shorter time-span. New findings can be seen in subsequent models of Table 1. In model 5 we can see that debt revisions were larger in unscheduled election years, where a government called an early election. Conversely, scheduled election years see no difference in debt revisions compared to non-election years. Looking at the interactive models 8–10, their findings are in line with our third hypothesis that the two election effects increase for years that also have higher financial market stress as measured by the FinStress variable.

To understand the direction and magnitude of these interactive effects, figures 3 and ?? show marginal effects of the election variables at various levels of financial market stress. At high FinStress levels, e.g. above 0.55,⁸ the marginal effect of being one more year removed from an election on debt revisions is about -0.2 percentage points of GDP. Figure ?? shows that having an unscheduled election during a similar high stress period increases debt revisions by about 2 percentage points of GDP.

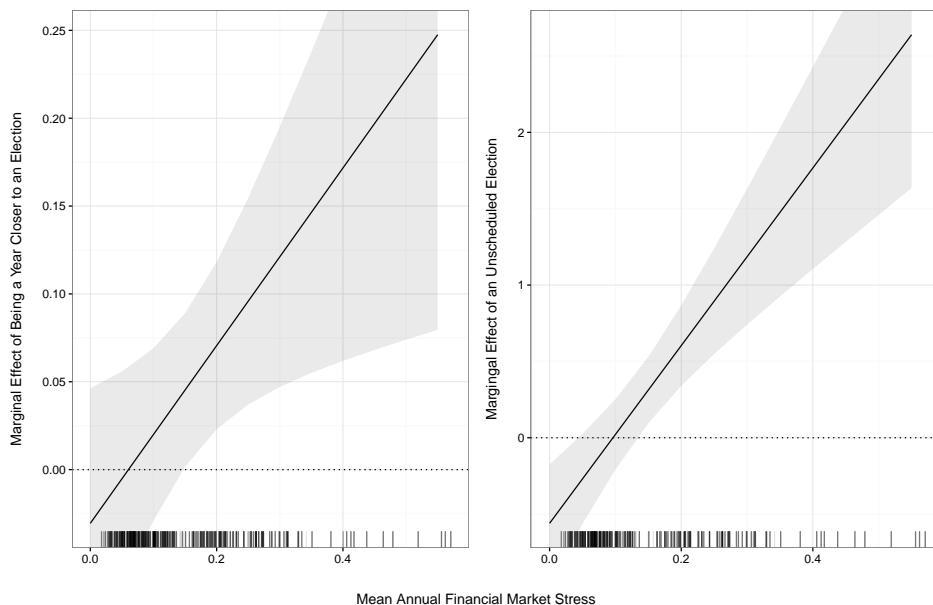
⁸Gandrud and Hallerberg (2015) find that countries classified by Laeven and Valencia (2012) as being in banking crises tend to have FinStress scores above 0.55, especially in developed economies such as those found in the EU.

Table 1: Linear Regression Estimation of Debt Revisions

	Dependent variable:									
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Yrs. Since Original	0.631*** (0.020)	0.557*** (0.026)	0.563*** (0.022)	0.628*** (0.020)	0.758*** (0.021)	0.628*** (0.021)	0.757*** (0.021)	0.626*** (0.021)	0.731*** (0.022)	0.776*** (0.024)
Eurozone	0.064 (0.160)									
Cent. Gov. Debt		0.003* (0.002)								
Gen. Gov. Deficit			-0.011 (0.009)							-0.003 (0.008)
Election Timing				0.047* (0.026)		0.047* (0.027)		-0.031 (0.047)		
Unscheduled Elect.					0.227* (0.129)		0.229* (0.132)		-0.560** (0.235)	-0.318 (0.207)
Scheduled Elect.					-0.003 (0.070)		-0.001 (0.073)		-0.057 (0.128)	-0.032 (0.099)
Financial Stress						0.039 (0.327)	0.191 (0.237)	-1.082* (0.643)	-0.049 (0.278)	0.002 (0.234)
Elect. Timing*Fin. Stress								0.506** (0.230)		
Unscheduled.Elect*Fin. Stress									5.818*** (1.443)	2.406* (1.366)
Scheduled.Elect*Fin. Stress									0.384 (0.731)	0.220 (0.558)
Constant	0.676*** (0.235)	0.541*** (0.194)	0.816*** (0.138)	0.654*** (0.178)	0.387*** (0.144)	0.648*** (0.188)	0.361** (0.151)	0.831*** (0.208)	0.353** (0.132)	0.377*** (0.116)
Country FE?	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	1,553	1,230	1,495	1,553	1,236	1,494	1,189	1,494	1,189	1,143
R ²	0.546	0.396	0.498	0.547	0.656	0.546	0.655	0.547	0.660	0.607
Adjusted R ²	0.538	0.384	0.488	0.539	0.647	0.537	0.646	0.538	0.651	0.596

Note: * p<0.1; ** p<0.05; *** p<0.01

Figure 3: Marginal Effect of Election Conditions at Various Levels of Financial Market Stress on **Debt Revisions**



Shaded area represents 95% confidence interval.

7 Conclusion

In this paper we have attempted to understand government fiscal rule stretching behavior during financial market stress and crises. The European Union provides a unique opportunity for studying this behavior as it has a common set of statistical rules—the ESA—and has a highly independent monitor—Eurostat—that frequently revisits and revises member state balance sheet statistics to ensure that they are similarly in line with these rules. We significantly deepen findings in previous work that elections are associated with revisions to government debt and deficit figures. We specifically examine behavior during unscheduled elections and periods of financial market stress. We find that government debt rule stretching is much more prevalent during financial crises and when elections are unscheduled, when governments have few opportunities to affect economic fundamentals.

Overall, it appears that deficit rule stretching, at least during our sample, may be more about avoiding running afoul of the Stability and Growth Pact’s three percent deficit limit, rather than being a result of electoral incentives or financial market stress. Nonetheless, more work is needed to understand the observed relationship between scheduled elections and rule stretching that we observe here.

A major takeaway from our work is that even among a group of developed economies with generally

strong economic institutions, that fiscal rule stretching is common and can significantly affect our knowledge about government spending and financial obligations. This is especially true during financial market stress. As such independent government accounting agencies, such as Eurostat, are a crucial component of get the numbers right, even if it takes a few years.

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Online Appendix

Crisis and selection into unscheduled elections

We ran a number of tests to examine whether or not governments select into unscheduled elections according to the prevailing level of financial market stress. Table A-1 shows results from a logistic regression where we tried to predict having an unscheduled election in a year for our sample based on annual average stress level. We can see that there is a null result. We made a similar finding when running a multinomial logistic regression (not shown), with unscheduled and scheduled elections as categories and no election as the reference category.

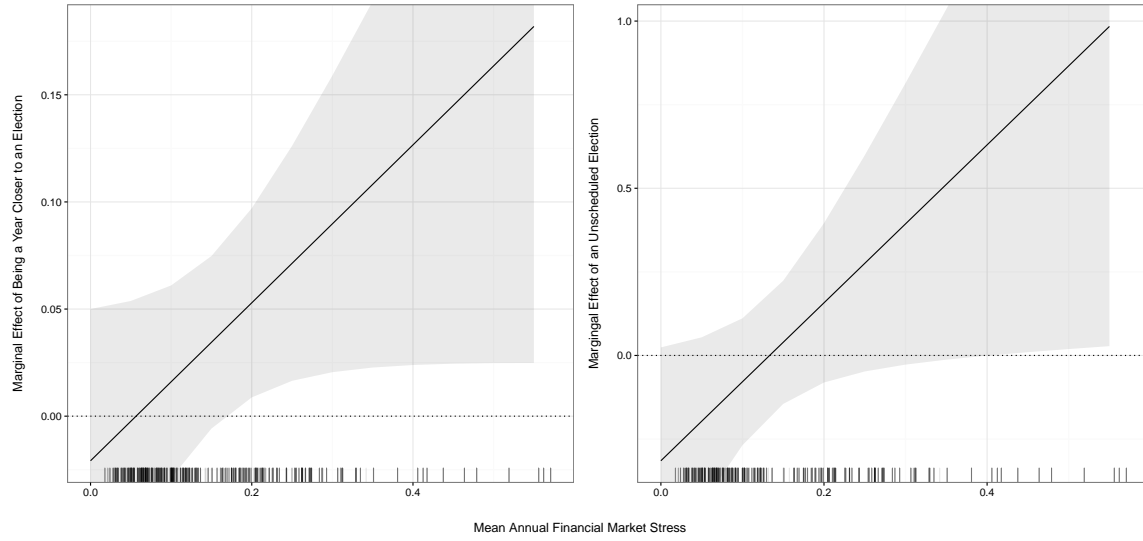
Table A-1: Logistic Regression Estimation of Having an Unscheduled Election

	<i>Dependent variable:</i>
	Unscheduled Election
Financial Stress	0.093 (0.148)
Constant	0.114 (0.083)
Country FE?	Yes
Observations	207
Log Likelihood	25.802
Akaike Inf. Crit.	2.397
<i>Note:</i>	*p<0.1; **p<0.05; ***p<0.01

Omitting Greece

Greece very large debt revisions in our sample. For 2007 through 2011 Greece's debt statistics were cumulatively revised upward by over 10 percentage points. To examine if this one case was driving our election results, we reran our models omitting Greece. The numeric results are show in Marginal effect plots from models without Greece for scenarios discussed in the main paper are in Figure A-1. We can see that the magnitude and of the effects decrease somewhat and the confidence bounds expand with Greece excluded.

Figure A-1: Marginal Effect of Election Variables on **Debt** Revisions (sample excluding Greece)



Shaded area represents 95% confidence interval.

Deficit Revisions

We ran similar models as those found in Table 1, but with cumulative deficit revisions as the dependent variable. These results are shown in Table A-2. We can see that there is no evidence in these models to support hypotheses regarding electoral effects on deficit revisions, including in interaction with financial market stress. This finding is in line with our expectations, as overwhelmingly fiscal rule stretching in response to financial crisis will be on the debt, rather than deficit component of the budget.

Table A-2: Linear Regression Estimation of Deficit Revisions

	<i>Dependent variable:</i>									
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Yrs. Since Original	0.684*** (0.019)	0.586*** (0.024)	0.635*** (0.020)	0.684*** (0.019)	0.818*** (0.017)	0.682*** (0.019)	0.818*** (0.017)	0.681*** (0.019)	0.819*** (0.017)	0.806*** (0.018)
Eurozone	-0.033 (0.059)									-0.019 (0.045)
Cent. Gov. Debt		-0.001 (0.001)								
Gen. Gov. Deficit			0.011*** (0.004)							0.006** (0.003)
Election Timing				-0.005 (0.009)		-0.006 (0.010)		-0.021 (0.017)		
Unscheduled Elect.					0.010 (0.042)		0.009 (0.042)		-0.073 (0.075)	0.029 (0.077)
Scheduled Elect.					0.026 (0.023)		0.021 (0.023)		0.056 (0.041)	0.049 (0.037)
Financial Stress						-0.069 (0.120)	-0.068 (0.082)	-0.290 (0.236)	-0.059 (0.090)	0.035 (0.096)
Elect. Timing*Fin. Stress								0.100 (0.091)		
Unscheduled.Elect*Fin. Stress									0.604 (0.446)	-0.357 (0.510)
Scheduled.Elect*Fin. Stress									-0.241 (0.236)	-0.213 (0.209)
Constant	-0.039 (0.086)	-0.034 (0.080)	-0.053 (0.060)	-0.062 (0.065)	-0.065 (0.047)	-0.052 (0.068)	-0.055 (0.048)	-0.016 (0.075)	-0.065 (0.049)	-0.029 (0.060)
Country FE?	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	1,553	1,230	1,495	1,553	1,236	1,494	1,189	1,494	1,189	1,143
R ²	0.559	0.442	0.497	0.559	0.761	0.559	0.763	0.559	0.764	0.730
Adjusted R ²	0.551	0.431	0.487	0.551	0.755	0.550	0.757	0.550	0.758	0.722

* p<0.1; ** p<0.05; *** p<0.01