Austerity and Anarchy:

Budget Cuts and Social Unrest in Europe, 1919-2008

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Abstract

Does fiscal consolidation lead to social unrest? Using cross-country evidence for the period 1919 to 2008, we examine the extent to which societies become unstable after budget cuts. The results show a clear correlation between fiscal retrenchment and instability. We test if the relationship simply reflects economic downturns, and conclude that this is not the case. While autocracies and democracies show a broadly similar responses to budget cuts, countries with more constraints on the executive are less likely to see unrest after austerity measures. Growing media penetration does not strengthen the effect of cut-backs on the level of unrest.

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1 Introduction

Social unrest has led to key turning points in history since, at least, the French Revolution. Marx saw it as the driving force of the transition of societies from feudalism to capitalism and, eventually, communism. The power of unrest as a catalyst for change manifests itself explicitly in regime changes, such as during the "Arab Spring" of 2010-2011, or it operates through expectations: The extension of the franchise in Western societies can be interpreted as an attempt to reduce the threat of revolution (Acemoglu and Robinson 2000).[1]. What leads to social unrest is less clear. Economic shocks are one important contributing factor: The demise of the Weimar Republic during the Great Depression is a prominent example of how economic hardship can translate into political instability and social unrest (Bracher 1978).[2]

In this paper, we examine one of the possible determinants of unrest and violent protests - fiscal policy. How do budget measures affect the level of social instability? The extent to which societies fracture and become unstable in response to drastic retrenchment in the government budget is a major concern for policymakers tackling large budget deficits: From Argentina in 2001 to Greece in 2010-11, austerity measures have often created waves of protest and civil unrest. Economic conditions can deteriorate further and faster if political and social chaos follows attempts to reign in spending[3]. Consequently, sustainable debt levels for countries that are prone to unrest may be lower than they otherwise would be.

We use a long panel dataset covering almost a century, focusing on Europe, 1919 to 2008. The continent went from high levels of instability in the first half of the 20th century to relatively low ones in the second, and from frequently troubled economic conditions to prosperity. It thus provides a rich laboratory of changing economic, social and political conditions. In terms of outcome variables, we focus on riots, demonstrations, political assassinations, government crises, and attempted revolutions. These span the full range of forms of unrest, from relatively minor disturbances to armed attempts to overthrow the

[1] In a related exercise, Boix 2003 models the incentives of the populace to resort to violence as a function of the wealth distribution and economic development.

[2] The French Revolution has also been interpreted in these terms (Soboul 1974; Doyle 2001). The view is controversial (Hunt 2004; Cobban 1964).

[3] Bloom 2009 documents how uncertainty shocks can drive output down.

established political order. We compile a new index that summarizes these variables, and then ask - for every percentage cut in government spending, how much more instability should we expect?

The data shows a clear link between the magnitude of expenditure cut-backs and increases in social unrest. With every additional percentage point of GDP in spending cuts, the risk of unrest increases. As a first pass at the data, Figure 1 examines the relationship between fiscal adjustment episodes and the number of incidents indicating instability (CHAOS). CHAOS is the sum of demonstrations, riots, strikes, assassinations, and attempted revolutions in a single year in each country. The first set of five bars show the frequencies conditional on the size of budget cuts. When expenditure is increasing, the average country-year unit of observation in our data registers less than 1.4 events. When expenditure cuts reach 1% or more of GDP, this grows to 1.8 events, a relative increase by almost a third compared to the periods of budget expansion. As cuts intensify, the frequency of disturbances rises. Once austerity measures involve expenditure reductions by 5% or more, there are more than 3 events per year and country - twice as many as in times of expenditure increases.

[Figure 1 here]

Exactly the same relationship can be observed in each of the four main subcategories of CHAOS. The frequency of demonstrations, assassinations, and general strikes rises monotonically with the scale of cuts. Only in the case of riots is there a small decline for the biggest cut-backs. In the case of demonstrations, the frequency of incidents appears to rise particularly fast as expenditure cuts pass the 3% threshold.

The strength of the link between austerity measures and unrest is our first important finding. Is the link causal? Other factors, such as generally depressed economic conditions, could drive up unrest and the need for cut-backs simultaneously. Controlling for economic growth does not change our results. This suggests that we capture more than a general association between economic downturns and unrest. To rule out other possible channels that might influence both cut-backs and unrest we refine our strategy by focusing on the sub-period 1970-2007, for which more detailed data is available. Controlling for ethnic fragmentation and unemployment does not change our result, it also doesn't change when

we add other possible tools available to governments to finance public deficit: inflation and public debt.

To demonstrate that causality runs from cut-backs to unrest, we refine the data in two ways: First, we analyze a more detailed dataset that gives information about the causes of each incident. Second, we use recently compiled data on changes in the government budget that follow directly from discretionary policy decisions (Devries et al. 2011). For both types of additional evidence, we find clear indications that the link runs from budget cuts to unrest. We also conduct placebo tests with other types of unrest - inspired by ecological issues and world peace, for example - and find no effect of budget measures.

Our findings are robust to a wide range of alternative specifications and further tests. Different measures of unrest do not affect our conclusions. We examine if the link between austerity and unrest changes as countries institutions improve. For most value of the Polity2 score of institutional quality, results are broadly unchanged. However, countries with very high levels of constraints on the executive show a weaker degree of association. Further, we examine if the spread of mass media changes the probability of unrest. This is not the case. If anything, higher levels of media availability and a more developed telecommunications infrastructure reduce the strength of the mapping from budget cuts to instability. We also test which part of the distribution of unrest is responsible for our results, using quantile regressions: The higher the level of unrest, the bigger the relative impact of additional budget cuts. Finally, we test for asymmetries in the relationship between unrest and austerity. Reductions increase instability, but spending increases do not cut the number of incidents to the same extent.

Earlier papers on the same topic have typically focussed on case studies, or on subsets of the developing world.[4] DiPasquale and Glaeser 1998 analyzed race riots in the US in the 1960s and early 1990s.[5] They find that ethnic heterogeneity and unemployment rates are a strong predictors of riots, and that poverty is relatively unimportant. Bohlken and Sergenti 2010 find that riot probabilities in India over the period 1982-95 dropped sharply when growth rates accelerated. Field et al. 2008 examine religious violence in Ahmedabad, India, in 2002, and argue that tenancy rights created neighborhoods that

[4]Theoretical work on unrest includes Kuran 1989, Tullock 1971, and Grossman 1991.

[5]The authors also analyse international data for the period 1960-85. They find that higher GDP reduces the incidence of riots, while urbanization rates are positively associated with them.

were more integrated, and hence more prone to violence.

Comparative work by Alesina and Perotti 1996 suggests that inequality leads to more unrest, and this adversely affects investment. Work on 23 African countries during the 1980s found that budget cuts had typically no effect on political and social stability. IMF interventions, on the other hand, often led to more frequent disturbances (Morrison et al. 1994). Paldam 1993 examines current account crises in seven South American countries during the period 1981-90, using high-frequency (weekly) data. He finds that the run-up to new austerity measures is associated with higher levels of unrest, but that actual implementation is followed by fewer disturbances. Similarly, Haggard et al. 1995 find that IMF interventions and monetary contractions in developing countries led to greater instability. Analyzing the period 1937-1995, Voth 2011 explores related issues for the case of Latin America. He finds that austerity and unrest are tightly linked in a majority of cases.

Relative to these papers, we make a number of contributions. To the best of our knowledge, ours is the first systematic analysis of how budget cuts affect the level of social instability and unrest in a broad cross-section of developed countries, over a long period. We also examine the exact causes of instability in a subset of the data where more detailed information is available, and find a strong link with austerity. This also allows us to perform placebo tests. Using detailed evidence on the motivations for government expenditure changes, we strengthen the causal link still further. Finally, we examine the link between instability and media penetration.

Other related literature includes work on the political economy of fiscal consolidation, and on its economic effects. The composition of fiscal adjustment has been examined; cutting entitlement programs tends to produce persistent improvements in the budget balance, while revenue measures and capital expenditure cuts have only temporary effects (Alesina and Perotti 1995). The timing of stabilization measures has been explored in war-of-attrition models, which view relative bargaining strength of different groups as crucial (Alesina and Drazen 1991). A rich literature has examined the macroeconomic effects of budget cuts. Giavazzi and Pagano 1990 and Alesina et al. 2002 find that cuts can be expansionary. Amongst the reasons suggested for this finding are a reduction in uncertainty about the course future spending (Blanchard 1990a), and a positive wealth

shock as a result of lower taxes in the future (Bertola and Drazen 1993).[6] Recently, work by the IMF has suggested that austerity measures may be less expansionary than previously thought; they may well have the standard negative Keynesian effects as a result of lower demand (IMF 2010b; Guajardo et al. 2011). We proceed as follows: Section 2 presents our data, and section 3 summarizes our main results. Robustness checks and extensions are discussed in section 4; section 5 concludes.

2 Data

In this section, we briefly describe our data and summarize its main features. We use two datasets - a long-term one which allows tracing out the broad patterns of unrest and austerity since 1919, as well as a short-term one that contains richer information on the causes of unrest. For both, we use information on unrest as well as on economic performance and budget measures. Five main indicators of domestic conflict in the long-term data will form the main focus of this study - general strikes, riots, anti-government demonstrations, political assassinations, and attempted revolutions. These data are part of the Cross National Time Series Dataset, compiled by Banks 2010 and his collaborators. The main source of data on unrest episodes are the reports of the *The New York Times*, while the variables' definition is adopted from Rummel and Tanter 1971. In addition, we use data on GDP, government revenue, expenditure, and the budget balance from a variety of sources.[7]. The long-term data has information on 32 European countries and covers the years from 1919 to 2008.[8]

Table 1 gives an overview of the main variables and their descriptive statistic for the long-term data. The average number of assassinations and general strikes was quite low in our sample, with less than 2 events in each decade. There were more riots and more demonstrations, 5-6 per decade. Attempted revolutions are quite rare, but some countries registered high levels of instability. The record in our sample is Germany in 1923, with 5

[6] Once the response of labor supply and capital formation is fully taken into account, these effects may not go through (Baxter and King 1993).

[7] Data on fiscal variables are from Mitchell 2007, OECD 2010 and IMF 2010a. See the Data Appendix for a detailed description of the sources of each fiscal variable

[8] The 32 European countries included in the long-term data are: Austria, Belgium, Bosnia and Herzegovina, Bulgaria, Croatia, Czech Republic, Denmark, Estonia, Finland, France, Germany, Greece, Hungary, Ireland, Italy, Latvia, Lithuania, Netherlands, Norway, Poland, Portugal, Romania, Russian Federation, Serbia, Slovak Republic, Slovenia, Spain, Sweden, Switzerland, Ukraine, United Kingdom, Yugoslavia.

recorded attempts at overthrow (with communist insurgencies in Saxony and Thuringia, the Hitler Beer Hall Putsch, and a separatist movement in the Rhineland). Assassinations and riots similarly show a broad range of observed values. Almost a century of data includes some extreme observations.[9] For example, Austria and Germany saw major output declines in 1945 and 1946, respectively. The biggest reduction in governments spending in our data occurred in Poland, in 1982; the second-largest, in Finland, in 1947. The start of wars is often associated with big increases in expenditure. The record-holder in our dataset is Hungary in 1940, with an increase of over 30 percent.

[Table 1 here]

To obtain a single measure of instability, we calculate CHAOS by taking the sum of the number of assassinations, demonstrations, riots, general strikes, and attempted revolutions. While a crude way of aggregating indicators, it turns out to be powerful.[10] In the robustness section, we show that alternative methods of reducing data complexity such as principal components analysis do not change our results.

For CHAOS, the average country in our sample registers 1.4 incidents per year. Instability was not constant over time. The maximum is higher - Italy in 1947 saw a total of 38 incidents, including 7 general strikes, 19 riots, and 9 anti-government demonstrations. Figure 2 gives an overview of the evolution over time, plotting the average of CHAOS as well as the maximum number of incidents observed. While there is no clear-cut pattern over time, some features emerge. The interwar period showed relatively high levels of unrest, with an average of 1.8 incidents per year, compared to 1.3 in the post-war period. The immediate post-World War II period, and the period form 1968 to 1994 also show unusually high levels of unrest. Comparatively speaking, the years since 1994 have been unusually tranquil (average CHAOS = 0.68).

[Figure 2 here]

The short-term data on unrest is from the European Protest and Coercion Database (EPCD) developed by Francisco 2006. The EPCD codes daily data on all reported protest

[9] In the data appendix we explain in detail how we deal with outliers.

[10] One alternative is the weighted conflict index (wci), as compiled by Banks 2010. It gives fixed weights determined to different forms of unrest: Demonstrations have a weight of 200, while political assassinations have a weight of 24.

events occurred in 28 European countries between 1980 and 1995. The data is constructed using the full-text reports from more than 400 newspapers in the Lexis-Nexis database. We restrict our attention to the same types of protest events covered in the long-term data: riots, demonstrations, political assassinations, general strikes, and attempted revolutions.[11] The main advantage of the EPCD over the Arthur BanksÕ database is that the former records the issue behind each protest, allowing us to test the relationship between austerity and unrest in a very precise way, even if only for a small subset of the overall dataset.[12]

There are relatively few protests that are caused by austerity measures. At the same time, when they happen, they involve a large number of participants - by far the largest number of protesters of any category, as Table 2 illustrates. These protests tend to be relatively peaceful, with few protesters arrested, injured or killed, and relatively few members of the security forces involved.

[Table 2 here]

In compiling information on expenditure and the budget balance data, we need to trade off the accuracy of information against availability over a long time span. For the 1919-2008 dataset, we rely on standard data sources on the central government revenue and expenditure relative to GDP from Mitchell 2007, augmented by data from the OECD 2010 and the IMF 2010a for the period thereafter. Expenditure changes will serve as the main explanatory variable. Figure 3 graphs changes in expenditure/GDP from one year to the next. The distribution is almost symmetric around the mean, with similar numbers of country-years witnessing expenditure increases and declines (857 vs 729). In an average year and country over the period, central government expenditure relative to GDP rose by 0.3%. The vast majority of observations is in the range of changes by less than 3%,

[11] We define as a single protest event an event recorded with the same entry in the following EPCD variables: "event", "protester" and "location". The variable "event" provides a short description of the protest and its reasons, "protester" indicates the protesting group or type (e.g. ferry workers, teachers, truck drivers), "location" is usually the city in which it took place. Events that last more than one day are collapsed in this way in a unique observation, where the number of protesters and security forces members are averages across days. Notice that we only consider protest events whose number of participants is above 100 for riots and demonstrations and above 1000 for general strikes (no threshold is used for assassinations and attempted revolutions). These are the same threshold used in the Banks 2010 database.

[12] For this data we are also able to distinguish among austerity-inspired protests between those whose primary cause is expenditure cuts and those mainly related to tax increases.

with a few outliers in the tails of the distribution (typically driven by the beginning and end of wars).

[Figure 3 here]

When focusing on the sub-period from 1970 to 2007 , we use cyclically adjusted fiscal data constructed by Alesina and Ardagna 2010.[13] In particular we employ their data on primary expenditure, government investment, total revenues and the primary budget balance. In this way we correct for both changes in interest payments and the immediate effect of the economic cycle, which drives both expenditure and revenue without any additional policy decision being taken. For a subsample of the data (1978-2008, 17 countries), we also use data by Devries et al. 2011. These authors examine in detail the policy changes that led to changes in a countryÕs fiscal stance. Only expenditure cuts or revenue increases motivated by a decision to press ahead with fiscal consolidation are considered.[14] Overall, Devries et al. 2011 find 173 periods of fiscal policy adjustment.

As a first pass at the data, we repeat the exercise in Figure 1 for output growth (Figure 4). We subdivide the sample into terciles, and examine how much the incidence of various indicators of unrest declines as growth accelerates. For the summary indicator (CHAOS), there are a little more than 2 incidents when growth is in the lowest tercile. This falls to 1.3-1.5 incidents as growth accelerates. There is also a clear pattern of decline for demonstrations and for assassinations. In the case of riots, the differences are smaller overall, whereas in the case of general strikes, there seems to be little pattern at all. Based on a first, visual inspection of the data, it seems that the link between budget cuts and unrest is clearer than the one with growth.

[Figure 4 here]

Next, we examine the correlation structure of our data in Table 3. Assassinations, general strikes, riots, revolutions and demonstrations are all positively and significantly correlated with each other. This supports our assumption that they reflect a broader underlying pattern of social instability and unrest. CHAOS is also positively correlated with the weighted conflict index (wci). Finally, Table 3 suggests that higher levels of

[13] Alesina and Ardagna use the method of Blanchard 1990b

[14] The approach is similar to the "narrative approach" pioneered by Romer and Romer 1989

expenditure and faster growth are associated with less unrest. The simple correlation of CHAOS with changes in the budget balance is positive and significant. Higher taxes and lower expenditure are associated with more unrest, but the relationship is not significant.

[Table 3 here]

In the case of output changes, the coefficient is negative, but insignificant (Table 3). The simple correlations suggest that these co-movements do not extend to all indicators of unrest equally: riots, revolutions, and demonstrations decline as expenditure rises, but assassinations and strikes seem - at a first pass - uncorrelated. Similarly, output growth seems to correlate negatively with assassinations, riots, revolutions, and demonstrations, but not with strikes. Next, we examine the connection between budget position, expenditure, and unrest more systematically.

3 Results

The graphical evidence in Figures 1 and 4 suggests a link between "hard times" - low growth and budget cut-backs - and unrest. Next, we examine if there is a systematic relationship between budget measures and social instability. In this section we also address the issue of causality, while in the next section we will test the robustness of our results.

3.1 Baseline Results

We estimate simple panel regressions of the type:

$$I_{it} = \alpha + \alpha_i + \alpha_t + \beta \Delta B_{it} + \Gamma X'_{it} + \varepsilon_{it} \tag{1}$$

where I_{it} denotes the level of instability in country i at time t, ΔB_{it} is a measure of the change in the budget position, α_i is a country-specific intercept, α_t is a time-specific dummy, and X_{it} is a vector of control variables.

We use CHAOS as the dependent variable in our baseline specification, and test the robustness of findings to alternative specifications later. CHAOS is a count variable. Hence we use Poisson Quasi-Maximum Likelihood estimation, with country fixed effects and year dummies. This takes the highly skewed distribution of the number of incidents

into account. Table 4 gives the main results. We find that expenditure cuts are strongly and significantly associated with the number of unrest incidents in any one country and year (column 1). The same is true of overall improvements in the budget position (column 3). We find a positive effect of tax increases, but the estimated standard error is large (column 2).

Under OLS with fixed effects and year-dummies, we find the same pattern: expenditure increases reduce instability in a powerful way (column 1). A one standard-deviation increase in expenditure cuts the number of incidents (CHAOS) by 0.4 per year and country. Tax increases have a positive sign, but the effect is not significant at standard levels of rejection (column 2). It is also small - a one standard deviation rise in the tax/GDP ratio increases unrest by less than 0.01 events. Overall, we find that improvements in the budget balance raise the level of unrest (column 3). As the results in columns (1) and (2) make clear, this reflects the impact of expenditure cuts, and not of tax increases.

[Table 4 here]

The downside of using the full sample, for the years 1919-2008, is that many important covariates are not available. For example, consistent and comparable data on unemployment rates is rare for earlier periods. To address this issue, we estimate the same regressions but with a full set of controls, for a subset of 16 countries over the period 1970-2007.

[Table 5 here]

We add changes in output per head, unemployment, ethnic fractionalization, polity scores, the debt/GDP level, as well as changes in inflation and the primary budget balance to our set of explanatory variables. For this subset of the sample, our fiscal variables are also more cleanly defined. We can use primary expenditure - net of debt servicing cost - which is conceptually superior. Some of the variation in primary expenditure and total revenue could, however, also reflect changes that are driven by automatic stabilizers. To take this into account we cyclically adjust all fiscal variables using the Blanchard methodology (as in Alesina and Ardagna 2010), in an attempt to clean as much as possible the change in fiscal variables from their cyclical component.

As Table 5 shows, the link between expenditure cuts and unrest remains unaffected, as is the predictive power of the budget position overall. In contrast to the results with the full sample, tax increases appear to be also strongly associated with instability. We will analyze this issue further when using information on the cause of each unrest event. Finally, in column 4, we examine if adjusting the budget position for debt service (using the primary surplus as the explanatory variable) changes our findings; it does not. In addition, we find positive coefficients on ethnic fractionalization and unemployment, which is in line with the results by DiPasquale and Glaeser 1998. Inflation is strongly and negatively associated with unrest, giving credence to theories that see it as a way to preserve "social peace" in economies under pressure (Feldman 1997).

Which component of CHAOS is most affected by austerity measures? In Table 6, we use the same specification as in Table 4 under Poisson QML, looking at the effect of expenditure cuts on each of the components of our aggregate indicator of instability in the full sample: general strikes, demonstrations, riots, assassinations, and attempted revolutions. Of the five outcome variables, all show the expected sign, and three of them - demonstrations, riots and attempted revolutions - are statistically significant. The variables that do not show a large, significant coefficient are assassinations and general strikes. On average, years with expenditure increases saw fewer general strikes, but there are numerous general strikes that are not an immediate reaction to economic conditions and budget measures (such as, for example, the 1926 general strike in Britain). For the other variables, the coefficients are large, indicating that austerity measures coincide with significant increases in demonstrations, attempted revolutions, riots, and assassinations. In all specifications, the effect of GDP growth on unrest is negative. In contrast to the results for expenditure changes, the effect is not tightly estimated.

[Table 6 here]

Table 7 takes this analysis one step further, by breaking the period 1919-2008 into four sub-periods. We distinguish the interwar years from the period of post-World War II reconstruction, the period of slowing growth in the 1970s and 1980s, as well as the years after the fall of the Berlin Wall after 1989. For the first two sub-periods, we use expenditure relative to GDP as the explanatory variable; thereafter, as a result of greater

data availability, it is primary (non-debt) expenditure. In each subperiod, we find the same pattern as in the long-run data. The only exception is the penultimate period, 1970-1989, when the effect of expenditure changes is not as tightly estimated as it is in the other sub-periods. The effect of changes in budget expenditure on unrest is strongest in the period after the fall of the wall, when the estimated coefficient is twice as large as in the earlier periods. The effect of GDP growth is negative except during the post-war boom: More growth was associated with more unrest. While it is difficult to test for the causes of this reversal exactly, it seems plausible that high rates of output growth may have encouraged worker militancy . At a time when many countries reached full employment, this effect seems to have dominated. The normal pattern of GDP growth reducing unrest reasserts itself after 1970. From 1970 onwards, we can control for additional variables, and do so. Inflation seems to have reduced unrest in the 1970s, in line with some of the political economy literature on the origins of high inflation (Samuelson and Solow 1960).

The fall of the Berlin wall saw the spread of Western-style democracy eastwards. The overall connection between austerity and social instability is the same in the expanded sample, and it is highly significant.

[Table 7 here]

3.2 Causality

The obvious challenge in interpreting equation (1) is the potential for omitted variable problems. It is possible that the economic cycle is simultaneously driving both unrest and the need for budget cuts. Above, we already control for GDP growth rates, and our main finding remains unaffected. However, the omitted variable problem would only be solved if we measured the effect of economic output on instability perfectly. Since this is unlikely, we present another type of analysis. We use a related dataset that offers much more detailed information, for a shorter time period, on the causes behind each unrest event. This allows us to demonstrate the connection between social instability and expenditure cuts more directly.

As described in the data section, the EPCD's dataset can be used to pin down the main motive behind each public demonstration. We examine if the public assemblies that

are motivated by complaints against austerity - as determined by the newspaper records in Lexis-Nexis - are significantly affected by actual changes in fiscal policy. Our approach here is similar to what has been called the "narrative approach" (Romer and Romer 1989). Table 8 gives the results. If we use the same specification as in Table 1 (where we analyzed the dataset spanning the period 1919-1999), we find similar results. Increasing expenditure lowers levels of unrest (column 1). In column 2, we examine the responsiveness of anti-tax demonstrations to revenue increases, and find a weakly positive relationship. The main finding, that anti-austerity protests surge when expenditure is cut, survives controlling for a host of additional variables, including country- and year-fixed effects.

[Table 8 here]

We can strengthen this result further by conducting a placebo test. In Table 9, we look at other protests, and test if they can be predicted by the same explanatory variables as in Table 8. Labour disputes and demonstrations about the state of the economy are less frequent when expenditure is expanding, but the link is not strong or statistically significant. Peace rallies, ecological issues, and unrest events in response to education policy are actually more frequent in times of fiscal expansion. Overall, the placebo test shows that only in the case of anti-austerity demonstrations is there a strong and significant link with changes in government expenditure.

[Table 9 here]

In Table 10 we use the IMF measure of policy-action based changes in expenditure, revenues and the budget balance to strengthen the argument for a causal link. Here, the source of variation is identified as cleanly as possible: Only changes in the budget balance motivated by concern about the long-run fiscal position are used. The downside is that we only have information on episodes of fiscal retrenchment (deficit reductions) and not on periods of fiscal expansion. Using this action-based measure as explanatory variables for unrest produces a negative, large and significant coefficient for change in expenditure, a positive and non significant coefficient for change in revenues and a positive and significant coefficient for the budget balance. The closer we get to measuring the impact of policy measures, the larger the coefficient becomes. This strengthens the case for a causal link between unrest and austerity.

[Table 10 here]

4 Robustness and Extensions

In this section, we examine the sensitivity of our results. We first look at interaction effects with institutional factors. Do countries with more accountable governments weather the storms of austerity better? We also test if the effect is driven by outliers, whether positive or negative changes in expenditure matter more for the effect on unrest, and whether the effect is constant in all parts of the distribution of the dependent variable. Greater constraints on the executive and more democracy should on the one hand reduce social conflict; on the other, there will be less repression by the authorities in more democratic countries with higher Polity scores. Which effect dominates is not clear ex ante. Table 11 demonstrates that in countries with better institutions, the responsiveness of unrest to budget cuts is generally lower. Where constraints on the executive are minimal, the coefficient on expenditure changes is strongly negative - more spending buys a lot of social peace. In countries with Polity2 scores above zero, the coefficient is about half in size, and less significant. As we limit the sample to ever more democratic countries, the size of the coefficient declines. For full democracies with a complete range of civil rights, the coefficient is still negative, but no longer significant. The link with growth is also relatively stable. Higher output generally dents the tendency to riot, demonstrate, assassinate, or strike in countries with low institutional quality. The only exception is in full democracies, where the connection is weaker but still negative.

[Table 11 here]

When does the link between budget cuts and unrest become particularly strong? We examine which part of the distribution of CHAOS shows a large impact of austerity measures. To do so, we estimate quantile regressions, where the conditional 5th to 95th percentile of the distribution of CHAOS is the dependent variable. Figure 5 illustrates the size of effects. The estimated coefficient is zero for much of the range. Only from the 80th percentile upwards - for country-year observations with two or more incidents - is the effect visible. It then grows rapidly as the estimated coefficient on expenditure changes

(and on output growth) increases at higher and higher percentiles of the distribution of CHAOS. This suggests that budget cuts matter relatively more for the really big waves of unrest.

[Figure 5 here]

How much does our main finding depend on the way in which we aggregate unrest? CHAOS is the simple sum of incidents. Instead, we can use the weighted conflict index, as compiled by Banks 2010. It encompasses a larger set of domestic conflicts including, in addition to the components of CHAOS, purges, major government crisis and guerrilla warfare. It also assigns different, fixed weights to each individual component. The correlation coefficient of the variable with CHAOS is 0.75, significant at the 1% level. Another alternative is to use the first principal component of the five indicators that go into CHAOS. They all enter with a positive weighting. The first principal component explains 0.42 of the overall variance. The correlation coefficient with CHAOS is 0.98. In Table 12, we use both wci and the first principal as dependent variables. Since the dependent variable is no longer a count variable, we use panel OLS, and obtain large and mostly significant coefficients for expenditure changes and the budget position. For the principal component, expenditure increases cut unrest significantly; for the wci, the standard error is slightly above the typical cut-off for significance. As before, there is no clear pattern for tax changes. The results are largely identical in terms of magnitude and significance with the baseline results in Table 4.[15] We conclude that the way in which we measure unrest does not affect our main finding.

[Table 12 here]

An additional factor that can be questioned involves the use of a count variable for unrest in the baseline results. The variable CHAOS is designed to capture the intensity of unrest, but it may be that it is influenced by a number of outliers with a high count of incidents. This would then make it easier to find significant effects. To examine this potential issue, we transform CHAOS into a simple dichotomous variable, with unrest

[15] A decrease of 1 GDP point in public expenditure is associated with an increase of 0.04 standard deviations in CHAOS, 0.03 standard-deviations in first principal component of chaos and 0.02 standard-deviations in wci.

coded as equal to unity if there are one or more incidents in a country in a single year. In Table 13, we re-estimate the baseline regression with panel logit using country- and year-fixed effects. We find the same results as before - expenditure cuts wreak havoc, tax increases do so only to a small extent and insignificantly. Overall, the budget balance matters for predicting unrest. We conclude that the role of outliers is not decisive in underpinning the relationship we established in baseline results.

[Table 13 here]

Which part of the variation in the explanatory variables is responsible for the link between austerity and unrest? Do increases in expenditure do as much to reduce unrest as cuts increase them? In Table 14, we examine this issue. Column (1) shows the results for expenditure increases. The coefficient is negative, but not large, and not significant. In contrast, if expenditure changes are negative, they matter a great deal for unrest, driving up CHAOS by 0.24 incidents for each standard deviation of expenditure cuts. Next, we repeat the exercise for output changes. Increases in output do much to cut unrest (col. 3), with a one standard deviation increase in output (3.77%) reducing CHAOS by 0.2 incidents on average. In contrast, declines do not set off major disruptions to the same degree (but the sample is relatively small). Overall, the results in table 12 confirm that the relevant identifying variation for expenditure changes comes from cuts; for output changes, it comes from positive growth, not recessions.

[Table 14 here]

Does greater media penetration increase or reduce unrest? Events in the Arab world in 2010 and early 2011 have led many observers to argue that greater media availability tightens the link between discontent and unrest. Data on media penetration is available in the Banks dataset.Three indicators are relevant - phone penetrations per capita, radio and television take-up.[16] Radio and television are unidirectional forms of media, allowing typically government-controlled messages to be broadcast to the population. If anything, they should make it easier for authorities to reduce unrest. Phones, on the other hand, allow peer-to-peer communication. All else equal, the expected effect is that they facilitate

[16]We disregard data on telegrams since they are unavailable after 1980.

organized protest. To analyze the data, and to avoid confusing results with the growing availability of broadcasting and telecommunications over time, we rank penetration rate in our sample in each year. We do so separately for each category, and then sum the ranks for each country-year. This gives a rank ordering of media penetration in country x in year y. We then divide the sample at the median. Table 15, col. (1) and (2) presents the results of estimating standard regressions for these two subsamples. We find that below-average media penetration is associated with a strong effect of expenditure cuts on unrest. Above the median, the effect disappears. In col. (3)-(6), we differentiate between uni-directional information media (info-media) and peer-to-peer telecommunications (peer-media). The attenuation of the effect of expenditure changes is milder for peer-media, and strong for info-media. For both types, the effect of economic conditions is always important above the median for media penetration, but below (in the case of peer media) the effect is small and poorly estimated. These results do not suggest that countries which, at any one point of time, have greater availability of mass media (relative to their neighbors) experience generally higher level of unrest.[17]

[Table 15 here]

[17]The obvious alternative is to condition on the absolute level of, say, phone penetration. Most of the variation in phone penetration, however, simply reflects GDP growth and the declining cost of telephones relative to all other goods; no clear pattern emerges.

5 Conclusions

The political economy literature on austerity suggests a paradox. There is no significant punishment at the polls for governments pursuing cut-backs (Alesina et al. 1998; Alesina et al. 2010), and no evidence of gains in response to budget expansion (Brender and Drazen 2008). Also, the empirical evidence on the economic effects of budget cuts is mixed, with some studies finding an expansionary effect, and others, a contractionary one.[18] Why, then, is fiscal consolidation often delayed, or only implemented half-heartedly? This paper suggests one possible reason why austerity measures are often avoided - fear of instability and unrest.[19] Expenditure cuts carry a significant risk of increasing the frequency of riots, anti-government demonstrations, general strikes, political assassinations, and attempts at revolutionary overthrow of the established order. While these are low-probability events in normal years, they become much more common as austerity measures are implemented. This may act as a potent brake on governments. In line with our results on expenditure, Woo 2003 showed that countries with higher levels of unrest are also more indebted. High levels of instability show a particularly clear connection with fiscal consolidation. We demonstrate that the general pattern of association between unrest and budget cuts holds in Europe for the period 1919-2008. It can be found in almost all sub-periods, and for all types of unrest. Strikingly, where we can trace the cause of each incident (during the period 1980-95), we can show that only austerity-inspired demonstrations respond to budget cuts in the time-series. Also, when we use recently-developed data that allows clean identification of policy-driven changes in the budget balance, our results hold. Finally, the results are not affected by using alternative measures of unrest. Contrary to what might be expected, we also find no evidence that the spread of mass media facilitates the rise of mass protests.

[18] Alesina and Ardagna 2010; Alesina et al. 2002; Guajardo et al. 2011. An early example in the literature is Giavazzi and Pagano 1990

[19] Alesina et al. 2010 also suggest that implementation of budget measures may be harder if the burden falls disproportionately on some groups. War-of-attrition models of consolidation are one alternative (Alesina and Drazen 1991).

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Data Appendix (For Online Publication Only)

Fiscal variables

Not cyclically adjusted fiscal variables (1919-2008):

Δ (exp/GDP): annual change in government spending as a share of GDP.

Δ (rev/GDP): annual change in government revenue as a share of GDP.

Δ (budget/GDP): annual change in government budget as a share of GDP.

Sources:

Mitchell (2007). The variables used are Total Central Government Revenue, Expenditure, GDP. The variables are usually expressed in millions of local currency units at current prices. We manually adjusted the values when the variables were expressed in different units, so that the ratios are always correct. Government budget is computed as revenues minus expenditure divided by GDP.

OECD (2010) The variables used are (codes reported in parenthesis): Total General Government Expenditure (GTE), Total General Government Revenue (GTR) and GDP ($B1\_GA$). The variables are expressed in local currency units at current prices. Government budget is computed as revenues minus expenditure divided by GDP: $(GTE - GTR)/B1\_GA$).

IMF (2010b) The variables used are Government Expense (% of GDP), Government Revenue including grants (% of GDP) and Cash Surplus/Deficit (% of GDP).

As a general rule we use - for OECD countries - data from Mitchell (2007) from 1919 to 1969 and data from OECD (2010) from 1970 to 2008. For non-OECD countries we use data from Mitchell (2007) from 1919 to 1990 and data from IMF (2010b) from 1991 onwards. Since there are a few exceptions to this rule, the dataset includes a variable called *source* that identify for each country-year observation the source of the fiscal variables.

Data cleaning: We exclude from all regressions fiscal data relative to the years of World War II (from 1939 to 1945, both included). We also decide to drop the change in fiscal variables in year 1919, since the change on the previous year might be affected by World War I. To take care of outliers we use as a general rule to drop all observations for which the change in the budget position with respect to the previous year is larger than 10% in absolute value. This procedure excludes 16 observations from the sample: Belgium in 1948 and 1949, Czech Republic in 1921 and 1924, United Kingdom in 1946 and 1947, Greece in 1930 and 1931, Ireland in 1965, Italy in 1920, 1923, 1936 and 1937, Netherlands in 1946 and 1948, Yugoslavia in1932.

Cyclically adjusted fiscal variables (1972-2007):

Δ (primary exp/GDP): annual change in cyclically adjusted current expenditure (% of GDP).

Δ (tot rev/GDP): annual change in cyclically adjusted total revenue (% of GDP).

Δ (gov invest/GDP): annual change in gross government investment (% of GDP).

Δ (primary budget/GDP): annual change in cyclically adjusted primary budget (% of GDP).

Source: Alesina and Ardagna 2010. The names of the variables in the original database are (in the same order as presented above): *blepgcurr*, *capogy*, *blept*, *blepprdef* (this last one taken with a minus sign).

Data cleaning: To take care of outliers we drop also in this subsample all observations for which the change in the budget position (including interest payments) with respect to the previous year is larger than 10% in absolute value. This procedure excludes 1 observation from the sample: Czech Republic in 1996.

Fiscal variables from the Action-based IMF database (1978-2008):

Δ $(\text{exp/GDP})_{ab}$: annual change in government expenditure (% of GDP).

Δ $(\text{rev/GDP})_{ab}$: annual change in taxes (% of GDP).

Δ $(\text{budget/GDP})_{ab}$: annual change in government budget (% of GDP).

Source: Devries et al. 2011

Social unrest variables

Domestic conflict data (1919-2008):

chaos: sum of gen_strikes, demonstrations, riots, assassinations, revolutions.

Principal Component Analysis: first principal component of chaos.

Weighted Conflict Index: it is a weighted sum of the 8 dimensions of domestic conflict as described in Banks 2010. It includes gen_strikes, demonstrations, riots, assassinations, revolutions as well as: guerrilla warfare, government crises and purges.

gen_strikes: any strike of 1,000 or more industrial or service workers that involves more than one employer and that is aimed at national government policies or authority.

demonstrations: any peaceful public gathering of at least 100 people for the primary purpose of displaying or voicing their opposition to government policies or authority, excluding demonstrations of a distinctly anti-foreign nature.

riots: any violent demonstration or clash of more than 100 citizens involving the use of physical force.

assassinations: any politically motivated murder or attempted murder of a high government official or politician.

revolutions: any illegal or forced change in the top governmental elite, any attempt at such a change, or any successful or unsuccessful armed rebellion whose aim is independence from the central government.

Source: Banks 2010

European Protest and Coercion Data (1980-1995):

We define as a single protest event an event recorded with the same entry in the following EPCD variables: *event*, *protester* and *location*. The variable *event* provides a short description of the main issues of each protest and it is the variable we use to sort protest events into different categories (expenditure cuts, tax increase, economy, peace, labour, education and ecology), *protester* indicates the protesting group or type (e.g. ferry workers, teachers, truck drivers etc.), *location* is the geographical location in which the event took place. Events that last more than one day are collapsed in this way in a unique observation, where the number of protesters and security forces members are averages across days. Our dependent variable is the number of events in each category occurred in each country-year. Notice that we only consider protest events whose number of participants is above 100 for riots and demonstrations and above 1000 for general strikes (no threshold is used for assassinations and attempted revolutions). These are the same threshold used in the Arthur Banks database.

Data cleaning: We exclude from our estimates one clear outlier, France in 1995, where we register 301 events, against the 2.1 protest events related to expenditure cuts per year that we register on average in Europe. This peak is due to the approval of the Juppé austerity plan that entered into force in 1996.

Source: Francisco 2006

Controls:

Primary budget: level of cyclically adjusted primary budget as a share of GDP.

Public debt/GDP: level of public debt as a share of GDP.

Δ (GDP/POP): annual growth in GDP per capita, PPP, 1990 Geary-Khamis dollars.

Media: sum of the rankings for each country in number of telephones (including cellular phones) per capita, radios per capita and televisions per capita.

Info-Media: sum of the rankings for each country in number of telephones (including cellular phones) per capita.

Peer-Media: sum of the rankings for each country in radios per capita and televisions per capita.

Sources:

Alesina and Ardagna 2010 for: Primary budget, Unemployment, Public debt/GDP, Δ inflation.

Maddison 2010 for: Δ (GDP/POP).

Patsiurko et al. 2011 for: Ethnic fragmentation

Banks 2010 for: Media, Info-Media and Peer-Media (name of the variables in the original dataset: *phone*6, *media*2 and *media*4)

Marshall and Jaggers 2010 for: Polity2 Index.

Figures

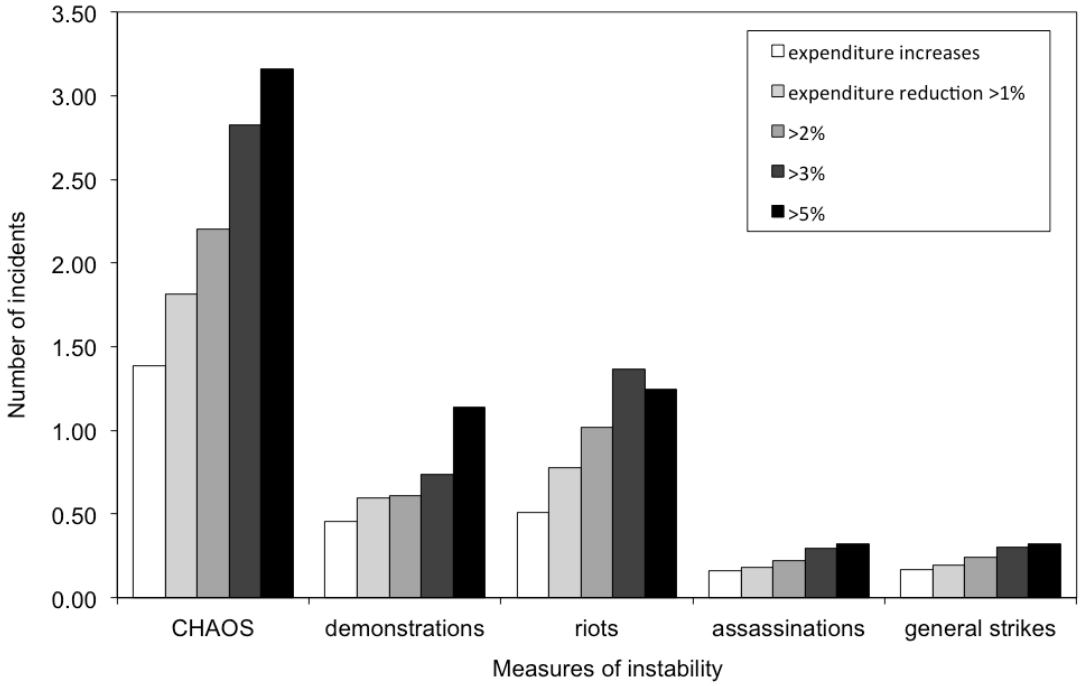

Figure 1 Frequency of incidents and the scale of expenditure cuts

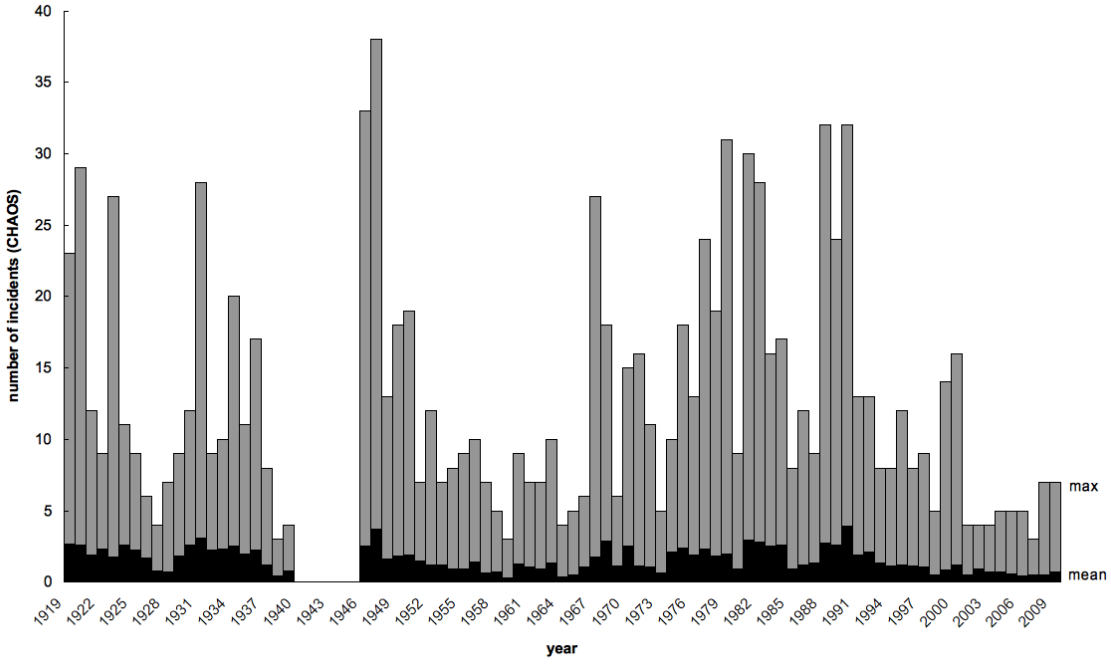

Figure 2 CHAOS over time

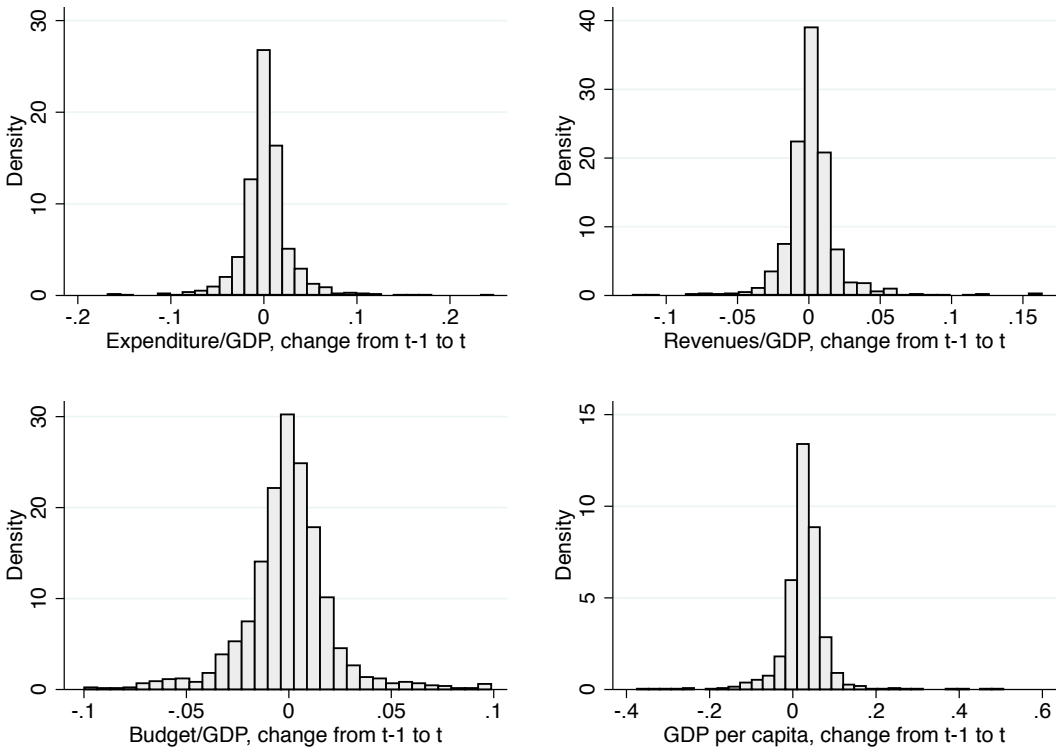

Figure 3 Changes in fiscal variables and GDP per capita, 1919-2008, all countries

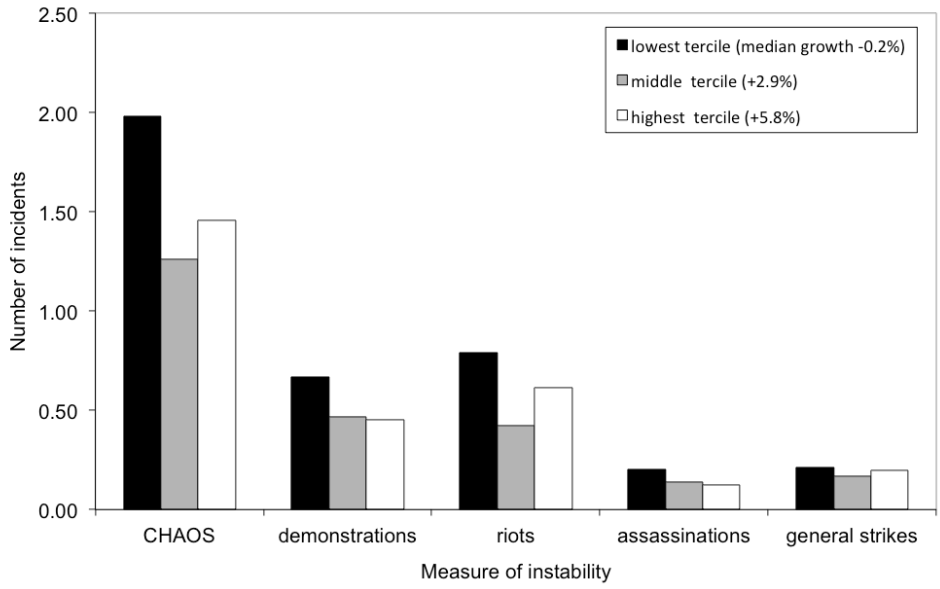

Figure 4 Frequency of incidents and economic growth

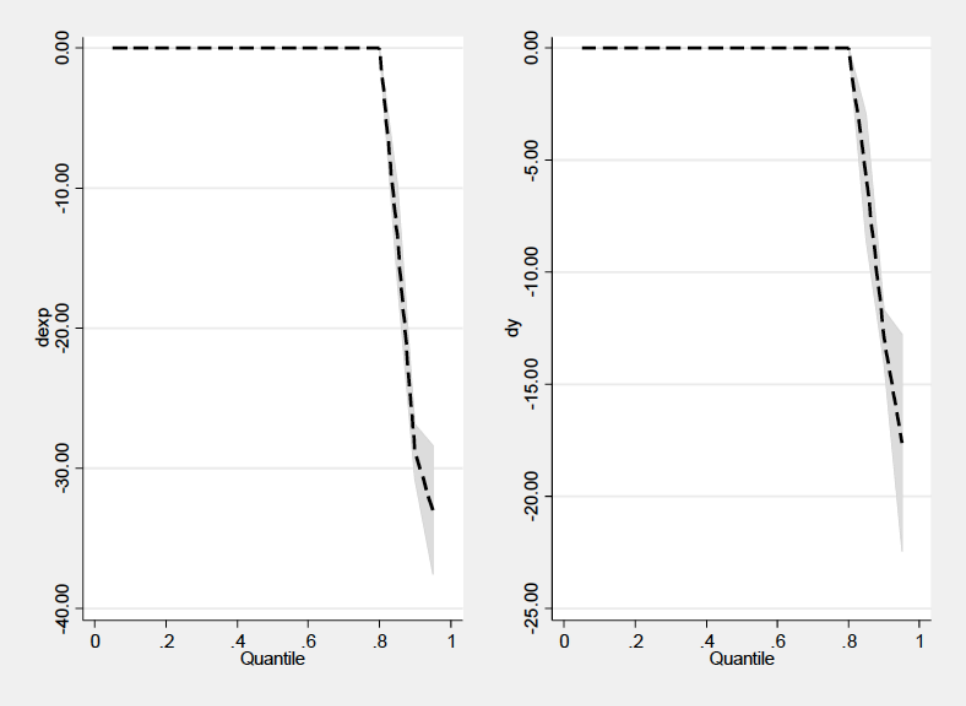

Figure 5 Quantile Regression Plot, Expenditure and Growth (95% confidence intervals)

Tables

Table 1 Descriptive statistics, main variables

Variable	Mean	Std. Dev.	Min.	Max.	N
general strikes	0.171	0.609	0	7	2236
demonstrations	0.487	1.461	0	17	2236
riots	0.55	1.832	0	25	2236
assassinations	0.152	0.696	0	15	2236
revolutions	0.082	0.334	0	5	2236
chaos	1.441	3.501	0	38	2236
Δ (exp/GDP)	0.002	0.026	-0.167	0.247	1586
Δ (rev/GDP)	0.002	0.019	-0.123	0.164	1529
Δ (budget/GDP)	0	0.022	-0.1	0.099	1522
Δ log (GDP/POP)	0.027	0.054	-0.374	0.506	1828

Table 2 Descriptive statistics, EPCD dataset

Main issue:	exp cuts	tax increase	economy	labour	ecology	peace	educ.
number of events	901	246	8,677	8,117	797	1,987	531
Averages per event:							
protesters	192,557	142,347	49,479	33,839	5,732	20,092	52,784
prot. arrested	.76	2	4.2	4.4	2	7.2	2.4
prot. injured	.36	.54	.3	.28	0.7	1.1	1
security forces	81	108	36	34	44	152	144
security forces injured	.35	.094	.12	.11	.092	.12	1.3

Table 3 Cross-correlation table

Variables	gen strikes	demonstations	riots	assassinations	revolutions	chaos	$\Delta \frac{exp}{GDP}$	$\Delta \frac{rev}{GDP}$	$\Delta \frac{budget}{GDP}$	$\Delta \log \frac{GDP}{POP}$
gen strikes	1									
demonstrations	0.352*	1								
	(0.000)									
riots	0.459*	0.462*	1							
	(0.000)	(0.000)								
assassinations	0.154*	0.189*	0.218*	1						
	(0.000)	(0.000)	(0.000)							
revolutions	0.175*	0.121*	0.257*	0.225*	1					
	(0.000)	(0.000)	(0.000)	(0.000)						
chaos	0.608*	0.769*	0.864*	0.440*	0.355*	1				
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)					
$\Delta \frac{exp}{GDP}$	-0.004	-0.050*	-0.061*	0.001	-0.031	-0.056*	1			
	(0.887)	(0.049)	(0.017)	(0.983)	(0.225)	(0.028)				
$\Delta \frac{rev}{GDP}$	0.060*	0.016	0.024	0.014	-0.019	0.032	0.554*	1		
	(0.020)	(0.546)	(0.357)	(0.581)	(0.453)	(0.223)	(0.000)			
$\Delta \frac{budget}{GDP}$	0.045*	0.011	0.070*	0.013	0.035	0.056*	-0.692*	0.217*	1	
	(0.084)	(0.658)	(0.007)	(0.613)	(0.178)	(0.029)	(0.000)	(0.000)		
$\Delta \log \frac{GDP}{POP}$	-0.012	-0.088*	-0.044*	-0.036	-0.092*	-0.077*	-0.141*	-0.038	0.170*	1
	(0.616)	(0.000)	(0.065)	(0.128)	(0.000)	(0.001)	(0.000)	(0.163)	(0.000)	

Notes: Significance levels: * p<0.1.

Table 4 Baseline result, full sample

Estimator:	Poisson fixed-effects			OLS fixed-effects		
Dependent var: chaos	(1)	(2)	(3)	(4)	(5)	(6)
Δ (exp/GDP)	-6.766***			-14.103**		
	(2.07)			(6.28)		
Δ (rev/GDP)		0.188			0.109	
		(2.07)			(4.33)	
Δ (budget/GDP)			8.131***			11.688**
			(2.47)			(5.59)
Δ log (GDP/POP)	–1.294	-0.709	-0.967	-2.362	0.034	-0.536
	(1.80)	(2.26)	(1.84)	(7.24)	(8.38)	(8.32)
Constant				5.419	5.705	5.463
				(3.55)	(3.81)	(3.59)
Year dummies	yes	yes	yes	yes	yes	yes
R^2				0.064	0.048	0.054
Observations	1350	1295	1288	1380	1325	1318
Groups	28	28	28	32	32	32

Notes: Standard errors clustered at country level in parentheses. Significance levels:*** p<0.01, ** p<0.05, * p<0.1. Countries included are: Austria, Belgium, Bosnia and Herzegovina, Bulgaria, Croatia, Czech Republic, Denmark, Estonia, Finland, France, Germany, Greece, Hungary, Ireland, Italy, Latvia, Lithuania, Netherlands, Norway, Poland, Portugal, Romania, Russian Federation, Serbia, Slovak Republic, Slovenia, Spain, Sweden, Switzerland, Ukraine, United Kingdom, former-Yugoslavia (in the Poisson specification: Slovenia, Serbia, Bosnia and Herzegovina, Lithuania dropped because all 0 outcomes in the chaos variables or too few observations).

Table 5 Baseline result, subsample: 1970-2007

Estimator:	Poisson fixed-effects			
Dependent var: chaos	(1)	(2)	(3)	(4)
Δ (primary exp/GDP)	-11.517**			
	(5.44)			
Δ (gov invest/GDP)		5.522		
		(5.74)		
Δ (tot rev/GDP)			13.926***	
			(4.31)	
Δ (primary budget/GDP)				12.532**
				(5.11)
Δ log (GDP/POP)	-19.079***	-14.447***	-9.857*	-15.582***
	(6.79)	(5.31)	(5.96)	(5.34)
Lag unemployment	1.847	3.025	2.782	1.075
	(7.97)	(7.94)	(8.38)	(7.89)
Ethnic fragmentation	3.146***	3.030***	3.154***	3.325***
	(0.97)	(0.95)	(0.99)	(1.06)
Polity2 index	-0.118	-0.060	-0.072	-0.131
	(0.25)	(0.24)	(0.23)	(0.25)
Log (POP)	15.579**	14.656**	12.859**	13.726**
	(6.09)	(6.07)	(5.74)	(6.20)
Public debt/GDP	-0.842	-0.767	-0.667	-0.767
	(1.11)	(1.18)	(1.15)	(1.14)
Δ inflation	-9.780***	-8.635***	-8.954***	-10.431***
	(3.22)	(3.13)	(3.08)	(3.06)
Primary budget	2.216	3.088	0.004	-1.498
	(5.00)	(5.32)	(4.64)	(4.89)
Year dummies	yes	yes	yes	yes
Observations	443	443	443	443
Groups:	16	16	16	16

Notes: Standard errors clustered at country level in parentheses. Significance levels:*** p<0.01, ** p<0.05, * p<0.1. All fiscal data are cyclically adjusted using the Blanchard methodology. Countries included: Austria, Belgium, Denmark, Finland, France, Germany, Greece, Ireland, Italy, Netherlands, Norway, Portugal, Spain, Sweden, Switzerland and United Kingdom.

Table 6 Fiscal adjustment and chaos by component of chaos, full sample

Estimator:	Poisson fixed effects				
Dependent var:	general strikes	demonstrations	riots	assassinations	revolutions
	(1)	(2)	(3)	(4)	(5)
Δ (exp/GDP)	-1.139	-7.462***	-7.297***	-5.407	-6.777***
	(4.57)	(1.99)	(2.22)	(3.90)	(2.45)
Δ log (GDP/POP)	-0.630	-3.017	-0.903	-1.245	-2.509
	(2.62)	(3.42)	(2.07)	(2.42)	(3.31)
Year dummies	yes		yes	yes	yes
Observations	1380	1380	1380	1380	1380
Groups	32	32	32	32	32

Notes: Standard errors clustered at country level in parentheses. Significance levels:*** p<0.01, ** p<0.05, * p<0.1. In this set of regressions we use a Poisson estimator with country dummies instead of a Poisson fixed-effects model to avoid non-symmetric or highly singular variance matrix in some specifications.

Table 7 Fiscal Adjustment and chaos by sub-period

Estimator:	Poisson fixed-effects			
	1919-1939	1946-1969	1970-1989	1990-2007
	(1)	(2)	(3)	(4)
Δ (exp/GDP)	-5.374*	-7.295***		
	(2.973)	(2.595)		
Δ (primary exp/GDP)			-9.524	-14.37**
			(7.335)	(6.343)
Δ log (GDP/POP)	-4.789***	8.676***	-22.62**	-18.10**
	(1.363)	(2.293)	(8.789)	(7.232)
Polity2 index	0.0349**	-0.0332	-0.325	5.967***
	(0.0163)	(0.0410)	(0.686)	(0.463)
Lag unemployment			8.976	-0.391
			(14.60)	(5.250)
Ethnic fragmentation			-3.025	3.238
			(149.4)	(3.091)
Log (POP)			6.320	10.90
			(14.25)	(11.43)
Public debt/GDP			-0.321	-0.729
			(1.095)	(1.194)
Δ inflation			-10.78***	-7.447
			(2.566)	(6.857)
Primary budget			9.384	11.28**
			(7.911)	(4.634)
Year dummies	yes	yes	yes	yes
Observations	200	317	146	264
Groups	15	15	9	16

Notes: Standard errors clustered at country level in parentheses. Significance levels:*** p<0.01, ** p<0.05, * p<0.1. The change in expenditure over GDP is the total expenditure over GDP until 1969 and it is the cyclically adjusted current primary expenditure over GDP starting from 1970. In the period between1919 and 1939 we exclude from the sample countries for which fiscal data is available but for which Polity2 score is lower than -8 (Italy 1928 to 1938, Germany 1933 and 1934, Austria 1934 to 1937 and Bulgaria 1935 to 1938).

Table 8 Protest events from EPCD, 1980-1995

Estimator:	Poisson fixed-effects		
protest event main issue:	expenditure cuts	tax increase	expenditure cuts
Δ (primary exp/GDP)	-32.975***		-35.707*
	(10.93)		(20.55)
Δ (tot rev/GDP)		-1.551	
		(24.21)	
Δ log (GDP/POP)			-18.325
			(20.11)
lag unemployment			16.408**
			(7.02)
Year dummies	no	no	yes
Observations	230	208	203
Groups	16	14	15

Notes: Standard errors clustered at country level in parentheses. Significance levels:*** p<0.01, ** p<0.05, * p<0.1.

Table 9 Placebo test, 1980-1995

Estimator:	Poisson fixed-effects						
protest event main issue:	exp cuts	economy	peace	labor	education	ecology	all
Δ (primary exp/GDP)	-35.707*	-5.528	7.649	-5.962	75.199*	7.860	0.435
	(20.55)	(5.98)	(8.98)	(4.90)	(40.31)	(8.91)	(6.03)
Δ log (GDP/POP)	-18.325	0.193	4.613	0.395	29.947*	1.109	-1.104
	(20.11)	(4.88)	(8.68)	(4.54)	(17.84)	(7.85)	(2.50)
Lag unemployment	16.408**	10.130**	9.401*	10.975**	-8.704	-4.683	2.121
	(7.02)	(4.67)	(5.39)	(5.22)	(17.20)	(8.08)	(2.88)
Year dummies	yes	yes	yes	yes	yes	yes	yes
Observations	203	203	203	203	150	203	203
Groups	15	15	15	15	11	15	15

Notes: Standard errors clustered at country level in parentheses. Significance levels:*** p<0.01, ** p<0.05, * p<0.1.

Table 10 IMF action-based fiscal adjustment, 1978-2007

Estimator:	Poisson fixed-effects		
Dependent var: chaos	(1)	(2)	(3)
Δ (exp/GDP)$_{ab}$	-46.917**		
	(20.24)		
Δ (rev/GDP)$_{ab}$		11.949	
		(22.93)	
Δ (budget/GDP)$_{ab}$			26.063**
			(12.27)
Δ log (GDP/POP)	-74.852**	-77.997**	-78.467**
	(33.69)	(31.64)	(31.86)
Lag unemployment	18.733	26.980*	26.349*
	(16.20)	(16.29)	(15.74)
Ethnic fragmentation	7.821	6.672	7.015
	(5.28)	(4.71)	(4.79)
Polity2 index	-0.704	-0.185	-0.101
	(1.08)	(1.13)	(1.03)
Log (POP)	34.682**	35.058*	34.411*
	(16.91)	(18.11)	(19.18)
Public debt/GDP	-6.897*	-7.214**	-7.527**
	(3.79)	(3.24)	(3.47)
Δ inflation	1.780	1.026	1.963
	(18.81)	(18.64)	(18.68)
Primary budget	9.125	9.188	10.314
	(6.51)	(6.83)	(6.34)
Year dummies	yes	yes	yes
Observations	101	101	101
Groups	11	11	11

Notes: Standard errors clustered at country level in parentheses. Significance levels:*** p<0.01, ** p<0.05, * p<0.1.

Table 11 Unrest and Institutional Quality

Estimator:	Poisson fixed-effects				
Subsamples: Polity2	< 0	> 0	> 5	> 8	$= 10$
	(1)	(2)	(3)	(4)	(5)
Δ (exp/GDP)	-7.627***	-3.489**	-2.831*	-3.047	-2.771
	(2.676)	(1.642)	(1.645)	(1.874)	(1.952)
Δ log (GDP/POP)	-4.551	-4.876***	-4.961***	-6.032**	-1.479
	(2.873)	(1.855)	(1.806)	(2.750)	(3.219)
Year dummies	yes	yes	yes	yes	yes
Observations	162	1188	1140	1009	882
Groups	9	28	28	23	18

Notes: Standard errors clustered at country level reported in parentheses. Significance levels:*** p<0.01, ** p<0.05, * p<0.1. In column (1) we use a Poisson estimator with country dummies instead of a Poisson fixed-effects model to avoid non-symmetric or highly singular variance matrix.

Table 12 Baseline result, alternative indicators of unrest

Estimator:	OLS fixed-effects					
Dependent var:	Principal Component Analysis			Weighted Conflict Index		
	(1)	(2)	(3)	(4)	(5)	(6)
Δ (exp/GDP)	-4.997*			-4136.228		
	(2.52)			(2505.22)		
Δ (rev/GDP)		0.559			-145.032	
		(2.32)			(2928.91)	
Δ (budget/GDP)			4.573**			4661.374*
			(2.19)			(2608.93)
Δ log (GDP/POP)	-1.131	-0.282	-0.553	-721.027	-476.209	-841.970
	(2.92)	(3.42)	(3.37)	(3497.03)	(4206.87)	(4196.26)
Constant	1.500	1.590	1.484	2529.253	2627.573	2520.204
	(1.44)	(1.53)	(1.44)	(1952.19)	(2044.53)	(1938.73)
Year dummies	yes	yes	yes	yes	yes	yes
R^2	0.067	0.055	0.061	0.076	0.066	0.069
Observations	1380	1325	1318	1380	1325	1318
Groups	32	32	32	32	32	32

Notes: Standard errors clustered at country level in parentheses. Significance levels:*** p<0.01, ** p<0.05, * p<0.1.

Table 13 Baseline result with chaos as a dichotomous variable

Estimator:	Logit fixed-effects		
	(1)	(2)	(3)
Δ (exp/GDP)	-4.974*		
	(2.89)		
Δ (rev/GDP)		1.926	
		(4.07)	
Δ (budget/GDP)			9.849**
			(4.03)
Δ log (GDP/POP)	-6.011**	-4.989*	-5.753**
	(2.56)	(2.71)	(2.78)
Constant	-1.954**	-1.946**	-1.793**
	(0.78)	(0.80)	(0.80)
Year dummies	yes	yes	yes
Observations	1355	1298	1291
Groups	27	27	27

Notes: Standard errors in parentheses. Significance levels:*** p<0.01, ** p<0.05, * p<0.1. The dependent variable is a dichotomous variable equal to 1 when chaos is greater than 0, and equal to 0 otherwise.

Table 14 Unrest, Expenditure Cuts and Growth

Estimator:	Poisson fixed-effects			
Subsamples:	Δ Exp>0	Δ Exp<0	Δ GDP>0	Δ GDP<0
	(1)	(2)	(3)	(4)
Δ (exp/GDP)	-1.641	-9.288***	-5.795***	-0.159
	(2.007)	(2.653)	(1.590)	(2.535)
Δ log (GDP/POP)	-3.865*	-2.974*	1.458	-6.762
	(2.308)	(1.715)	(2.123)	(6.345)
Year dummies	yes	yes	yes	yes
Observations	699	618	1153	187
Groups	26	24	28	20

Notes: Standard errors clustered at country level reported in parentheses. Significance levels:*** p<0.01, ** p<0.05, * p<0.1.

Table 15 Media Penetration and Unrest

Estimator:	Poisson fixed-effects					
	Media		Info-Media		Peer-Media	
Subsamples:	<median	>median	<median	>median	<median	>median
	(1)	(2)	(3)	(4)	(5)	(6)
Δ (exp/GDP)	-8.699***	-0.832	-9.599***	-0.809	-8.670***	-8.121***
	(2.409)	(4.989)	(2.532)	(4.898)	(1.975)	(1.705)
Δ log (GDP/POP)	-9.984**	-7.977	-9.996**	-8.504*	-0.764	-8.666***
	(5.029)	(4.879)	(4.052)	(4.770)	(2.361)	(1.965)
Year dummies	yes	yes	yes	yes	yes	yes
Observations	266	501	287	481	545	786
Groups	13	20	13	20	20	17

Notes: Standard errors clustered at country level reported in parentheses. Significance levels:*** p<0.01, ** p<0.05, * p<0.1. Media includes phones, radio and TV. Infor-Media includes radio and TV. Peer-Media includes phones. In column (2) and (4) we use a Poisson estimator with country dummies instead of a Poisson fixed-effects model to avoid non-symmetric or highly singular variance matrix.