(Why) Do Revolutions Spread?

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Abstract

The research of revolutions - attempts to overthrow or change the government relying on mass movements - has been mostly limited to qualitative case studies. Big-N, quantitative studies have focused on events that are similar, but not identical to revolutions, like civil wars, political violence, democratization or general instability. This paper introduces a new dataset of revolutionary events, covering independent states between 1946 and 2007. It tests if such events are "contagious", because opposition leaders and citizens use the situation in neighboring, similar countries as a heuristic to gauge hidden support for a popular uprising in their own country. The spatial model employed provides evidence that revolutions do indeed spread between autocracies, but less so among democracies. This is consistent with the mechanism suggested, as preference falsification is more common in autocracies. Other factors that increase the chance of a revolutionary event occuring are the size of the population, having a neither highly authoritarian nor very democratic regime, democratic neighbors and the absence of free resources like oil. Sharing regime type and geographical region with a country in revolution makes it more likely that the unrest will spread, while sharing the same cultural zone does not.[1]

1 Introduction

Why does the population of a seemingly stable country suddenly rise up against its government? A myriad of explanations have been put forward, from long-term underlying factors like poverty and oppression to short-term triggers like fraudulent elections or other ideosyncratic events that make the regime appear weak. In this paper, I focus on one such trigger factor, and examine quantitative evidence for the widespread belief that neighboring revolutions can cause such uprisings, in other words that revolutions are "contagious". I assemble a new database of revolutionary events covering almost all independent countries of the world since World War II, and conduct a quantitative test of the hypothesis that revolutions spread from a country to its neighbors. I also explore which characteristics of the "sending" and the "receiving" countries are conducive for such a spread.

Scholars have examined and found quantitative evidence that coups d'état (Li and Thompson, 1975), democratization (Brinks and Coppedge, 2006; Gleditsch and Ward, 2006), state failures (Goldstone, 2001) and civil wars (Gleditsch, 2007) spread to neighboring countries. However, no systematic test of a similar hypothesis for revolutions has been conducted so far, probably because the field has been neglected by quantitative research and no well-established global database on revolutions exists[2]. While a qualitative approach has its advantages, especially with such rare phenomena, it is often unclear how representative the few cases examined are, or if a certain finding is not due to pure coincidence. Some of the revolutionary events are also treated more prominently than others in the research.

As basis for my dataset I use the only database with a "revolution" variable in it, the Cross-National Time-Series Data Archive (Banks, 2008). Unfortunately, "revolution" there means any attempt to illegally change the government, and thus contains also events that occur without the level of popular participation commonly associated with revolutionary events, like coups d'état or secessions, and records for some country years appear highly improbable - some apparently had up to 9 "revolutions" in one year.

The following three sections of this paper introduce the first of its new contributions to the research on revolutions - the new dataset of "revolutionary events" covering almost all independent countries between 1946 and 2007. In section 2 I elaborate on their three defining elements: demands for political change with a sufficiently high level of *threat* [3] *to the central government*, coming from *groups*

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[2]Fortunately, while I was working on this paper, Chenoweth and Stephan (2011) have compiled a list of protest campaigns (whose definition seems to be very close to my definition of revolutionary events) and plan to expand this dataset in the future.

[3]This particular criterion could be questioned for two reasons. Firstly, it is difficult to measure objectively. I adress this problem in section 3 by relying on several different sources for the assessment of the threat level. Furthermore, section 5.2 shows that results holds if I use a lower

not currently associated with the regime, which rely mainly on *mass movements.* Using those criteria, I explain the coding procedure in section 3 and present some summary statistics in section 4.

Many qualitative regional studies have claimed that revolutions do spread, but given the human tendency to see patterns in random events, this could be due to selective perception. Furthermore, Franzese and Hays (2008, 574) point out that even a statistically signficant clustering can be explained by a clustering of similar trigger factors, like regional economic shocks or changes in the international order. The difficulty of distinguishing between those two explanations is known in the literature as "Galton's problem". The second contribution of this paper is thus the use of statistical analyses to control for other factors likely to increase the revolutionary potential, which might render the "contagion" effect spurious. Causes commonly mentioned in the scholarly literature are discussed in section 2.2, which also introduces the statistical model later used. Those control variables are added throughout section 5, without changing the main results: an increase in the percentage of neighbors experiencing a revolutionary event increases the chance of such an event occurring in a given country. Revolutions do spread.

Despite that this seems to be common wisdom, only a few scholars have tried to explain why revolutionary events would spread to neighbors (Beissinger, 2007). Based on the work of Timur (Kuran, 1989, 1991, 1997) and others (Lohmann, 1994), section 2.1 explains how both organizers and potential participants will be much safer and thus more willing to engage in opposition activities if there is massive support for a revolution. However, no one knows the exact level of support because of the widespread public preference falsification common under authoritarian regimes. I argue that if there is a popular upraising in another, similar country - and neighbors are often similar in regard to their political and economic development, culture, etc - citizens can use these events as a heuristic device to gauge potential support for a revolution in their own country. In section 5.1 I find that the "contagion" effect is stronger and more robust if the country is an autocracy, and if the neighboring countries with the revolutionary event are authoritarian - which is consistent with the mechanism proposed.

Finally, as a fourth new contribution, I explore in section 5.3 which similarities make a particular country a good heuristic device. Preliminary results suggest that sharing the same regime type is important, while a shared language, cultural zone or religion doesn't appear to be relevant. The conclusion summarizes the main points, describes the weaknesses of the approach chosen for this study and

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level of threat (anti-government demonstrations with more than 100 participants). Secondly, it can introduce selection bias. For instance, we might never observe certain demonstrations or revolutionary events because the government anticipates them and arrests opposition leaders or cracks down on demonstrators before they can constitute a visible threat. Section 5.2 discusses the selection bias in more detail.

discusses further research necessary in this area.

2 Revolutions, revolutionary events, and their causes

2.1 Definitions and mechanisms

For a long time, researchers have focused on "the 'great revolutions' of England (1640), France (1789), Russia (1917), and China (1949)" (Goldstone, 2001, 140), defined by Theda Skocpol as "rapid, basic transformations of a society's state and class structures [...] accompanied and in part carried through by class-based revolts from below" (Skocpol, 1979, 4). This definition lent itself to research based on case studies with emphasis on the outcome. Unsuccessful revolutions were seldomly studied, and comparisons with non-events - revolutions that failed to materialize - did not occur systematically.

During the 1980s, the field diversified and started to explore anti-colonial struggles, the relatively peaceful collapse of communist rule in Eastern Europe and the Soviet Union, and guerrilla wars and popular mobilization in Latin America. As Goldstone (2001: 142) points out, those apparently disparate phenomena still had three elements in common: "(a) efforts to change the political regime that draw on a competing vision (or visions) of a just order, (b) a notable degree of informal or formal mass mobilization, and (c) efforts to force change through noninstitutionalized actions such as mass demonstrations, protest, strikes, or violence."

This definition is closer to the notion of revolution that one uncovers in the work of scholars trying to formally model transitions to democracy (Acemoglu and Robinson, 2001) or threats to leadership survival (Bueno de Mesquita and Smith, 2009). There, revolutions are usually initiated by (a) actors outside the inner circle of power, which (b) threaten to change the government or the political system by unconstitutional[4] means mainly through (c) mass movement action. These three elements - intention and credible threat to change the government, externality of the leading actors, and popular participation - define what I will call "revolutionary events" in the new database. As the spread of civil war has already been docu-

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[4] "Non-constitutional" as defined by the ruling regime. The revolutionaries themselves usually see their demands as compatible with an alternative interpretation of the constitution. During most of the "color revolutions", for instance, the regimes evoked their constitutionally granted power to establish public order, while the opposition claimed that previous elections had violated electoral laws.

mented by Gleditsch (2007), I will, however, exclude organized armed uprisings typical of civil wars. I restrict popular participation to (not necessarily peaceful) actions aimed at demonstrating the level of support in the broader populace, like strikes, demonstrations, and riots. Another reason for excluding armed uprisings is that they, while contagious as well, likely follow a different spread mechanism than the one I am about to propose.

The underlying mechanism for a spread of "revolutionary events" I propose is an adaptation of Kuran (1997)'s explanation of why revolutions are hard to predict. According to him, there is always a difference between the preferences that individuals (secretly) hold and those which they profess in public. The gap is due to the fact that even in a "free" democracy, expressing unpopular opinions comes at least at the price of social ostracism. Under authoritarian regimes it might spell imprisonment or death. But by so falsifying their preference, citizens of an authoritarian state make others believe that the true level of opposition is lower than in reality, further adding to the pressure to adapt public preferences. Thus, even if a vast majority of its citizens would support a revolution, a country may appear calm and stable. However, a simple display of disobedience by a small group may encourage others to reveal their true preferences, resulting in a cascade of very sudden changes in public preferences and thus, potentially, in a revolution.

Kuran's theory, unlike others, can also explain why such a cascade might occur between countries. If there is widespread uncertainty about true public preferences and the costs associated with "guessing incorrectly" are high, then individuals might use events in similar countries as a heuristic to gauge the secret support for the opposition in their own country. And as neighboring countries often are similar to each other (Gleditsch, 2007, 299), the occurrence of a revolutionary event in a neighboring country will presumably increase their estimate of hidden discontent and thus their willingness to take part in protest activities.

Note that public preference falsification is less relevant for armed uprisings: in order to achieve their immediate goal of overthrowing the government, armed groups do not necessarily need broad support, as countless successful military coups indicate. Also, armed revolutionaries who fail to overthrow the government can still hope to carve out a stronghold of their own, and assuage their greed, if not their grievance, to use Collier and Hoeffler (2004)'s terms. The spread of civil wars documented by Gleditsch (2007) is thus likely due to a different mechanism, for example the result of refugee flows, regional availability of small arms and safe havens, etc.

There are of course, also other possible mechanisms that could explain the spread of revolutions: a revolutionary event might serve as a focal point, to use Shelling's terminology, which helps opponents of a regime to coordinate on a time (and, potentially, means) to express their grievances. However, it is not clear why a

nearby revolutionary event would serve as a better focal point than one far away, or why not any kind of event - revolutionary or not - would do. In fact, analysts often trace the onset of a revolution (retroactively) to a trigger event. Unfortunately, those trigger events are usually so ideosyncratic that there is no reliable data available[5] on how often a similar event did not trigger a revolution. Hence I won't be able to test those theories later on, except for the the proposed role of (fraudulent) elections that help coordinate an uprising (Fearon, 2011; Tucker, 2007). However, these theories, while providing an explanation why an uprising would start, don't offer any obvious explanation for how a revolutionary event would spread to a neighboring country.

If my adaption of Kuran's theory describes the true mechanism, then two corollaries concerning the "receiving" and the "sending" country's political system follow. Firstly, the effect of a neighboring revolutionary event will be stronger if the receiving country (whose chances of experiencing a revolutionary event we are trying to predict) is a "closed" regime[6]. In autocracies, citizens are prone to preference falsification, suspect that others engage in it as well, but are unable to asses its exact level. Through the mass media, opinion polls, and simple interaction with each other, citizens of more open societies have a much clearer image of the genuine level of support for their government, and thus are less likely to resort to estimates based on events in neighboring countries.

Secondly, because of widespread preference falsification and no independent means of establishing true public opinion, revolutionary events in authoritarian neighbors should be more surprising, and thus more likely to cause a sudden change in public opinion. The three hypotheses I intend to test are thus:

1. An increase in the share of neighboring countries experiencing a revolutionary event in the previous year increases the chance of such an event occurring in a given country.

2. The more autocratic the regime in a given ("receiving") country, the bigger the increase in revolutionary potential induced by neighboring events.

3. If the revolutionary events occur in a neighboring authoritarian country, the increase in revolutionary potential in the given ("receiving") country will be more pronounced than if the "revolutionary" neighbor is democratic.

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[5]Examples would be the self-immolation that triggered the Arab Spring or the hecklers interrupting Ceausescu's speech in Romania 1989 - collecting a complete dataset on self-immolations or catcalls during ruler's speeches seems an impossible task.

[6]It seems possible that extremely closed regimes would be able to prevent the news about a neighboring events spreading in their population. This would create a bias against finding any evidence for the spread of revolutions, and this downward bias would be more pronounced in authoritarian regimes.

2.2 The model and control variables

I model the onset of a revolunary event in the following way:

$$Pr(y_{i,t_k}^{revonset} = 1) = \frac{e^{\theta_{i,t_k}}}{1+e^{\theta_{i,t_k}}}$$

where

$$\theta_{i,t_k} = \mathbf{F}(y_{-i,t_0-t_{k-1}}) \cdot \gamma + \mathbf{X}_{i,t_0-t_{k-1}} \cdot \beta + \mathbf{G}(y_{i,t_0-t_{k-1}}) \cdot \lambda + \epsilon$$

The probability of observing the onset of a revolutionary event in a country i in time period t_k is a function of the latent "revolutionary threat" variable θ in that time period and country. The revolutionary threat, in turn, is determined by three different factors:

The first element is a vector of functions ($\mathbf{F}$) of revolutionary events occurring in earlier time periods in other countries ($-i$). In this paper, the main function used is the percentage of neighbors within a 500km radius (as calculated by the R package cshape (Weidmann et al., 2010)) having experienced a revolutionary event in the previous year[7]. I thus follow Beck et al. (2006, 28) in assuming that spatial influences operate with a temporal lag. If, as recent developments in the Middle East would indicate, revolutionary events spread faster than that, this method will bias the results against finding a significant influence[8].

In section 5.3, I also consider functions of previous revolutionary events in countries with similar regime types, language or culture zones.

Note that while the dependent variable is the "onset" of a revolutionary event, the independent variable measures simply if a country is experiencing such an event. In most cases, this does not make a difference, as most revolutionary event last less than a year. I have decided for this course of action because it is not immediately clear when in the course of several months' of mass movement action the neighboring citizens will update their beliefs. Nevertheless, main results remain the same if only the onset of a revolutionary event is taken for both the dependent and independent variable. Results are also very similar using a count-model with the number of strikes, riots and anti-government protests as dependent and the average number of such events in neighboring countries as the main independent variable.

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[7]This specification follows Gleditsch (2007). The results are similar for any cut-off between 1 and more than 2500km, and for more complicated measures that weigh more proximate countries more heavily. As the latter measure is not as easily interpretable as the share of "afflicted" countries, the 500km cut-off was nevertheless retained as basic model.

[8]Using a lag of two years, the results remain the same in some of the models and specifications, but not in others. Any lag larger than two years yields no significant results.

The second element is a vector of country specific control variables ($\mathbf{X}$). These are important in spatial models, because, if the true model contains both domestic (unit-level), spatially correlated outside influences (common shocks) and interdepent factors ("contagion"), then incorrectly modelling or ignoring any of the three will lead to an overestimation of the remaining factors (Franzese and Hays, 2008, 6). A plethora of possible alternative causes of revolutions have been suggested in the literature (Goldstone, 2001), and no standard model has emerged so far. I thus include the most commonly mentioned factors for which data is readily available, and which are likely confounders as they tend to cluster geographically as well:

Material well-being/long-term grievances (GDP per capita) - some of the most robust findings in the field of regime stability is the fact that countries with higher income tend to be more stable (e.g. Przeworski and Limongi (1997), possibly because deprivation of basic necessities fuels grievances. Alternatively, Bueno de Mesquita and Smith (2010) suggest a non-linear (inverted U) relationship: a very low GDP per capita causes grievances, but citizens lack the "coordination goods" (e.g. means of communication or transportation) to organize mass resistance.

Relative (short-term) deprivation (Growth in GDP)[9]: Gurr (1971) has suggested to focus on relative deprivation instead, and claimed that revolutions are especially common when increasing expectations cannot be met - for instance during economic downturns. A drop in economic growth has indeed been associated with the occurrence of successful revolutions (Kricheli and Livne, 2011), and is often attributed to a growth in unemployment.

The direction of the effect of *regime type* is frequently debated and might depend on the type of instability concerned (Goldstone et al., 2010). In our case, democracies should experience fewer revolutionary events, as the people have more legal means at their disposal to oust unpopular governments.

Finally, *population size* is often included in models of instability, as a "larger population is more likely to contain some group willing to rebel" (Gleditsch, 2007, 300).

This list is obviously far from exhaustive, but for many of the other causes proposed by qualitative studies, quantitative measures are not available or limited to a few observations. I do however experient with adding additional control variables, like the effects of government oppression or a high percentage of young

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[9]I use GDP growth instead of GDP per capita growth for two reasons. Firstly, while the exact level of GDP growth is often already a contested number in an authoritarian regime (see the debates surrounding China's figures), population estimates add another sources of error. Secondly, as data on unemployment is often unreliable or not available I use GDP growth also to capture the suggested effect of unemployment. Higher population growth certainly means that - in the long term - more jobs have to be created to keep unemployment low (and GDP growth high), but the newborn do not immediately enter the job market. GDP growth thus seems the more direct measure of the quantity of interest.

people among the population (Urdal, 2006).

The third element is a vector of functions ($\mathbf{G}$) of the country's "revolutionary history". As the unit of analysis is country-years, observations are not independent of each other. I follow Carter and Signorino (2010) by including time since the last occurrence of the event, as well as time squared and cubed to model time dependency.

3   Assembling the dataset of revolutionary events

Banks' "Cross-National Time Series Archive" appears to be the only dataset covering revolutions comprehensively. Unfortunately, its "revolutions" variable should rather be called "government instability". It is defined in the accompanying manual as "illegal or forced change in the top government elite, any attempt at such a change, or any successful or unsuccessful armed rebellion whose aim is independence from the central government". Thus it also contains attempts from within the government or by small military groups, which are more commonly referred to as coups d'état.

An independent coding of all the 10'000 country-years between 1946 or the year of independence and 2007 would have been a formidable task. Instead I chose to double-check Banks' 1300 country-years with at least one "revolution" to assess the nature of the events, and consult alternative compilations of revolutions[10] to ensure that events not recorded in Banks are not left out. The latter led to an additional 16 revolutionary events covering 33 country-years being added. Start or end dates of some events sometimes also needed adjustment, if the sources consulted indicated so.

The examination of each "revolution" in Banks followed a two-step procedure during which a variety of sources issued by different national and international organizations were consulted - see list in annex C[11].

It turned out that Banks' "revolutions" contain very different events: from IRA's terrorist attacks in Northern Ireland and the activities of the Sendero Luminoso in Peru, to countless military coups, both by high-level generals and lower level officers, armed uprisings by ethnic groups demanding autonomy or secession (e.g. the Tamils in Sri Lanka), urban and jungle guerrillas claiming to represent minorities, oppressed classes or the rural population (e.g. the FARC in Colombia or the Zapatistas in Mexico) to strikes, riots or demonstrations with demands for

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[10]E.g. the 114 state-failure events examined in the State Failure Task Force (Goldstone et al., 2010), and the 30 revolutions listed by John Foran (2005)

[11]For events that occurred before those organizations started reporting on a regular basis, accounts in scientific journals were also consulted

the resignation of top officials or a change in government policy (e.g. Kyrgyzstan's "Tulip Revolution").

The three defining elements mentioned in section 2.1 helped to single out revolutionary:

External nature of the threat: if the leaders of the attempt to change the government were themselves high-ranking officials or close relatives of the current leaders (e.g. the overthrow of Zahir Shah by his cousin in Afghanistan 1973), I classified it as a coup d'état. Unless the sources consulted mentioned accompanying or preceding anti-government demonstrations or riots, I also didn't code military coups by low-ranking officers as revolutionary events, because even "mutinies" rarely involve more than a few hundred combatants and thus lack the element of "mass participation". I ignored demonstrations occurring after the fact, because of the difficulty of assessing the sincerity of such displays. "Pacted" transitions or reforms from above, like Gorbachev's Perestroika, or Taiwan's transition to democracy, were also not coded as revolutionary events[12].

Mass participation is thus a key component: in order for an event to be coded as revolutionary event, the sources consulted had to mention demonstrations, strikes, or riots demanding the resignation of the government or a change in its key policy. Unfortunately, the sources didn't consistently mentioned the number of participants in such protest activities, which could have been a good measure for the level of the opposition's support and threat to the government.

The level of threat to the government is probably the aspect most difficult to asses. By defining revolutionary events not in terms of their outcome on society and the political system, one necessarily has to define a threat threshold level beyond which an event qualifies as a serious attempt to overthrow the government. At least the threat level of unsuccessful revolutions thus remains a subjective estimate that may differ considerable between observers. With a few exceptions I thus relied on Banks' assessment. However, the other sources usually mentioned actions by the government supporting the notion of a serious threat (e.g. declaration of state of emergency, deploying the army, mass arrests, etc).

If none of the sources mentioned any event that even remotely resembled a threat to the regime in a given country year, I departed from Banks' assessment and coded it as non-revolutionary. Of the 1305 country-years under scrutiny (containing altogether 1608 "revolutions"), 264 had at least one event which I couldn't clearly identify. If not completely miscoded, those 264 events are likely less noticeable threats like attempted coups and assassinations, or strives within the

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[12]This is not to claim that the opposition's mobilization strategy wasn't an important factor in those changes, or that those countries could not have experienced a revolution if the government wouldn't have implemented reforms. But in the case of Taiwan, for instance, the demonstrations never seem to have posed such an immediate threat that the opposition could have forced the regime's hand.
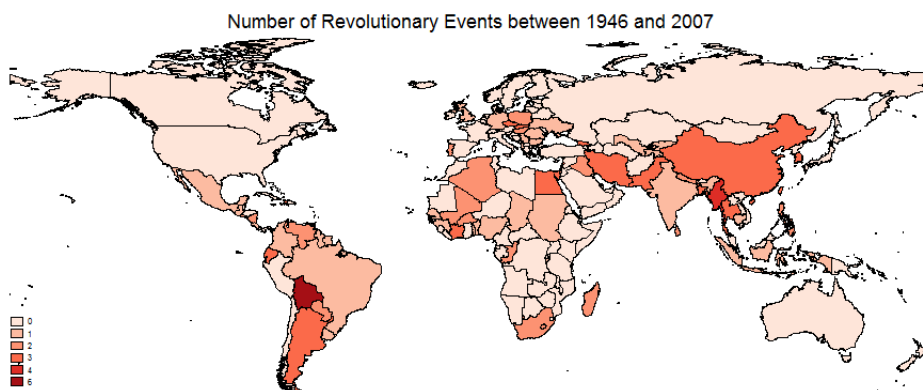
Figure 1: Map of Revolutionary Events Worldwide 1946-2007

government coalition.

Civil wars often pose a serious threat to governments, and there is a considerable overlap between the concepts of revolution and civil war. While some revolutionary events have occurred with no or little bloodshed, others have quickly deteriorated to an armed conflict (e.g. the recent events in Libya) or protracted terrorist campaigns (Algeria after 1992). As I am not interested in the eventual outcome of a revolutionary event, I saw no reasons not to code the initial years until deterioration as revolutionary. Civil wars that were triggered by coups d'état or limited armed uprisings (e.g. Fidel Castro's guerrilla operations against Batista), however, were coded as revolutionary events only during the period when they were accompanied by strikes, demonstrations or riots (outside the rebel controlled areas), or else not coded as revolutionary events at all.

4 Descriptive statistics

The final dataset covers all the world's countries - with the exception of a few small (island) states - from 1946 or their year of independence and 2007. It contains more than 8760 observations (country-years), but only 123 revolutionary events[13], which last for 246 country-years. Figure 1 shows a map of the world, indicating for each country how many revolutionary events have occurred since 1946 or the year of its independence from colonial occupation.

Contrary to common perception, revolutionary events do not only occur in authoritarian regimes, although they are more common there. About a third of all

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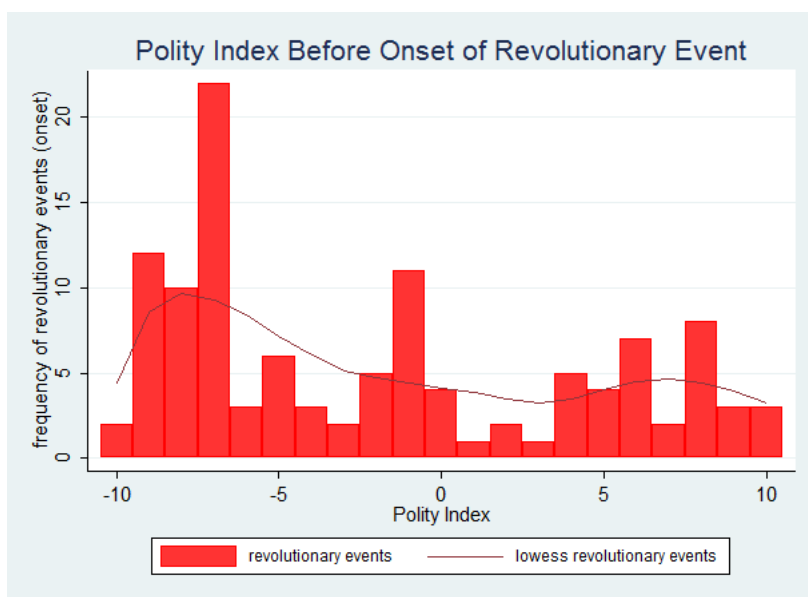[13]For a complete list of revolutionary events, see annex D

Figure 2: Revolutionary events under different regimes

revolutionary events have occured in countries that are classified as democracies by Cheibub et al. (2009)[14].

This is not just an artifact of the specific definition or measurement of democracy. If Gurr et al. (2011)'s Polity Index is used instead, the results look rather similar, as figure 2 shows: 16 revolutionary events have occurred in countries that have a Polity score of 7 or above, a level often taken as cut-off point for "full democracies". Some of them are secessionist movements, for which we might question the level of popular participation or threat to the central government, e.g. Great Britain's "Irish troubles", Israel's Intifadas, or India's Kashmir conflict. But they also contain the Philippine's "People Power II" demonstrations against President Estrada, resulting in the Supreme Court declaring the presidency vacant, or the unrest in Costa Rica in 1948 that led the National Republicans to admit their electoral defeat.

Revolutionary events also appear fairly equally distributed across time (figure 3), with the exception of two noticeable increases in the 1970s and around 1990.

How "good" is this new measure of revolutionary events? In the absence of a well-established alternative measure for revolutions with which one could correlate the new measure, this question is difficult to answer. However, there is one

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[14]They follow Przeworski (2000) in defining democracy according to whether executive and legislative powers are elected, whether (opposition) parties exist, are legal and have seats in the legislature, and if there was alternation in power.
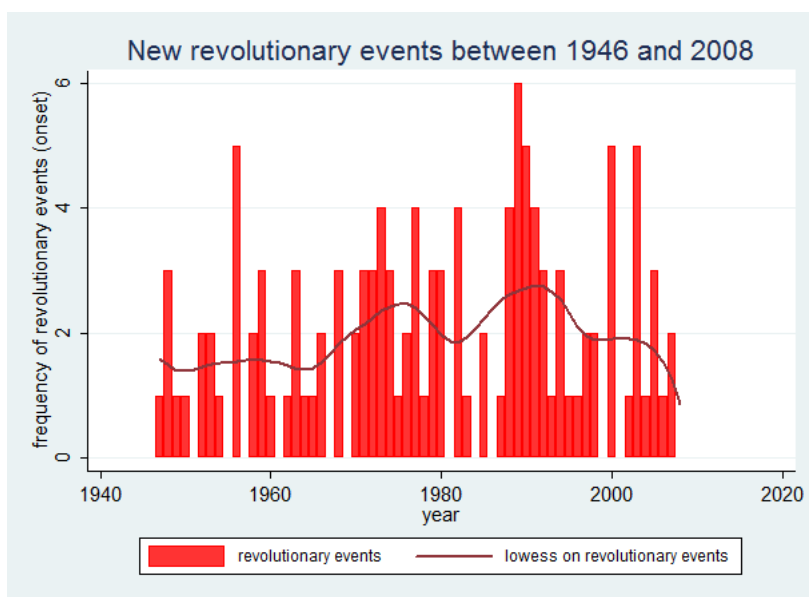
Figure 3: Revolutionary events over time

indication that the new database is indeed superior in measuring revolutionary events as defined above. Banks' database contains variables measuring the number of anti-government demonstrations and riots with at least 100 participants, and the number of general strikes aimed at national government policies or authority. Those three activities form an essential part of the opposition's repertoire during revolutions, and one would thus expect them to occur more often in country-years with revolutionary events than those without. This is indeed true for both Banks' and my new measure.

But as table 1 shows, the coefficients for the three activities added individually or jointly are more robustly significant with the new revolutionary events measure, despite the fact that Banks' dataset draws from the same source (the New York Times) for all four variables. The fit of the models using the new measure is also higher (see the expected percentage correctly predicted - ePCP) and adding the number of activities improves the predictive power over the modal category (ePRE) more[15].

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[15]ePCP is a measure of goodness of fit, calculating how many country-years the model correctly predicts as experiencing an onset of a revolution/revolutionary event or not. The ePRE indicates how much the model improves over one that would simply predict the modal category (no revolution) every time. Both ePCP and ePRE take into consideration how far off the prediction was (see Herron (1999)). The STATA code of Christopher N. Lawrence (http://www.cnlawrence.com/research/data) was used to calculate those measures.

Table 1: Number of anti-government demonstrations, strikes, and riots occurring in the same year as a "revolution" (Banks) or revolutionary event (new), logistic regression with standard errors (in parentheses) clustered on countries

revolution measure	Banks	Banks	Banks	Banks	new	new	new	new
demonstrations	0.101+			0.069	0.278**			0.241**
	(0.052)			(0.048)	(0.104)			(0.088)
strikes		0.252**		0.165*		0.785**		0.625**
		(0.052)		(0.073)		(0.111)		(0.127)
riots			0.086*	0.029			0.186**	-0.032
			(0.035)	(0.025)			(0.067)	(0.045)
constant	-1.774**	-1.753**	-1.761**	-1.795**	-3.850**	-3.793**	-3.724**	-3.979**
	(0.096)	(0.095)	(0.095)	(0.097)	(0.175)	(0.141)	(0.153)	(0.147)
country years	8260	8260	8260	8260	8732	8735	8734	8731
revolutionary country years	1258	1258	1258	1258	239	239	239	239
Log pseudolikelihood	-3502.82	-3511.90	-3507.18	-3495.31	-1011.25	-1026.94	-1052.74	-970.85
ePCP	74.32%	74.27%	74.30%	74.38%	94.93%	94.89%	94.76%	95.06%
ePRE	0.56%	0.35%	0.47%	0.76%	4.75%	4.00%	1.64%	7.17%

Significance levels: +: 10%   *: 5%   **: 1%

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5 Analysis of explanatory and control variables

5.1 Testing the main hypotheses

Table 2 presents the main results for the three hypotheses [16], with each model predicting the probability that a given country experiences the onset of a revolutionary event.

The first column of table 2 shows that an increase in the percentage of countries within a 500 km radius of the given country's borders experiencing a revolutionary event significantly increases the chance of a revolutionary event occurring. Not all of the control variables fare that well - while all of them except GDP growth do at least have the expected sign, only the democracy dummy and the log of the population have a significant effect[17]. Given these results, it is unsurprising that the fit of the model is weak, and increases predictive power by less than one percent.

In column 2 I add an interaction term between the democracy dummy and the neighborhood variable. As Berry et al. (2010) point out, a non-signficant interaction term does not necessarily mean that the calculation of a meaningful difference will be insignificant. And indeed do the results of the calculation correspond to the predictions of hypothesis 2: if the percentage of neighbors with revolutionary increases from half a standard deviation below the mean to half a standard deviation above, an autocracy is 0.18 percentage points more likely to experience a revolutionary event (from a yearly baseline probability of 1.13%, holding all covariates at their mean). This is equivalent to an almost 16% increase in the probability of experiencing a revolutionary event, and statistically significant at the 5% level. In a democracy, which has a yearly baseline probability of a revolutionary event occurring of 0.69%, the same change leads only to an insignficant increase of 0.05 percentage points.

Comparing the effect of revolutionary events in neighboring authoritarian states (columns 3 and 4) with those in neighboring democracies (columns 5 and 6) finds some evidence for hypothesis 3, as only the coefficients in the first two columns

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[16]Annex A explains the sources for all the variables used, annex B presents summary statistics

[17]Using non-corrected GDP per capita data from the World Bank (instead of purchasing power corrected and imputed data from Gleditsch (2008)), which unfortunately is only available for some countries starting in 1961, the coefficient does reach significance on at least the 10% level in most models. GDP growth remains insignificant. Alternative measures of relative deprivation (e.g. change in GDP growth with respect to previous year, difference to neighbors' growth or GDP per capita, infant mortality, unemployment, the level of government oppression or the "youth bulge") also remain insignificant. The same ist true for adding GDP per capita squared in order to simulate the possibility that citizens in an underdeveloped country might have grievances, but are unable to coordinate because they lack the resources.

Table 2: Explaning the onset of revolutionary events: logistic regression with standard errors (in parentheses) clustered on countries

dep. var.: revolution onset	R1	R2	R3	R4	R5	R6
% neighbors with rev. events	1.680*	1.949*				
	(0.749)	(0.804)				
neighboring rev. events * democracy		-1.113				
		(1.809)				
rev. events in neighboring autocracies			2.534*	2.766*		
			(1.056)	(1.178)		
rev. events (aut.) * democracy				-0.849		
				(2.292)		
rev. events in neighboring democracies					0.706	1.403
					(1.473)	(1.425)
rev. events (dem.) * democracy						-2.853
						(4.749)
democracy (dummy)	-0.541*	-0.489+	-0.508+	-0.477+	-0.514+	-0.486+
	(0.272)	(0.276)	(0.271)	(0.278)	(0.273)	(0.270)
log GDP per capita	-0.096	-0.098	-0.110	-0.112	-0.108	-0.109
	(0.094)	(0.094)	(0.093)	(0.094)	(0.095)	(0.095)
GDP growth	0.008	0.008	0.008	0.008	0.008	0.008
	(0.009)	(0.009)	(0.009)	(0.009)	(0.009)	(0.009)
log of population	0.218**	0.220**	0.201**	0.201**	0.203**	0.204**
	(0.058)	(0.057)	(0.057)	(0.057)	(0.057)	(0.057)
no. of previous rev. events	0.223	0.223	0.254+	0.254+	0.245+	0.243+
	(0.136)	(0.135)	(0.135)	(0.135)	(0.134)	(0.135)
time since last rev. events	-0.047	-0.048	-0.045	-0.046	-0.049	-0.049
	(0.049)	(0.050)	(0.050)	(0.050)	(0.050)	(0.050)
time squared	0.002	0.002	0.002	0.002	0.002	0.002
	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
time cubed	-0.000	-0.000	-0.000	-0.000	-0.000	-0.000
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
constant	-5.261**	-5.274**	-5.053**	-5.056**	-5.009**	-5.012**
	(0.953)	(0.949)	(0.939)	(0.937)	(0.935)	(0.933)
No. of country years	7312	7312	7346	7346	7346	7346
No. of rev. onsets	104	104	104	104	104	104
Log pseudolikelihood	-524.37	-524.23	-524.97	-524.92	-526.84	-526.67
ePCP	97.21%	97.21%	97.22%	97.22%	97.22%	97.22%
ePRE	0.60%	0.61%	0.58%	0.58%	0.53%	0.54%

Significance levels: +: 10%   ∗ : 5%   ∗∗ : 1%, independent variables are lagged by one year

are significant. An increase in revolutionary events among neighboring democracies apparently doesn't significantly increase the chance of revolutionary event occurring under either regime type. This is exactly what we would expect see if the "contagiousness" of revolutions is due to neighboring citizens adjusting their estimations about each other's preferences - neither would we see much public preference adjustments in open societies, nor would we expect events in open societies to cause surprise and hence adjustments in neighboring countries.

5.2 Robustness checks and supporting evidence

What if the results hinge not on the definition and measurement of democracy, but on that of revolutionary event? A second person is currently coding the material independently, in order to check for inter-coder reliability. In the meantime, Banks (2008)'s dataset provides an alternative measure for revolutionary events: the variable "anti-goverment demonstrations".

Independent of the results from the second coding, this an alternative measure seems useful. If there are fewer revolutionary events under democracies than among autocracies, the difference in significance could simply be due to the small number of observations in democracies. Table B in annex B thus shows that the results hold if revolutionary events are measured with this lower threat threshold: it uses the occurrence of "anti-government demonstrations" - which are roughly equally common under democracies and autocracies - as dependent variable instead.

Among the control variables, only one change is notable: if revolutionary events are measured with this very low level or threat, the regime type becomes a significant predictor - democracies are much more likely to see demonstrations in any given year than autocracies. This is not surprising and tails with the general observation (Ginkel and Smith, 1999) that the opposition in autocracies is less likely to demonstrate because it's costly. But if they do, it is a sign that they are sure of their own ability to threaten the government.

Another worry is the small size of our dataset, which could make the results depend unduely on one or a few cases. While more than 7000 observations of about 180 countries might appear more than sufficient, King and Zeng (2001, 143) point out that in rare events data, "non-events" often contain little information. Their rule of thumb would indicate that this dataset contains as much information as a more "balanced" dataset of 300-600 country-years - a number close to the threshold where logit coefficients start to suffer from small sample bias. I have thus rerun all models presented here using their estimate that corrects for problems due to finite samples and rare events. However, the size of the coefficients, their significance, and other estimates remained the same.

Nevertheless, with an effective number of 100 revolutionary events in a bit more than half a century, it seemed advisable to test if the results depend on the presence of one specific country or year in the sample. Dropping one country at a time and rerunning the logistic regression of column 2 in table 2 reveals that if either Nepal or Ivory Coast were excluded from the sample, the size of the coefficient would decrease, but remain significant on the 10% level. The relationship between neighboring revolutionary events and event onset in authoritarian countries becomes less robust if the year 1989 is omitted (see figure 5 in annex B) - although it remains significant on the 10% level if King and Zeng's rare events estimation technique is used. Year fixed effects also leave the results unchanged. Overall the estimations thus do not seem to be unduly influenced by one specific observation.

Is it possible that the effect is an artifact of the data-collecting process? One alternative explanation for the clustering of revolutions could be a "spotlight" effect: once a revolution unfolds, the attention of journalists and the global public will be drawn to the region as a whole, and newspapers are thus more likely to report on another revolutionary event occuring nearby. The use of country reports and other sources that appear in regular intervals and focus on one country only to verify the entries in the database make this explanation less likely, however. It is possible, though, that neighboring events influence how the country report assesses the threat level of a local event: if a dictator has just been overthrown nearby, a relatively small demonstration might suddenly appear a lot more dangerous - and thus worthy of being mentioned - than it would otherwise have seemed.

So far I have measured the level of openness of a society (and hence how well the government and opposition know the masses' "true" preferences) using a dichotomous measure defined mainly by whether elections result in changes in the government or not. However, the more relevant aspect is presumably the level of freedom of speech and press or open party competition in a country. As a first robustness check, I thus substitute a different measurement of democracy, the Polity IV index (Gurr et al., 2011)[18].

Scholars have often proposed an inverted U-shaped connection between democracy and stability, with the very autocratic and very democratic regimes being most stable (Urdal, 2006, 613). Column 1 in table 3 shows this to be true for revolutionary event attempts as well: a higher Polity score increases the chance of such an event, but this effect is at some point overpowered by the negative effect of Polity score squared.

Figure 4[19] below demonstrates the effect of one standard deviation increase in the percentage of neighbors experiencing a revolutionary event for countries with

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[18]For easier readability, the polity score was recoded to range from a highly autocratic 0 to a highly democratic 20.

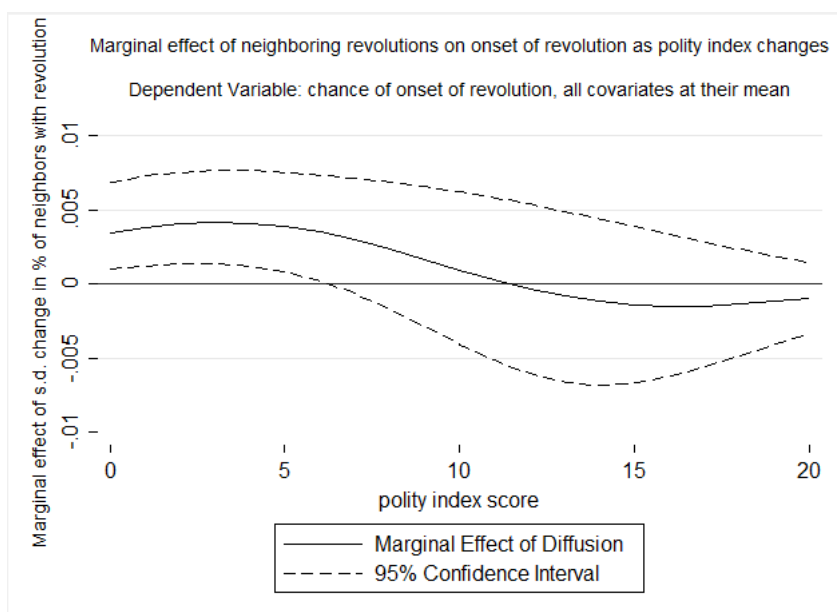[19]Brambor et al. (2006)'s code was used to construct this graph

Figure 4: Percentage point increase in the chance of experiencing a revolutionary event if the percentage of neighbors with revolutionary events increases by one standard devation for different regimes ranging from 0 (highly authoritarian) to 20 (highly democratic)

different polity index scores.

The effect of neighboring revolutionary events is, as predicted, the largest for very authoritarian governments and decreases as the regimes becomes more democratic. Beyond an (adjusted) Polity index score of 6 the effect is not significant anymore[20].

Some of the earlier attempts to explain revolutions, like that of Skocpol, endorsed relatively deterministic explanations: a combination of structural and societal factors cause revolutions, while the actions of the government and the revolutionaries seemed irrelevant (Goldstone, 2001, 140). I treat them as strategic actors instead, as players that anticipate the actions of others and act accordingly. This implies, however, that the actions that we observe are interdependent and censored (Smith, 1999, 1254), and it seems worth pondering how strategic action not just by the opposition (which is the focus of this paper), but also by the government, might bias the results.

In analogy to Smith's assessment on how strategic behavior changes what out-

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[20]In figure 6 in annex B, the same relationship is even more distinct for revolutionary events measured at the lower threat level of anti-government demonstrations.

Table 3: Explaning the onset of revolutionary events. Logistic regression with standard errors (in parentheses) clustered on countries

Dep. variable: rev. onset	R7	R8	R9	R10	R11
% of neighbors with rev. events	3.929**	6.252*	3.843**	4.562*	0.0803*
	(1.253)	(2.638)	(1.235)	(1.954)	(0.0375)
neighboring rev. events	-0.360+	-0.963**	-0.358+	-0.437	-0.0055*
* polity index	(0.213)	(0.351)	(0.207)	(0.292)	(0.0024)
polity index score	0.244**	0.255*	0.253**	0.170+	0.0027*
	(0.075)	(0.116)	(0.076)	(0.096)	(0.0013)
polity squared	-0.014**	-0.014**	-0.013**	-0.008+	-0.0002**
	(0.003)	(0.005)	(0.004)	(0.005)	(0.0001)
log GDP per capita	0.010	-0.094	-0.027	-0.096	-0.0007
	(0.101)	(0.142)	(0.107)	(0.116)	(0.0012)
GDP growth	0.006	0.012	0.005	-0.004	0.0001
	(0.010)	(0.018)	(0.010)	(0.013)	(0.0002)
log population	0.245**	0.166+	0.226**	0.220**	0.0027*
	(0.065)	(0.097)	(0.064)	(0.075)	(0.0011)
no. of previous rev. events	0.177	0.058	0.169	0.093	0.0049+
	(0.147)	(0.200)	(0.146)	(0.162)	(0.0026)
oil exports		-0.079+			
		(0.046)			
oil exports * polity		0.006*			
		(0.003)			
% neighbors democratic			1.298*		
			(0.568)		
% neighbors democratic			-0.091+		
* polity			(0.051)		
Δ US military aid				-0.000	
				(0.000)	
foreign aid shock (dummy)				0.514*	
				(0.237)	
Soviet Union 1989				1.962*	
				(0.994)	
Africa 1990-92				0.290	
				(0.719)	
λ					-0.0185*
					(0.0081)
No. of country-years	6546	3511	6434	3996	5934
No. of rev onsets	101	59	101	69	94
Log pseudolikelihood	-495.53	-277.71	-492.11	-334.07	3960.03
ePCP	98.46%	98.32%	98.43%	96.64%	-
ePRE	0.83%	1.24%	0.89%	1.01%	-

Significance levels: $+$:10% $*$:5% $**$: 1%, independent variables are lagged by one year, time, time squared, time cubed, and constant omitted

comes we will actually observe in conflicts between two governments, we would expect to see less revolutionary events occurring than in a "deterministic" world. Successful and unsuccessful revolutions are costly for both sides, and thus both sides should prefer a negotiated transition or reform. Thus we are less likely to find any impact of neighboring revolutionary events: assuming that the support for revolution is higher than previously thought, the opposition will indeed try to initiate revolutionary activities - but as the government can observe neighboring events as well, it will attempt to counteract any revolutions, thus biasing the size of any effect downwards.

The size of this downward bias, however, presumably varies with how well the government can in fact react to increased revolutionary pressure. This ability is of course multi-faceted, and hence I will once again focus on factors that could create the appearance of "contagion". Bueno de Mesquita and Smith (2010) provide a theoretical explanation and evidence that dictators with access to "free" resources like oil revenues (which, as we know, also show regional clustering) or foreign aid are more likely and able to respond to an increase in unrest with repression. If revolutionaries facing such dictators are aware of this, they should thus hesitate to initiate protests. In table 3, I add as control variable the percentage that oil exports make up of a country's GDP and an interaction term with the polity index. The respective coefficient in column 2 does indeed have the expected sign and is significant. As predicted by the selectorate theory, it diminishes in size the more democratic a government is, as the positive sign of the interaction term indicates. Access to oil revenues thus is not just detrimental for democratization in general, but also appears to specifically prevent revolutions[21]. Note, however, that the effect of neighboring revolutionary events remains once again positive and significant.

As mentioned in section 2.2, common regional shocks could account for an apparent "clustering" of revolutionary events. Any regional economic shock is likely to be proxied by GDP growth. A regional policy shock, however, is not as easily captured. What if the withdrawal of support by a regional hegemon (e.g. the refusal of Gorbachev to intervene on behalf of the communist regime in Poland after the 1989 elections) instead of a domino effect causes a cluster of revolutions?

One such common foreign policy shock could be a change in the international system that prompts countries to withdraw support for (authoritarian) regimes in a region - as supposedly happened to developing countries (especially in Africa) after the end of the cold war. Such a change would presumably show up in the level of international aid granted to a government. Column 4 in table 3 thus adds a dummy indicating a decrease in the level of foreign aid (of GDP) from one year

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[21]Other free resources like foreign aid or nontax revenue did not turn out to have a significant effect

to the other[22].

Alternatively, withdrawal of military support could increase the revolutionary threat. However, at least the change in military support by the United States (for which there is reliable data available) does not seem to have any effect[23].

Unfortunately, there is no such data on support provided by the Soviet Union. I thus include a dummy variable for all Soviet Union clients in 1989, as well as dummy for African states in 1990-1992 (the end of the cold war). While the coefficient on the Soviet Union dummy is signficant, the main result is again unchanged.

Column 5 finally shows the result of a spatial error model. In a spatial error model the value of y is not expected to be influenced by the value of y for other countries - instead, the errors of the model are assumed to be spatially correlated (Ward and Gleditsch, 2008, 55). Such a correlation of the errors among certain countries (e.g. allies of the Soviet Union) at certain times is exactly what one would expect to see if the "common foreign policy shock" hypothesis is correct. I thus estimate the following cominbed lag and error model, using two different weight ("neighborhood") matrices - the first matrix ($\mathbf{F}$ for the spatially and temporally lagged revolutions) assigns as neighbors again countries that are geographically close, while the second ($\mathbf{W}$ for the error terms) assigns as neighbors allies[24] in different years (see also Franzese and Hays (2007, 10):

$$\mathbf{y}^{revonset} = \gamma\mathbf{F}\mathbf{y} + \mathbf{X}\beta + \epsilon$$

$$\epsilon = \lambda\mathbf{W}\epsilon + u$$

Where $\mathbf{X}$ is a vector of domestic factors including past revolutions.

Unfortunately, available macros or packages for STATA and R only calculate spatial error models for continuous dependent variables. The results of the final column in table 3, where I use a generalized least square maximum likelihood estimation technique implemented in the "spatreg" macro for STATA, should thus be viewed with caution. The coefficient on λ, the measure of how correlated the error terms among allies are, is indeed signficant, but so is that on the neighboring revolutionary events[25].

By now this study has presented several pieces of evidence that revolutionary events do indeed spread. But there are alternative mechanisms that could explain

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[22]Any other way of coding the foreign aid shock, e.g. just taking the difference in aid level over time or a dummy for considerable decreases in foreign aid, are not signficant and do not change the main result.

[23]Data from http://gbk.eads.usaidallnet.gov/, missing values assumed to be 0 USD

[24]The data on alliances worldwide between 1950-2000 was downloaded from the correlates of war project, version V3.03 Gibler and Sarkees (2004)

[25]It is negative because the alliance data had to be coded in reverse - i.e. 1 meaning not allied, 0 meaning allied.

such a spread. Brinks and Coppedge (2006), for instance, have established that authoritarian countries surrounded by democracies are more likely to democratize. Two explanations for this "democratic diffusion" effect have been suggested. Cederman and Gleditsch (2004) model a system in which democracies (a) do not attack each other, (b) aid other democracies under attack, and (c) foster democratic reforms in neighboring countries. Modelski and Perry (1991, 32) treat the process as a form of diffusion of technological innovation. Being exposed to a democracy in the neighborhood, citizens, opposition and government members have a higher chance of learning about democracy's advantages. It thus seems possible that a high number of democratic neighbors increases the revolutionary potential: either because those neighbors provide support for the opposition, or because citizens value the possible success of a revolution more. In both cases such an increase is likely to be less pronounced or inexistent in democratic "receiver" countries. In this alternative mechanism, the cause of the "revolutionary spread" wouldn't be the neighboring revolutionary event itself, but the fact that some revolutions will succeed in creating democratic regimes in the region, which then in turn excert influence on neighboring countries.

To separate those two effects, I estimate what Plümper and Neumayer (2010) call a "spatial-x" model. In this model, the independent variables of neighboring countries influence the dependent variable of the country in question, irrespective of the outcome in the dependent variable in the neighboring countries. In table 3, column 3, I add a control variable for the percentage of democratic neighbors. This control is indeed significant, and so is its interaction term with the receiving state's polity score - a closer analysis reveals that a standard deviation change in the percentage of democratic neighbors[26] (from about 18% to 50%) significantly increases the chance of a revolutionary event occurring in any authoritarian regime with a (adjusted) Polity score lower than 7.

However, the coefficients of the neighboring revolutionary events variable and its interaction term remain significant and their size is largely unchanged. Thus democratic "peer pressure" seems to contribute to the revolutionary potential, but neighboring revolutionary events maintain their own specific impact.

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[26]As the democracy dummy is coded according to what regime was in place at the end of the year, I use the percentage of democratic neighbors in the previous year as independent variable. It is possible that it would take the revolutionaries more than until the end of the year of revolution onset to achieve their goal. However, not lagging the variable does not change the results.

5.3 Why do revolutionary events spread to some countries and not to others?

Table 4 explores the question why some countries would be perceived as similar enough to serve as heuristics for a given country. Column 1 examines the effect of revolutionary events occurring in countries with a shared official languages. Column 2 looks at countries in the same "culture zone" - as defined by Huntington (1997) and implemented by Henderson (2005, 467-9)[27]. Column 3, finally, looks at countries with similar regime types. Following Geddes (1999b), the possible regime types are democratic, military, personalist, one-party and monarchy.

The coefficients all have the expected sign, but remain insignificant except for that on similar regime types. One possible interpretation of this result is that revolutionary events grant the opposition in other countries the opportunity to gauge the strength of their regime type in suppressing the people.

This finding is relatively robust to changing the measure of openness and adding different control variables (remains significant at least on the 10% level) and dropping countries or years from the dataset, which is surprising, given the relatively high correlation (between 0.19 and 0.39) between the different conceptions of "neighborhood".

6 Conclusion

In my research, I have put together what I believe is the first systematic database on revolutionary events worldwide between 1946 and 2007, and have found evidence in favor of the hypothesis that revolutions "spread" to neighboring countries. The effect appears to be robust to controlling for a variety of confounders, and remains significant even when a different measure for revolutionary events with a lower-threat level is used. The results should nevertheless be treated with caution, as the methods and models used cannot account for all possible sources of endogeneity. Especially reverse causality is a possible problem, as governments anticipate revolutionary events and react accordingly. Nevertheless, the evidence suggests that future research into the causes of revolutions should not treat them as independent events and instead attempt to model the interdependence.

I have also found evidence for a specific mechanism through which revolutionary events spread. The effect of neighboring events is stronger in autocracies,

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[27]Huntington conceptualizes cultural zones as clearly delineated geographic areas mainly determined by a pre-eminent religion. It has been criticized (e.g. Henderson (2005) but seems to correspond to a notion often employed by the wider public. The different culture zones are: Western, Latin American, African, Islamic, Sinic, Hindu, Orthodox, Buddhist, and Japanese.

Table 4: Non-geographical concepts of "neighborhood": logistic regression with standard errors (in parentheses) clustered on countries

Dep. var.: rev. onset	R12	R13	R14	R15
% neighbors with rev. events	3.823**	3.517**	2.846+	2.194
	(1.203)	(1.348)	(1.493)	(1.589)
neighbors' rev. events	-0.302	-0.298	-0.244	-0.146
* polity	(0.194)	(0.220)	(0.231)	(0.230)
rev. in countries with	1.005			0.140
same language	(1.652)			(1.882)
same language rev. events	-0.646+			-0.410
* polity	(0.376)			(0.375)
rev. in countries with		1.407		0.212
same culture/religion		(2.552)		(3.048)
same culture rev. events		-0.021		0.208
* polity		(0.269)		(0.242)
rev. in countries with			7.796*	10.253**
same regime type			(3.681)	(3.785)
same regime type rev. events			-1.367*	-1.711*
* polity			(0.659)	(0.708)
polity index score	0.264**	0.271**	0.286**	0.352**
	(0.078)	(0.083)	(0.091)	(0.102)
polity squared	-0.014**	-0.016**	-0.015**	-0.018**
	(0.004)	(0.004)	(0.004)	(0.004)
log GDP per capita	0.006	-0.013	0.026	0.008
	(0.101)	(0.107)	(0.107)	(0.115)
GDP growth	0.005	0.010	0.005	0.009
	(0.010)	(0.010)	(0.013)	(0.013)
log population	0.235**	0.272**	0.244**	0.256**
	(0.065)	(0.073)	(0.069)	(0.077)
No. of country years	6546	5971	5971	5517
No. of rev onsets	101	89	93	83
Log pseudolikelihood	-494.27	-434.28	-451.27	-397.89
ePCP	96.99 %	97.09%	96.96%	97.08%
ePRE	0.88%	1.01%	0.98%	1.31%

Significance levels: +:10%   *:5%   **:1%, independent variables are lagged by one year; time, time squared, time cubed, and constant omitted

and revolutionary events occurring under authoritarian regimes are more likely to threaten neighboring regimes. This is consistent with the hypothesis that citizens in a closed country use revolutionary events and demonstrations in other countries as a way to estimate support for opposition activities in their own country. It appears, however, that not any revolutionary event anywhere on the world will do: the revolutionary event has to occur in a country with a similar regime and in geographical proximity, while shared religious believes, culture or official language don't seem to play an important role.

However, the regime types created by Geddes (1999a) are a relatively crude measure for how governments differ amongst each other, and hence further analysis is needed to establish which similarities in the government structures make the revolutionary threat spread. Svolik (2012)'s continuous classifications of different aspects of regimes could form the basis for a "similarity matrix" between different regimes.

The study is limited by the small sample size - the number of revolutionary events that have occurred so far is rather small for a quantitative analysis. And while I am reasonably confident that this new database now contains all successful revolutions, I am less certain about unsuccessful ones, as these depend on the elusive concept of "level of threat to the government". I will be able to at least test for inter-coder reliability once the material has been fully coded by a second person.

My initial interest was to explore how different governments react to revolutionary pressure, using neighboring revolutions as a reasonably exogenous shock. Table 5 gives a first preview of my future research. It shows how governments react with reforms to increased revolutionary pressure from abroad - the dependent variable is the difference between the polity score one year before and one year after the country year under observation. The positive coefficient indicates that most governments react with reforms (column 1). This effect is not just due to successful revolutions replacing authoritarian governments or governments reacting to revolutionary pressure in their own countries, as column 2 shows. There, I add a dummy for whether the country in question has experienced a revolutionary event during the previous, current or following year. Neighboring revolutionary events remain a signficant predictor.

The influence of neighboring revolutionary events is robust to confounders like the initial level of openness and economic developments - which all have a signficant effect on their own - (column 3) and exists independently of the level of democratization among neighbours (column 4). Recoding the dependent variable to a binary "Polity score did/did not increase" gives similar results.

However, all is not well: neighboring revolutions also appear to increase the chance of the country's human rights records being downgraded by Amnesty In-

Table 5: Reforms as reaction to increased revolutionary pressure: regression with standard errors (in parentheses) clustered on countries

Dep. var.: 2 year Δ polity score	C1	C2	C3	C4
share of neighbors with rev. events	1.727*	1.574*	1.235*	1.246*
	(0.689)	(0.666)	(0.619)	(0.618)
rev. event		0.903**	1.016**	1.014**
		(0.229)	(0.233)	(0.233)
polity index score			-0.075**	-0.085**
			(0.010)	(0.010)
log GDP per capita			0.291**	0.249**
			(0.049)	(0.048)
GDP growth			-0.010**	-0.010**
			(0.004)	(0.004)
share of democratic neighbors				0.672**
				(0.162)
constant	0.097**	0.047	-1.344**	-1.162**
	(0.028)	(0.031)	(0.278)	(0.277)
N	7251	7251	6848	6714
R^2	0.00	0.01	0.06	0.06

Significance levels: +:10% *:5% **:1%, independent variables lagged by one year, except rev. events and neighboring rev. events

ternational or the US State Department (Gibney et al., 2009). While the latter result is not that robust, it still indicates that governments' reactions are far from uniform. In my next study, I hope to explore these initial results, and examine what makes regimes react to such an "external shock" in one way or the other.

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A Sources and coding of variables

revolutionary events, no. of previous revolutionary events, times since last revolutionary event, time squared, time cubed: see section 3.

anti-government demonstrations: Banks (2008)

democracy dummy, un region codes: Cheibub et al. (2009)

single party rule, personalist rule, military rule, monarchy: Geddes (1999b), with update from Wright (2008). Missing values that Cheibub et al. (2009) code as military or royal dictatorship were coded as military rule or monarchy, respectively. Bahrain and Brunei were coded as monarchies throughout their existence.

polity index: Gurr et al. (2011). The index was recoded to 0 (least democratic) to 20 (most democratic) instead of -10 to 10.

infant mortality rate, youth bulge: replication data from Urdal (2006)

level of oppression: Gibney et al. (2009). Level of oppression was coded as the mean of the Amnesty International and State Department ratings when both were available, else the rating available was taken. Recoded to 0 (least oppressive) to 4 (most oppressive) instead of 1 to 5.

ln GDP per capita: Gleditsch (2008)

GDP growth: Heston et al. (2009), missing values filled in with calculations based on Gleditsch (2008)

log population: Gleditsch (2008)

neighbors: the R-Package "cshapes" was used to identify countries within 500km of a given country (http://nils.weidmann.ws/projects/cshapes/)

cultural zones: codings are based on Henderson (2005) interpretation of Huntington's cultural zones

official languages: country entries in Encyclopedia Britannica, different editions.

oil exports: Bueno de Mesquita and Smith (2010)

B Additional Tables and Figures

Table A: summary statistics for variables used

| Variables	N	mean	SD	min	max
revolutionary events	8760	0.028	0.165	0	1
onset of revolutionary event	8636	0.014	0.118	0	1
demonstrations	8732	0.188	0.390	0	1
demonstration onset	7961	0.109	0.311	0	1
neighboring revolutionary events	8324	0.030	0.083	0	1
neighboring rev. events in autocracies	8544	0.021	0.064	0	.5
neighboring rev. events in democracies	8544	0.008	0.044	0	1
democracy dummy	8489	0.429	0.495	0	1
log GDP per capita	7972	7.582	1.349	3.989	10.902
GDP growth	8380	1.862	6.9882	-65.076	134.130
log population	7903	8.545	2.029	2.079	14.074
number of previous rev. events	8637	0.374	0.748	0	5
time since last rev. event	8760	19.014	15.063	0	62
single party rule	7413	0.227	0.419	0	1
personalist rule	7369	0.151	0.358	0	1
military rule	7539	0.092	0.289	0	1
monarchy	7619	.093	0.290	0	1
polity index	7543	10.016	7.498	0	20
infant mortality rate	6803	73.448	54.446	2	263
youth bulge	6806	18.08	2.290	10.2	25.4
level of oppression	4833	1.413	1.119	0	4
oil exports	4001	2.691	9.411	0	91.569
percentage of democratic neighbors	6884	0.349	0.322	0	1
difference to neighbors' Polity score	6883	0.466	6.497	-15.85	18.5
rev. events worldwide	8693	4.250	2.427	0	12
rev. events in same region	8475	0.028	0.0668	0	1
rev. events in same regime type	7354	0.029	0.039	0	0.333
rev. events in same cultural zone	7413	0.027	0.047	0	1
rev. events same off. language	8749	0.019	0.047	0	1
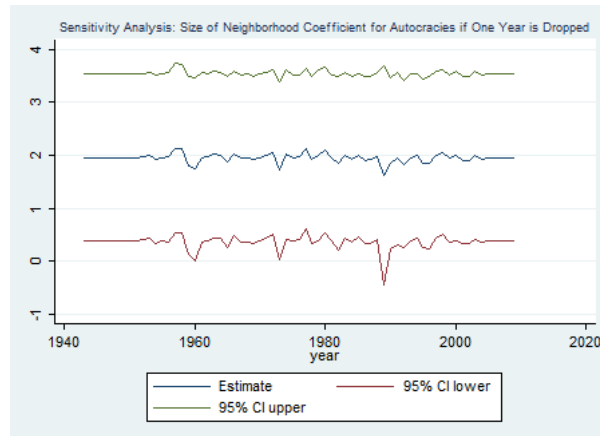
Figure 5: Robustness of results to dropping one year at a time, size of coefficient on neighboring revolutionary events, with 95% confidence interval
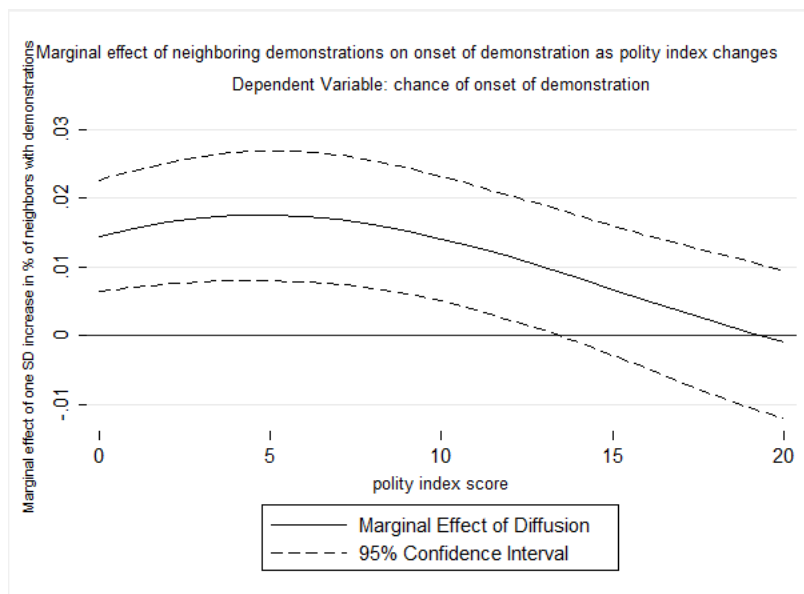

Figure 6: Effect of neighboring demonstrations for regimes ranging from 0 (highly authoritarian) to 20 (highly democratic)

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Table B: Explaning the onset of anti-government demonstrations: logistic regression with standard errors (in parentheses) clustered on countries

Dep. variable	anti-gov. demonstr.	anti-gov. demonstr.	anti-gov. demonstr.
neighboring anti-gov. demonstrations	0.710**		
	(0.251)		
neighboring anti-gov. demonstrations * democracy	-0.677*		
	(0.314)		
neighboring demonstrations in autocracies		0.814**	
		(0.304)	
neighboring demonstrations (aut) * democracy		-0.964*	
		(0.456)	
neighboring demonstrations in democracies			0.587
			(0.369)
neighboring demonstrations (dem) * democracy			-0.489
			(0.434)
democracy dummy	0.512**	0.512**	0.512**
	(0.136)	(0.136)	(0.136)
log GDP per capita	-0.019	-0.019	-0.019
	(0.038)	(0.038)	(0.038)
GDP growth	0.003	0.003	0.003
	(0.007)	(0.007)	(0.007)
log population	0.237**	0.237**	0.237**
	(0.032)	(0.032)	(0.032)
no. of previous rev. events	0.100**	0.101**	0.097**
	(0.021)	(0.022)	(0.021)
No. of country years	6766	6766	6766
No. of rev onsets	774	774	774
Log pseudolikelihood	-2254.96	-2256.02	-2257.46
ePCP	80.73%	80.72%	80.72%
ePRE	4.89%	4.86%	4.84%

Significance levels: $+$: 10% $*$: 5% $**$:1%, indep. variables lagged by one year; time, time squared, time cubed and constant omitted

C Sources used during verification process

1st step

- www.onwar.com

- www.globalsecurity.org

- www.refworld.org

- www.cia.gov/library/publications/the-world-factbook

- www.wikipedia.org

2nd step

- country descriptions in the political handbook of the world (Banks et al 2010)

- country background notes issued by the US Department of State

- yearly country reports issued by Amnesty International and Human Rights Watch

- the Economist Intelligence Unit's quarterly country reports

- Banks, A. S., T. C. Muller, & W. R. Overstreet (Eds.). 2010. Political handbook of the world 2008. Washington: CQ Press. Retrieved from Political Handbook of the World Online Edition, http://library.cqpress.com/phw/.

D Complete List of Revolutionary events

1. Albania, 02/1997-08/1997 (collapse of pyramid scheme brings down government, UN helps to restore order)

2. Algeria, 10/1988 (riots in Algiers spread to other cities), not mentioned in Banks

3. Algeria, 05/1991-03/1992 (FIS calls for demonstrations and strikes to change electoral law, military arrests supporters and drives even relatively moderate Islamists underground in 1992), deterioration into civil war/terrorist campaign

4. Argentina, 1959-05/1973 (Peronist opposition to military government)

5. Argentina, 06/1982-10/1983 (protests force military junta to hold fair elections in 1983)

6. Argentina, 05/1989-03/1990 (riots caused by inflation, state of emergency declared)

7. Bangladesh, 10/1990-12/1990 (surge of opposition sweeps president from power, caretaker regime installed), not mentioned in Banks

8. Bangladesh, 03/1994-03/1996 (continuous boycott of the parliament by opposition, student protest and strikes)

9. Bangladesh, 05/2003-12/2008 (opposition again boycotts parliament, military care-taker government takes over, opposition finally wins by landslide in December 2008)

10. Benin, 10/1963 (military coup is preceded by ethnic clashes and strikes and demonstrations against the president)

11. Bolivia, 07/1946 (mob storms President Villarroel's palace and kills him)

12. Bolivia, 05/1949-05/1952 ("National Revolution")

13. Bolivia, 1956-1960 (opposition to "Stabilization Plan")

14. Bolivia, 1973-1974 (suppression of general strike against devaluation of Bolivian peso)

15. Bolivia, 11/1979-10/1982 (widespread civil disorder forces resignation of military regime)

16. Bolivia, 09/2003-10/2003 ("Bolivian Gas War")

17. Brazil, 1954-1956 (strikes)

18. Bulgaria, 11/1989-06/1990 (collapse of communist regime)

19. Burkina Faso, 01/1996 (President resigns after continuous worker strikes, hands power to a government of senior army officers)

20. Burkina Faso, 09/1990-1991 (clashes between government and opposition forces after military commander announces presidential candidacy)

21. Burundi, 04/1972-09/1972 (Hutu uprising provokes reprisals by ruling Tutsi), deterioration into genocide

22. Cambodia, 03/1970 (demonstrations against overthrow of Prince Sihanouk), deterioration into civil war

23. Chad, 11/1965 (tax revolts in Gura prefecture and Malgam riots, founding of FROLINAT), deterioration into civil war

24. China, 11/1956-07/1957 ("Hundred Flowers Campaign" encourages criticism of government, later put down)

25. China, 03/1959 (nationalist uprising in Tibet, flight of Dalai Lama), deterioration into guerilla war

26. China, 04/1989-06/1989 (student protests on Tiananmen Square), not mentioned in Banks

27. Colombia, 04/1948 ("Bogotazo" - violent riots after assassination of presidential candidate Jorge Gaitan), deteriorates into decade of violence ("la Violencia")

28. Comoros, 1997-1998 (antigovernment strikes and violent demonstrations, secession movements in Anjouan and Moheli)

29. PR Congo, 07/1963 (three day uprising overthrows President Fulbert Youlou; "les Trois Glorieuses")

30. PR Congo, 10/1990-1992 (after general strike, military announces introduction of multiparty system, subsequent unrest results in renewed military interventions)

31. Costa Rica, 03/1948-04/1948 (National Republicans accept electoral defeat after widespread unrest)

32. Cuba late 1955 - 01/1959 (Cuban Revolution)

33. Czechoslovakia, 02/1948 (demonstrations force appointment of Communist government)

34. Czechoslovakia, 01/1968-08/1968 ("Prague Spring"), not mentioned in Banks

35. Czechoslovakia, 08/1988-12/1989 (dissident protest during 20th anniversary of Soviet Invasion during "Prague Spring", later general opposition to Communist government)

36. Dominica, 05/1979-06/1979 (president flees country after demonstrations)

37. East Timor 04/2006-06/2006 (demonstrations supporting dismissed soldiers precipitate gang violence and clashes, international forces restore peace)

38. Ecuador, 03/1966 (general strike against increasing taxes, joined by student groups and labor results in military reformers stepping down)

39. Ecuador, 01/2000 (debt-restructuring efforts generate waves of protests, president Mahuad declares state of emergency, military coup and countercoup)

40. Ecuador, 04/2005 (weeks of public protests, congress dismisses President on the grounds of having abandoned his post)

41. Egypt, 01/1952-07/1952 ("Cairo Fires", rioters loot districts associated with "Westernized" elite, put down with the help of British), not mentioned in Banks

42. Egypt, 06/1953-12/1954 (Muslim Brotherhood challenges regime in riots, clashes and civil tumult)

43. Egypt, 01/1977 (the "Bread Riots" bring hundred of thousand of protesters in most major cities onto the streets, army puts protest down, but government reinstitutes subsidies shortly thereafter), level of threat dubious, not mentioned in Banks

44. El Salvador, 03/1972 (young officer's coup after presumed electoral fraud, supported by demonstrations of some residents in the capital)

45. El Salvador, 1977-1980 (revolutionary organizations launch protest waves, number of strikes increase throughout 1977 and peak in 1979 and 1980), deterioration in guerilla/civil war

46. Georgia, 09/1991-01/1992 (violent protests and arrests of opposition in the wake of attempted coup in Moscow, anti-government demonstration turns violent in September, president flees country in January 1992)

47. Georgia, 11/2003 ("Rose Revolution" - protest against electoral fraud oust president)

48. Georgia, 11/2007 (mass protests, president declares state of emergency but accepts demands for snap presidential elections)

49. German Democratic Republic, 09/1989-03/1990 (Fall of Berlin Wall)

50. Grenada, 03/1979 ("JEWEL Revolution")

51. Guatemala, 03/1982 (protests against electoral fraud, bloodless coup aimed to "restore democracy" results in military regime)

52. Guinea, 01/2007-02/2007 (after massive strikes, president changes cabinet repeatedly, elections are first postponed, finally never take place)

53. Guyana, 01/1998-06/1998 (elections for successor to president who died in office causes intense controversy, with opposition boycotting parliament)

54. Haiti, 12/1956-09/1957 (general strike forces resignation of military government)

55. Haiti, 10/1985-02/1986 (riots and demonstrations spread from Gonaive to the whole country, president Duvalier departs to France)

56. Haiti, 09/2003-02/2003 (widespread demonstrations, followed by rebel uprising eventually storming the capital)

57. Hungary, 10/1956-11/1956 (Hungarian Revolution)

58. Hungary, 03/1989-04/1990 (Overthrow of Communist government)

59. India, 03/1987-ongoing (demonstrations in Jammu and Kashmir)

60. Indonesia 01/1994-05/1998 (political opposition to the PDI under Megawati Sukarnoputri)

61. Iran, 06/1952-07/1952 (Shah re-appoints Mosaddegh as prime minister after five days of mass demonstrations)

62. Iran, 06/1963 ("Uprising of June 1963", protests against arrest of Khomeini)

63. Iran, 10/1977-02/1979 (Iranian Revolution)

64. Iraq, 03/1991-04/1991 (Kurds and Shiites rise up after Baghdad has lost Gulf War)

65. Iraq, 07/2003-ongoing (resistance to US occupation)

66. Israel, 12/1987-1993 (1st Intifada)

67. Israel, 09/2000-02/2005 (2nd Intifada)

68. Ivory Coast, 03/1992-05/1992 (mass demonstrations prompt the president to launch democratization), level of threat dubious

69. Ivory Coast, 01/1995-10/1995 (opposition protest against unfavorable electoral code)

70. Ivory Coast, 10/2000 (coup attempt amid elections, riots and mutinies, initiator withdraws after widespread demonstrations)

71. Kyrgyzstan, 03/2005 ("Tulip Revolution")

72. Lebanon, 07/1958 (Muslim pro-Nasser demonstrations demand that Lebanon join the United Arab Republic, Christians oppose)

73. Lebanon, 04/1975 (Lebanon's civil war begins with a series of demonstrations by fishermen in Sidon), deterioration into civil war

74. Lebanon, 02/2005-06/2005 ("Cedar Revolution": after the assassination of Rafik Hariri, demonstrations force the retreat of Syrian troupes from Lebanon, and the disbanding of the pro-Syrian government), not mentioned in Banks

75. Lesotho, 07/1998-09/1998 (opposition protests against election results deteriorate into civil disorder, South Africa intervenes)

76. Madagascar, 04/1971-04/1972 (peasant uprising against tax burden in Tulear province, suppressed by the army), not mentioned in Banks

77. Madagascar, 04/1972-05/1972 (student protests are suppressed, but government is dissolved and president finally resigns)

78. Mali, 03/1980 (student-led anti-government demonstrations are put down)

79. Mali, 01/1991-08/1991 (again student-led anti-government demonstrations, this time supported by workers, a group of officers finally arrest the president)

80. Mexico, 1994-ongoing (Zapatista movement)

81. Myanmar, 05/1974-1977 (series of strikes and anti-government demonstrations)

82. Myanmar, 08/1988-09/1988 (student-led disturbances culminate in a popular outpouring of more than 100'000 demonstrators in Yangon, president resigns, but military again seizes power)

83. Myanmar, 09/1996 (weeklong demonstrations in Yangon and Mandalay, regime re-imposes house arrest on Aung San Suu Kyi, the opposition leader)

84. Myanmar, 08/2007-09/2007 ("Saffron Revolution": removal of fuel subsidies spark demonstrations, the junta suppresses the uprising after Buddhist monks join in), not mentioned in Banks

85. Nepal, 11/1950-02/1952 (revolution restores the power of King Tribhuvan)

86. Nepal, 02/1990-05/1991 ("People's Movement")

87. Nicaragua, 1973 ("Sandinista" movement is revived after widespread corruption during distribution of international relief for earthquake leads to strikes and demonstrations), deterioration into guerilla/civil war

88. Nicaragua, 01/1978-07/1979 (Chamorro's assassination provokes mass demonstration and nationwide strikes)

89. Nigeria, 06/1993-08/1993 (after elections military refuses to authorize return to civilian rule, which results in serious rioting in Lagos)

90. Pakistan, 03/1971-07/1972 (East Pakistani demonstrate for independence), deteriorates into civil war

91. Pakistan, 02/1973 (Balochistani independence movement), deteriorates into civil war

92. Pakistan, 01/1977-07/1977 (opposition refuses to accept election results, government agrees to fresh elections, but military stages coup)

93. Paraguay, 01/1947-08/1947 (struggle between Feberistas and Colorados extends togeneral population and results in Paraguayan Civil War)

94. Paraguay, 1959-1960 (resistance to Stroessner's regime)

95. Philippines, 08/1983-02/1986 (People Power Revolution)

96. Philippines, 11/2000-01/2001 (People Power II, president Estrada, accused of corruption, is deposed by Supreme Court)

97. Poland, 06/1956-12/1956 ("Bread and Freedom" riots, worker's strikes result in wage rises and other reforms)

98. Poland, 1980-07/1983 (Labour movement Solidarnosc opposes Communist government, strikes), not mentioned in Banks

99. Poland, 04/1988-09/1989 (Labour movement Solidarnosc opposes Communist government, strikes until government accepts Round Table discussions), not mentioned in Banks

100. Portugal, 03/1962 (student demonstrations in Lisbon in reaction to crackdown of regime on student associations)

101. Portugal, 04/1974-11/1975 ("Carnation revolution")

102. Romania, 12/1989-05/1990 (Uprising against communist rule ends with execution of Ceausescu and his wife)

103. Senegal, 12/1982-1983 (inhabitants of Casamana demonstrate for autonomy), deteriorates into (limited) civil war

104. South Africa, 04/1976-04/1977 (student protests against Afrikaans as medium of instruction, disturbances and racial violence spread to different townships)

105. South Africa, 07/1985-02/1990 (anti-apartheid struggle intensifies)

106. South Korea, 03/1960-04/1960 (student-led demonstrations against ballot tampering force the president's resignation), not mentioned in Banks

107. South Korea, 09/1974-10/1979 (student protests against president are joined by Christian organizations, president is assassinated in 1979)

108. South Korea, 04/1980-05/1980 (widespread series of labor strikes escalate into mass student demonstrations, uprising in southern city of Kwangju is suppressed)

109. Sri Lanka, 1953 (president resigns after massive "hartal" - general strike - by left parties)

110. Sri Lanka, 04/1971 (People's Liberation Front attempts to overthrow government, order is restored at the cost of an estimated 20'00 deaths)

111. Sudan, 08/1964-11/1964 (student demonstrations in Khartoum spread to the provinces and trigger strikes, military government ultimately resigns), not mentioned in Banks

112. Suriname 1982-1984 (right-wing military uprisings and other antigovernment conspiracy reported, austerity measures provoke strikes)

113. Tajikistan 03/1992-08/1992 (arrest of opposition politicians triggers widespread antiregime protests), deterioration into civil war

114. Thailand 06/1973-10/1973 (government falls because of widespread student demonstrations)

115. Thailand 01/1976 (strikes, gigantic rallies and student protests - Thammasat University massacre - but students are no longer supported by general population and some decide to join armed opposition), deterioration into civil war

116. Thailand 02/1992-05/1992 (opposition contests appointment of non-elected general for prime minister, general resigns and parliament adopts demands of opposition), not mentioned in Banks

117. Togo 10/1991-02/1994 (sentencing of opposition figures ignites series of protests and strikes, period of instability with coups and negotiations)

118. Trinidad and Tobago 04/1970-04/1974 ("Black Power" demonstrations and labor unrests result in imposition of state of emergency twice)

119. Ukraine 11/2004-01/2005 ("Orange Revolution"), not mentioned in Banks

120. United Kingdom 10/1968-1969 ("The Troubles" start with civil rights marches), deterioration into civil war/terrorism campaign

121. Uruguay 1968-1969 (strikes and student demonstrations in the wake of the Tupamaros' activities), not mentioned in Banks

122. Uzbekistan 05/2005 (demonstrations in Andijan in favor of indicted Islamic businessmen is crushed by the government)

123. Venezuela 01/1958 (public dissatisfaction leads to air force unit bombing Caracas, some weeks later, a general strike in Caracas is joined by the armed forces, president flees country)

124. Venezuela 02/2002-02/2003 (general strikes and demonstrations lead to a coup attempt, strikes continue until early 2003)

125. Yugoslavia 08/2000-10/2000 (opposition contests necessity of runoff election, constitutional court finally declares opposition candidate the winner)