

Economic Breakdown and Collective Action

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ABSTRACT

While social movement scholarship has emphasized the role of activists in socially constructing grievances, we contend that material adversity is a reoccurring precondition of anti-state mobilization. We test the effect of economic decline on the count of large-scale, anti-government demonstrations and riots. Using multiple sources of newspaper reports of contentious events across 145 countries during the period 1960-2006, we find a statistically significant negative relationship between economic growth and the number of contentious events, controlling for a variety of state-governance, demographic, and media characteristics. We find that the effect is strongest under conditions of extreme economic decline and in non-democracies. These findings highlight the need for social movement scholars to take seriously the role of economic performance as an important factor that enables mobilization.

KEYWORDS: social movements; grievances; collective action; mobilization; economy.

Thomas Friedman (2011) wrote in the *The New York Times* that, “We know the big causes [of Arab Spring]—tyranny, rising food prices, youth unemployment, and social media.” Similarly, after noting that the Arab Spring can be attributed to “the inability of governments in that region to provide jobs and better economic opportunities for their people,” *Time* magazine’s Michael Schuman (2011) claimed that China is less likely to experience similar events due to their relatively low unemployment rate. Politicians also make the link between economic conditions and collective action: days before the occupy Wall Street movement began, New York City’s Mayor Michael Bloomberg said on his radio show, “You have a lot of kids graduating college who can’t find jobs. That’s what happened in Cairo. That’s what happened in Madrid. You don’t want those kinds of riots here” (Taylor 2011).

While journalists and other elites embrace the relationship between adversity and collective action, social movement theorists have debated the role of material conditions as a factor that is likely to trigger anti-state political action (Buechler 2004; Useem 1998). Early scholars such as Neil Smelser (1968), Ted Gurr (1970), Ralph Turner and Lewis Killian (1972), and Francis Fox Piven and Richard Cloward (1977) saw adversity, expressed in grievances, as a precondition for mobilization. Paradigms since then, including resource mobilization (e.g., McCarthy and Zald 1973), political process (e.g., McAdam 1982; Tilly 1978), and new social movement theory (e.g., Kriesi et al. 1995;

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Melucci 1980) have focused on adversity as important only to the extent that social movement actors could construct or frame the problem as salient for mobilization.

An alternative approach holds that under specific conditions, material adversity is likely to positively influence mobilization. States that cannot provide economic growth may see demonstrations, riots, or strikes as individuals facing economic strain (e.g., McVeigh 2009) or an imbalance between their expected standard of living and current condition (e.g., Snow et al. 1998). Citizens in countries with declining economies may lose confidence in state leadership or possibly perceive the state as less legitimate (e.g., Coleman 1990; Walton and Ragin 1990). Economic decline, in combination with other factors, may increase the risk for a social revolution (e.g., Foran 2005).

Although scholars, journalists, and politicians attribute material adversity as a cause of mobilization, empirical tests of the relationship have been limited. Prior work in this area has primarily focused on a single movement (e.g., Jenkins, Jacobs, and Agnone 2003; Kawalerowicz and Biggs 2014; McVeigh 1999; Pfaff 2006; Richards et al. 2013; Simmons 2014), but economic grievances can express themselves politically in a variety of forms (Calhoun 1997). Some comparative cross-national research has been completed, but that research utilized cross-sectional studies limited to a subset of countries (e.g., Skocpol 1979; Walton and Ragin 1990), where findings may be impacted by unmeasured country-level factors. Additionally, empirical work on the relationship between economic adversity and mobilization has produced mixed or null findings (e.g., Andrew and Biggs 2006). Thus while we have some evidence that economic adversity is correlated with the rise of specific movements, we do not know whether states are likely to see anti-government demonstrations when nations go through periods of economic decline. Additionally, political opportunity theory suggests that some state characteristics, such as the level of democracy, are likely to influence whether political discontent is channeled into less confrontational forms of action, such as voting. This interaction may at least partially explain previous contradictory findings.

In this article, we test the relationship between economic adversity and collective action with a cross-national time-series analysis of the count of protest demonstrations and anti-state contentious events (i.e., protests, demonstrations, or riots targeting the government) reported in newspapers from three different sources of data for 145 countries between 1960 and 2006, for a total of 4,877 observations. Using negative binomial regression models that control for a variety of state-governance, demographic, and media effects along with unmeasured year and country effects, we find a statistically significant negative relationship between the number of demonstrations and other contentious events in a given year and the level of economic growth within a given country. We find that the effect is strongest under conditions of economic decline and in countries with low levels of democracy. These findings are robust across the entire time period that we examine, and to the inclusion of several additional measures for which we have only incomplete data, including the degree of income inequality and organizational density. Our work suggests that scholars of social movements need to take more seriously the importance of economic performance as a reoccurring precursor for collective mobilization, and that the inclusion of grievances would strengthen existing theoretical paradigms.

GRIEVANCES IN SOCIAL MOVEMENT THEORY

Scholars of collective behavior once viewed hardship as an instrumental precursor to protest and social movements (Buechler 2004; Useem 1998). In this view, alienation, psychological discomfort, or other negative emotional conditions drove people to challenge the established political order. Relative deprivation theory suggests that individuals who perceive a discrepancy between their expectations and experienced realities engage in collective action aimed at reducing or eliminating the expectation-reality gap (see Davies 1962). Relative deprivation theorists emphasized grievances as the psychological link between adversity and political action across a range of outcomes including civil strife (Gurr 1968, 1970), revolution (Davies 1962, 1969), and political violence (Feierabend, Feierabend, and Nesvold 1969).

Alternatively, breakdown theory focuses on the dissolution of social solidarity that occurs during periods of social change. “The significance of economic change,” according to Piven and Cloward (1977), “is . . . not simply that people find their expectations frustrated and so feel anger. It is also that when the structures of daily life weaken, the regulatory capacities of these structures, too, are weakened” (p. 11). In contrast, Spilerman (1970, 1971, 1976) found that relative deprivation or social breakdown had little impact on which cities experienced riots in the 1960s. Social solidarity rather than isolation among African American residents best explained which areas protested. Tilly and coauthors (Lodhi and Tilly 1973; Rule and Tilly 1972; Shorter and Tilly 1974; Snyder and Tilly 1972; Tilly, Tilly, and Tilly 1975) refined the solidarity model, and found that social cohesion provided individuals with a communication network, a set of common values and symbols around which to organize, a tradition of participation, and an authority structure (Useem 1980), all of which facilitated mobilization. Social breakdown brought about by economic decline as a mechanism for stimulating protest therefore seemed quite implausible.

Resource mobilization theory (RMT) and political process theory (PPT) both further deemphasized the relationship between material adversity and protest. For example, John McCarthy and Mayer Zald (1977) claimed that while grievances were a component of movements, the connection was weak at best. Instead of grievances existing in direct relation to movements, McCarthy and Zald (1977) contended, “grievances and discontent may be defined, created, and manipulated by issue entrepreneurs and organizations” (p. 1215). Many influential RMT scholars also rejected any link between grievances and protest (e.g., Gamson 1975; Jenkins and Perrow 1977; Oberschall 1973, 1978; Snyder and Tilly 1972; Tilly 1977, 1978). With the rise of PPT (e.g., McAdam 1982; McAdam, McCarthy, and Zald 1996) grievances were almost entirely removed from the agenda as a viable precondition for collective action, as the focus turned to the importance of political opportunities (e.g., the openness of the state to challengers), mobilizing structures, and framing processes (McAdam 1996). Subsequent challenges to the political process approach (e.g., Armstrong and Bernstein 2008; Goodwin and Jasper 1999) have moved social movement scholarship even further away from looking at objective conditions as a cause for mobilization.

OBJECTIVE HARDSHIP

In contrast to the dominant social movement theoretical paradigms, we argue that, “misery matters” (McClintock 1998) in explaining large-scale anti-state collective action because economic crises produce widely felt disruptive events that may threaten the legitimacy of the state. While resources and political opportunities help to explain conditions for collective mobilization, the absence of objective hardships has produced an incomplete theoretical framework for the study of social movements. Although dominant paradigms in social movement scholarship have turned away from considering economic hardship as a fundamental cause of collective mobilization, empirical and theoretical works outside of these paradigms have continued to provide evidence of the link (e.g., Foran 2005; McVeigh 2009; Snow and colleagues 1998; Snow, Soule, and Cress 2005).

There are several conditions under which economic hardship is likely to matter for mobilization across different constituencies and in different contexts. For citizens already in precarious economic conditions, declining economic conditions may make an already insecure existence untenable. As per David Snow and colleagues’ (1998) quotidian disruption thesis, events or potential events that interfere with normal routines of existence can be the basis of political mobilization. Economic hardship becomes a quotidian disruption when it “entails dramatic alterations in subsistence routines because of changes in the ratio of resources to claimants or demands” (Snow and Soule 2009:37). That is, movements are more likely to emerge either when there is a dramatic decline in economic conditions without a commensurate decline in expectations, or when a growth in demands outpaces available resources.

Measures of economic hardship, including unemployment, poverty, manufacturing decline, and unaffordable housing, are associated with increased activism among the homeless, even in models that controlled for all these factors simultaneously (Snow et al. 2005). Additional support for the role of objective hardship comes from Daniel Myers and Beth Caniglia (2004) who found that poverty and unemployment were both associated with the likelihood that a U.S. city experienced a riot in the late 1960s. Craig Jenkins and colleagues (2003) produced a more nuanced approach, finding that the relationship between objective hardship and collective mobilization had a threshold effect—increasing unemployment was associated with protest, but when economic hardship was very high, protest was more limited. Jenkins and colleagues (2003) argued that this threshold resulted from a paucity of resources or organizations that may be available in times of extreme hardship. In contrast, other research has found no relationship between mobilization and economic grievances (e.g., Andrews and Biggs 2006) or other types of grievances (e.g., Longhofer and Shofer 2010).

The relationship between economic hardship and mobilization extends beyond economically disadvantaged populations. A decline in the economic conditions of the middle class may also spur middle-class support for revolutionary movements in less developed and less democratic contexts. John Foran (2005) notes that in addition to broadly aggravating non-economic grievances, economic decline, “might play the role of final straw in radicalizing the middle class into action” (p. 23). Theda Skocpol (1979), too, argues economic pressure is one potential contributory factor to state crises, which may precede social revolutions. In his analysis of third world revolutions, Foran (2005) finds that economic decline, in combination with other factors, is a necessary cause for social revolutionary attempts. At the individual level, Karl-Dieter Opp (2000) also finds that resource deficits, measured by unemployment and low income, were correlated with a discontent with one’s personal situation, which was positively associated with political action.

Rory McVeigh’s (2009) power-devaluation thesis posited a broader link between adversity and mobilization for the middle class. He identified three “strains”—decreases in economic, political, and status-based power—that “alter individuals’ perceptions of their circumstances and provide opportunities to construct new interpretive frames that generate support for right-wing mobilization” (McVeigh 2009:43). It is through framing and cognition that actors perceive that they can make a difference by mobilizing around grievances generated by power devaluation. According to this approach, objective hardship makes social movement claims more credible and resonant, turning non-activists into activists. For example, Nella Van Dyke and Sarah Soule (2002) found that a decline in manufacturing jobs was positively associated with the organizational count of U.S. far-right organizations.

For all residents, economic decline may reduce the legitimacy of the government, especially when state policies are viewed as imposed from abroad. In discussing third world food riots, John Walton and David Seddon (1994) argue that “it is the crisis and the process of reconstruction of global capitalism that began in the late 1960s and early 1970s that has generated the economic policies of liberalization and austerity that have themselves given rise to the upsurge in popular unrest across the developing world . . .” (p. 22). Marc Bellemare (2014) also found that rising food prices produced increases in social unrest (e.g., food riots), but that the volatility of food prices was not associated with increasing riots. Decreasing access to subsistence goods in particular may increase the likelihood of protests as items such as rice or gasoline may have “meanings beyond their material value” (Simmons 2014:529-30). In addition, Walton and Charles Ragin (1990) located adversities in the form of economic hardship and social structural issues as components that led to protest. However, they further contended that these grievance mechanisms were operationalized through regime disintegration, which in turn provided the opportunity for protest activity. Protests and revolutions, therefore, are in part a political and social response to the enforcement of economic policies and the decline in economic conditions.

H1: Large-scale anti-state collective mobilization is negatively correlated with a nation’s economic growth.

Rather than a continuous relationship between growth and political action, there might instead exist a threshold effect (Jenkins et al. 2003). That is, the amount of positive economic growth may be uncorrelated with protest, but severe economic downturns might spark collective action. Snow and colleagues (1998) noted Charles Tilly's (1978) argument that "threats" were more likely to produce collective action than "opportunities." Snow, drawing on Daniel Kahneman and Amos Tversky's (1979) "prospect theory," argued that individuals are loss averse and are more willing to fight to regain something they lost than for new gains.

H2: States that experience severe economic decline will experience an increase in anti-state collective action, while average or positive growth will be uncorrelated with collective action.

The relationship between economic decline and protest is likely to be impacted by state structures. States that have a relatively open political opportunity structure may see economic discontent channeled into more routine and conventional forms of political participation, such as electoral politics. Further, "If a favorable governing coalition is in power, reform movements with a large organized membership can offer electoral support in exchange for entry into the polity" (Jenkins 1983:547). In democratic societies, polity access is a viable option for resolving grievances. For instance, Jacopo Ponticelli and Hans-Joachim Voth (2011) found that budget cuts led to protests, especially in autocracies. "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" (Ponticelli and Voth 2011:25-26). States with more limited opportunities for formal participation are more likely to see discontent expressed through more assertive means.

H3: The relationship between economic decline and anti-state collective action will be strongest in not fully democratic states.

In sum, we expect that a relationship exists between economic conditions and collective action, which may vary based on the degree of economic decline or specific state characteristics. While there is some limited empirical evidence to support this hypothesized relationship, no studies using more than one country across multiple years have ever been conducted. More generally, we seek to test the extent to which objective adversity measures—which have long been discounted in favor of other mobilizing factors—are significantly correlated with contentious political action.

METHODS

We test our hypotheses about the relationship between economic growth and political mobilization by analyzing the frequency of media-reported anti-government protest demonstrations, strikes, and riots. We include 145 countries during the period from 1960 to 2006, totaling 4,877 observations, as not all countries existed in all years and not all data was available for all countries in all years. Our dependent variables are from three data sets of newspapers' event counts, and measures of population demographics and state characteristics are from a variety of publicly available data sets.

The unit of analysis is the nation state-year. We include all nations with a population greater than one million. We include all years for which there are available data on the independent variables, but, as noted below, none of the three outcome-measure sources individually covers the entire span.

Outcome

Our outcome of interest is the number of large-scale, contentious political events in each country in a given year. To control for variation in national political repertoires, we combine data on different types of political action (e.g., anti-government demonstrations, general strikes, and riots) that target

the state. Summary information is provided in Table 1. For both demonstrations and riots, we use three sets of event counts, each based on different newspaper sources.

While using newspaper data to describe social movement activities is not without biases, it is the standard source for measuring movement activities (Earl et al. 2004; Ortiz et al. 2005). We employ multiple sources of data to ensure that our findings are not limited by the reporting habits of a single set of newspapers (Jackman and Boyd 1979). Additionally, our data sets include regional newspapers from several countries, which further expands the potential range of coverage and limits reliance on large U.S.-based newspapers. The events captured in the three data sets are biased towards events that are larger and more newsworthy; they likely do not include the majority of demonstrations that happen in a given year in a given country. This is likely to be especially true for democratic countries in recent decades, as protest has increasingly become a part of the institutionalized political repertoire and therefore less newsworthy (Meyer and Tarrow 1998). To the extent that economic adversity increases these small and routine protests, restricting our analysis to these types of events decreases the likelihood that we will find a statistically significant relationship between adversity and protest. Our focus, both empirically and theoretically, is on large-scale anti-state protests that are likely to present substantial challenges to the governing regime.

The first set of newspaper event data are from Arthur S. Banks's "Cross-National Time-Series" data archive (Banks and Wilson 2001). We analyze the years 1960 through 2006, which includes a total of 4,290 observations across 129 states. Events are coded from *The New York Times*. A demonstration is defined as "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" (Banks and Wilson 2001). General strikes are defined as, "Any

Table 1. Descriptive Statistics

Variable	N	Mean	P10	P50	P90	SD	SDB	SDW
Banks demonstration	4,860	.73	0	0	2	2.17	1.91	.96
Taylor demonstration	2,310	4.89	0	0	8	21.96	15.11	15.25
King demonstration	1,940	16.41	0	5	44	34.24	12.97	30.61
Banks riot	4,861	.46	0	0	1	1.91	1.72	.77
Taylor riot	2,310	2.39	0	0	6	9.02	7.53	4.78
King riot	1,940	1.28	0	0	4	3.32	2.53	2.08
Economic growth	5,269	.02	-.04	.02	.07	.05	.05	.02
Military personnel	5,269	.69	.10	.44	1.59	.76	.35	.66
Polity democracy score	5,269	.41	-9	-1	10	7.60	4.29	6.35
POS opening	5,095	.08	0	0	0	.28	.27	.08
POS closing	5,095	.03	0	0	0	.17	.17	.04
GDPpc (ln)	5,269	8.04	6.65	8.02	9.58	1.08	.29	1.03
Population (ln)	5,269	16.06	14.32	15.96	17.91	1.46	.27	1.44
Youth (%)	5,269	.32	.22	.34	.39	.06	.03	.06
NYT coverage	5,269	5.35	3.40	5.42	7.18	1.53	.53	1.44
Urban (%)	5,269	48.31	16.10	47.7	82	24.02	6.97	22.73
Organizations	1,985	296.56	8	65	495	736.12	181.81	467.45
Discrimination	3,474	.20	0	.12	.5	.22	.08	.20
Gini coefficient	3,273	38.50	25.45	37.24	52.45	10.37	3.44	9.73
Government cons.	4,573	15.20	8.64	14	23.20	6.21	3.71	5.34

Notes: (N) includes non-missing observations; (mean) sample mean; (P10) value at the 10th percentile, (P50) value at the median; (P90) value at the 90th percentile; (SD) the overall standard deviation; (SDB) the standard deviation between countries; (SDW) and the standard deviation within countries.

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” (Banks and Wilson 2001). Riots are “any violent demonstration or clash of more than 100 citizens involving the use of physical force” (Banks and Wilson 2001). This excludes assassinations, guerrilla warfare, and purges.

In Banks’s data, the median number of demonstrations for a country in a given year is 0, the mean is .7, and the 90th percentile is 2. That is, for the average country in an average year, no anti-state protests are reported, and in less than 10 percent of the cases are more than two anti-state protests reported. Reports of riots are rarer, with a median of 0, a mean of .5, and a value at the 90th percentile of 1. For both outcomes, the amount of variation within nations is roughly twice as large as the amount of variation between nations, based on the ratio of the within- and between-nation standard deviations.

The second set of newspaper event-count data are from the “World Handbook of Political and Social Indicators III, 1948-1982” (Taylor and Jodice 1986). We analyze data for the years 1960 to 1982, which include a total of 1,907 observations in 108 countries. Event-count data are from the *The New York Times* for all countries, combined with coverage from one regional news source for each country. Regional papers include *L’Annee Politique*, *Africa Diary*, *Middle East Journal*, *Asian Recorder*, *Associated Press*, *Archiv der Gegenwart*, *Keesings*, and *Current Digest*. Newspaper events are coded as demonstrations if the event is a “non-violent gathering of people organized for the announced purpose of protesting a regime, a government, or one of its leaders; its ideology, policy, or intended policy; or its previous action or intended action” (Taylor and Jodice 1986). Strikes are coded as present when there is “a work stoppage by a body of industrial or service workers or a stoppage of normal academic life by students to protest a regime or its leaders’ policies or action” (Taylor and Jodice 1986), which excludes strikes for exclusively economic motives. An event is coded as a riot if it “is a violent demonstration or disturbance involving a large number of people. Riots are distinguished from protest demonstrations by the presence of violence. Violence implies the use of physical force and is usually evidenced by the destruction of property, the wounding or killing of people, the use of riot control equipment . . . and by the rioters’ use of various weapons” (Taylor and Jodice 1986). These categories exclude regime supported demonstrations, armed attacks, and political assassination. Consistent with its inclusion of more sources than the Banks data, the Taylor data report seven times as many demonstrations, and six times as many riots.

The third set of newspaper event-count data are from Gary King and Will Lowe’s (2003) “10 Million International Dyadic Events.” Unlike the other two data sets, which are based on human coding of newspaper archives, the King data are based on computer coding of Reuters news feeds. While this process may involve some loss of accuracy in that actors and events are coded from the articles without human filtering (beyond the construction of the content analysis program), it has the advantage of being based on news briefs filed by all local Reuters bureaus worldwide. This data contains a much more comprehensive list of political events than those that are printed in *The New York Times* and other newspapers. We analyze data from the years 1990, the first year of the data set, until 2004, totaling 1,791 observations across 139 states.

As one would expect, because it is based on local news feeds rather than reports in major papers, the King data include the most events, with a mean of 16.4 demonstration events per country, the median number of demonstrations is 5, and the 90th percentile is 44. Reports of riots are rarer, with a median of 0, a mean of 1.3, and a value at the 90th percentile of 4. The Taylor riot count is higher than the King riot count, averaging 2.4 events per country year. The Taylor demonstration count, however, is lower than the King data, with a mean of 5, median of 0, and value at the 90th percentile of 8. Consistent with prior research (Myers and Caniglia 2004), this suggests that riots are more newsworthy, and when they are noticed at all are likely to make international news sources in addition to local ones.

Our outcome measures are moderately correlated with each other. The Banks demonstration data has a correlation coefficient of .47 with the Taylor data and .29 with the King data. The riot data

show higher levels of cross-data set correlations. Banks riot data has a correlation coefficient of .70 with the Taylor data and .55 with the King data. The Taylor data and King data do not overlap for any time periods, so we cannot estimate correlations between the two. The degree of correlation suggests that the three different sources of data are capturing the underlying phenomena, but with unique biases. Modeling the relationships across all three data sets provide a more robust test of the hypothesis than relying on a single data source.

Explanatory Measures

Our models include a measure of economic adversity and sets of explanatory measures linked to state governance characteristics, national demographics, and media practices. Each measure varies within nations and across years and is lagged by one year to control for reverse causation, except for grievances that are theorized to have an immediate impact. [Table 1](#) presents the count of non-missing observations, the mean, values at the 10th, 50th, and 90th percentiles, the overall standard deviation, and the standard deviation between and within countries.

Our key independent measure of economic adversity is the annual percent change in per capita gross domestic product (GDP), adjusted for purchasing power parity. Data are from Gapminder ([Lindgren 2008](#)), which is primarily based on the World Bank's International Comparison Programs, with additional data from the United Nations (UN), Angus Maddison, and the CIA *World Fact Book* ([Lindgren 2008](#)).¹ As noted in [Table 1](#), mean economic growth is 1.85 percent per year and median growth is 2.2 percent, with 80 percent of countries experiencing an annual change in economic performance between -4 percent and 7 percent.

We include a set of measures of state characteristics that are related to the level of democracy, whether the democratic context is opening or closing, the economic capacity of the state, and military capacity. Social movement scholars, political sociologists, or political scientists have previously highlighted each as important characteristics of the state that are likely to influence political engagement.

We include a measure of democracy from "Polity IV Project's Political Regime Characteristics and Transitions, 1800-2009" ([Marshall and Jaggers 2009](#)). This measure ranges from -10 for complete autocracies to 10 for full democracies. We also use this score to construct a variable that measures whether the political context is opening or closing to new challengers, which is a key component of state's political opportunity structure (POS).² Opening political opportunities, in this case, are measured by observations where the democracy score is higher than the prior year, and are coded dichotomously. Observations for which the democracy score is lower than the prior year are coded as having a closing POS.

We also include a measure of state capacity based on the proportion of a country's GDP that is government consumption expenditures, a measure of the size of the government as a fraction of the overall economy. Drawing from POS models, we hold that states that have a minimal capacity to implement policy are less likely to be the target of collective action, because the rewards for winning are much lower than in states that can distribute rewards effectively. Further, we contend that government consumption is a fairly good measure of the state's centrality in national affairs. Data are from the World Bank, as distributed by [Barbara Wejnert \(2007\)](#).

1 An alternative way to measure hardship is unemployment. Unemployment is a particularly difficult concept to measure cross-nationally because of the different means of labor markets and labor market participation across different political and economic contexts. Additionally, it requires a decent amount of state infrastructure to accurately estimate, and unemployment data is available for less than a quarter of our cases. This was concentrated in more recent times in more advanced economies (e.g., $n = 43$ in 1984), as historical unemployment data is almost non-existent in poor and weak states. While change in per capita GDP may not be a perfect proxy of economic growth, it more accurately captures economic conditions in agricultural and non-capitalist economies.

2 Other aspects of POS are partially captured by other measures in our models, including state spending as a percent of GDP (as a measure of state output capacity) and military personnel as a measure of repressive capacity.

To control for the repressive capacity of the state, we include a measure of the military strength. We use the proportion of the adult population in the military, using data from the Correlates of War National Material Capabilities data set (v 3.02) as described in [David Singer \(1987\)](#).

National Demographics

We include several demographic measures that are likely to be correlated with the overall number of collective action events, including population size, economic development, and urbanicity. Larger countries are more likely to have more and larger demonstrations, as a protest of 20,000 people would constitute 1 in every 100 residents in a nation of two million, but only 1 of every 15,000 residents in a nation of 300 million. Population size data are from the UN Population Division, as distributed by Gapminder ([Lindgren 2008](#)). We include the log of population size in all models. We also include a measure of the size of the youth bulge. Previous research suggests that non-democratic countries facing demographic crises, especially those countries with large populations of young people ([Goldstone 1991](#)), are likely to see higher levels of political violence ([Urdal 2006](#)). Our youth bulge measure is constructed by estimating the proportion of the working age population, those between 15 and 64 who are under 24 ([UN Population Division 2011](#)). Our measure of economic development is the log of the per capita GDP adjusted for purchasing power parity. While our economic grievance measure is the annual change in GDP, our demographic control is the absolute level, and data are from the same source ([Lindgren 2008](#)). The final demographic measure is the proportion of the population living in urban areas. Isolated rural residents are unlikely to gather in sufficient numbers to form a newsworthy protest event, because of the travel distance involved and the lack of information about events. This measure of urbanicity is from the United Nations, as distributed by Gapminder ([Lindgren 2008](#)), and relies on each nation's own definition of what constitutes an urban area. This has the drawback of lacking uniformity across places and time, but has the advantage of being sensitive to local contexts.

The number of reported demonstrations in a given country is also likely a function of the amount of the overall media coverage that a country receives. Protests occurring in a country that is out of the international spotlight and where few or no reporters are located are unlikely to receive the same amount of coverage as similar events in more media saturated environments. Our measures of state demographic and economic development likely capture some of this, as does our modeling strategy of controlling for unmeasured year and country effects. As an additional control we include a count of the number of times the country's name was mentioned in *The New York Times* in a given year. The authors collected this data.

ANALYTIC STRATEGY

We employ negative binomial regression models because our dependent variables are counts. As the outcomes are likely to be a function of both our independent variables and unmeasured characteristics related to both the observation's country and year, we adopt a strategy that focuses on the correlation between the outcomes and the within-country and within-year variation in the independent measures. While techniques for estimating fixed-effects models for linear outcomes are well developed in current statistical packages (e.g., *xtreg*, *y*, *x*, *fe* in Stata), no such standard estimators are available for negative binomial models ([Allison 2009](#)).³ We therefore employ an alternate strategy of demeaning or centering each variable by subtracting the year- and country-specific means of each independent measure before entering them in regression models ([Baltagi 2008:35-36](#)). In addition to the centered variables, we also include the year and country specific means in our models and estimate them using a method that allows the dispersion parameter to vary between issues, to produce

3 For example, Stata's help manual for *xtnbreg* notes that, "Here 'random effects' and 'fixed effects' apply to the distribution of the dispersion parameter, not to the *xb* term in the model."

what Paul Allison (2009) calls “hybrid” models.⁴ As Allison demonstrates, these models produce estimates that are almost identical to the fixed-effects estimation strategies of including issue and year indicator variables, but have the advantage of allowing issue or year invariant variables to be included in the models. This strategy also avoids difficulties in model convergence if we were to estimate the year and issue fixed effects by including them each as dummy variables.

FINDINGS

We test the effects of economic growth on the count of demonstrations and riots under a variety of conditions. In each of the tables, columns 1 through 3 model demonstrations and columns 4 through 6 model riots. In addition, for each table, columns 1 and 4 are based on the Banks data; 2 and 5 are based on the Taylor data; and 3 and 6 are based on the King data.

Table 2 shows models of the count of anti-state demonstrations and riots reported in news accounts according to our three sources as functions of economic growth, state characteristics, population demographics, and media practices. Across all six models, economic growth is negatively correlated with the measure for contentious events ($p < .05$). While the effect is statistically significant across many models, there is some variation in the magnitude. Comparing a period of high negative growth (a 5 percent contraction in the per capita GDP) with a period of high economic growth (a 5 percent expansion in the per capita GDP) our models predict that demonstration would happen 57 percent more often according to the Banks data, 33 percent more often in the Taylor data, and 12 percent more often in the King data. Riots would happen 80 percent more often based on the Banks data model, 59 percent more often based on the Taylor data, and 21 percent more often in the King data.⁵ This is consistent with our first hypothesis that large, anti-state collective mobilization is negatively correlated with economic growth.

Unlike economic growth, whose effect is statistically significant in all of the models, none of the control variables were statistically significant across all the data sets for both measures. Among the state characteristics, an opening of the POS is positively and statistically significantly associated with all three riot models, and both the Banks and Taylor demonstration models; in the King demonstration models, the effect is positive, but not statistically significant. In contrast, POS closing is not statistically significant in any of the models. Level of democracy is statistically significant in the Taylor and King riot models, although the effect is in a different direction; level of democracy had a positive impact in the Taylor demonstration data, but a negative effect in the King demonstration data, although neither was statistically significant. Alternative specifications, such as including a squared term, including separate measures of democratic and autocratic characteristics, or including individual components of the scale, did not produce any substantially different results. Neither percentage of military personnel in the population nor government spending produced statistically significant findings, except in the Taylor demonstration data where government spending is positive and statistically significant.

None of the demographic measures were consistently statistically significant across the models. Population size is positive and statistically significant for both the Banks models, but not for the Taylor or King models. Per capita GDP is positively correlated with and statistically significant in both outcomes in the King data, negatively correlated with and statistically significant in the Banks demonstration data, and uncorrelated with either of the Taylor measures or the Banks riot data. Proportion urban is statistically significant only in the Banks riot data, where it has a positive effect. The youth bulge measure has a positive and statistically significant effect in the Taylor riot data. Our additional control for media coverage has a positive effect in three of the models: The Taylor and

4 Our final model in pseudo Stata code is as follows: $xtnbreg y dm_x c_x t_x, re$ —where t_x is the year-mean of x , c_x is the country mean of x , and dm_x is the x minus the year and country means. We do not report the coefficients for these mean measures (see Allison 2009:67), though they are available upon request.

5 Predicted values are computed using the Stata margins command.

Table 2. Baseline Negative Binomial Models for the Reported Number of Contentious Events for Each Year in Each Country

	<i>Banks Demonstration</i> (1)	<i>Taylor Demonstration</i> (2)	<i>King Demonstration</i> (3)	<i>Banks Riot</i> (4)	<i>Taylor Riot</i> (5)	<i>King Riot</i> (6)
Adversity						
Economic growth	−4.404** (−6.70)	−3.074** (−3.95)	−1.151* (−2.56)	−5.808** (−7.15)	−4.792** (−5.47)	−1.948* (−2.08)
State						
Democracy score (polity)	−.012 (−1.66)	.014 (1.52)	−.013 (−1.60)	−.005 (−.54)	.024* (2.15)	−.035* (−2.26)
POS opening	.393** (4.24)	.469** (4.21)	.106 (1.72)	.363** (3.05)	.389** (2.91)	.281* (2.19)
POS closing	.089 (.59)	−.071 (−.43)	.048 (.45)	−.325 (−1.52)	−.014 (−.07)	−.024 (−.10)
Military personnel	.173 (1.49)	.053 (.40)	.106 (.94)	.182 (1.26)	−.030 (−.18)	.268 (.97)
Government spending	.015 (1.53)	.033* (2.49)	.011 (1.38)	−.015 (−1.21)	.012 (.74)	.029 (1.63)
Demographics						
Population (ln)	2.143** (7.10)	−.228 (−.43)	.534 (1.28)	1.517** (4.07)	.201 (.28)	.278 (.29)
GDPpc (ln)	−.453** (−2.81)	.269 (.92)	.477* (2.38)	−.313 (−1.66)	.479 (1.35)	1.218** (2.67)
Urban (%)	.016 (1.92)	−.015 (−.93)	.006 (.41)	.022* (2.32)	−.027 (−1.33)	.030 (1.03)
Youth (%)	1.562 (.92)	−1.038 (−.43)	−1.787 (−1.15)	3.849 (1.77)	6.262* (2.00)	.232 (.06)
Media						
NYT coverage	.045 (.63)	.270** (3.36)	.170** (2.78)	−.013 (−.15)	.195* (2.28)	.063 (.45)
Constant	−5.852** (−2.99)	−9.081** (−5.32)	−10.520** (−9.35)	−1.760 (−.77)	−8.654** (−4.54)	−13.290** (−6.79)
Observations	4,269	1,907	1,765	4,269	1,907	1,765
Countries	127	108	137	127	108	137
BIC	8,316.8	6,968.6	10,303.1	6,214.9	5,756.6	4,263.3

Notes: All models include controls measures for country and year. All variables, except adversity measures, are lagged by one year; *t* statistics in parentheses.

**p* < .05 ** *p* < .01 (two-tailed tests)

King demonstration models as well as the Taylor riot model. The media measure has no statistically significant effect in the other models.

One potential reason for the mixed findings among the demographic, state, and media measures is that the data sets cover different time periods. While the Banks data set temporally overlaps with the other two, the Taylor and King data cover different periods. A second potential reason for these mixed findings is that many of the variables changed slowly within countries. While there is a great deal of variation across the world in rates of urbanization, there is much less variation within a given country over 11 years, the length of the King data set. As noted above, in order to control for

Table 3. Negative Binomial Models for the Reported Number of Contentious Events for Each Year in Each Country Split by GDPpc Growth into Positive and Negative Growth Categories

	<i>Banks Demonstration (1)</i>	<i>Taylor Demonstration (2)</i>	<i>King Demonstration (3)</i>	<i>Banks Riot (4)</i>	<i>Taylor Riot (5)</i>	<i>King Riot (6)</i>
Negative GDP growth	-5.053* (-5.85)	-2.899* (-2.36)	-1.725* (-2.62)	-5.919* (-5.44)	-5.260* (-4.28)	-.771 (-.56)
Positive GDP growth	-2.785 (-1.76)	-3.353* (-1.97)	-.134 (-.14)	-5.521* (-2.70)	-3.788 (-1.83)	-4.685 (-1.94)
Observations	4,269	1,907	1,765	4,269	1,907	1,765
Spline dif	.264	.853	.245	.878	.594	.214
Countries	127	108	137	127	108	137
BIC	8,323.9	6,976.1	10,309.3	6,223.2	5,763.8	4,269.2

Notes: Control variables, identical to those in Table 2, are included in the models, but not shown; *t* statistics in parentheses.
**p* < .05 ** *p* < .01 (two-tailed tests)

unmeasured country and year effects that may lead to spurious findings, our modeling strategy looks only at within-country variation. While these models are conducive to estimating parameters when the key variables have a great deal of within country variation, like rate of economic growth, they may lack the statistical power to capture the effect of factors that primarily vary between countries. As such, non-significant findings regarding slow-moving variables should be interpreted cautiously.

The models in Table 3 test the extent to which the relationship between collective action and protest is linear, or whether there is a high degree of demonstrating when the economy is poor, but a relatively flat relationship after that point. We include the same set of control variables as the baseline model in all models, however, we only present the most relevant parameters for space reasons.⁶ To test for differential impact, we use a linear spline with a knot at 0 percent GDP growth.⁷ Across all of the six models—for all but King riot data—negative economic growth has a statistically significant correlation with demonstration and riot counts. Positive growth has a negative coefficient in all models but is only statistically significant in two of the models, Taylor demonstration and Banks riot. For each of the models, we additionally test whether there is a statistically significant difference between the negative and positive growth measures. For each of the models there is no statistically significant difference between the two coefficients. In order to evaluate the robustness of this effect, we modeled it with alternate cut points, such as above and below -2 percent or above and below 2 percent (not presented). In none of these cases was there a statistically significant difference between the coefficients measuring growth above and below the cut point. This suggests that shifts in GDP growth have a relatively constant effect on protest, whether that one-unit shift is between medium and low growth or between negative and very negative growth. We consider this modest evidence against the second hypothesis. While we find that negative growth is correlated with protest, we cannot reject the possibility that the impact of positive growth is statistically different from that of negative growth.

Table 4 explores the extent to which democracies are better able to contain adversity-related grievances. We test this by interacting each country's democracy score with the measure for economic adversity. Consistent with our third hypothesis, we find some evidence that economic conditions are

⁶ Complete tables available from the first author.

⁷ Creating a linear spline relaxes the assumption that the effect of economic growth on collective action is linear. In this case, the economic growth variable is split into two separate variables: one for when growth is negative and another for when growth is positive. Separate coefficients are estimated for each of the two variables.

Table 4. Negative Binomial Models for the Reported Number of Contentious Events for Each Year in Each Country

	<i>Banks Demonstration (1)</i>	<i>Taylor Demonstration (2)</i>	<i>King Demonstration (3)</i>	<i>Banks Riot (4)</i>	<i>Taylor Riot (5)</i>	<i>King Riot (6)</i>
Economic growth	−4.138* (−6.35)	−3.178* (−4.08)	−1.232* (−2.75)	−5.353* (−6.71)	−4.909* (−5.60)	−1.621 (−1.74)
Economic growth x democracy	.428* (3.12)	−.162 (−.84)	.529* (3.11)	.430* (2.51)	.236 (1.03)	1.203* (3.98)
Democracy	−.012 (−1.62)	.015 (1.60)	−.011 (−1.34)	−.003 (−.29)	.026* (2.30)	−.031* (−2.00)
Observations	4,269	1,907	1,765	4,269	1,907	1,765
Countries	127	108	137	127	108	137
BIC	8,119.5	6,913.1	1,0291.2	5,992.3	5,709.6	4,245.5

Notes: Models include interactions between economic growth and level of democracy. All models include indicator measures for country and year. All variables, except adversity measures, are lagged by one year. Controls variables, identical to those in Table 2, are included in the models, but not shown; *t* statistics in parentheses.

* $p < .05$ ** $p < .01$ (two-tailed tests)

more relevant for producing demonstrations in non-democracies than in democracies. In four of the six models there are statistically significant and positive coefficients on the variable measuring the interaction between economic growth and democratization. That is, non-democracies are more likely to see an increase in demonstrations when economic conditions decline. For example, the Banks model would predict that a shift from high economic growth to high economic decline doubles the expected count of demonstrations in non-democracies, while in democracies, the expected count only increases by 17 percent.

Supplemental Models

In addition to our core concerns, there is an additional set of theoretically relevant variables that are not available for the full set of cases over the full time period. These include measures of organizational capacity, changes in government spending, ethnic discrimination, and economic inequality. Below, we test the effect of each of these measures on collective action and whether or not the measures moderate the impact of economic decline.

Scholars of social movements have long highlighted the role of organizations in fostering collective action by mobilizing resources (e.g., McCarthy and Zald 1977). While cross-national organizational data are not available for several of the years and countries included in our study, we utilize available data compiled by Beth Gharrity Gardner and Evan Schofer (2012) on a count of associations in 120 countries from 1970-2006. The data were compiled from the Encyclopedia of Associations online and utilize founding dates to establish an estimate of the number of organizations in each country during this time period. As seen in Table 5, the relationship between economic growth and collective action remains negative and statistically significant despite the inclusion of the organizational measure in all models except the King riot data. The organizational measure is statistically significant and positively correlated with the occurrence of riots in the Banks data but statistically significant and negatively correlated with demonstrations in the Taylor data. These mixed findings suggest that organizational capacity may influence aggregate protest levels, although the relationship is not straightforward.

Table 5. Negative Binomial Models for the Reported Number of Contentious Events for Each Year in Each Country including a Robustness Check for Organizational Presence

	<i>Banks Demonstration (1)</i>	<i>Taylor Demonstration (2)</i>	<i>King Demonstration (3)</i>	<i>Banks Riot (4)</i>	<i>Taylor Riot (5)</i>	<i>King Riot (6)</i>
Economic growth	−4.023* (−5.64)	−4.030* (−3.62)	−.924* (−1.98)	−6.040* (−6.54)	−5.968* (−4.78)	−1.936 (−1.94)
Organizations	.015 (1.12)	−.086* (−2.49)	−.013 (−.83)	.081* (3.54)	.008 (.14)	−.010 (−.24)
Observations	3,246	863	1,667	3,246	863	1,667
Countries	123	92	132	123	92	132
BIC	6,590.5	3,547.0	9,967.3	4,305.3	2,614.3	4,136.2

Notes: All models include control measures for country and year. All variables, except adversity measures, are lagged by one year. Controls variables, identical to those in Table 2, are included in the models, but not shown; *t* statistics in parentheses.

**p* < .05 ** *p* < .01 (two-tailed tests)

One potential mechanism by which GDP growth may impact collective action is through cuts in government spending, such as welfare programs (e.g., Ponticelli and Voth 2011). To test this relationship, we include an additional measure of change in government spending. Data are also from Gapminder (Lindgren 2008) and utilize the government spending measure we include in all models. As shown in Table 6, for the set of countries where this data are available, change in government spending does not have a statistically significant relationship with demonstration for any of the outcome measures. Even while controlling for government spending and the change in government spending, the relationship between economic growth and demonstrations is negative and statistically significant in all six models. This provides little evidence that declines in state spending can offset protests associated with economic decline.

We also test whether our findings are robust to other measures of adversities. Prior research has highlighted the potential impact of economic inequalities (e.g., Gurr and Moore 1997; Jenkins et al. 2003; Sigelman and Simpson 1977) and racial grievances (e.g., Gurr 1993b, 2000; Oliver 1989). For instance, economic inequality and class exploitation were found to correlate with the number of deaths in violent rebellions, controlling for other factors (Boswell and Dixon 1993). Along similar lines, Lee Sigelman and Miles Simpson's (1977) cross-national analysis of 49 nations found that income inequality was linked to political violence during a different time period. It has also been noted that racial grievances lead to collective action; "when ethnic identity is highly salient, it is likely to be the basis for mobilization and political action" (Gurr 2000:6). Similarly, Jutta Kawalerowicz and Michael Biggs (2014) found that neighborhoods with high ethnic fractionalization were more likely to produce rioters in the 2011 London riots.

As with the organizational data, data are available for only some of our countries for some of the years in our study. Our level of economic inequality is measured by the Gini coefficient. Our measure is based on data collected in the UN-WIDER World Income Inequality Database (UNU-WIDER 2008) as enhanced by Frederic Solt (2009). The racial discrimination measure is from the Minorities at Risk data set (Gurr 1993b). The measure used is the proportion of the population experiencing political discrimination, either through neglect, formal or informal processes of exclusion, or the absence of remedial policies.

Table 7 shows our baseline model with an additional control for each country's income inequality, as measured by the Gini coefficient. Income inequality is not significantly correlated with collective action in any of the models. Economic growth remains significantly negatively correlated with

Table 6. Negative Binomial Models for the Reported Number of Contentious Events for Each Year in Each Country included in the Banks Data

	<i>Banks Demonstration</i> (1)	<i>Taylor Demonstration</i> (2)	<i>King Demonstration</i> (3)	<i>Banks Riot</i> (4)	<i>Taylor Riot</i> (5)	<i>King Riot</i> (6)
Economic growth	-4.386* (-6.67)	-2.874* (-3.64)	-1.164* (-2.59)	-5.777* (-7.09)	-4.840* (-5.44)	-1.943* (-2.07)
Government spending	.016 (1.60)	.040* (2.81)	.009 (1.05)	-.013 (-1.07)	.010 (.62)	.029 (1.54)
Government spending growth	.087 (.50)	.500 (1.50)	-.060 (-.57)	.110 (.45)	-.113 (-.32)	.021 (.08)
Observations	4,269	1,907	1,765	4,269	1,907	1,765
Countries	127	108	137	127	108	137
BIC	8,324.9	6,974.0	1,0310.2	6,223.1	5,764.0	4,270.8

Notes: This model includes additional variables to account for austerity measures. All models include indicator measures for country and year. All variables, except adversity measures, are lagged by one year. Controls variables, identical to those in Table 2, are included in the models, but not shown; *t* statistics in parentheses.

**p* < .05 ** *p* < .01 (two-tailed tests)

Table 7. Negative Binomial Models for the Reported Number of Contentious Events for Each Year in Each Country included in the Banks Data

	<i>Banks Demonstration</i> (1)	<i>Taylor Demonstration</i> (2)	<i>King Demonstration</i> (3)	<i>Banks Riot</i> (4)	<i>Taylor Riot</i> (5)	<i>King Riot</i> (6)
Economic growth	-3.540* (-4.36)	-3.793* (-2.99)	-1.159* (-2.32)	-5.741* (-5.34)	-4.734* (-3.24)	-1.342 (-1.22)
Income inequality	.005 (.42)	.022 (1.34)	.015 (1.77)	.013 (1.00)	.036 (1.91)	-.008 (-.44)
Observations	2,807	813	1,475	2,807	813	1,475
Countries	115	73	121	115	73	121
BIC	6,297.0	3,856.4	9,035.0	4,311.4	2,939.7	3,840.2

Notes: This model includes an additional adversity measure for economic inequality. All models include control measures for country and year. All variables, except adversity measures, are lagged by one year. Controls variables, identical to those in Table 2, are included in the models, but not shown; *t* statistics in parentheses.

**p* < .05 ** *p* < .01 (two-tailed tests)

collective actions in five of the six models, all but the King riot count, where the coefficient is negative but not statistically significant.

As shown in Table 8, the inclusion of a measure of racial and ethnic discrimination similarly has little effect on the models. In only the Taylor demonstration model is discrimination significantly correlated with collective action, and the relationship is positive. Additionally, as with the inclusion of the income inequality measures, the impact of economic growth remains statistically significant in all of the models except the King riot data.

Table 8. Negative Binomial Models for the Reported Number of Contentious Events for Each Year in Each Country included in the Banks Data

	<i>Banks Demonstration</i> (1)	<i>Taylor Demonstration</i> (2)	<i>King Demonstration</i> (3)	<i>Banks Riot</i> (4)	<i>Taylor Riot</i> (5)	<i>King Riot</i> (6)
Economic growth	−4.359* (−6.10)	−3.251* (−3.75)	−1.700* (−3.49)	−5.557* (−6.40)	−5.237* (−5.33)	−1.672 (−1.57)
Racial/ethnic discrimination	.856 (1.78)	1.326* (2.12)	−.292 (−.93)	.695 (1.21)	.341 (.49)	.296 (.42)
Observations	2,875	1,409	1,167	2,875	1,409	1,167
Countries	92	77	98	92	77	98
BIC	6,500.7	5,746.8	7,350.2	5,005.4	4,883.8	3,321.1

Notes: This model includes an additional adversity measure for discrimination. All models include control measures for country and year. All variables, except adversity measures, are lagged by one year. Controls variables, identical to those in Table 2, are included in the models, but not shown; *t* statistics in parentheses.

* $p < .05$ ** $p < .01$ (two-tailed tests)

Robustness Checks

In the Appendix, we include two checks on the robustness of our findings. First, we explore the extent to which the relationship between economic growth and collective action might have changed over time, and whether this accounts for the difference in the magnitude of the effect between the different data sets. To test variation across time we split the data into three periods: 1960 to 1974, to capture the historical period associated with the 1960s protest wave; 1975 to 1990, to account for the late Cold War era; and 1991 to 2006, to capture the period after the fall of the Soviet Union. Additionally, the first and third periods generally match the time periods covered by the Taylor and King data, respectively.⁸

As shown in Table A1, the impact of economic growth is statistically significant and negatively correlated across each of the time periods for both of the outcomes in the Banks data. We find no consistent time trend for demonstrations, as the coefficient for the economic variable is largest in the first time period, but smallest in the second time period. For the riot measure, the impact increases over each time period, but the difference between the second and third time periods is minimal. Overall, we find little evidence of a changing relationship between economic conditions and collective action between 1960 and 2006. As an alternative specification we model time as a linear variable, which we include as both a main effect and in interaction with economic growth. In none of the models⁹ was the interaction effect statistically significant.

As a second check on the robustness of our findings we ran additional models that included a lagged measure of protest as an independent variable in order to correct for possible serial correlation. As shown in Table A2, the inclusion of the lagged dependent variable does not substantively change the effect size of economic growth, and the measure is statistically significant in all six models. We do not include the measure in our main models because of concerns that the lagged dependent variable will downwardly bias coefficient estimates (Achen 2000). While methods have been developed for including lagged versions of the dependent variable as covariates in linear regression models (e.g., Arellano and Bond 1991), unfortunately none are available for count models.

In sum, the relationship between economic decline and collective action is relatively robust to the inclusion of the additional control measures and we find little evidence that the effect size has

⁸ Our temporal findings are robust to various period cut points. Analysis available from the first author.

⁹ Not presented here but available from the authors.

changed over time. We note two limitations in our study. First, reverse causation could be at work. A very successful nationwide strike could cause a fair amount of economic havoc in a country, both directly during the strike and indirectly by scaring away international investors. While this no doubt happened in a small number of cases, we believe that in the vast majority of the cases the direction is going from grievance to demonstration. In one of the only studies that explores the consequences of political instability for economic growth, [Kenneth Bollen and Scott Jones \(1982\)](#) found that in developing countries political instability had an insignificant effect on economic growth, based on an index that included riots, strikes, and protests. Additionally, we do not have a measure of the mediating mechanism between economic hardship and contentious politics, such as an individual-level measure of grievance or density of local friendship networks, which we might expect to reduce the effect size of the economic adversity measure. However, even if our model is underspecified, economic decline remains a “fundamental cause” ([Link and Phelan 1995](#)) of social movement mobilization. That is, if including a measure of the presence of a resonating mobilizing frame as a measure in our models reduced the effect of economic hardship to zero, we would take this to mean that material adversity is one of the pre-conditions that allows for successful movement framing, not that economic hardship did not matter.

SUMMARY AND CONCLUSION

In our study we tested the relationship between economic adversity and anti-government collective actions, such as demonstrations, riots, and general strikes. In our cross-sectional time series analysis we found strong evidence that the level of contentious events in a given country is closely tied to the country’s economic performance, controlling for state-level political opportunities, demographics, and media practices, along with unobserved time and country effects. During periods of economic decline countries were more likely to experience anti-government demonstrations and riots. These findings are robust across multiple measures of collective action and multiple data sources. We found some evidence that this relationship is strongest in non-democracies and when the economic decline is severe. We also found that the relationship between economic decline and political action has not significantly changed over the period covered by the study, and that the finding is robust to the inclusion of a measure of organizational capacity. Further, we found little evidence that two other measures of adversity, group discrimination or income inequality, were correlated with the count of demonstrations.

We also highlight our finding about the strength of openings in the political opportunity structure, as measured by recent shifts in the level of democracy in spurring demonstrations. We believe this to be the first large and cross-national time-series analysis of this prominent concept. Our findings also highlight the unpredictability of demonstrations. The only measures that were consistently correlated with political action were our measures of economic adversity and political opportunity structure openings. All other factors remain inconsistent indicators at best, as no other factor was consistently significantly correlated with political action.

These findings highlight the need for social movement scholars to take seriously the role of economic performance as an important factor that enables mobilization. While we found little evidence that high economic performance inhibits demonstrations when compared to average economic performance, countries with shrinking GDPs were more likely to face collective challenges. While we were unable to test a specific causal mechanism in this research, we have included multiple robustness tests to uncover possible moderating and mediating conditions. As a preliminary investigation of the conditions under which grievances are most likely to lead to protest, we ran multiple models that looked at the interaction of GDP growth with key factors, such as political openings and state capacity. None of the interaction effects were statistically significant. These findings indicate that the relationship between material hardship and large-scale anti-state protest is not dependent on any one of these key factors. In addition, we included a series of other potential explanatory variables such as

region (e.g., OECD, Africa, etc.) and union density. The effect is robust across regional variation and to the inclusion of a measure for union presence.

While we cannot provide a causal mechanism, we do offer a possible interpretation. Average populations experience negative GDP growth through increased unemployment and decreased wages. While unemployment may increase biographical availability, there is little evidence that this new free time translates into more time spent at demonstrations (Snow and Soule 2009). Instead, individuals who have suffered personal economic setbacks, or those who fear that they might, are likely to find the mobilizing frames of social movement actors salient. Those not directly impacted may also feel that the state has failed at one of its prime functions of providing economic opportunities, and has therefore lost some of its legitimacy (e.g., Coleman 1990). This is also consistent with the robust, cross-national finding in political science that voters punish incumbents during bad economic times and reward them in good economic times (Lewis-Beck and Stegmaier 2000). This anti-government activism need not be restricted to movements that are exclusively economic or state focused, as other movements in times of economic crisis have been linked to nationalism (Calhoun 1997), right-wing populism (McVeigh 2009), the Zapatista uprising (Inclán 2008) and a variety of revolutionary and anti-colonial ideologies (Foran 2005; Skocpol 1979). Our evidence is mixed as to whether economic disruption is likely to cause systemic quotidian disruption (e.g., Piven and Cloward 1977) before mobilization, as the evidence was mixed with regards to the link between economic growth in the second-lowest quartile and collective action. It is likely, however, that changes associated with even modest economic decline create enough personal quotidian disruptions that the individual incidents add up to a modest amount of anti-government activities.

At both personal and systemic levels, people are likely to experience economic decline differently than economic inequality and group discrimination such that it creates individual incentives to mobilize. First, with notable exceptions, such as after the collapse of the Soviet Union, economic inequality exhibits little year-to-year variation that individuals can observe, in contrast to unemployment rates. As such, economic inequality lacks the shock value often associated with mobilization. It could also be that, over the time period we examine, there was no broad consensus that a fundamental function of the state was to level incomes. Lacking this sentiment, individuals might not feel the same lack of faith in the state and the normal channels of participation when inequality increases as when unemployment increases. While the occupy Wall Street movement has made income inequality into a political issue, it has happened within the context of the Great Recession.

Although previous work indicated a relationship between other measures of adversity and mobilization, we find no significant effect for group discrimination or economic inequality. To explain our null finding for group discrimination, we offer two possibilities. First, the level of repression and marginalization associated with our measure may imply a level of state control that prohibits any form of collective action. That is, these groups may be too repressed to contemplate mounting a serious challenge. A second possibility, also related generally to political opportunity theory, is that groups that were successful in mobilizing against discrimination may remain active even after the end of formal discrimination, as new state policies increase the incentive to mobilize for positive gains, rather than to just end negative sanctions. In either or both cases, we found no correlation between discrimination and demonstration, as we reported.

Finally, our call for bringing economic hardship back into social movement theory does not imply that movements arise solely out of economic crisis, because movements also may be generated through other hardships, such as the suddenly imposed grievance of a nuclear accident (Walsh 1981; Walsh and Warland 1983), moral shocks (Jasper and Poulsen 1995), or the political opportunity structure (McAdam 1982). Mobilization around economic grievances likely requires both a widespread sense that the current economic conditions violate the moral economy (e.g., Scott 1976; Walton and Seddon 1994) and political entrepreneurs to frame the events (Tarrow [1994] 2011). More generally, we are not dismissing theories of new social movements (Melucci 1980) or movements of “affluence” (Kerbo 1982) as incorrect. Just because grievances can be socially constructed

does not mean that material conditions are irrelevant for mobilizing, and we find that economic adversity and collective action are linked so strongly as to suggest that economic conditions cannot be overlooked as an important precursor to social movement mobilization.

APPENDIX

Table A1. Negative Binomial Models for the Reported Number of Contentious Events for Each Year in Each Country included in the Banks Data

	<i>Pre-1975</i> <i>Protest</i> (1)	<i>Pre-1975</i> <i>Riot</i> (2)	<i>1975-1991</i> <i>Protest</i> (3)	<i>1975-1991</i> <i>Riot</i> (4)	<i>Post-1991</i> <i>Protest</i> (5)	<i>Post-1991</i> <i>Riot</i> (6)
Economic growth	−5.211* (−2.63)	−4.284* (−2.17)	−3.348* (−2.86)	−5.756* (−3.78)	−3.848* (−3.23)	−6.159* (−3.81)
Observations	974	974	1,210	1,210	1,517	1,517
Countries	85	85	101	101	124	124
BIC	1,824.5	1,924.7	2,569.5	1,859.9	2,772.9	1,584.1

Notes: The data is split into three periods. All models include indicator measures for country and year. All variables, except adversity measures, are lagged by one year. Controls variables, identical to those in Table 2, are included in the models, but not shown; *t* statistics in parentheses. **p* < .05 ** *p* < .01 (two-tailed tests)

Table A2. Negative Binomial Models for the Reported Number of Contentious Events for Each Year in Each Country included in the Banks Data

	<i>Banks</i> <i>Demonstration</i> (1)	<i>Taylor</i> <i>Demonstration</i> (2)	<i>King</i> <i>Demonstration</i> (3)	<i>Banks</i> <i>Riot</i> (4)	<i>Taylor</i> <i>Riot</i> (5)	<i>King</i> <i>Riot</i> (6)
Economic growth	−3.442* (−5.46)	−2.695* (−3.50)	−1.143* (−2.66)	−4.656* (−5.87)	−3.576* (−4.26)	−1.909* (−2.07)
Lagged dependent variable	.515* (13.20)	.224* (7.16)	.339* (11.25)	.713* (14.15)	.438* (12.00)	.309* (5.46)
Observations	4,267	1,907	1,765	4,267	1,907	1,765
Countries	127	108	137	127	108	137
BIC	8,163.6	6,928.1	1,0182.5	6,047.4	5,633.3	4,241.1

Notes: This model also includes a lagged dependent variable. All models include indicator measures for country and year. All variables, except adversity measures, are lagged by one year. Controls variables, identical to those in Table 2, are included in the models, but not shown; *t* statistics in parentheses.

**p* < .05 ** *p* < .01 (two-tailed tests)

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