Economic Backwardness and Social Tension*

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Abstract

We propose that relative economic backwardness contributes to the build-up of social tension and the incidence of both nonviolent and violent forms of opposition to the current political regime. We take inspiration from Gerschenkron's (1962) essay on economic backwardness and more recent findings on international comparisons and status-seeking to develop a testable hypothesis. We use information on a large number of countries and years from a new dataset on episodes of organized mass movements and find that greater economic backwardness is indeed consistently linked to a higher probability of seeing both violent and nonviolent forms of civil unrest. IV estimations using three different instruments, including distance to either London or Washington, D.C., and mailing speeds and telegram charges around 1900, suggest that this relationship is causal.

Keywords economic backwardness, economic development, conflict, social tension

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"[...] great delays in industrialization tend to allow time for social tensions to develop and to assume sinister proportions."

(Gerschenkron 1962, p. 28)

1 Introduction

Human history is marked by upheaval and violence, and though violence in modern times has thankfully been on the decrease (Pinker 2011), we are still confronted with descriptions of mass demonstrations, political violence and civil conflict in the daily news. Why is it that some countries have witnessed increasing political stability, economic prosperity, and no large-scale civil violence for half a century or longer, while others seem caught in a vicious circle of poverty, social unrest, political upheaval, and even civil war?

In this article, we add to the understanding of the origins of social tension and organized political violence. We develop a theory of economic backwardness and social unrest inspired by the classic contribution by Alexander Gerschenkron (1962)[1] and test its implications empirically. We argue that economic backwardness can bear serious consequences for a country's peace and stability. In the spirit of Gerschenkron (1962), we interpret economic backwardness in terms of a country's distance from the world development frontier: it is a measure of relative economic (under-) development that includes technological know-how, welfare, and consumption possibilities.[2] Particularly in an era of the globalization of information, a backward country's poor or underprivileged population compares its situation not only with that of its better-off co-nationals, but also with the situation of peers in neighboring countries and places farther afield.[3] An unfavorable comparison, coupled with a political regime's inability or unwillingness to respond to growing popular discontent, can then lead to a dangerous build-up of social tension.[4] We propose that the greater a country's economic backwardness with respect to the development leader, the higher its probability of witnessing organized forms of social tension such as mass demonstrations for political regime change, revolutions and insurrections against foreign occupiers.[5]

[1] We refer to the collection of essays titled "Economic backwardness in historical perspective", first published in 1962, which includes not only the article that gave the title to the whole volume, but also other related essays. The main essay on economic backwardness was originally published in 1951.

[2] Our concept of economic backwardness is similar to the distance to the technological frontier found in the recent growth literature (e.g., Acemoglu et al. 2006, Madsen et al. 2010).

[3] James (1987) considers international comparison and emulation effects, and Valente (2009) recently formalized the idea of international comparison in a growth model.

[4] Gurr (1970: ch.4) discusses rising expectations due to exposure to new ways of life and how they can affect "frustration-aggression" and political violence.

[5] Note that we are not attempting to explain the incidence of strikes or other forms of popular dissent that don't aim at achieving maximalist political outcomes such as political regime change, secession or liberation from foreign occupiers. We also stop short of trying to explain full-blown armed civil war,

Gerschenkron studied the history of industrialization in Europe up to the mid-20th century and pointed out that Russia's "delayed industrial revolution" was surely to blame for the violent revolution of 1917 and the subsequent establishment of the dictatorial Soviet government (Gerschenkron 1962: 28). Had Russian serfdom been abolished earlier than it was, he hypothesized, the discontent among the peasantry that was the driving force behind the Russian Revolution would not have built up as it did, and economic development would have come about more gradually. In sum, "[i]f the Soviet experience teaches anything, it is that it demonstrates *ad oculos* the formidable dangers inherent in our time in the existence of economic backwardness" (ibid., 29). He later generalized this observation to state that delayed industrialization would lead to "mounting tension between the prevailing economic conditions and the promise offered by rapid industrial developments" (ibid., 362).

Gerschenkron's work was published at a time when income gaps and economic backwardness between countries around the world were on the rise.[6] The end of the colonial era across Africa and many parts of Asia brought a large number of newly independent – and very poor – nations onto the global stage. Many of these countries have still not seen economic take-off and lag ever-further behind the most highly developed nations. In our theory, we contend that Gerschenkron's insights into how economic backwardness can contribute to the emergence of social tension and large-scale violence are still useful today, particularly when combined with the concept of international comparison and status-seeking behavior.

We empirically test the hypothesized link between economic backwardness and social tension by using extensive new data on violent and non-violent mass movements at the country-year level for the post-war period (Chenoweth and Lewis 2013). Our indicator of backwardness is a simple measure of a country's distance to the economic (and technological) world leader. In Gerschenkron's context of pre-WWII industrialization, the development leader was England. During the last century, however, the United States have been at the forefront of global economic development. We therefore take the ratio of a country's per-capita income relative to that in the United States as our main measure of economic backwardness.

Across a large series of pooled OLS and logit estimations, we find that economic backwardness is an important and hitherto neglected factor in explaining organized social unrest. Greater backwardness increases a country's likelihood of seeing nonviolent and violent mass demonstrations for regime change or secession. In a second step, we take potential endogeneity issues seriously by instrumenting our backwardness measure together with income per capita. We use the minimum physical distance to either London or Washington, D.C., and mailing speed and telegram charges around 1900 as exogenous instruments for economic backwardness. To the best of our knowledge, the

although our dataset does include a small number of civil wars (see Section 3 below).

[6] Bairoch (1981), for example, makes the point that before the industrial revolution, income disparities between most countries in the world were very limited, with the ratio between richest and poorest country being in the range of 1.0 to 1.6.

latter two are entirely novel instruments for (relative) economic development. Linear two-stage estimations confirm our findings of a positive link between backwardness and the probability of witnessing violent and nonviolent mass movements, and suggest moreover that the relationship is causal. In addition, the impact of backwardness has been on the increase in recent decades, consistent with the idea that status comparisons have become easier with globalization.

The backwardness indicator has a strong positive effect on the social tension outcomes. However, this does not necessarily mean that the indicators that have been proven to be related to conflict in other studies – particularly income per capita, population size, institutional measures, and resource wealth – are no longer relevant. Instead, we contend that backwardness is a new and complementary factor that can help us explain the incidence of violent and nonviolent social unrest across countries.

Our paper is closely related to the conflict literature that has established that per capita income levels are one of the key factors affecting the likelihood of an armed civil conflict (see, among others, Collier and Hoeffler 2004; Fearon and Laitin 2003; Hegre and Sambanis 2006). This research has linked increased conflict likelihood in poor countries to frustration and grievances, low opportunity cost in recruiting rebels, and lack of military capabilities and state capacity to prevent and suppress armed conflicts. In our analysis, higher per capita income does not reduce the likelihood of violent forms of social tension once we include backwardness, suggesting that these may be more an outcome of *relative* backwardness rather than the *absolute* level of income. Particularly in our two-stage estimations, we even find that richer countries in absolute terms are more likely to experience forms of social tension that fall short of outright civil war, especially nonviolent mass movements, which may be explained by the strong empirical relation between income levels and democracy, and the institutionalized channels for (peacefully) expressing social and political dissent that democracies offer.

The present paper is also linked to classical contributions in sociology and political science that explore the origins of revolutions and social unrest. Davies (1962: 6) proposed his upside down J-curve to illustrate how revolutions are more likely to occur after a sharp reversal of fortune following a prolonged period of "objective economic and social development". People compare their current situation with their own situation in the immediate past, and the greater the mismatch between what people want and what they get, the greater the "revolutionary mood". However, the size of this gap is independent of income levels, i.e., poor satisfied people will not revolt (and the truly impoverished will be too preoccupied with survival to spare much thought for revolution), while rich dissatisfied people may well do so. This contradicts our own theory, because we believe that greater backwardness is linearly related to greater likelihood of social unrest. The J-curve is what Gurr (1970) in his seminal contribution referred to as "progressive deprivation", one of three potential sources of "relative deprivation" that can lead to political violence. Another source is "aspirational deprivation": expectations, e.g., for welfare and political freedom, rise while capabilities (i.e., what you have) remain

the same. Gurr's theory reinforces our argument that people compare their own status with that of others in other nations, but Gurr remained vague about the mechanism behind the comparison.

In order to shed light on this comparison mechanism, our contribution draws on the large literature on status-seeking and catching-up with the Joneses. The idea of a comparison of one's own social status with peers was first advanced by Veblen (1899). Status-seeking behavior and its economic spillovers has since been the subject of a large number of articles. For example, Duesenberry (1949) argued that consumption choices are influenced by the desire to catch up on the social scale; Frank (1997) elaborates on human beings' tendency to make interpersonal comparisons; Cole et al. (1992) and Bakshi and Chen (1996) see status-seeking as a fundamental ingredient of capitalism; and Carroll et al. (1997) and Alvarez-Cuadrado et al. (2004) look at growth spillovers of status seeking. All these papers, however, focus on status comparison and catching-up behavior *within* a country. James (1987) first posited that status-seeking behavior is important at the international level, too; it may be a consequence of the modernization process and have important welfare effects. The idea of international comparison and status-seeking has been formalized more recently by Valente (2009), who develops an endogenous growth model with status-seeking individuals and uses the findings to help explain the extraordinary growth of the Asian Tiger countries.

Finally, our paper contributes to the broader context of the recent political economy literature that looks at historical origins of development. Acemoglu et al. (2006) and Acemoglu and Robinson (2012) refine and extend many of the arguments for why some countries industrialize earlier than others that were advanced by Gerschenkron half a century before. Their focus, however, is on economic development as the final outcome rather than on social tension and unrest.

The rest of the paper is organized as follows. Section 2 develops the theory and testable hypothesis; Section 3 describes the data and methodology; Section 4 discusses the results of the empirical analysis; and Section 5 concludes.

2 A theory of economic backwardness and social tension

We argue that economic backwardness comes at a cost: it not only entails widespread poverty in the backward country, but also increases the risk of falling prey to political instability characterized by mass demonstrations for regime change or secession, and possibly even civil war. As mentioned above, we draw inspiration from Gerschenkron's (1962) seminal contribution on economic backwardness in pre-WWII Europe. In his essay, Gerschenkron describes and compares three countries' industrialization experiences. In chronological order of industrialization – and therefore also in increasing order of economic backwardness – he discusses the experiences of France, Germany, and

Russia. Although Gerschenkron does not expliticly define the meaning of "economic backwardness", the main determining factor is the relative level of industrialization, the benchmark for comparison for the period being England.

Gerschenkron's main focus is on analyzing the patterns of industrialization in these countries, pointing out commonalities and differences, and developing a general theory on the pattern of and mechanisms behind industrialization. His main insight was that there is no fixed sequence to the industrialization steps that each country must take on the road to economic development: instead, knowledge can be adapted from earlier industrializers, opening the possibility for faster catching-up for lagging countries. He finds three aspects that have proven to be important for industrialization and the level of economic backwardness. The first is the institutional set-up found in the later industrializers, starting with the prerequisite of eliminating institutional obstacles such as serfdom, the absence of political unification and trade barriers; and proceeding to the development of the – industrial or universal – banking sector, or – in the case of the most backward – the need for heavy state intervention in financing the early stages of industrialization. The widespread corruption and "general distrust of the public" in Imperial Russia is also mentioned as an obstacle to business-driven industrialization (ibid.: 19). These points have clearly found an echo in the recent literature on institutions and development (e.g., Acemoglu et al. 2006; Acemoglu and Robinson 2012). The second aspect concerns country-specific characteristics such as the presence of natural resources, which favors industrialization. The third and final aspect is the intellectual climate: what today would be considered part of the national culture. This is mainly defined by political ideology, in particular varying degrees of socialist ideology.[7] We take these aspects into account in our empirical approach below when choosing the control variables.

An important observation is that the delay in industrialization, and consequently in economic progress and potential social change it entails, could be a deliberate policy pursued by the political leaders. A few years after the Russian Revolution, Edwards (1927: 3) stated that all revolutions stem from the "repression of [people's] elemental wishes", their ideas and ambitions, and that the greater the repression, the greater the violence during revolutions. Such delaying tactics by the ruling élites were employed for example in Austria-Hungary, Bulgaria and Russia (Gerschenkron 1962). They were also part of the French colonial policies, which sought to avoid competition for the French economy by dominated territories' industries; belated attempts at industrialization of the colonies by the Vichy regime proved only half-hearted (Coquery-Vidrovitch 1981). Such obstructionist policies are however not confined to the pre-WWII or colonial period. For example, Ekundare (1981) shows how post-colonial Nigerian development policies were hostage to socio-political constraints, particularly regional and ethnic politics, which – deliberately or not – delayed industrialization and economic development. This political

[7] Gerschenkron distinguished between Saint-Simonian ideas in 19th century France and Marxism in Russia.

perspective of economic backwardness has been formalized by Acemoglu and Robinson (2006). In their model, political élites can have an incentive to block technological and institutional progress because these threaten the *status quo* and increase the likelihood of the élite's replacement.

Unfortunately, the missed opportunity for development is likely to come at the price of mounting social tension, eventually proving the delaying tactics of the political élite to be short-sighted, as change may be forced on it by a discontented population. Gerschenkron used the metaphor of missing "the bus that is supposed to take a country across its great spurt of industrialization"; the wait for the next bus could be a long one, and it might not be "as convenient or as fast as its predecessor" (Gerschenkron 1962: 363). During this time of forced delay, the tensions created by the suppressed forces of economic development could erupt into outright violent conflict, as witnessed in Russia.

In order to distill these ideas into a theory of economic backwardness and social tension, we are missing one crucial element: the comparison with the frontier. The notion that there is self-awareness on the part of a (large section of a) country's population of the position on the ladder of relative backwardness is implicit in Gerschenkron's writings. However, we can theoretically question the desire for catching-up (and even for freedom from suppression) in the under-developed country. Why should there be mounting discontent among a suppressed entrepreneurship and labor force in a backward country? Why should they seek development in the first place and not be satisfied with the *status quo*, even as the economic development frontier moves farther and farther away?

Gurr (1970) discussed the gap between a collectivity's "value expectations" – "the average value positions to which its members believe they are justifiably entitled" – and "value capabilities" – "the average value positions its members perceive themselves capable of attaining" (Gurr 1970: 27). This gap between aspirations and capabilities, termed "relative deprivation" (in later contributions "grievances" or "sense of injustice"), determines a group's propensity to political violence. As mentioned above, one source of relative deprivation is "aspirational deprivation", where aspirations rise but capabilities remain the same. Rising aspirations are born from exposure to new modes of life through modernization, especially mass communications media and the spread of literacy and Western-style education. However, in a slightly convoluted argument, men are only likely to be mobilized by such demonstration effects if they are already in a situation of deprivation, and especially if their situation is getting worse (Gurr 1970: ch. 4).[8]

Gurr's theory brings us a step closer to the mechanism behind the comparison with the frontier, which together with political repression or ineptitude acts as a catalyst for

[8] Another diffrence of our theory w.r.t. to that of Gurr (1970) lies in the focus of what affects the sense of backwardness or "relative deprivation": although often vague in the details, Gurr includes not only economic factors, but also political freedom as part of "values". While we believe that the repression of political freedom can play a role in the link between backwardness and social unrest, our focus is on economic freedom, indicated by our choice of income per capita gaps as the measure of backwardness.

social tension in backward countries. But why would people harbor these aspirations in the first place, particularly if their own situation had *not* objectively deteriorated? We believe the answer lies in a psychological trait that is closely related to economic behavior: in human beings' tendency for interpersonal comparison and their desire for status (Frank 1997). Commonly referred to as "catching-up with the Joneses", this behavior has been shown to apply not only to individuals within a country, but also at the international level. Individuals in underdeveloped, backward countries compare their situation with that of peers in advanced countries and seek to catch up with them. Although the awareness of relative status and development has plausibly been accentuated by the rapid pace of globalization and spread of the internet during the most recent decades, James (1987) argues that this comparison has been present for longer. He cites education, work in foreign firms' affiliates, advertising, and historical contact through colonialism as potential mechanisms for "positional taste transfer" (ibid.: 455). In his view, this international taste transfer then leads to positional consumption by the poor, with important welfare implications.[9] More formally, Valente (2009) shows how the incorporation of preferences for international status-seeking into a growth model can affect convergence in growth rates and income levels. The model's predictions are consistent with the extraordinary growth performance of East Asian countries. There is as yet much less literature on international comparisons and catching-up behavior than on the within-country counterpart, but the contributions point in a clear direction: individuals in poorer countries not only look to better-off countries for their role models, but actively seek to emulate these models.

Goods consumption lies at the heart of the theory of "catching up with the Joneses", but the human tendency for international comparison and imitation goes beyond the desire to buy new goods. Recent events in the Arab Spring, and arguably also in the Euro-Maidan demonstrations in the Ukraine, show that the frustrated desire to catch up with the frontier can extend to the political sphere, particularly in repressive regimes. China's considerable efforts to censor the information available on web-based media and social network sites demonstrate the totalitarian regime's fear of how its population may be influenced by outside impressions and critical comparisons with the rest of world. The Turkish government's recent (failed) attempt to deny access to social media websites believed to have disseminated information on high-level corruption and to have incentivized mass demonstrations for political opposition also show how modern mass media can impact not only consumption but political preferences. In the past, even in a country such as the Soviet Union, whose communist ideology was heavily biased towards holding up its own model as the global frontier to be aspired to, many people looked to the West – particularly Britain and the United States – for their cultural and political ideals. [10]

[9] In a European context, Becchetti et al. (2010) find that the comparison with better-off neighboring countries over the past thirty years has also had an impact on personal well-being and happiness perceptions, with the effect being stronger the greater the media exposure.

[10] There is ample anecdotal evidence of how the younger Soviet generations in the Cold War era

In general, it is plausible to affirm that a country's relative economic position with respect to the most advanced nations matters for those excluded from the development process. If the desire to catch up with the development leaders is being – actively or inadvertently – suppressed by the political élites, this sense of exclusion can result in organized resistance against the political regime; resistance which may take the form of peaceful demonstrations for change, or violent campaigns against the regime.

In sum, we propose that the main mechanism at work is a combination of international comparison, and therefore the awareness of relative backwardness, and the inability to emerge from economic backwardness because of either political ineptitude or the outright suppression of any entrepreneurial activity, which is perceived by the élite as a potential threat to the *status quo*. Economic backwardness may lead to a dangerous build-up of social tension; once the lid is off, it can erupt into nonviolent mass protests against the current regime, or even revolution. Our novel theoretical explanation for social tension and conflict is schematically summarized in Figure 1. Based on these arguments, we propose the following simple testable hypothesis: Greater economic backwardness will, all else equal, lead to a higher probability of experiencing episodes of mass civil unrest.

We do not contend of course that economic backwardness is the main culprit for mass demonstrations or even civil wars. Instead, we believe that it complements other theories on the origins of conflict. In particular, economic backwardness is related to but distinct from income per capita (i.e., income levels), which has proven to be one of the most robust explanatory factors in cross-country conflict studies (see e.g., Hegre and Sambanis 2006). The former measures relative economic development, while the latter measures absolute levels of development. Poor countries have consistently been found to be more prone to armed conflict, be it because of low opportunity costs of enganging in combat (Collier and Hoeffler 2004), or because per capita GDP can be interpreted as a measure of state capacity (Fearon and Laitin 2003). Accordingly, we will consider both the relative and the absolute effect of economic development simultaneously, as there is little reason to believe that one excludes the other.

3 Empirical approach and data description

3.1 Methodology

In order to test our hypothesis, we use a two-pronged approach. First, we use linear estimations to establish the basic relationship. We concentrate on pooled OLS estimations, but also show our baseline specifications using pooled logit, which is the common approach used in the large empirical armed conflict literature. We estimate the following

sought to own Western status symbols such as branded jeansware and listened to Beatles music. The latter was officially banned in the Soviet Union because of the band's perceived politically corruptive effects.

basic model:

$$social\ tension_{it} = a + b + \alpha_1 \cdot backwardness_{it} + \alpha_2 \cdot incomepc_{it} + \alpha_3 \cdot X_{it} + \epsilon_{it}. \quad (1)$$

We have a range of zero-one dummies as our dependent variable *social tension* in year t in country i. Our main explanatory variable is the *backwardness* indicator, and our focus will be on its coefficient α_1. In the conventional conflict literature, *income per capita* has been found to be the most robust explanatory variable, and we will include it in our baseline specifications. The vector X includes other conflict-literature suspects (population size, political regime type, fractionalization measures, growth, oil rents); a dummy for the post-Cold War period; and a few variables derived from the theory above (a proxy for corruption, years since unification or independence, and a socialist country dummy). All variables are described in more detail below. a is the constant term, b are decade dummies, and ϵ the error term. We report robust standard errors clustered at the country level in all tables. For logical reasons and as a first step towards addressing endogeneity issues, we lag all time-varying variables but the Cold War and socialist country dummies and time since independence by one year.

However, this approach still leaves some open questions regarding the endogeneity not only of our main explanatory variable, backwardness, but also of one of the most important other explanatory variable in the conflict literature, namely income per capita. In particular, we have to consider the possibility of reverse causality. Economic backwardness is a slow-changing variable, and a one-year lag cannot exclude potential reverse-causality issues: social tension can build up over many years and flare up several times if the underlying problems are not solved, a pattern which in turn could affect backwardness, as conflict becomes a setback for development. Similarly, income per capita is also likely to be affected by reverse causality, as (the threat of) conflict – whether violent or not – is often quickly mirrored by the economy, for example due to the pull-out of investment in uncertain times[11]. In both instances, the bias is likely to augment the effect on social tension and drive results in our favor. In addition, while we add a wide variety of control variables, it is impossible to exclude completely the possibility of omitted variable bias, which would likely attenuate the effect of backwardness and income per capita found in OLS estimations. Which type of bias is stronger is an empirical question.

Our second strategy explicitly deals with endogeneity by instrumenting economic backwardness and per capita income in a series of pooled two-stage least squares (2SLS) estimations.[12] In addition to a second stage similar to equation (1) above, we add a

[11] Alesina et al. (1996), for example, analyze the effect of political instability – defined as the probability of a government collapse – on economic growth and find that instability significantly dampens economic performance.

[12] See Miguel et al. (2004) for a similar instrumental variables approach using pooled 2SLS to estimate the impact of economic factors on conflict in Africa. Brunnschweiler and Bulte (2009) also used an IV approach to instrument income per capita in conflict estimations.

first-stage estimation of economic backwardness and per capita income levels:

$$backwardness_{it} = c + b + \beta_1 \cdot I_i + \beta_2 \cdot X_{it} + \varepsilon_{it}. \quad (2)$$

$$incomepc_{it} = d + b + \beta_3 \cdot I_i + \beta_4 \cdot X_{it} + \varepsilon_{it}. \quad (3)$$

We have a total of three different exogenous instruments I at our disposal, which allows us to achieve a strong first-stage identification and to test for overidentifying restrictions. We next describe our data in more detail.

3.2 Data description

Our panel dataset covers the years 1946-2006 and includes up to 161 independent states and over 7'100 country-years. The dataset covers countries for which we have economic data and Polity IV data for regime type, and that had a population larger than 500'000 in 2012. Summary statistics are presented in the Appendix.

Dependent variables. We use two different measures for social tension, our dependent variable, to test our hypothesis. The measures cover both violent and nonviolent forms of social unrest and are taken from a new panel dataset on nonviolent and violent campaign onsets – the NAVCO 2.0 dataset (Chenoweth and Lewis 2013).[13]

NAVCO 2.0 data is available for the period 1946-2006 and includes all mass movements or "campaigns" that have a clear maximalist (political) objective (such as expelling a foreign occupier, secession, or changing a regime), at least 1'000 participants, and recognizable leadership. This definition well accords with our theory on widespread social tension stemming from economic backwardness.[14] The dataset includes campaigns that are observable in the sense that tactics used by the participants are overt and documented. NAVCO is unique because it includes both violent and nonviolent mass movements in a detailed cross-country time-series dataset. To be coded as a nonviolent campaign, the mass movement must rely primarily on nonviolent tactics such as boycotts and civil disobedience, and it must not seek to threaten or harm the opponent. Conversely, violent campaigns primarily rely on violent tactics. This means that some violence can occur in a nonviolent campaign, but it cannot be the campaign's main means to the end.[15]

[13] In separate estimations available from the authors, we also performed all estimations with the onset of an armed civil conflict from the UCDP/PRIO Armed Conflict Database v.4-2012 (Gleditsch et al. 2002; Themnér and Wallensteen 2012) as our dependent variable. Results were weaker particularly for the IV estimations, but the signs were always in line with our main estimations using the NAVCO dataset. There are 58 country-years that are recorded in both datasets and the correlation between violent campaigns recorded in the NAVCO dataset and armed civil conflicts in the UCDP/PRIO dataset is 0.28.

[14] The dataset does not include one-off events, random riots, revolts, or spontaneous mass acts. Most coups d'état are not included in the NAVCO dataset, as they are rarely mass movements.

[15] An important note to keep in mind is that the definition of violent and nonviolent campaigns focuses on the strategy of the mass movement, not on the answering strategy of the government, which may be violent in all cases.

In total, the NAVCO dataset has 251 campaign onsets, of which 142 are violent. Our dataset includes 216 NAVCO onsets, as we do not consider independence campaigns in colonial countries. We code dummies for the different onset types: 1) onset of a campaign (either violent or nonviolent), 2) onset of a nonviolent campaign, and 3) onset of a violent campaign. In total, we have 126 violent onsets and 90 nonviolent onsets. We concentrate on the NAVCO nonviolent episodes and all (violent plus nonviolent) episodes, as we believe these are the most interesting.[16]

Economic backwardness. Our main independent variable of interest is the proxy for economic backwardness (*Backwardness*). Gerschenkron himself proposed "the size of per capita income" as a natural measure of comparison of backwardness (Gerschenkron 1970: 99). We construct a yearly backwardness measure based on the ratio of a country's GDP per capita to the GDP per capita of the technology leader – the U.S. in the post-WWII period.[17] The ratio is constructed using Maddison data (Bolt and van Zanden 2013), with GDP per capita measured in 1990 international Geary-Khamis (Purchasing-Power-Parity-adjusted) dollar terms. The data is available on a yearly basis from 1800-2010 for 163 countries. The ratio can take values larger than 1 because a handful of countries have higher GDP per capita than the U.S. in some years. To ease interpretation of the results, the ratio is multiplied by -1, so that higher values correspond to greater backwardness. This means that we expect backwardness to be positively linked to social tension. The countries with the largest ratios (that is, the most backward countries) in the first decade of the 21st century are the Democratic Republic of the Congo (DRC), Sierra Leone, Burundi, Niger, Chad and the Central African Republic.

Other covariates. We include the usual covariates from conflict studies. Of particular interest will be (the natural logarithm of) real GDP per capita (*Income pc*), which is probably the single most robust explanatory variable from the empirical conflict literature. The data for this, as well as for yearly per-capita real GDP growth (*Growth*) and the (natural log of) population size (*Population*) come from the Maddison dataset.[18] For regime type we use the revised "polity2" variable (*Polity*) from the Polity IV dataset (Marshall et al. 2013), which varies from -10 (most autocratic) to 10 (most democratic). As an alternative, we construct regime-type dummies by assigning the country to be an *Autocracy* if the original "polity" score is smaller than -5; a *Democracy* if the "polity" score is larger than 5; and a so-called *Anocracy* for all the other "polity" scores, including those coded as missing. This allows us to check for possible non-linear effects of our *Polity* measure. To control for the recent experience of a campaign, we include

[16] Results using only NAVCO violent campaigns as the dependent variable were weaker but otherwise consistent with those employing all episodes.

[17] Note that this measure of backwardness is very similar to the proxy for "proximity to the technology frontier" for an industry employed by Acemoglu et al. (2006).

[18] We completed missing population data in the Maddison dataset (Germany 1991-2011, Yugoslavia 1992, Montenegro 2007-2011, Ethiopia 1951-1992, Yemen 1951-1989, Vietnam 1955-1975) with data from the World Development Indicators (World Bank 2013) and the Penn World Tables (PWT 8.0, Feenstra et al. 2013).

a dummy indicating whether there was a campaign in the previous year. We use data on *Oil rents* from the World Bank Development Indicators (World Bank 2013, available only after 1970) to take the resource-curse hypothesis into account,[19] and data on ethnic, religious and linguistic fractionalization (*Ethnic*, *Religious*, *Linguistic fractionalization*, respectively) from Alesina et al. (2003) to control for the effects of ethnicity. Further control variables include decade dummies and a post-Cold War dummy to include time effects.

In addition to the standard conflict-literature covariates, we consider several control variables that are directly linked to the theory above. Gerschenkron (1962) mentions corruption as an obstacle to industrialization. Widespread corruption may also provide a further reason for mass discontent. With these considerations in mind, corruption could potentially affect both backwardness and social tension, and not taking it into account could lead to omitted variable bias. We use the political competition (*Polcomp*) variable from the Polity IV dataset (Marshall and Jaggers 2013) as a proxy for corruption. Our reasoning is that greater political competition leads to greater transparency at all levels of government, and from there also greater transparency and therefore less corruption within the – non-elected – bureaucracy. We use this proxy as, to our knowledge, no other, more direct measure of corruption is available since the end of WWII. Political competition is a component variable of the composite "polity" measure, but it is coded on a scale of 0 to 10 (ten being the most competitive system) and captures a specific aspect of a political regime. We revise the political competition variable provided by the Polity IV dataset in a similar manner to that used to construct the "polity2" measure: cases of foreign "interruption" (-66) are treated as missing; cases of "interregnum" or anarchy (-77) are assigned a score of zero; and cases of "transition" (-88) are prorated on a case-by-case basis across the span of the transition. The correlation coefficient between the *Polity* and *Polcomp* variables is 0.23, so they are not measuring the same aspects of political institutions.

Gerschenkron (1962) also mentioned country-specific culture as an important factor for economic backwardness; in particular, having a socialist system may (negatively) influence the development pattern. We construct a *Socialist* dummy based on Kornai (1992) and completed with information from the CIA Factbook for recent years. Finally, we also control for time since 1945 or from the year of independence, if this is later than 1945 (*Independence*), since according to the theory, national unification is a necessary prerequisite for the industrialization process to take off. We expect social tension due to delayed development to increase with time since independence.

Instrumental variables. We use three different exogenous instruments for economic backwardness and income per capita. Our first instrument is a simple measure of geographical distance of a country's present-day capital from either London or Wash-

[19] See, e.g., Lujala (2010). Gerschenkron (1962) also mentioned the importance of natural resources for industrialization and economic backwardness.

ington, D.C., whichever is shorter (*Distance*, in log kilometers).[20] Gerschenkron (1962) pointed out that the temporal progression of industrialization from England across the channel to France, Germany and then Russia was no mere geographical coincidence, but reflected the greater time for diffusion of new technologies the further away from the center of industrialization. This conviction is echoed by Bairoch (1988: 259), who states that "... there is a definite correlation between the time when the process of change got under way and the distance from England: the countries nearest to England were as a rule the soonest affected by the Industrial Revolution." Since our focus is on the period after WWII, when the United States firmly rose to the forefront as the world technology leader, we add the U.S. capital as the second "pole of attraction", next to London. The closer a country is to either of these two technology and development leaders, the faster we expect the diffusion process to have been, and the less backward the country should be. For similar reasons, countries closer to London or Washington, D.C. should also have higher incomes per capita.

We argue that this instrument is unlikely to affect the social tension outcomes other than through backwardness or income. One possible objection could invoke military policy: an intervention by either the U.S. or the UK in a country experiencing episodes of social tension, and particularly armed civil conflict, is more likely the greater the geographical proximity. However, aspects of *realpolitik* have historically played a greater role in such situations than physical distance, for example in the U.S. intervention in Vietnam, or NATO's intervention in Libya and its non-intervention in nearby Syria. Political "spheres of interest" reach beyond geography, and geographical distance is unlikely to systematically prevent (or encourage) the eruption of social tension.

Our second instrument is based on mailing times around 1900 from either London or Washington, D.C. This is, to our knowledge, the first time this information has been used. We calculate mailing speed from either London or Washington, D.C – whichever is faster – as miles covered per "mailing day" in 1903 (the first year after 1900 for which we found documentation for both the United Kingdom and the United States), using data on mailing times for regular correspondence (i.e., not packages) in days from Post Office Department (1903) and Post Office (1903).[21] [22] Where mailing time information for letters was missing, we used mailing times for parcels from the Post Office Guide (1903), subtracting three days (the average additional time for processing bulkier mail). Where this was also missing, we used extensive additional sources on travel times and transportation routes at the time to approximate the mailing time between the closest country we had data for and the entry point or the capital for the country in question. In the remaining cases, we assigned one of three values depending on whether

[20] This data is provided by Kristian Gleditsch at http://privatewww.essex.ac.uk/~ksg/data-5.html.

[21] Sometimes we had several cities for one country. In these cases, we chose the city with the shortest correspondence time, adjusting our distance calculations accordingly.

[22] We are grateful to Jenny Lynch from the United States Postal Service and to employees of The Royal Mail Archive for their generous help in finding the data.

the country could be reached roughly within 1-2 months, 3-6 months, or longer.[23] W e divided the mailing days by the (approximate) miles covered by the correspondence at the time. This distance was given for all countries listed in Post Office Department (1903), and supplemented by own calculations based on mailing route information for the remaining countries, using either the capital or main entry point for each country (the choice was determined by the information on mailing times).[24] We then took the natural logarithm to construct our final measure, *Mailingspeed,* which is expected to be negatively (positively) linked to backwardness (income).

We argue that mailing times are directly related to economic development and backwardness. Not only did it take longer for correspondence to reach the more remote parts of the world; but at equal distances, letters reached a more developed and better-connected country before its more "backwater" counterpart. For example, a letter posted in Washington, D.C. could be read by the recipient in Moscow, Russia a mere ten days later; while a correspondent in Bolivia would have to wait 37 days to receive written news from the U.S. – even though Bolivia's capital is, at around 5100 miles, physically closer to Washington, D.C. than Moscow (over 5500 miles). We do not expect mailing times to have a direct effect on social tension or violent civil conflict. Former colonies might have been more developed on average than their non-colonial neighbors, and (former) colonies are arguably more likely to have experienced conflict of some form in the post-WWII period. However, this link turns out to be tenuous at best: the correlation of our mailing speed measure with a British colony dummy is 0.05, and -0.38 for French colonies. Nevertheless, we will address this point in our robustness analysis below.

The third instrument is also novel: we use telegram charges around 1900. In the 19th century, the telegraph had launched "the greatest revolution in communications since the development of the printing press" (Standage 1998: 2), and not having access to the telegraph network soon became a disadvantage for business. There were significant differences in the way the telegraph system was used in America and Europe. In America, 80-90 percent of cables were business-related and telegraph managers were more concerned "about dispatch than low tariffs... [In Europe] the telegraph is used principally for social correspondence" (Gardiner Hubbard, quoted in Standage 1998: 158). There were also differences in the organization of the telegraph systems on either side of the Atlantic: while largely seen as a public utility in Europe and consequently run by public monopolists (e.g., by the Post Office in the United Kingdom), in America the telegraph networks were privately owned, though by the start of the 20th century Western Union had a virtual monopoly in the United States.

Telegram pricing principles were however similar across the globe. The cost of send-

[23] These remaining countries are Afghanistan, Bhutan, Burkina Faso, Chad, Kazakhstan, Kyrgyzstan, Mali, Mongolia, Nepal, Niger, Rwanda, Swaziland, and Tajikistan. A dummy variable for countries where we had to use own estimates was insignificant. The codebook for this and all other variables will be made available by the authors.

[24] For sea and navigable rivers we used Google Earth to calculate the distances and for a large portion of inland destinations we used the length of train travel.

ing a telegram depended on distance and the number of words in the message. Included in the charge was also the labor cost, right down to the messenger boy (or the telegraph officer himself in rural parts) who delivered the telegram from the telegraph office to the door of the receiver (Downey 2002). The pricing basis was usually for ten words, and then per word for each additional word (Ross 1928, Downey 2002). By the end of the 19th century, the cost of sending a telegram had been greatly reduced thanks to a combination of widely expanded cable networks and growing competition with telephones. In the 1870s, charges for international telegrams had still been particularly high: a transatlantic telegram cost £20 or around $100 at the time (Standage 1998). For exmaple, around 1900 the charge of an international telegram sent from London to Bathurst (today's Banjul) in The Gambia (42 pence per word) would have taken into account not only the distance covered (around 2700 miles), but also the relatively poor infrastructure and related extra costs of getting the cable to its recipient in that colonial outpost. Sending a telegram several thousand miles further to more developed Bathurst in New South Wales, Australia would have cost 8 pence less (34 pence per word) than sending it to the West African Coast.

The data for telegram prices is based on "*Charges for foreign and colonial telegrams*" in pence per word as listed in Post Office (1903). The historical tables are remarkably complete: only for 11 countries do we have no available information at all on the charging principle applied. For these, we combine information from historical maps of telegram networks with information on the communication routes at the time compiled to construct our mailing speed variable (see above) to assign charges based on remoteness.[25] We then take the natural logarithm to construct our instrument *Telegram*. The correlation with economic development is evident from the criteria for telegram pricing: we expect telegram charges to be positively (negatively) linked to backwardness (income). Our arguments in favor of exogeneity of this instrument are similar to the ones for the mailing speed variable given above. Any potential direct effect on social tension in the post-WWII period should be additionally mitigated by the fact that the use of telegrams went on a sharp decline at the start of the 20th century as telephone systems expanded rapidly.

4 Results

4.1 OLS and logit estimations

Table 1 shows the main results from OLS estimations. Panel A shows the findings for nonviolent campaigns only, while Panel B show the findings for all campaigns. Column 1 gives a parsimonious baseline specification including only some of the most robust variables from the conflict literature, plus a post Cold War dummy and decade dummies.

[25] A dummy variable for our own estimates was not significant.

First, we note that backwardness is positive and significant with both dependent variables; the coefficients also have similar magnitudes. In column 8, we show pooled logit results using our baseline specification for easier comparison with the conflict literature. Columns 2-7 add different control variables. Backwardness remains positive and significant in all specifications. These initial findings suggest that economic backwardness indeed has a strong positive relation with social tension, both violent and nonviolent.

When we look at the other explanatory variables, we find one striking difference with respect to the conventional conflict literature in the coefficient for income per capita. What has been found to be arguably the strongest and most consistent (violent) conflict-reducing factors is now positive and significant when considering nonviolent campaigns (Panel A), and insignificant when it comes to all types of campaigns (Panel B). This suggests two things: first, social tension that falls short of civil war may have more to do with *relative* economic backwardness and the comparison with the frontier than absolute income and the influence of opportunity costs. Second, more affluent countries tend, on average, to engage more in nonviolent movements for political change than in violent ones. Chenoweth and Lewis (2013) found a similar positive – albeit insignificant – sign on their income per capita variable in their exploratory estimations for nonviolent campaigns.[26] They posit that nonviolent resistance emerges where resistance is supposedly more difficult. We might add that high income per capita is strongly correlated with democracy, and that strong democratic systems offer more institutionalized opportunities for the (non-violent) expression of popular dissent than autocracies or weak democracies.

Polity mostly has a negative and significant sign, suggesting that (stronger) democracies see fewer campaigns on average. A break-down into regime types – Autocracy, Democracy and Anocracy (the latter is the omitted variable – in column 2 to take a non-linearity in the influence of regime type into account shows that democracies have a clear tendency to experience fewer episodes of social tension, with highly significant, negative coefficients in both panels. Autocracies also see fewer campaigns in general (Panel B), but they are neither more nor less likely to see nonviolent forms of mass protest than other political systems.[27] This is consistent with the non-linear relationship between political regimes and social unrest that has been found in the conflict literature, where weak regime types (i.e., anocracies) are most prone to experiencing civil war.

Larger countries, measured by their population size, consistently see more social tension on average, which again confirms the findings from the armed conflict literature. The strong decreasing trend in the frequency of both violent and nonviolent campaigns since the end of the Cold War from that literature is also confirmed. Ethnic fraction-

[26] Chenoweth and Lewis (2013) used the Fearon and Laitin (2003) dataset combined with the new NAVCO data that they present. The timeframe was therefore limited to 1946-1999.

[27] Exploratory estimations using an interaction term between democracy and backwardness showed that democratic backward countries were much more likely to see both nonviolent and any form of mass campaign than non-democratic countries. Backwardness itself became insignificant. However, analysing this point in detail is left for further research.

alization (column 3) and economic growth (column 4) have no effect on nonviolent or violent episodes of social tension. In additional estimations shown in the Appendix, language and religious fractionalization also have no significant relation with any form of social tension.[28] Oil-rich countries – measured by their resource rents – show no discernible difference in their pattern of social tension (column 7), which runs counter to the conventional resource curse hypothesis.

Turning to our theory-specific covariates, we note that our proxy for corruption – political competition – shows contradictory effects in column 5: it doesn't appear to affect the likelihood of nonviolent campaigns at all, while it has a strong campaign-reducing effect when all campaigns are considered. This finding is driven by the violent campaigns, and it confirms our expectation that less corruption (i.e., more political competition) is linked to less social unrest. Finally, socialist countries show a slightly increased frequency of nonviolent mass campaigns (Panel A, column 6). This is interesting, as to our knowledge it is the first time that a "socialism effect" has been considered in the conflict and social tension context. It is possible that this effect is driven by the peaceful demonstrations against the Soviet regime in Central and Eastern Europe during – and especially at the end of – the Cold War. However, robustness tests limiting the period of analysis to post-1970 diminish instead of increase the socialism effect (see Table 2).

In Table 2, we restrict the sample period to after 1970 to exclude the immediate post-colonial years, which could be influencing the results.[29] One possibility is that the importance of economic backwardness as an explanatory factor for social tension has been on the increase in more recent decades, with rising globalization making the comparison with other countries easier and more immediate. In fact, what we find is not only a confirmation of our earlier results, but even a slight strengthening: the magnitude of the coefficients is larger compared with the results from the full time period. Another noteworthy difference with the restricted time period is that autocracies have no longer been significantly less likely to see any type (violent or nonviolent) of campaign in recent decades (column 2, Panel B). In further estimations shown in the Appendix, we add more covariates and finally perform a "kitchen-sink" estimation with all explanatory variables, none of which affects our main results.

4.2 Instrumental variables estimations

Although suggestive and remarkably clear, the results of the pooled OLS and logit estimations discussed above leave some open questions regarding the exogeneity of our main explanatory variable, economic backwardness, as well as income per capita. In particular, we noted the possibility of reverse causality, as well as the ever-present chance of omitted variable bias. Can we further strengthen our conclusions by drawing a credible

[28] We do not include all fractionalization measures together as they are highly correlated amongst each other.

[29] We do not include oil rents as a control variable, as the oil rent data is only available for the post-1970 period.

causal link between greater economic backwardness and increased probability of social tension? This is what we attempt in our pooled 2SLS estimations that use distance and mailing speed from London or Washington, D.C., and telegram charges as exogenous instruments for backwardness and GDP per capita.

The main results are presented in Tables 3-4, with specifications echoing those of the OLS estimations presented above. The 2SLS results confirm the findings from the OLS estimations for backwardness, which has a strong, positive effect on the likelihood of witnessing either nonviolent campaigns, or any type of social unrest campaign included in NAVCO. Compared with the OLS results, the magnitude of the coefficients for backwardness is approximately tripled for nonviolent campaigns, and increases around five-fold for all types of campaigns. We also confirm the strong positive effect of income per capita on nonviolent campaigns and – interestingly – a weaker but still positive effect on all forms of mass campaign. The latter seems to contradict the armed-conflict literature, where the richer countries show less tendency to descend into civil war. Our results suggest that episodes of social unrest that fall short of civil war, and especially nonviolent mass movements, are more likely to happen in richer countries. However, given the very small overlap between the NAVCO dataset and armed conflict datasets such as UCDP/PRIO, we do not see these findings as a true contradiction. Note that the sizeable increase in the magnitude of both the backwardness and income per capita coefficients suggests that the upward bias in OLS estimations due to reverse causality is much smaller than the probable downward bias caused by omitted variables.[30]

In terms of impact, the models indicate that backwardness has a substantial effect on social tension. This can be illustrated by using a couple of examples. From 1987 onwards for over a decade, Mexico experienced a series of protests aimed at the government that the NAVCO dataset codes as a nonviolent campaign. In 1986, Mexico's score for backwardness was -0.27, which incidentally is also the mean score for backwardness in our dataset. Results from our baseline model for nonviolent campaigns (Table 3, column 1) suggest that had Mexico been one standard deviation (0.27) less backward – i.e., with a similar score to Israel (-0.57) and Singapore (-0.51) – the risk of a nonviolent campaign would have been reduced by 2.1 percentage points. The contrast is even starker when comparing Mexico to its northern neighbor: being at a par with the United States, the development leader, would have decreased Mexico's probability of a nonviolent campaign by an additional 3.5 percentage points, or 5.6 percentage points in total. As an illustration for violent campaigns, we can consider the South African rebellion against apartheid that emerged in 1984 with the introduction of the new constitutions. The movement used both violent and nonviolent methods and is coded as a violent campaign in the NAVCO dataset. In 1983, South Africa's score for backwardness was -0.22, placing it among the most developed countries in Africa. One standard

[30] Miguel et al. (2004) similarly found that reverse causality bias from conflict to economic growth was not as great as expected. In their case, they argued that it was outweighed by the bias introduced by measurement error in economic data in African countries.

deviation decrease in that score, bringing it to the same level as Spain (-0.50), would have lowered the likelihood of conflict by 3.5 percentage points (using the estimate from Table 4, column 1). These are large and substantial impacts considering that the average rates for nonviolent and violent campaign onsets in our dataset are 0.012 and 0.027, respectively.

The control variables have largely similar effects on the chance of witnessing social tension and armed conflict as found in the OLS and logit estimations. Differences include ethnic fractionalization, which now passes the threshold of significance and has a general campaign-increasing effect (column 3, Table 4); and oil rents, which has a negative effect on the likelihood of witnessing nonviolent campaigns (column 7, Table 3), running counter to the idea of a natural resource curse.

In Tables 5-6, we again restrict the sample period to post-1970 to see whether the effect of backwardness has been on the increase. We see a notable increase in the magnitude of the coefficients for backwardness, particularly when it affects nonviolent campaign onset (Table 5), confirming that the effect of backwardness on social tension has increased in parallel with the increase in globalization. Additional sensitivity tests with more covariates (see the Appendix) also support our main results.

Instrument validity. Our causal interpretation of the effects of economic backwardness and income per capita rests on the strength of our instruments. In order to test this, in addition to our theoretical arguments given above in Section 3.2, we also consider a range of more formal tests. All 2SLS tables provide first-stage exogenous instrument t-statistics, excluded instrument F-statistics, and Hansen J test p-values at the bottom. The Hansen J test for over-identifying restrictions can never reject the joint null hypothesis that our three instruments are valid, i.e., uncorrelated with the error term, and that they are correctly excluded from the second-stage equation. The instrument t-statistics show that our instruments are strong and affect backwardness and income in the expected manner. The distance variable proves to be a slightly better instrument for backwardness than income per capita, while mailing speeds around 1900 show a stronger link with income per capita. Telegram charges around 1900 are a good instrument for both endogenous RHS variables across nearly all our specifications. The partial r-squareds for both first-stage estimations are comfortably high, generally ranging between 0.20-0.27, showing that our combination of instruments is able to capture a good part of the variation in backwardness and income per capita. The excluded instrument F-statistics also give reassurance that the inference is robust to the possibility of weak instruments: in our main tables, they only dip slightly below 10 (the generally accepted "rule of thumb" value) in the backwardness first-stage estimations for the onset of nonviolent campaigns, when adding ethnic fractionalization as a control variable (column 3, Tables 3 and 5).[31]

[31]Note that we also kept track of the Stock-Yogo weak identification test statistics, based on the Kleibergen-Paap rk Wald F statistic and using the critical values for Cragg-Donald F statistics and i.i.d. errors. The results suggest that we can generally not reject the null hypothesis that the true

Our final test of instrument strength lies in the consideration of factors that may put into question our exclusion restriction. Earlier, we argued that colonial status was unlikely to affect the validity of our instruments. This point is worth testing explicitly, so we add a dummy for colonial status in 1903 (the year for which two of our three instruments was gathered). The results (columns 4-5 in Tables 9-10 in the Appendix) show that our conclusions are not affected. We note that the excluded instrument F-statistic dips to 8.70 in one case for the backwardness estimation (the "kitchen-sink" specification in column 5, Table 9), and that distance is no longer a good instrument for income per capita. However, our main results for backwardness or income per capita are not affected.

5 Conclusion

In this paper we extend and investigate the idea first put forward by Alexander Gerschenkron (1962) that economic backwardness can increase the emergence of social tension. Our paper has three novel points: first, we develop a theory that combines insights from economic history and political economy with recent findings on individuals' tendency for international comparison and status-seeking. Economic backwardness, defined as relative economic under-development or distance from the technology and development frontier, increases the likelihood of witnessing outbursts of social tension among a population whose desire for catching-up with the development leader is being frustrated.

Second, we test our hypothesis on the link between economic backwardness and social tension empirically, using new data on non-violent and violent forms of social unrest and mass movements. Third, our approach includes not only pooled OLS estimations and, for comparison with the empirical conflict literature, pooled logit estimations, but also two-stage least squares estimations. The latter address the potential endogeneity of backwardness and income per capita by using three exogenous instruments, two of which are entirely novel.

The results strongly suggest that economic backwardness contributes to the emergence of social tension in the form of mass (violent and nonviolent) movements. This effect is not only causal, but also appears to be on the increase in more recent decades, in parallel with rising globalization and the diffusion of rapid channels of communication. This supports our theoretical link between economic backwardness and international comparison with better-off peers as a mechanism that contributes to the eruption of social tension.

Of course, we do not propose economic backwardness as the main explanation for mass demonstrations, much less outright armed civil conflict. Instead, we believe that it

significance level of hypothesis tests based on 2SLS is below 20%. However, given our use of clustered s.e., the exact critical values that apply are in fact unknown, which is why we prefer to use the excluded instrument F-statistics as our test of weak identification (see Baum et al. 2007 for a formal discussion and recommendation of this point).

complements other theories on the origins of political violence and conflict. In particular, it is related to the measures of absolute levels of development based on income per capita that have proven to be one of the most robust explanatory factors in cross-country conflict studies. In our findings, income levels often do not only become insignificant when included together with economic backwardness, but actually show that higher income levels increase the likelihood of mass movements that fall short of civil war. This holds also when we instrument income levels. These results open up interesting new avenues for future research.

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7 Figures and tables

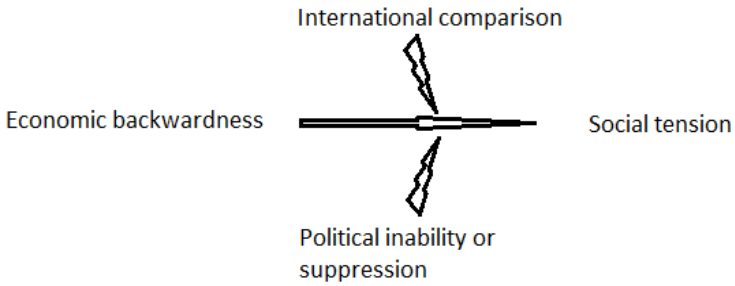

Figure 1: Schematic representation of the theory

Table 1: Basic estimations of backwardness and social tension

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A: non-violent								
Backwardness	0.023**	0.024**	0.023**	0.023**	0.023**	0.021**	0.027***	3.33**
	(2.48)	(2.51)	(2.41)	(2.39)	(2.50)	(2.34)	(2.66)	(2.17)
Income pc	0.0072**	0.0081**	0.0071**	0.0071**	0.0072**	0.0065**	0.0082**	0.82**
	(2.31)	(2.53)	(2.12)	(2.20)	(2.23)	(2.24)	(2.44)	(2.41)
Population	0.0040***	0.0040***	0.0045***	0.0041***	0.0040***	0.0037***	0.0047***	0.25***
	(3.53)	(3.61)	(3.48)	(3.52)	(3.52)	(3.44)	(3.71)	(4.78)
Post Cold War	-0.023**	-0.023**	-0.020*	-0.023**	-0.023**	-0.022**	-0.022**	-1.52***
	(-2.19)	(-2.25)	(-1.96)	(-2.20)	(-2.19)	(-2.11)	(-2.13)	(-3.32)
Polity	-0.00095***		-0.00097***	-0.00097***	-0.00096**	-0.00076***	-0.0013***	-0.080***
	(-4.27)		(-4.05)	(-4.25)	(-2.02)	(-3.91)	(-4.64)	(-4.36)
Autocracy		-0.0016						
		(-0.44)						
Democracy		-0.018***						
		(-4.17)						
Ethnic frac.			-0.00060					
			(-0.084)					
Growth				0.019				
				(1.22)				
Polcomp					0.000020			
					(0.019)			
Socialist						0.015*		
						(1.87)		
Oil rents							-0.00044	
							(-1.18)	
R^2	0.011	0.012	0.011	0.011	0.011	0.013	0.013	
Panel B: all								
Backwardness	0.023*	0.021*	0.027**	0.024*	0.023*	0.022*	0.020	3.22***
	(1.89)	(1.75)	(2.07)	(1.88)	(1.87)	(1.75)	(1.53)	(3.32)
Income pc	-0.0026	-0.0012	-0.0011	-0.0020	-0.0017	-0.0031	-0.0034	0.23
	(-0.68)	(-0.32)	(-0.26)	(-0.51)	(-0.47)	(-0.84)	(-0.81)	(1.26)
Population	0.0071***	0.0074***	0.0076***	0.0072***	0.0071***	0.0068***	0.0078***	0.19***
	(5.34)	(5.51)	(4.94)	(5.39)	(5.29)	(5.07)	(5.03)	(5.09)
Post Cold War	-0.031**	-0.033**	-0.029**	-0.034**	-0.031**	-0.031**	-0.030**	-0.95***
	(-2.40)	(-2.55)	(-2.23)	(-2.55)	(-2.36)	(-2.33)	(-2.35)	(-3.25)
Polity	-0.00075**		-0.00076**	-0.00080**	0.0010	-0.00059*	-0.0012***	-0.023
	(-2.39)		(-2.29)	(-2.47)	(1.24)	(-1.82)	(-3.11)	(-1.59)
Autocracy		-0.014**						
		(-2.37)						
Democracy		-0.028***						
		(-4.63)						
Ethnic frac.			0.0090					
			(0.96)					
Growth				-0.033				
				(-1.12)				
Polcomp					-0.0040**			
					(-2.32)			
Socialist						0.012		
						(1.17)		
Oil rents							-0.000079	
							(-0.16)	
R^2	0.014	0.016	0.014	0.014	0.015	0.014	0.017	
Observations	7,129	7,200	6,928	7,008	7,129	7,129	4,982	7,129
Countries	161	161	155	160	161	161	161	161

Notes: Columns (1)-(7) use pooled OLS estimations, column (8) uses pooled logit. The dependent variable in panel A is the onset of a non-violent campaign, and in Panel B the onset of any type of campaign. All specifications control for an ongoing episode in the previous year and include decade dummies and a constant term (not shown). All time-varying variables except the post-Cold War dummy are lagged one year. S.e. are clustered at the country level. Robust t-statistics in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

Table 2: Backwardness and social tension in post-1970 period

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Panel A: non-violent							
Backwardness	0.027***	0.030***	0.028**	0.027**	0.027***	0.025**	3.99***
	(2.67)	(2.81)	(2.56)	(2.61)	(2.66)	(2.52)	(2.64)
Income pc	0.0077**	0.0089**	0.0075**	0.0075**	0.0075**	0.0072**	0.84**
	(2.28)	(2.48)	(2.04)	(2.20)	(2.14)	(2.21)	(2.56)
Population	0.0046***	0.0044***	0.0050***	0.0046***	0.0046***	0.0043***	0.25***
	(3.78)	(3.78)	(3.75)	(3.77)	(3.79)	(3.61)	(4.65)
Post Cold War	-0.022**	-0.023**	-0.019*	-0.023**	-0.022**	-0.021**	-1.49***
	(-2.15)	(-2.21)	(-1.91)	(-2.15)	(-2.15)	(-2.07)	(-3.26)
Polity	-0.0012***		-0.0013***	-0.0012***	-0.0015***	-0.0011***	-0.090***
	(-4.61)		(-4.43)	(-4.61)	(-2.62)	(-4.18)	(-4.44)
Autocracy		0.000089					
		(0.018)					
Democracy		-0.019***					
		(-3.46)					
Ethnic frac.			-0.0017				
			(-0.20)				
Growth				0.028			
				(1.31)			
Polcomp					0.00065		
					(0.54)		
Socialist						0.014	
						(1.48)	
R^2	0.013	0.012	0.013	0.013	0.013	0.014	
Panel B: all							
Backwardness	0.023*	0.024*	0.026*	0.022*	0.024*	0.020	3.43***
	(1.69)	(1.77)	(1.81)	(1.66)	(1.80)	(1.50)	(3.13)
Income pc	-0.0027	-0.00032	-0.0017	-0.0024	-0.00094	-0.0032	0.23
	(-0.65)	(-0.073)	(-0.35)	(-0.56)	(-0.24)	(-0.77)	(1.18)
Population	0.0078***	0.0080***	0.0088***	0.0078***	0.0078***	0.0074***	0.20***
	(5.05)	(5.05)	(5.95)	(5.13)	(5.00)	(4.84)	(5.08)
Post Cold War	-0.031**	-0.032**	-0.028**	-0.033**	-0.030**	-0.029**	-0.91***
	(-2.36)	(-2.50)	(-2.19)	(-2.49)	(-2.30)	(-2.26)	(-3.11)
Polity	-0.0012***		-0.0013***	-0.0013***	0.0019*	-0.0010***	-0.042**
	(-3.39)		(-3.25)	(-3.51)	(1.86)	(-2.83)	(-2.50)
Autocracy		-0.0098					
		(-1.15)					
Democracy		-0.033***					
		(-4.20)					
Ethnic frac.			0.0065				
			(0.55)				
Growth				-0.025			
				(-0.67)			
Polcomp					-0.0068***		
					(-3.34)		
Socialist						0.017	
						(1.37)	
R^2	0.017	0.019	0.018	0.017	0.019	0.017	
Observations	5,104	5,159	4,957	5,066	5,104	5,104	5,104
Countries	161	161	155	160	161	161	161

Notes: Columns (1)-(6) use pooled OLS estimations, column (7) uses pooled logit. The dependent variable in panel A is the onset of a non-violent campaign, and in Panel B the onset of any type of campaign. All specifications control for an ongoing episode in the previous year and include decade dummies and a constant term (not shown). All time-varying variables except the post-Cold War dummy are lagged one year. S.e. are clustered at the country level. Robust t-statistics in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

Table 3: IV estimations for backwardness and social tension - Nonviolent campaigns

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Backwardness	0.077***	0.073**	0.096**	0.077***	0.075***	0.065**	0.095***
	(2.66)	(2.54)	(2.35)	(2.59)	(2.63)	(2.33)	(2.78)
Income pc	0.022***	0.022***	0.028**	0.022***	0.022***	0.018**	0.024***
	(2.82)	(2.90)	(2.24)	(2.72)	(2.91)	(2.54)	(3.00)
Population	0.0041***	0.0041***	0.0050***	0.0043***	0.0041***	0.0038***	0.0049***
	(3.81)	(3.87)	(3.82)	(3.81)	(3.79)	(3.69)	(3.85)
Post Cold War	-0.024**	-0.024**	-0.021**	-0.024**	-0.024**	-0.023**	-0.023**
	(-2.30)	(-2.35)	(-2.10)	(-2.30)	(-2.28)	(-2.21)	(-2.26)
Polity	-0.0010***		-0.0010***	-0.0010***	-0.00087*	-0.00077***	-0.0013***
	(-3.21)		(-2.97)	(-3.19)	(-1.84)	(-2.98)	(-3.61)
Autocracy		-0.0021					
		(-0.54)					
Democracy		-0.020***					
		(-3.49)					
Ethnic frac.			0.011				
			(0.96)				
Growth				0.0038			
				(0.21)			
Polcomp					-0.00035		
					(-0.30)		
Socialist						0.013*	
						(1.69)	
Oil rents							-0.00075*
							(-1.65)
First stage backwardness							
Distance	2.76	2.72	2.91	2.72	2.59	2.70	2.33
Telegram	2.61	2.41	1.71	2.58	2.53	2.71	3.02
Mailingspeed	-1.94	-1.88	-1.43	-1.93	-2.11	-2.11	-1.84
Excl. instr. F-stat.	16.9	14.9	9.07	16.4	17	16.4	16.3
Partial R^2	0.22	0.20	0.18	0.23	0.21	0.23	0.22
First stage income pc							
Distance	-1.53	-1.42	-1.87	-1.52	-1.29	-1.48	-0.76
Telegram	-4.70	-4.65	-2.81	-4.63	-4.63	-4.80	-5.09
Mailingspeed	3.14	3.11	2.52	3.15	3.40	3.25	3.57
Excl. instr. F-stat.	26.2	24.5	12.2	25.5	27	25.8	30.4
Partial R^2	0.26	0.24	0.18	0.26	0.26	0.26	0.29
Hansen J stat. p-val	0.93	0.96	0.93	0.96	0.94	0.95	0.99
Observations	7,129	7,200	6,928	7,008	7,129	7,129	4,982
Countries	161	161	155	160	161	161	161

Notes: All estimations are pooled 2SLS. The dependent variable is the onset of a non-violent campaign. All specifications control for an ongoing episode in the previous year and include decade dummies and a constant term (not shown). All time-varying variables except the post-Cold War dummy are lagged one year. First stage information includes exogenous instruments' t-statistics, partial R-squareds, excluded instruments' F-statistics, and Hansen J statistic p-value. S.e. are clustered at the country level. Robust z-statistics in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

Table 4: IV estimations for backwardness and social tension - All campaigns

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Backwardness	0.13***	0.12***	0.17***	0.13***	0.11**	0.12**	0.14***
	(2.78)	(2.67)	(2.82)	(2.85)	(2.49)	(2.52)	(2.71)
Income pc	0.021*	0.023**	0.036**	0.022*	0.018	0.018	0.022*
	(1.79)	(2.03)	(2.09)	(1.90)	(1.62)	(1.47)	(1.72)
Population	0.0073***	0.0076***	0.0083***	0.0075***	0.0072***	0.0071***	0.0079***
	(5.43)	(5.51)	(5.26)	(5.51)	(5.40)	(5.22)	(4.60)
Post Cold War	-0.034***	-0.035***	-0.032**	-0.036***	-0.033**	-0.033**	-0.033**
	(-2.60)	(-2.72)	(-2.49)	(-2.75)	(-2.53)	(-2.53)	(-2.56)
Polity	-0.00053		-0.00061	-0.00057	0.0012	-0.00036	-0.0010**
	(-1.26)		(-1.30)	(-1.32)	(1.48)	(-0.85)	(-1.99)
Autocracy		-0.015**					
		(-2.43)					
Democracy		-0.028***					
		(-3.67)					
Ethnic frac.			0.026*				
			(1.85)				
Growth				-0.055*			
				(-1.81)			
Polcomp					-0.0040**		
					(-2.18)		
Socialist						0.0092	
						(0.85)	
Oil rents							-0.00043
							(-0.71)
First stage backwardness							
Distance	3.05	2.98	3.18	3.00	2.86	2.99	2.83
Telegram	2.75	2.40	1.72	2.55	2.50	2.69	3.07
Mailingspeed	-2.12	-2.06	-1.63	-2.11	-2.26	-2.31	-2.10
Excl. instr. F-stat.	18.3	16.5	10.4	17.9	18.3	18.1	18.8
Partial R^2	0.26	0.20	0.18	0.22	0.21	0.22	0.22
First stage income pc							
Distance	-1.72	-1.59	-2.05	-1.71	-1.46	-1.65	-0.98
Telegram	-4.73	-4.72	-2.86	-4.66	-4.67	-4.84	-5.29
Mailingspeed	3.36	3.34	2.76	3.37	3.58	3.50	3.98
Excl. instr. F-stat.	27.5	26	13.9	26.8	28.1	27.4	33.6
Partial R^2	0.22	0.24	0.19	0.26	0.26	0.26	0.30
Hansen J stat. p-val	0.61	0.75	0.79	0.53	0.68	0.66	0.99
Observations	7,129	7,200	6,928	7,008	7,129	7,129	4,982
Countries	161	161	155	160	161	161	161

Notes: All estimations are pooled 2SLS. The dependent variable is the onset of any type of campaign. All specifications control for an ongoing episode in the previous year and include decade dummies and a constant term (not shown). All time-varying variables except the post-Cold War dummy are lagged one year. First stage information includes exogenous instruments' t-statistics, partial R-squareds, excluded instruments' F-statistics, and Hansen J statistic p-value. S.e. are clustered at the country level. Robust z-statistics in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

Table 5: IV estimations for backwardness and social tension - Nonviolent campaigns post-1970

	(1)	(2)	(3)	(4)	(5)	(6)
Backwardness	0.11***	0.10***	0.12**	0.10***	0.11***	0.097***
	(2.86)	(2.83)	(2.46)	(2.78)	(2.90)	(2.72)
Income pc	0.027***	0.027***	0.033**	0.027***	0.027***	0.025***
	(2.95)	(2.98)	(2.27)	(2.86)	(3.00)	(2.80)
Population	0.0046***	0.0045***	0.0055***	0.0047***	0.0046***	0.0043***
	(3.96)	(3.94)	(4.04)	(3.98)	(3.96)	(3.80)
Post Cold War	-0.024**	-0.024**	-0.021**	-0.024**	-0.024**	-0.023**
	(-2.29)	(-2.36)	(-2.09)	(-2.29)	(-2.29)	(-2.22)
Polity	-0.0013***		-0.0013***	-0.0013***	-0.0013*	-0.0011***
	(-3.33)		(-3.15)	(-3.32)	(-1.89)	(-3.14)
Autocracy		-0.0015				
		(-0.29)				
Democracy		-0.021***				
		(-3.03)				
Ethnic frac.			0.013			
			(0.89)			
Growth				0.0086		
				(0.36)		
Polcomp					-0.000047	
					(-0.033)	
Socialist						0.012
						(1.24)
First stage backwardness						
Distance	2.65	2.57	2.83	2.64	2.57	2.64
Telegram	2.94	2.81	1.94	2.93	2.81	2.96
Mailingspeed	-1.72	-1.67	-1.32	-1.73	-1.79	-1.90
Excl. instr. F-stat.	17.9	16.3	9.25	17.7	17.3	17.7
Partial R^2	0.22	0.20	0.18	0.23	0.21	0.23
First stage income pc						
Distance	-1.36	-1.18	-1.71	-1.39	-1.22	-1.34
Telegram	-4.81	-4.87	-2.86	-4.80	-4.62	-4.83
Mailingspeed	2.96	2.97	2.52	2.97	3.09	3.09
Excl. instr. F-stat.	27.6	27.5	12.5	27.3	26.6	27.5
Partial R^2	0.26	0.25	0.18	0.27	0.25	0.27
Hansen J stat. p-val	0.85	0.81	0.71	0.82	0.85	0.67
Observations	5,104	5,159	4,957	5,066	5,104	5,104
Countries	161	161	155	160	161	161

Notes: All estimations are pooled 2SLS for the post-1970 period. The dependent variable is the onset of a nonviolent campaign. All specifications control for an ongoing episode in the previous year and include decade dummies and a constant term (not shown). All time-varying variables except the post-Cold War dummy are lagged one year. First stage information includes exogenous instruments' t-statistics, partial R-squareds, excluded instruments' F-statistics, and Hansen J statistic p-value. S.e. are clustered at the country level. Robust z-statistics in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

Table 6: IV estimations for backwardness and social tension - All campaigns post-1970

	(1)	(2)	(3)	(4)	(5)	(6)
Backwardness	0.16***	0.14***	0.20***	0.16***	0.13**	0.15**
	(2.75)	(2.73)	(2.85)	(2.83)	(2.54)	(2.52)
Income pc	0.027*	0.028**	0.041**	0.030**	0.025*	0.024
	(1.92)	(2.07)	(2.09)	(2.06)	(1.88)	(1.63)
Population	0.0077***	0.0080***	0.0094***	0.0079***	0.0077***	0.0074***
	(4.51)	(4.59)	(5.65)	(4.64)	(4.60)	(4.46)
Post Cold War	-0.033***	-0.034***	-0.032**	-0.035***	-0.032**	-0.032**
	(-2.59)	(-2.71)	(-2.48)	(-2.72)	(-2.48)	(-2.50)
Polity	-0.0011**		-0.0011**	-0.0011**	0.0022**	-0.00086*
	(-2.07)		(-2.00)	(-2.10)	(2.17)	(-1.68)
Autocracy		-0.012				
		(-1.40)				
Democracy		-0.034***				
		(-3.68)				
Ethnic frac.			0.028			
			(1.54)			
Growth				-0.055		
				(-1.45)		
Polcomp					-0.0075***	
					(-3.36)	
Socialist						0.013
						(1.00)
First stage backwardness						
Distance	3.21	3.06	3.35	3.19	3.09	3.21
Telegram	2.96	2.85	2	2.96	2.85	2.98
Mailingspeed	-1.96	-1.88	-1.59	-1.97	-1.99	-2.16
Excl. instr. F-stat.	20.4	18.6	11.4	20.3	19.7	20.6
Partial R^2	0.22	0.21	0.18	0.23	0.21	0.23
First stage income pc						
Distance	-1.73	-1.48	-2.05	-1.74	-1.53	-1.70
Telegram	-4.92	-5.01	-2.95	-4.90	-4.75	-4.93
Mailingspeed	3.24	3.23	2.82	3.24	3.32	3.41
Excl. instr. F-stat.	30.3	29.9	15	30	29.1	30.6
Partial R^2	0.27	0.25	0.19	0.27	0.26	0.27
Hansen J stat. p-val	0.92	0.87	0.77	0.93	0.80	0.76
Observations	5,104	5,159	4,957	5,066	5,104	5,104
Countries	161	161	155	160	161	161

Notes: All estimations are pooled 2SLS for the post-1970 period. The dependent variable is the onset of any type of campaign. All specifications control for an ongoing episode in the previous year and include decade dummies and a constant term (not shown). All time-varying variables except the post-Cold War dummy are lagged one year. First stage information includes exogenous instruments' t-statistics, partial R-squareds, excluded instruments' F-statistics, and Hansen J statistic p-value. S.e. are clustered at the country level. Robust z-statistics in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

Table 7: APPENDIX: Descriptive statistics

Variable	Obs	Mean	Std. Dev.	Min	Max
NAVCO nonviolent campaigns	7151	0.012	0.108	0	1
NAVCO all campaigns	7151	0.027	0.161	0	1
Backwardness	7831	-0.258	0.267	-2.571	-0.007
Income pc	7831	7.974	1.082	5.315	10.667
Population	7809	9.228	1.782	4.824	18.229
Polity	7831	0.31	7.503	-10	10
Autocracy	7831	0.387	0.487	0	1
Anocracy	7831	0.237	0.425	0	1
Democracy	7831	0.376	0.484	0	1
Polcomp	7831	5.1	3.752	0	10
Ethnic frac.	7603	0.454	0.262	0.002	0.93
Language frac.	7374	0.387	0.296	0.002	0.923
Religious frac.	7648	0.419	0.236	0.002	0.86
Oil rents	5663	-4.556	5.111	-9.21	5.345
Growth	7705	0.019	0.061	-0.954	0.57
Independence	7831	28.063	17.456	0	65
Socialist	7831	0.101	0.301	0	1
Colony	7831	0.387	0.487	0	1
Distance	7831	8.116	1.343	0	9.676
Mailingspeed	7830	5.762	0.462	4.327	6.908
Telegram	7831	3.046	1.269	-0.693	5.142

Table 8: APPENDIX: OLS estimations - sensitivity analysis

	(1)	(2)	(3)	(4)	(5)
Panel A: nonviolent					
Backwardness	0.022**	0.022**	0.022**	0.027**	
	(2.38)	(2.12)	(2.34)	(2.47)	
Income pc	0.0067**	0.0065*	0.0067**	0.0075**	
	(2.13)	(1.80)	(2.10)	(2.00)	
Population	0.0038***	0.0039***	0.0039***	0.0050***	
	(3.38)	(3.15)	(3.20)	(3.48)	
Polity	-0.00097***	-0.00095***	-0.00092***	-0.0018***	
	(-4.28)	(-3.99)	(-3.96)	(-2.87)	
Post Cold War	-0.023**	-0.021**	-0.020**	-0.019*	
	(-2.17)	(-2.06)	(-2.05)	(-1.86)	
Independence	0.00014			0.00010	
	(1.19)			(0.71)	
Lang. frac.		-0.00096			
		(-0.15)			
Rel. frac.			-0.0016		
			(-0.28)		
Polcomp				0.0012	
				(0.99)	
Oil rents				-0.00041	
				(-1.02)	
Ethnic frac.				0.0020	
				(0.26)	
Socialist				0.0080	
				(0.93)	
Growth				0.030	
				(1.32)	
R^2	0.011	0.011	0.010	0.014	
Panel A: all					
Backwardness	0.022*	0.029**	0.021*	0.022	
	(1.72)	(2.09)	(1.66)	(1.54)	
Income pc	-0.0036	-0.00043	-0.0034	-0.0013	
	(-0.93)	(-0.094)	(-0.87)	(-0.28)	
Population	0.0068***	0.0071***	0.0072***	0.0084***	
	(5.04)	(4.91)	(5.00)	(5.81)	
Polity	-0.00078**	-0.00073**	-0.00073**	0.0018*	
	(-2.43)	(-2.21)	(-2.27)	(1.79)	
Post Cold War	-0.031**	-0.028**	-0.029**	-0.028**	
	(-2.38)	(-2.11)	(-2.25)	(-2.18)	
Independence	0.00026			0.00032	
	(1.38)			(1.53)	
Lang. frac.		0.011			
		(1.28)			
Rel. frac.			-0.0068		
			(-0.90)		
Polcomp				-0.0068***	
				(-3.29)	
Oil rents				-0.00018	
				(-0.40)	
Ethnic frac.				0.0066	
				(0.58)	
Socialist				0.0096	
				(0.80)	
Growth				-0.017	
				(-0.43)	
R^2	0.014	0.014	0.014	0.022	
Observations	7,129	6,700	6,959	4,803	
Countries	161	151	156	154	

Notes: The dependent variable in panel A is the onset of a non-violent episode, and in Panel B the onset of any type of episode. All specifications control for an ongoing conflict in the previous year and include decade dummies and a constant term (not shown). All time-varying variables except the post-Cold War dummy are lagged one year. S.e. are clustered at the country level. Robust t-statistics in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

Table 9: APPENDIX: IV sensitivity analysis - Nonviolent campaigns

	(1)	(2)	(3)	(4)	(5)
Backwardness	0.077***	0.092**	0.079**	0.076***	0.11***
	(2.72)	(2.47)	(2.37)	(2.75)	(2.95)
Income pc	0.022***	0.029**	0.022**	0.022***	0.029**
	(2.89)	(2.39)	(2.49)	(2.66)	(2.53)
Population	0.0041***	0.0044***	0.0041***	0.0042***	0.0058***
	(3.70)	(3.40)	(3.47)	(3.86)	(3.67)
Polity	-0.0010***	-0.0011***	-0.0010***	-0.0010***	-0.0017**
	(-3.26)	(-3.03)	(-2.99)	(-3.09)	(-2.32)
Post Cold War	-0.024**	-0.023**	-0.022**	-0.024**	-0.021**
	(-2.30)	(-2.20)	(-2.16)	(-2.29)	(-2.06)
Independence	0.000023				0.000032
	(0.17)				(0.22)
Lang. frac.		0.016			
		(1.43)			
Rel. frac.			0.0042		
			(0.58)		
Polcomp					0.00084
					(0.61)
Oil rents					-0.00098
					(-1.61)
Ethnic frac.					0.014
					(1.24)
Socialist					0.0048
					(0.55)
Growth					0.017
					(0.69)
Colony				0.0021	-0.0039
				(0.36)	(-0.60)
First stage backwardness					
Distance	2.59	2.88	2.50	2.38	1.99
Telegram	2.63	1.86	2.75	1.98	2.05
Mailingspeed	-1.97	-1.27	-1.78	-1.31	-1.53
Excl. instr. F-stat.	17.2	9.22	15.8	11.6	8.70
Partial R^2	0.22	0.18	0.22	0.16	0.15
First stage income pc					
Distance	-1.30	-1.67	-1.51	-0.89	-0.29
Telegram	-4.87	-3.30	-3.92	-3.57	-3.35
Mailingspeed	3.25	1.96	3.13	2.46	3.18
Excl. instr. F-stat.	28.1	13.2	22.7	18.4	15.7
Partial R^2	0.27	0.17	0.25	0.19	0.19
Hansen J stat. p-val	0.93	0.71	0.89	0.91	0.73
Observations	7,129	6,700	6,959	7,129	4,803
Countries	161	151	156	161	154

Notes: All estimations are pooled 2SLS. The dependent variable is the onset of a nonviolent campaign. All specifications control for an ongoing episode in the previous year and include decade dummies and a constant term (not shown). All time-varying variables except the post-Cold War dummy are lagged one year. First stage information includes exogenous instruments' t-statistics, partial R-squareds, excluded instruments' F-statistics, and Hansen J statistic p-value. S.e. are clustered at the country level. Robust z-statistics in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

Table 10: APPENDIX: IV sensitivity analysis - All campaigns

	(1)	(2)	(3)	(4)	(5)
Backwardness	0.13***	0.15***	0.14***	0.13***	0.16***
	(2.90)	(2.68)	(2.76)	(2.94)	(3.19)
Income pc	0.021*	0.034**	0.023*	0.020	0.035**
	(1.87)	(1.98)	(1.82)	(1.59)	(2.19)
Population	0.0071***	0.0078***	0.0075***	0.0072***	0.0096***
	(5.19)	(5.26)	(5.14)	(5.15)	(5.57)
PostCold War	-0.033***	-0.030**	-0.032**	-0.034***	-0.032**
	(-2.59)	(-2.32)	(-2.46)	(-2.61)	(-2.45)
Polity	-0.00056	-0.00076*	-0.00058	-0.00052	0.0020*
	(-1.31)	(-1.71)	(-1.32)	(-1.20)	(1.85)
Independence	0.00013				0.00020
	(0.60)				(1.00)
Lang. frac.		0.034**			
		(2.31)			
Rel. frac.			0.0043		
			(0.48)		
Polcomp					-0.0075***
					(-3.30)
Oil rents					-0.0011
					(-1.63)
Ethnic frac.					0.027*
					(1.69)
Socialist					0.0043
					(0.35)
Growth					-0.039
					(-0.95)
Colony				-0.0022	-0.0061
				(-0.25)	(-0.63)
First stage backwardness					
Distance	2.86	3.18	2.76	2.56	2.31
Telegram	2.58	1.86	2.65	1.96	2.14
Mailingspeed	-2.16	-1.52	-1.99	-1.48	-1.82
Excl. instr. F-stat.	18.6	18.2	17.1	12.7	10.6
Partial R^2	0.22	0.35	0.22	0.16	0.16
First stage income pc					
Distance	-1.44	-1.85	-1.70	-0.95	-0.31
Telegram	-4.92	-3.32	-3.80	-3.60	-3.54
Mailingspeed	3.49	2.21	3.40	2.64	3.57
Excl. instr. F-stat.	29.3	14.3	24.5	19.4	18.3
Partial R^2	0.27	0.17	0.25	0.19	0.20
Hansen J stat. p-val	0.62	0.25	0.77	0.61	0.63
Observations	7,129	6,700	6,959	7,129	4,803
Countries	161	151	156	161	154

Notes: All estimations are pooled 2SLS. The dependent variable is the onset of any type of campaign. All specifications control for an ongoing episode in the previous year and include decade dummies and a constant term (not shown). All time-varying variables except the post-Cold War dummy are lagged one year. First stage information includes exogenous instruments' t-statistics, partial R-squareds, excluded instruments' F-statistics, and Hansen J statistic p-value. S.e. are clustered at the country level. Robust z-statistics in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$