# 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 nonviolent and violent conflict. We test our hypothesis using data on organized mass movements and armed civil conflict. The findings show that greater economic backwardness is consistently linked to a higher probability of seeing the onset of violent and especially nonviolent forms of civil unrest. We provide evidence that the relationship is causal in IV estimations using new instruments, including mailing speeds and telegram charges around 1900. The magnitude of the effect of backwardness on social tension increases in the 2SLS estimations.

**Keywords** economic backwardness, economic development, conflict, social tension, IV estimation

**JEL codes** O10, C23, F50

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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 social and political upheaval and violence, and we are regularly confronted with images of mass demonstrations, civil unrest and conflict. In this article, we develop and test a hypothesis on how relative economic backwardness affects social unrest and armed conflict, inspired by a classic essay by Alexander Gerschenkron (1962),[1] and thus add to the understanding of the economic origins of social tension and organized political violence.

Gerschenkron's work was published at a time when income gaps and economic backwardness between countries around the world were on the rise. 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. 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.

Our focus is on a measure of between-country inequality. 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

[1] We refer to the collection of essays titled "Economic backwardness in historical perspective", published in 1962, which includes the title essay (first published in 1951) and other related essays.

[2] We discuss within-country inequality and how it relates to backwardness and conflict in Section 2. Our concept of economic backwardness is similar to distance to the technological frontier found in the recent growth literature (e.g., Acemoglu et al. 2006, Madsen et al. 2010).

places farther afield (James 1987; Valente 2009). 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. 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, or even armed civil conflict.

We empirically test the hypothesized link between economic backwardness and social tension at the country-year level for the post-WWII period by using new data on violent and nonviolent mass movements (Chenoweth and Lewis 2013), as well as established data on armed civil conflict from the UCDP/PRIO Armed Conflict Dataset (Gleditsch et al. 2002). Our indicator of backwardness is a simple measure of a country's distance to the world economic (and technological) leader: we use the ratio of a country's per-capita income relative to that in the United States.

Across a series of pooled OLS and logit estimations, we find that economic backwardness is an important and hitherto neglected factor particularly in explaining the onset of nonviolent and any type of mass movements, and to a lesser degree also of armed conflict. 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. Linear two-stage estimations reinforce our findings of a positive link between backwardness and the probability of witnessing new violent and nonviolent mass movements, and suggest moreover that the relationship is causal. Instrumental variables (IV) results for armed conflict onset remain weak, but still show a clear positive relationship with backwardness.

In addition, we find some evidence that the impact of backwardness has been on the increase in recent decades, coinciding with deepening globalization and the rise of the internet. This supports the idea that between-country inequality is an important conflict-generating mechanism. Our proposition is further supported by the fact that our measure for back-

wardness is robust to controlling for other mechanisms, including income and within-country inequality levels. Overall, our results suggest that backwardness is a new and complementary factor that can help us explain the onset of social unrest across countries.

In this article, we make three important contributions to the existing literature on the causes of social unrest. First, in our analysis we distinguish between relative and absolute economic development. Second, we consider both nonviolent and violent forms of social tension, including armed civil conflict. Third, we introduce two entirely novel exogenous instruments – mailing speed and telegram charges around 1900 – to determine the causality of the effect of backwardness on social tension.

The rest of the article 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 On economic backwardness and social tension

Gerschenkron (1962) studied the history of industrialization in Europe up to the mid-20th century and pointed out that Russia's "delayed industrial revolution" was to blame for the violent revolution of 1917 and the subsequent establishment of the dictatorial Soviet government (ibid.: 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. He wrote that "[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).[3] Gerschenkron alludes to

[3]It is worth noting that there are three aspects that Gerschenkron found particularly important for industrialization and the level of economic backwardness. The first is the institutional set-up, from basic political unification to corruption of the bureaucracy, which is also underlined in the recent literature on institutions and development (e.g., Acemoglu and Robinson 2012). The second concerns country-specific characteristics

the fact that economic backwardness is a between-country concept whose main determining factor is a country's relative level of industrialization, the benchmark for comparison for the pre-WWII period being England.[4]

A delay in industrialization, and consequently in economic progress and potential social change it entails, could be a deliberate policy pursued by the political leaders.[5] 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 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. Unfortunately, the delayed economic progress, be it intentional or not, can result in a discontented population; the tensions created by the lack of economic development could erupt into bloody conflict, as witnessed in Russia.

In order to distill Gerschenkron's 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 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

such as the presence of natural resources, which favors industrialization. The third and final aspect is the intellectual climate, mainly defined by political ideology, and in particular varying flavors of socialist ideology. We take these aspects into account in our empirical approach below when choosing the control variables.

[4]For example, "... it is the history of advanced or established industrial countries which traces out the road of development for the more backward countries" (Gerschenkron 1962: 6).

[5]The political perspective of economic backwardness was formalized by Acemoglu and Robinson (2006).

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?

We believe the answer lies in human beings' tendency for interpersonal comparison and their desire for status (Veblen 1899). Commonly referred to as "keeping up with the Joneses", this behavior has been shown to apply not only to individuals within a country (see, e.g., Duesenberry 1949; Frank 1997; Alvarez-Cuadrado et al. 2004), 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.[6] 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). Valente (2009) shows formally how the incorporation of preferences for international status-seeking into a growth model can affect convergence in growth rates and income levels, and Aronsson and Johansson-Stenman (2015) include international comparisons into a model of public-good provision and international tax coordination. The literature on international comparisons and keeping-up behavior points 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.

Based on our theoretical arguments, we propose the following testable hypothesis: Greater relative economic backwardness compared to other countries 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

[6] Gurr's (1970) theory on "aspirational deprivation" as a source of rebellion is closely related to ours. He argues that 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. But he does not explain why people harbor these aspirations in the first place, or why they would rebel if their own situation had not objectively deteriorated.

demonstrations and armed civil conflict, nor can we pretend to find incontrovertible evidence in favor of our theory. Instead, we believe that our theory complements others on the origins of conflict, and in our estimations we control for factors that could falsify our hypothesis. Importantly, 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 armed conflict studies.[7] Several recent contributions have tried to disentangle the causal links between income levels or income growth and conflict outcomes by using IV estimations (e.g., Miguel et al. 2004; Brückner and Ciccone 2010; Bergholt and Lujala 2012). Therefore, we include income per capita in our estimations and instrument it alongside relative backwardness.

Economic backwardness, or between-country inequality, is also distinct from within-country inequality as a potential driver of conflict. The idea that an inequitable distribution of resources across a population contributes to the emergence of discontent and violence goes back to the Ancient Greek philosophers.[8] Data challenges in empirical studies – especially concerning inequality – have however meant that no consensus has been reached on this class-based theory. Many recent contributions instead focus on inequality between ethnic groups or other types of inequality.[9] Backwardness understood as between-country inequality could influence within-country inequality: we might expect the delaying tactics by the élites described above to result in greater income inequality within a country, which in turn could affect conflict emergence. At best then, backwardness would have an indirect effect on social tension. However, recent evidence shows that developing countries are not necessarily more unequal than developed ones – the reality may in fact be the opposite (e.g., Piketty 2014). We try to control for these effects by including a measure of income inequality, as well as ethnic fractionalization, in our estimations.

[7] For a useful review of the conflict literature, see Blattman and Miguel (2010).

[8] See Cramer (2005) for an interesting historical discussion and literature review.

[9] Esteban and Ray (2011) present a theoretical model that includes ethnic polarization, fractionalization and inequality as determinants of conflict incidence. This model is tested empirically by Esteban et al. (2012) where, contrary to the theoretical prediction, greater inequality between ethnic groups (weakly) decreases conflict incidence. Caprioli (2005) finds that countries with higher levels of gender inequality are more likely to experience intrastate conflict.

There is little consensus on what factors are most robustly linked to the onset of nonviolent conflicts; and to our knowledge, no contribution to date attempts to establish causality. Chenoweth and Stephan (2011) and Svensson and Lindgren (2011) study the particularities of nonviolent civil resistance and their outcomes. Two other articles use the same dataset we employ to measure nonviolent conflict: Gleditsch and Rivera (2015) show that there is significant diffusion of nonviolent campaigns between neighboring states, and Butcher and Svensson (2016) show that modernization – defined in terms of absolute development and measured by the share of manufacturing value-added in GDP – is positively related to the onset of nonviolent campaigns. Using a different dataset, Chenoweth and Ulfelder (2015) find that youth bulges, regional contagion and (the lack of) civil liberties present the strongest relation to nonviolent uprisings, but that measures of modernization – including the GDP share of manufacturing (and services) – are not strongly linked to nonviolent uprisings.

# 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, concentrating on pooled OLS estimations.[10] We also show our parsimonious baseline specifications using pooled logit, which is the most common approach used in the conflict literature. We estimate the following basic model:

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

We have a range of zero-one dummies as our dependent variable *social tension* in year $t$ in country $i$. Our interest is mainly on the *backwardness* indicator and its coefficient $\alpha_1$. However, absolute development is also important: *income per capita* has been found to be

[10] We do not include country fixed effects, see e.g., Beck and Katz (2001) for a good theoretical motivation.

the most robust explanatory variable in the armed conflict literature. It is moreover a proxy for modernization, one explanation for the emergence of nonviolent conflict. We will therefore pay particular attention also to the coefficient $\alpha_2$.[11] The vector $X$ includes other common suspects from the violent and nonviolent conflict literature, as well as variables derived from the theory above; all are described in more detail below. $a$ is the constant term, $b$ are common dummies for decade $T$, and $\epsilon$ the error term. We report robust standard errors clustered at the country level in all tables.[12] For logical reasons and as a first step towards addressing endogeneity issues, we lag most time-varying variables by one year (exceptions are the Cold War and socialist country dummies, and time since independence and since the last conflict ended).

However, this approach leaves some open questions regarding the endogeneity not only of our main explanatory variable, relative backwardness, but also of 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 resolved, 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 quickly mirrored by the economy, for example, due to the pull-out of investment in uncertain times. 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.

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. In addition to a second stage similar to equation (1) above, we add first-stage estimations of economic backwardness and per capita income levels:

[11] Backwardness and income per capita are of course highly correlated. In robustness tests, we drop income per capita and find even stronger results for backwardness alone (see the Appendix).

[12] Two-way clustered errors on country and years yielded similar results, see the Appendix.

$$backwardness_{it-1} = c + b_T + \beta_1 \cdot I_i + \beta_2 \cdot X_{it-1} + \varepsilon_{it}. \quad (2)$$

$$incomepc_{it-1} = d + b_T + \beta_3 \cdot I_i + \beta_4 \cdot X_{it-1} + \varepsilon_{it}. \quad (3)$$

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. In Eq. 2 and 3, $c$ and $d$ are constant terms, $b$ are decade dummies, $\varepsilon$ denote error terms, and $X$ is a vector of control variables as described above.

## 3.2 Data description

Our panel dataset covers the years 1946-2011 and includes up to 163 independent states and over 7,800 country-years. The dataset contains countries for which we have economic data and Polity IV data for regime type (Marshall et al. 2016), and that had a population larger than 500'000 in 2012. Summary statistics are presented in the Appendix.[13]

**Dependent variables.** We use three measures for social tension, our dependent variable, to test our hypothesis. The variables come from two separate datasets with different definitions of conflict, providing a good robustness test for our hypothesis.

The first two 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). NAVCO 2.0 data is available for the period 1946-2006 and includes all sustained 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. Short-lived movements, as well as most coups d'état, are not included. This definition well accords with our theory on widespread social tension stemming from economic backwardness.

The dataset includes campaigns that are observable in the sense that tactics used by the

[13]Replication data and detailed replication instructions will be made available upon publication of the article. All analysis were conducted in STATA 14.

participants are overt and documented. 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 with at least 1,000 campaign-related deaths. This means that some violence can occur in a nonviolent campaign, but it cannot be the campaign's main means to the end.[14] In total, the NAVCO dataset has 251 campaign onsets, of which 142 are violent. Our dataset includes 216 NAVCO onsets (we do not consider independence campaigns in colonial countries), of which 90 are nonviolent onsets. We will focus on the onset of nonviolent and of any type of campaign as two of our dependent variables, but show baseline results for violent campaign onsets in the Appendix.[15]

We also use information on the onset of armed civil conflict from the annually updated UCDP/PRIO Armed Conflict Dataset, which covers the period 1946–2014 (Gleditsch et al. 2002; Pettersson and Wallensteen 2015; version v.4-2015). We include all internal and internationalized internal conflicts using the lower threshold of 25 annual battle-related deaths. An onset is coded one if there is a new conflict, or if there has been a reactivation of a conflict that has been inactive for more than two calendar years. In total our dataset has 290 armed conflict onsets. The correlation between violent campaigns recorded in the NAVCO dataset and armed civil conflicts in the UCDP/PRIO dataset is 0.28. In total, there are 58 country-years that are recorded in both datasets.

A country with an ongoing conflict (campaign) can experience a new conflict outbreak: we therefore do not drop years with ongoing conflict (campaign), keeping our full sample size. This means that the subsequent years with ongoing conflict (campaign) – without a new outbreak – are coded as zero. To control for the possibility that a country that is already experiencing conflict (campaign) has a different likelihood of experiencing a new conflict (campaign), we add a dummy variable that denotes the conflict (campaign) status in the country in the previous year. To control for the fact that countries that recently have

[14] An important point 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 always be violent.

[15] Results for the onset of only violent campaigns tend to be weaker.

experienced conflict are in general more likely to experience a new confict, we also include a count variable for the calendar years without any conflict since the end of the latest conflict (in logs), as suggested by Beck et al. (1998).[16]

**Economic backwardness.** Our main independent variable of interest is the proxy for relative economic backwardness (*Backwardness*). Gerschenkron himself proposed "the size of per capita income" as a natural starting point to compare 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. 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.** There are many common explanatory variables used in the armed and nonviolent conflict literature, as well as some specific ones that have proven significant. Common factors include the (natural logarithm of) real GDP per capita (*Income pc*), the single most robust covariate from the empirical conflict literature. Yearly per-capita real GDP growth (*Growth*) and the (natural log of) population size (*Population*) are also in common; data for all three measures come from the Maddison dataset.[17] Political institutions play a

[16] For the NAVCO dataset, we include the years of peace since the end of the last campaign of any type.

[17] 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). The Maddison dataset includes population data only up until 2008. The years 2009-2011 were extrapolated using the Maddison data.

salient role in explaining all forms of conflict: we use the revised "polity2" variable (*Polity*) from the Polity IV dataset, which varies from -10 (most autocratic) to 10 (most democratic). To measure non-linear effects of Polity, we also 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 an *Anocracy* for all the other "polity" scores, including those coded as missing.

Other variables include data on *Ethnic*, *Religious* and *Linguistic fractionalization* from Alesina et al. (2003) to control for the grievance effects of ethnic divides. Income *Inequality* is measured by the Gini coefficient. We use the dataset by Solt (2016), which provides inequality data comparable across countries (The Standardized World Income Inequality Database SWIID v.5.1).[18] Decade dummies and a post-Cold War dummy control for time effects. Data on *Oil rents* from the World Bank Development Indicators (World Bank 2013, available only after 1970) tests the resource-curse hypothesis for violent conflict (e.g., Brunnschweiler and Bulte 2009; Lujala 2010) and takes into account the importance that Gerschenkron (1962) gave to the presence of natural resources for industrialization and economic backwardness. Countries that are very open to trade have been hypothesised to engage less in conflict (e.g., Bussmann and Schneider 2007); data for *Openness* (i.e., GDP share of the sum of imports and exports) also come from the World Bank Development Indicators. Measures for *Civil liberties* from Freedom House and *Youth bulges* were both obtained from Chenoweth and Ulfelder (2015), who found them to be particularly important in explaining nonviolent social conflict. The civil liberties index varies from 1 (most liberal) to 7 (least liberal), and *Youth bulges* are measured by the proportion of the population aged 15-24 years. A measure of *Contagion* has also been found to be important for explaining nonviolent conflicts onset (Chenoweth and Ulfelder 2015; Gleditsch and Rivera 2015). We construct a dummy variable denoting the occurrence in a given year of another campaign or armed civil conflict (respectively, for NAVCO and UCDP/PRIO datasets) in the same UN region.[19]

[18] We maximise the available data by interpolating missing years, but inclusion of inequality still substantially reduces our sample size.

[19] The UN regions are: Eastern Africa, Middle Africa, North Africa, Southern Africa, Western Africa,

We also consider several control variables that are directly linked to our theory. Gerschenkron (1962) mentions corruption as an obstacle to industrialization. Widespread corruption may also provide a further reason for mass discontent. We use the political competition (*Polcomp*) variable from the Polity IV dataset (Marshall et al. 2016) 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 less corruption within the – non-elected – bureaucracy. We use this proxy as, to our knowledge, there is no more direct measure of corruption available for the entire period. 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.[20] Gerschenkron (1962) also mentioned country-specific culture and particularly a socialist system as an important factor for the later industrializers. 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,* natural log), 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 Washington, D.C., whichever is shorter (*Distance*, in log kilometers).[21] 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

Caribbean, Cantral America, South America, Northern America, Central Asia, Eeastern Asia, Southern Asia, South-Eastern Asia, Western Asia, Eastern Europe, Northern Europe, Southern Europe, Western Europe, Australia and New Zealand, and Melanesia.

[20] We revise the interregnum authority scores on *Polcomp* (i.e., -66, -77, and -88) into conventional polity scores in a similar manner to that used to construct the "polity2" measure (see Marshall et al. 2016: 17 for details). The correlation coefficient between the *Polity* and *Polcomp* variables is 0.23, so they are not measuring the same aspects of political institutions.

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

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. Following the same logic, countries closer to London or Washington, D.C. should 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).[22] Where mailing time information for letters was missing,

[22]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.

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 the country could be reached roughly within 1-2 months, 3-6 months, or longer.[23] We 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

[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. Dropping all own-coded countries for the mailing speed and telegram charge IVs from the analysis weakens the strength of the mailing speed IV, but results carry through and IVs remain valid (see the Appendix). The codebook for the instruments will be made available by the authors.

[24] For sea and navigable rivers we used Google Earth to calculate the distances and for many inland destinations we used the length of railway travel at the time.

at best: the correlation of our mailing speed measure with a British colony dummy is 0.05, and -0.38 for French colonies.

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 sending 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 very high: a transatlantic telegram cost £20 or around $100 at the time (Standage 1998). By 1900 the charge of an international telegram sent, for example, 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: we have no available information at all on the charging principle applied for less than two dozen countries. 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. 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 during the first decades of the 20th Century as telephone systems rapidly expanded.

# 4 Results

## 4.1 OLS and logit estimations

We will discuss in turn the results using NAVCO and UCDP/PRIO data for our dependent variable, social tension. Table 1 shows the main OLS results for the NAVCO data. Panel A shows the findings for nonviolent campaigns only, while Panel B shows the findings for all campaigns. Column 1 gives a parsimonious baseline specification including only the most robust variables from the conflict literature. In column 9, we show pooled logit results using our baseline specification for easier comparison with the existing literature. Columns 2-7 add different control variables; Column 8 adds them all together.[25]

First, we note that backwardness is positive with both dependent variables and that the

[25] Note that columns 6 and 8 are restricted to the period after 1970 due to oil rent data availability.

coefficients also have similar magnitudes across most specifications. The results are highly significant for the nonviolent campaigns, but less so when all campaigns are included. These initial findings suggest that economic backwardness indeed has a positive relation especially with nonviolent social tension.

When we look at the other explanatory variables, we find one striking difference with respect to the conventional armed conflict literature in the coefficient for income per capita. What is arguably the strongest and most consistent (violent) conflict-reducing factor 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 outright armed civil conflict has to do with relative economic backwardness and the comparison with the frontier, as well as with absolute income and the influence of opportunity costs. Second, more affluent countries tend, if at all, to experience nonviolent campaigns rather than violent ones. A plausible explanation can be that democratic countries, which also tend to have higher income per capita levels, offer more institutionalized opportunities for the (nonviolent) expression of popular dissent than autocracies or weak democracies.[26]

The results for violent civil conflict, using the UCDP/PRIO data, are shown in Table 2. Specifications are analogous to those in Table 1. Backwardness is positive in most OLS estimations, but only significant in the logit model (column 9). GDP per capita is negative and mostly significant, in line with the existing literature.

The remaining covariates show similar results for all measures of social tension. Polity mostly has a negative and significant sign for NAVCO campaigns, suggesting that (stronger) democracies see fewer campaigns on average. It is mostly insignificant for UCDP/PRIO armed conflict onsets. Larger countries, measured by their population size, consistently see more

[26]Income per capita was also positive (but insignificant) in Chenoweth and Lewis' (2013) study on nonviolent campaigns. Collier and Rohner (2008) found that in countries that were below an income threshold, democracy increased the probability of political violence; conversely, richer democracies saw less political violence. 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.

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 fractionalization (column 2) has no effect on nonviolent or all campaigns, but increases the likelihood of armed civil conflict.[27] Economic growth (column 3) and oil rents (column 6) show no link with any form of social tension. In column 7 we include the Gini coefficient as a measure for inequality: the results for NAVCO campaigns show that countries with higher levels of inequality are less likely to experience a campaign. The inclusion of inequality more than doubles the magnitude of backwardness, while nearly halving the sample size.[28] Inequality does not have any link to armed civil conflict. Inclusion of all covariates together in column (8) confirms the findings from the piecewise additions.

Turning to our other theory-specific covariates, we note that our proxy for corruption – political competition – doesn't appear to affect the likelihood of nonviolent campaigns or armed conflict 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 supports our expectation that less corruption (i.e., more political competition) is linked to less social unrest. Finally, socialist countries show an increased frequency of nonviolent mass campaigns and a decreased frequency of armed conflict. 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 largely 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 below).

[27] In additional estimations shown in the Appendix, language and religious fractionalization also have no significant relation with NAVCO campaigns, but language franctionalization is positively linked to armed conflict.

[28] The change is due both to the drop in the number of observations, and the inclusion of inequality.

## 4.2 Instrumental variables estimations

Although suggestive, the results of the pooled OLS and logit estimations presented above leave open questions regarding the exogeneity of our main explanatory variable, economic backwardness, as well as income per capita (see Section 3). Can we further strengthen our conclusions by drawing a credible causal link between greater economic backwardness and increased probability of social tension? This is what we do 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 for NAVCO campaigns and in Table 5 for UCDP/PRIO armed civil conflicts, with specifications analogous to those of the OLS estimations presented above.

The 2SLS results generally strengthen the findings from the OLS estimations for backwardness: it has a strong, positive effect on the likelihood of a country witnessing a new nonviolent and any type of campaign included in NAVCO. The only exception is when we add within-country inequality to explain nonviolent campaign onsets (column 7 in Table 3): the inclusion decreases the magnitude of the backwardness coefficient and renders it insignificant. Backwardness is again significant when we add all controls in column 8. Although not significant at conventional levels, backwardness positively affects the onset of armed conflict, too.[29] Compared with the OLS results, the magnitude of the coefficients for backwardness has more than tripled for nonviolent campaigns, and increased around six-fold for all types of campaigns. The change in magnitude for armed conflict is even larger.

We also confirm the strong positive effect of income per capita on nonviolent campaign onset and – interestingly – find a similiar positive effect for all forms of mass campaign. Our 2SLS results underline that episodes of social unrest that fall short of armed civil conflict, and especially nonviolent mass movements, are *more* likely to happen in richer countries. Income per capita shows no effect on armed conflict: the inclusion of relative development has made the impact of absolute development disappear.

In terms of impact magnitude, the models indicate that backwardness has a substantial

[29] Significance levels on the backwardness coefficient are generally between 10-20%.

effect on social tension, comparable with that of income levels. This can be illustrated by using a couple of examples from our NAVCO results, which are the most robust. 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 the onset of a nonviolent campaign would have been reduced by 2.2 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. The impact of income per capita has similiar magnitude: one standard deviation increase in income per capita increases the likelihood of nonviolent campaign onset by 2.3 percentage points.

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 constitution. 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 deviation decrease in that score, bringing it to the same level as Spain (-0.50), would have lowered the likelihood of conflict by 3.4 percentage points (using the estimate from Table 4, column 1). The impact of income per capita on all mass movements remains at the same level as for non-violent campaigns. These are large and substantial impacts considering that the average rates for nonviolent and all 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 has a weak campaign-increasing as well as armed conflict-increasing effect; economic growth, which lowers the likelihood of witnessing the onset of all types

of NAVCO campaigns; and the dummy for socialist countries, which is only significantly decreasing armed conflict onset.

## 4.3 Instrument validity and sensitivity analysis

Our causal interpretation of the effects of economic backwardness and income per capita in the 2SLS estimations rests on the strength of our instruments. In order to test this, in addition to our theoretical arguments given in Section 3, 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 for NAVCO results. In the case of the UCDP/PRIO armed conflicts, the test rejects the joint null hypothesis at $p$=0.1 level when within country inequality is included in the model (columns 7 and 8, Table 5).

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 variables across most specifications. The partial r-squareds for both first-stage estimations are comfortably high, generally ranging between 0.14-0.30, 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 are always above 10 (the generally accepted 'rule of thumb' value), the only exception again being when within country inequality is included in the UCDP/PRIO armed conflict models.[30]

[30] Stock-Yogo weak identification test statistics suggest that we cannot reject the null hypothesis that the true significance level of hypothesis tests based on 2SLS is below 20% in most cases. 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).

To further test the strength of our instruments, we conduct additional analyses (see the Appendix for results).[31] To check the validity of our distance IV, we substitute it with alternative measures and find that our results hold.[32] Mailing times and telegram charges could be affected by either conflicts that occured in the 19th century – which possibly correlate with the probability of seeing conflict after WWII – or by a co-determinant of conflict. To check the former, we controlled for historical conflict between 1816-1910. To check the latter, we added dummies for colonial status, or for being a British or French colony in 1903, as this could affect not only development, but also social tension. We further control for trade, since geographical proximity affects trade intensity and development, and potentially armed conflict (Bussmann and Schneider 2007). Results are robust in all cases. We also add the instruments one-by-one and in pairs taking each endogenous variable separately; again, the instruments perform well and results are largely consistent.[33] Finally, all three instruments are time-invariant, which might reduce their validity especially in more recent years. However, their relation to the endogenous variables is surprisingly strong and consistent over time (see Figure 1 in the Appendix). In sum, although we can never fully rule out all threats to identification, our exogenous instruments appear to be strong.

In further sensitivity tests (see the Appendix), we first restrict the sample period to after 1970. Rising globalization has made the comparison with other countries easier and more immediate and it is thus plausible that the importance of economic backwardness as an explanatory factor for social tension has been on the increase in more recent decades. The results show this to be the case for nonviolent campaign onsets: the magnitude of the coefficients is larger compared with the results from the full time period. Results for all types

[31] We thank the Editor and three anonymous reviewers for suggesting these tests.

[32] We use only distance to Washington, D.C and find very similar results to the ones using minimum distance to Washington, D.C. or London. Using placebo distances to Russia, China, or Brazil as a substitute IV instead did not work, strengthening our argument on the role of proximity to the technological leaders. Finally, distance to Nigeria – one of the least developed regions in the world – worked with opposite signs, again supporting our argument.

[33] Backwardness on its own is no longer a significant determinant of nonviolent campaign onset, but is highly significant in the case of all types of campaign and armed conflicts: without controlling for development levels, relative development appears to strongly affect violent forms of social tension. Income per capita on its own instead has no effect on the onset of nonviolent campaigns, but significantly reduces the likelihood of onset of any type of campaign and of armed conflict.

of campaigns and for armed conflict do not change substantially. We also include different control variables, namely years since independence; language and religious fractionalization instead of ethnic fractionalization; dummies for autocracy and democracy instead of Polity;[34] contagion effects; youth bulges; and civil liberties.[35] None of these much affects our results, with the exception of youth bulges: albeit not significant themselves, their inclusion generally weakens our findings. Substituting decade dummies with year dummies also has little effect on the results.

We further test our theory using two measures of civil war onset as the dependent variable, taking data from the Correlates of War and the UCDP/PRIO databases, both with similarly coded intra-state conflicts with at least 1,000 battle-related combatant fatalities within a twelve-month period. Both backwardness and income per capita are insignificant. This is in line with many previous studies on the determinants of armed conflict, where results for full–blown civil war are generally weak.

Finally, we collapse our data into a cross-country dataset by using variable means for the period 1946-2011 for each country. The cross-country OLS results for backwardness are often even stronger than those of the pooled regressions and backwardness has a positive and significant impact on all types of civil unrest, including violent campaigns and armed civil conflict in 2SLS estimations.[36]

# 5 Conclusions

In this paper we extend and investigate the idea first put forward by Gerschenkron (1962) that economic backwardness can increase the emergence of social tension. Our paper has three

[34] Democracies have a clear tendency to experience fewer episodes of nonviolent and violent campaigns, with highly significant, negative coefficients. Autocracies also see fewer campaigns in general, but they are neither more nor less likely to see nonviolent forms of mass protest than other political systems. Both democracies and autocracies show a weak tendency to experience fewer armed conflicts. These findings are consistent with the non-linear relationship between political regimes and social unrest that has been found in the armed conflict literature, where weak regime types (i.e., anocracies) are most prone to experience civil conflict.

[35] Civil liberties and nonviolent campaigns have an inverted U-shape relation in pooled OLS estimations, as in Chenoweth and Ulfelder (2015): the countries in which people have the least and most civil rights are less likely to experience a campaign onset.

[36] Because of the much smaller sample size, first stage results are weaker in cross-section 2SLS regressions.

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 keeping-up with the development leader is being frustrated. The inability to escape from backwardness may be due to political ineptitude, or the outright suppression of entrepreneurial activity, which is perceived by the élites as a potential threat to the status quo.

Second, we test our hypothesis on the link between economic backwardness and social tension empirically, using new data on nonviolent and violent forms of mass movements, and established data on armed civil conflict. 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 to determine causality. 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 movements, particularly nonviolent ones, and to a lesser degree also armed conflict. This effect is not only causal, but there is also evidence that it has been on the increase in more recent decades for the case of nonviolent campaigns, 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 or armed civil conflict. Instead, we believe that it 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 armed civil conflict. This holds also when we instrument income levels.

Our results may serve as a warning to governments that missed opportunities for economic development will come at the price of mounting social tension and unrest. Economic development is not only desirable for its own sake, but also because a widening gap between development leaders and laggards poses serious risks for internal stability in the countries left behind.

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Table 1: Backwardness and social tension: NAVCO campaigns

| | (1) OLS | (2) OLS | (3) OLS | (4) OLS | (5) OLS | (6) OLS | (7) OLS | (8) OLS | (9) logit |
|---|---|---|---|---|---|---|---|---|---|
| *Panel A: nonviolent* | | | | | | | | | |
| Backwardness$_{t-1}$ | 0.023** | 0.023** | 0.022** | 0.022** | 0.021** | 0.026** | 0.046** | 0.036** | 20.6** |
| | (2.26) | (2.23) | (2.19) | (2.27) | (2.15) | (2.38) | (2.37) | (1.99) | (2.04) |
| Income p.c.$_{t-1}$ | 0.006** | 0.006* | 0.006* | 0.006* | 0.006* | 0.007** | 0.007 | 0.007 | 1.97** |
| | (2.01) | (1.94) | (1.92) | (1.97) | (1.96) | (2.16) | (1.49) | (1.41) | (2.16) |
| Population$_{t-1}$ | 0.007*** | 0.007*** | 0.007*** | 0.007*** | 0.006*** | 0.008*** | 0.007*** | 0.006*** | 1.61*** |
| | (6.07) | (5.89) | (6.03) | (6.06) | (6.32) | (6.54) | (5.21) | (3.92) | (7.80) |
| Polity$_{t-1}$ | -0.001*** | -0.001*** | -0.001*** | -0.001* | -0.001*** | -0.001*** | -0.002*** | -0.002* | 0.92*** |
| | (-4.31) | (-4.12) | (-4.32) | (-1.86) | (-4.02) | (-4.76) | (-4.70) | (-1.92) | (-3.88) |
| Post Cold War | -0.024** | -0.021** | -0.024** | -0.024** | -0.023** | -0.023** | -0.036** | -0.031** | 0.18*** |
| | (-2.30) | (-2.07) | (-2.31) | (-2.29) | (-2.22) | (-2.24) | (-2.39) | (-2.16) | (-3.43) |
| Ethnic frac. | | 0.0010 | | | | | | 0.013 | |
| | | (0.15) | | | | | | (1.45) | |
| Growth$_{t-1}$ | | | 0.012 | | | | | 0.011 | |
| | | | (0.83) | | | | | (0.30) | |
| Polcomp$_{t-1}$ | | | | -0.0002 | | | | -0.0002 | |
| | | | | (-0.22) | | | | (-0.13) | |
| Socialist | | | | | 0.013* | | | 0.025** | |
| | | | | | (1.72) | | | (2.01) | |
| Oil rents$_{t-1}$ | | | | | | -0.0003 | | 0.0001 | |
| | | | | | | (-1.27) | | (0.028) | |
| Inequality$_{t-1}$ | | | | | | | -0.001*** | -0.001** | |
| | | | | | | | (-2.81) | (-2.11) | |
| $R^2$ | 0.014 | 0.014 | 0.014 | 0.014 | 0.015 | 0.016 | 0.025 | 0.026 | |
| *Panel B: all* | | | | | | | | | |
| Backwardness$_{t-1}$ | 0.020* | 0.023* | 0.022* | 0.020* | 0.019 | 0.017 | 0.071*** | 0.068*** | 18.8*** |
| | (1.70) | (1.88) | (1.80) | (1.67) | (1.57) | (1.33) | (3.38) | (3.09) | (3.23) |
| Income p.c.$_{t-1}$ | -0.003 | -0.001 | -0.002 | -0.002 | -0.003 | -0.003 | 0.004 | 0.007 | 1.17 |
| | (-0.80) | (-0.28) | (-0.54) | (-0.61) | (-0.93) | (-0.79) | (0.84) | (1.25) | (0.97) |
| Population$_{t-1}$ | 0.01*** | 0.010*** | 0.01*** | 0.009*** | 0.009*** | 0.010*** | 0.010*** | 0.01*** | 1.39*** |
| | (6.74) | (6.80) | (6.76) | (6.55) | (6.37) | (7.27) | (6.26) | (5.41) | (6.79) |
| Polity$_{t-1}$ | -0.001*** | -0.001** | -0.001*** | 0.001 | -0.001** | -0.001*** | -0.002*** | 0.0003 | 0.97* |
| | (-2.65) | (-2.61) | (-2.79) | (1.13) | (-2.15) | (-3.45) | (-3.27) | (0.29) | (-1.95) |
| Post Cold War | -0.033** | -0.030** | -0.033** | -0.032** | -0.032** | -0.032** | -0.037** | -0.032** | 0.36*** |
| | (-2.52) | (-2.36) | (-2.53) | (-2.48) | (-2.46) | (-2.47) | (-2.33) | (-2.07) | (-3.45) |
| Ethnic frac. | | 0.0097 | | | | | | 0.011 | |
| | | (1.08) | | | | | | (0.89) | |
| Growth$_{t-1}$ | | | -0.047 | | | | | -0.067 | |
| | | | (-1.62) | | | | | (-1.23) | |
| Polcomp$_{t-1}$ | | | | -0.004** | | | | -0.005** | |
| | | | | (-2.35) | | | | (-2.53) | |
| Socialist | | | | | 0.011 | | | 0.002 | |
| | | | | | (1.05) | | | (0.15) | |
| Oil rents$_{t-1}$ | | | | | | -0.0002 | | 0.00001 | |
| | | | | | | (-0.65) | | (0.018) | |
| Inequality$_{t-1}$ | | | | | | | -0.001** | -0.001** | |
| | | | | | | | (-2.61) | (-2.13) | |
| Observations | 7115 | 6914 | 7073 | 7115 | 7115 | 4968 | 3601 | 3197 | 7115 |
| Countries | 160 | 154 | 159 | 160 | 160 | 160 | 133 | 130 | 160 |
| $R^2$ | 0.015 | 0.016 | 0.015 | 0.016 | 0.016 | 0.018 | 0.027 | 0.028 | |

*Notes*: The dependent variable in Panel A is the onset of a nonviolent campaign, and all types of campaigns in Panel B. Col. (9) shows marginal effects after logit. Backwardness is defined as $(GDPpc_{it}/GDPpc_{USt}) \cdot -1$. All specifications control for an ongoing episode in the previous year and peace years and include decade dummies and a constant term (not shown). 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: UCDP/PRIO armed conflicts

| | (1) OLS | (2) OLS | (3) OLS | (4) OLS | (5) OLS | (6) OLS | (7) OLS | (8) OLS | (9) logit |
|---|---|---|---|---|---|---|---|---|---|
| Backwardness$_{t-1}$ | 0.006 | 0.012 | 0.006 | 0.006 | 0.009 | -0.0026 | 0.011 | 0.012 | 9.69*** |
| | (0.34) | (0.64) | (0.35) | (0.32) | (0.49) | (-0.15) | (0.34) | (0.35) | (2.59) |
| Income p.c.$_{t-1}$ | -0.011* | -0.007 | -0.011* | -0.011* | -0.011* | -0.014** | -0.010 | -0.012 | 0.92 |
| | (-1.93) | (-1.17) | (-1.78) | (-1.93) | (-1.84) | (-2.41) | (-1.20) | (-1.17) | (-0.51) |
| Population$_{t-1}$ | 0.011*** | 0.012*** | 0.011*** | 0.011*** | 0.012*** | 0.012*** | 0.012** | 0.012** | 1.36*** |
| | (3.49) | (3.60) | (3.52) | (3.48) | (3.67) | (3.20) | (2.22) | (2.19) | (5.13) |
| Polity$_{t-1}$ | 0.0001 | 0.0001 | 0.0001 | 0.001 | -0.0002 | -0.00003 | 0.001 | 0.004* | 1.00 |
| | (0.15) | (0.23) | (0.11) | (0.57) | (-0.36) | (-0.046) | (0.53) | (1.72) | (0.32) |
| Post Cold War | -0.045*** | -0.044*** | -0.045*** | -0.045*** | -0.046*** | -0.045*** | -0.052*** | -0.046** | 0.42*** |
| | (-3.02) | (-2.98) | (-3.03) | (-3.01) | (-3.08) | (-3.04) | (-2.79) | (-2.53) | (-3.90) |
| Ethnic frac. | | 0.023** | | | | | | 0.013 | |
| | | (2.02) | | | | | | (0.76) | |
| Growth$_{t-1}$ | | | -0.014 | | | | | 0.026 | |
| | | | (-0.35) | | | | | (0.53) | |
| Polcomp$_{t-1}$ | | | | -0.001 | | | | -0.008** | |
| | | | | (-0.62) | | | | (-2.43) | |
| Socialist | | | | | -0.021** | | | -0.050*** | |
| | | | | | (-2.29) | | | (-3.27) | |
| Oil rents$_{t-1}$ | | | | | | 0.001 | | 0.001 | |
| | | | | | | (0.91) | | (1.34) | |
| Inequality$_{t-1}$ | | | | | | | 0.0001 | -0.0004 | |
| | | | | | | | (0.21) | (-0.95) | |
| Observations | 7745 | 7524 | 7707 | 7745 | 7740 | 5645 | 4088 | 3674 | 7745 |
| Countries | 162 | 154 | 161 | 162 | 161 | 162 | 136 | 131 | 162 |
| $R^2$ | 0.026 | 0.027 | 0.026 | 0.026 | 0.027 | 0.031 | 0.032 | 0.038 | |

*Notes*: The dependent variable is the onset of an armed conflict. Col. (9) shows marginal effects after logit. Backwardness is defined as $(GDPpc_{it}/GDPpc_{USt}) \cdot -1$. All specifications control for an ongoing conflict in the previous year and peace years and include decade dummies and a constant term (not shown). 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: NAVCO nonviolent campaigns

| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
|---|---|---|---|---|---|---|---|---|
| Backwardness$_{t-1}$ | 0.083*** | 0.10** | 0.084*** | 0.081*** | 0.073** | 0.100*** | 0.058 | 0.091** |
| | (2.85) | (2.52) | (2.81) | (2.79) | (2.54) | (3.14) | (1.39) | (2.18) |
| Income p.c.$_{t-1}$ | 0.022*** | 0.029** | 0.022*** | 0.021*** | 0.018** | 0.024*** | 0.013 | 0.017 |
| | (2.76) | (2.29) | (2.70) | (2.82) | (2.52) | (3.09) | (1.03) | (1.30) |
| Population$_{t-1}$ | 0.006*** | 0.007*** | 0.007*** | 0.006*** | 0.006*** | 0.008*** | 0.007*** | 0.007*** |
| | (5.96) | (5.73) | (5.92) | (5.96) | (6.22) | (6.17) | (5.02) | (3.30) |
| Polity$_{t-1}$ | -0.001*** | -0.001*** | -0.001*** | -0.001* | -0.001*** | -0.001*** | -0.002*** | -0.002 |
| | (-2.91) | (-2.77) | (-2.92) | (-1.69) | (-2.70) | (-3.47) | (-3.24) | (-1.62) |
| Post Cold War | -0.025** | -0.023** | -0.025** | -0.025** | -0.024** | -0.024** | -0.036** | -0.032** |
| | (-2.42) | (-2.22) | (-2.44) | (-2.41) | (-2.35) | (-2.39) | (-2.37) | (-2.22) |
| Ethnic frac. | | 0.013 | | | | | | 0.015 |
| | | (1.11) | | | | | | (1.29) |
| Growth$_{t-1}$ | | | -0.001 | | | | | 0.005 |
| | | | (-0.081) | | | | | (0.12) |
| Polcomp$_{t-1}$ | | | | -0.0004 | | | | 0.00001 |
| | | | | (-0.37) | | | | (0.006) |
| Socialist | | | | | 0.012 | | | 0.021 |
| | | | | | (1.52) | | | (1.62) |
| Oil rents$_{t-1}$ | | | | | | -0.001 | | -0.0002 |
| | | | | | | (-1.58) | | (-0.29) |
| Inequality$_{t-1}$ | | | | | | | -0.001** | -0.001*** |
| | | | | | | | (-2.18) | (-3.00) |
| **1st st. backwardness** | | | | | | | | |
| Distance | 3.07 | 3.21 | 3.05 | 2.89 | 3.00 | 2.85 | 4.57 | 4.76 |
| Telegram | 2.56 | 1.68 | 2.55 | 2.51 | 2.70 | 2.86 | 0.35 | -0.40 |
| Mailingspeed | -2.09 | -1.68 | -2.07 | -2.18 | -2.29 | -2.36 | -0.71 | -1.45 |
| Excl. instr. F-stat. | 17.4 | 10.4 | 17.1 | 17.4 | 17.4 | 19.1 | 10.3 | 10.5 |
| Partial $R^2$ | 0.21 | 0.18 | 0.21 | 0.20 | 0.22 | 0.22 | 0.19 | 0.18 |
| **1st st. income pc** | | | | | | | | |
| Distance | -1.69 | -2.06 | -1.72 | -1.44 | -1.61 | -1.00 | -2.11 | -2.25 |
| Telegram | -4.69 | -2.77 | -4.64 | -4.65 | -4.83 | -4.92 | -2.27 | -0.90 |
| Mailingspeed | 3.28 | 2.76 | 3.26 | 3.42 | 3.44 | 4.34 | 1.90 | 2.98 |
| Excl. instr. F-stat. | 26.1 | 13.9 | 25.7 | 26.6 | 26.4 | 33.7 | 10 | 9.21 |
| Partial $R^2$ | 0.25 | 0.18 | 0.25 | 0.25 | 0.25 | 0.30 | 0.14 | 0.14 |
| Hansen J stat. $p$-val | 0.82 | 0.70 | 0.82 | 0.81 | 0.73 | 0.70 | 0.19 | 0.22 |
| Observations | 7115 | 6914 | 7073 | 7115 | 7115 | 4968 | 3601 | 3197 |
| Countries | 160 | 154 | 159 | 160 | 160 | 160 | 133 | 130 |

*Notes*: All estimations are pooled 2SLS. The dependent variable is the onset of a nonviolent campaign in the NAVCO dataset. Backwardness is defined as $(GDPpc_{it}/GDPpc_{USt}) \cdot -1$. All specifications control for an ongoing episode in the previous year and peace years and include decade dummies and a constant term (not shown). 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: NAVCO all campaigns

| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
|---|---|---|---|---|---|---|---|---|
| Backwardness$_{t-1}$ | 0.13*** | 0.17*** | 0.13*** | 0.11*** | 0.12*** | 0.13*** | 0.11** | 0.15*** |
| | (2.98) | (2.95) | (3.06) | (2.60) | (2.71) | (2.80) | (2.34) | (2.78) |
| Income p.c.$_{t-1}$ | 0.020* | 0.034** | 0.021* | 0.017 | 0.017 | 0.021* | 0.011 | 0.021 |
| | (1.79) | (2.10) | (1.91) | (1.59) | (1.49) | (1.71) | (0.76) | (1.24) |
| Population$_{t-1}$ | 0.0095*** | 0.011*** | 0.01*** | 0.009*** | 0.009*** | 0.010*** | 0.011*** | 0.011*** |
| | (6.29) | (6.41) | (6.33) | (6.27) | (6.10) | (6.20) | (6.03) | (4.47) |
| Polity$_{t-1}$ | -0.001 | -0.001 | -0.001 | 0.001 | -0.0004 | -0.001** | -0.0014* | 0.001 |
| | (-1.26) | (-1.37) | (-1.41) | (1.43) | (-0.91) | (-2.06) | (-1.86) | (0.58) |
| Post Cold War | -0.035*** | -0.033*** | -0.036*** | -0.034*** | -0.034*** | -0.034*** | -0.038** | -0.033** |
| | (-2.71) | (-2.60) | (-2.72) | (-2.64) | (-2.65) | (-2.65) | (-2.37) | (-2.14) |
| Ethnic frac. | | 0.026* | | | | | | 0.014 |
| | | (1.87) | | | | | | (0.87) |
| Growth$_{t-1}$ | | | -0.066** | | | | | -0.077 |
| | | | (-2.20) | | | | | (-1.41) |
| Polcomp$_{t-1}$ | | | | -0.004** | | | | -0.005** |
| | | | | (-2.19) | | | | (-2.38) |
| Socialist | | | | | 0.008 | | | -0.002 |
| | | | | | (0.73) | | | (-0.13) |
| Oil rents$_{t-1}$ | | | | | | -0.0004 | | -0.0002 |
| | | | | | | (-0.86) | | (-0.23) |
| Inequality$_{t-1}$ | | | | | | | -0.001** | -0.001*** |
| | | | | | | | (-2.56) | (-3.02) |
| **1st st. backwardness** | | | | | | | | |
| Distance | 2.96 | 3.08 | 2.93 | 2.80 | 2.89 | 2.58 | 4.35 | 4.57 |
| Telegram | 2.60 | 1.82 | 2.60 | 2.55 | 2.74 | 2.98 | 0.61 | -0.010 |
| Mailingspeed | -2.14 | -1.77 | -2.13 | -2.23 | -2.35 | -2.52 | -0.77 | -1.61 |
| Excl. instr. F-stat. | 17.6 | 10.9 | 17.4 | 17.6 | 17.6 | 20 | 11 | 11.6 |
| Partial $R^2$ | 0.20 | 0.17 | 0.20 | 0.20 | 0.21 | 0.21 | 0.19 | 0.21 |
| **1st st. income pc** | | | | | | | | |
| Distance | -1.46 | -1.85 | -1.49 | -1.22 | -1.38 | -0.45 | -1.70 | -1.76 |
| Telegram | -4.79 | -2.95 | -4.75 | -4.73 | -4.93 | -5.01 | -2.44 | -1.11 |
| Mailingspeed | 3.37 | 2.86 | 3.35 | 3.50 | 3.53 | 4.56 | 2 | 3.12 |
| Excl. instr. F-stat. | 26.4 | 14.6 | 26 | 26.7 | 26.8 | 35 | 10.7 | 9.75 |
| Partial $R^2$ | 0.24 | 0.18 | 0.24 | 0.24 | 0.25 | 0.30 | 0.14 | 0.30 |
| Hansen J stat. $p$-val | 0.74 | 0.98 | 0.78 | 0.80 | 0.79 | 0.74 | 0.45 | 0.52 |
| Observations | 7115 | 6914 | 7073 | 7115 | 7115 | 4968 | 3601 | 3197 |
| Countries | 160 | 154 | 159 | 160 | 160 | 160 | 133 | 130 |

*Notes*: All estimations are pooled 2SLS. The dependent variable is the onset of any type of campaign in the NAVCO dataset. Backwardness is defined as $(GDPpc_{it}/GDPpc_{USt}) \cdot -1$. All specifications control for an ongoing episode in the previous year and peace years and include decade dummies and a constant term (not shown). 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: UCDP/PRIO armed conflict

| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
|---|---|---|---|---|---|---|---|---|
| Backwardness$_{t-1}$ | 0.12 | 0.16 | 0.12 | 0.12 | 0.14* | 0.12 | 0.043 | 0.068 |
| | (1.50) | (1.63) | (1.49) | (1.50) | (1.65) | (1.44) | (0.53) | (0.71) |
| Income p.c.$_{t-1}$ | 0.0077 | 0.025 | 0.0082 | 0.0074 | 0.015 | 0.014 | -0.014 | -0.0030 |
| | (0.40) | (0.91) | (0.43) | (0.39) | (0.72) | (0.69) | (-0.69) | (-0.11) |
| Population$_{t-1}$ | 0.011*** | 0.012*** | 0.011*** | 0.011*** | 0.012*** | 0.012*** | 0.012** | 0.013* |
| | (3.51) | (3.56) | (3.53) | (3.50) | (3.67) | (3.15) | (2.22) | (1.96) |
| Polity$_{t-1}$ | 0.00074 | 0.00065 | 0.00070 | 0.00096 | 0.00032 | 0.00011 | 0.0013 | 0.0037* |
| | (1.21) | (0.99) | (1.14) | (0.88) | (0.50) | (0.13) | (1.25) | (1.71) |
| Post Cold War | -0.047*** | -0.047*** | -0.048*** | -0.047*** | -0.049*** | -0.047*** | -0.053*** | -0.047** |
| | (-3.15) | (-3.12) | (-3.15) | (-3.14) | (-3.21) | (-3.12) | (-2.85) | (-2.55) |
| Ethnic frac. | | 0.033* | | | | | | 0.014 |
| | | (1.76) | | | | | | (0.68) |
| Growth$_{t-1}$ | | | -0.031 | | | | | 0.020 |
| | | | (-0.80) | | | | | (0.38) |
| Polcomp$_{t-1}$ | | | | -0.00048 | | | | -0.0076** |
| | | | | (-0.23) | | | | (-2.38) |
| Socialist | | | | | -0.024** | | | -0.054*** |
| | | | | | (-2.39) | | | (-3.03) |
| Oil rents$_{t-1}$ | | | | | | 0.00022 | | 0.00062 |
| | | | | | | (0.27) | | (0.70) |
| Inequality$_{t-1}$ | | | | | | | -0.00037 | -0.00071 |
| | | | | | | | (-0.79) | (-1.38) |
| **1st st. backwardness** | | | | | | | | |
| Distance | 3.15 | 3.23 | 3.13 | 3.02 | 3.08 | 2.94 | 4.48 | 4.70 |
| Telegram | 2.26 | 1.56 | 2.26 | 2.24 | 2.41 | 2.36 | 0.080 | -0.62 |
| Mailingspeed | -1.74 | -1.42 | -1.74 | -1.82 | -2.02 | -2.08 | -0.70 | -1.48 |
| Excl. instr. F-stat. | 16.2 | 10.3 | 16 | 16.2 | 16.9 | 17.3 | 9.15 | 9.69 |
| Partial $R^2$ | 0.20 | 0.18 | 0.20 | 0.20 | 0.21 | 0.21 | 0.19 | 0.19 |
| **1st st. income pc** | | | | | | | | |
| Distance | -1.83 | -2.11 | -1.87 | -1.65 | -1.73 | -1.16 | -2.28 | -2.46 |
| Telegram | -4.14 | -2.63 | -4.11 | -4.12 | -4.30 | -4.14 | -1.96 | -0.70 |
| Mailingspeed | 2.91 | 2.48 | 2.91 | 3.01 | 3.17 | 3.90 | 1.85 | 2.92 |
| Excl. instr. F-stat. | 24.9 | 14.2 | 24.7 | 25.4 | 26.1 | 30.5 | 9.51 | 9.03 |
| Partial $R^2$ | 0.23 | 0.17 | 0.23 | 0.23 | 0.24 | 0.28 | 0.14 | 0.15 |
| Hansen J stat. $p$-val | 0.78 | 0.43 | 0.73 | 0.78 | 0.89 | 0.34 | 0.080 | 0.090 |
| Observations | 7745 | 7524 | 7707 | 7745 | 7740 | 5645 | 4088 | 3674 |
| Countries | 162 | 154 | 161 | 162 | 161 | 162 | 136 | 131 |

*Notes*: All estimations are pooled 2SLS. The dependent variable is the onset of an armed conflict in the UCDP/PRIO dataset. Backwardness is defined as $(GDPpc_{it}/GDPpc_{USt}) \cdot -1$. All specifications control for an ongoing conflict in the previous year and peace years and include decade dummies and a constant term (not shown). 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. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$