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Covid-19 and Internal Conflict: Does income inequality matter?

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Abstract

Pandemics can cause internal conflict and threatened political stability. This article examines if initial income inequality has moderated the effect of Covid-19 on internal conflict for a sample of up to 100 countries during 2020 and 2021. Regression analyses provide evidence that countries with initially higher inequality experienced an increased marginal conflict effect of per capita Covid deaths. The moderation effect shows an 0.011 percentage point higher inter conflict risk change and 3.6% more conflict events for a one-unit higher initial Gini Index. I argue that grievances and relative deprivation were more prevalent in societies with higher initial inequality and the socioeconomic burden of Covid-19 found more fertile ground to nurture conflict. Inequality may have lowered state and societal capacity to effectively mitigate adverse effects, as inequality can predict lower interpersonal and institutional trust. I do not find evidence that Covid-19 unequivocally increased internal conflict. The effect only occurs when initial income inequality is above the 75th percentile, Gini Index of 43. Further, inequality appears to moderate the effect for conflict in direct *and* indirect relation to the pandemic. The effect is robust for different variable measurements, samples, and endogeneity.

1. Introduction

In early 2020, the world looked down the barrel of what shaped up to be the biggest challenge to public health since the Spanish Flue Pandemic of the late 1910's. The novel Coronavirus variant SARS-CoV2 that causes the respiratory disease Covid-19 emerged in Mainland-China and quickly spread from a localized outbreak to a worldwide pandemic. While the obvious damages to human lives, human livelihoods and societal mental health were both immediately in the focus of public and academic discourse, historians and social scientists began pointing out another potentially devastating side effect of the pandemic: *civil disorder, violent conflict and political instability* (Censolo and Morelli, 2020). Public discontent fueled by social isolation and opposition to restrictions aimed at combating the spread of the disease, enormous costs to health and economic security, and social grievances aggravated by the pandemic have historically made pandemics and epidemics "incubators of conflict" (Cenoslo and Morelli (2020). The immediate post-Covid world showed eerie similarities to the predictions. The United States were shock by large waves of civil unrest, South America experienced an intense comeback of anti-government protests, numerous African governments turned violence against its own citizens amidst the health emergency, militants in India and Mali used the pandemics'

vacuum to escalate their campaigns, and in Europe, Covid-conspiracy movements have led to intense strains on civil and public order (ACLED, 2021a). However, as conflict data later on will show, there appears to be great between-country differences in conflict during the pandemic. With this study, I aim to contribute to the ongoing investigation, whether and what role Covid played for such coinciding conflict and conflict patterns.

Covid-19 emerged in a world which appeared to be on track towards a serious break in history anyway. Climate Change threatens to negate decades of economic and environmental development, civil wars increased sixfold in the 2010's (von Einsiedel 2017), and the rising tide of authoritarianism caused waves of democratic backsliding (Freedom House, 2021). Systems and institutions are, rightfully, critically challenged in the face of these issues. Particularly the role of hyper-globalized capitalism has been increasingly under scrutiny. Hailed as the bringer of prosperity and peace by the Washington Consensus in the 1990's, those promises have been in doubt. One of the most crucial issues that coincide with the emergence of Covid-19 is the phenomenon of rising within country income and wealth inequality between individuals, or "vertical inequality". In 2020, it reached levels akin to late 19th century industrialization (World Inequality Lab, 2022). Since Thomas Piketty (2014) popularized the issue beyond academic journals, scholars have searched for causes and consequences of economic inequality (Dabla-Norris et al., 2015). While it has been linked to slower economic and human development, and erosions of interpersonal and institutional trust, most relevant for this study is the hypothesized link between vertical income inequality and conflict. The idea is not novel. Related quotes go back as far as Plutarch in the first century AD. Early related literature (see Østby, 2013 for a review) argued based on a number of differing theoretical approaches that inequality induces grievances among the less affluent members of societies, which will ultimately and inevitably result in violent conflict. This general framework is expanded by empirical research, which shows the adverse influence of income inequality on a number of specific variables that can increase the likelihood of violent conflict and decrease the capacity of formal and informal institutions to solve conflicts efficiently.

From these observations arises the question if the phenomena are linked. Did a priori higher income inequality contribute to the emergence of post-pandemic conflict? And crucially, can distributional policies alleviate conflict going forth? Theoretical backing provides the history of pandemics. First, they come with adverse economic and societal consequences: they induce and deepen grievances (Censolo and Morelli, 2020). They strain the pockets of individuals alike governments and test the fabric of social contracts. Countries with higher initial inequality enter

pandemics with already higher levels of grievances and inter-societal tensions, i.e., higher conflict potential. During the crisis, the necessary threshold of additional strain to trigger conflict may thus be lower. Second, economic and political institutions in unequal countries are unlikely to suddenly distribute social costs, and hence grievances, equally (id.; Nickens, 2020). When systems that perpetuate inequality continue to do so during a pandemic, the social groups that already carry grief and anger will be particularly strained. Unequal societies destabilize more and earlier, as relatively more, relatively aggravated people are overproportionally burdened. Third, the trigger of conflict itself, income inequality, may increase during and after pandemics (Furceri et al., 2020, p.151-152). If state institutions are unable to effectively remedy an unequal distribution of the pandemics' costs, conflict may simply increase by virtue of the conflict potential inherent in income inequality. Again, unequal countries may be particularly at risk. Iacoella et al. (2021, p.4) argue that areas with high initial inequality may experience a "perfect storm" during Covid-19. The economic costs of the health crisis amplify deeper existing grievances and push societies towards conflict. Hereon, this article asks the following questions: Can the level of Covid-19 affectedness lead to higher conflict prevalence and explain cross-country differences in conflict during the pandemic? Furthermore, are differences in initial income inequality shaping the degree to the pandemic induced conflict?

This article aims not to contribute to the still unsettled scholarly debate on inequality's direct effect on conflict. Rather, it should be viewed narrowly as a context specific and problem-oriented piece of research: It aims to investigate the impact of Covid-19 on conflict and particularly the importance of income inequality as a moderator. Therefore, the study attempts to advance the literature of Covid-19's effect on conflict by investigating possible distributional dimensions. The topic already receives much attention in scholarly debate and public discourse. Multiple disciplines address the issue from different starting points and in diverse geographical scope. Methods are equally diverse, ranging from qualitative and descriptive analysis to econometric modelling. Distinct facets of conflict have been addressed: Interstate war (Bapat, 2020), social stability within countries (Censolo and Morelli, 2020), nationalistic and ethnic conflict (Woods et al., 2020), authoritarian attitudes (Filsinger and Freitag, 2022) demonstration patterns (Iacoella et al. 2021; Lackner et al. 2021; Plümper et al. 2021; van der Zwet et al. 2021) and intrastate conflict (Farzanegan and Gholipour, 2021). Studies' build on exisiting work exploring the connection between previous pandemics and conflict (Gonzalez-Torres and Esposito, 2017; Sedik and Xu, 2020; Barret and Chen, 2020; Cervellati et al., 2022). Note that I use the generic term pandemic to describe both pandemics and epidemics.

To my knowledge, testing if initial income inequality moderates the marginal conflict effect of Covid-19 and explains global cross-country differences in conflict during pandemic, is novel. Herein, I hope to contribute to econometric studies that examine the relation between pandemics and conflict explicitly on a global level (Berret and Chen, 2020; Farzanegan and Ghoulipour, 2021; Mehrl and Thurner, 2020; Sedik and Xu, 2020;). Data availability now allows to analyze the global effect of Covid on conflict for the entire first 24 months of the pandemic – which expands on previous work. More specifically, I aim to contribute specifically to the literature that investigates the income distribution as a moderator. Differing from the one-country setting of Iacoella et al. (2021) and expanding the analysis of past pandemics (Sedik and Xu, 2020) to the Covid-19 pandemic allows for novel insight. Data availability now allows to analyze the global effect of Covid on conflict for the entire first 24 months of the pandemic – which expands on previous work. While the distributional dimension of pandemic conflict is theoretically well appraised (Censolo and Morelli, 2020), global empirical investigations are still lacking. If we can establish broader validity that inequality shapes the conflict effect of pandemics, we can refine our theoretical understanding when and where pandemics become serious security risks.

And indeed, empirical results suggest that the level of Covid-19 affectedness, measured by deaths per capita, had globally on average a larger increasing effect on internal conflict in countries with higher initial inequality. Furthermore, I do not find robust evidence that the level of Covid-19 deaths increased internal conflict at all observed levels of inequality. Only in countries with inequality after the 75th percentile appears Covid to have significantly increase conflict. While this article is only a first attempt, analyzing factors that explain the differences of conflict between countries during the acute phase of the pandemic, such as initial inequality, can be crucial to understand where and how conflict may be emerging, resuming or consolidating going forward. Evidence of a distributional moderation effect implies that future polices in unequal countries should have a consider redistributive effects, especially as results suggest that low inequality countries were able to politically stabilize during the past two years.

The remainder of the article is structured as follows: Section 2 provides theoretical background on the relationship between conflict, pandemics and inequality and aims to apply it to Covid-19. Empirical research is appraised as well. Section 3 outlines the empirical approach, data selection and discusses potential shortcomings. Sections 4 and 5 presents principal econometric results and qualitative elaboration with country case studies. Section 6 discusses endogeneity issues. Sections 7, 8 and 9 expand the empirical investigation and provide robustness tests. Section 10 puts' results into perspective and concludes the article.

2. Review of theoretical and empirical literature

The theoretical background of this study draws on multiple fields of research. Central are two relationships: between pandemics and conflict and inequality and conflict. Presented arguments will draw on present research in relation to Covid-19 and connect broad concepts to the applied empirical case. Previous empirical results will be discussed to give a backdrop for the articles' findings. The goal of this section is to flash out the theoretical links which connect epidemics to conflict, and that initial income inequality can be and important moderator of this effect.

2.1. The relationship of pandemics and conflict

Pandemics are surprisingly regular events. For instance, the 21st century alone saw already five major global disease outbreaks SARS, H1N1, MERS, Ebola and Zika (Sedik and Xu, 2020, p.6). Therefore, we have plenty of historical evidence to analyze the consequences of rapid disease spread with regards to conflict. And the historical evidence suggests that social unrest and political instability are likely accompanying ills (Censolo and Morelli, 2020). In a comparative historical study of 57 past major epidemics, the authors note that in the long run, a robust pattern of social unrest develops during most cases. Also aggregating across five Cholera epidemics, they find that the likelihood of armed rebellion has doubled after the end of the epidemic. However, at a second look, the relationship is more complex. Let's first examine factors which imply that pandemics increase conflict. Specifically affected appears to be intrastate conflict, conflict that occurs between state and/or non-state actors located in the same sovereign country, which is subsequently the relevant unit of analysis. I include even peaceful protests in the analysis. While these may not come across as conflict, mass movements with common goals can be predictors of violent conflict. Consider for instance events after the Arab Spring in Libya, from mass protests to coup d'état, to civil war (Encyclopedia Britannica, 2022).

2.1.1. Epidemics can increase conflict

Epidemics appear to affect three general spheres by which they facilitate conflict: the relationship between state actors, non-state actors and society, the individual as well as collective psyche, and socioeconomic wellbeing (Censolo and Morelli, 2020). The spheres are interrelated in their effect on conflict and can reinforce each other. Disease outbreaks require state intervention to curtail the spread, such as movement restrictions, bans on gatherings, and a shutdown of the economy. Interventions deeply disrupt individuals' lives and livelihoods, which can increase animosity against the state. Additionally, pandemics are stress tests for institutions and public trust. They will reveal the degree of competency and preparedness of

political and economic institutions. If they fail the stress test, for instance if political leadership is not able to react quickly enough or social safety nets prove insufficient, public discontent and distrust can increase and thus the likelihood of conflict (Barret and Chen, 2020, p.3). For instance, the Unites States experienced protests movements calling on the government to start supporting rent payments of lower incomes (Akbar, 2020) and to increase labor protection regulations for frontline workers (Widdimcombe, 2021). Similar movements have been recorded around the globe (ACLED, 2021a, p.4). Commentators also posed that citizens are more likely to turn to violence during pandemics, as non-violent avenues of political participation, protests and elections, have been considerably less available in the pandemic (Murillo, 2020). Thus, discontent may simmer and unload in violence. And while public health emergencies often call for decisive government action, authoritarian and even populist governments have often exploited crisis to expand and entrench their power vis-à-vis the population, possibly resulting in higher levels of violence against the population (Censolo and Morelli, 2020). During Covid-19, South-Asian regimes cracked down on critics and the media (ACLED, 2021a, p.10) and African States violently enforced lockdowns in March 2020 (id., p.7). Some authors moreover fear that the rerouting of government resources in response to the health emergency, particularly of security forces, can leave power vacuums that armed non-state actors can exploit (Polo, 2020, p.6-9). Polo (2020, p.8) provides descriptive evidence from Afghanistan, where the Taliban used the pandemic to step up attacks and provide an alternative public health response, challenging political stability and legitimacy along multiple dimensions.

In relation, pandemics and the subsequent state interventions are incisive shocks to the individual and collective psyche. Social connections are upended and worries about livelihoods and can increase intrahousehold tensions and conflict. Pandemics induce fear, depression, anger and despair (Mucci et al., 2020; Torales et al., 2020), which can increase individuals' propensity for violence and a more frequent search for violent outlets (Censolo and Morelli, 2020; Killgore et al., 2021;). Moreover, individual feelings can shape collective beliefs. Eidelson (2013, p.219) synthesizes that the collective beliefs of superiority, injustice, vulnerability, distrust and helplessness are steering groups towards conflict. The psychological effects of pandemics can stoke such believes. This finds reflection in the historical observation that pandemics induce fear of "others" (Censolo and Morelli, 2020). People tend to look for scapegoats, responsible for the spread of the disease, which can lead to discrimination, and subsequently violence, against social or racial groups. This tendency held true during Covid-19 as people of Asian descent were victim to hate crimes, purely on the basis that SARS-CoV2 originated in China

(Gover et al., 2020, p.653-659). During Covid-19, the ACLED (2021a, p.10) also observed a surge in violence against healthcare workers, which were seen as part of the system that perpetuated the perceived injustices of the response to the virus. Connectedly, Covid-19 has been accompanied by a pandemic of conspiracy theories surrounding the origin, severeness and actual existence of the virus (Douglas, 2021, p.270). Under the psychological burden of the pandemic, some turned to conspiracy theories to make sense of the situation (id.). Moreover, people followed partisan and ideological beliefs and adopted the conspiratorial inclinations of leaders (Uscinski, 2020, p.6). The effects on conflict can be enormous. Bhatti et al. (2021, p.3) show that violence against healthcare workers is often motivated by conspiracy believes. Similarly, the ACLED (2021b, p.1) monitored anti-restriction protests in the US, often containing groups believing in Covid conspiracies, such as right-wing militias. 55% of protests turned violent when such groups were present, compared to 4% when they were not (id.).

Lastly, pandemics and government interventions have massive social costs, impacting everything from economic welfare to individuals' health. Economic structures are disrupted, and the global economy recedes. Covid-19, for instance, caused the worst recession since the great depression of 1929 (Zumbrun, 2020). Morelli et al. (2020) show that such a decrease in the divisible surplus of countries can increase the risk of violent conflict, as people vie for power over the lower resource base. Crucially, these struggles seldomly affect groups similarly, as existing systems can distribute social costs of pandemics unequally (Censolo and Morelli, 2020). Pre-pandemic inequalities are likely exacerbated. Thus, the authors refer to epidemics as "social incubators". Pandemics provide fertile ground in which societal inequalities and tensions grow. For instance, Wade (2020) points out that the bubonic plague in the 14th century nurtured class tensions between peasants and feudal lords, erupting in widespread revolt decades later. While the such events may or may not occur today, we can already find similar potential seeds for conflict after the Covid-19 outbreak, which derive from unequal social costs. Virus exposure appears to be higher among lower incomes with less opportunities to work from home or suspend work until conditions are more secure and who live in areas where self-isolation is less feasible (Brown and Ravallion, 2020, p.27; Jung et al., 2020, p.319; Pires et al., 2021, p.37-43). Poorer populations have more comorbidities, worse access to healthcare and are more likely to die from Covid (id, p.44-46; Reeves and Rothwell, 2020). Economically vulnerable populations also bear the brunt of the economic fallout, as they are more likely to lose their job and have a priori less safety nets to fall on (Mongey et al., 2020, p.17). Apart from increasing cleavages between income groups, pandemics can also amplify economic and social

grievances between racial and ethnic groups. Brown and Ravallion (2020, p.24) find evidence that infections are higher among Black-Americans. Similar evidence exists for marginalized indigenous groups in South America, for instance Brazil (Pires et al., 2021, p.39). Moreover, occupations that lost relatively more jobs are overproportionally filled by minorities in the United States, yet they were relatively underrepresented in re-hirings as the economy reopened (Montenovo et al., 2020, p. 16-18). The distribution of social costs appears to follow existing cleavage and act as an amplifier that reinvigorates movements aiming to fight inequalities. Commentators argued that protest movements in direct relation to Covid-19, such as "cancel the rents" or labor protests, are simply picking up prior grievances (Akbar, 2020; Widdimcombe, 2021). The same is true for existing racial justice movements, which began criticizing the unequal pandemic burden of minorities (ACLED, 2020a, p.17).

The unequally distributed economic burden may also influence armed conflict. Mehrl and Thurner (2020) argue that the economic crisis following the pandemic can increase recruitment into rebel groups. Opportunities for earning a legal livelihood are shrinking and opportunity costs for challenging the incumbent regime which proves unfit to provide for its citizens, decline. Moreover, the option to earn income illegally in the process can facilitate recruitment. Research during Covid-19 suggests that people who experienced pandemic related economic hardship were more likely to participate in protests and become politically active (ACLED, 2021a, p.5). Empirically, Lackner et al. (2021, p.4-9) establish this relationship for the United States. More exposure to Covid-19 related unemployment and death led to negative emotional stress which in turn showed to be an important predictor of social unrest.

The unequally distributed social costs together with psychological stressors and strains on the relationship between state and citizens suggest that pandemics increase conflict. If grievances persist past the pandemics acute phase, countries may face even more instability in the decades after the outbreak ends. Yet, the full picture is less unambiguous, at least in the short-term.

2.1.2. Epidemics can decrease conflict (in the short-term)

Censolo and Morelli (2020) point out that the short-term after an outbreak is dominated by conflict in direct relation to the pandemic. It crowds out conflict in relation to other issues. However, immediately after and during the outbreak, all conflict, especially protests, largely stops. The ACLED (2021a, p.2) observed that for instance the Chilean labor movement and the Hong Kong democracy movement nearly ceased their activity completely in March 2020. Bloem and Salemi (2020, p.8) and Metternich (2020) confirm a massive global short-term

decline in protests and violent demonstrations. Chiefly responsible are fear of contagion and movement restrictions (Censolo and Morelli, 2020). Further, the pandemic disrupted necessary logistics, such as transportation networks (Barret and Chen, 2021, p.4). The final effect of pandemics on conflict in the short-term partially depends on the degree to which unrelated conflict is crowded out and directly related conflict emerges. Moreover, conflict deterrence based on increased government activity and monitoring immediately after the outbreak must be considered (Censolo and Morelli, 2020). Also, governments appear to have some leverage to influence public opinion, and therefore conflict, in relation to the pandemic. Farzanegan and Gholipour (2021, p.15-20) find evidence that governments which provided little monetary Covid-19 relief faced an increased risk of internal conflict. An efficient support response can lessen economic burdens, alleviate psychological stress and reinforce trust in the government. Response tailored towards relatively disadvantaged groups can address grievances efficiently.

Additonally, political scientists have long described the "rally-around-the-flag" effect (Baum, 264-265; Mueller, 1970, p.21). Originally related to foreign policy events, the effect describes the phenomenon that incumbent governments enjoy higher approval ratings immediately after a significant event. People unite behind common causes, put partisanship aside and support the institutional response. Subsequently, the propensity for conflict can decline. Woods et al. (2020, p.819) have explicitly stated that the effect could arise after the Covid-19 outbreak. And indeed, the effect is observed in countries such as Germany (Tagesspiegel, 2020) and Denmark (Nielsen and Lindvall (2021, p.1188-1192). Moreover, international institutions called for cease fires at the start of the pandemic to unite resources behind the humanitarian effort required by Covid-19 (Mehrl and Thurner, 2020). There is some evidence of receding military activity in at least four countries early in the pandemic (Ide, 2021). The ambiguity of theoretical arguments can be unsatisfying regarding our understanding of pandemics and conflict. Historical and descriptive analysis provide starting points, but more rigorous empirical methods are needed to establish a sound relationship. Hence, I turn to empirical studies next to complement the picture.

2.1.3. Empirical evidence on the effect of epidemics on conflict

Past pandemics provide a good bit of evidence in favor of a conflict increasing effect. Sedik and Xu (2021) investigate the H1N1, SARS, MERS, Ebola and Zika outbreaks of the 21st century. In a panel Vector Autoregression Model, the authors provide global evidence for up to 133 countries that civil disorder increases about ¼ of a standard deviation about two years after the outbreak. Notably, the positive effect is only significant at the 10% level more than a year

after the outbreak. Similar evidence is found by Barret and Chen (2020). They use cross-section and panel regressions for 130 countries to decompose temporal effects of major epidemics between 1990 and 2019 on social unrest. In the long-term, 29 years, epidemics are associated with a significantly higher likelihood of social unrest. For timeframes up to 2 years post-outbreak, the authors find robust evidence that epidemics *decreased* the likelihood of social unrest. Similarly ambiguous results for the short-term exist for Covid-19. Metternich (2020) provides global evidence that protests significantly declined in the early stages of the pandemic. Mehrl and Thurner (2020) use a difference-in-difference approach to establish a causal effect of Covid-19 outbreaks on armed conflict. The authors find no global average effect of Covid-19. Conflict decreased in Southeast Asia and Europe, while it increased in the Middle East.

Yet, there is also evidence which suggests that pandemics increase conflict, no matter the timeframe. This is true for past epidemics as well as Covid-19. Gonzalez-Torres and Esposito (2017), using a difference-in-difference model, conclude that the Ebola epidemic in West Africa in 2014 significantly increased intra-state conflict already 8 months after the first case. Similar evidence is found by Cervellati et al. (2022) regarding Malaria outbreaks in Africa, using equivalent methods. For Covid-19, Farzanegan and Gholipour (2021) provide evidence that higher Covid-19 case fatality rates increased internal conflict risk in 2020 for 102 countries.

The empirical literature does not clarify a lot. Pandemics often do not appear to affect conflict in the short-term, and sometimes even increase political stability. Yet, other scenarios show strong increases in conflict immediately after the outbreak. Furthermore, heterogeneity across regions is often present. While I strongly suggest that confusion is partially driven by differences in initial economic conditions, there is at least one other caveat. Østby (2013, p.225) points out that the dependent variable, or measurement of conflict, has caused confusion in studies related to inequality and conflict. I argue that the same issues prevail here. Compare for instance the results of Metternich (2020) and Mehrl and Thurner (2020). Protests may be much more responsive to pandemics compared to armed conflict. Pandemics may thus lead to very different conclusions regarding conflict patterns. Conflict encompasses different actions, by different actors, with different opportunity costs and readiness to exploit national crisis. Hence, we need careful consideration comparing studies, and generalized claims have to be justified. Yet, the evidence still suggests ambivalent short-term effects, even after accounting for measurements differences. Comparable measures of social unrest, which are crucially aggregating different conflict types indicate different short-term effects of pandemics (Barret and Chen, 2020; Sedik and Xu, 2021). In the long-term however, epidemics appear to

unequivocally drive conflict. My study is constraint to the first 24 months of the Covid-19 pandemic, or "the short-term". I derive the following hypothesis:

H1: Higher levels of Covid-19 affectedness, measured by deaths per capita, had no effect on internal conflict during the first 24 months of the pandemic.

Moreover, the timeframe allows us to investigate the more interesting and contested part of the relationship. Censolo and Morelli (2020) already assess that the final effect of pandemics on conflict can be expected to differ across settings: it is conditional on initial socioeconomic factors. The factor I pose as central, is initial income inequality. In the next section, I will briefly outline how inequality can shape conflict.

2.2. The relationship of income inequality and conflict

The concept of income inequality is will be referring to is within country vertical income inequality, or "inter-individual inequality" (Østby, 2013, p.206). That is, we measure and compare the differences in incomes between individuals: we stack them up on vertical scales based solely on their income. It is the most commonly known and applied inequality concept. There are two mechanisms that can link it to increased conflict.

2.2.1. The direct link – grievance theories

Scholars have long suggested that conflict is not simply a product of absolute income levels, otherwise developed nations should be perfectly conflict free. Instead, the distribution of income matters for conflict (Nagel, 1974, p.453). In principle, any theory that postulates a direct link between income inequality and intrastate conflict makes the following assertion: lower incomes experience grievances based on the differences between a desired state and an actualized state (Østby, 2013, p.208-211). That is, grievance theories focus on relative outcomes, or subjective comparisons (Nagel, 1974, p.454). There are a number of historical variants of this concept (see Østby, 2013, pp.208-213). Karl Marx conceptualized inequality as class struggle between exploited working class and rich industrialists. Differences in and injustice of outcomes leads workers to revolt. Sigmund Freud defined the underlying psychological mechanisms as the frustration-aggression principle. The most prominent concept however are relative deprivation theories, which expand the general principles. In its earliest iteration, Davies (1962, p.5-6) defines relative deprivation as the difference in expected (economic) need satisfaction and realized need satisfaction. Conflict is expected to occur during phases where the two diverge drastically for large parts of the population. In Davies' eyes,

phases of prolonged economic development install an expectation in people that they will be able to reap the fruits for their labor when development levels off. A divergence of expected and actual need satisfaction is perceived as a robbery of fair shares of wealth earned with past efforts. A similar approach is developed by Gurr (1970), who conceptualizes relative deprivation as the difference between what people want to obtain and what they think they can obtain. The greater the difference, the greater the grievances and potential for conflict. Østby (2013, p.209) points out that these theories are not explicitly focusing on the comparison and inequality between individuals' income as the origin of conflict, but rather an inequality of individually expected and obtained income. The notion of relative deprivation that is commonly cited as the reason for conflict in empirical studies is "synchronic relative deprivation" (Østby, 2013, p.209). The basic tenet is that people compare their economic situation with a reference group, for instance with fellow citizens. When their income compares unfavorably to the reference group, they feel frustration and anger, and are more likely to mobilize and ultimately engage in conflict. Crucially, to feel grievances, people need to perceive better outcomes as attainable, that is, they need to perceive that they are in some form held back from achieving better outcomes. Income inequality can thus by itself be linked to conflict. More recent literature has pointed out how inequality induces specific unfavorable socioeconomic outcomes for the relatively disadvantaged, adding to grievances and conflict.

2.2.2. The indirect link – inequality shapes specific grievances

The literature on consequences of income inequality is broad. I focus on consequences that I perceive to be most relevant for linking inequality to conflict. I will be brief and not extensive in detailing individual mechanics. Important are the implications for conflict.

One factor shaped by inequality are opportunity costs to engage in conflict, which can be thought of as the forgone income and security of individuals once they make the decision to participate in conflict (Freytag et al., 2011, p.6). Opportunity costs shape the incentive structures to engage in conflict or maintain the status quo. In this vein, we think about conflict as having intrinsic possible rewards. Protests may lead to favorable political changes, while more violent and assertive forms of conflict can force redistribution, and participants may derive positive psychological stimuli. Freytag et al. (2011, p.12) demonstrate that lower income and consumption per capita increases terrorism, by decreasing opportunity costs and incentivizing toppling the status quo. There is a natural relation to income inequality. Pickett and Wilkinson (2015, p.1769-1774) show that inequality leads to adverse health outcomes.

Worse health can in turn be a predictor of lower income, as people are unable to exert their full productivity and earning potential. Thus, inequality, through its effect on health, lowers conflict opportunity costs and induces grievances that make participation in conflict more likely. Thorbecke and Charumilind (2002, p.1487-1489) provide an overview of the evidence that inequality in incomes predicts lower educational attainment and human capital. If we assume that human capital is one of the most important predictors of higher present and future income, inequality again increases the aversion of people for the status quo, hence lowering opportunity costs of conflict. Elgar and Aitken (2010) show that income inequality can lower interpersonal trust, which in turn lowers thresholds to engage in crime. If we extrapolate, it is evident that this effect also matters for internal conflict. The examples demonstrate that inequality can alter incentive structures of individuals so that conflict is perceived as viable remedy for grievances.

Furthermore, inequality has been identified as a predictor of lower trust in public institutions (Bergbauer et al., 2022). People perceive institutions as incapable to alleviate relative deprivation and grievances and are more likely to revolt against them. Indeed, this may be part of the channels that explain the result by MacCulloch (2005, p.93) where inequality is a significant predictor of revolutionary preferences. Additionally, factors such as nationalism (Solt, 2008, p.14-18) and political polarization (Gu and Wang, 2021, p.10-17) appear to positively correlate with income inequality. Nationalisms has been discussed as a cause of inter-group conflict within countries (Woods et al., 2020, p.808-809). Political polarization has been discussed along similarly (Esteban and Schneider, 2008, p.134-136). By eroding institutional trust and compromising peaceful political discourse, income inequality can perpetuate an environment where conflict is perceived as the modus operandi of political engagement.

While the theoretical relationship between vertical inequality and conflict is intuitive, the proposition is empirically unsettled. Østby (2013) presents an overview of this qualitative-quantitative puzzle. Explanations vary. Some argue the capability and opportunity to engage in conflict is more important (Collier and Hoeffler, 2004; Fearon and Laitin, 2003). Stewart (2011) argues that between-group inequality is the relevant concept that links inequality to conflict. Others believe that the relationship is specific to certain timeframes and contexts (id., p.211). There is much more to be said about the puzzle, but what I want to stress is that we cannot expect inequality to be a clear predictor of conflict across scenarios. Yet, I will argue next why I think there are straightforward reasons to believe that income inequality can increase the conflict effect of pandemics and shaped cross-country differences during Covid-19.

H2: Initial income inequality has not influenced internal conflict during 2020/2021.

2.3. Inequality's moderating effect of the pandemic-conflict relationship

Firstly, there is a simple mechanic effect. There is evidence that past pandemics exacerbated income inequality (Furceri et al., 2020, p.139-141) and that Covid-19 is not an exception (IMF, 2022, p.157). Relative deprivation and grievances increase. According to theoretical considerations, populations with higher levels of income inequality will harbor relatively deeper grievances, and relatively larger parts of the populations will be affected (Krieger and Meierrieks, 2016, p.5). Thus, the level of additional grievances necessary to reach a threshold after which conflict erupts may be lower in more unequal countries. Additional to increasing vertical inequality, there is evidence that existing horizontal, or between-group, inequality is also increasing (Brown and Ravallion (2020, p.24; Montenovo et al., 2020, p. 16-18; Pires et al., 2021, p.39, 50-53). Stewart (2011) argues that horizontal inequality along socioeconomic dimension between defined groups is an important link to conflict as well.

Additionally to this direct link, we can again identify how specific consequences of inequality fuel grievances and conflict during the pandemic. More unequal countries may have worse population health, e.g., more people with health conditions that amplify Covid's mortality. Thus, the pandemic will exacerbate health inequalities caused partially by income inequality. Health related inequalities have in fact already led to conflict via protest movements that critiqued the relatively higher burden on minorities in the United States (ACLED, 2020, p.17). The World Bank (2020, p.29-30) points out that losses in human capital during Covid-19 are more pronounced among low-income groups. They have less resources for remote learning and insecure health and nutritional conditions, which are physical determinants of human capital. In more unequal countries, frustration around the distribution of human capital may already be higher, increasing the willingness of people to engage in conflict over it. Moreover, through the channels of higher economic and physical vulnerability, inequality may also increase the population level of psychological stress during pandemics which likely increases conflict.

Furthermore important during pandemics may be the consequences of inequality on public trust in authorities and institutions. Devine et al. (2020) synthesize the early literature and conclude that higher levels of public trust enable more efficient and frictionless implementations of measures such as lockdowns (id., p.282). While that in and of itself can already predict lower conflict, higher efficiency of policies can decrease the number of Covid-19 cases that countries face, attenuating its overall burden and particularly the impact on more affected lower incomes.

Lowering the burden on lower incomes, can reduce the pandemics adverse effects on income inequality. Thus, a more efficient response can be linked to lower conflict propensity. Additionally, rally-around-the-flag effects, solidarity and voluntarily compliance with countermeasures also likely depend on interpersonal trust. Connecting this to the adverse effects of inequality on trust (Elgar and Aitken, 2010), there is reason to believe that countries with higher initial inequality will face more frictions and conflict during a pandemic. Potentially higher political polarization in more unequal countries may contribute to a divisive and conflict laden response to the disease as well (Cornelson and Miloucheva, 2020, p.8-9).

The evidence suggests that political and economic systems which lead to less distributional equality likely continue to do so during a pandemic. Thus, pandemics particularly amplify existing grievances in more unequal countries, as less a priori institutional safeguards are in place to avoid increasing inequality. Censolo and Morelli (2020) argue that this distribution of social costs crucially determines the level of conflict after an epidemic. We have reviewed some of the possible channels above and there is sparse empirical evidence that supports the claim. Sedik and Xu (2020, p.9-11) conclude that during previous pandemics, social unrest increased as a function of increasing income inequality. Iacoella et al. (2021, p.11-12) find that the likelihood of US counties to experience protests in relation to Covid-19 policies increased in the level of initial income inequality. Thus, I derive the following hypothesis:

H3: Initially higher income inequality increased the marginal effect of pandemic affectedness on internal conflict during 2020/2021 vis-a-vis similarly affectedness but less inequality.

3. Data and Methodology

Section 3 will lay out data sources and variable specifications and present the empirical strategy. Summary statistics can be found in the Appendix.

3.1. Dependent variable choice - measuring internal conflict

I choose two different sets of dependent variables to measure conflict patterns during the first 24 months of the pandemic. Firstly, a note on the definition of conflict in this study is in order. Conflict always refers to intrastate conflict and includes all events that occur within the borders of a nation state between state and/or non-state actors. The first variable I use is the internal conflict risk index from the Political Risk Service (PRS, 2022). It is widely used in the literature (Farzanegan and Gholipour, 2021; Sedik and Xu, 2020) The index measures the level of political violence within a country and the ensuing risks for governance and foreign

investments. The score is aggregated from three subcomponents: *the extent of civil war, the level of terrorism, and the prevalence of civil disorder,* which includes violent demonstration and strikes, criminal activity and extensive civil disobedience. Assessed are acts perpetrated by forces opposing the state, and acts perpetrated by the state against its people. Originally, the index ranges from 0 to 12. Lower scores indicate higher risks of internal conflict, higher scores indicate lower risks. In order to interpret the variable as a measurement of conflict rather than stability, the index is rescaled. Higher scores represent higher levels of conflict. This is achieved by subtracting the original score from 13. The index thus ranges from 1, lowest level of internal conflict, to 13, highest level of internal conflict. Switzerland had the highest rating of political stability in 2019, i.e., the lowest risk of internal conflict, a score of 12. Rescaled, Switzerland has a score of 1, the lowest possible level of internal conflict. The method of rescaling is chosen to avoid zero values, as the main specification uses the growth rate of the index, calculated as:

$$\Delta Internal\ Conflict_i = \frac{Internal\ Conflict_{\ 2021,i} - Internal\ Conflict_{2019,i}}{Internal\ Conflict_{2019,i}} * 100$$

The main dependent variable is the percentage growth rate of conflict between the last year pre-pandemic and the end of the second pandemic year. It allows to gauge how conflict has developed after 24 months of the pandemic, relative to its individual starting point, which I am most interested in, rather than only assessing the effect during the first year or effect differences between the years. We can study how the pandemic affected the relative pattern of change and not only the level of conflict, which may be highly persistent across years. This contrasts the analysis from existing studies, using mainly indices in levels or count variables. Expanding the analysis to patterns of change is important to establish the robustness of the relationship. Change of conflict ranges considerably between -21.87% and 41.80%, with a mean of -0.45%.

Importantly, the PRS index is a subjective conflict index (PRS, nd.). Experts assign points to a pre-set group of factors that proxy political risks. The index only indirectly represents conflict that actually occurred. As with any subjective index, there are question regarding its validity and correlation to objectively occurring conflict. Internal conflict is a very salient phenomenon and may be less troubled by validity issues, but a degree of risk remains. Therefore, I also use an objective index of conflict. The number of conflict events in 2020 and 2021 within a country is retrieved from the Armed Conflict Location and Event Data Project (ACLED, 2022), commonly used in empirical research (Cervellati et al., 2022; Mehrl and Thurner, 2021). The variable serves as a robustness test of results obtained with subjective conflict data. It allows to rule out that results were caused by the Index's measurement process and were not truly

observing objective conflict. ACLED data is derived from news sources, government reports and scholarly research (ACLED, 2021c). The count variable indicates the number of events per country during the years 2020 and 2021. Events consist of violent events (Battles, explosions/remote violence, violence against civilians) and demonstration events (Protests and riots), between state and/or non-state actors. Recorded events are akin to the criteria along which the PRS internal conflict rating is assessed. Thus, the type of conflict is similar in the outcome variables, making results comparable. Conflict events range from only 2 events in the United Arab Emirates to 35754 events in the United States, with a mean of 3168.78.

In that, we see that objective indices come with their own caveats. First, they are dependent on the level of reporting and primary source material emerging from a country and carry the risk of media-bias. The pandemic likely decreased the level of reporting on conflict events due to travel restrictions and shifted attention (Mehrl and Thurner, 2021; Metternich, 2020). Moreover, the degree of reporting depends on the political system of the country, with conflict being more likely reported in democracies (Krieger and Meierrieks, 2016, p.13). Both issues imply that recorded conflict events are likely too low, and the predictive power of independent variables may be underestimated. A larger issue is selection bias regarding countries for which data is reported. The variety of source languages used to construct the database determines the levels of reporting accuracy (Herkenrath and Zoll, 2011). Only 6% of nationally reported protests are found in international, English-speaking sources (id.). Additionally, the measurement error is not constant across countries, with geographical distance from English-speaking news hubs and the international relevance of the reported-on country influencing the likelihood of reporting. Dietrich and Eck (2020) corroborate this and attest severe international underreporting of events in Africa. Practically, this skews the study's sample towards more economically developed countries, and limit generalizability of results. This is account for by using ACLED data, which draws on sources in more than 20 languages and supplements media reports with verified social media reports and local conflict observatories (ACLED, 2022). Therefore, it has significant advantages to databases limited to English news sources, such as the Cross-National-Time-Series Data Archive (Herkenrath and Zoll, 2011, p.164). However, a degree of selection bias likely remains, and generalizations of results careful and justified.

3.2. Key independent variables – measuring Covid-19 affectedness

The aim of this study is to capture the effect of the Covid-19 pandemic on internal conflict patterns, under the assumption that the initial level of inequality is a moderator. I will measure

the Covid-19 affectedness of a country as the reported Covid-19 deaths per million people between 1 January 2020 31 December 2021. The source is Our World in Data (OWID, 2022) which also discusses the (dis)advantages of the measurements I reference next. Adjusting for population size facilitates cross-country comparisons of the magnitude of Covid-19's impact. OWID (2022) shows that 1000 deaths in Iceland correspond to 2941 deaths per million and only 3 deaths per million in the US. The impact difference becomes immediately clear. I use the logarithm of Covid-19 deaths per million, to ease fitting a linear model to the data. The original variable has a range between 3.21 deaths per million in China and 6076 in Peru, with a mean of 1278.18. Log deaths per capita range between 1.17 and 8.71, with a mean of 6.38.

I choose confirmed deaths over confirmed cases for a technical reason. How close reported cases are to actual cases is dependent on the number of conducted tests. The degree to which countries perform tests varies greatly (id.). In my sample, Ethiopia performed a total of 35 tests per 1000 people as of December 31, 2021, whereas the United States performed over 2100 per 1000 people. Detected cases will considerably differ between countries, not because of actual public health dynamics but the degree of testing, introducing measurement error in the approximation of a countries Covid affectedness and limiting country comparisons. Moreover, measurement error arises as the propensity of testing within countries changes over time (id.). Early on, a lot of cases where undetected as testing infrastructure was limited. Additionally, testing was more or less incentivized by policies in different phases of the pandemic. Unfortunately, confirmed deaths suffer from similar flaws (id.). For instance, the definition of a Covid death differs between countries and thus the number of recorded deaths. Further, the accuracy and comparability of death counts are also reliant on a country's ability to record all deaths, which likely varies. However, I assume that the number of detected deaths is closer to true deaths than detected cases to are to true cases. The basic notion is that a death, as a much rarer and drastic event, is more likely to enter official records. Even though the number of global unreported deaths is estimated to be up to four times higher (Adam, 2022), that factor is likely much higher for cases, for instance given the fact that up to 35% of all cases are asymptomatic and unlikely to be captured statistically (Sah et al., 2021, p.4). I consider deaths to be a closer approximation of the true toll of Covid-19 than cases, as they are less sensitive to measurement error. I also prefer deaths per capita to measures mortality risks which rely on case counts, such as the Infection Fatality Rate (IFR) based on an estimation of actual cases, or the Case Fatality Rate (CFR) based on confirmed cases. Estimates of excess mortality that try to remedy the shortcomings of reported deaths have become prominent and could improve the precision of

estimates. Yet, they are not available for a large enough sample of countries, induce sample selection bias towards developed countries and are still not free from estimation uncertainties and comparability issues (OWID, 2022).

Using deaths as a measure of pandemic affectedness has a drawback: It fails to adequately capture disruptions of the pandemic which induce conflict but are not directly reflected in population health statistics. Draconian lockdowns as in China can be effective in keeping deaths down but may lead to unrest. The same is true for disruptions of the world economy that can plunge countries into turmoil, even if they themselves did not experience large Covid outbreaks. Examples are rising food prices supply chains disruptions or falling export revenues (Moyer and Kaplan, 2020). Yet, this likely causes an underestimation of the pandemics' conflict effect.

3.3. Key independent variables – measuring income inequality

I measure income inequality with the Gini Index. It is a well-known indicator of vertical inequality, and often used in empirical work focusing on the relationship of income inequality and conflict (e.g., Iacoella et al., 2021; Krieger and Meierrieks, 2016) The Gini Index is derived from a Lorenz-Curve (Sitthiyot and Hollsaut, 2020). It plots the cumulative normalized share of people from lowest to highest income, or income groups, ranging from 0 to 1, against the cumulative normalized share of income, ranging from 0 to 1. The 45-degree line is the line of perfect equality. Every individual, and hence every share of individuals, earns the same share of income (id.). The further the Lorenz Curve deviates from the 45-degree line, the higher the inequality. The Gini Index is calculated as the ratio of the area between the lines, and the total area below the 45-degree line. Thus, the Gini Index is scaled from 0 to 1, and can be expressed as a percentage, ranging from 0 to 100. 0 is perfect equality and 100 perfect inequality. The Gini is chosen for three reasons: first, it is intuitive and as a standardized index, we can readily compare countries (id.). Second, the index summarizes inequality across the whole distribution in a single statistic, as the deviation from the 45-degree line is measured across all income groups (Mancini et al., 2008, p.126). The index does not omit income groups from the statistic as do for example comparisons of income shares, such as the top 10% bottom 10% ratio. Third, the index is the most widely available measure of cross-country differences in income inequality, which allows an extension of the investigative scope (Solt, 2020, p.1184). The index is not perfect. I discuss drawbacks later on when an alternative measure of inequality is used.

Data on the Gini Index comes from the Standardized World Income Inequality Database (SWIID) (Solt, 2022). I measure inequality *before* the pandemic in different years depending

on availability. Specifically, I limit the measurement period to 2015 to 2019. That way, I overcome the problem that 2019 data is not available for a number of countries and guarantee that the Gini Index is still a relevant proxy of the pre-pandemic state of inequality. Ca. 75% of observation are measured in 2019 and 2018. The approach aligns with my research aim to investigate the degree to which initial inequality before the pandemic shapes the marginal conflict effect of Covid affectedness. The SWIID has two objectives that make it perfect for cross-country analysis: it aims to maximize geographical coverage and country comparability (Solt, 2020, p.1183). The SWIID is a secondary dataset and relies on the Luxembourg Income Study (LIS) as a primary anchor because the LIS already corrects for country differences in the survey data that form the basis of the aggregate Gini. The SWIID then estimates the relationships between the LIS Ginis and *all other available* Ginis for the same country-year observations and uses the results to impute missing LIS values (Solt, 2020, 1189-1193). Solt (2020, p.1195) shows that the difference between estimated and observed Ginis is only statistically significant in 9% of observations and on average smaller than 2 Gini points. Hence, I consider the SWIID Gini a valid measure of cross-country differences in income inequality. I use the Gini Index of disposable income, or Net-Gini. It describes the inequality of incomes *after redistribution*. People likely assess relative deprivation and redistributive institutional performance based on the final, tangible distribution of incomes. Hence, the Net-Gini is appropriate when considering the effect of inequality on conflict (Krieger and Meierrieks, 2016, p.10). It ranges from 22.7 in Slovakia to 65.1 in Namibia, with a mean of 37.641.

3.4. Control variables

Apart from Covid-19 and income inequality, other variables have likely influenced cross country variation in internal conflict during 2020 and 2021. I include an extensive set of control variables to reduce omitted variable bias. The variables are commonly used in applied empirical work on the origins of conflict. All variables, apart from Covid-19 related government support, are measured *before the pandemic* in 2019, with the exception of the internal migrant stock, ethnic fractionalization index and PolityIV index.

First, I include the level of economic development, measured as the logarithm of *per capita GDP in constant 2015 US$*. The data comes from the World Development Indicators (WDI) (World Bank (WB), 2022a). Low average per capita income may be associated with higher grievances and lower opportunity costs of conflict, even without a better situated reference group (Freytag et al., 2011, p.9). Thus, it is expected that income per capita is

negatively associated with conflict. Yet, a Chatham House report (2020) summarizes determinants of successful protest movements, including professionalization and international coordination, which are conceivably influenced by financial resources of organizers and participants. This would render the relationship between income and conflict less clear.

Second, I include *annual GDP per capita growth (%)* (WB, 2022a). Business cycles may determine conflict for much of the same reasons as above. Blomberg and Hess (2002, p.86) demonstrate that a recession will increase the probability of internal conflict the next year. Hence, I expect growth negatively associate with internal conflict.

Third, I control for *annual population growth (%)* (WB, 2022a). Collier (2006, p.6) demonstrates that faster population growth predicts civil wars. He assesses that faster growing populations strain the capacity of institutions to provide adequate levels of job and schooling opportunities and lowers economic growth, increasing recruitment into conflict parties (id., p.11). I expect the variable to positively correlate with conflict.

Fourth, I control for *unemployment (% of the total labor force)* (WB, 2022a). I expect unemployment to be associated with lower opportunity costs of conflict and a more fruitful recruitment for conflict parties. Moreover, unemployment may undermine trust in political institutions. The variable is expected to positively relate to internal conflict.

Fifth, I include the *Consumer Price Index* (WB, 2022a) as a proxy for inflation. Inflation can lead to decaying living standards and threatens particularly the livelihoods of economically vulnerable groups. Inflation should positively relate to conflict.

Sixth, I control for *natural resource rents (% of GDP)* (WB, 2022a). Collier and Hoeffler (2004) assess that conflict is driven by economic opportunities. Lootable natural resources are both funding for movements, as well as a source that satisfies greed of conflict parties (Collier and Hoeffler, 2004, p.564). Natural rents should positively relate to internal conflict.

Seventh, I include the *population aged 15-24 (% of total population)* (WB, 2022a). Farzanegan and Witthuhn (2016, p.4) assess that a large young population without economic prospects has more incentives to challenge the status quo violently, as they face unsatisfactory situations longer, and it becomes less costly to risk participation in opposition movements. Larger "youth bulges" should be associated with more conflict.

Eighth, I control for the *international migrant stock (% of population)* (WB, 2022a). A higher stock of migrants may indicate political stability and economic desirability of the destination country. Yet, large migration flows have been shown to induce conflict. Benček and Strasheim (2016) show that violent conflict increased in Germany after immigrant inflows in

2015. More migrants are expected to increase conflict. The variable is measured as the average from 2015-2019, as many observations are missing for 2019.

Ninth, I include a measure of *democracy*, the PolityIV index by the Center for Systemic Peace (2022), measured in 2018. More democratization may mean less political oppression, lowering motivation for conflict, and enable more legitimate political participation, lowering the necessity for violent conflict (Hegre, 2014). More democratic countries are expected to see less conflict. Higher values mean more democracy.

Tenth, I control for the *quality of institutions*. I use a composite measure that provides one statistic for economic and political institutions. I use the mean value of the six Worldwide Governance Indicators (WB, 2022b). They include control of corruption, regulatory quality, government effectiveness, rule of law, political stability and absence of violence and voice and accountability, which measures press freedom and political participation. Higher values indicate better institutional quality. I expect countries with better institutions to see less conflict by increasing the satisfaction of citizens and public trust, thus lowering incentives for conflict.

Eleventh, I control for the *degree of Ethnic Fractionalization* in 2013. Source is the Historical Index of Ethnic Fractionalization Dataset (Drazanova, 2019). The index measures the probability of two individuals in a country not being of the same ethnic group. Higher values indicate higher fractionalization. Less homogenous societies more likely experience grievances along ethnic lines that escalate into conflict (Fearon and Laitin, 2003, p.5). Krieger and Meierrieks (2016, p.29) find that higher ethnic tensions increase terrorism. The variable should positively correlate to internal conflict.

Twelfth, I include a measure of monetary *government relief* paid out during Covid-19 to alleviate adverse effects. Data come from the IMF (2022). I use the sum of additional spending and forgone revenue (% of 2020 GDP). The variable is used by Farzanegan and Gholipur (2021, p.13). The authors show that particularly low levels of relief payments can increase conflict risk (id., 17). Higher relief should be associated with lower internal conflict.

Lastly, I include the *initial level of internal conflict risk*, as the internal conflict risk rating in 2019 (PRS, 2022). Countries with higher conflict risk in 2019 may experience more conflict during the pandemic. I expect the coefficient to be positive. The variable is included in models with ACLED data, as 2019 ACLED data is broadly missing.

3.5. Empirical strategy

The baseline econometric specification has the following form when I use internal conflict change between 2019 and 2021, as the dependent variable:

$$\Delta InternalConflict_i = \alpha + \beta_1 lnC19Deaths_i + \beta_2 Gini_i + \beta_3 (lnC19Deaths_i * Gini_i) + \beta_4' Z_i + \varepsilon_i$$

Using the number of conflict events in 2020 and 2021, the specification changes to:

$$ConflictEvents_i = \alpha + \beta_1 lnC19Deaths_i + \beta_2 Gini_i + \beta_3 (lnC19Deaths_i * Gini_i) + \beta_4' Z_i + \varepsilon_i$$

The equations are estimated with ordinary least squares (OLS) and robust standard errors to control for heteroskedastic error terms. The subscript i denotes country i $LnC19Deaths$ is the natural logarithm of Covid-19 deaths per million people between January 1, 2020, and December 31, 2021. $Gini$ is the level of the Gini index before the pandemic, measured between 2015 and 2019. β_3 captures the effect of the interaction between deaths and inequality. I expect $\beta_1 > 0$, a higher per capita death rate increases internal conflict. Further, I expect $\beta_3 > 0$. The final effect of per capita deaths on conflict is larger in countries with initially higher income inequality. Z is a vector of control variables including regional dummies that aim to capture heterogenous effects on conflict that are due to unquantifiable regional idiosyncrasies. Regions are classified according to the World Bank. α is the constant and ε_i is the error term of country i. Subsequently, the empirical results will be presented and discussed.

4. Empirical effects of Covid and income inequality on internal conflict change

Table 1 reports the result of OLS regressions, where the percentage change of the PRS internal conflict index between 2019 and 2021 serves as the dependent variable. Column 1 presents the effect of the logarithm of Covid-19 deaths on conflict change, without controlling for other determinants of conflict. We see that the coefficient is positive but misses significance at the 10% level. In Column 2, regional fixed effects are added. The coefficient is significant at the 5% level and positively related to conflict change. Column 3 yields equivalent results after adding initial inequality, which is not significant at the minimal conventional 10% level.

In Column 4, the moderating effect of inequality is added. The coefficient is positive. Ceteris paribus, the marginal effect of log Covid deaths per capita on internal conflict will increase when the initial Gini Index increases by one percent. The marginal effect on internal conflict is unchanged if inequality remains unchanged. However, the coefficient is insignificant. When adding all control variables in Column 5, the interaction term coefficient is significant, with a p-value is 0.043. Ceteris paribus, the marginal effect of log Covid-19 deaths per million will on average increase internal conflict risk during the pandemic by an additional 0.424/100=0.00424 percentage points (pp), when the initial level of the Gini Index increases by one percent.

Table 1. Internal conflict risk change, Covid-19 deaths and initial income inequality.

	Dependent variable: Rate of internal conflict risk change 2019 to 2021 (in %)					
Explanatory variables	(1)	(2)	(3)	(4)	(5)	(6)
Log Covid deaths per capita (C19 deaths)	1.174 (.719)	1.932** (.931)	2.519*** (.872)	2.212***	.285 (1.026)	.418 (.822)
Net-Gini Index 2015 to 2019 (Gini)			-.267 (.284)	-.274 (.279)	-.109 (.402)	-.229 (.315)
C19 deaths * Gini				**.145** **(.114)**	**.424**** **(.203)**	**.331**** **(.127)**
Log GDP per capita 2019					2.8 (3.181)	1.945 (2.365)
GDP per capita growth rate 2019					.769 (.831)	.96 (.68)
Population growth rate 2019					3.952 (2.592)	4.15** (1.899)
Covid 19 monetary government relief					.691*** (.26)	.588** (.23)
Unemployment rate 2019					-.488 (.456)	-.232 (.362)
Consumer Price Index 2019					.004 (.023)	.009 (.023)
Youth Bulge 2019					-.105 (1.001)	-.667 (1.045)
Natural rents % of GDP 2019					-.058 (.324)	-.008 (.315)
Migrants as % of population 2015 to 2019					-.381*** (.141)	-.311*** (.107)
Ethnic Fractionalization 2013					7.56 (7.019)	5.5 (6.02)
Institutional Quality 2019 (WGI average)					-6.52 (4.973)	-4.37 (4.4)
Democratization 2018 (PolityIV)					.404 (.349)	.356 (.323)
Internal conflict risk 2019 (ICRG index)					-2.124 (1.749)	-.99 (1.531)
Regional dummies	NO	YES	YES	YES	YES	NO
Observations	106	106	106	106	92	92
R^2	.026	.047	.06	.077	.337	.29

*Notes: Constant not reported. The estimation technique is Ordinary Least Squares. Heteroskedasticity robust standard errors are in parentheses. Columns 3 to 6 use demeaned values of log Covid-19 deaths per capita and the Gini Index See the explanation in Section 4.1. *** p<0.01, ** p<0.05, * p<0.1.*

The average Gini Index in the sample of Column 5 is 37.6. A *one percent* increase thus corresponds to a difference in Gini Indices of 0.376 points. Consequently, a *one-point* higher Gini Index, on average, increases the effect of Covid-19 deaths on conflict change by approximately 0.00424x2.66=0.011 pp. A one standard deviation (SD) increase in the Gini

Index, 8.8 points, on average, increases the effect of Covid-19 deaths on internal conflict by 0.011x8.8=0.097 pp, keeping the level of Covid-19 deaths and other covariates constant. The moderating effect appears to be not overly substantive at, but the average marginal effect masks some larger substantive effects at different levels of inequality, which are assessed later on.

Column 6 presents a more parsimonious specification. I exclude regional effects, as an F-Test indicates that they are not jointly significantly different from zero, $\chi 2=0.77,\ p=0.59$. Additionally, no single effect is significant on its own. I use a parsimonious model to balance a tradeoff between model fit and statistical power. The tradeoff is particularly relevant in cross-sectional models which commonly little observations, or information to feed the model. I estimate a quite demanding model numerous covariates to avoid omitted variable bias and increase the model fit. However, as parameters are added, the information from observations available for statistical tests decreases, i.e., the number of available degrees of freedom goes down. There is a tradeoff between model fit and avoiding omitted variables, and power of statistical test. This is particularly problematic if additional covariates do not yield explanatory power. The model fit, measured by R^2, decreases from 0.337 to 0.290 when excluding regional fixed effects. At that expense, the model needs to estimate six parameters less. The approach is not free from criticism, which is why two specifications are presented, one aiming to increase fit and one less demanding to compare results. The interaction term stays positive at the 5% level in Column 6. The coefficient of the interaction term in Column 6 decreases in size and indicates a positive moderation effect of 0.00331 pp per one percent Gini increase.

Let's consider the final marginal effect of log deaths per capita calculated as follows:

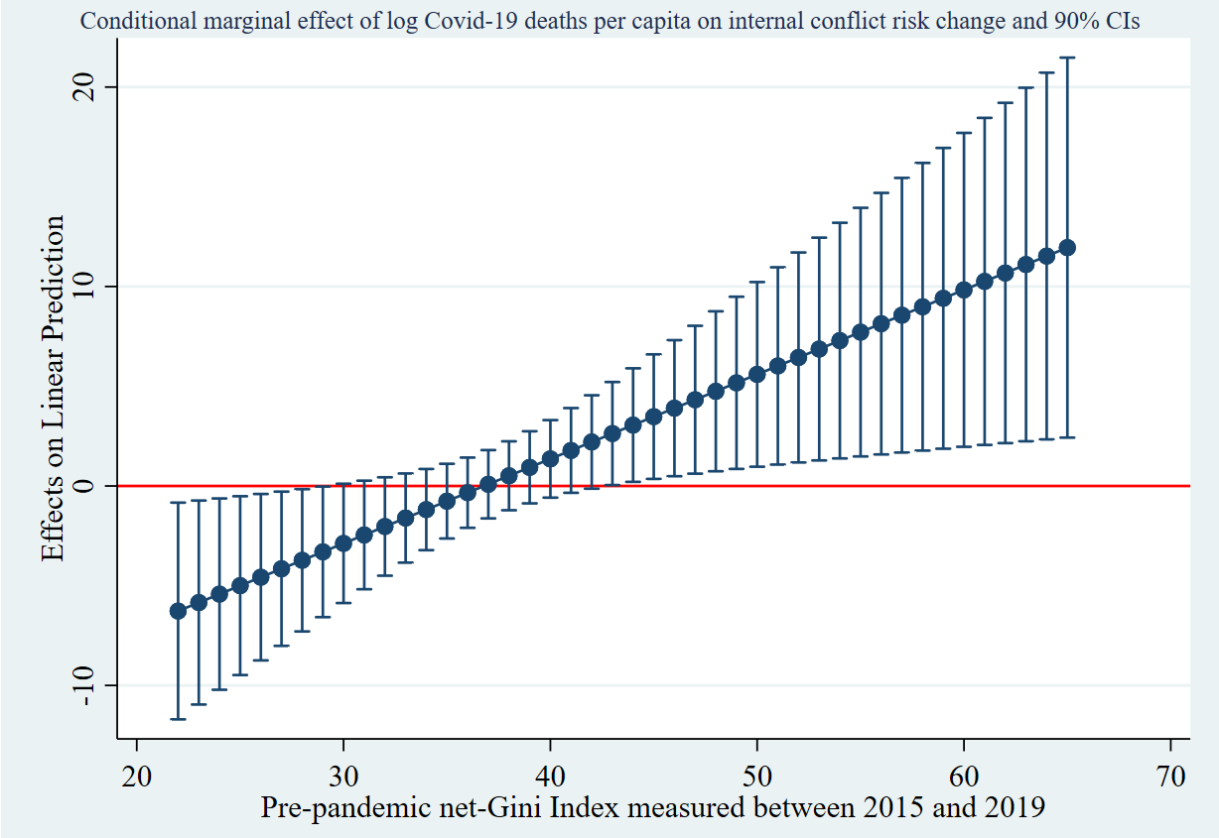
$$\frac{\partial \Delta Internal\ Conflict_i}{\partial lnC19Deaths_i} = -0.1559 + 0.00424 * (Gini_i)$$

The marginal conflict effect of log Covid-19 deaths per capita, results not reported, expressed by the derivative above, is made up of what is labeled for convenience its "direct" and "indirect", effect (Farzanegan and Witthuhn, 2016, p.16). The direct effect corresponds to the effect of deaths on conflict in case the Gini Index is zero. However, this reading is unrealistic, as the minimum sample Gini is 22.7, which is why the variable is reported in a different specification. To get an interpretable direct effect, I *demean* the Gini Index. That is, I subtract the sample mean from each observation. The variable range now contains zero, which is equivalent to a country with precisely mean inequality, a Gini Index of 37.641. The direct effect in Column 5 is thus the effect that Covid-19 deaths per capita have on internal conflict change in a country with mean inequality. The effect is positive, indicating that a higher death rate is

associated with increased internal conflict risk, in countries with mean inequality. However, Column 5, and 6 for that matter, also indicate that this effect is not statistically significant. Hence, we know that countries with mean inequality need not necessarily fear destabilizing effects from Covid deaths. We also that Covid-19 deaths do have a significant and positive additional indirect marginal conflict effect, moderated by the higher levels of initial inequality.

To assess the final effect as the sum of direct and indirect effect, I investigate the marginal effect of Covid deaths on conflict change at all observed Gini Indices. Figure 1 displays the marginal effect of a unit increase in log total deaths on internal conflict change based on Column 5, at different Gini levels, keeping all other covariates at their mean. I adopt this method in all marginal effect graphs in this article, as is done by Krieger and Meierrieks (2016, p.14).

Figure 1. Marginal effects of Covid deaths on internal conflict change at different Gini Indices.



The marginal effect of Covid-19 deaths per capita is significant when the 90% confidence bands do not include the zero line. We observe a global average marginal effect of Covid-19 deaths, i.e., pandemic affectedness, that is not fully in line with prior empirical and theoretical work, which largely considers Covid-19 affectedness to unequivocally drive conflict (Farzanegan and Gholipour, 2021, p.17). If that was the case, we would probably expect that the effect of Covid deaths on conflict is significant and positive at all levels of inequality, including mean levels. The results in Figure 1 imply a more differentiated effect: Only for countries with a Gini above

37 is the marginal effect of Covid-19 deaths on conflict positive, and only for countries with a Gini above 43 is the effect significant. Hence, there is no evidence that the marginal effect of Covid-19 is unequivocally destabilizing. Rather the overall effect appears to be moderated by the level of initial inequality, with three distinct effect intervals:

1. Stabilizers. Countries with very little inequality and Gini Indices between 22 and 29 did not experience, on average, higher levels of internal conflict based on their per capita Covid deaths. Quite the opposite. The average marginal effect of log total deaths per capita on internal conflict change in the significant interval is between -0.06 pp and -.003 pp.
2. Unaffected areas. Countries around mean Gini Indices, between 30 and 42, do, on average, not experience any effect of Covid-19 deaths on their level of internal conflict. The marginal effect is not significant at conventional levels.
3. Centers of conflict. In countries with initial Gini Indices above 42, which is roughly equal to 75th percentile levels, the pandemics' death toll exhibited, on average, a significant and sizeable increasing effect on internal conflict change. The average marginal effect of log total deaths per million on the linear prediction of internal conflict change in the significant interval is between +0.026 pp and up to +0.12 pp.

Moreover, the interaction appears quite important for explaining cross-country conflict variation. Only government relief and the stock of migrants are significantly associated with conflict in Column 5. When I leave the interaction term out, the relative explained variation in conflict change drops by drops by 25%, as the R^2 decreases from 0.337 to 0.208. In the next section, I will qualitatively assess the results and hopefully contribute to refining our understanding of Covid-19's effect on internal conflict.

4.1. Interpreting the results – different inequality, different effects

Figure 1 provides evidence that Covid affectedness does on average not per-se affect between-country differences in internal conflict risk changes during 2020 and 2021. Rather the combination of low, respectively high, initial inequality and Covid-19 affectedness appears to explain between-country variation, The average country in the sample for instance will likely feel no marginal conflict effect from their Covid death rate. This conditionality of Covid's destabilizing effect on initial inequality corroborates the notion of heterogenous effects found by early descriptive analysis (Ide, 2021) and econometric work (Mehrl and Thurner, 2021). The analyses find significant differences between regional conflict patterns following the Covid-19 outbreak. Censolo and Morelli (2020) similarly note that cross-country differences of the effect

of pandemics on conflict are dependent on a range of preconditioning factors. Additionally, and connected to heterogenous short-term effects, pandemics may only be unequivocally destabilizing in the medium and long-term (id., Barrett and Chen, 2021, p.12, 16-19). If we assume that the level of initial grievances and social disruptions which are exacerbated and caused by the pandemic, are proportionate to the level of the conditioning factor inequality, we may expect only the most unequal countries to experience conflict immediately after a pandemic's onset. Countries with initially more cohesive and satisfied societies may be immune against such an effect, or it takes longer to cause grievances deep enough to erupt into conflict. We find support of this assessment in Figure 1. Subsequently, I will interpret results more closely and use country examples to illustrate some of the channels that may be at play.

4.1.1. Stabilizers

In Figure 1, for countries with an initial Gini between the minimum of 22 and 29, the marginal effect of Covid deaths per million is *negative and significant.* Here, Covid-19 deaths, on average, decreased the risk of internal conflict. The effect becomes less negative with each Gini point. Within this interval, we may experience some of the channels that link pandemics to lower conflict and more stability. Countries with very low inequality had societal, economic and political preconditions in place that attenuated adverse effects of the pandemic. Possibly, countries in this interval deal with less initial grievances and avoided additional grievances sufficient to trigger conflict. Moreover, equal societies may be more likely to developed feelings of solidarity and support for its political leadership. This is consistent with the *rally around the flag* hypothesis (Mueller, 1970; Baum, 2002). Crisis rally people behind a common sense of purpose, inducing (temporary) unity. Such effects may decrease conflict. The effect was visible during the successfully handled first Covid wave in Germany, with a Gini Index of 29. Support for the Merkel government and majority partner CDU soared (Tagesspiegel, 2020). Surveys by infratest-dimap show an increase in support for the CDU of 13 pp between February and May 2020 (infratest-dimap, 2022), which constitutes the largest leap in support for the party in the whole Merkel-era. Similar evidence exists for low inequality countries such as Denmark. Nielsen and Lindvall (2021, p.1188-1192) show that Danish government and health authorities pretty consistently enjoyed the support and trust of citizens. The government has more capabilities to effectively deal with the pandemic and public discontent may be lower and less violent. It may not come at a surprise that rally around the flag and stabilization effects occur in low inequality countries. Research indicates that low inequality predicts higher trust in

political institutions (Bergbauer et al., 2022) and higher interpersonal trust (Elgar and Aitken, 2011). Both can be determinants of the degree of solidarity and unite during crisis.

Denmark had a Gini Index of 26.9 prior to the pandemic and is within the interval in Figure 1 where Covid-19 deaths had a negative marginal effect on conflict. And indeed, we find that Denmark's internal conflict risk index decreased by 1.12% during the pandemic. Let's draw a stylized example to illustrate the importance of low inequality. Indonesia experienced similar deaths per capita, 561 to 521. Yet, Indonesia's internal conflict risk increased 5.7% during the pandemic. The country had a 20-point higher Gini index in 2019. While this is certainly only partially comparable, Meckelburg and Bal (2021) point out that Indonesia faced significant conflict between central and regional governments regarding competences to intervene during the pandemic. The authors further fear that particularly poor sectors of Indonesia would be hardest hit by the virus, and that the informal community networks they rely on for resources would collapse (id., p.85). Such tendencies may induce conflict. Early on there also emerged reporting about escalating tension over land and worker rights, which had been brewing before Covid (Jong, 2020). We may infer that inequality in Indonesia, at least to a degree, shaped pre-pandemic grievances and alienated parts of society to a degree were the pandemic acted as a catalyst and escalated into conflict. We also find some evidence that the intensity of conflict was higher in Indonesia. Only 0.7% of all conflict events in Denmark turned violent, while this number is almost 2.4% in Indonesia (ACLED, 2022).

4.1.2. Centers of conflict

In Figure 1, for countries with an initial Gini between the minimum of 43 and 65, the marginal effect of Covid deaths per million is *positive and significant.* Here, Covid-19 deaths, on average, increased the risk of internal conflict. This result is in line with the theory that initially higher inequality triggers the conflict potential of pandemics. Let's consider an example to illustrate the channels. Brazil has been one of the world's most unequal countries for decades. Despite recent progress, Brazil's Gini Index declined from 50.2 in 2006 to 46.3 in 2014, the country entered the pandemic with a Gini of 48.5 in 2019 (Solt, 2022). The value puts it in the top 10% unequal countries in the sample of Table 1. Moreover, the central government's response to the pandemic has been lackluster. In January 2021, the Lowy Institute (Lowy Institute, 2021) curated a Covid response performance indicator, led by New Zealand and Vietnam. Brazil was ranked dead last. The country recorded 2894 deaths per million as of December 31, 2021, which is in the top 10% as well. Internal conflict risk increased by 14.6% during the pandemic, a top

10% value again. Against global trends, protests increased by 133% in March 2020, following the first recorded Covid case on February 26, 2020 (ACLED, 2020b). The ACLED accredits much of the increase to the lax response of Brazil's central government, as people protested for stricter regulations (id.). However, Brazil's initially high political polarization contributed majorly, as a number of counterprotests broke out, which reinforced original protest efforts. Note how this is diametrically opposed to rally effects in more equal economies. Political tensions in Brazil are partly riding this high because President Bolsonaro and his cabinet were coming of corruption allegations before the pandemic hit (Phillips, 2021) and slashed education budgets in 2019 (Phillips, 2019). Political polarization also contributed to tensions between local and federal governments, as governors implemented stricter policies unilaterally (ACLED, 2020b). Different societal factions adopted federal or local doctrine depending on allegiance. In April 2020 in Rio de Janeiro, this led to an escalating war between police militias in support of Bolsonaro and drug trafficking groups which enforced quarantines in the city's favelas, with numerous casualties (id.). We have seen above that political cleavages may be predicted by income inequality (Gu and Wang, 2021, p.10-17).

One reason for tensions between different government levels are long-standing conflicts over control of resources and land (Menton et al., 2021). Tensions escalated as the Minister of the Environment declared that the federal government use the pandemic to facilitate economic goals with regards to land and resources, as the public focused on the pandemic (Spring, 2020). Unequal land ownership is one of the leading causes of income inequality in Brazil (Assunção, 2006, p.1). Thus, the claims by the central government exacerbated grievances that were already present before the pandemic, and spurred conflict.

Brazil's inequality is probably nowhere more tangible than in the large cities with its skyscrapers bordering favelas. Favelas with their high population density and low living standards become a refuge of low-income classes. Research by Goularte et al. (2021) demonstrates that mental health issues resulting during stay-at-home policies, were particularly pronounced in low-income classes and young people. One of the most prevalent symptoms was anger. The overproportional mental health burden of low incomes can lead to higher conflict potential and aggressiveness particularly in more unequal societies. One major stressor for low incomes is financial insecurity, a grievance which likely preceded the pandemic and may have been amplified during the crisis. Pires et al. (2021, p.50-53) argue that the Auxílio Emergencia emergency payment program to lower incomes may have only postponed a crisis of escalating inequality. The authors point out that the program is temporally limited, and systemic forces of

the Brazilian economy continued to disadvantage low incomes and less educated groups during Covid (id.) The groups lost more labor income, were more likely unemployed and less likely to qualify for emergency credits. Thus, they were at risk of plunging into poverty as soon as emergency payments end, with inequality escalating. If true, conflict may have ensued via the channel of exacerbated inequality and relative deprivation. Brazil has also been shown to suffer endemic crime as a partial consequence of inequality (Szwarcwald et al., 1999), channeled by lower interpersonal trust (Elgar and Aitken, 2010). If interpersonal trust remains low, Brazil may fall quicker into conflict during a challenging health crisis, than a more egalitarian society.

Brazil appears to be a prime example that illustrates how initial inequality can unleash the destructive potential of pandemics, by aggravating grievances, fueling political polarization and undermining public and interpersonal trust. In the next section, I analyze the relationship with regards to an objective measure of internal conflict.

4.2. Competing interaction terms

In Table A3, I include competing interaction terms into the specification in Column 5 to control whether income inequality falsely measures the moderating effect of other variables. Conceptually, these variables represent competing theoretical hypotheses which aim to explain conflict at large and during pandemics. The hypotheses are laid out in the discussion of explanatory variables. The competing interactions I include are between log Covid-19 deaths per capita log GDP per capita, natural resource rents (% of GDP), Ethnic Fractionalization Index and PolityIV index. Whether competing interactions are included individually in addition to the interaction with the Gini Index, or all together, the interaction between inequality and Covid-19 deaths remains significant at the 5% level and quantitatively unchanged, see Table A3. It is further the only interaction that reaches significance. Results strengthen believe in the importance of the distributional channel of pandemic conflict.

5. Do Covid and income inequality affect objective measures of conflict?

Turning to objectively counted conflict events next, we encounter a frequent issue in empirical work with count data. The variable is overdispersed, that is the variance is bigger than the mean. Often, a Poisson regression is used to model count data. The Poisson distribution however assumes equi-dispersion, or that the variance is equal to the mean. Thus, an extension of a Poisson regression may be necessary to adequately deal with overdispersion (Ver Hoef and Boveng, 2007, p.2766). A common solution is the use of a negative binomial regression, where the variable is assumed to follow a Poisson distribution, but the equi-dispersion assumption is

relaxed, and the variance allowed to differ from the mean (id.). An application of the concept in the conflict literature is Plümper et al. (2021) who estimate determinants of protests events during Covid-19 in Germany and Krieger and Meierrieks (2016) who estimate the effect of inequality on terrorist attacks. However, a number of studies have pointed out that the negative binomial model may be only better at solving the overdispersion problem in very specific circumstances (Berk and MacDonald, 2008, p.280), or when the dispersion is precisely following a negative binomial distribution (Blackburn, 2014, p.615). Wooldridge (2010, p.648-649) argues thus that Poisson estimators are consistent even when equi-dispersion is violated, and that inference testing is still possible. It is not necessarily clear which estimator is preferable. I thus estimate the model with a negative binomial regression first, based on strong over-dispersion, and re-estimate the model with a Poisson regression for robustness.

5.1. Empirical effects of Covid and income inequality on conflict events

Table 2 reports the results of negative binomial regressions, where the number of conflict events from the ACLED is regressed on the same right hand side variables as above. The logarithm of Covid-19 deaths per million is significantly and positively associated with conflict events in Column 1, but the coefficient loses significance when regional dummies are added. Likewise, when all controls are added in Column 5, the coefficient of the demeaned logarithm of deaths per capita is insignificant. The effect at different levels of inequality is assessed later on.

The interaction coefficient is positive and turns statistically significant at the five 5% level in Column 5. Ceteris paribus, the marginal effect of log Covid-19 deaths per million on internal conflict events during the pandemic will be bigger when the initial level of the Gini Index increases by one percent. When the Gini Index remains unchanged, no additional destabilizing effect occurs. Column 5 indicates that the positive and significant moderating effect of income inequality is robust, independent of measuring conflict subjectively or objectively. Confidence increases that the predictions of Hypothesis 3 are in fact supported by the data. The initial income distribution again appears to be an important determinant of the pandemics' marginal impact on internal conflict and partially explains conflict differences between countries Column 6 gain uses a parsimonious specification. Variables are adapted based on the results of Column 5. Regional effects are jointly significant from zero, $\chi 2(6)=18.29$, $p=0.006$, and thus included. I reduce parameters by excluding variables that are not significant at the 10% level, as practiced by Farzanegan and Gholipour (2021, p.17). All excluded variables are not jointly significant as well, $\chi 2(5)=3.34$, $p=0.648$. Results do not change appreciably.

Table 2. Internal conflict events, Covid-19 deaths and initial income inequality.

Explanatory variables	*Dependent variable: Number of internal conflict events in 2020 and 2021*								
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Log Covid deaths per capita (C19 deaths)	.16* (.088)	.137 (.134)	.144 (.144)	.123 (.18)	-.176 (.142)	-.173 (.139)	.839 (.119)	.038 (.136)	-.297** (.116)
Net-Gini Index 2015 to 2019 (Gini)			-.004 (.023)	-.003 (.024)	.013 (.029)	.011 (.021)	1.013 (0.292)	-.056** (.027)	.015 (.029)
C19 deaths * Gini				**.004 (.015)**	**.036*** (.012)**	**.035*** (.01)**	**1.036*** (0.012**	**.023 (.017)**	**.034** (.014)**
Log GDP per capita 2019					1.095*** (.306)	1.076*** (.273)	2.99*** (.913)	1.635*** (.366)	.928*** (.335)
GDP per capita growth rate 2019					.009 (.072)		1.01 (0.073)	.072 (.056)	.005 (.071)
Population growth rate 2019					.202 (.273)		1.223 (.334)	-.429 (.315)	.297 (.316)
Covid 19 monetary government relief					.038 (.031)		1.039 (0.032)	.034 (.03)	.046 (.036)
Unemployment rate 2019					-.002 (.038)		.998 (.037)	.052 (.035)	.001 (.04)
Consumer Price Index 2019					.003* (.002)	.003* (.002)	1.003* (.002)	.006** (.003)	.003* (.002)
Youth Bulge 2019					-.168** (.081)	-.131** (.065)	.845** (.069)	-.081 (.097)	-.136 (.087)
Natural rents % of GDP 2019					-.054* (.028)	-.042 (.026)	.947* (.026)	-.116** (.052)	-.048* (.029)
Migrants as % of population 2015 to 2019					-.048** (.021)	-.043* (.023)	.953** (.021)	-.029 (.027)	-.071*** (.02)
Ethnic Fractionalization 2013					.245 (.718)		1.238 (-918)	1.45* (.751)	1.029
Institutional Quality 2019 (WGI average)					-1.49*** (.532)	-1.251*** (.474)	.225*** (.12)	-1.966*** (.483)	-1.237** (.509)
Democratization 2018 (PolityIV)					.105** (.041)	.106*** (.039)	1.11** (.0457)	.117*** (.039)	.109*** (.041)
Internal conflict risk 2019 (ICRG index)					.902*** (.148)	.961*** (.14)	2.464*** (.364)	.716*** (.181)	.807*** (.164)
Estimation technique	NB	NB	NB	NB	NB	NB	IRR	Poisson	OLS
Regional dummies	NO	YES	YES	YES	YES	YES	YES	YES	YES
Pseudo R^2 / R^2	.0018	.0142	.0142	.0143	.0667	.0649	.0667	.7	.0685
Observations	105	105	105	105	92	92	92	92	92

*Notes: Constant not reported. Heteroskedasticity robust standard errors are in parentheses. Columns 3 to 9 use demeaned values of log Covid-19 deaths per capita and the Gini Index to make their direct effect interpretable. See the explanation in Section 4.1. *** p<0.01, ** p<0.05, * p<0.1. NB=Negative binomial regression, IRR=Incidence Ratio Rate, OLS=Ordinary Least Squares.*

Significant other predictors are income per capita, inflation, the youth bulge, the stock of migrants, the quality of institutions, the level of democracy and the initial level of internal conflict risk. While inflation, institutional quality, the level of democratization and initial internal conflict risk are of hypothesized direction, income per capita and the youth bulge are of counterintuitive positive and negative direction. Here, model specifics need to be kept in mind. Conflict is explained in the specific and short context of the pandemic. The effects are likely driven by idiosyncrasies of this period. Table 3 finds evidence that well- developed countries experienced more conflict and countries with a larger young population experienced less. This notion is corroborated by Fiertz (2021), who show that economically well-developed countries suffered relatively stark drops in state fragility in 2020. Among the ten highest increases in the State Fragility Index (FSI, 2021) are the United States, Spain and Belgium. Additionally, Polo (2020, p.6) points out that a large number of social disorder events occurred in countries without ongoing armed conflict prior to the pandemic, which are likely more developed. I do not have a fully developed theory why well-developed countries may experience more conflict. One difference between countries is Covid affectedness. Upper middle- and high-income countries recorded 1779 and 1542 deaths per million. Lower-middle and low-income countries only 547 and 71 deaths per million. Log Income per capita strongly correlates with log Covid deaths per capita, $\rho=0.388$, $p=0.000$. Possibly, Covid only destabilizes countries after a threshold of deaths is met, which was not the case in less developed regions. Income per capita negatively correlates with the youth bulge, $\rho=-0.774$, $p=0.000$, which may explain the counterintuitive sign on the youth bulge as well.

Column 8 and 9 report robustness results for different estimation techniques. Column 8 reports the results for the specification in Column 5 using an OLS regression. I model overdispersion by log-transforming conflict counts which. The variance is now smaller than the mean. However, there are still some questions whether it is appropriate to transform the data structure of the dependent variable instead of adopting an estimation approach that fits the data. Moreover, log transformations can reduce observations, as some count outcomes are equal to zero. Nonetheless, the result is robust. The interaction remains statistically significant and positive with a p-value of 0.018. A one percent increase in the Gini index on average increases the effect of Covid-19 deaths per capita on conflict events by on average 0.034 pp. A one point Gini thus corresponds to an average increase of approximately 0.090 pp. Column 8 presents the results of a Poisson regression. The interaction term remains positive but is of slightly smaller magnitude as in the negative binomial regression and misses significance with a p-value of

0.18. Depending on how we interpret the literature on the appropriateness of Poisson models for overdispersed count data, Column 8 allows for two readings: overdispersion in the conflict events variable cannot be adequately modelled with a Poisson regression and inferences from Poisson models are thus invalid. Or the differences in significance imply that the original model is misspecified, and results not robust, in case we believe that Poisson models can model overdispersion. The results are encouraging as far as they largely support earlier findings. Yet, they also serve as reminders that empirical modelling decisions and interpretations of the empirical literature, greatly shape the confidence in results.

Figure 2. Marginal effects of Covid deaths on conflict events at different Gini Indices.

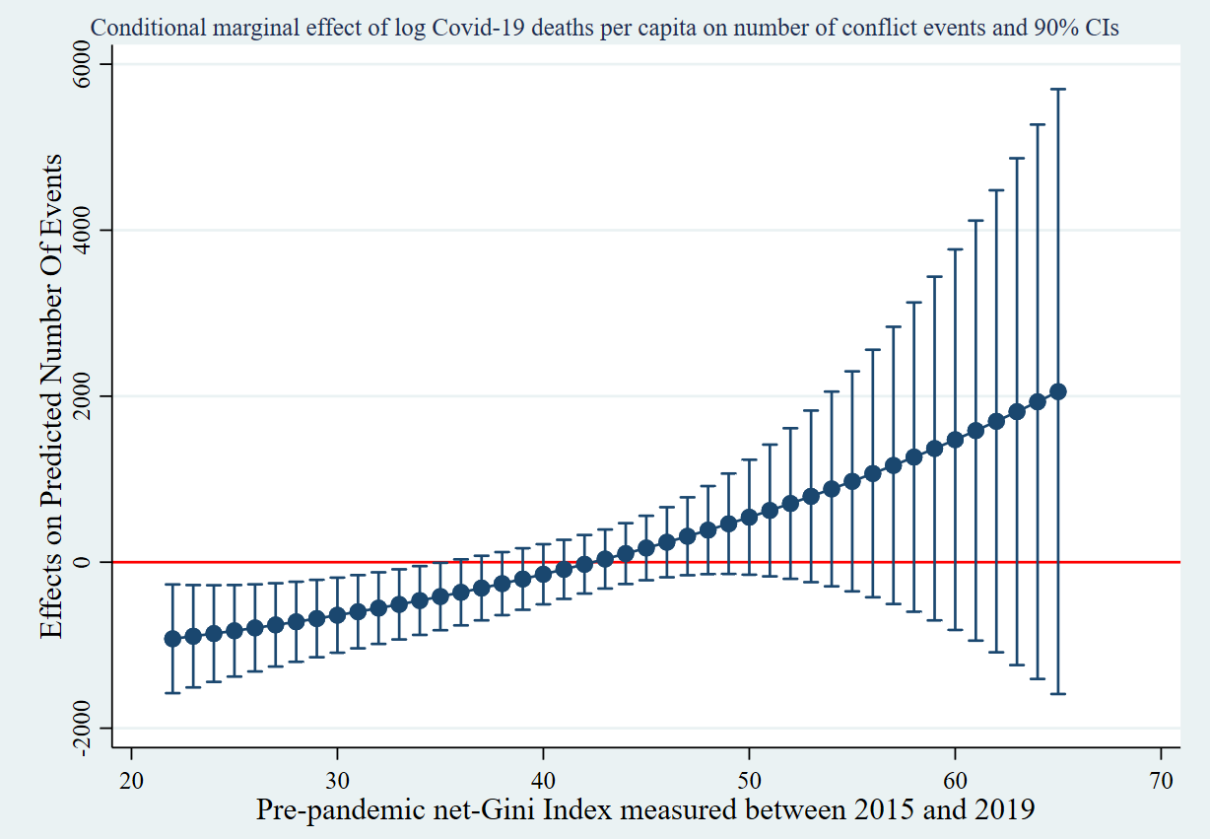


Figure 2 again reports the marginal effect of log Covid deaths on internal conflict at all observed Gini levels, keeping other covariates at their means. The pattern mirrors Figure 1. The average marginal conflict effect across all levels of Covid deaths and inequality is positive, substantive, and increasing in inequality, but appears to be driven by effects at lower inequality levels while conflict increasing effects at above mean inequality are less significant. However, when decomposing the average marginal effect later on, it becomes evident that for higher levels of Covid-19 affectedness, conflict events are significantly increasing in inequality also after mean levels. Again, we observe that the marginal effect of the pandemic on conflict is negative for low inequality. The results mirror Figure 1 and are opposed to predictions of large part of the

literature, which imply that the marginal effect is per-se positive and significant, independent of the level of inequality. A more detailed appraisal of the effect seems to be in order.

First, we have to keep the used timeframe in mind. There is some empirical evidence indicating historically a short-run reduction of conflict immediately after the outbreak. Metternich (2020) observes a sharp decline in protests after Covid emerged. Barrett and Chen (2021, p.16-19) find that 13 to 24 months after the past pandemics broke out, which is equivalent to my timeframe the likelihood of social unrest decreased (id, p.17). At the same time, they find cross-sectional evidence for a long-run increase in conflict, where the timeframe is 29 years (id., p.12). The authors attest that their historic pattern appears to hold during Covid (id., p.18-19). Results in Figures 1 and 2 support this result, while expanding it by highlighting the importance of the initial income distribution as a moderator of the effect, and that conflict increasing effects are still possible at high levels of inequality. Barret and Cheng (2021, p.16) hint at the conditionally of the effect of pandemics on conflict, as they attest mitigating and scarring effects after pandemics, and that the country-specific end result will depend on which effect dominates over time. The conditionally is also highlighted in Censolo and Morelli (2020), who specifically make the case that initial socioeconomic conditions will shape the final effect. The marginal effect figures support this view. Short-run conflict after pandemics does not occur per-se but is moderated by the initial income distribution that determines the emergence and balance of stabilizing and destabilizing forces.

5.1.1. Competing interaction terms

I again include competing interaction terms to the specification in Table 2, Column 5. Results are presented in Table A4. In columns 1 to 4, I add each interaction separately. No interaction coefficient is statistically significant. The interaction between inequality and Covid deaths remains significant at least at the 5% level in all Columns and is quantitatively unchanged compared to Column 5, Table 2. In Column 5, I include all interactions jointly to see if any of them wrongfully captures variation explained by another factor. The interaction of interest remains significant, while only the interaction with income per capita reaches significance. The coefficient is positive, and significant at the 5% level. Results are encouraging. The interaction with inequality does not appear to wrongfully capture variation in truth explained by other economic and political variables. Results lend some credence to a relative deprivation theory of conflict, at least in the context of health crises. Tentative evidence suggests that an intersection of grievances *and* economic capabilities of the population can particularly increase

the destabilizing effect of pandemics. This highlights that conflict theories do not need to be mutually exclusive. Grievances may still shape conflict propensity; opportunities facilitate organization and recruitment. The factors can act as complements.

5.1.2. Substantive effects of Covid-19 on conflict events

Plümper et al. (2021, p.2242) note that coefficients in non-linear models, e.g., negative binomial models, have a less straightforward interpretation. There are different approaches of making the substantive effect of variables visible. I follow Krieger and Meierrieks (2016, p.14-15) and choose two methods. First, I calculate Incidence Ratio Rate (IRR) coefficients.[1] Negative binomial regressions calculate the logarithm of the expected count in relation to explanatory variables. Coefficients express how the expected count changes after a ceteris paribus change in an explanatory variable. Coefficients can be imagined as the difference of the expected log count of the dependent variable when the explanatory variable is unchanged and the expected log count of the dependent variable when the explanatory variable is changed by one unit. The IRR takes advantage of the fact that the difference of two log values is equal to the log of their quotient, i.e., their ratio. IRR coefficients equal the ratio of counts that are expected when explanatory variables change by one unit or remain unchanged. The difference between the ratio and one, times 100, yields the percentage change in the number of counts after a unit increase of the predictor. IRR's show a predictor's influence on the occurrence rate of an event, its incidence, during a given timeframe.

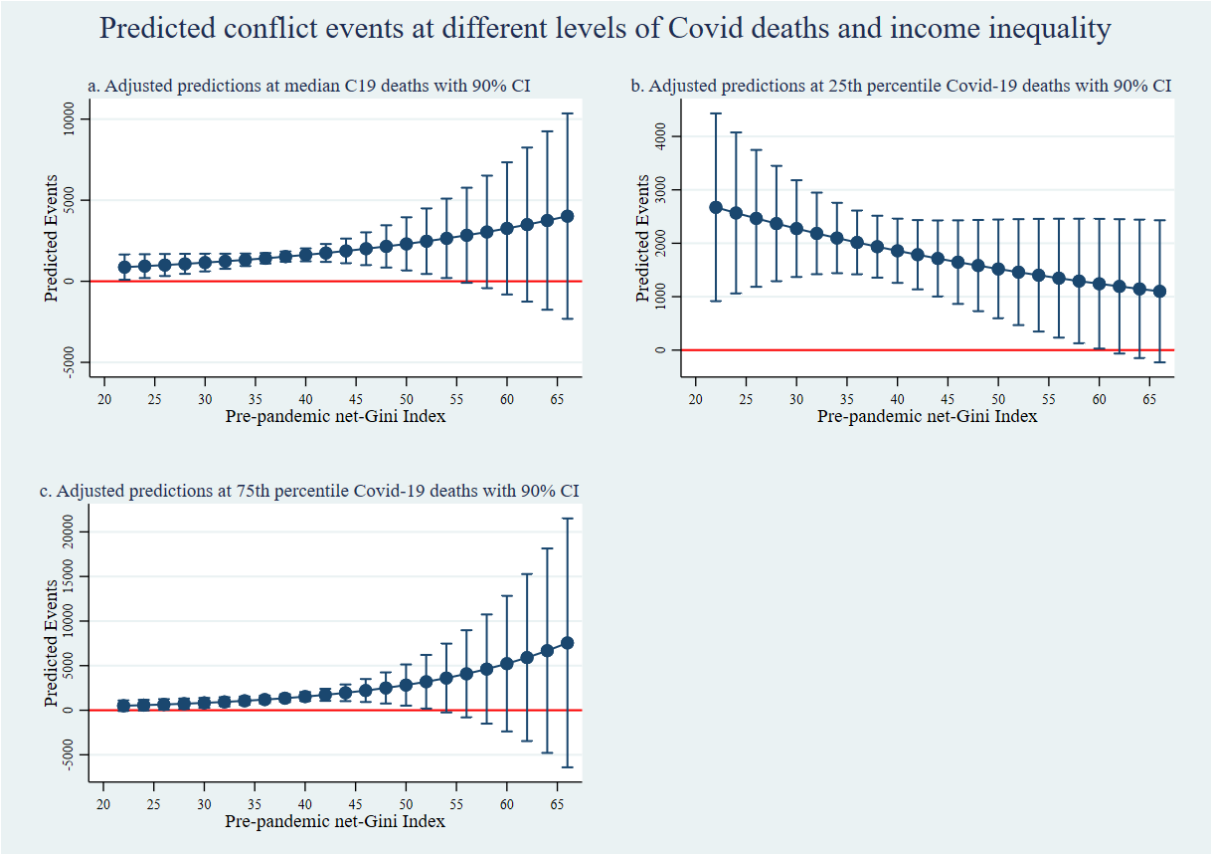
Column 7 in Table 2 reports the IRR coefficients for the specification in Column 5. Coefficients above 1 indicate positive associations with the number of conflict events, coefficients below 1 negative associations. The interpretation blueprint follows Buis (2010). The IRR for the interaction term between the Gini Index prior to the pandemic and log Covid-19 deaths per million is 1.036. Calculating the effect of the interaction as *(1.036-1)x100* yields 3.6. The marginal effect of log Covid-19 deaths per million on conflict events increases ceteris paribus on average by 3.6% for a one-unit higher initial Gini Index. Next, I further illustrate the moderating effect of initial inequality on the relationship between a given number of Covid deaths and conflict events. I use predictive marginal effects to make substantive effect sizes salient, a method used by Krieger and Meierrieks (2016, p.14-15).

[1] My discussion of the IRR draws on an article by the University of California at Los Angeles: https://stats.oarc.ucla.edu/stata/output/negative-binomial-regression/ [accessed 10.06.2022].

Figure 3 displays the predicted number of conflict events across different levels of the Gini Index, while keeping Covid deaths per capita and other covariates fixed. The only induced variation is in the level of initial inequality. Results are based on the specification Column 5, Table 2. Conceptually, we can think of the different inequality levels as representing different countries, who share a given number of Covid deaths and covariate values. We can then, ceteris paribus, observe average cross-country differences in predicted events at different inequality levels. Note that this is not equal to the marginal effect of Covid-19 deaths per capita at different levels of inequality as expressed in Figure 1 and 2. Figure 3 allows only an inference about the marginal impact of Covid deaths. What we can examine however is how conflict differed during the pandemic between countries which were perfectly similar in their Covid-19 affectedness but different in their initial inequality. As this aligns with the idea behind Hypothesis 3, and allows a more detailed investigation of it, we would expect that the predicted number of events increased ceteris paribus in income inequality. The different panels approximate the effect at different levels of log Covid-19 deaths to observe changes in the effect at higher and lower levels of pandemic affectedness. I depict results at 25^{th} and 75^{th} percentile log Covid deaths. Krieger and Meierrieks (2016, p.14) keep all covariates at means. I follow the approach but keep regional effects at means as well, which represent the prevalence of regions in the sample.

Panel A displays how the number of predicted conflict events at *median* log Covid-19 deaths per capita depends on average on initial inequality. The effect is monotonously increasing in inequality, with slightly higher marginal effects at higher inequality levels. Higher initial inequality, on average, increases the number of predicted conflict events in countries with median Covid deaths. The prediction is significant until Gini levels above 50. Comparing predicted events, we also see a substantively significant effect of inequality. For a country with median deaths and a Gini-index of 29, approximately one SD, or 9 Gini points, below the mean, the model predicts on average 1112 events in 2020 and 2021. For a country with mean inequality, a Gini-Index of 38, the prediction is already 1520 events. For a country with a Gini-Index of 46, approximately one SD above sample mean, the prediction yields 2077 conflict events. On average, the predicted number of conflict events in countries with median Covid-19 deaths and inequality *one SD below the mean* is 26.842% *lower* than in countries with mean inequality. Conversely, the predicted number of conflict events is 36.645% *higher* in countries with inequality *one SD above the mean*, compared to countries with mean inequality. The absolute difference of predicted events between the minimum and maximum Gini Index for which the marginal prediction is significant, 22 and 55, is 2741-872=1869 events.

Figure 3. Predicted conflict events at different levels of Covid deaths and inequality.



Panel B reports for log Covid-19 deaths per million *of the 25th percentile, or first quartile,* representing the median of values in the lower 50%. There is no theoretical reason to believe that the moderating effect of initial inequality should differ in its direction at different levels of Covid-19 deaths. Perhaps the expected effect sizes may differ, but not their direction. In Panel b however, the direction does indeed change. Ceteris paribus, the model predicts less conflict events in countries with higher initial inequality. The prediction is significant for nearly all Gini levels. Almost all countries at or below 25th percentile Covid deaths per capita are either developing countries, particularly Sub-Saharan-Africa and developed nations in East Asia. Conflict patterns among them shows that countries with higher initial Gini indices experienced *less* conflict events in 2020 and 2021. For instance, Sub-Saharan countries have an average Gini index of ca. 44, while East Asian and Pacific countries have an average Gini index of ca. 35.[2] Yet, the African countries only experienced and average of 1021 events, while East Asian and Pacific countries experienced 3280 events. This is reflected in simple correlations: conflict events *negatively* correlate to the Gini index in countries with Covid deaths in the 25th percentile or below, $\rho = -0.406$, $p=0.054$. This counterintuitive result may support the observation made

[2]Angola, Ethiopia, Ivory Coast, Gabon, Gambia, Ghana, Kenya, Malawi, Nigeria, Sierra Leone, Tanzania, Togo, Uganda, Zambia, South Korea, Australia, China, New Zealand, Japan, Singapore.

above, that particularly better developed countries, which typically have lower inequality vis-à-vis developing countries, struggled with conflict during the pandemic. The more equal East Asian countries, which experienced significantly more conflict, have an income per capita level almost 20 times higher as the Sub-Saharan countries, US$ 39743 compared to US$ 1826. Additionally, Panel B adds to the notion that the pandemic is not unequivocally increasing violent conflict (Ide, 2021; Mehrl and Thurner, 2021). Violent conflict is more likely found in less developed, more unequal countries, a notion not necessarily supported in Panel B. The data may support the reading that in countries *with low Covid-19 exposure*, conflict reduced in regions with higher inequality, such as Sub-Saharan-Africa, relative to increasing conflict in more developed regions with lower inequality such as East Asia and the Pacific, explaining the counterintuitive moderation effect of inequality. Yet, the effect is of lower magnitude than the positive effect above. Events are on average ca. 19.897% *higher* one SD *below* mean inequality and 16.589 % *lower* one SD *above* mean inequality. 2320 to 1935 to 1614 events.

Panel C reports the results for the log Covid-19 deaths per million *of the 75th percentile, or third quartile,* representing the median of values in the higher 50%. The pattern is similar to Panel A, though results are only significant between Gini Indices of 25 and 52. Predicted events increase monotonously in initial inequality. Again, a slightly non-linear relationship is visible. Countries with Gini's of SD below mean can expect, on average, 42.477% less conflict events than countries with mean inequality. Conversely, countries with Gini's one SD above mean, can expect almost 73.871% more conflict events. 777 to 1351 to 2349 events.

Concludingly, Figure 3 suggests that the marginal moderation effect of inequality on the average prediction appears to be larger and positive at higher levels of Covid deaths per capita. For lower levels of virus exposure, the data suggests that the average predicted number of conflict events decreases in initial inequality, even though at a smaller magnitude. While this certainly challenges my hypothesis, the average overall estimate of the moderation effect is positive and highly statistically significant, per Table 2. There is considerable evidence in favor of the hypothesis that the marginal effect of log Covid-19 deaths per capita on conflict events is positively moderated by initial income inequality. The substantive analysis provides indication that the effect may be driven by countries with above median Covid-19 exposure. Going beyond average effects, we find that the impact of inequality can be substantively significant as well.

5.2. Differentiating directly pandemic related and unrelated conflict events

In addition to the main dependent variables above, I also use two variables that distinguish conflict events directly related, and indirectly or not related to the pandemic. I consider overall internal conflict the main focus of this article and thus mainly aim to augment the results above with the investigation. The distinction of event relation to the pandemic allows to explore whether the conditional positive relationships were solely driven by effects on conflict in direct relation to the pandemic, or whether indirectly related events were influenced as well. The ACLED database (ACLED, 2022) lists as conflict events which are directly related to the Covid-19 pandemic for instance attacks on healthcare workers, attacks against civilians by state actors enforcing Covid-19 policies, and demonstrations in response to government policies aimed at curbing the spread of the virus. All events are linked to an actor or decision in direct relation to Covid-19. Indirect events are for instance changes in demonstration and conflict patterns after the pandemic broke out, or demonstration events that are primarily connected to broader socioeconomic issue and are only indirectly influenced by the pandemic, such as the BLM movement. Theoretically, directly related conflict can be influenced by a pandemic and initial inequality, similarly to general conflict events.

Table 3. Differentiating conflict with direct and indirect relation to Covid-19.

Explanatory variables	*Dependent variable: No. of conflict events directly related to Covid-19*			*Dependent variable: No. of conflict events indirectly (un)related to Covid-19*		
	(1) Events	(2) Events	(3) Log Events	(4) Events	(5) Events	(6) Log Events
Log Covid deaths per capita (C19 deaths)	-.029 (.146)	-.106 (.16)	-.03 (.171)	-.19 (.148)	.051 (.145)	-.336*** (.119)
Net-Gini Index 2015 to 2019 (Gini)	.001 (.029)	.013 (.059)	.007 (.027)	.017 (.031)	-.062** (.027)	.02 (.031)
C19 deaths * Gini	**.027* (.016)**	**.01 (.019)**	**.025* (.013)**	**.036*** (.013)**	**.024 (.019)**	**.036** (.015)**
Estimation technique	NB	Poisson	OLS	NB	Poisson	OLS
Observations	92	92	90	92	92	90
Pseudo R^2 / R^2	.089	.735	0.65	.067	.7	.67

*Notes: Constant not reported. Heteroskedasticity robust standard errors are in parentheses. All columns use demeaned values of log Covid-19 deaths per capita and the Gini Index to make their direct effect interpretable. See the explanation in Section 4.1. All columns include the full set of explanatory variables presented in Section 3.4. The complete table is reported in the Appendix as Table A5. All Columns include regional dummies. *** p<0.01, ** p<0.05, * p<0.1. OLS=Ordinary Least Squares, NB=Negative Binomial Regression.*

Table 3, Columns 1 and 4 present the results of negative binomial regressions that use the specification of Table 2, Column 5, with directly related and with indirectly or unrelated events as dependent variables. Unrelated events are overall events minus direct events. The demeaned

log of Covid deaths per capita is not significantly influencing events in direct and indirect relation to the virus. The interaction term is again positive in both columns and statistically significant at least at the 10% level. The average marginal conflict effect of deaths per capita will again be larger in countries with higher initial inequality. This is true for directly and indirectly related conflict events. Iacoella et al. (2021, p.11-12) provide similar evidence, and show that the likelihood of Covid-related protests across the United States was higher in counties with more stringent policy measures, which simultaneously had higher initial income inequality. Table 5 indicates a similar moderation effect of inequality also on the marginal effect of physical Covid affectedness directly related conflict. The findings do not come as a surprise, given the reviewed the literature. Grievances directly related to Covid-19, such as disease prevalence and death (Pires et al., 2021, Jung et al., 2020), adverse labor market effects (Mongey et al., 2020) and insecure livelihoods are very much distributed along class divides and expected to be larger in initially more unequal countries which may also hold for relative deprivation (Dorn et al., 2020; Reeves and Rothwell, 2020). For robustness, I re-estimate the models using Poisson and OLS models. Results are similar to the overall event count. The interaction term is positive for all methods. It is again not significant at conventional levels when I use a Poisson regression. For OLS regressions, the interaction is again statistically significant at the 10% level. The IRR coefficients for both interactions are 1.037 respectively 1.027. The effect of Covid deaths per capita on unrelated events appears to increase more when inequality is increased by one unit, 3.70%, compared to 2.71% for directly related events.

Figures A1 and A2 in the Appendix repeat the analysis of predicted marginal effects, using the same assumptions for covariate values as in Figure 3. Figure A1 shows the effect for conflict directly related to Covid-19. The pattern is in line with overall events in Figure 3, but predictions are of smaller magnitude. Considering indirect events in Figure A2, the pattern mirrors Figure 3 even more, and not surprisingly so as the majority of overall events are indirectly related to Covid-19, ca. 86%. Positive moderating effects may again stem from countries with above median pandemic exposure.

5.3. Interpreting the results – pandemics as amplifiers of existing issues

I aim to augment the case studies above by outlining how pandemic related conflict may have been influenced by initial inequality and how it spilled over into conflict only indirectly related to the pandemic. Let's consider the United States, which experienced an enormous amount of turmoil during 2020 and 2021.

Table 4. Predicted conflict events with the United States log Covid deaths.

	Net-Gini Index pre-pandemic 2019	*Precited Number of Conflict events*	*Percentage difference compared to country inequality prediction*
United States = 7.82 Ln Covid deaths per capita	39 United States level	1408	/
	30 One SD deviation below US	768	-45.455%
	38 One Point below US	1316	-6.534%

Notes: Predicted overall conflict events are estimated for the US log Covid deaths per capita, 7.818, with the specification of Table 2, Column 5. All covariates are held at their means.

The country has the recorded the highest absolute number of directly and indirectly related events, 7109 and 28645. Even if we consider the numbers somewhat inflated relative to other countries due to media bias, they are still staggeringly high. The US also has double the median number of directly related events per hundred thousand people, and almost as much in terms of indirectly related. The ICRG internal conflict score increased by 21.67% In fact, Wade (2020) even draws parallels between the level of conflict in the US and the English peasant revolt following the bubonic plague. The US entered the pandemic after periods of increasing economic inequalities and no movement in overcoming systemic racial inequalities (Buckley and Barua, 2020, p.3-5). Vertical and horizontal economic inequality may have contributed to phenomena that made the US a tinderbox during the pandemic: increasing affective polarization and radical political partisanship (Iyengar et al., 2019, p.2.), and levels of trust in government institutions and each other so low, that a majority of Americans considered it a hurdle to solving major problems (Rainie et al., p.3). 9% of Americans cited rising inequality as a direct cause of such issues. According to the mechanics of pandemic conflict, we expect particularly cases such as the US to erupt following a pandemic. Applying the predictive model of Figure 3 to the US log deaths per capita, and the country's Gini Index in 2019, 39.1, we find some inferential support for the theory. The Gini level is part of the interval in which predicted conflict for the given level of Covid deaths is on average significantly increasing in inequality. The model predicts 45.455% less conflict events, for one SD lower Gini of 30 compared to the US Gini of 39. Already 7% less events are predicted for a one-point lower Gini, see Table 4. The disruptions of Covid-19 appear to have cracked pre-existing fault lines in American society.

For instance, a major topic in the early phase of the pandemic were the troubles of lower incomes, particularly minorities, to pay rent and being targeted by evictions (Hepburn et al., 2020, p.9). Movements such as *cancel the rents* in the US critiqued inappropriate government support schemes for economically vulnerable tenants, which piled pandemic relates stressors

onto the relatively more people that already felt relatively deprived in less equal societies (Krieger and Meierrieks, 2016, p.5).[3] As a brief, simplified example, let's compare the United States with the similarly affected Belgium, 2486 versus 2435 deaths per capita. The descriptions of the pandemic related events for Belgium by the ACLED (ACLED, 2022) never contain the keywords "rents", "tenants" or "unemployment benefits". Protests regarding government support mainly focus on the culture and arts subsection of the economy. By May 30, 2020, the US had already seen more than 20 events in direct relation to the theme of rent cancellation (id.). What differentiates the countries, among other factors, is that Belgium has a Gini Index 13 points lower than the US, 26.1 versus 39.1. Moreover, Belgian institutions appear to be much more capable to soften the blows of economic shocks to lower income groups. In 2019, redistributive efforts lowered the Net-Gini in Belgium by 46.5% compared to the Market-Gini (Solt, 2022). The relative redistribution of income is only 25.2% in the United States.

An example of the US's relatively less capable redistributive systems is the federal governments CARES act of April 2020, which provided valuable aid during Covid-19 and a moratorium on evictions. Congress however failed to pass equivalently helpful legislation (ACLED, 2020a, p.20) when the act expired at the end of July 2020, putting a large number of low-income households in renewed economic danger (Williams, 2020). At the same time, big business kept turning profits while making more than questionable personnel decisions (MacMillan et al., 2020). Understandably, Akbar (2020) argues that movements such as cancel the rents always have a connotation of class divide and economic inequality in the United States and actively aim to redesign systems of power. The pandemic, and particularly its economic fallout, seem to have aggravated existing grievances within large segments of the population, a tendency not necessarily visible in countries with an initially lower level of income inequality. The tendency of combining pre-existing calls for systemic change with pandemic specific grievances can be observed in May 2020, when cancel the rents started signifying solidarity with the BLM movement after the murder of George Floyd (ACLED, 2020a, p.17-20; ACLED, 2022).

The pandemic aggravated the trend of the US economy to produce systemic income inequality and distributed the costs of the pandemic very much along existing class and racial lines (ACLED, 2020a, p.17-20; Buckley and Barua, 2020, p.3,8). Censolo and Morelli (2020) synthesize this as one of the moderators that historically drove conflict during and after pandemics. Indeed, the ACLED (2021, p.21) concluded that the economic impact of the

[3] See the "cancel the rents" homepage for an overview of their opinions and goals: https://www.canceltherents.org/ [accessed 28.06.2022]

pandemic, and particularly its distribution, acted as a catalyst for protest participation in the US. For instance, the organization assesses that the BLM movements turnout in summer 2020 was inextricably linked with the socioeconomic consequences of Covid-19 for economically and racially disadvantaged groups (id., p.17). Systemic issues around the distribution of income, wealth and opportunity preceded the pandemic in the US. During the pandemic, as the same issues worsened, a vital blend of pre-existing and acute grievances fueled overall conflict on a massive scale. Early in the pandemic, existing grievances appear to find a mirror in issues directly related to the pandemic, which ultimately contribute to an even stronger focus on the original fields of conflict. Whether such dynamics are feasible at the same scale in initially more equal societies, such as Belgium, is theoretically unlikely and the empirics supports this view. For a Gini of 26 and Belgium's deaths per capita, the same model as above predicts 594 conflict events, compared to 1509 for a Gini of 39. The significant positive effect of the interaction term on indirectly related events indicates that conflict in connection to the pandemic can supersede their connection to the disease and contribute to broader conflict. During Covid-19 this appears to have happened rather quickly. To be feared is a continuation of the trend even after cases slow down. Censolo and Morelli (2020) provide past examples were pre-existing ethnic and economic disparities were exacerbated by pandemics and contributed to revolutions and civil wars decades later. The US may have already made a similar, but much less drastic, experience: the January 6th riots were an explosion of partisan rage, fueled by the parties' reaction to Covid. Right wing militia in fact referred to the event as a beginning revolution (Hennessy-Fiske, 2021).

6. Endogeneity

By assumption, the single equation regression models above treat the direct effect of Covid-19 deaths and its interaction with income inequality as a source of exogenous variation to explain cross-country differences of internal conflict during the pandemic. In that, explanatory variables are assumed to be *uncorrelated with the error term*, or *exogenous explanatory variables*, which determine the *endogenous dependent variable* (Wooldridge, 2009, p.86-88). If the assumption is violated, the estimators are no longer unbiased. We cannot be certain that estimated sample effects on the dependent variable are representative of their true effects in the full population (id.). We are under- or overestimating the variables effect. Additionally, we can no longer make sure causal inferences that explanatory variables have a true effect different from zero on the dependent variable. We speak of endogenous explanatory variables, hence the issues name: *endogeneity*. Wooldridge (2009, p.512, 554) points out that endogeneity can arise due to

omitted variables, measurement error, and simultaneity. Next, I outline how endogeneity may affect previous results, how the empirical approach takes the issue into consideration and present result that tackle the issue.

Omitted variables can lead to endogeneity. If important conflict determinants are missing from the models, they are automatically included in the error term. If the determinants also correlated with explanatory variables, the explanatory variables are no longer independent of the error term and endogeneity is present. The estimates would be biased in magnitude and/or direction, and capture conflict variation truly explained by other factors. I choose a large set of explanatory variables to minimize this risk. Controlling for all relevant factors may be impossible and a residual risk prevails. Some drivers of conflict may be intangible or hard to quantify. Thus, regional effects are included. Moreover, a lack of imagination in modelling may lead to omitted variables. Explanatory variables measured with systematic error can lead to residual variable values entering the error term. This may be particularly present at global levels when data based on different sources and concepts has to be made comparable. The issue is addressed by relying on high quality sources which make efforts to reduce measurement error, standardize variables globally, and gather data comprehensively across the globe.

6.1. Simultaneity of Conflict and Covid-19 deaths

Both issues above are fixable by collecting better data (Wooldridge, p.554). The most problematic issue, simultaneity, is not fixable that way. Simultaneity occurs when contemporaneously measured dependent and explanatory variables are jointly determined (id.). In this case, we can no longer make causal claims about the direction of the relationship between dependent and explanatory variables. I include all variables, apart from Covid-relief and deaths, at pre-Covid levels. The data structure rules endogeneity out as dependent conflict variables cannot simultaneously affect pre-determined explanatory variables (Farzanegan and Witthuhn, 2016, p.15; Iacoella et al., 2021, p.10). This is not possible with regards to Covid-19 deaths, which have to be measured during the pandemic, simultaneous to conflict. The research question is how conflict patterns react when Covid deaths exogenously increase, depending on initial inequality. In case Covid deaths not only affect conflict, but conflict affects Covid deaths simultaneously, we can no longer reliably answer this question.

And indeed, there is evidence that simultaneity can arise. One main cause are previous conflict events that act as super spreaders. Dave et al. (2021, p.25-27) show that after the January 6th riots, the spread of Coronavirus increased in the counties where most protestors resided.

Conflict events that increase cases will inadvertently increase Covid deaths. The dependent variables may thus be directly influencing the key independent variable. Additionally, Iacoella et al. (2021, p.13) point out that protests, and conflict at large, may affect the stringency of governments Covid policies. Subsequently, changes in cases and deaths may be visible. Further, past research shows by Babajide et al. (2021, p.5-8) that internal conflict can decrease state capacity, for instance that countries with more internal conflict face lower tax revenue and govern less effectively. Moreover, policing large scale protests consumes state-resources. The Canadian "Freedom Convoy", a protest movement against Covid-19 policies, is said to have cost the Canadian government up to $35 million in policing (CTVNews, 2022). State capacity and specifically state fiscal capacity, may be crucial to effectively plan, coordinate and deploy resources during a pandemic. Capable states can build emergency facilities, support wages and help healthcare providers to hire more staff. As conflict can reduce state capacity, it may undermine the effectiveness of the government's response and lead to more severe cases and deaths. I assume that the arguments apply to subjective and objective internal conflict variables.

6.2. Instrumenting Covid-19 deaths

To remedy the endogeneity issue, I use an instrumental variable (IV) approach. The approach follows related studies by Iacoella et al. (2021, p.12-13) and Gonzalez-Torres and Esposito (2017, p.17). The endogenous explanatory variable (EEV) is approximated with an *instrument*, which explains the EEV very well, but is uncorrelated with the dependent variable, and thus the error term. The more relevant the instrument is for the EEV and the less it is for the dependent variable, the more suitable it is. I instrument log Covid deaths per capita in country i with average log Covid deaths per capita in neighboring countries, following ideas by Dietrich et al. (2021) and Iacoella et al. (2021), who use neighboring cases to instrument policy responses.[4]

Followingly, I theoretically justify the instruments' choice. One reason why Covid-19 deaths in neighbor countries can be correlated to domestic Covid-19 deaths *imported cases* (Russel et al., 2021, p.12). Infected travelers enter countries and increase domestic Covid-19 spread. While international travel plays a role for imported cases, the number of border crossings between neighbor countries is likely larger, increasing the probability of imported cases. For instance, large pockets of cases in Germany in March 2020 were traced to ski tourists arriving back from Austria. While imported cases were prevented by travel restrictions in the early phase of the pandemic (Russell et al. 2021, p.17-18). Zhong et al. (2021, p.4) show that by June 2020,

[4] A list of neighbors and methodology used to construct the instrument can be found in the Appendix in table A11.

already 63% of 625 global travel restrictions were ineffective. It is moreover likely that later in the pandemic as additional safety measures such as vaccines were available, travel resumed on a wider scale and imported cases increased in importance. The average importance of neighbor-imported cases is also visible in the continuous emergence of hotspot regions. Figure A3 compares levels of global Covid spread per million per country on July 1, 2021, and December 1, 2021 (OWID, 2022). Hotspots in southern Africa, South America and the east of Eurasian have moved by December not just by countries but by regions, now affecting central Europe and North America. This indicates the possible prevalence of regional spillover effects and a positive relationship between neighboring and domestic cases.

For the instrument to be valid, it also needs to only influence the dependent variable through its effect on the EEV (Wooldridge, 2009, p.563). Neighboring deaths are restricted to affecting domestic conflict only through their effects on domestic deaths. Is this assumption reasonable? Probably. It is unlikely that higher Covid affectedness in neighboring countries will induce citizens to cross borders and engage in conflict in the domestic country to any statistically meaningful degree. On the contrary, higher cases in neighbor countries may impede such travel. A caveat may be a solidarity effect between anti-government protest movements during the pandemic. In case a neighboring country experiences a surge in Covid-19 cases and deaths and more stringent polices are enacted, protest-movements in the domestic country may organize domestic protests out of a sense of solidarity with the affected neighbor. However, the quantitative importance of such an effect on overall domestic conflict is questionable. Therefore, I believe that the instrument fulfills both requirements and it is thus appropriate to instrument domestic cases with neighboring cases. The interaction of log Covid deaths with inequality is also instrumented with neighboring log Covid deaths, interacted with initial inequality, as the interaction also contains the EEV.

6.3. Results

Column 4 presents two-stage least squares (2SLS) regressions, compared to their pendants from Table 1, Columns 1 and 5. In a 2SLS regression, the EEV are regressed on exogenous explanatory variables and the instruments in a first stage. In a second stage, the dependent variable is regressed on the exogenous explanatory variables and the first stage estimates of the EEV, excluding instruments. Thus, Table 5 does not contain observed values of the EEV (Wooldridge, 2009, p.565-567). Staiger and Stock (1997) show that the first stage f-statistic for significance of excluded instruments should be above 10 to consider instruments strong. Yet, it

is unclear if the test is precise with more than one endogenous regressor. Technically, only Covid deaths are endogenous but their inclusion in the interaction creates another endogenous regressor. Consequently, I compare the minimum eigenvalue against the critical value for two endogenous regressors and two excluded instruments (Stock and Yogo, 2001, p.42). F-statistics are still presented. The minimum eigenvalue of 14.574 is bigger than the critical value 7.03. At the 5% level, the Null-Hypothesis that the 2SLS introduces more than 10% bias compared to OLS is rejected. Instruments can be considered reasonably strong. I consider critical values appropriate, even though they assume homoskedasticity, and heteroskedasticity robust standard errors are used, as non-robust standard errors do not significantly differ.

Column 2 shows that the direct effect of log Covid deaths per capita is larger in IV estimates and now significant at the 5% level. In Column 4, the coefficient of the interaction remains positive. Moreover, the coefficient remains significant at the 5% level.[5] Compared with equivalent OLS estimates in Column 3, we observe that the relative bias, i.e., the difference in coefficient size, is neglectable. The coefficient decreased by only 0.003, 0.331 (OLS) versus 0.328 (2SLS). Moreover, the demeaned effect of log Covid-19 deaths capita is unaltered in the IV model and remains positive but insignificant. Note that I cannot test the Null-Hypothesis that all instruments are indeed exogenous, i.e., independent of the second-stage error term. This is due to the fact that the model is exactly identified, i.e., uses as many instruments as endogenous variables (Wooldridge, 2009, p.535). Typical tests, for instance the Sargan J-test (Sargan, 1958, p.404) utilize over-identification, that is instruments exceed EEV, to test for instrument exogeneity. In case the Null of exogenous instruments is rejected, the 2SLS model suffers from the same problem as the OLS model. I made a theoretical case why the instrument is uncorrelated with the second stage error term and assume that the instrument is valid. However, the reader should be cautioned that this rests on theoretical considerations alone.

In the IV case, the decision between negative binomial and Poisson estimator, between which there is clear a priori preference, is now easier. There is no direct IV equivalent for the negative binomial estimator. Krieger and Meierrieks (20216, p.16) opt for using an estimator that again relaxes the equi-dispersion assumption. Yet, based on the discussion regarding the Poisson estimators' robustness under overdispersion, I use an IV Generalized Method of Moments (GMM) Poisson estimator, the pendant and comparison to the normal Poisson model.

[5]The IV 2SLS model in Column 2 is estimated without regional dummies, as they are not jointly significant ($\chi 2(6) = 3.89$, $p=0.704$) nor individually. The approach is akin to Krieger and Meierrieks (2016, p.17).

Table 5. Instrumental variable regression results.

	Dependent variable: Internal conflict risk change (%)				*Dependent variable: No. of conflict events*			
Explanatory variables	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Log Covid deaths per capita (C19 deaths)	1.174 (.719)	1.97** (.932)	.418 (.822)	2.149 (1.847)	.168 (.108)	.163 (.147)	.038 (.136)	-1.232 (.918)
Net-Gini Index 2015 to 2019 (Gini)			-.229 (.315)	-.241 (.293)			-.056** (.027)	.056** (.033)
C19 deaths * Gini			**.331** **(.127)**	**.328** **(.156)**			**.023** **(.017)**	**.071** **(0.03)**
Log GDP per capita 2019			1.945 (2.365)	1.083 (2.263)			1.635*** (.366)	.848* (.486)
GDP per capita growth rate 2019			.96 (.68)	1.072* (.625)			.072 (.056)	-0.044 (.127)
Population growth rate 2019			4.15** (1.899)	4.902** (2.034)			-.429 (.315)	.276 (.361)
Covid 19 monetary government relief			.588** (.23)	.648*** (.218)			.034 (.03)	.031 (.035)
Unemployment rate 2019			-.232 (.362)	-.298 (.344)			.052 (.035)	.014 (.058)
Consumer Price Index 2019			.009 (.023)	.011 (.022)			.006** (.003)	.002 (.003)
Youth Bulge 2019			-.667 (1.045)	-.442 (.97)			-.081 (.097)	-.233* (.122)
Natural rents % of GDP 2019			-.008 (.315)	.069 (.286)			-.116** (.052)	-0.06 (.038)
Migrants as % of population 2015 to 2019			-.311*** (.107)	-.32*** (.106)			-.029 (.027)	-0.071* (.029)
Ethnic Fractionalization 2013			5.5 (6.02)	5.049 (5.529)			1.45* (.751)	1.467 (1.805)
Institutional Quality 2019 (WGI average)			-4.37 (4.4)	-2.41 (4.53)			-1.966*** (.483)	-1.676** (.688)
Democratization 2018 (PolityIV)			.356 (.323)	.167 (.373)			.117*** (.039)	.175** (.081)
Internal conflict risk 2019 (ICRG index)			-.99 (1.531)	-.81 (1.405)			.716*** (.181)	.854*** (.206)
Estimation technique	OLS	2SLS	OLS	2SLS	Poisson	IV-GMM Poisson	Poisson	IV-GMM Poisson
Minimum Eigenvalue	/	65.19	/	14.57	/	/	/	/
First stage f-statistics	/	119.8	/	18.1; 39.3	/	/	/	/
Regional effects	NO	NO	NO	NO	NO	NO	YES	YES
Pseudo R^2 / R^2	.026	.014	.29	.262	.031	/	.07	/
Observations	106	106	92	92	105	105	92	92

*Notes: Constant not reported. Heteroskedasticity robust standard errors are in parentheses. Columns 3,4, 7 and 8 use demeaned values of log Covid-19 deaths / neighbor Covid-19 deaths per capita and the Gini Index. See explanation in Section 4.1. Log Covid deaths are instrumented with log neighboring countries Covid deaths. OLS=Ordinary Least Squares, 2SLS=Two Stage Least Squares. *** p<0.01, ** p<0.05, * p<0.1.*

Column 5 presents the Poisson estimates from Column 8, Table 2. Column 6 presents the results for the same specification and the IV GMM Poisson estimator. The effect of log Covid deaths in Column 6 mirrors not-instrumented results. The effect is positive but insignificant. The coefficient on the interaction term in Column 8 remains positive and now after instrumenting Covid deaths, the coefficient is now statistically significant at the 5% level. Moreover, its substantive size increased as well. The IRR of the interaction term is 7.311%, compared to 1.638% in the normal Poisson model (not reported). In Column 8, the coefficient of log Covid deaths per capita on conflict events is now significant at the 10% level, and of the previously appraised negative direction, similar to Figure 1 and 2, at mean inequality levels. Concludingly, IV results mirror earlier negative binomial regressions more so than earlier Poisson results.

6.4. Discussion of instrumental variable regression results

Concludingly, the results of the IV regressions are very encouraging. I do not find evidence that endogeneity threatens the validity of models that use PRS internal conflict change as the dependent variable. Moreover, possible endogeneity bias is negligible in size. The OLS model appears to be well suited to make statical inferences about the impact of Covid-19 deaths and income inequality on internal conflict change.

Interpreting the results for the number of overall conflict events is somewhat less straightforward. Where the negative binomial regression detects robustly a positive moderating effect of inequality on the marginal conflict effect of Covid deaths per capita, Poisson regressions fail to detect any significant relationships. However, as soon as I instrument Covid deaths, the interaction term with initial inequality is significant at the 5% level and quantitatively large. Results based on the IV-GMM Poisson estimator are nearly indistinguishable from the results of the negative binomial models. When comparing results, there appears to be some evidence for a more prevalent endogeneity bias in the relationship between observed conflict events and Covid-19 deaths, compared to subjective conflict measures. Objective measures may better reflect the potential channels through which conflict can influence Covid deaths. For instance, every event that can act as a super spreader is reflected in the event count variable. Conversely, if such an event, for whatever reason, is not reflected in the subjective conflict measure, the potential simultaneous determination of conflict and Covid deaths is more blurred. Yet, the results only indicate that the baseline Poisson results may have underestimated marginal conflict effects of Covid in countries with higher inequality. Given the overall evidence, I believe it is rather safe to assume that the original models did not

suffer from a great degree of endogeneity bias. Particularly the OLS models in Table 1 prove robust. I did not directly test the negative binomial models for endogeneity bias. However, the similar Poisson models yield some evidence that if endogeneity bias is present, it leads to an underestimation of effect sizes in the original models that use overall conflict events as the dependent variable. Thus, I am sufficiently confident to make the inference that the negative binomial models did also not suffer from endogeneity bias and argue that it is reasonable to use the original estimators in extended and robustness analysis. Krieger and Meierrieks (2016, p.32) and Iacoella et al. (2021, p.31) also revert back to the negative binomial estimator after establishing endogeneity bias is not an issue with another estimator. An additional reason is that IV estimators are less efficient than their not-instrumented counterparts, as they only approximate the EEV, and thus preferable is the original estimator does not suffer from endogeneity bias (Wooldridge, 2009, p.101-103). We can move on with careful optimism regarding the earlier results for overall conflict events as well.

7. The effect of Covid-19 and income inequality on conflict intensity

Until now, I considered the effect of Covid-19 deaths, and the moderating effect of income inequality, on cross-country differences in the overall number of conflict events. In addition to the simple count of events, parts of the conflict literature suggest measuring the impact of explanatory variables on *conflict intensity* (see e.g., Alesina and Perotti, 1996, p.1208; Gonzalez-Torres and Esposito, 2017, p.13). Conflict intensity is measured as the number of conflict events per capita. I use conflict events per one hundred thousand people in country *i* as the dependent variable, with population data from the World Bank (WB, 2022). It is a priori not necessarily clear whether the absolute number of conflict events or their relative intensity is a better proxy to compare the destructiveness of conflict between countries. Alesina and Perotti (1996, p.1208) point out that a single events, such as an assassination of a state leader, can have similar destructive effects in large and small countries. In this case, conflict should be measured in absolute terms. However, the effect significance of a given number of conflict events on a county's economy, society and political system depends on the population size (id.). Hence, conflict should be measured in relative terms, i.e., the intensity of conflict per capita.

Table 6 reports the results for conflict events per one hundred thousand capita as the dependent variable. As I transform the dependent variable from a count scale to a continuous scale, I use a linear estimator, which is more efficient (Wooldridge, 2009, p.101-102). The OLS regression in Column 1 is based on Column 5, Table 1. I again find evidence that supports a destabilizing

effect of Covid-19 deaths, conditional on initial income inequality levels. The interaction term is robustly positive and significant at least at the 10%. The effect is robust to the inclusion of other determinants of conflict, regional dummies and parsimonious modelling in Column 6. I again instrument log Covid deaths per capita with log neighboring Covid deaths per capita. It may be reasonable to infer that event intensity is simultaneously determined similarly to overall events, and thus may not suffer from endogeneity bias. Yet, I still report IV results, which show no significant difference. Results are unchanged in magnitude and significance. Again, regional effects are omitted in IV estimates as they are not jointly or individually significant.

Table 6. The effect of Covid deaths and income inequality on conflict intensity.

	Dependent variable: Conflict events per 100.000 people		
Explanatory variables	(1)	(2)	(3)
Log Covid deaths per capita (C19 deaths)	-1.31	-.637	1.119
	(.96)	(1.057)	(1.261)
Net-Gini Index 2015 to 2019 (Gini)	.061	.006	-.25
	(.236)	(.215)	(.173)
C19 deaths * Gini	**.191***	**.199****	**.205***
	(.111)	**(.097)**	**(.113)**
Estimation technique	OLS	OLS	2SLS
Minimum Eigenvalue (CV in parentheses)	/	/	14.574 (7.03)
First-stage f-statistics	/	/	18.034; 39.3
Regional effects	YES	YES	NO
Observations	92	92	92
R^2	.46	.306	.367

*Notes: Constant not reported. Heteroskedasticity robust standard errors are in parentheses. All columns use demeaned values of log Covid-19 deaths per capita and the Gini Index to make their direct effect interpretable. See the explanation in Section 4.1. All columns include the full set of explanatory variables presented in Section 3.4. The complete table is reported in the Appendix as Table A6. *** p<0.01, ** p<0.05, * p<0.1. OLS=Ordinary Least Squares, 2SLS=Two Stage Least Squares. CV= Critical value for 10% relative maximum 2SLS bias. Regional effects excluded in Column 3, as they are neither jointly significant, ($\chi 2(6) = 6.55$ $p=0.365$) , nor individually.*

Concludingly, Table 6 provides evidence that the interaction of Covid-19 deaths and initial inequality may not only have mattered for the differences in the absolute number of conflicts between countries. What is more is that the interaction appears to influence differences in the intensity of conflict between countries, though at a smaller scale. Not only singular events may be driven by the relationship, also the relative occurrence of conflict during the Covid -19 pandemic appears to be subject of the same mechanics.

8. Results for an alternative measure of income inequality: The Palma Ratio

I use the Gini Index to measure initial income inequality. It is available in high quality, with good comparability across countries based on the SWIID database. Furthermore, it is well known and has an intuitive interpretation. However, the Gini Index is not a perfect representation of income inequality. Atkinson (1970, p.256) observes that the Gini Index is not

sensitive to changes at the tails of the income distribution. Thus, the Gini may not fully reflect inequality that arises from increasing diversity between the richest and poorest of the economy and underestimate inequality. Sitthiyot and Holsaut (2020) illustrate this effect with the countries of Greece and Thailand, which have the same Gini Index of 36. Yet, the share of income held by the richest 10% relative to the poorest 10% is 60% higher in Greece than in Thailand, 13.8 to 8.6. We would probably not treat the countries as equals when examining income inequality beyond their Gini. Palma (2011, p.103) describes this as the "homogenous-middle vs. heterogenous tails" phenomenon: On average, middle incomes, deciles 5 to 9, appropriate about half of national income (id., p.103-104). While this is stable on a global level, relative income shares of the richest and poorest vary greatly between countries. Under Palmas theorems, the Gini index will not capture complete inequality adequately, but only one facet of it. It is most responsive to changes in the middle of the distribution, which may be least likely and most similar between countries (Palma, 2011, p.122). In Palmas eyes, inequality originates from the ability of the rich to increase their incomes at the expense of lower classes (id., 121-122). To fully appraise between-country inequality, we need to measure differences in income shares between the rich and the poor.

The implications for empirical work are straightforward. Any empirical model relying on the Gini Index alone may only capture specific facets of income inequality and thus not utilize the full explanatory power of the concept. Moreover, dependent variables can be more or less responsive to different facets of income inequality. Palma argues that distributional struggles are largely fought out between the rich and the poor, as the middle class is able to keep income relatively stable (id.). Extrapolating the argument, we may expect that class struggles and conflict increase in the diversity of income between the highest and lowest income strata. Thus, the difference in incomes between the tails may reflect the conflict potential of inequality better than differences in the middle. Thus, Palma (2011) proposes to measure inequality as the ratio of the tenth decile income share relative to the sum of income shares of deciles one to four, the so-called *Palma Ratio.* I prefer the Palma Ratio to ratios such as the top 10% to bottom 10% ratio, as increasing the income of the lowest 40% may be a more relevant measure of human development (UN, 2022a). Further, only considering the most extreme differences in income may be a too myopic concept of inequality that ignores effects on conflict caused by differences in income shares of lower-middle incomes.

In Table 7, I substitute the Gini Index for the Palma Ratio. The Palma Ratio comes from the World Income Inequality Database (WIID) of the United Nations (UN, 2022b)., I measure

inequality before the pandemic and due to less available data, I extend the measurement period from 2015 to 2012. Yet only 3 observations are measured before 2015. Akin to the SWIID, the WIID is a secondary dataset that makes data as comparable between countries. The Palma Ratio takes values from ¼ to ∞, where ¼ represents perfect equality and higher values more inequality. A Palma Ratio of 1 indicates that the top 10% earn four times the income of the bottom 40%. It ranges between 0.823 in Slovakia and 10.261 in Zambia, and a mean of 1.851. As the indices represent different concepts of inequality, we may be able to make inferences about the facets of inequality that have the largest potential of increasing the marginal conflict effect of Covid deaths per capita.

Table 7. Internal conflict, Covid-19 deaths and the moderating role of the Palma Ratio.

Explanatory variables	*Dependent variable: Internal conflict risk change (%)* (1)	*Dependent variable: No. of conflict events* (2)
Log Covid deaths per capita (C19 deaths)	.518	-.223
	(1.064)	(.143)
Palma ratio 2012 to 2019 (Palma)	1.881	.381***
	(1.527)	(.117)
C19 deaths * Palma	**1.554****	**.162*****
	(.695)	**(.054)**
Estimation technique	OLS	NB
Observations	91	91
Pseudo R^2 / R^2	.313	.069

*Notes: Constant not reported. Heteroskedasticity robust standard errors are in parentheses. All columns use demeaned values of log Covid-19 deaths per capita and the Gini Index to make their direct effect interpretable. See the explanation in Section 4.1. All columns include the full set of explanatory variables presented in Section 3.4. The complete table is reported in the Appendix as Table A7. *** p<0.01, ** p<0.05, * p<0.1. OLS=Ordinary Least Squares, NB=Negative Binomial Regression.*

Table 7 presents the results. Specifications are based on Table 1, Column 5 for internal conflict change and Table 2, Column 5 for the number of overall conflict events. Results are very similar. The interaction term is again significant at 5% when using internal conflict change as the dependent variable. Ceteris paribus, a country's marginal conflict effect of Covid-19 deaths per million will on average increase by 1.554/100=0.01554 pp, when its initial level of the Palma Ratio is higher by one percent. Comparably, the increase of the marginal conflict effect of Covid deaths was 0.00424 pp after a one percent increase of the Gini Index. The substantive effect of a higher initial Palma Ratio on the marginal conflict effect of Covid deaths is larger by about a factor of four, compared to the effect of a higher initial Gini Index.[6]

[6] Note the difference of one observation between Table 1 and 2 Column 5 and Table 5, Column 1 and 2. For Saudi Arabia, no Palma Ratio was available for a reasonable timeframe before the pandemic. Excluding Saudi Arabia from the estimation in Table 1 does not change results appreciably.

Figure 4. The marginal conflict effect of Covid-19 deaths at different levels of the Palma Ratio.

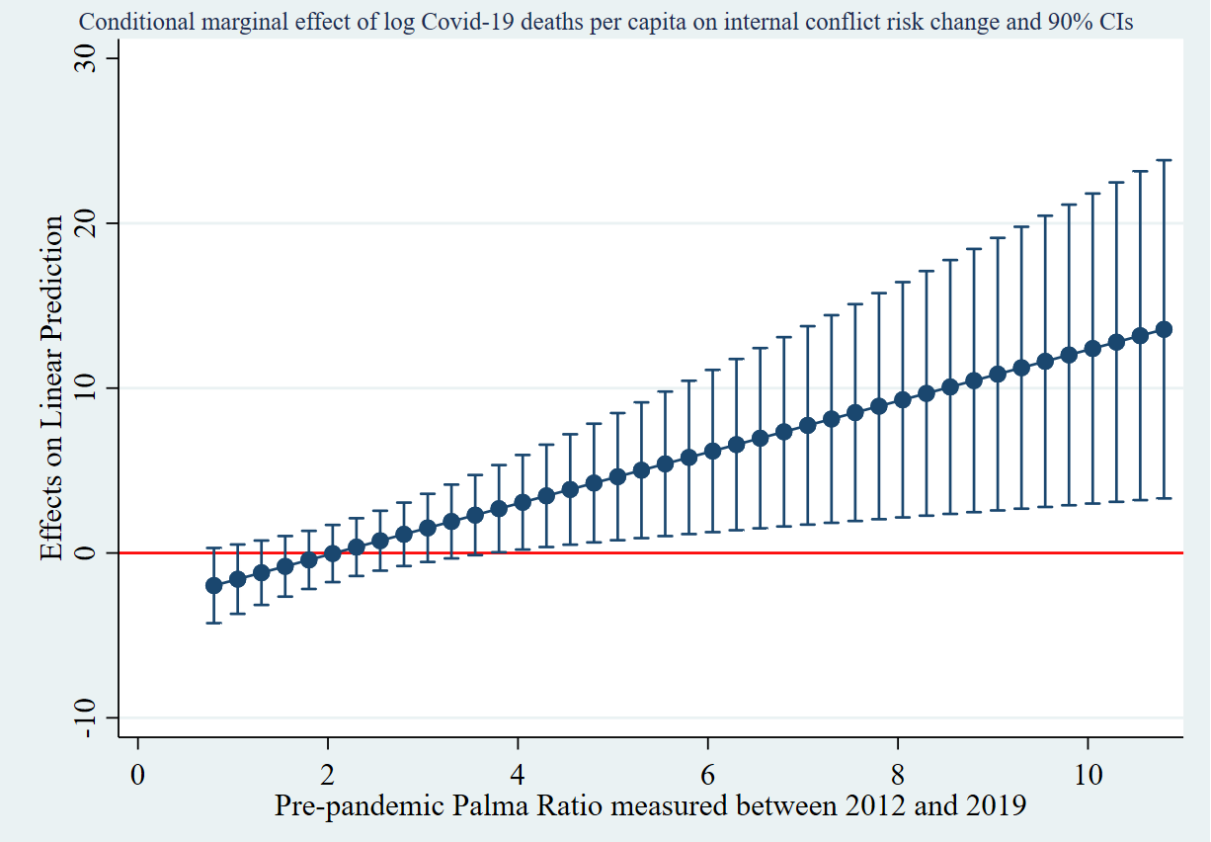


Figure 4 assess the marginal effect log Covid deaths per capita on internal conflict at different levels of the Palma Ratio. The figure is similar to the marginal effect at different Gini Index levels. The marginal conflict effect of Covid deaths again monotonously increases in inequality. We observe that the marginal effect is not positive along the whole range of inequality, but only above 2, which corresponds to above-mean inequality. The effect is only significant for levels above 3.8. Results are visible in Table 5 as well, as the demeaned effect of log deaths is insignificant whereas the interaction term is. While we again find a range of low inequality for which the marginal conflict effect of Covid is negative, the effect is not significant, compared to Figure 1. Apparent is also the greater average positive effect of Covid deaths on conflict, compared to Gini Index results, as predicted by the larger moderating effect of the Palma Ratio. To assess at which level the Palma Ratio moderates inequality relative to the Gini Index, I compare the decile level of inequality where the marginal effect become first significant. The thresholds of 1.7 (Palma) and 43 (Gini) correspond approximately to the 74^{th,} and 85th percentile of the sample distribution. The Palma Ratio appears to moderate the relationship only at higher inequality. However, the substantive effect is much larger after the threshold.

Column 2 presents the results with overall conflict events as the dependent variable. Again, results are similar to the Gini Index. We observe the same small negative but insignificant effect

of log Covid deaths per capita at mean levels, per the direct coefficient. The interaction term is once again positive and significant at the 5% level. The IRR for the interaction term between the Palma Ratios prior to the pandemic and log Covid-19 deaths per million is 1.175. The marginal effect of log Covid-19 deaths on conflict events increases on average by 17.5 percent for a one-unit higher initial Palma Ratio. The IRR for the Gini Index corresponded to an increase of 3.6%. The effects are not directly comparable as Indices are differently scaled. What is comparable, are *one SD effects*. For the *Palma Ratio*, the one SD IRR is equal to 17.5*1.851 = 32.393%. For the *Gini Index*, the one SD IRR is equal to 3.6*8.801 = 31.684%. While the marginal effect of Covid deaths on internal conflict change is much stronger influenced by the Palma Ratio compared to the Gini, this tendency is much smaller for overall conflict events.

Table 7 is encouraging for the robustness of earlier results. It appears that the relevance of initial income inequality as a factor that amplifies the destructiveness of pandemics is independent from the way we measure inequality. Additionally, we find tentative evidence that inequality manifested in a high diversity between top and bottom income groups leads to a more destabilizing effect of Covid-19 deaths, vis-à-vis inequality between incomes in the middle of the distribution. Differences between deciles 5 and 9, may be less tangible and do not affect subsistence. Also, opportunity costs of conflict may be higher here and a lower willingness to participate in conflict may result. Differences between the tales however may be more visible and determine salient socioeconomic outcome differences. Images of skyscrapers amidst slums in Rio de Janeiro and homeless encampments in downtown Los Angeles may evoke stronger feelings of relative deprivation in both the affected population and other groups that perceive solidarity with the relatively deprived.[7]

As an additional robustness test, I re-run the specifications above, using the Gini Index from the WIID (UN, 2022), to rule out that results with the SWIID Gini are driven by idiosyncrasies in the calculation and correction of the index. As Table A8 indicates, results for both internal conflict change and conflict events are robust vis-à-vis the substitution, which is encouraging.

9. Additional robustness tests

To further test validity of results for the two dependent variables, robustness tests are conducted.

First, I substitute the average Covid-19 case fatality rate (CFR) of country i during 2020 and 2021 for Covid-19 deaths per capita. The variable is used for instance by Farzanegan and

[7] Pictures: https://tinyurl.com/3fex4r9d (Alamy Photo, 2022), https://tinyurl.com/4xtkmssd (LA Times, 2020)

Gholipour (2021, p.17). Table A9 in the Appendix reports the results. I only report the results for the complete specification, which makes the models comparable to their equivalents, in Column 5, Tables 1 and 2. Results are quantitatively and statistically robust for internal conflict change. The Result for the number of conflict events is not robust to the change in the Covid-19 proxy, as the interaction is no longer significant. However, this does not necessarily invalidate the effect of physical pandemic affectedness on conflict above. The CFR, as the approximate individual risk of dying once infected, measures *personal affectedness*, while per capita deaths approximate the *collective affectedness* of a population. While related, it is possible that population affectedness carries more conflict potential and as a different aspect of affectedness, is differently moderated by inequality. For instance, policies that shut down the economy in Germany and induced massive, unequally distributed social costs were tied to per capita case thresholds. This indicates that caution is advised when extrapolating the findings of this article to all and other facets of pandemic affectedness.

Next, I test whether the global moderation effects are driven by a specific country. I run a series of regressions where I leave one country out of the sample at a time. Iacoella et al. (2021, p.14-15) use a similar approach. Estimates are based on Column 5, Table 1. Figure A4 reports the results. I plot the coefficient sizes of the interaction term coefficient and 95% confidence intervals (CI) for each of the 92 iterations. For six country-exclusions, zero-values are included in the 95% CI: South Africa, Ethiopia, Zambia, Bulgaria, Uruguay and Colombia. Yet, all coefficients are at least significant at the 10%. I repeat the same test for the number of conflict events, based on Column 5, Table 2. Here, zero-values are never included, all iterations are at least significant at the 5% level. Coefficient sizes are also by and large similar to the average effect. Therefore, I conclude that no single country is driving the average effect found above.

To additionally test the robustness of results for a bigger sample, I exclude less available explanatory variables from Columns 5 in Table 1 and 2, while aiming to maintain reasonable model fit. The idea also approaches the power versus fit tradeoff in another way (Barret and Chen, 2020, p.15). In Table A10, observations increase to 100. R^2 and Pseudo- R^2 decrease to 0.276 and 0.062. For both dependent variables, results of the moderating relationship are unchanged. The robustness tests validate earlier findings for the moderating relationship of income inequality, which is robust to different samples and explanatory variables.

10. Concluding thoughts

This article aimed to analyze the effect of the Covid-19 pandemic on between-country difference in internal conflict. Specifically, it investigated the moderating effect of the initial

income distribution on the marginal conflict effect of Covid-19. For 2020/2021, the evidence indicates that the marginal effect of log Covid deaths per capita was significantly higher in countries with higher pre-pandemic income inequality. A one-unit higher initial Gini Index increased the marginal effect of Covid-19 on internal conflict risk change by 0.011 pp and conflict events by 3.6%. The relationship is robust for different variable measurements, samples and techniques correcting for endogeneity. Some concerns persist regarding the generalizability of the relationship beyond pandemic affectedness measured as deaths per capita.

There is some evidence that the moderation effect holds for conflict in direct relation and only indirect or no relation to Covid-19. The study does not find evidence that Covid unequivocally increased internal conflict. The finding differs from previous studies that find pandemics to drive conflict across unequivocally. On the contrary, there appears to exist a range of low inequality which induced a stabilizing, negative marginal effect of Covid deaths. The initial income distribution appears to decisively shape the marginal conflict effect of the pandemic. The novel finding builds a good stepping-stone for investigating the relationship. One approach could be an investigation of conflict patterns between regions. Results also indicate that vertical inequality, despite its bad reputation in empirical work, can be relevant for conflict but only during societal ruptures such as Covid-19. An historical empirical inquiry would be interesting. Also, the mediating factors that relay the effect of inequality are interesting to investigate. Some candidates from the literature are interpersonal and institutional trust, political polarization and captured political power. In Table A12, I conduct a preliminary first analysis. I regress data from the World Values Survey (WVS, 2022) for 2017 to 2019 on the periods average Net-Gini and regional dummies. Results suggest that inequality correlated with decreased odds of interpersonal trust in general and especially trust of other nationalities before the pandemic, which may make unified efforts to master the crisis difficult. Further, people in more unequal countries reported less educational attainment, which can be a predictor of low socioeconomic status and thus conflict during the pandemic. I do not find positive correlates between inequality and lower confidence in institutions. Yet, inequality appears to correlate with stronger perceptions of corruption, which may indicate asymmetric power relations between segments of society which induce particular strains during crisis.

Results have to be taken with the usual precautions regarding small sample sizes, measurement error and selection biases in cross-sectional econometrics. Even after addressing the issues, caution is still advised when applying the broad global findings to specific contexts with altered parameters. At any rate, the results are robust enough, and partially substantive enough, to

consider distributional policies a key instrument to alleviate conflict in the face of public health crises. For instance, as of writing this in August 2022, Germany is discussing subsidies for energy companies in the ongoing economic turmoil and Ukraine war. If such policies are not explicitly considering distributional consequences, the already heated socioeconomic climate post-pandemic may spill over into renewed social disorder. History should be a warning that even minor disruptions immediately in the wake of pandemics are unlikely to subside, as the revolts after the bubonic plague indicate. Tackling systemic causes of distributional issues that have been exposed by the pandemic *now* can be one avenue to move productively forward.

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Appendix

Table A1. Summary Statistics.

Variable	Obs.	Mean	SD.	Min	Max
Internal conflict risk change (%) between 2019 and 2021	92	-.446	11.683	-21.875	41.803
Number of conflict events in 2020 and 2021	92	3168.783	5881.403	2	35754
Number of conflict events in 2020 and 2021 per hundred thousand people	92	10.691	11.485	0.020	69.485
Covid-19 deaths per million people in 2020 and 2021	92	1278.183	1189.423	3.21	6075.946
Log Covid-19 deaths per million people in 2020 and 2021	92	6.383	1.622	1.166	8.712
Net-Gini Index in 2015 to 2019	92	37.641	8.801	22.7	65.1
Palma ratio in 2012 to 2019	92	2.417	1.851	.823	10.261
GDP per capita in 2019 (constant $US 2015)	92	18014.44	20328.26	401.3927	88413.19
Log GDP per capita in 2019	92	9.086	1.318	5.995	11.39
GDP per capita growth rate in 2019	92	1.952	2.369	-7.469	7.382
Population growth rate in 2019	92	.999	1.037	-1.61	3.542
Covid 19 monetary government relief (% of GDP) in 2020 and 2021	92	6.371	5.387	.028	25.502
Unemployment rate in 2019	92	6.545	4.952	.5	28.47
Consumer Price Index in 2019	92	157.496	74.762	99.547	508.339
Youth Bulge in 2019 (number of people 15- to 24-year-old as % of total population)	92	14.564	3.94	8.462	21.605
Natural rents as % of GDP in 2019	92	3.29	5.351	0	26.194
International migrants as % of population as average in 2015 to 2019	92	8.408	11.953	.071	88.404
Ethnic Fractionalization Index in 2013	92	.427	.247	.019	.883
Institutional Quality in 2019 (WGI average)	92	.241	.83	-1.214	1.759
Democratization in 2018 (PolityIV Index)	92	6.109	5.188	-10	10
Internal conflict risk in 2019 (ICRG index)	92	3.755	1.14	1	6.292

Notes: Statistics are based on the observations in the specification of Table 1 and 2, Column 5.

Table A2. List of countries.

Albania	Denmark	Kazakhstan	Poland	Ukraine
Angola	Dominican Republic	Kenya	Portugal	United Arab Emirates
Armenia	Ecuador	Korea, Republic	Romania	United Kingdom
Australia	Egypt	Latvia	Russia	United States
Austria	El Salvador	Lithuania	Saudi Arabia	Uruguay
Bangladesh	Estonia	Malawi	Serbia	Vietnam
Belarus	Ethiopia	Malaysia	Sierra Leone	Zambia
Belgium	Finland	Mexico	Singapore	Zimbabwe
Bolivia	Gabon	Moldova	Slovakia	
Botswana	Gambia	Mongolia	Slovenia	
Brazil	Germany	Myanmar	South Africa	
Bulgaria	Ghana	Namibia	Spain	
Canada	Greece	Netherlands	Sri Lanka	
Chile	Honduras	New Zealand	Sweden	
China, Peoples' Rep.	Hungary	Nigeria	Switzerland	
Colombia	Indonesia	Norway	Tanzania	
Costa Rica	Ireland	Pakistan	Thailand	
Cote d'Ivoire	Israel	Panama	Togo	
Croatia	Italy	Paraguay	Tunisia	
Cyprus	Jamaica	Peru	Turkey	
Czech Republic	Japan	Philippines	Uganda	

Notes: Table A2. List of countries contained in Table 1 and 2, Column 5. Observations = 92. Sub-Saharan Africa: 18 countries, South Asia: 3 countries, North America: 2 countries, Middle East & North Africa: 5 countries, Latin America & Caribbean: 15 countries, Europe & Central Asia: 36 countries, East Asia & Pacific: 13 countries.

Table A3. Competing interaction terms and internal conflict change.

	Dependent variable: Internal conflict risk change (%)				
Explanatory variables	(1)	(2)	(3)	(4)	(5)
Log Covid deaths per capita (C19 deaths)	-9.882	1.031	1.561	-1.118	-4.981
	(7.704)	(1.135)	(1.861)	(1.377)	(9.196)
Net-Gini Index 2015 to 2019 (Gini)	-.042	-.081	-.08	-.168	-.092
	(.409)	(.412)	(.409)	(.415)	(.436)
C19 deaths * Gini	**.475****	**.466****	**.441****	**.489****	**.578****
	(.217)	**(.209)**	**(.208)**	**(.226)**	**(.249)**
C19 deaths * GDP p.c.	1.11				.586
	(.829)				(.91)
C19 deaths * Natural Rents		-.239			-1.45
		(.2)			(.332)
C19 deaths * Ethnic fractionalization			-2.845		-2.236
			(3.674)		(4.915)
C19 deaths * Democratozation				.285	.265
				(.2)	(.219)
Log GDP per capita 2019	2.961	2.311	3.339	2.433	2.705
	(3.139)	(3.266)	(3.337)	(3.118)	(2.555)
GDP per capita growth rate 2019	.611	.739	.76	.61	.515
	(.83)	(.836)	(.824)	(.793)	(.807)
Population growth rate 2019	3.984	4.789*	3.785	4.173	4.494
	(2.59)	(2.785)	(2.596)	(2.676)	(2.924)
Covid 19 monetary government relief	.596**	.642**	.667**	.708***	.611**
	(.258)	(.276)	(.268)	(.256)	(.267)
Unemployment rate 2019	-.394	-.522	-.468	-.503	-.455
	(.462)	(.46)	(.446)	(.454)	(.465)
Consumer Price Index 2019	.007	-.001	.006	.012	.011
	(.024)	(.023)	(.023)	(.025)	(.027)
Youth Bulge 2019	-.255	-.308	-.108	.151	-.058
	(1.027)	(1.034)	(1.001)	(1.016)	(1.071)
Natural rents % of GDP 2019	-.042	-.293	-.092	.005	.145
	(.329)	(.358)	(.33)	(.293)	(.332)
Migrants as % of population 2015 to 2019	-.313**	-.35**	-.407***	-.339**	-.309*
	(.152)	(.141)	(.15)	(.138)	(.173)
Ethnic Fractionalization 2013	4.99	5.877	8.266	7.561	5.856
	(7.125)	(7.493)	(6.97)	(7.139)	(7.983)
Institutional Quality 2019 (WGI average)	-7.003	-6.821	-6.493	-6.446	-6.846
	(4.923)	(4.898)	(4.923)	(5.032)	(4.839)
Democratization 2018 (PolityIV)	.496	.371	.312	.681*	6.2*
	(.355)	(.328)	(.363)	(.366)	(.354)
Internal conflict risk 2019 (ICRG index)	-1.857	-1.826	-2.229	-2.036	-1.823)
	(1.782)	(1.718)	(1.808)	(1.767)	(1.848)
Observations	92	92	92	92	92
	.349	.351	.341	.361	..376
Estimation technique	OLS	OLS	OLS	OLS	OLS

*Notes: Constant not reported. Heteroskedasticity robust standard errors are in parentheses. All columns use demeaned values of log Covid-19 deaths per capita; the Covid-19 case fatality rate and the Gini Index to make their direct effect interpretable. See the explanation in Section 4.1. *** p<0.01, ** p<0.05, * p<0.1. OLS=Ordinary Least Squares.*

Table A4. Competing interaction terms and the number of internal conflict events.

	Dependent variable: No. of conflict events				
Explanatory variables	(1)	(2)	(3)	(4)	(5)
Log Covid deaths per capita (C19 deaths)	-1.07 (.718)	-.227 (.169)	-.346 (.304)	-.162 (.141)	-1.938** (.954)
Net-Gini Index 2015 to 2019 (Gini)	.02 (.029)	.013 (.028)	.012 (.028)	.014 (.029)	.028 (.029)
C19 deaths * Gini	**.039***** **(.012)**	**.034***** **(.012)**	**.036***** **(.012)**	**.035***** **(.012)**	**.039***** **(.012)**
C19 deaths * GDP p.c.	.099 (.078)				.165* (.092)
C19 deaths * Natural Rents		.013 (.018)			.008 (.023)
C19 deaths * Ethnic fractionalization			.346 (.468)		.564 (.499)
C19 deaths * Democratozation				-.004 (.014)	-.013 (.017)
Log GDP per capita 2019	1.048*** (.304)	1.099*** (.298)	1.036*** (.32)	1.103*** (.309)	.963*** (.32)
GDP per capita growth rate 2019	-.007 (.075)	.007 (.073)	.011 (.072)	.012 (.074)	-.006 (.077)
Population growth rate 2019	.246 (.281)	.163 (.28)	.224 (.282)	.201 (.272)	.311 (.305)
Covid 19 monetary government relief	.032 (.03)	.039 (.03)	.042 (.03)	.038 (.031)	.034 (.029)
Unemployment rate 2019	.007 (.037)	-.004 (.038)	-.012 (.041)	-.002 (.038)	-.003 (.04)
Consumer Price Index 2019	.004* (.002)	.004* (.002)	.003* (.002)	.003* (.002)	.003 (.003)
Youth Bulge 2019	-.191** (.079)	-.162** (.082)	-.17** (.082)	-.171** (.084)	-.22** (.091)
Natural rents % of GDP 2019	-.055* (.029)	-.041 (.038)	-.049* (.029)	-.056* (.029)	-.043 (.044)
Migrants as % of population 2015 to 2019	-.041* (.023)	-.05** (.021)	-.046** (.021)	-.05** (.023)	-.038* (.021)
Ethnic Fractionalization 2013	-.079 (.803)	.277 (.716)	.128 (.739)	.266 (.733)	-.423 (.856)
Institutional Quality 2019 (WGI average)	-1.497*** (.534)	-1.471*** (.533)	-1.544*** (.535)	-1.491*** (.533)	-1.603*** (.542)
Democratization 2018 (PolityIV)	.109** (.043)	.113** (.045)	.123** (.052)	.101** (.047)	.135** (.064)
Internal conflict risk 2019 (ICRG index)	.935*** (.149)	.894*** (.149)	.908*** (.148)	.9*** (.149)	.954*** (.151)
Observations	92	92	92	92	92
	.068	.067	.067	.067	..069
Estimation technique	NB	NB	NB	NB	NB

*Notes: Constant not reported. Heteroskedasticity robust standard errors are in parentheses. All columns use demeaned values of log Covid-19 deaths per capita; the Covid-19 case fatality rate and the Gini Index to make their direct effect interpretable. See the explanation in Section 4.1. *** p<0.01, ** p<0.05, * p<0.1 . NB=Negative binomial regression.*

Table A5. Differentiating the effect for different types on conflict.

	Dependent variable: No. of conflict events directly related to Covid-19			*Dependent variable: No. of conflict events (un)indirectly related to Covid-19*		
Explanatory variables	(1)	(2)	(3)	(4)	(5)	(6)
Log Covid deaths per capita (C19 deaths)	-.029 (.146)	-.106 (.16)	-.03 (.171)	-.19 (.148)	.051 (.145)	-.336*** (.119)
Net-Gini Index 2015 to 2019 (Gini)	.001 (.029)	.013 (.059)	.007 (.027)	.017 (.031)	-.062** (.027)	.02 (.031)
C19 deaths * Gini	**.027* (.016)**	**.01 (.019)**	**.025* (.013)**	**.036*** (.013)**	**.024 (.019)**	**.036** (.015)**
Log GDP per capita 2019	1.158*** (.326)	1.621*** (.354)	.807** (.379)	1.047*** (.315)	1.601*** (.382)	.928*** (.343)
GDP per capita growth rate 2019	-.024 (.075)	-.106 (.118)	-.071 (.073)	.014 (.073)	.08 (.056)	.015 (.072)
Population growth rate 2019	.325 (.336)	-.659** (.311)	.267 (.341)	.193 (.277)	-.364 (.33)	.263 (.322)
Covid 19 monetary government relief	.148*** (.038)	.072** (.029)	.121*** (.035)	.018 (.03)	.026 (.033)	.036 (.038)
Unemployment rate 2019	-.015 (.035)	.013 (.043)	-.019 (.036)	0 (.039)	.06 (.036)	.003 (.042)
Consumer Price Index 2019	0 (.003)	-.002 (.003)	-.002 (.003)	.004* (.002)	.006** (.003)	.004* (.002)
Youth Bulge 2019	-.171** (.078)	-.059 (.09)	-.157* (.094)	-.17** (.084)	-.101 (.106)	-.122 (.093)
Natural rents % of GDP 2019	-.069* (.04)	-.082* (.046)	-.024 (.037)	-.048* (.029)	-.121** (.055)	-.043 (.029)
Migrants as % of population 2015 to 2019	-.059*** (.021)	-.024 (.02)	-.077* (.041)	-.05** (.021)	-.035 (.029)	-.07*** (.02)
Ethnic Fractionalization 2013	.262 (.7)	1.229 (.906)	1.003 (.863)	.259 (.742)	1.49* (.794)	1.059 (.896)
Institutional Quality 2019 (WGI average)	-1.499** (.653)	- 1.687*** (.433)	-1.055* (.597)	-1.375** (.541)	-2.016*** (.504)	-1.184** (.513)
Democratization 2018 (PolityIV)	.084* (.051)	.095 (.075)	.091* (.047)	.103** (.042)	.114*** (.038)	.107** (.042)
Internal conflict risk 2019 (ICRG index)	.75*** (.182)	.457*** (.157)	.647*** (.195)	.934*** (.152)	.732*** (.187)	.843*** (.168)
Estimation technique	NB	Poisson	OLS	NB	Poisson	OLS
Pseudo R^2 / R^2	.089	.735	0.65	.067	.7	.67
Observations	92	92	90	92	92	92

*Notes: Constant not reported. Heteroskedasticity robust standard errors are in parentheses. All columns use demeaned values of log Covid-19 deaths per capita and the Gini Index to make their direct effect interpretable. See the explanation in Section 4.1. All Columns include regional dummies. *** p<0.01, ** p<0.05, * p<0.1. OLS=Ordinary Least Squares, NB=Negative Binomial Regression.*

Table A6. The effect of Covid deaths and income inequality on conflict intensity.

	Dependent variable: Conflict events per 100.000 people		
Explanatory variables	(1)	(2)	(3)
Log Covid deaths per capita (C19 deaths)	-1.31	-.637	1.119
	(.96)	(1.057)	(1.261)
Net-Gini Index 2015 to 2019 (Gini)	.061	.006	-.25
	(.236)	(.215)	(.173)
C19 deaths * Gini	**.191***	**.199****	**.205***
	(.111)	**(.097)**	**(.113)**
Log GDP per capita 2019	-2.11		1.505
	(3.279)		(2.476)
GDP per capita growth rate 2019	.398		.765
	(.706)		(.805)
Population growth rate 2019	4.402		2.183
	(2.867)		(2.211)
Covid 19 monetary government relief	-.52**	-.667**	-.39**
	(.259)	(.313)	(.187)
Unemployment rate 2019	.544		.402
	(.344)		(.285)
Consumer Price Index 2019	.022		.023
	(.023)		(.02)
Youth Bulge 2019	-.855		-.605
	(.904)		(.725)
Natural rents % of GDP 2019	-.457		-.467**
	(.282)		(.237)
Migrants as % of population 2015 to 2019	.346*	-.035	.222
	(.204)	(.11)	(.149)
Ethnic Fractionalization 2013	-14.674		-11.239
	(9.723)		(7.105)
Institutional Quality 2019 (WGI average)	-4.46		-5.98*
	(4.293)		(3.609)
Democratization 2018 (PolityIV)	.975***	.407	.798**
	(.352)	(.257)	(.317)
Internal conflict risk 2019 (ICRG index)	1.954		1.238
	(1.519)		(1.127)
Estimation technique	OLS	OLS	2SLS
R^2	.46	.306	.367
Minimum Eigenvalue (CV in parenthesis)	/	/	14.574 (7.03)
First-stage f-statistics	/	/	18.034; 39.3
Regional effects	YES	YES	NO
Observations	92	92	92

*Notes: Constant not reported. Heteroskedasticity robust standard errors are in parentheses. All columns use demeaned values of log Covid-19 deaths per capita and the Gini Index to make their direct effect interpretable. See the explanation in Section 4.1. *** p<0.01, ** p<0.05, * p<0.1. OLS=Ordinary Least Squares, 2SLS=Two Stage Least Squares CV= Critical value for 10% relative maximum 2SLS bias.*

Table A7. Internal conflict, Covid-19 deaths and the moderating role of the Palma Ratio.

Explanatory variables	*Dependent variable: Internal conflict risk change (%)* (1)	*Dependent variable: Number of conflict events* (2)
Log Covid deaths per capita (C19 deaths)	.518	-.223
	(1.064)	(.143)
Palma ratio 2012 to 2019 (Palma)	1.881	.381***
	(1.527)	(.117)
C19 deaths * Palma	**1.554****	**.162*****
	(.695)	**(.054)**
Log GDP per capita 2019	1.433	1.115***
	(3.118)	(.286)
GDP per capita growth rate 2019	1.178	.099
	(1.02)	(.071)
Population growth rate 2019	4.028*	.323
	(2.32)	(.287)
Covid 19 monetary government relief	.813***	.052*
	(.282)	(.029)
Unemployment rate 2019	-.584	-.036
	(.429)	(.033)
Consumer Price Index 2019	.02	.005***
	(.031)	(.002)
Youth Bulge 2019	.165	-.181**
	(.888)	(.079)
Natural rents % of GDP 2019	.051	-.063**
	(.333)	(.029)
Migrants as % of population 2015 to 2019	-.31**	-.047***
	(.146)	(.017)
Ethnic Fractionalization 2013	6.474	.141
	(6.601)	(.662)
Institutional Quality 2019 (WGI average)	-3.839	-1.505***
	(4.573)	(.505)
Democratization 2018 (PolityIV)	.57*	.143***
	(.338)	(.04)
Internal conflict risk 2019 (ICRG index)	-1.727	.962***
	(1.814)	(.15)
R^2 / Pseudo- R^2	.313	.069
Estimation technique	OLS	NB
Observations	91	91

*Notes: Constant not reported. Heteroskedasticity robust standard errors are in parentheses. All columns use demeaned values of log Covid-19 deaths per capita and the Gini Index to make their direct effect interpretable. See the explanation in Section 4.1. *** p<0.01, ** p<0.05, * p<0.1. OLS=Ordinary Least Squares, NB=Two Stage Least Squares.*

Table A8. Results using the Gini Index from the WIID (UN, 2022b).

Explanatory variables	*Dependent variable: Internal conflict risk change (%)* (1)	*Dependent variable: Number of conflict events* (2)
Log Covid deaths per capita (C19 deaths)	.388	-.208
	(1.012)	(.137)
Net-Gini Index 2012 to 2019 from WIID (Gini-WIID)	.319	.073**
	(.398)	(.035)
C19 deaths * Gini-WIID	**.282***	**.033****
	(.143)	**(.013)**
Log GDP per capita 2019	1.357	1.032***
	(3.078)	(.294)
GDP per capita growth rate 2019	1.231	.083
	(1.088)	(.073)
Population growth rate 2019	3.936	.269
	(2.441)	(.262)
Covid 19 monetary government relief	.817***	.043
	(.288)	(.028)
Unemployment rate 2019	-.552	-.032
	(.457)	(.041)
Consumer Price Index 2019	.019	.005**
	(.031)	(.002)
Youth Bulge 2019	-.008	-.191**
	(1.04)	(.088)
Natural rents % of GDP 2019	-.013	-.059*
	(.338)	(.031)
Migrants as % of population 2015 to 2019	-.324**	-.049***
	(.141)	(.017)
Ethnic Fractionalization 2013	7.785	.273
	(6.929)	(.697)
Institutional Quality 2019 (WGI average)	-3.934	-1.332**
	(4.525)	(.52)
Democratization 2018 (PolityIV)	.542	.136***
	(.381)	(.042)
Internal conflict risk 2019 (ICRG index)	-2.149	.936***
	(1.77)	(.146)
R^2 / Pseudo- R^2	0.3133	0.0683
Estimation technique	OLS	NB
Observations	91	91

*Notes: Constant not reported. Heteroskedasticity robust standard errors are in parentheses. All columns use demeaned values of log Covid-19 deaths per capita and the Gini Index to make their direct effect interpretable. See the explanation in Section 4.1. *** p<0.01, ** p<0.05, * p<0.1. OLS=Ordinary Least Squares, NB=Two Stage Least Squares.*

Table A9. Results using the Case Fertility Rate as the measure of Covid-19 affectedness.

	Dependent variable: Internal conflict risk change (%)		*Dependent variable: No. of conflict events*	
Explanatory variables	(1)	(2)	(3)	(4)
Log Covid deaths per capita (C19 deaths)	.285 (1.026)		-.176 (.142)	
Covid-19 Case fatality rate deaths per capita (CFR)		.008 (1.322)		.259 (.172)
Net-Gini Index 2015 to 2019 (Gini)	-.109 (.402)	-.064 (.43)	.013 (.029)	-.026 (.029)
C19 deaths * Gini	**.424**** **(.203)**		**.036***** **(.012)**	
CFR * Gini		.322** (.14)		-.016 (.024)
Log GDP per capita 2019	2.8 (3.181)	2.081 (3.235)	1.095*** (.306)	1.19*** (.314)
GDP per capita growth rate 2019	.769 (.831)	.905 (.91)	.009 (.072)	.03 (.067)
Population growth rate 2019	3.952 (2.592)	.799 (2.624)	.202 (.273)	-.004 (.253)
Covid 19 monetary government relief	.691*** (.26)	.816*** (.272)	.038 (.031)	.059* (.031)
Unemployment rate 2019	-.488 (.456)	.06 (.351)	-.002 (.038)	.059* (.033)
Consumer Price Index 2019	.004 (.023)	.013 (.027)	.003* (.002)	.005** (.002)
Youth Bulge 2019	-.105 (1.001)	.764 (1.012)	-.168** (.081)	-.033 (.084)
Natural rents % of GDP 2019	-.058 (.324)	.054 (.273)	-.054* (.028)	-.046* (.028)
Migrants as % of population 2015 to 2019	-.381*** (.141)	-.339** (.144)	-.048** (.021)	-.051** (.021)
Ethnic Fractionalization 2013	7.56 (7.019)	10.531 (7.182)	.245 (.718)	.699 (.684)
Institutional Quality 2019 (WGI average)	-6.52 (4.973)	-3.671 (4.805)	-1.49*** (.532)	-.86 (.546)
Democratization 2018 (PolityIV)	.404 (.349)	.367 (.344)	.105** (.041)	.053 (.041)
Internal conflict risk 2019 (ICRG index)	-2.124 (1.749)	-2.342 (1.84)	.902*** (.148)	.791*** (.143)
Estimation technique	OLS	OLS	NB	NB
Regional effects	YES	YES	YES	YES
R^2 / Pseudo R^2	.337	.291	.067	.065
Observations	92	92	92	92

*Notes: Constant not reported. Heteroskedasticity robust standard errors are in parentheses. All columns use demeaned values of log Covid-19 deaths per capita; the Covid-19 case fatality rate and the Gini Index to make their direct effect interpretable. See the explanation in Section 4.1. *** p<0.01, ** p<0.05, * p<0.1. OLS=Ordinary Least Squares, NB=Negative binomial regression.*

Table A10. Results for a wider sample of countries.

	Dependent variable: Internal conflict risk change (%)		*Dependent variable: No. of conflict events*	
Explanatory variables	(1)	(2)	(3)	(4)
Log Covid deaths per capita (C19 deaths)	.285 (1.026)	.729 (1.041)	-.176 (.142)	-.151 (.165)
Net-Gini Index 2015 to 2019 (Gini)	-.109 (.402)	-033 (.333)	.013 (.029)	.009 (014)
C19 deaths * Gini	**.424****	**.367****	**.036*****	**.034****
	(.203)	**(.183)**	**(.012)**	**(.014)**
Log GDP per capita 2019	2.8	2.376	1.095***	.739***
	(3.181)	(3.288)	(.306)	(.273)
GDP per capita growth rate 2019	.769 (.831)	.594 (.754)	.009 (.072)	-.078 (.061)
Population growth rate 2019	3.952	6.298***	.202	.037
	(2.592)	(2.372)	(.273)	(.243)
Covid 19 monetary government relief	.691*** (.26)	.629** (.25)	.038 (.031)	.036 (.032)
Unemployment rate 2019	-.488	-.462	-.002	-.002
	(.456)	(.417)	(.038)	(.035)
Consumer Price Index 2019	.004		.003*	
	(.023)		(.002)	
Youth Bulge 2019	-.105	-.259	-.168**	-.152*
	(1.001)	(1.001)	(.081)	(.088)
Natural rents % of GDP 2019	-.058		-.054*	
	(.324)		(.028)	
Migrants as % of population 2015 to 2019	-.381*** (.141)	-.288** (.113)	-.048** (.021)	-.064*** (.018)
Ethnic Fractionalization 2013	7.56		.245	
	(7.019)		(.718)	
Institutional Quality 2019 (WGI average)	-6.52 (4.973)	-7.735* (4.235)	-1.49*** (.532)	-1.03** (.454)
Democratization 2018 (PolityIV)	.404 (.349)	.51 (.345)	.105** (.041)	.092** (.041)
Internal conflict risk 2019 (ICRG index)	-2.124 (1.749)	-2.155 (1.525)	.902*** (.148)	.945*** (.138)
Estimation technique	OLS	OLS	NB	NB
Observations	92	100	92	100
Regional effects	YES	YES	YES	YES
R^2 / Pseudo R^2	.337	.276	.067	.062

*Notes: Constant not reported. Heteroskedasticity robust standard errors are in parentheses. All columns use demeaned values of log Covid-19 deaths per capita; the Covid-19 case fatality rate and the Gini Index to make their direct effect interpretable. See the explanation in Section 4.1. *** p<0.01, ** p<0.05, * p<0.1. OLS=Ordinary Least Squares, NB=Negative binomial regression.*

Table A11. List of countries used to construct the instrument.

Country	Neighbors	Country	Neighbors
Albania	Greece	*Luxemburg*	Germany, Belgium, France
Angola	Namibia, Zambia	*Malawi*	Mozambique, Tanzania, Zambia
Armenia	Iran, Turkey	*Malaysia*	Indonesia, Australia, Philippines
Australia	New Zealand, Malaysia, Philippines	*Malta*	France, Spain, Italy
Austria	Germany, Hungary, Czech Republic, Italy, Slovenia, Slovakia, Switzerland	*Mexico*	United States
Bangladesh	India, Myanmar	*Moldova*	Romania, Ukraine
Belarus	Latvia, Lithuania, Poland, Russia, Ukraine	*Mongolia*	China, Russia
Belgium	Germany, France, Luxemburg, Netherlands	*Mozambique*	Malawi, South Africa, Tanzania, Zambia, Zimbabwe
Bolivia	Argentina, Brazil, Chile, Paraguay, Peru	*Myanmar*	China, Bangladesh, India, Thailand
Botswana	Namibia, South Africa, Zambia, Zimbabwe	*Namibia*	Angola, Botswana, South Africa, Zambia
Brazil	Argentina, Bolivia, Colombia, Paraguay, Peru, Suriname, Uruguay, Venezuela	*Netherlands*	Belgium, Germany
Bulgaria	Greece, Turkey, Romania, Serbia	*New Zealand*	Australia
Canada	United States	*Nigeria*	Gabon, Ivory Coast
Chile	Argentina, Bolivia, Peru	*Norway*	Finland, Sweden, Russia
China	India, Kazakhstan, Vietnam, Myanmar, Pakistan, Russisa, Mogolia	*Pakistan*	India, China, Iran
Colombia	Brazil, Ecuador, Panama, Peru, Venezuela	*Panama*	Colombia, Costa Rica
Costa Rica	Panama	*Paraguay*	Argentina, Bolivia, Brazil
Cote d'Ivoire	Ghana, Liberia	*Peru*	Bolivia, Brazil, Chile, Colombia, Ecuador
Croatia	Hungary, Serbia, Slovenia	*Philippines*	Vietnam, China, India
Cyprus	Turkey, Israel, Egypt	*Poland*	Germany, Czech Republic, Russia, Lithuania, Belarus, Slovakia, Ukraine
Czech Republic	Germany, Poland, Austria, Slovakia	*Portugal*	Spain
Denmark	Germany	*Romania*	Bulgaria, Hungary, Moldova, Serbia, Ukraine
Dominican Republic	Jamaica, Venezuela, Colombia	*Russia*	Belarus, China, Estonia, Finland, Kazakhstan, Latvia, Lithuania, Mongolia, Norway, Poland, Ukraine
Ecuador	Colombia, Peru	*Saudi Arabia*	United Arab Emirates
Egypt	Israel, Zambia	*Serbia*	Bulgaria, Croatia, Hungary, Romania

Table A11. Continued.

Country	Neighbors	Country	Neighbors
El Salvador	Mexico, Panama, Costa Rica	*Sierra Leone*	Liberia
Estonia	Latvia, Russia	*Singapore*	Malaysia
Ethiopia	Kenya, Somalia	*Slovakia*	Austria, Czech Republic, Hungary, Poland, Ukraine
Finland	Sweden, Russia, Norway	*Slovenia*	Austria, Croatia, Italy, Hungary
France	Germany, Belgium, Switzerland, Italy, Spain, Luxemburg	*Somalia*	Ethiopia, Kenya
Gabon	Angola, Nigeria	*South Africa*	Botswana, Mozambique, Namibia, Zimbabwe
Gambia	Sierra Leone, Liberia, Ivory Coast	*Spain*	France, Portugal
Germany	Denmark, Poland, France, Austria, Belgium, Luxemburg, Switzerland, Czech Republic, Netherlands	*Sri Lanka*	India, Malaysia, Bangladesh
Ghana	Ivory Coast, Togo	*Suriname*	Brazil
Greece	Albania, Bulgaria, Turkey	*Sweden*	Finland, Norway
Honduras	El Salvador	*Switzerland*	Austria, France, Germany, Italy
Hong Kong	China	*Taiwan*	China, Taiwan, Philippines
Hungary	Austria, Croatia, Romania, Slovakia, Slovenia, Ukraine, Serbia	*Tanzania*	Kenya, Malawi, Mozambique, Uganda, Zambia
Iceland	Norway, Sweden, United Kingdom	*Thailand*	Malaysia, Myanmar
Indonesia	Malaysia	*Togo*	Ghana
India	China, Bangladesh, Myanmar, Pakistan	*Tunisia*	Egypt, Italy, Spain
Iran	Armenia, Pakistan, Turkey	*Turkey*	Armenia, Greece, Bulgaria, Iran
Ireland	United Kingdom, France, Belgium	*Uganda*	Kenya, Tanzania
Israel	Egypt	*Ukraine*	Russia, Belarus, Moldova, Poland, Romania, Slovakia
Italy	Austria, France, Slovenia, Switzerland	*United Arab Emirates*	Saudi Arabia
Jamaica	Mexico, Dominican Republic	*United Kingdom*	Ireland, France, Belgium
Japan	China, South Korea, Taiwan	*United States*	Canada, Mexico
Kazakhstan	China, Russia	*Uruguay*	Argentina, Brazil
Kenya	Ethiopia, Somalia, Tanzania, Uganda	*Venezuela*	Brazil, Colombia
Korea, Republic	China, Japan, Taiwan	*Vietnam*	China
Latvia	Belarus, Lithuania, Russia, Estomia	*Zambia*	Angola, Botswana, Malawi, Mozambique, Namibia, Tanzania, Zimbabwe
Liberia	Ivory Coast, Sierra Leone	*Zimbabwe*	Botswana, Mozambique, South Africa
Lithuania	Belarus, Latvia, Poland, Russia		

Neighbors are the closest bordering countries. In case of Islands or countries for which no neighbor was included in the sample, I approximate the neighboring cases by using the closest available data points. For instance, Australian neighbor cases are constructed from the cases of New Zealand, Malaysia and the Philippines.

Table A12. Multinomial regressions of the effect of income inequality on potential mediating variables.

	Most people can be trusted		*Trust in people of another nationality*		*Confidence in Government*
Most people can be trusted	-.069*** (.003)	Trust completely	-.04*** (.006)	A great deal	.163*** (.004)
Need to be very careful	Base outcome	Trust somewhat	-.058*** (.003)	Quite a lot	.065*** (.003)
		Do not trust very much	Base outcome	Not very much	Base outcome
Observations	45460	Do not trust at all	.042*** (.003)	None at all	.027*** (.004)
Pseudo R^2	.079	Observations	43986	Observations	44060
		Pseudo R^2	.05	Pseudo R^2	.082

	Perceptions of corruption in the country		*Educational Attainment*
There is no corruption in my country	.034*** (.008)	Early childhood education	.167*** (.007)
2	-.049*** (.008)	Primary education	.108*** (.004)
3	-.098*** (.006)	Lower secondary education	.069*** (.004)
4	-.091*** (.006)	Upper secondary education	Base Outcome
5	-.08*** (.004)	Post-Secondary non-tertiary education	-.06*** (.005)
6	-.092*** (.004)	Short cycle tertiary education	(.005) -.117*** (.005)
7	-.103*** (.004)	Bachelor or equivalent	-.049*** (.004)
8	-.094*** (.004)	Master or equivalent	-.149*** (.007)
9	-.063*** (.004)	Doctoral or equivalent	-.097*** (.012)
There is a great deal of corruption in my country	Base outcome		
Observations	45389	Observations	45707
Pseudo R^2	.047	Pseudo R^2	.076

*Notes: Standard errors are in parentheses. Coefficients describe odds ratios for one unit increases of the average Net-Gini Index, relative to the base outcome. *** p<.01, ** p<.05, * p<.1. Regional dummies included.*

Figure A1. Predicted conflict events directly related to Covid-19 at different levels of Covid deaths and income inequality.

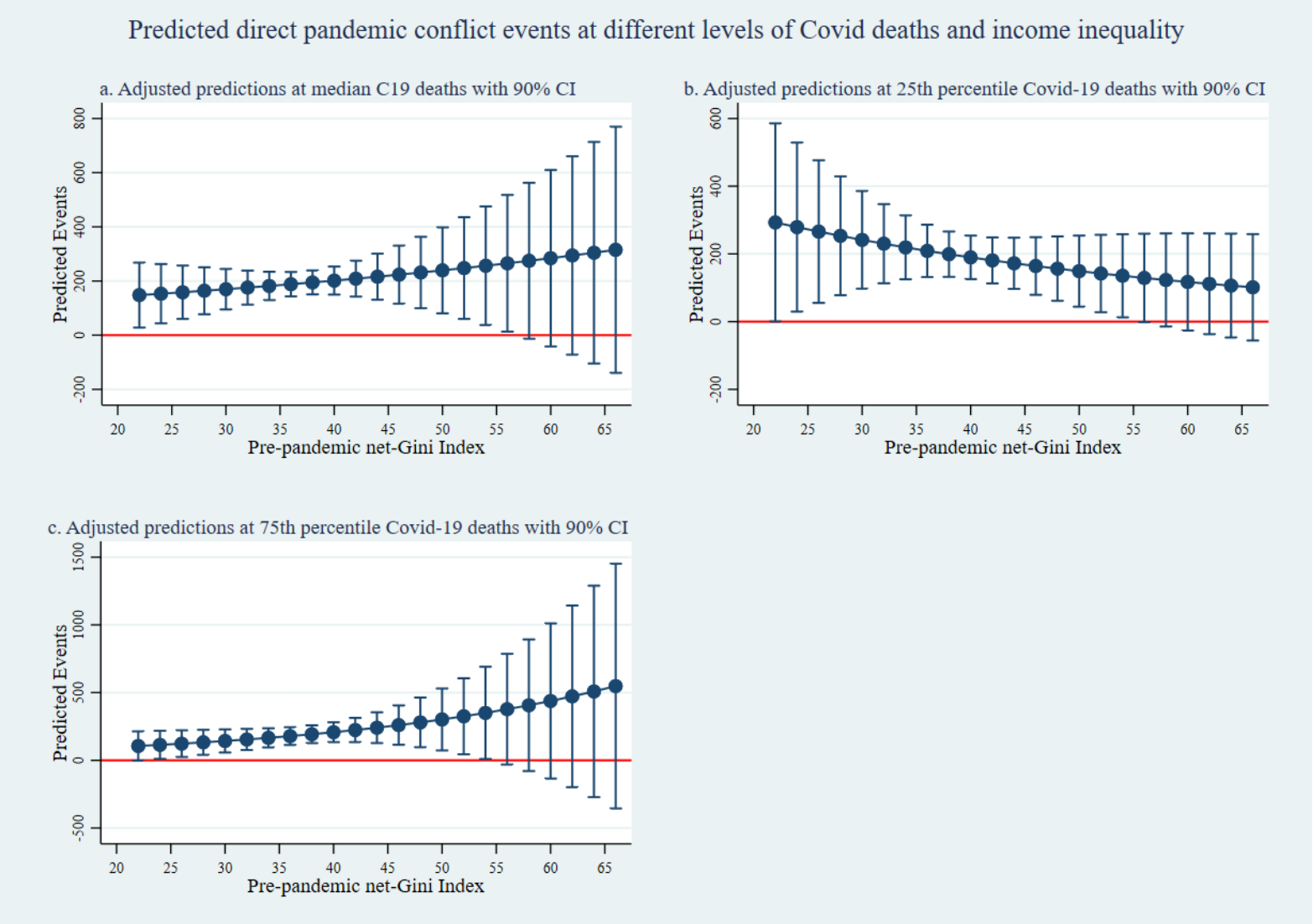


Figure A2. Predicted conflict events indirectly related to Covid-19 at different levels of Covid deaths and income inequality.

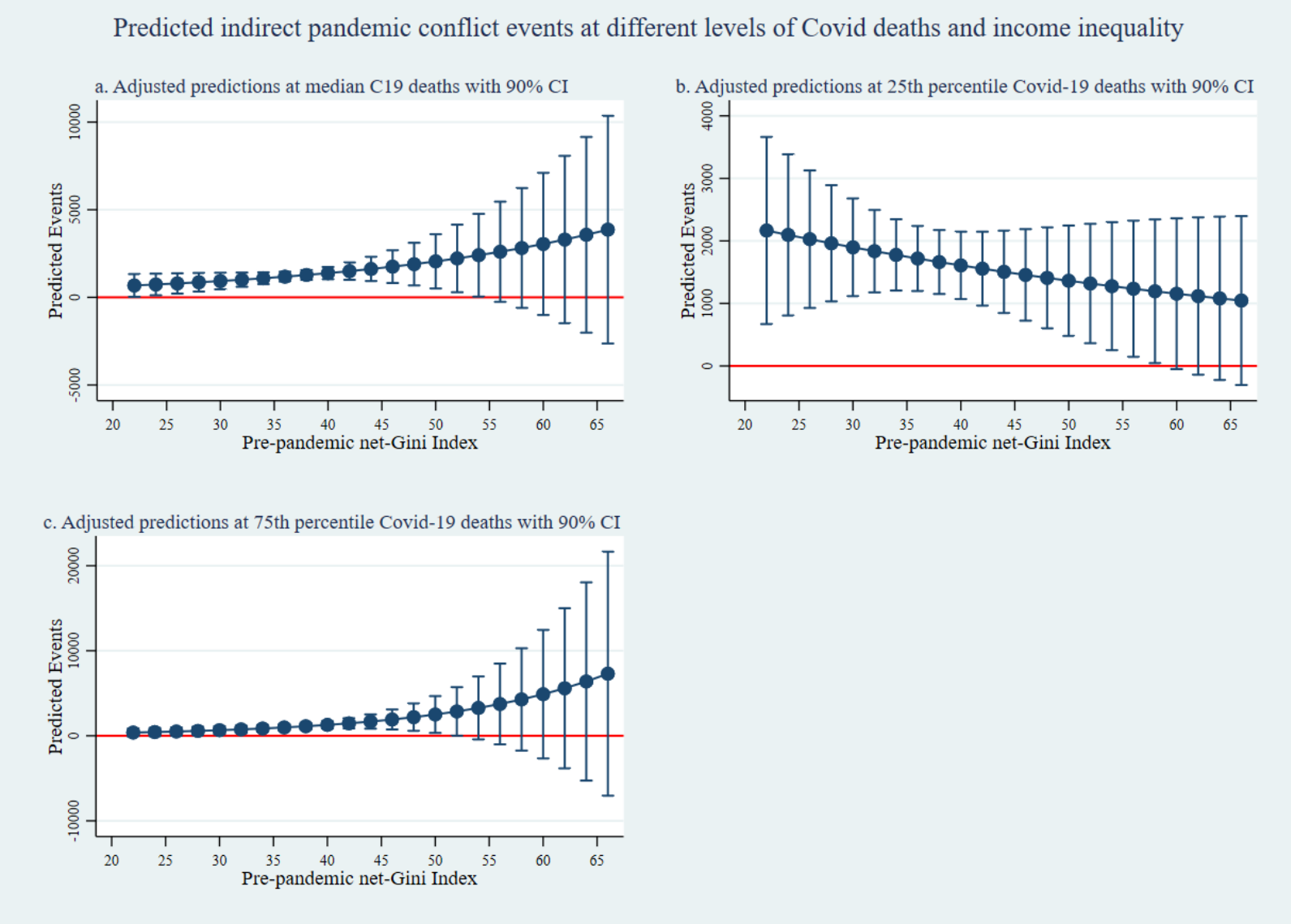
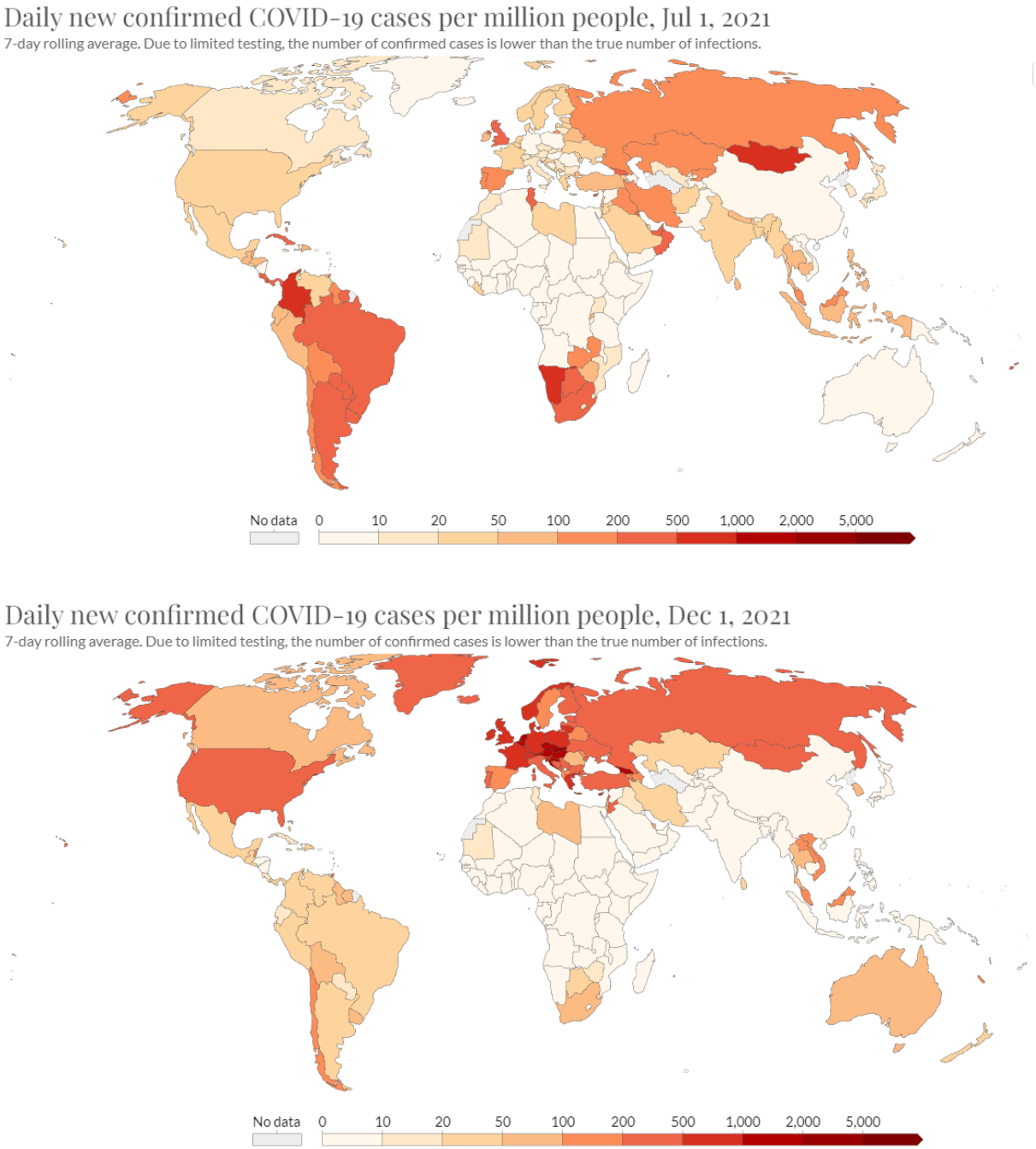
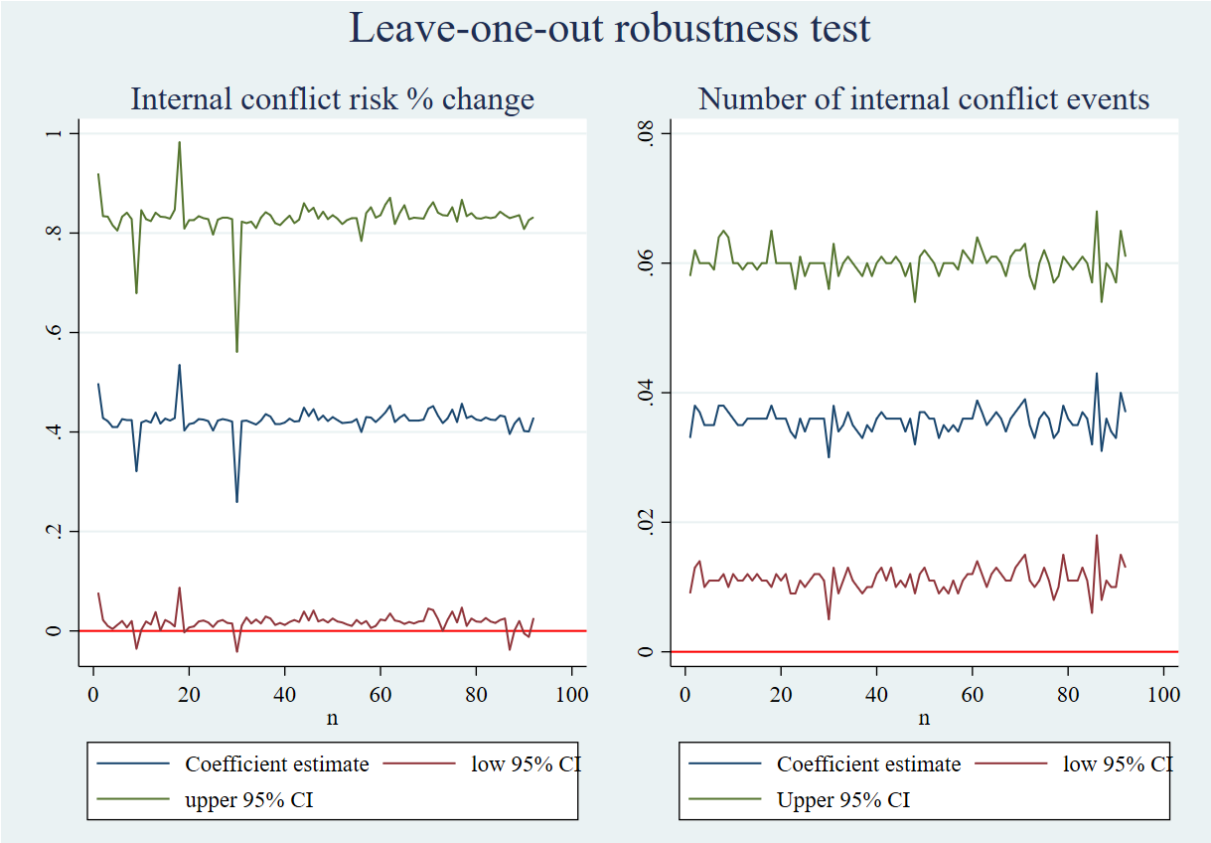


Figure A3. Development of Covid-19 across regions.



Source: OWID (2022) *Coronavirus Pandemic (COVID-19)*. Our World in Data. Available from https://ourworldindata.org/coronavirus.

Figure A4. Leave-one-out robustness test.



Declaration of authorship

By signing this declaration, I confirm that I have completed the present thesis independently, without help from other and without using resources other than indicated and named. All phrases that are taken directly or indirectly from other sources (incl. electronic resources), quoted verbatim or paraphrased are indicated accordingly. I am aware that any violation of this declaration will results in the work being graded as 'failed' (0 grade points, ECTS-Grade F).

Marburg, 22 August 2022