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## **BUILDING BRIDGES TO PEACE: A QUANTITATIVE EVALUATION OF POWER-SHARING AGREEMENTS**

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**POLITICAL ECONOMY**

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# BUILDING BRIDGES TO PEACE: A QUANTITATIVE EVALUATION OF POWER-SHARING AGREEMENTS

## Abstract

Power sharing is a central pillar of armed conflict mediation and de-escalation attempts worldwide and is thus employed as a tool to reduce political violence. In this study we introduce a set of novel empirical methods that aim to explicitly address the challenge of estimating the impact of power sharing agreements. We find that power sharing agreements reduce political violence in the short-term. The average effect of power sharing agreements – of the 440 agreements surveyed – is a 8% decrease in the occurrence of violence and a 18% drop in intensity of armed violence. Political power sharing provisions which are embedded in a comprehensive agreement with other power, judicial and resource-related provisions are most effective. These comprehensive agreements have an effect that is larger (a 10% and a 30% decrease in occurrence and intensity respectively) and appears to strengthen with time. We also argue that power sharing agreements can therefore provide a 'bridge' out of the conflict trap by reducing political violence in the short-term and by strengthening the institutional protection of rights.

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# Building Bridges to Peace: A Quantitative Evaluation of Power-Sharing Agreements

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## 1. Executive summary

**Power sharing** refers to sharing of political power by different groups in the same government, like, for example, in a coalition government. Power sharing is a central pillar of armed conflict mediation and de-escalation attempts worldwide and is thus employed as a tool to reduce political violence. A large majority of **peace agreements** include power sharing provisions, the most recent ones include Libya, the Mindanao agreement and Colombia.

But does power sharing work in practice to reduce political violence? At face value, the news is not good. Almost 90% of power-sharing agreements do not result in a complete halt of political violence, and there is some controversy regarding their role in key cases such as Afghanistan and Iraq, where conflict parties lack political legitimacy amongst the international community.

Quantitative studies should be able to provide answers but have been hampered by the fact that power-sharing agreements are not agreed upon in a political vacuum. They are the result

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of the specific national, regional and geopolitical configurations in which they are agreed upon. Power-sharing agreements are introduced during periods of intense political violence in an explicit attempt to formalise a (re-)distribution of power - one that is more congruent with the actual distribution of power and resources in a given country- and thereby reduces the continuation of violence.

It is a known fact in the conflict literature that countries can fall into the so-called **conflict trap**, which is very difficult to escape (Collier and Sambanis 2002, Rohner and Thoenig 2021, Mueller and Rauh 2022). The trap is characterized by repeated cycles of political violence.<sup>1</sup> Most power-sharing agreements are agreed while the country is inside the trap, with the intent of breaking it. Therefore, it is hard to distinguish the effect of the power sharing agreement from the general political context that generates persistent political violence, i.e., the conflict trap. Without explicit handling of this “endogeneity problem”, the fact that the political risk is causing both the violence and the agreement, any attempt to measure effects lead to a positive correlation of power sharing with political violence.

In this study we therefore introduce a set of empirical methods that explicitly address this challenge by adjusting for the political circumstances and levels of risk under which power-sharing agreements are signed. We define power sharing as the specific divisions and amalgamations of power that ensure groups some sort of equal ‘participation’ in the state’s structures, and/or shared ‘ownership’ of resources. This can take the form of a central government of national unity (“pooling”), a federation (“dispersion”) or the introduction of independent, non-governmental institutions that act as a check on power in the executive (“constraining”). The agreements we study often combine different elements which makes them hard to study separately. We therefore focus on studying all power-sharing agreements and comprehensive agreements separately.

However, we will also try to establish which elements seem to be necessary to secure peace in the longer run. For this we study long-term institutional changes in violence and institutions using the V-Dem dataset. We then analyse the role of comprehensive power-sharing agreements and how they might help constitute a basis for longer-term institutional changes and lasting reductions in political violence.

We provide four key findings:

1. Power-sharing agreements reduce political violence in the short term. The average effect of power-sharing agreements – of the 440 agreements surveyed – is an 8%

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<sup>1</sup> Throughout we use the definition of Uppsala Conflict Data Program (UCDP) for armed political violence. The basic unit of analysis for the UCDP’s Georeferenced Event Dataset (GED) dataset is the “event”. UCDP defines an event as: “An incident where armed force was by an organised actor against another organized actor, or against civilians, resulting in at least 1 direct death at a specific location and a specific date”. See Sundberg et al (2013) and Croicu and Sundberg (2016) for more details.

decrease in the occurrence of violence and an 18% drop in intensity of armed violence. An example of such a short-run effect is Libya, where violence intensity decreased significantly (more than 50%) after the first power sharing agreement was introduced, however violence resurged soon afterwards.

2. Political power sharing provisions which are embedded in a comprehensive agreement with other power, judicial and resource-related provisions are most effective. These comprehensive agreements have an effect that is larger (a 10% and a 30% decrease in occurrence and intensity respectively) and appears to strengthen with time. The 1992 Rome peace agreement for Mozambique illustrates this effect well, with an agreement that helped pacify the country for almost a decade.
3. Power-sharing agreements can also contribute to long-term reductions in violence. Lasting power-sharing agreements are associated with a fall in violence occurrence from 80% to 30% of countries. In Nicaragua, following the conclusion of two power-sharing agreements in 1990, violence intensity decreased by 30%, and this reduction was sustained over time.
4. Political systems with egalitarian political features that respect civil and political rights and provide equal access to justice, public services and jobs are most strongly associated with reductions in violence. The same effect cannot be seen for provisions that strengthen deliberation and participation at the institutional level. Our interpretation is that power-sharing agreements can provide a 'bridge' out of the conflict trap - by reducing political violence in the short term and by strengthening the institutional protection of rights in comprehensive agreements.

Based on the evidence presented in this paper, we propose the following recommendations for international peacemakers and peace researchers:

1. Power-sharing agreements reduce armed political violence. They work best if they are embedded in comprehensive agreements which include provisions aimed at legally constraining the government in addition to provisions which regulate the sharing of political power.
2. Participatory and deliberative elements of democracy are less important for the reduction of political violence than constraining elements, such as access to justice or service delivery. Therefore, international peacemakers should prioritize building state capacity by strengthening access to justice and basic services, rather than democratic capacity via the holding of elections.
3. In the short term, power-sharing agreements need to reflect the underlying "elite bargain", i.e. the informal distribution of power and resources in the given context, as closely as possible in order to avoid its collapse and the resurgence of violence. Mediators and special envoys should invest in gaining an intimate understanding of these configurations of power before engaging in negotiations and avoid "power mismatches" between the formal and informal distributions of power.
4. More research is needed to examine the range of instruments, structures and processes that ensure agreements are implemented and last, both in the short and long term. An examination of the specific causes of success or failure of individual

power-sharing agreements, both in terms of their content, i.e. the lack of inclusion of significant elites/of powerful political individuals and groups at the negotiating table and in the agreement, or the influence of external actors on the conflict parties would be worthwhile. The strong military presence of foreign actors, like in Iraq of Afghanistan, also seems a worthy research topic.

## **2. Why should we care about power sharing?**

Political violence is associated with tremendous human suffering of the directly exposed individuals, population displacements and long-term scarring of the affected economies. If current trends persist, by 2030 more than half of the world's poor will be living in countries affected by high levels of violence (OECD 2015). On average, for every \$1 spent on prevention, up to \$16 can be saved in terms of the cost of conflicts (WB State Fragility, Conflict, and Violence 2020-2025).

In the absence of a military solution, negotiated resolutions of the conflict tend to result in some form of power sharing agreement between conflict protagonists, in most cases with outside help. It is worth asking whether such initiatives can, in theory, be successful. Hörner et al. (2015) study this issue by applying the theory of mechanism design to the study of international conflict resolution. They show that, despite only being capable of making unenforceable recommendations, mediators can be effective as arbitrators. These encouraging findings contrast with recent work by Canidio and Esteban (2021) who show that conflict parties can have incentives to arm themselves more with mediation. As discussed in Blattman (2022), this already indicates that, depending on the underlying reason for conflict, we can be more or less optimistic about the role played by mediated negotiations. Fearon (1995) posits that one of the reasons for political violence is that dynamic shifts in power do not allow for a bargaining solution. For the government it is easier to repress groups in society that are gaining strength and might not be contained in the future than to negotiate and share power. Without a way to commit, violence can break out. Acemoglu and Robinson (2001, 2005) apply this logic to explain the adoption of democratic institutions more generally. They see democratizations as a reaction of the elite to a temporary threat of violence by the majority. The uprising population knows that if it disarms, the government can repress it again, and therefore has an incentive to engage in violence. In this situation, institutional changes can provide a commitment device for the elite. This commitment can solve the dynamic power problem posited by Fearon and avoid violence.

Gates et al. (2016) and Strom et al. (2017) motivate the use of power-sharing agreements along these lines. They first typify three categories of mechanisms through which power sharing works.

1. Pooling of power: representatives of designated parties or groups hold particular offices or participate in particular decision-making processes.

2. Dispersion of power: distribution of authority among groups or regions in a well-defined pattern. “Dispersiveness of small and localized communities find representation” (Storm et al., 2015)
3. Constraining of power: limiting agent’s power (a party or social group) to protect vulnerable groups, increasing the cost of repressing.

According to their study, the third category is most strongly associated with reductions in violence. Gates et al (2016) postulate that power-sharing institutions work best if they constrain governments from abusing less powerful groups and individuals, solving the commitment problem. This provides less incentives for ordinary citizens to join potential insurgents, making conflict less likely.

There are variants of this argument with subtle but important differences. Besley and Persson (2011), for example, stress the role of cohesive institutions. It is this commitment to an even distribution of public resources that reduces violence. If a gain in executive power does not shift the resource allocation, then incentives to fight for executive power are reduced. Besley and Persson (2011) test their ideas using a measure of cohesive institutions and find that increasing cohesiveness indeed stops natural disasters or aid shocks of using the state’s resources for nation-building.

Cheng et al (2018) instead model the state as a *limited access order*. They stress the role played by the rent distribution underpinning peace agreements. In their view, institutions need to reflect the underlying configuration of power and resources. Where this is not satisfied, the incentives to violence increase. However, they also attribute institutions some degree of exogenous power over elite behaviour, by determining the context in which they will make decisions. Elites play a role in shaping the pathway to conflict resolution given the patterns of development, the global/regional contexts, and pre-existing social structures. What is important here is how strong these institutions are. In fluid situations, like in Iraq or Afghanistan, elites will not feel bound by dismantled or collapsing institutions. In other situations, like in Northern Ireland or Indonesia, where state institutions are more durable and some function in consistent ways regardless of who is in charge, elites will feel compelled to act inside the framework of these institutions.

There is a striking disconnect, however, between the academic literature on power sharing and the policy world. In policy circles the dispersion or constraining of power is not referred to as ‘power sharing’. In diplomacy, ‘power sharing’ mainly refers to two or more conflict parties sharing executive power, e.g. in a “government of national unity” (similar to a coalition government). We will follow the academic literature by focusing on a data-driven definition based on “comprehensive” agreements defined in the next section.



### 3. Data description

This section describes the various datasets used in our quantitative analysis of power-sharing agreements. Over the past years, we have seen a significant improvement in the possibilities of examining this topic, due to the development of four datasets: fine-grained data on armed political violence and data on power-sharing agreements, data on political institutions and forecasts of conflict risks at the monthly level. We discuss these in turn.

Our goal is to conduct a study of monthly data for as many countries as possible reaching as long back in time as possible. As a result of this ambition, we restrict our analysis to a combination of the Uppsala Conflict Data Program (UCDP) (Sundberg and Melsnder 2013, Davies et al 2022) to measure armed political violence. We aggregate the Georeferenced Event Dataset (GED) at the country/month level summing over all types of fatalities. This gives us a dataset from 1989-2021 for over 170 countries.

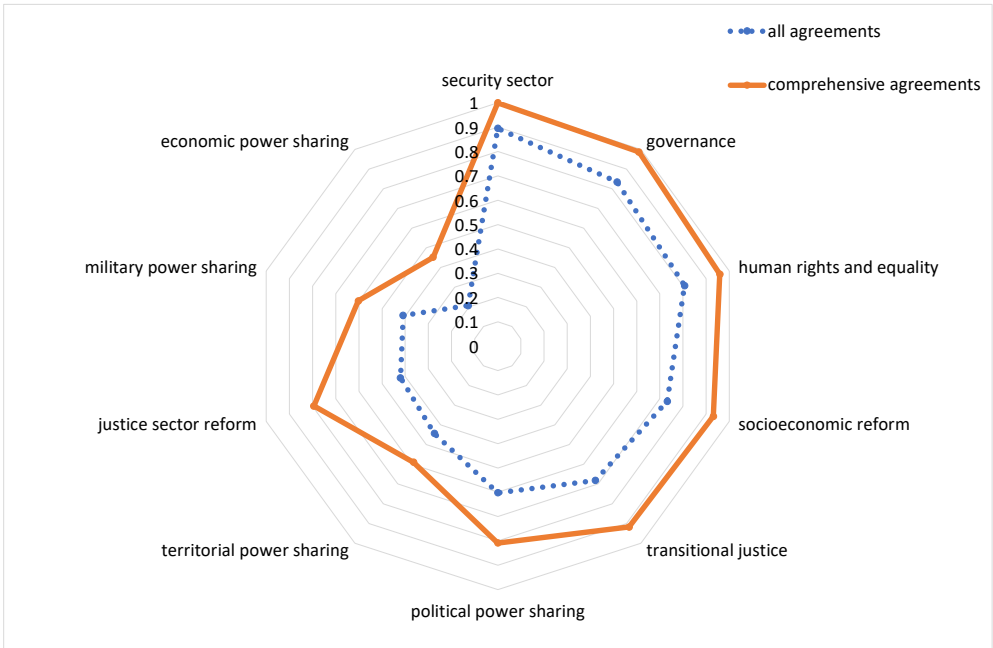
We combine the resulting data with the PA-X Peace Agreement Database (Bell et al 2021) and Bell and Badanjak (2021) to capture power-sharing agreements. The PA-X dataset codes all peace agreements in the period between 1989 and 2020. A peace agreement is defined as a formal, publicly available document, produced after discussion with conflict protagonists and agreed to by some or all of them, addressing violence with a view to ending it.

Given the centrality of the PA-X data we discuss it in detail. The PA-X dataset codes different types of power sharing (political, territorial, economic, military). Power sharing refers to the specific divisions and amalgamations of power that ensure groups enjoy some form of equal ‘participation’ in the state’s structures, and/or shared ‘ownership’ of resources. *Political* power-sharing is defined using Lijphart’s criteria, focusing on the establishment of, for instance, an executive grand coalition, the introduction of proportional representation in legislatures, mutual veto (or weighted majorities) in areas of group’s ‘vital interests’, and segmental (by concept, e.g. ‘sport’, ‘education’) autonomy. Given the specific interest of policymakers regarding this definition, we will analyze it separately from other definitions which tend to disperse power instead of sharing it. Territorial power sharing in PA-X is defined as divisions of power on a territorial basis. Economic power sharing is defined as joint participation in economic institutions, or territorial fiscal federalism. Military power sharing refers to provisions which share power in the institutions of the police, army or security ministries.

In our main empirical analysis, we do not distinguish between different types of power sharing, but we analyze them jointly. As a result, we have more than 440 power-sharing agreements in monthly data from 1989-2020 for over 170 countries - more than 70,000 country/month observations. However, in our study we will mostly focus on so-called *comprehensive* agreements which play a special role in changing conflict dynamics. These are defined by PA-X as agreements between parties that are engaged in an ongoing discussion,

manage to agree on substantive issues in a comprehensive attempt to re-solve the respective conflict. When we focus on comprehensive agreements, we have 73 agreements in the data.

Figure 1 shows the composition of all and comprehensive power-sharing agreements along a small subset of the dimensions tracked by PA-X. The dotted, blue line shows the share of elements present in all power-sharing agreements. The solid, orange line shows the share present in comprehensive agreements. The main take-away is that comprehensive agreements have a lot more elements. The orange line runs outside the blue line on all categories. All comprehensive agreements mention the security sector, close to 90% mention human rights and equality, over 80% mention political power sharing and close to 70% mention justice sector reforms. Justice sector reform is also a big outlier in terms of increase in mentions from all to comprehensive agreements.



**Figure 1: Comparison of all vs comprehensive power-sharing agreements**

To analyze long-term institutional changes, we add the Varieties of Democracy (V-Dem) data to capture political institutions. V-Dem is one of the standard datasets in the political science literature on political institutions and it tracks many aspects of these institutions for countries worldwide.

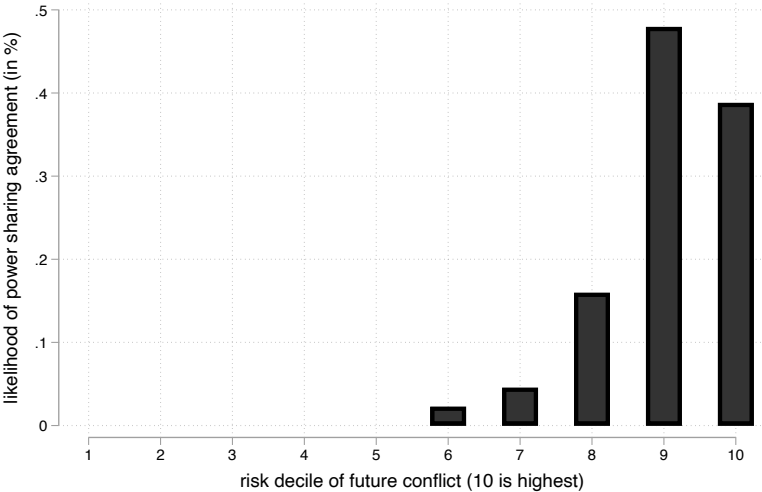
Finally, we use data generated using our methodology at [conflictforecast.org](http://conflictforecast.org) to generate forecasts of future violence outbreaks at the monthly level for the period 2000-2020. Conflictforecast.org provides monthly updates of armed conflict risks at various horizons and definitions of armed conflict using millions of news articles and conflict histories from UCDP. Unless specified otherwise we will rely on a forecast that relies on the UCDP data alone, i.e.

the forecast is using violence dynamics to forecast violence in the next 12 months using a machine learning algorithm. This allows us to study forecasting errors around the adoption of power-sharing agreements. In the following section we discuss the importance of this adaptation for the purpose of this study.

#### 4. Power sharing as an endogenous treatment – An overview

Identifying the impact of power-sharing agreements is complicated by the fact that these agreements are specifically targeted at addressing violence or situations with a lot of future potential for violence.

Figure 2 shows the average propensity of the adoption of a comprehensive power-sharing agreement from PA-X with increasing forecasted risk of an outbreak of violence, taken from conflictforecast.org. The forecast only considers violence dynamics known up to the point of the agreement and, in this way, is not directly affected by the agreement. Clearly, the adoption of a power-sharing agreement is very highly associated with future violence. The baseline likelihood of a power-sharing agreement is extremely low, but it increases to almost 0.5% per month for the highest risk deciles.



**Figure 2: Likelihood of power-sharing agreement by conflict risk decile**

This pattern in the data matches the recounts of diplomats whose actions are motivated, in part, by the prevention of armed violence. It is therefore entirely plausible that policymakers target situations with threatening violence dynamics, i.e. situations where future violence is most likely.

Why is this a problem for identifying the effect of power-sharing agreements? In Figure 3 we illustrate the typical context of peace agreements in directed acyclic graph (DAG). Circles indicate variables and arrows indicate causal relationships. In Figure 3 we are interested in identifying the marked circle – the effect of peace agreements with power-sharing provisions and future violence. However, peace agreements are introduced as a reaction to a specific country context. Often this context is characterized by an active armed conflict which is itself driven by competition over resources or executive power. This competition will independently affect conflict risk, i.e. the risk of armed violence continuing or re-emerging, with or without an agreement in place. But because the peace agreement is, in part, itself a reaction to these factors, it becomes impossible to distinguish the effect of these problems and the effect of the peace agreement. In the jargon of causal inference, the backdoor criterion is violated.

This violation imposes a potential bias of the effect of any study that tries to analyze the effect of power-sharing agreements and violence. If agreements work imperfectly, we will find that agreements are associated with increased violence compared to situations without peace

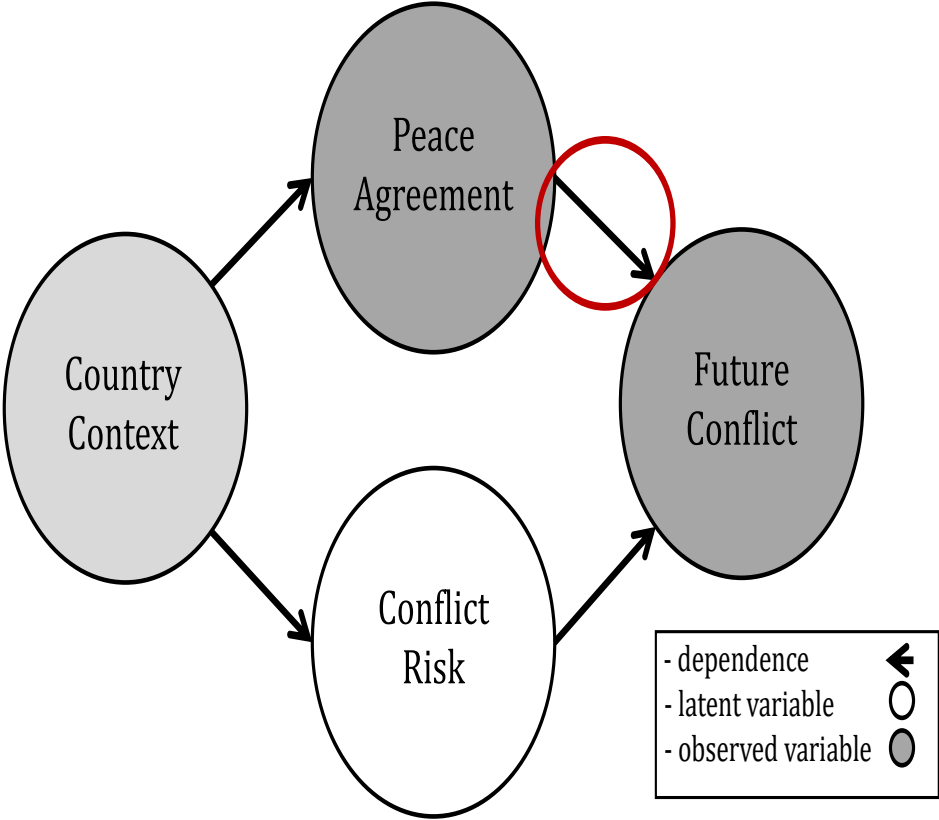


Figure 3: The identification problem

agreements. Blaming peace agreements for violence is then akin to a situation in which a medical treatment to a severe illness is blamed for the following poor health. It is necessary to consider the conditions under which the treatment was administered.

We will follow a standard event study approach to get around this problem. This method looks at violence dynamics before and after the adoption of power-sharing agreements. This controls for factors at the country level that stay constant during the event study window – in our case 6, 12 and 18 months.

However, the availability of an archive of past forecasts of conflict also allows us to control for conflict risk or, put differently, it allows us to track the evolution of forecast errors around the adoption of power-sharing agreements. Given that this method is novel we will discuss it in detail in the following section.

Note, that our approach has limitations which need to be taken into account when interpreting the results. It is likely that other policies, like mediation, foreign aid, or external security controls, are implemented to support the peace agreement we study. If these other policies have an effect and their timing perfectly coincides with the month of the power-sharing agreement, then our method will capture the overall effect of these policies. There is no way for us to take care of this problem and our results should therefore be regarded as the overall evaluation of a policy instruments with its supporting policies in place.

An alternative approach would be to try and find exogenous variation in the policy instrument. Such an approach has, for example, been implemented to study the effect of foreign food aid on armed conflict by Nunn and Qian (2014). The problem with this approach, is that exogenous variation in the policy instrument means that the policy is not endogenous, i.e. it is not demand-driven. But foreign policies which are not driven by local requirements and a demand for intervention by local actors might not be the most effective type of foreign interventions. Estimates are then causally identified but the treatment is a very specific one so that results don't generalize.

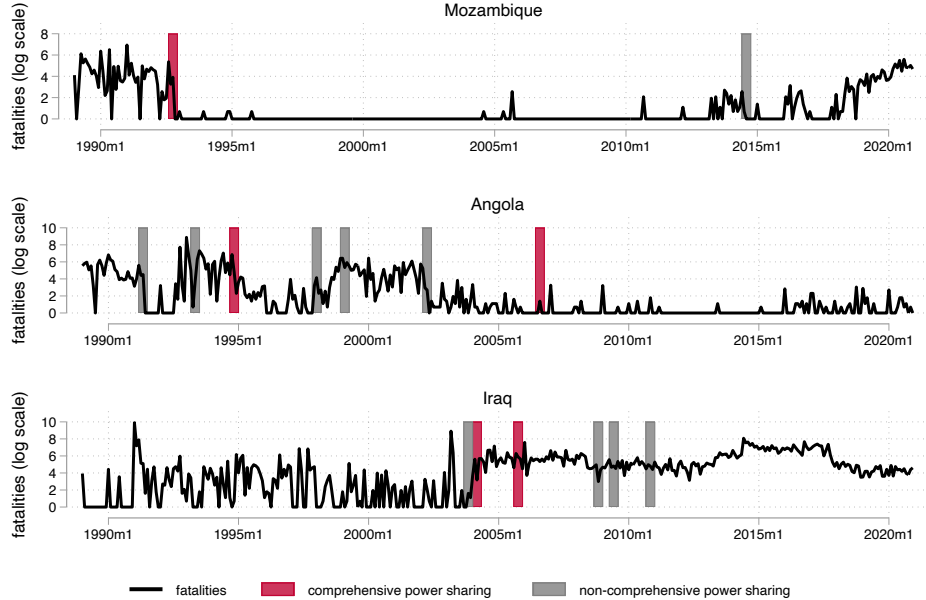
## **5. Results**

### **5.1. Case studies**

We first explore the link between violence and power sharing along the lines of three case studies. These case studies are selected in a completely subjective way and serve to illustrate the aspects discussed above and to motivate our empirical estimation strategy. We will show average, and therefore generalizable, effects in the next section.

Our first case study, shown in the top panel of Figure 4, is Mozambique. The country experienced high levels of political violence in the beginning of the sample with around 150 fatalities per month (5 on the log scale). These levels of violence were the result of the Frelimo-Renamo conflict, which lasted fifteen years (1977-1992) and ended with the 1992 Rome peace agreement. The comprehensive agreement led to the establishment of multiparty elections in 1994. We mark the agreement with a red line - the dramatic drop in violence after the agreement is clearly visible.

Frelimo has won every election since, amidst widespread allegations of fraud and suppression of the opposition. Renamo has maintained an armed guerrilla force, and violence has occasionally erupted between them and the government, such as in 2013 and 2016, although it has never reached the level of intensity previously seen. In reaction to a particularly bloody outbreak in 2014, a new peace agreement was signed. Again, we see a decrease in violence following this agreement. The recent violence in the north of Mozambique involves a violent extremist group (IS) who was not part of the initial power sharing deal.



**Figure 4: Power sharing case studies**

The middle panel of Figure 4 demonstrates the case of Angola. Here the effect of power sharing is less clear with violence levels being higher despite a cycle of seven consecutive power-sharing agreements, two of them comprehensive. In several cases, the number of fatalities decreased dramatically after the adoption of an agreement. For example, in 1991 the UNITA and the MPLA government signed the Bicesse Peace Agreement. The

agreement provided for the establishment of a multi-party system, which allowed presidential elections to be held the following year. In the aftermath there was a brief episode of peace, but violence broke out again in 1992. A slightly longer stabilisation can be observed after the comprehensive agreement in 1994, however, violence is again only de-escalating for a few years.

The case of Angola illustrates clearly how local and external actors use power sharing repeatedly to decrease violence, with actors re-negotiating the distribution of power repeatedly. Sometimes this seems to reduce violence temporarily and at other times there is no effect, which means the content of the PS deal did not sufficiently provide incentives to cease violent competition. Figure 4b also illustrates the endogeneity of agreements well: Once violence recedes, so does the frequency of power sharing attempts.

The history of Angola is therefore aligned with the theoretical model proposed in Figure 3 and the selection analysis shown in Figure 2. Power-sharing agreements are a reaction to a dire situation. The aftermath of power sharing is therefore, on average, still violent, but on a lower level. What is important to note, however, is that often, the months following directly after a power sharing agreement has been agreed, are less violent than the months preceding it. We will return to testing this proposition statistically in the next section.

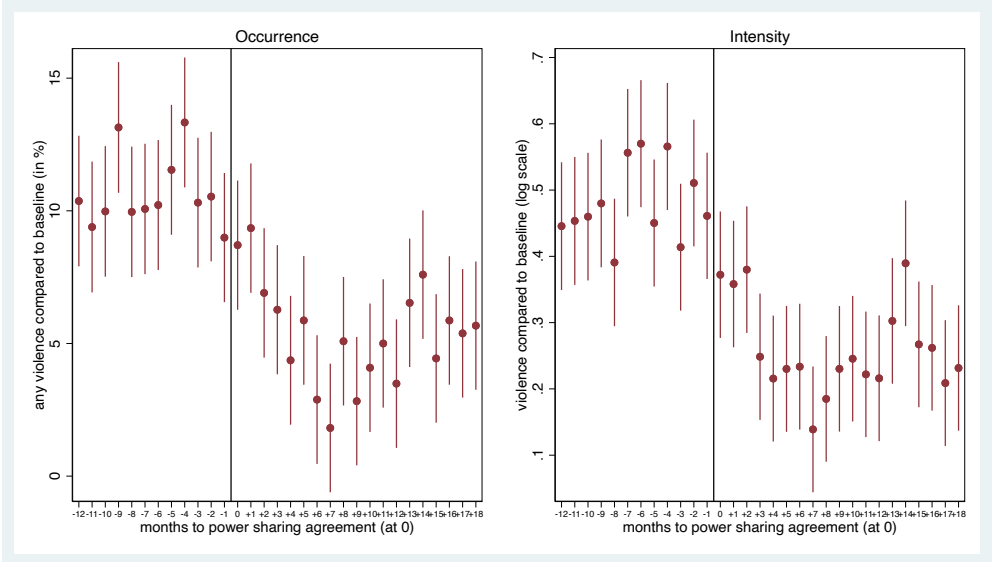
It is important to bear in mind that amongst recorded power-sharing agreements there are quite a few that seem to have had no or even a negative effect. The bottom panel of Figure 4 shows the case of Iraq where the first power sharing agreement was concluded after the US-led Iraq invasion. The agreement preceded a dramatic escalation in violence which the following agreements could not appease. These agreements could also not prevent an insurgency by the Sunni tribes – who were not part of the deal – and who later formed the Islamic State which pushed violent deaths to unprecedented level. We will return to this example, but it should be kept in mind that what we document is the average quantitative effect of peace agreements which includes failures like Iraq.

## 5.2. Event study results

We start the quantitative analysis with a simple event study. The idea of the design here is to track violence in the country/months leading up to the signing of a power sharing agreement and align them at an imaginary month 0 and we then measure how violence changes following this month 0. Controlling for factors that are constant for each respective country (country fixed effects).

We start with event studies of all 440 agreements in Figure 5. The dots in the left panel of Figure 5 show the average share of any violence occurrence compared to the country average, i.e. taking country context into account, around the adoption of a power-sharing agreement. The Figure shows the twelve months and eighteen months after an agreement. We are plotting the average violence occurrence, the average probability of violence following power-sharing

agreements while statistically considering the situation of a country. What we see is again a clear downward shift that starts one month after agreements, then bounces back slightly and stabilizes around 5 percentage points lower than before the power sharing agreement.



**Figure 5: Violence occurrence and intensity after power sharing**

In the right panel of Figure 5 we show the same experiment but now tracking average violence in intensity (in logs). Clearly, there is a negative trend in violence which starts after the adoption of a power-sharing agreement. The effect is around 0.2 log points. Again, we see a slight resurgence of violence. While the dots represent the average level, clearly not all countries will experience this same level of violence reduction. The distribution around this average is symbolized by the thin vertical lines, which represent the 95% confidence interval for the average.

Table 1, Panel A confirms these results in simple regressions controlling for country fixed effects but maintaining the event-study character by restricting the sample to the months before and after the adoption of a power sharing agreement. From left to right we vary the length of this event study window from 6 months on the left to 12 and 18 months on the right. In line with Figures 5 and 6 we find that the drop with power sharing is most pronounced and precisely estimated around the 6-month window. The size of the coefficients here indicates a 5-percentage point drop in occurrence which is a 8% drop when compared to the 6 months before the agreement. Table 1, Panel A, column (2) indicates a drop of violence intensity by 0.2 log points or 18%. Coefficients in the 12- and 18-months window stay negative but are statistically insignificant. This points to a very quickly fading effect of power sharing.



**Table 1: Event study results***Panel A: All power-sharing agreements*

|                     | (1)                   | (2)                  | (3)                  | (4)                | (5)                 | (6)                |
|---------------------|-----------------------|----------------------|----------------------|--------------------|---------------------|--------------------|
|                     | 6 months window       |                      | 12 months window     |                    | 18 months window    |                    |
|                     | any violence          | violence intensity   | any violence         | violence intensity | any violence        | violence intensity |
| after power-sharing | -0.0505**<br>(0.0216) | -0.221**<br>(0.0952) | -0.0471*<br>(0.0248) | -0.143<br>(0.116)  | -0.0344<br>(0.0315) | -0.0737<br>(0.145) |
| Observations        | 3,605                 | 3,605                | 5,353                | 5,353              | 6,577               | 6,577              |
| R-squared           | 0.483                 | 0.555                | 0.490                | 0.553              | 0.497               | 0.556              |

*Panel B: Comprehensive power-sharing agreements*

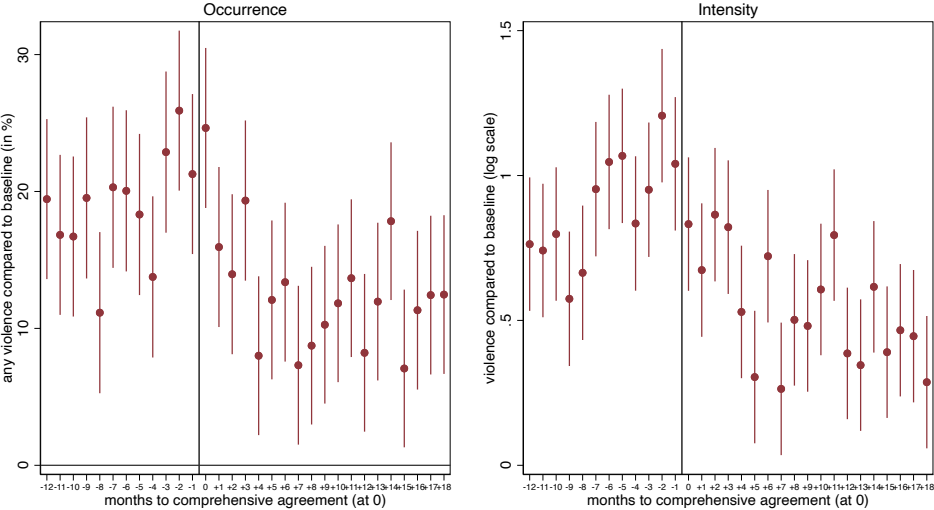
|                     | (1)                  | (2)                 | (3)                   | (4)                 | (5)                   | (6)                 |
|---------------------|----------------------|---------------------|-----------------------|---------------------|-----------------------|---------------------|
|                     | 6 months window      |                     | 12 months window      |                     | 18 months window      |                     |
|                     | any violence         | violence intensity  | any violence          | violence intensity  | any violence          | violence intensity  |
| after power-sharing | -0.0497*<br>(0.0293) | -0.364**<br>(0.178) | -0.0773**<br>(0.0313) | -0.375**<br>(0.184) | -0.0838**<br>(0.0373) | -0.400**<br>(0.196) |
| Observations        | 853                  | 853                 | 1,522                 | 1,522               | 2,120                 | 2,120               |
| R-squared           | 0.517                | 0.579               | 0.479                 | 0.535               | 0.489                 | 0.530               |

Robust standard errors, clustered at the country level, in parentheses. \*\*\* p<0.01, \*\* p<0.05, \* p<0.1. Any violence is a dummy that is equal to 1 if there is any fatality according to UCDP in that month. Violence intensity are log(fatalities +1). All regressions restrict the sample in a window around the adoption of power-sharing agreements and control for country fixed effects.

This should not come as a surprise, however, as power-sharing *agreements* defined in the PA-X database often reflect ongoing negotiations instead of final agreements, leading to a high frequency of agreements. After only 10 months over 50% of the 440 agreements in our dataset were replaced by new ones. Of the comprehensive agreements close to 90% are still in place after 10 months. This means that looking at comprehensive agreements will give us a better way to measure real breakthroughs in negotiations.

In Figure 6 we show the same event studies but now showing violence trends around comprehensive agreements. The left panel of Figure 6 shows the occurrence of any violence. This drops by close to 10 percentage points with an agreement and there is less of a notable rebound. In the right panel of Figure 6 we show results tracking average violence in intensity

(in logs). Again, there is a negative trend in violence which starts after the adoption of a power-sharing agreement. The effect is around 0.5 log points which is a violence reduction of 40%. However, strikingly, there seems to also be a trend component due to which violence intensity keeps decreasing over time.



**Figure 6: Violence occurrence and intensity after comprehensive agreement**

Table 1, Panel B confirms these visual impressions. The effect sizes are sizeable. Violence occurrence is relatively high before the adoption of a power-sharing agreement with close to 70% of months in the year before suffering from some violence so that Table 1, Panel B, column (3) suggests a fall by roughly 10% in violence occurrence when comparing the 12 months after adoption to the 12 months before adoption. Table 1, column (4) suggests a fall by 0.375 log points or about 30%. Strikingly, point estimates become larger and statistically more significant with increasing window size. Whereas causality is harder to pin down here it is clear that the short-term effects of power sharing do not seem to reverse. We will return to this point in section 6.

**5.3. An event study looking at forecast errors**

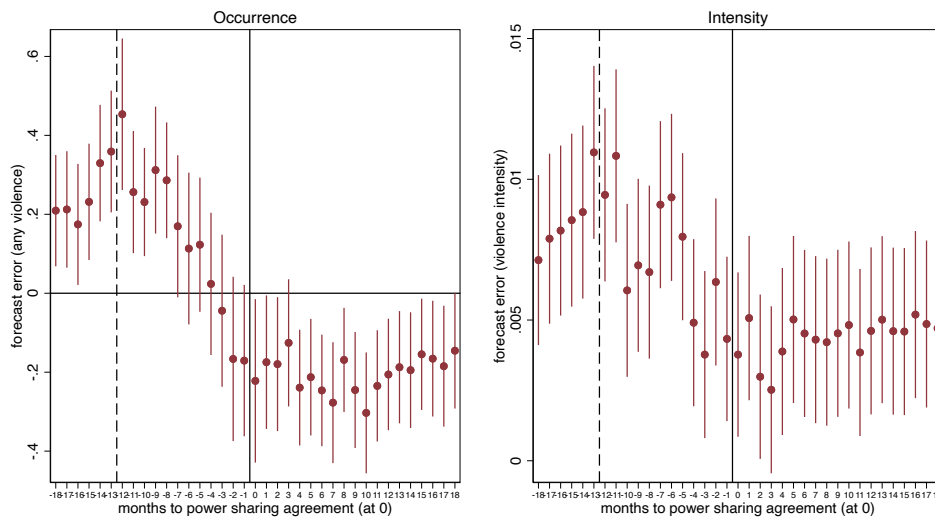
Conflict forecasts provide the possibility to see whether the adoption of power-sharing agreements led to positive surprises after their adoption. Our methodology studies forecast errors. Given information available at time *t*, was the forecast for the next 12 months too optimistic or too pessimistic? The forecast error here is defined as the true realization minus the forecast – both measured as averages in the 12 consecutive months. Positive errors mean

that, at the time of the forecast, we are too optimistic and underestimate future violence. Negative forecast error values mean that we are too pessimistic in our forecast.

We conduct two such studies in Figures 7. In the left panel of Figure 7 we show the results using violence occurrence. The Figure shows two important dates as vertical lines. First, the adoption of the power-sharing agreement is marked as a solid line, again at time 0. But now we are showing 12 months ahead forecast errors so that the months before the adoption date 0 are already treated. As an example, at -6 we are comparing the forecast made 6 months before the adoption of a power-sharing agreement with the realized onsets in the next 12 months. We capture this with a second dashed line at -12 which indicates the moment the forecast horizon starts to include the power-sharing agreement. We see a striking pattern in which the forecast error starts to drop dramatically before the adoption of the power-sharing agreement for both outbreaks and intensity.

Our forecast uses millions of news articles, and a complex way of capturing conflict dynamics using machine learning which is geared to get the best possible estimate of future violence using all possible information available. The news text also includes factors like reports about diplomatic interventions or economic factors. But, in the months leading up to 0 the forecasts only use information available at the time  $t$  between -18 and 0, i.e. these are rolling forecasts. This allows us to capture the situation as it was seen by actors at the time in the forecast. Forecast errors therefore capture by how much the forecast, using violence dynamics and text observed up until  $t$ , can capture the actual evolution of violence in the following 12 months.

It is therefore clear from Figure 7 that power-sharing agreements target situations with vicious conflict dynamics captured by an upward drift of the forecast error of our forecasting system. Agreements are then associated with a dramatic and systematic trend reversal in the forecasting error, i.e. even taking all possible information into account that is available at months -12 to 0 the system is not able to predict that power sharing will bring down violence. From a forecasting perspective this is also extremely interesting as it suggests that improving forecast quality might be difficult. The pattern we see in Figure 7 from months -12 to 0 cannot be explicitly included in the forecast as the information of the agreement is not available before month 0 after 0 there are no visible changes in the forecast error.



**Figure 7: Forecast error around the adoption of power sharing**

The pattern in the right panel of Figure 7 shows forecast errors for violence intensity forecasts (the forecast is on  $\ln(\text{fatalities per capita})$ ). Forecasting violence intensity is harder overall because movements in intensity are obviously larger making errors larger. However, we see the same dramatic trend reversal within the forecasting window as in the forecast of any violence outbreaks.

These findings reinforce the event studies in that violence seems to fall significantly after the adoption of a power sharing agreement. The novelty in the forecast risk view is that it seems impossible to anticipate the effect of the power-sharing agreement from violence dynamics or news events even by a machine learning algorithm explicitly targeting the best possible forecast of violence.

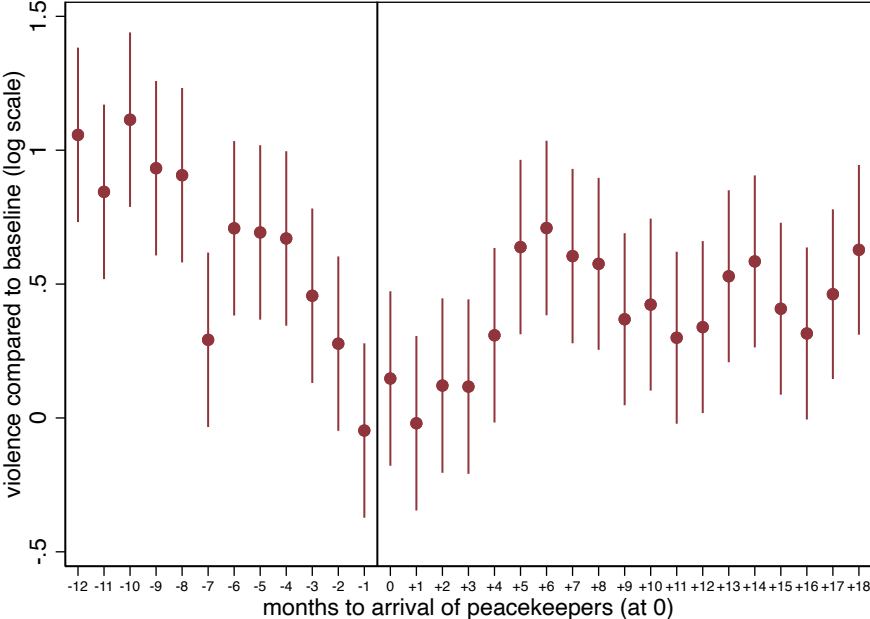
#### 5.4. Robustness and additional results

An important concern with the identification strategies in the previous section is that power-sharing agreements are accompanied by other policies, often put in place by the international community, which also reduce violence and coincide with the implementation of a power-sharing agreement. This would lead to omitted variable bias in as far as these other policies are not part of the comprehensive peace agreement but are additional measures which simply coincide with the agreement.

One, crucial, policy which is closely linked to international attempts of pacification are peacekeeping missions. We have therefore collected two additional datasets for our sample

period from the UN webpages. First, the number of total peacekeeping troops present in a country and second, the monthly budget spent on peacekeeping in a country. In the latter case we had to typically use interpolation between quarterly or even yearly reports such that the timing is not precisely measured. However, presence of troops is relatively well recorded with even a handful of peacekeepers being tracked.

Figure 8 shows how the arrival of peacekeeping troops relates with violence intensity. Quite clearly, the arrival (at month 0) is anticipated by a sharp decline in violence intensity reaching a minimum a month before the troops arrive. This does not mean that peacekeeping does not play a role in violence reduction, but it suggests that other factors, like power-sharing agreements, are at play.



**Figure 8: Violence intensity and start of peacekeeping missions**

We confirm the robustness of our main results in Table 2. Here we fix the window size to 12 months before and after the signature of an agreement and control for peacekeeping activity in three different ways. The main finding is that the coefficients on the power-sharing treatment dummy does not change dramatically when compared to columns (3) and (4) in Table 1. Sometimes statistical significance suffers but the overall role in violence reduction is borne out by these regressions. The same is true for other window sizes. We also find negative coefficients for our peacekeeping variables but they are never statistically significant.

**Table 2: Robustness to peacekeeping controls**

|                         | (1)                   | (2)                   | (3)                   | (4)                   | (5)                   | (6)                   |
|-------------------------|-----------------------|-----------------------|-----------------------|-----------------------|-----------------------|-----------------------|
|                         | any<br>violence       | violence<br>intensity | any<br>violence       | violence<br>intensity | any<br>violence       | violence<br>intensity |
| after power-<br>sharing | -0.0780**<br>(0.0350) | -0.353*<br>(0.199)    | -0.0761**<br>(0.0359) | -0.313<br>(0.200)     | -0.0748**<br>(0.0337) | -0.342*<br>(0.195)    |
| peacekeeping<br>budget  | 0.000543<br>(0.00797) | -0.0155<br>(0.0366)   |                       |                       |                       |                       |
| peacekeeping<br>troops  |                       |                       | -0.00149<br>(0.0177)  | -0.0756<br>(0.0734)   |                       |                       |
| peacekeepers<br>present |                       |                       |                       |                       | -0.0329<br>(0.144)    | -0.423<br>(0.689)     |
| Observations            | 1,522                 | 1,522                 | 1,522                 | 1,522                 | 1,522                 | 1,522                 |
| R-squared               | 0.479                 | 0.535                 | 0.479                 | 0.539                 | 0.480                 | 0.537                 |

Robust standard errors, clustered at the country level, in parentheses. \*\*\* p<0.01, \*\* p<0.05, \* p<0.1. Any violence is a dummy that is equal to 1 if there is any fatality according to UCDP in that month. Violence intensity are  $\log(\text{fatalities} + 1)$ . All regressions restrict the sample in a window of 12 months around the adoption of power-sharing agreements and control for country fixed effects. Peacekeeping budget is  $\ln(\text{budget} + 1)$ , peacekeeping troops is  $\ln(\text{troops} + 1)$  and peacekeepers present is a dummy for when  $\text{troops} > 0$ .

A difficult concern to address is reverse causality. Violence clearly tends to trend upwards before the adoption of power-sharing agreements. Our interpretation is that this is due to the targeting of situations which are escalating. However, an alternative interpretation is that mediation attempts can increase violence because they increase the incentives to engage in violence to strengthen bargaining power (Canidio and Esteban 2020). This would make the months right before an agreement a bad control group. Table 3 checks for this possibility by dropping the 3 months before the adoption month. Results are robust to this.

**Table 3: Robustness to dropping the months before the adoption month**

| VARIABLES           | (1)<br>6 months window |                     | (3)<br>12 months window |                    | (5)<br>18 months window |                    |
|---------------------|------------------------|---------------------|-------------------------|--------------------|-------------------------|--------------------|
|                     | any violence           | violence intensity  | any violence            | violence intensity | any violence            | violence intensity |
| after power-sharing | -0.0558*<br>(0.0301)   | -0.416**<br>(0.171) | -0.0771**<br>(0.0331)   | -0.357*<br>(0.186) | -0.0804**<br>(0.0393)   | -0.409*<br>(0.207) |
| Observations        | 669                    | 669                 | 1,360                   | 1,360              | 1,973                   | 1,973              |
| R-squared           | 0.526                  | 0.580               | 0.491                   | 0.538              | 0.500                   | 0.534              |

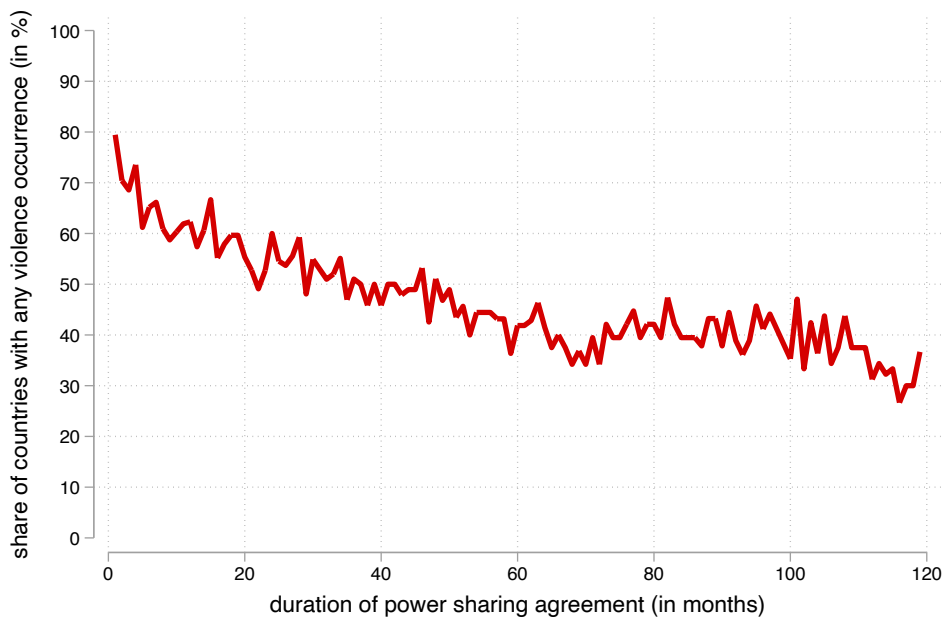
Robust standard errors, clustered at the country level, in parentheses. \*\*\* p<0.01, \*\* p<0.05, \* p<0.1. Any violence is a dummy that is equal to 1 if there is any fatality according to UCDP in that month. Violence intensity are log(fatalities +1). All regressions restrict the sample in a window around the adoption of power-sharing agreements and control for country fixed effects. We also drop the 3 months before adoption from the sample.

## 6. Building bridges: The aftermath of power-sharing agreements

### 6.1. The duration of power-sharing agreements

We have already seen in the case studies, power-sharing agreements are no panacea. They are deployed repeatedly, often within months. Even comprehensive agreements can be tried more than once in the history of a country. In this context it is worth asking whether there is any hope for long-term effects of power-sharing agreements.

In Figure 9 we show the average occurrence of violence in the aftermath of comprehensive power-sharing agreements. The clear take-away is that a dramatic drop of the likelihood of violence in the long run. After 10 years only around 30% of all countries that implemented power sharing still see any violence at all. Given the baseline of about 80% this is a very substantial gain.



**Figure 9: Stabilization in the long run**

This means that power-sharing agreements can be part of a broader shift towards more peaceful equilibria. Put differently, countries can escape the conflict trap and power-sharing agreements might be a part of the escape plan. One direct channel through which power-sharing agreements could affect long-term trajectories are political institutions. This is well-understood in policy circles. International organizations like the UN/DPPA stress that peace agreements have a clear link to “constitution making” (Berghof Foundation and UN/DPPA 2020) and this means they can have a profound impact on the development on political institutions through their role.

However, for policy purposes it is interesting to understand what the institutional features are that are associated with such long-term changes in violence. For this purpose, we now turn towards the V-Dem dataset. Instead of trying to prove a specific mechanism our goal here is explicitly to data-mine to see which cluster of institutional features arise with long-term falls of violence.

## 6.2. Long-run institutional changes and violence

The Variants of Democracy (V-Dem) dataset allows us to analyse what type of institutional changes are associated with reductions in violence. We begin the analysis with the top layer

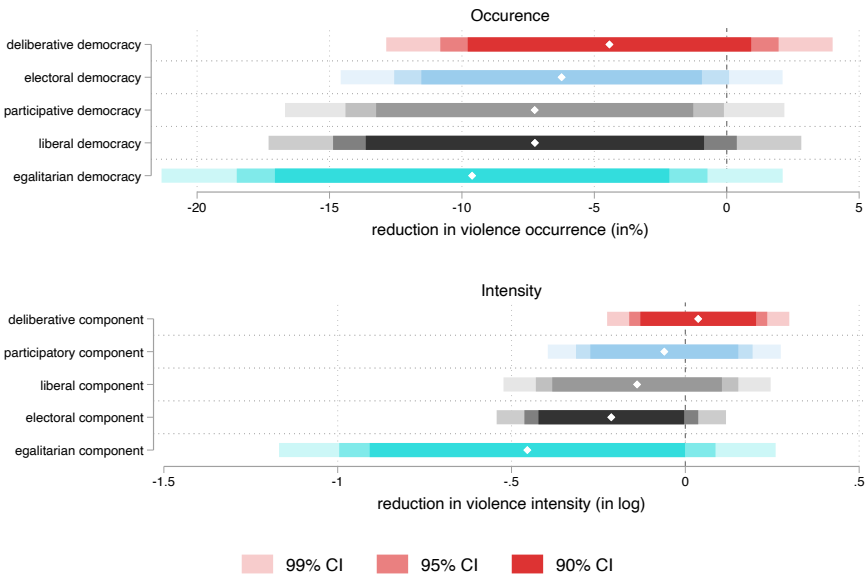


of the dataset which captures the five different components that V-Dem measures. These are:

1. Deliberative: consultation and engaged society
2. Participatory: popular vote, elected local/regional government
3. Liberal: judicial & legislative constraints on executive
4. Electoral: clean elections, freedom of expression, suffrage
5. Egalitarian: equal protection and access

Figure 10 shows how strongly these elements are correlated with the extent of political violence occurrence and intensity. Throughout this section we control for country fixed effects and month fixed effects, i.e. the associations shown here control for things like the country history and geography. Importantly, this implies that the results we show here are based on changes at the country level, i.e. our findings are based on changes that have previously been observed at the country level in the period 1989 to 2022.

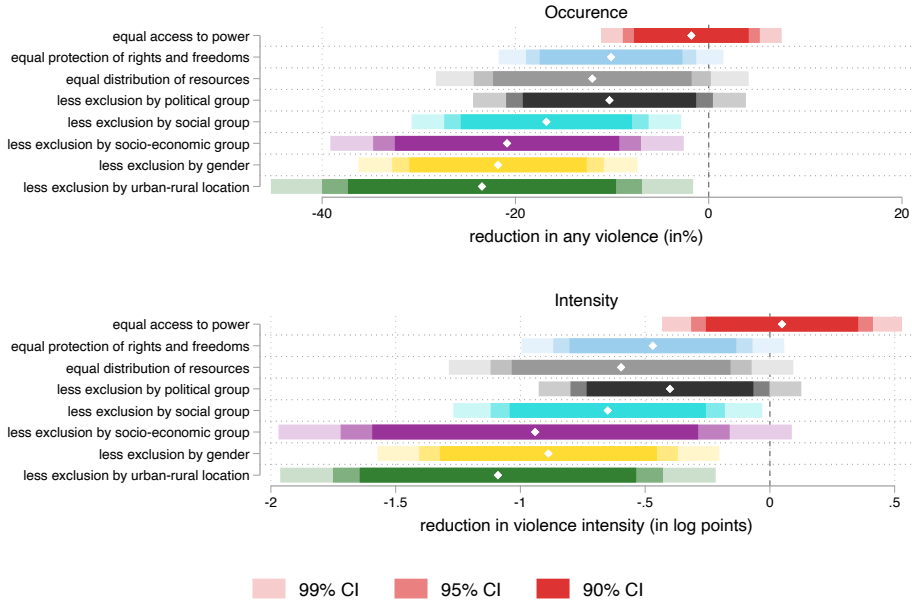
The white dots in Figure 10 represent the average country experience in our dataset. The bars indicate the uncertainty around this average experience. Broader bars indicate that specific histories can differ more from the average. In the top panel we find that all components of democracy measured by V-Dem are associated with reductions in violence – even when we control for country context and international context. We find the strongest associations in the liberal, electoral and egalitarian components and weaker associations with the deliberative and participatory components. The egalitarian component in the top panel of Figure 10 is particularly strongly associated with reductions in violence, which might suggest that broadening horizontal and vertical inclusion can decrease violence. An increase of one standard deviation in this component is associated with a 10% reduction in violence.



**Figure 10: Facets of democracy and reductions in violence**

The top panel of Figure 11 looks at facets of the egalitarian component as measured by the V-Dem dataset. What is, perhaps, surprising is the lack of an association with the “equal access to power” variable in V-Dem. The elements that are most closely related to violence reductions are the absence of exclusion across political, social, socio-economic, gender or geographic dimensions. These variables capture access to power, public services, justice and civil liberties and whether these are restricted for specific groups.

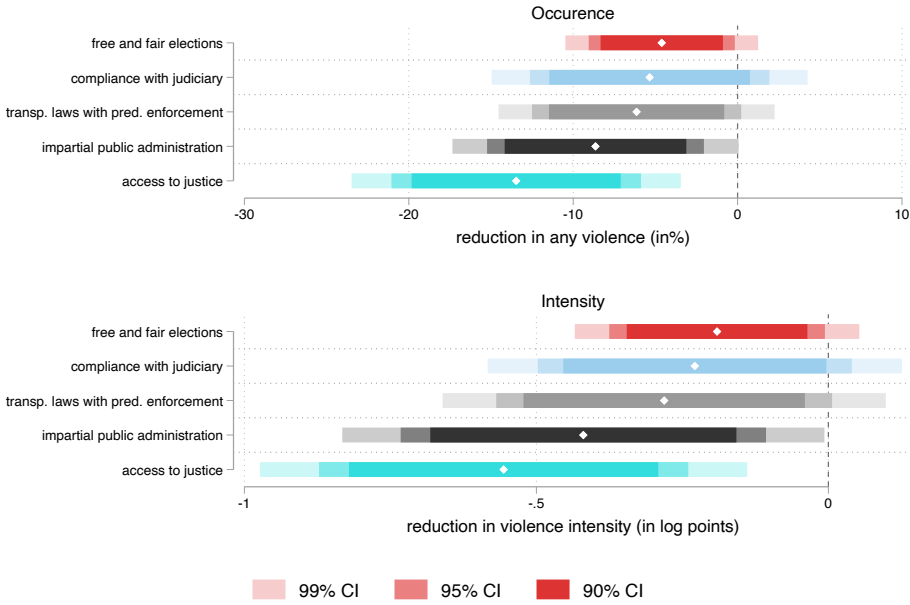
The associations in the bottom panel of Figure 10 are large. Violence is reduced by between 15% and 25% when exclusion is reduced by one standard deviation. The association is even stronger if we look at violence intensity in the bottom panel of Figure 11. A coefficient size of slightly below -1 in Figure 11 implies that violence reduces by almost two thirds. This is a very strong and statistically significant association. Despite not knowing whether this is a causal effect, tracking exclusion indicators should be an important task for policymakers.



**Figure 11: Egalitarian dimensions and reductions in violence**

However, even if we believed that a part of these associations is causal, running from institutions to reductions in violence, it is hard to take away concrete policy advice. What are the concrete institutional features that are most strongly associated with reductions in

violence? Our analysis, summarized in Figure 12, suggests that improvements in the strength and neutrality of the legal system and public administration might play a key role. Likewise, fair access to public sector jobs and business opportunities are strongly associated with reductions in violence. The most significant reduction in violence is observed with strong and equal access to justice. A darling of the international community, free and fair elections, seems to be associated with reductions in violence but this association is not pronounced.

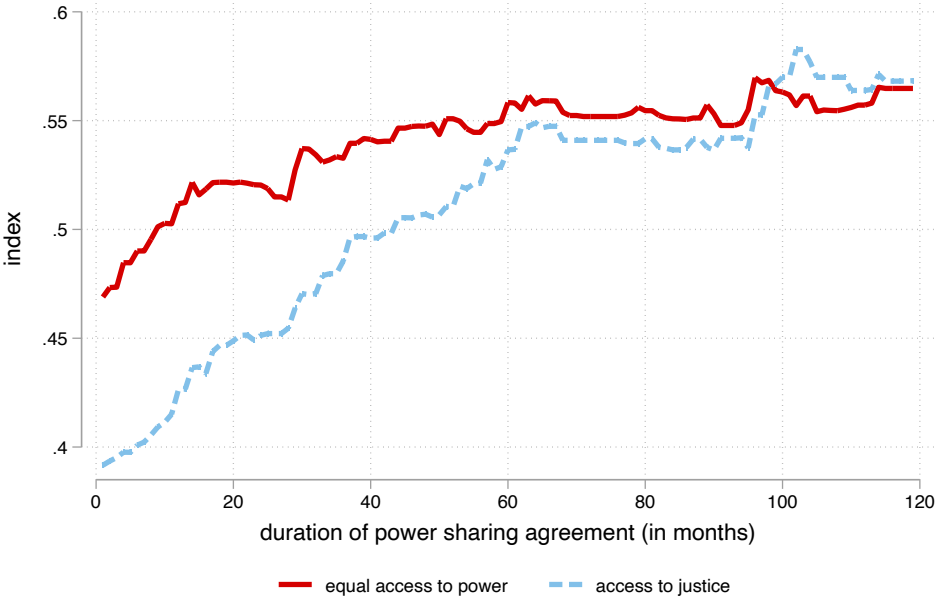


**Figure 12: Institutional features and reductions in violence**

It is worth bearing in mind that reductions in violence with equal access to power seem less robust in the V-Dem dataset. Instead, violence reduction is associated with additional changes that provide access to justice, a fair administration, and public sector jobs in addition to increasing power sharing and improving the deliberation process. This opens a dimension to power sharing in which the behaviour of the elite towards other parts of the population seems key for reductions in violence.

In Figure 13 we show further evidence for this link using just two institutional features in the aftermath of power-sharing agreements. We know from Figure 9 that agreements that last longer are also associated with stronger reductions in violence. In Figure 13 we show that they are also associated with a strengthening of power sharing, narrowly defined, and the constraining of power through access to justice.

It is plausible that it is this strengthening of institutions within the first years after power sharing that makes agreements stable and reduces violence. We will now add our conclusions based on this interpretation of the empirical regularities.



**Figure 13: Duration of power sharing and access to institutions**

**7. Discussion**

Part of the reason why the conflict trap persists is that violence narrows the options for forward-looking decision making. This in turn is required for institutional or structural changes and for a sustainable reduction in violence. The conflict trap is also reflected at the level of power-sharing agreements, as more than half of all agreements are amended, or replaced by other agreements within a year.

But this study shows that comprehensive power-sharing agreements nonetheless have a direct short-term effect on violence. In some cases, the short-term reductions in armed violence seem to persist. We show that this is linked to comprehensive agreements which are also associated with wider institutional development and dramatic reductions in violence. Some countries escape the conflict trap. Power-sharing agreements should therefore be seen as both a short-term solution and a facilitator of broader changes. The broader changes that are required seem to include improvements in the strength and neutrality of the legal system and public administration, fair access to public sector jobs and services, and business opportunities. The disproportional appearance of judicial reforms in comprehensive agreement indicates that, perhaps, the nucleus for these broader changes can be laid at the time of the agreement.

These findings suggest a direct link to the political science literature on power sharing which highlights that constraining power might play an important role. In this view, it is not about how to divide executive power but how to constrain it. Power sharing in this sense of the expression is achieved when access to justice, public sector jobs, the legal system and administration are not exclusively used for patronage by the narrow group that holds executive power. But there could also be a slightly different interpretation in which a neutral administration and access to justice represent non-excludable public goods. This would then be more in line with the idea that violence is reduced when state capacity is used to produce non-excludable public goods instead of excludable rents. Besley and Persson (2011) argue that the incentives of the incumbent government to invest in state capacity is key to understand how economic development, the distribution of resources and political violence evolve in the long run. Investment incentives increase when either institutions are cohesive, or power is not contested, or the state is needed by the incumbent group to provide non-excludable public services. The goal of power-sharing agreements should be to improve social cohesion and frame the state as providing services, as compared to distributing rents. This is of course challenging in countries where the elite relies on patronage, formal institutions are weak, where deals are mostly informal, and alliances change frequently.

The quantitative results in this report can directly be interpreted within the influential framework proposed by Cheng et al. (2018), who stress that interactions and (mis)alignments between political settlements, elite bargaining and peace agreements may explain whether and how wars are terminated, and differing trajectories of post-war transition. According to Cheng et al. (2018), large-scale violence will only stabilize “*when the distribution of benefits in a society, supported by its institutions (e.g., political positions, business opportunities) is consistent with the distribution of power in society, and the economic and political outcomes of these institutions are sustainable over time*”. They stress that this includes both the horizontal relationships between different parts of the elites and the vertical relationships between elites and their constituencies. Our results, in particular regarding the role of access to justice, indicate that the most stable and peaceful bargain results seem to be those that manage to combine a solution for the horizontal elite bargain with institutional changes that address the vertical dimension.

Put differently, a lack of access to justice is a key factor for the mobilization of political violence and building institutions that broaden access should therefore start within the peace process. This is also where our findings echo the case study findings in the Pathways for Peace report by the World Bank/UN (2018), who conclude that countries that find pathways to sustainable peace have eventually tackled the messy and contested process of institutional reform. Often, the transition moment that led to sustainable peace is based on a shift away from security-led responses and toward broader approaches that mobilized a range of sectors in support of institutional reforms.

But there are other, complementary views on our findings which suggests that power-sharing agreements could be an entry point into changing the logic of the elite bargain itself by

introducing elements of public goods, such as a perception of justice or national identity. North et al (2007) propose that fragile countries represent a *limited access order* where elites use the state order to extract rents. Escaping this set-up is a necessary condition to escape the conflict trap. In this view, peace agreements need to complement the elite bargain with elements that allow for a change in the overall equilibrium in which the state can provide broader, non-excludable benefits. If the members of a group have a sense of national identity, for example, then this will outlast changes in political power and the negotiation over patronage. This is important because the model of a negotiation as a zero-sum game does not include the option of non-excludable public goods such as national identity, trust, a sense of fairness or legitimacy. If elites manage to provide non-excludable public goods to their citizens, then this defuses the conflict. Put differently, the elite needs to be able to shift towards providing broader benefits beyond their narrow in-group. This makes agreements of power sharing more robust to shifts in de facto power.

How realistic is this? It has been shown that a sense of national identity can be affected by single events (Depetris-Chauvin et al 2020). If a peace agreement establishes a nucleus of unity this might be a starting point for broader legitimacy. Rohner et al (2013) point to lack of trust between the different groups in conflict as one of the reasons of the conflict trap. Practitioners involved in mediation and peace talks stress the importance of building trust during negotiations (Freeman and Clark 2020). The protection of rights and access to justice might be essential elements in re-building this trust towards other groups and, hence, provide an escape route out of the conflict trap.

Several caveats apply to this project. First, identification hinges on our controls for future risk. If these fail to capture conflict dynamics in a systematic way which is correlated with the adoption of a peace agreement, the result will be an underestimation of the effect of a peace agreement. The biggest problem for our quantitative estimates occurs if other initiatives, like the deployment of a peacekeeping force, co-occurs with the month of a power sharing agreement. We see this as unlikely given the sharp, monthly variation we exploit. Our results should nonetheless be seen in this context – agreements are not concluded in isolation but impacted by the initiatives of a range of actors, including external actors, the provision of financial incentives (aid), security guarantees (deployment of peacekeepers), etc. Quantitative researchers could, given data on state visits, aid, peacekeeping or adopted UN resolutions, try to disentangle the effects of these different contextual elements.

Second, if armed groups anticipate that agreements will cement the distribution of political power, then it is possible that violence increases before an agreement is struck. Our results for comprehensive agreements suggest slightly positive pre-trends to the adoption of such agreements. However, we do not see pre-trends for all agreements, and these should therefore be seen as a lower bound of the effect. A clear way forward for research here is a further disaggregation of our quantitative analysis. With an actor-based focus it would be possible to see whether actors that are excluded from a peace agreement are more likely to engage in violence than those that are included. Insights could also be gained from a geographic

disaggregation in which the participation or exclusion of specific ethnic groups could be linked to spatially disaggregated violence data. Another possibility is the development of a fully dynamic model in which armed political violence is modelled jointly with the timing and content of peace agreements. Recent theoretical work has opened avenues in this direction (Meirowitz et al 2019).

A clear limitation for practitioners is that deals with external actors or actors which are shunned by the international community might yield results but may have a legal, strategic, and moral price which is too high to pay. Our quantitative results should be interpreted with this in mind: all agreements are trying to achieve something very difficult in a particular context. In this context their effect is even more remarkable.

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