Conflict-related sexual violence and the Universal Periodic Review of the United Nations Human Rights Council *

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Abstract

The regular peer review of the human rights records of all member states of the United Nations (UN) has been touted as one of the main innovations that came with the creation of the UN Human Rights Council (UNHRC), replacing the decried UN Commission on Human Rights (UNCHR). Some studies have highlighted that a large share of recommendations made in the Universal Periodic Review (UPR) are accepted by the state under review and, subsequently, states report having implemented them. Hence, while on paper the UPR appears to work like a charm, we know rather little whether the recommendations made by peers in the UPR actually change the situation on the ground. We address this gap by offering a study on how recommendations dealing with sexual violence in the UPR affect the behavior of states under review engaging in conflict-related sexual violence (CRSV). Drawing on data for the first three UPR cycles we find mixed evidence for such effects. While in some analyses we find that such recommendations affect the behavior of the states under review, in others, these effects fail to materialize. These results, though obtained in a limited policy area of a particular type, question the optimistic assessments regarding improvements in human rights due to the UPR while also providing hope for the potential effectiveness of the human rights regime.

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1 Introduction

In 2006, the UN Commission on Human Rights was replaced by the UN Human Rights Council (UNHRC). The most innovative aspect of the new Council was the creation of the Universal Periodic Review, a mechanism in which all UN members, regardless of their human rights records or their commitment to international human rights treaties, would be reviewed by other states. Two decades after its creation, we know little about whether this practice has meant a meaningful reform in the international human rights regime.

Questions about institutional effectiveness are relevant in a world in which states engage in backlash against the liberal order and liberal ideas. Scholars have studied the effectiveness of international human rights treaties (Simmons, 2009), self-reporting mechanisms (Creamer and Simmons, 2019; Creamer and Simmons, 2020), courts (Jo and Simmons, 2016; Haglund, 2019; Haglund and Welch, 2021), and petition mechanisms (Schoner, 2024). However, we know less about peer-review mechanisms in which politicization plays a significant part (Carraro, 2019; Terman and Voeten, 2018). We study the effectiveness of the Universal Periodi Review in the area of conflict-based seuxal violence.

Sexual violence in conflicts is an endemic problem, both as perpetrated by state and non-state actors. This was recognized by the United Nations (UN) through the adoption by the Security Council of Resolution 1820 in 2008, aiming at protecting women from violence (Binningsbø and Nordås, 2022).[1] This resolution and a deed of commitment with a similar aim proposed by the NGO Geneva Call to non-state actors (see `https://www.genevacall.org/areas-of-intervention/`)[2] aim to reduce sexual violence in the context of conflicts. Whether this resolution, respectively the deed of commitment, actually achieve their goal is, however, hard to assess, given the absence of reporting mechanisms.

[1]Based on this resolution and UNSC Resolution 1960, the UN Secretary General submits since 2012 on a yearly basis to the UN General Assembly a report on "Conflict-related sexual violence" (`A/66/657*S/2012/33` being the first one, and `S/2025/389` the most recent one. These reports, according to a very preliminary analysis, refer to 114 country-years, amongst which 38 refer to state forces perpetrating CRSV coming from 8 countries, namely the Central African Republic (2022-2025), Côte d'Ivoire (2016), Democratic Republic of the Congo (2014-2016), Myanmar (2023-2025), Somalia (2015-2025), South Sudan (2014, 2016), Sudan (2016-2025), and the Syrian Arab Republic (2017-2020). In 111 of the 114 country-years, non-state actors were mentioned as perpetrating CRSV.

[2]See also `https://www.genevacall.org/wp-content/uploads/2024/02/Official-DoC-Prohibiting-sexual-violence-and-gender-discrimination-French.pdf`

Another way in which conflict-related sexual violence (CRSV), especially as perpetrated by state actors, may be addressed is in another organ of the UN, namely in the Universal Periodic Review (UPR)[3] of the United Nations Human Rights Council (UNHRC). The UPR process relies on a peer review of all UN member states on a regular basis in which recommendations on how to improve the human rights record are addressed to the states under review. A recent study by Johansson (2024) highlights that CRSV is frequently addressed in the UPR, and systematically in reviews of countries that are involved in a conflict and perpetrate considerable CRSV: "[a]ll governments with a documented record of widespread, massive and/or systematic CRSV . . . have been shamed for this by at least one foreign government during their review in the UPR." (Johansson, 2024, 436).[4] The main focus of her study is, however, to understand which countries make recommendations on CRSV to other countries and not the effectiveness of the latter.

While this is an important first step, Johansson's (2024) study does not allow for an assessment of the effects of addressing sexual violence in this peer-review process. The very specific issue of CRSV, however, lends itself well to assess whether recommendations in the UPR-process actually have tangible effects (for a related study focusing on trials, see Binningsbø and Nordås, 2022) and to our knowledge no study has so far tried to do this. Hence, the research question we wish to address in this paper is to what extent recommendations made by UN member-states in the UPR process regarding CRSV actually lead targeted governments to change their behavior in conflict. By addressing this research question, we can also contribute to the broader question of whether recommendations made in the UPR process do actually have tangible consequences, i.e., if policies are implemented as a consequences thereof. It has to be noted, however, that CRSV is in this regard a special case, as UNSC resolution 1820 obliges UN member states to take the necessary measures to protect women from sexual violence in the context of armed conflicts. Hence, implementing policies that lead to a decrease of conflict-related sexual violence, for instance after a state raised this issue in the UPR, reflects, in most cases, a changed implementation of existing rules.

[3]With the announcement by the Trump administration that it will not participate in new UPR cycle (see `https://www.reuters.com/world/europe/us-withdraws-key-un-human-rights-report-draws-criticism-rights-advocates-2025-08-28/`, consulted September 9th, 2025), the universality of this review process has taken a hit.

[4]Fifteen countries fall in this category and the eight countries mentioned in UN Secretary General's reports are all part of this list.

Consequently, what we propose in what follows is an analysis of the extent of sexual violence perpetrated by a state after the latter has received recommendations by its peers in the UPR process. To do so, we rely on data on recommendations coded by Johansson (2024).[5] We combine this data with information on sexual violence as collected by Cohen and Nordås (2014) (`http://www.sexualviolencedata.org/dataset/` as well as similar information on sexual violence perpetrated by government forces available in the ACLED-data initiated by Raleigh, Linke and Hegre (2010) (for a similar use, see Fourati, Girard, Laurent-Lucchetti et al., 2025). As the first UPR reports were adopted in 2008, we can study the link between recommendations and sexual violence for the period between 2008 and 2021 (date when the SVAC-data ends) (see also Nordås and Cohen, 2021).

In the next section we briefly review work on the UPR and, more specifically, its effectiveness. Based on this, we formulate an expectation regarding the consequences of recommendations on sexual violence on state behavior. In section three we present the data as well as the type of analysis we use for our evaluation, before presenting in section four our results. In the last section, we conclude with a summary of our findings and an outlook on further useful studies to assess the effectiveness of the UPR.

2 Literature

Both academics and practitioners put a lot of hope into the main innovation that came with the replacement of the UNCHR by the UNHRC in 2006, namely the UPR (see Kälin and Jimenez, 2003) While other aspects of this main human rights body of the UN changed as well (like the number of sessions, the election of its members, etc., see for instance Voeten, 2016) it was hoped for that having regular peer reviews would improve the operation of this body (Ghanea, 2006; Chetail, 2007; Müller, 2007; Rajagopal, 2007; Forsythe and Park, 2009; Chetail, 2010; Cox, 2010; Davies, 2010; McMahon and Ascherio, 2012; Voss, 2022; Etone, Nazir and Storey, 2024). While critiques warned against exaggerated expectations (Seligman, 2011; Freedman, 2013; Freedman and Houghton, 2017), scholars started to assess the workings of the UPR in

[5]We have initially envisaged also to consider the recommendations in the UPR Sexual Rights Database (`https://www.uprdatabase.org` as well as the newly coded recommendations (starting with the third UPR cycle) provided by the NGO UPR Info. In both cases, it appears, however, that all member states engaging in CRSV according to the SVAC-data (Cohen and Nordås, 2014) during the UPR process were the subject of recommendations on sexual violence according to these two data sources (while a narrower, conflict-related coding is used by Johansson, 2024).

more detail. As this process is a peer review, recommendations presented by the peers of the state under review (SUR) became an important focus (e.g., McMahon, 2010; McMahon, 2012; Freedman, 2013; McMahon, Busia and Ascherio, 2013; Freedman and Houghton, 2017; Terman and Voeten, 2018; Terman and Byun, 2022). While the initial preoccupation was mostly descriptive, namely assessing what recommendations were made and whether they were accepted (McMahon and Botwicz, 2024), more recent work focuses on more specific research questions. Thus, Terman and Voeten (2018) show that the UPR is also used for strategic purposes, as recommendations are only made in specific situations for allies. Similarly, both Johansson (2024) and Ma (2024) study what leads peers to make recommendations to particular states under review. While the latter author proposes a more general study, Johansson (2024) focuses on recommendations made by peers with regard to CRSV. Similarly, Kim (2023, 2024) uses text analysis to uncover networks based on the contents of recommendations.

Thus, it seems fair to say that the literature on the UPR has moved from more descriptive studies to research that focuses on more specific research questions. This has led the field to get a better understanding of what happens in a UPR, what member states make recommendations and of what type, etc. An important aspect, however, is to a large extent still eluding the literature on the UPR, namely, whether this peer review is effective. While some studies assess the share of peer recommendations that a state under review accepts compared to all reviews (e.g., McMahon, 2012), others focus on the latter's explanation in the subsequent UPR cycle what it has done to address the recommendations (e.g., McMahon and Botwicz, 2024). While these studies provide interesting insights, they do not address directly the issue of implementation, namely whether the state under review has taken in practice (legal) actions to address the recommendations. One of the few studies, relying on reports of NGOs etc. to assess the implementation of recommendations has been produced by the NGO UPR Info (2014) for the first cycle of reviews, noting a rather good implementation record (but also the difficulty of covering all areas of implementation). Hence, it can non surprise that McMahon and Botwicz (2024, 109) argue that

> [a] definitive answer as to whether the UPR is having a meaningful impact in the promotion of human rights globally remains elusive. This must await the development of a comprehensive methodology capable of assessing recommendation implementation globally.

This is not surprising, as assessing whether recommendations are implemented

(voluntarily, as there are no enforcement mechanisms) is fiendishly difficult, as a quick look at the large literature on the much more formalized implementation process of legislation adopted in the European Union quickly shows (e.g., König and Mäder, 2014).

Hence, in what follows we offer an initial assessment on how a very specific set of recommendations made in UPR processes, namely those focusing on sexual violence, lead to changes in policy, especially among member states that perpetrate CRSV.

Siding with the more optimistic assessments of the UPR by some scholars, we expect that recommendations, even in an area as touchy as sexual violence in countries affected by civil conflict should lead to policy changes. Hence, we posit the following expectation

E1: Countries receiving recommendations on sexual violence in their UPR will strive to reduce sexual violence perpetrated by their armed forces in civil conflicts.

While, as stated above, at least at the international level UN member states are bound by UNSC resolution 1820 to protect women, receiving recommendations in this domain is unlikely to lead to new laws being adopted (as recommendations in other domains might aim for), but is more likely to change the behavior on the ground (for similar arguments linked to sexual violence perpetrated by UN peace-keeping troops, see Johansson and Hultman, 2019).

3 Data and methods

As our main independent variable is formed by peer recommendations in the first three UPR cycles (2008-2022). With this data, we can look at their effects for (at most) 578 country-cycle pairs (192 in the first cycle, 193 in the second and third cycle). As we use Johansson's (2024) codings of recommendations, and her data ends in 2019, we have, in the most limited dataset, 500 observations of member states receiving recommendations (possibly on sexual violence).

We combine these country-UPR cycle observations with a country-year dataset (generated with the help of the `peacesciencer` package) to yield a dataset covering the period from 2000 to 2019 (respectively 2021).[6] As we are interested in the ways in

[6]This latter end-point is due to the fact that the sexual violence data (SVAC) by Cohen and Nordås (2014) ends in 2021. As we consider that UPR recommendations might be presented as a function of CRSV committed in the past, we (arbitrarily) extend the dataset back to 2000. In some of the analyses, we restrict the time period further, by only considering information extending back four years before the first UPR. We chose four years, as a UPR cycle takes four and a half year, which implies that a SUR has at most five years to address recommendations until the next UPR cycle.

which recommendations affect UN member states having received recommendations regarding sexual violence, we combine this dataset with Cohen and Nordås's (2014) SVAC dataset. We use the codings these authors propose both for state actors and rebel forces (as control) and retain (separately but also combined) all that are based on reports by Amnesty International, Human Rights Watch, and the US State Department. Hence, these three annual scores will form our dependent variables.

While Johansson's (2024) analyses focuses on what leads peers to make recommendations in the UPR (and hence uses a dyadic (per cycle) dataset) and thus uses both state under review (SUR) and peer-specific variables, given our focus, we only retain the latter. These are the following:

- Fatalities from the UCDP GED data (Sundberg and Melander, 2013): We aggregated the "best" estimate to the country-year level.
- Civil-society repression as measured by the VDem project (Coppedge and Gerring, 2011): we used the "v2csreprss" mean estimate.
- Human rights record of the SUR: We use the latent human rights score from Fariss (2019).
- Years since the adoption of UNSC resolution 1820 in 2008 (Binningsbø and Nordås, 2022): instead of using a counter of years since 2008 we simply added as covariate the year.
- Conflict-related sexual violence perpetrated by the rebels (and government forces Cohen and Nordås, 2014): We aggregate the CRSV scores based on reports by Amnesty International, Human Rights Watch and the US State Department as recorded in the SVAC-data by taking the respective maximum values. Contrary to Johansson (2024) we use the original ordinal SVAC-codes (ranging from 0 (none) to 3 (massive) as well as a dichotomized version distinguishing between cases of no CRSV (0) and those with at least isolated CRSV (1). We also create a variable that combines these scores (both ordinal and dichotomous) by again taking the maximum value across the three sources.

Turning to our main independent variable, namely UPR recommendations on CRSV, we proceeded in different ways, relied in the end, however, on Johansson's (2024) codings. First, relying on the database provided by the *Sexual Rights Initiative* we

downloaded all recommendations that had been assigned to either of the two issue categories: "violence against women/gender-based violence" respectively "sexual violence." We aggregated these recommendations to the UPR cycle SUR level and counted the number of accepted and non-accepted recommendations in these two issue areas. As it turns out, in all cycles all SUR received recommendations in this area except Israel in the first UPR cycle (2008). There is, however, variation in whether or not a recommendation was accepted.[7] While this variable would allow us to cover the full period for which Cohen and Nordås (2014) provide information on sexual violence committed by states and non-state actors, it turns out that all SURs that, according to the SVAC-data engage in CRSV received such recommendations. Thus, this very extensive coding by the *Sexual Rights Initiative* is, however, probably too encompassing.[8] For this reason we will rely in this iteration of the paper on Johansson's (2024) codings of recommendations based on her reading of the *UPR Info* information.

4 Results

By design of the UPR recommendations induce a staggered treatment, making simple fixes problematic (see Callaway and Sant'Anna, 2021; Athey and Imbens, 2022; Imai, Kim and Wang, 2022). Given the rather peculiar characteristics of the data, namely that almost all member states engaged in conflict-related sexual violence received recommendations regarding sexual violence, we resort to two types of analyses.[9]

In a first step we resort to simple panel regression with a "within" estimator (i.e., with country-level fixed effects). As covariates we use the ones employed by Johansson (2024), namely the number conflict-fatalities, the human rights scores from Fariss (2019), a measure of civil society repression (Coppedge and Gerring, 2011), the extent of sexual violence perpetrated by the respective rebel forces and the state forces as well

[7]More specifically, in the first three cycles we find that in 42 instances a SuR rejected all recommendations in the area of sexual violence. On the other hand, only in twelve case did a SUR accept all recommendations by its peers.

[8]Almost similarly encompassing is the coding provided by *UPR Info* starting with the third UPR cycle. Out of 184 SURs receive recommendations regarding sexual violence according to the *Sexual Rights Initiative* database, only 35 did not appear in the codings provided by UPR Info. Some initial assessments based on searching for "conflict-related" in the text of the recommendations suggests that this might provide a useful alternative, which we aim to pursue in future iterations of this paper.

[9]As all member states under review (with the exception of Israel in 2008) received such recommendations according to the data stemming from the *Sexual Rights Initiative*, we refrain from using this indicator. In future iterations of this paper these issues will be addressed more directly, also by extending the UPR Info coding, as well as coding recommendations for their explicit reference to CRSV.

the latter lagged up to four years, as well as the year (which is identical to controlling for the years since the adoption, in 2008, of UNSC resolution 1820 (see Table 8 in the Appendix for descriptive statistics on all variables used in the analyses). In abundance of caution we also re-estimate every model by adding an indicator (and its lags) which equals one for states under review in a particular year.[10]

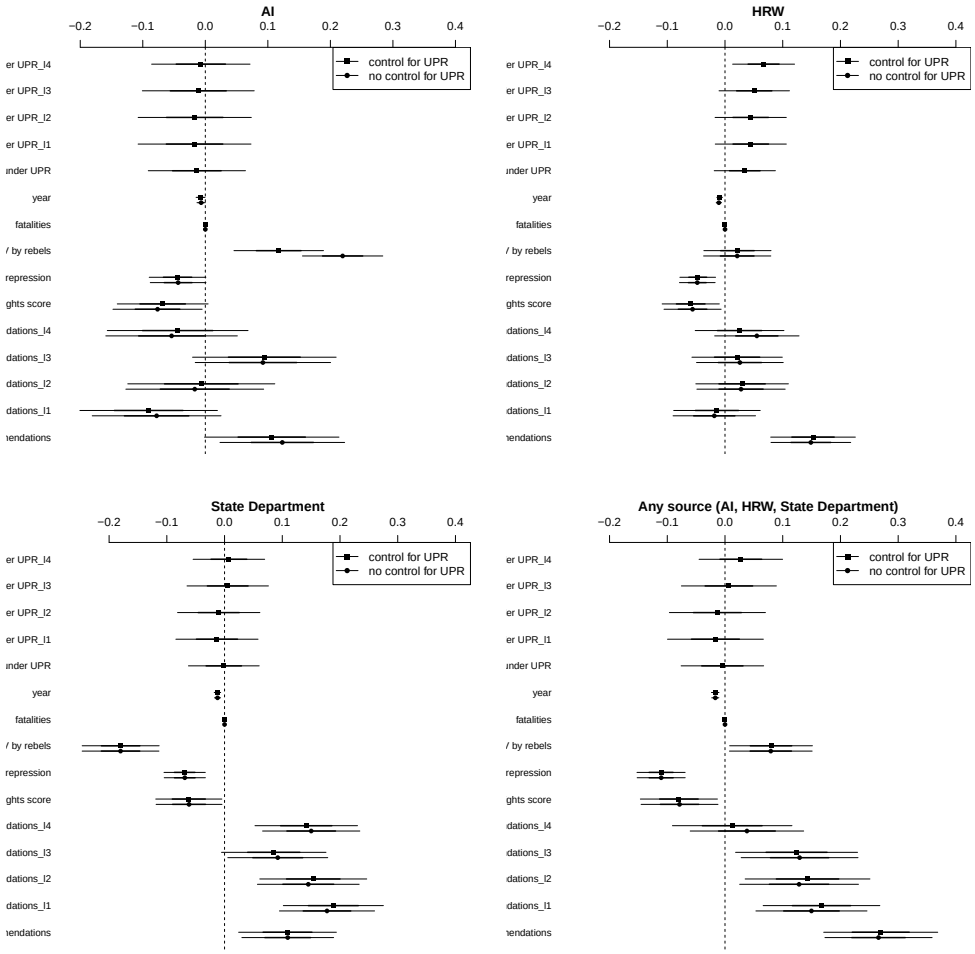

Figure 1: Panel regressions (coefficients and 95 % confidence intervals): recommendations on sexual violence coded by Johansson (2025) and ordinal outcome

Second, as we are in a typical situation of staggered treatments (as a function of the session in which a UPR is carried out for a particular member state), we also resort

[10]Consequently, the coefficient for `Cycle` variables indicates by how much on average a country subject to a peer review behaves differently from a country not subject to such a review in this year, while the coefficient for the `recommendation` variable indicates by how much, on average, a state under review committing CRSV behaves differently than other states under review.

to the estimator based on matching treated observations with observations with similar treatment histories as proposed by Imai, Kim and Wang (2022).[11] Also this approach is not without problems, given the peculiarities of our staggered treatment.

Initial results from the first approach are depicted in Figure 1. We run four models for each of the three dependent variables and their combination assuming both a contemporaneous effect of the recommendations, as well as effects with lags of up to four years. As the four coefficient plots show that evidence for tangible effects of recommendations regarding CRSV are hard to find, at best we might find an effect running counter to our expectations. When we consider the measure based on the reports of Amnesty International, we only find a negative effect on the extent of sexual violence perpetrated by the governments in the first and fourth (and slightly in the second) years after the UPR. In the shorter term the effects are even positive. For the measures based on reports by the Human Rights Watch we find largely positive effects, with the single, marginal exception of recommendations adopted in the previous year. For the measure based on the US State department, we even find systematically negative effects, which then leads to largely similar results when we consider the combined measure. What Figure 1 also shows is that controlling for being subject to an UPR (`Cycle`-variables) does barely affect the main results and the coefficients for these additional controls are quite small and come with considerable uncertainty. Notable is, however, that for the results based on the HRW reports, CRSV on average increases in the fourth year after a UPR.

While the results depicted in Figure 1 rely on an ordinal outcome measure (which is hardly ideal in this context), we find when considering a dichotomous version of the dependent variables (see Figure 2) largely similar results. With this underlying linear probability model we only find for the analyses based on HRW reports a small difference, as UPR recommendations received two years ago decrease the chances that a state perpetrates CRSV.

The analyses presented so far do not attempt to take into account the fact that receiving recommendations on sexual violence is not exogenous. This is amply illustrated by Johansson's (2024) finding that all states she identified as having received recommendations on sexual violence in their UPR were committing such crimes at the time of their review. As we use a less stringent definition of CRSV (see above), we do find some UPR states under review that perpetrated CRSV without receiving recom-

[11] Given that UPR cycles last for four and a half year, we used time periods of four years in this matching exercise.

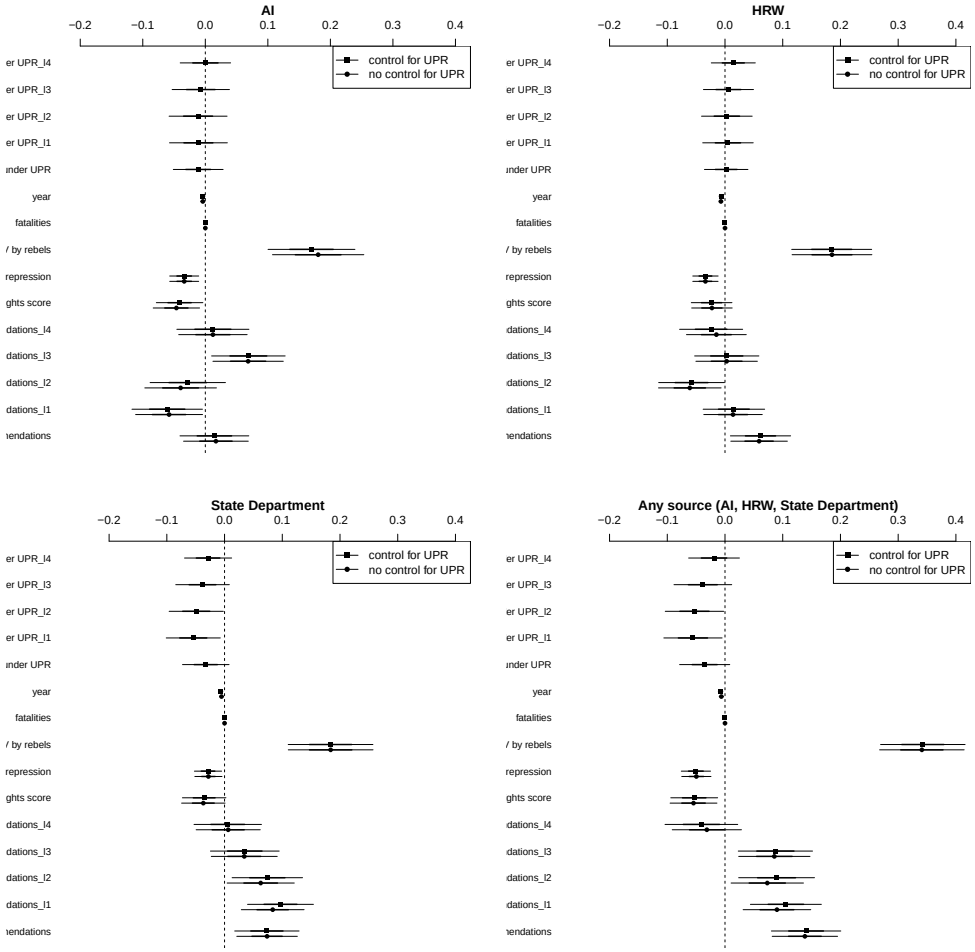

Figure 2: Panel regressions (coefficients and 95 % confidence intervals): recommendations on sexual violence coded by Johansson (2025) and dichotomous outcome

mendations, namely Kenya (2015), Lebanon (2015), Russia (2013), Sri Lanka (2008) and Syria (2011) (see table 2 in the Appendix).

We nevertheless resorted to Imai, Kim and Wang's (2022) approach based on panel matching in an exploratory fashion, relying on the codings provided by (Johansson, 2024).[12] Figure 3 reports the average treatment effects on the treated based on matched

[12]For these analyses we used the same covariates for the matching and requested matched sets of size five. Given the particular setup of the data, however, not for all treated cases could such sets be determined. The actually sizes of the matched sets were 2 (3), 6 (3), 7 (2), 137 (4), 139 (7), 140 (5) and 143 (5) (see Figure 7 in the Appendix for more details).

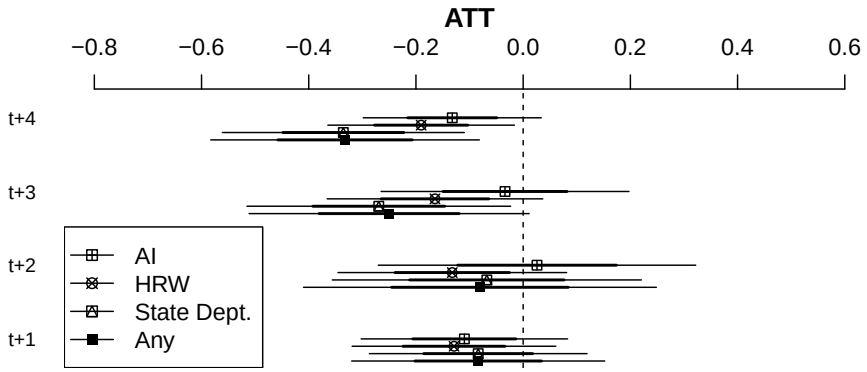

Figure 3: Matched panel regressions (average treatment effect on the treated and 95 % confidence intervals): recommendations on sexual violence coded by Johansson (2025) and ordinal outcome

observations and several controls[13] for the four years preceding the treatment.[14] Contrary the simpler panel linear regressions we fine more evidence in line with our expectations. With one single exception the average treatment effect on the treated is negative, implying that CRSV decreases after CRSV-related recommendations. Over time this effect increases and is largest four years after the UPR.

When considering versions of the outcome measures that are dichotomous we find largely similar results (see Figure 4). UPR recommendations appear to reduce the practice of CRSV especially in the longer term. Interesting to note is that this effect is especially pronounced for the measure based on reports from the US State Department, and thus also for the combined measure.

While these analyses provide some mixed evidence for some effects of UPR recommendations based on CRSV, an issue that appears with the data we have used so far is that in some sense both the treatment as well as the outcome measures are based on shamings. More specifically, reports by AI, HRW or the US State Department all have (at least partially) the aim to shame states for the practice of CRSV (and also broader human rights abuses). UPR recommendations in this regard can be considered as similar, as also the title of Johansson's (2024) study ("Talk of shame") suggests.

[13]The latter were the previous level of sexual violence perpetrated by the government, sexual violence perpetrated by rebel forces, civil-society repression, the human rights record of the state under review and years.

[14]Note that these longer term effects can only be estimated with observations from the first two cycles, as our data on sexual violence from Cohen and Nordås (2014) ends in 2021.

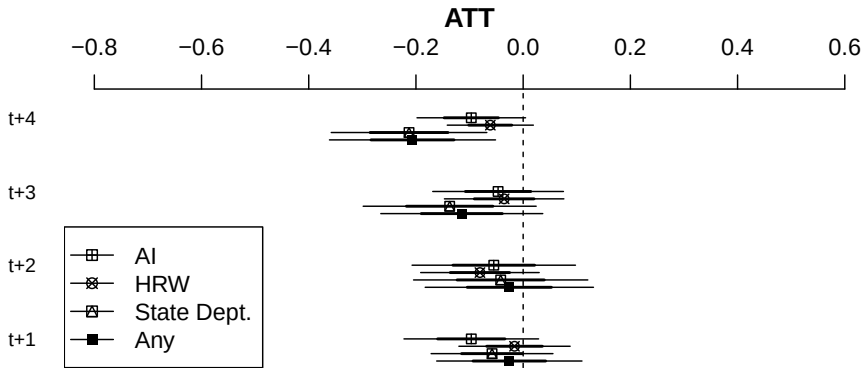

Figure 4: Matched panel regressions (average treatment effect on the treated and 95 % confidence intervals): recommendations on sexual violence coded by Johansson (2025) and dichotomous outcome

Consequently, beyond the endogeneity issue of UPR recommendations, we are likely to face equally a problem of reports that are more or less critical of certain states as a function of what strategies of shaming have been undertaken in the past.

One way around this issue (at least partially) would consist of relying on "objective" measures of CRSV. Data provided by ACLED (Raleigh, Linke and Hegre, 2010) in part fulfils this criterion as it reports events related to sexual violence perpetrated by (amongst others) state actors and also reports the number of fatalities. A drawback of this data source is, however, that its coverage is much more limited in time (see Figure 6 in the Appendix for an illustration).[15] Nevertheless, we carried out a panel matched analyses and depict in Figure 5 the ATT for two outcome measures based on ACLED-data, namely the number of events of sexual violence perpetrated by state forces and the number of fatalities in these events.[16] As the number of country-years for which ACLED-data offers information on sexual violence is much smaller, the number of treated observations and the pool of observations for the matched controls are much smaller, making the estimates more uncertain. Despite of this a trend appears, namely that there is little evidence suggesting that UPR recommendations lead to fewer CRSV events, respectively fatalities. Quite to the contrary the results suggest that both the number of events and the number of fatalities increases and this increase

[15] In Tables 9-11 in the Appendix we report the results of comparisons between these two sources of CRSV-data.

[16] Again, we requested matched sets of size five, but the actual sizes were 3 (3), 4 (2), 6 (2), 32 (4), 33 (2), 34 (5), and 35 (1) (see Figure 7 in the Appendix for more details).

in the latter case becomes even more important as time since the UPR recommendations progresses.

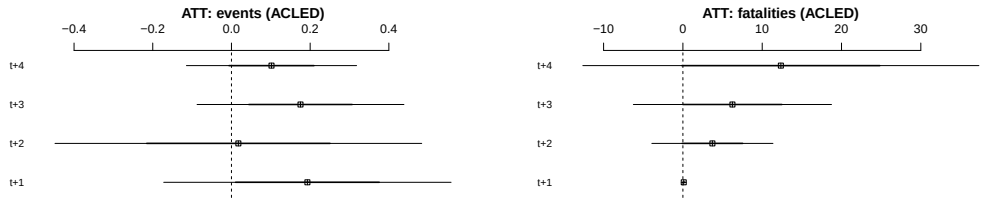

Figure 5: Matched panel regressions (average treatment effect on the treated and 95 % confidence intervals): recommendations on sexual violence coded by Johansson (2025) and ACLED coded sexual violence

5 Conclusion

While among both scholars and stakeholders there emerged some timid optimism with respect to the UNHRC when it replaced the UNCHR and especially its newly institutionalized UPR, solid assessments of the effects of the latter are largely missing from the scholarly literature (or if they exist, these assessments are partial, see McMahon and Botwicz, 2024). In this paper we attempted in a very preliminary way to address this issue. Focusing on a very specific policy domain, namely sexual violence perpetrated by state actors in civil conflicts, we assessed whether states receiving recommendations regarding sexual violence in the UPR adjusted their behavior. The results we reported are mixed. While there appears some evidence that especially over a longer period of time, namely when the next UPR is approaching, states under review are less likely to engage in CRSV after having received recommendations. The extent of this decrease (and whether it can be detected) depends, however, on the sources we rely upon and the assumptions we make when analyzing the data. While when relying on assessments of CRSV perpetrated by states under review based on reports from international NGOs or the US State Department (as the SVAC-data does, see Cohen and Nordås, 2014) we do find a reduction of CRSV following UPR recommendations (depending on the model we employ), when considering event data by Raleigh, Linke and Hegre (2010) the effect becomes positive.

These results have, however, to be taken with a large grain of salt. For our endeavor, the codings of recommendations as dealing with sexual violence is of some concern.

Considering recommendations that are coded either by *UPR Info* or the *Sexual Right Initiative* as dealing with sexual violence suggests that all states perpetrating sexual violence receive recommendations. This is also something that Johansson2024 notes for the more restrictive coding of recommendations she employs. At the same time the main outcome measure we employ, drawing on Cohen and Nordås's (2014) SVAC-data, raises the issue of the extent this data reflects practices of CRSV or the shaming thereof. As analyses relying on the ACLED-data (Weidmann, 2016)which comes with different problems, see for instance come to different results, it is not obvious which results are more convincing.

Hence, while the results presented in this paper allow for a first glimpse at the effects of UPR recommendations on CRSV, the analyses can and should be still improved. First, a more comprehensive coding of recommendations extending the one undertaken by Johansson (2024) would allow for a broader coverage. Second, a more detailed exploration of the reasons that lead to an apparent perfect overlap of states under review perpetrating CRSV and those obtaining UPR recommendations seems necessary. Third, with these two elements in hand it might be possible to develop a better strategy for the analysis of the data.

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Appendix

Data

We created a new dataset using as starting point the most recent version of the SVAC-data that covers the years 1989 to 2021. We aggregated this data to the country year summing up the values separately for the three sources used, namely AI, HRW and State Department both for state and rebel forces. It bears noting that this data only covers countries retained by UCDP, excluding thus a host of smaller states. Hence we merged it with a state x year dataset for this period created by `peacesciencer` package. To this we added Johansson's (2024) dataset aggregated to the country and year level. To get information on the years in which a UN member state was subject to an UPR, we used the full set of UPR recommendations made available by UPR Info and determined for each state the year in which it was reviewed in cycles 1 through 4.[17] This allowed us to determine for all UN member states when they were reviewed. Following Johansson (2024) we add from the V-Dem dataset (Coppedge and Gerring, 2011) the indicator for civil society repression. From the UCDP-GED dataset (Sundberg and Melander, 2013) we use the best estimates for fatalities. From the replications dataset of Fariss (2014) we use the measure of human rights respect. We add from the ACLED dataset (Raleigh, Linke and Hegre, 2010) the number of events coded as sexual violence and the number of fatalities aggregated to the country x year level. From the Quality of Government data (Terman and Voeten, 2018) we added the information from UCDP regarding the information on civil conflicts (Davies, Pettersson, Sollenberg and Öberg, 2025).

Johansson (2024, 436) recodes the SVAC-data as follows:

> The Sexual Violence in Armed Conflict dataset also includes estimates based on United States Department of State (USDOS) Reports. As I wish to see the information that reach the international community at large, rather than one government in particular, I leave estimates by the USDOS out. The original variable is ordinal, ranging from zero to three. Zero and one equal zero in my recoded variable. Two and three equal one.
>
> . . .
>
> I create a variable indicating the maximum value across HRW and AI estimates during the current year and the previous two.

As our endeavor is of a different type, coding CRSV in this way is too restricte. We wish to understand whether states perpetrating CRSV improve on their record once they obtain UPR recommendations. Consequently, whether it is AI and HRW, respectively the US State Department that report on it, is largely secondary, and hence we use all three SVAC sources. Second, we also consider that even low levels of CRSV

[17] We checked this list against the information provided by the UNHRC.

should be affected by UPR recommendations. Hence, using all values of the SVAC codings we obtain the following table of countries that, in the year of their UPR were involved in an internal conflict and over the period of the last three years (including the year of the UPR) engaged in CRSV (according to any of the three sources):

SUR	recommendations	year
Colombia	1.00	2008
	1.00	2013
Russia	0.00	2013
Nigeria	1.00	2009
Central African Republic	1.00	2009
	1.00	2013
Chad	1.00	2009
Kenya	0.00	2015
Burundi	1.00	2008
Ethiopia	1.00	2009
	1.00	2014
	1.00	2019
Mozambique	1.00	2016
Libya	1.00	2015
Sudan	1.00	2016
South Sudan	1.00	2016
Egypt	1.00	2019
Syria	0.00	2011
	1.00	2016
Lebanon	0.00	2015
India	1.00	2012
	1.00	2017
Myanmar (Burma)	1.00	2011
	1.00	2015
Sri Lanka	0.00	2008
Thailand	1.00	2011
Philippines	1.00	2012

Table 1: UPR SUR that committed CRSV in year of UPR or any of the two preceding years according to SVAC-data and recommendations in the UPR

When carrying out the same selection based on the information from ACLED (, Raleigh, Linke and Hegre, 2010) we obtain the much reduced set of cases when considering only events where the perpetrator is the army, the police forces, the military etc.:

SUR	recommendations	year	fatalities
Sudan	1.00	2016	2
South Sudan	1.00	2016	0
Myanmar (Burma)	1.00	2011	0
	0.00	2021	0

Table 2: UPR SUR that committed CRSV in year of UPR or any of the two preceding years according to ACLED-data and recommendations in the UPR

cycle	year	number of countries
1st cycle	2008	10
	2009	10
	2010	5
	2011	9
2nd cycle	2012	6
	2013	11
	2014	9
	2015	6
	2016	7
3rd cycle	2017	5
	2018	14
	2019	9

Table 3: Number of countries receiving recommendations regarding sexual violence per year and cycle according to Johansson (2025)

	2008	2009	2010	2011	2012	2013	2014	2015	2016	2017	2018	2019	2020	2021
Afghanistan	0	0	1	0	0	0	0	1	0	0	0	0	0	1
Burkina Faso (Upper Volta)	0	0	0	0	0	0	0	0	0	0	0	0	0	1
Burundi	1	1	0	0	0	0	0	0	0	0	0	0	0	0
Cameroon	0	0	0	0	0	0	0	0	0	0	0	0	1	1
Central African Republic	0	0	0	0	0	1	1	1	1	1	1	1	0	1
Chad	0	1	0	0	0	0	0	0	0	0	0	0	0	0
Colombia	1	1	1	1	1	1	1	1	1	1	0	0	1	1
Congo, Democratic Republic of (Zaire)	1	1	0	0	1	1	0	0	0	1	0	0	0	1
Cøte D'Ivoire	1	1	0	0	0	0	0	0	0	0	0	0	0	0
Ethiopia	0	0	0	0	0	0	0	0	0	0	0	0	0	1
Georgia	1	1	0	0	0	0	0	0	0	0	0	0	0	0
India	0	0	0	0	0	0	1	1	1	1	0	0	0	0
Iraq	0	1	0	0	0	0	1	1	1	1	1	0	0	1
Libya	0	0	0	1	1	0	0	2	1	0	0	0	0	0
Mali	0	0	0	0	1	1	1	1	1	0	0	0	1	0
Mozambique	0	0	0	0	0	0	0	0	0	0	0	0	0	1
Nigeria	0	0	1	1	1	2	1	1	1	1	1	1	1	1
Philippines	0	0	0	0	0	0	0	0	0	1	0	0	0	0
Rwanda	0	0	0	1	0	0	1	1	0	0	0	0	0	0
Somalia	0	1	1	1	1	1	1	1	1	1	0	1	0	1
South Sudan	0	0	0	0	0	1	1	1	1	1	1	1	1	1
Sudan	0	1	1	0	0	0	0	0	0	0	0	0	0	0
Syria	0	0	0	0	0	0	1	1	1	1	1	1	1	1
Uganda	0	0	1	1	0	0	0	0	0	0	0	0	0	0
Ukraine	0	0	0	0	0	0	0	0	1	1	0	0	0	0
Yemen (Arab Republic of Yemen)	0	0	0	0	0	0	0	1	0	0	1	0	0	0

Table 4: UN member states committing sexual violence since the first UPR cycle until 2021 per year (*Source:* Cohen et al (2014)

statename	year of UPR while perpetrating sexual violence
Burundi	2008
Central African Republic	2013
Central African Republic	2018
Chad	2009
Colombia	2008
Colombia	2013
Cøte d'Ivoire	2009
India	2017
Iraq	2014
Libya	2015
Mali	2013
Mozambique	2021
Nigeria	2013
Nigeria	2018
Philippines	2017
Rwanda	2011
Rwanda	2015
Somalia	2011
Somalia	2016
Somalia	2021
South Sudan	2016
Syria	2016
Uganda	2011
Ukraine	2017

Table 5: UN member states committing sexual violence and receiving recommendations by their peers in UPR per year (*Source:* Cohen et al (2014)

country	start	end
Afghanistan	2008	2024
Algeria	2009	2024
Angola	2008	2024
Azerbaijan	2009	2023
Bangladesh	2008	2022
Burundi	2009	2022
Central African Republic	2009	2024
Chad	2008	2022
China	2008	2023
Colombia	2009	2023
Cøte d'Ivoire	2009	2023
Democratic Republic of Congo	2008	2024
Egypt	2008	2024
Eritrea	2008	2024
Ethiopia	2008	2024
Georgia	2008	2023
India	2009	2024
Indonesia	2009	2022
Iran	2008	2024
Iraq	2008	2023
Israel	2008	2024
Jordan	2010	2024
Kenya	2008	2024
Lebanon	2008	2023
Libya	2008	2024
Malaysia	2010	2023
Mali	2009	2023
Mozambique	2008	2024
Myanmar	2008	2023
Nepal	2009	2023
Nigeria	2009	2024
Pakistan	2009	2024
Peru	2009	2023
Philippines	2009	2024
Russian Federation	2009	2023
Rwanda	2008	2023
Somalia	2008	2024
South Sudan	2008	2024
Sri Lanka	2009	2023
Sudan	2008	2024
Syria	2008	2024
Tajikistan	2009	2023
Tanzania	2008	2024
Thailand	2008	2024
Tunisia	2010	2024
Turkey	2008	2024
Uganda	2008	2023
Ukraine	2010	2024
Uzbekistan	2009	2022

Table 6: Time period covered for UN member states perpetrating sexual violence (*Source:* Cohen et al (2014)

Table 7:

Statistic	N	Mean	St. Dev.	Min	Max
ai_prev.x	1,957	0.058	0.348	0	3
state_prev.x	1,957	0.091	0.436	0	3
hrw_prev.x	1,957	0.039	0.278	0	3
ai_prev.y	1,957	0.033	0.264	0	3
state_prev.y	1,957	0.069	0.386	0	3
hrw_prev.y	1,957	0.033	0.270	0	3
sv_shaming_45_dummy	1,957	0.032	0.175	0	1
v2csreprss	1,740	0.905	1.487	−3.725	3.346
best	1,957	66.772	1,276.210	0	43,780
theta_mean	1,559	0.930	1.665	−2.303	5.336
Cycle_d	1,957	0.183	0.387	0	1
Cycle_dl	1,957	0.188	0.391	0	1
Cycle_dl2	1,956	0.198	0.398	0	1
Cycle_dl3	1,955	0.201	0.401	0	1
Cycle_dl4	1,953	0.203	0.403	0	1
sv_shaming_45_dummy_l	1,957	0.036	0.187	0	1
sv_shaming_45_dummy_l2	1,956	0.038	0.192	0	1
sv_shaming_45_dummy_l3	1,955	0.038	0.192	0	1
sv_shaming_45_dummy_l4	1,954	0.034	0.181	0	1
tot_prev.x	1,957	0.116	0.498	0	3
tot_prev.y	1,957	0.086	0.434	0	3

Table 8: Descriptive statistics

SVAC and ACLED

The following tables compare information on sexual violence perpetrated by state forces according to the ACLED-data (Raleigh, Linke and Hegre, 2010)[18] with the SVAC-coding (Cohen and Nordås, 2014), taking into account that the coverage of ACLED has been extended only recently towards a more global one.[19] The first three tables shows this for country-years that are, according to UCDP-data conflict years, while the remaining tables cover all years.

Country	year	fatalities
Cuba	2018.00	
Haiti	2022.00	X
Jamaica	2010.00	
Jamaica	2012.00	X
Trinidad and Tobago	2004.00	
Dominica	2011.00	
Grenada	2005.00	
Saint Vincent and the Grenadines	2009.00	X
Saint Vincent and the Grenadines	2010.00	
Saint Vincent and the Grenadines	2011.00	X
Saint Vincent and the Grenadines	2012.00	
Saint Vincent and the Grenadines	2021.00	
Guatemala	2000.00	
El Salvador	1989.00	X
El Salvador	1992.00	
El Salvador	1993.00	
Nicaragua	2018.00	
Colombia	2017.00	

Table 9: Cases with ACLED events, respectively fatalities and no AI report during civil war

[18]This data was directly read from a `json`-file using the ACLED `API` (see `https://acleddata.com/api/acled/read?limit=600000&sub_event_type=Sexualviolence`, accessed November 1st, 2025).

[19]Another dataset worth exploring would be Bahgat, Nordås and Østby's (2016) `GEO-SVAC` dataset.

Country	year	fatalities
Cuba	2018.00	
Haiti	2022.00	X
Jamaica	2010.00	
Jamaica	2011.00	X
Jamaica	2012.00	X
Trinidad and Tobago	2004.00	
Dominica	2014.00	X
Grenada	2005.00	
Saint Vincent and the Grenadines	2009.00	X
Saint Vincent and the Grenadines	2010.00	
Saint Vincent and the Grenadines	2012.00	
Saint Vincent and the Grenadines	2021.00	
Guatemala	2000.00	
El Salvador	1989.00	X
El Salvador	1992.00	
El Salvador	1993.00	
Nicaragua	2018.00	
Colombia	2017.00	

Table 10: Cases with ACLED events, respectively fatalities and no HRW report during civil war

Country	year	fatalities
Cuba	2018.00	
Jamaica	2012.00	X
Trinidad and Tobago	2004.00	
Dominica	2011.00	
Dominica	2014.00	X
Grenada	2005.00	
Saint Vincent and the Grenadines	2010.00	
Guatemala	2000.00	
El Salvador	1989.00	X
El Salvador	1992.00	
El Salvador	1993.00	
Colombia	2017.00	

Table 11: Cases with ACLED events, respectively fatalities and no State Department report during civil wars according to SVAC-data

Analyses

Figures 6 and 7 depict the treatment distribution for the analyses based on the SVAC- and ACLED-data, as well as information on the matched sets.

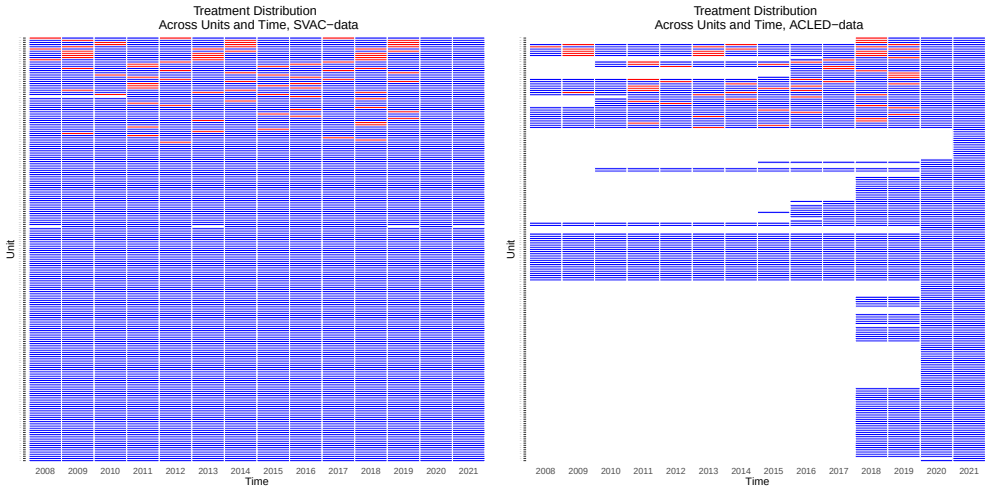

Figure 6: Treatment distribution for analyses based on SVAC- and ACLED-data

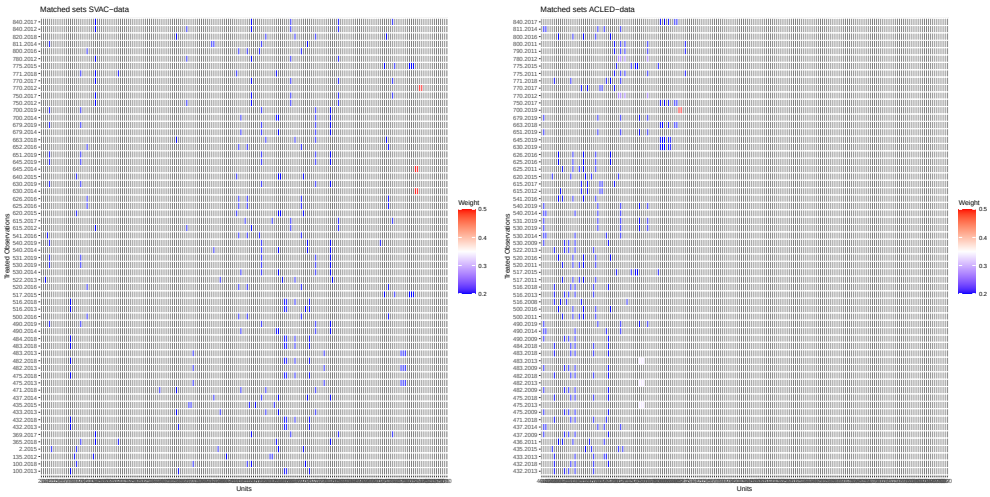

Figure 7: Matched sets for analyses based on SVAC- and ACLED-data