Climate-related Disasters and Temperature Extremes Are Not Associated with Conflict Risk in Pacific Island Countries

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Abstract

Concerns about climate change and conflict are ubiquitous. Systematic research on the climate-conflict nexus in the Pacific is lacking, despite the region's high vulnerability to climate change as well as several past episodes of political instability. Using a new dataset, we analyze the impact of climate-related disasters and of temperature extremes on social conflict events in Fiji, Solomon Islands, and Vanuatu. According to widespread expectations, climate change should engender more social conflicts due to intensified grievances, resource competition (especially about land), and rural-to-urban migration. The results indicate that in the three countries studied, disaster shocks and temperature extremes do not noticeably affect conflict risks, even after controlling political, economic, and environmental context factors. Moreover, major cities, commonly considered most prone to climate-related conflicts in the region, experience lower-than-average conflict risks following disasters. These findings indicate that Pacific Island countries may be more resilient to climate-related conflict risks than commonly assumed.

Key words: climate change; Pacific; resilience; security; war; violence

1 Introduction

Climate change is increasingly considered a security concern by politicians, militaries, peacebuilders, and development practitioners (Vogler, 2023). There is an emerging scholarly consensus that climate change increases the risk of violent conflict within states if certain scope conditions like ethnic exclusion or poor adaptive capacity are present (Buhaug et al., 2023; Ide et al., 2020; Mach et al., 2019; Sharifi et al., 2021).

So far, research has mostly focused on Africa, Asia, and the Middle East. A systematic literature review did not find a single empirical study on the climate-conflict nexus in the

Pacific Island region (Adams et al., 2018). This lack of research impedes our ability to analyze whether theories of the climate-conflict nexus are valid across different geographical settings and world regions. Furthermore, portraying regions like the Pacific as being at high risk of climate-related conflict without empirical evidence risks a stigmatization of these regions. Considering the Pacific Island countries as unable to deal with environmental stress and destined to experience violence in the future could, for instance, preclude much-needed investments in adaptation and livelihood diversity (Barnett, 2017; Telford, 2023).

The lack of studies on climate change and conflict in Pacific Island countries is especially problematic because the region is widely considered to have low levels of human development and to be highly vulnerable to climate change[1] (ND-GAIN, 2023; Weir & Virani, 2011). The region's geopolitical relevance has also been growing with renewed competition between China and the USA (and their respective allies) (Tarte, 2022). Regional experts argue that livelihood insecurity, migration, and land conflicts resulting from climate change can increase conflict risks in the Pacific (Boege, 2022; Campbell, 2022). The Boe Declaration signed by the leaders of the Pacific Island states also argues "that climate change remains the single greatest threat to the livelihoods, security and wellbeing of the peoples of the Pacific"(Pacific Islands Forum, 2018).

Yet comprehensive cross-case evidence on a climate-conflict nexus in the region is still missing. We address this knowledge gap by analyzing the relationship between climate impacts and conflict risks in three Pacific Island countries: Fiji, Solomon Islands, and Vanuatu. We selected those three because they are among the largest four Pacific Island countries in terms of population and land area, making them crucial cases to study. They also display significant variation, for instance in terms of human development and political system, which increases the external validity of our results (we nevertheless encourage an extension of our study to the other Pacific Island countries) (Boege, 2022). Yet, by focusing on countries with a history of political instability and that are highly prone to be adversely affected by climate change, our study follows a most-likely case design for the climate-conflict nexus (Gerring & Seawright, 2007).

[1] Vulnerability is understood here as both a result of exposure to and the inability to address climate change's impacts.

A key reason for why climate-conflict research has so far largely ignored the Pacific Island countries is the lack of quantitative data for the region. To address this gap, we create the first geo-coded dataset on social conflict events in Fiji, Solomon Islands, and Vanuatu for the period 2012-2020 (see section 3).

In the next sections, we discuss our theoretical expectations (2) as well as data and methods (3). Afterwards, we present our results (4). We find that neither temperature extremes nor climate-related disasters have a statistically significant effect on increasing the risks of social conflicts in the Pacific Island countries under study, even when accounting for socio-economic and political context factors. This indicates that the Pacific is could be rather resilient to the climate-conflict nexus, hence casting doubt on the emerging consensus about climate change indirectly increasing conflict risks (5).

2 Theoretical Expectations

In deriving our empirical expectations, we pay specific attention to the indirect pathways underlying the climate-conflict nexus, which have been identified as a major research task by recent assessments of the climate security literature (e.g., von Uexkull & Buhaug, 2021). These pathways help us to identify the most likely cases where the indirect impacts of climate change should presumably lead to conflict. We focus on three (overlapping) pathways, namely climate change (1) contributing to grievances against state institutions (e.g., about insufficient disaster relief) and (2) intensifying resource competition between social groups (e.g., over increasingly scarce land and water resources), both of which might be accelerated by (3) climate-related migration. For instance, if extreme temperatures and disasters adversely impact rural livelihoods, people affected may migrate to urban areas (permanently or temporarily). These migrants can hold grievances against the government or compete about resources with existing residents. For those residents, immigration for political, economic, or cultural reasons could induce grievances (Brzoska & Fröhlich, 2016; Koubi, 2019).

Existing qualitative research on the Pacific Island countries shows that the current and projected impacts of climate change may already contribute to grievances (pathway 1). In recent years, popular discontent has emerged over state-induced (planned) and unplanned resettlements, lack of proper state support after disasters, and economic turbulences induced

by extreme weather events (Boege, 2016; Weir & Virani, 2011). Such discontent is most likely to transform into manifest conflict events like protests in urban areas, where high population density facilitates mobilization and collective action (Staniland, 2010; Toft, 2002).

Furthermore, several recent qualitative studies indicate that climate change affects land and water availability in the Pacific. This can, in turn, contribute to competition over these resources (pathway 2), and particularly regarding land. Livelihoods in the region are often based on agriculture and land has a high spiritual value to many Pacific Islanders. Again, such conflicts may be more likely in urban areas of the Pacific Island countries (even though they will also occur in rural regions), not just because of higher mobilization potential, but also because land is more scarce and valuable there (Shibata, 2022; Weir & Virani, 2011). For instance, a descriptive analysis of our new dataset reveals that 18.6% of all social conflict events are directly related to three natural resources: forests (4.1%), water (0.3%), and especially land (14.1%).[2]

Finally, existing research arrives at diverging conclusions on whether climate-related migration increases conflict risks in Africa and Asia (pathway 3) (Koubi et al., 2021; Petrova, 2021; Reuveny, 2007). Likewise, evidence for an urbanization-conflict nexus is equivocal (Buhaug & Urdal, 2013). By contrast, for the Pacific, there is considerable qualitative evidence that climate-induced environmental stress (and particularly disasters like floods and storms) induce migration from rural to urban areas. Capital cities are the most likely destination due to more social services and jobs being available there. Such migration is expected to happen in the first months after a disaster when resources for in-situ adaptation and recovery are most limited. In turn, migration increases competition over land, which is already scarce in urban areas, and can also contributes to general grievances about a lack of government services (particularly as many displaced persons move into informal settlements) (Boege, 2022; Campbell, 2022; Shibata et al., 2023).

Our new datasets contain several events explicitly in line with this logic. In April 2014, for instance, a series of intense floods struck Solomon Islands. On 16 May of the same year, a group of people displaced by the floods to an informal settlement in the capital city Honiara

[2] We coded a conflict as related to natural resource if local news media explicitly reported access to, usage of, or control over land, water, or forests as an issue of contention.

started a protest-turned-riot because they considered the government rehabilitation package insufficient to cover their disaster-induced losses (event ID 28).

Two additional pathways are frequently discussed with respect to the climate-conflict nexus. (4) Climate change can weaken states, for example by reducing tax income or by overburdening security forces with disaster response tasks, leaving governments unable to deal with challengers like insurgents or warlords. (5) Climate change could enhance the recruitment opportunities of armed groups because, all else being equal, people whose livelihoods has been devastated by extreme climate events face lower opportunity costs to join armed groups (Buhaug et al., 2023; Ide, 2023; Koubi, 2019). However, both pathways depend on the existence of organized armed groups (like rebels or warlords) that can exploit climate-related state weakness and recruitment opportunities. Because no such groups existed in Fiji, Solomon Islands, and Vanuatu between 2012 and 2020, we do not focus on these pathways.

Unfortunately, testing the grievances, resource scarcity, and migration pathways directly for Fiji, Solomon Islands, and Vanuatu is not possible (for statistical analyses) due to insufficient data availability. However, we check whether there is a difference in the impact of climate-related extreme events in major urban areas, where climate-related immigration, migration-related tensions, and the mobilization potential of grievances should be higher (see above and Campbell, 2022). Additional research could further test these pathways specifically either by connecting more fine-grained quantitative data (e.g., in the form of surveys, see Koubi et al., 2021) or in the form of additional qualitative case studies (see Shibata et al., 2023).

3 Methods

The unit of analysis is the 0.5-decimal degree (~55km x 55km cells at the equator) year. Due to data availability, we focus on the period 2012 – 2020. Summary statistics for all variables are provided by Table S1.

To operationalize the dependent variable (social conflict), we built a unique geo-referenced, time-coded dataset on social conflict events in Fiji, Solomon Islands, and Vanuatu for the period 2012-2020 (with few news sources providing comprehensive coverage for the years before 2012). In line with the literature on contentious politics (Tilly & Tarrow, 2015), we

define social conflict events as situations where at least two social groups perceive their interests to be incompatible with each other and take contentious actions to enforce their interests, resulting in events like protests, riots, assaults, and violence against civilians. To identify social groups, we rely on claims made by the event participants (describing themselves as "local residents" etc., while also ascribing a collective identity to their opponents) or on attributions made by local media (referring to the conflict parties as "police", "disaster victims" etc.). Each action carried out by a designated actor (or group of actors) at a specific place during a limited period was considered a conflict event. Data was collected from local media and news sources, which increases the reliability of the data (see SI for a list of sources and a codebook).

In constructing this dataset, we are motivated by the fact that standard conflict datasets are generally unsuitable to analyze conflict risks in the Pacific. Existing data either only include high-intensity violent events (which are rare in the region) in selected countries (UCDP), do not cover the Pacific Island countries at all (SCAD; see Hendrix & Salehyan, 2012), or only cover the region since 2022 and record few events due to their strong reliance on international news media (ACLED) (Öberg & Yilmaz, 2025).

Our two main independent variables are climate-related disasters and temperature extremes. To identify storms, floods, and droughts in the three Pacific Island states, we used the Global Dataset on Geocoded Disaster Locations (GDIS) to identify climate-related disasters in the three countries under study (Rosvold & Buhaug, 2021). As the dataset only covers the years up to 2018, we coded the remaining years following the codebook compiled by the dataset's creators. For descriptive statistics, Figure S1 visualizes the geographical distribution of climate-related disasters and social conflict events in Fiji (panel a), Solomon Islands (panel b), and Vanuatu (panel c). Figure S2 plots annual correlations and Figure S3 plots a breakdown of social conflict rates by type across the three countries. To test an alternative climate-conflict pathway highlighted in the extant research (Ge et al., 2022; Helman & Zaitchik, 2020), we also test the effects of temperature. To this end, we relied on average annual country-level temperature data from the World Bank's Climate Change Knowledge Portal (World Bank, 2024a).

To account for alternative confounders, our analyses also employ a set of control variables and potentially relevant context factors from both the literature on violent conflict and

climate security (Ge et al., 2022; Koren & Schon, 2023). These controls include first variables to account for the effect of development and state capacity on social conflict, for instance, due to greater ability of the state to prevent conflict and lower opportunity for contention. This variable is operationalized as average levels of pixels with illumination based on the DMSP-OLS system within each 0.5-degree cell, calibrated for time series analysis (2012 estimates). Another possibility is that cropland and local agricultural production pressures might shape social conflict (Koren and Schon, 2023). To account for this, we include an indicator for percentage of the 0.5 degree grid cell that was farmland (based on year 2000 estimates).

Next, we account for proximity to the equator (which can affect the frequency and intensity of storms) by acconting for 0.5 degree cell area, seeing that due to the Merctor projection, cells located closer to the equator are slightly smaller. Another possbility is that both social conflict events and storm damage might be more likely in rural areas. To account for this risk, we also included a control estimating the travel time from the center of the 0.5-degree grid cell to the nearest major city (50,000 or more residents). These controls all came from the PRIO-GRID dataset (Tollefsen et al., 2012). Additionally, in some of our robustness models, we use capital city locations. This variable was coded manually based on each city's coordinates.

Although nighttime lights and travel distance may overlap, we decided to still include them in some of the models since they also capture distinct dimensions of development, accessibility, and political centrality. To illustrate our results are not driven by this decision, our baseline model is estimated without controls, and subsequent specifications add them sequentially; results remain stable across models. Moreover, our VIF tests and ridge regression models (described below) address the risk any findings (or lack thereof) are driven by multicollinearity concerns such as this one. Finally, capital city location may also overlap with these indicators, but since this variable is only used in one robustness check, not as a main control, this is less of a concern.

In addition to 0.5-degree cell level indicators, our models also include country level variables to account for the potential political and national confounders. Here, we first account for (log) country population size annually, considering bigger countries might be at a higher risk, e.g., due to having more social groups that perceive their interests as incompatible. We also

account for the size of the economy and its potential impact on both conflict and disaster preparedness by including the (log) gross domestic product per capita (GDP, in 2015 US$). These two indicators were obtained from the World Bank (2024b). Finally, conflict frequency might be shape by the existence of democratic institutions to mitigate tensions. To this end, we include a control for democracy operationalized base on the democracy score from the updated Democracy-Dictatorship data (Bjørnskov & Rode, 2020).

Our modeling strategy designed to identify the effect of climate-related disasters and temperature on conflict proceeds in four steps, adjusting the sets of controls to account for specific concerns. The baseline specification (eq. 1) estimates the relationship between disasters and social conflict while adjusting only for grid-cell fixed effects, lagged conflict, and a linear time trend, thus ensuring that no potential multicollinearity across the independent variables is driving the results. In the second step (eq. 2), we add country-level controls for temperature, population size, GDP per capita, and life expectancy to capture national context while accounting for all constant (non-time varying) cell level confounders. The third specification (eq. 3) instead includes country fixed effects to account for within-country heterogeneity, while adding both time-varying and constant 0.5-degree cell-level covariates for nighttime lights, farmland, and travel distance to the nearest city, and omitting country-level controls to ensure these are not impacting the observed relationships. The final specification (eq. 4) then combines both sets of controls in addition to using country fixed effects.

This sequential approach ensures that any relationship between climate disasters, temperature and conflict is not driven by the specific concerns each model seeks to address. To additionally address temporal and spatial dependence, all models include a lagged dependent variable and a linear time trend. Standard errors are clustered by grid cell to adjust for local autocorrelation and spatial clustering. This modeling strategy balances parsimony with rigor and helps ensure that null findings are not artifacts of model misspecification. Moreover, it helps to effectively minimize concerns about serial autocorrelation and spatial clustering. The inclusion of a lagged dependent variable and a linear time trend captures persistence in conflict dynamics over time, while clustering standard errors by grid cell adjusts for within-cell correlation across years and local spatial dependence. This approach provides a conservative test of our hypotheses, as failure to account for such dependence would

typically inflate significance rather than produce null results. Accordingly, the stability of our findings across all specifications reinforces that they are not artifacts of temporal or spatial autocorrelation.

Formally, the models are identified using the following equations:

$$y_{it} = \beta_1 s_{it} + \beta_2 d_{it} + \beta_3 y_{it-1} + \beta_4 t_t + \boldsymbol{\omega_i} + \epsilon_i \quad (1)$$

$$y_{it} = \beta_1 s_{it} + \beta_2 d_{it} + \beta_3 y_{it-1} + \beta_4 t_t + \beta_{5-8} X_{jt} + \boldsymbol{\omega_i} + \epsilon_i \quad (2)$$

$$y_{it} = \beta_1 s_{it} + \beta_2 d_{it} + \beta_3 y_{it-1} + \beta_4 t_t + \beta_{5-7} Z_{it} + \boldsymbol{\psi_i} + \epsilon_i \quad (3)$$

$$y_{it} = \beta_1 s_{it} + \beta_2 d_{it} + \beta_3 y_{it-1} + \beta_4 t_t + \beta_{5-8} X_{jt} + \beta_{9-11} Z_{it} + \boldsymbol{\psi_i} + \epsilon_i \quad (4)$$

Here, the subscripts *i*, *j*, and *t*, correspond to the 0.5-degree grid cell, country, and year, respectively, and β are each independent variable's respective coefficient; y_{it} is a variable measuring social conflict events and y_{it-1} its lag; s_{it} are the number of storms and d_{it} are the number of non-storm disasters in a given cell during a given year; $\boldsymbol{\tau_t}$ is the time trend; X_{jt} are country level controls for temperature, (log) life expectancy at birth, (log) population, and (log) GDP per capita; Z_{it} are 0.5-degree cell level controls for nighttime light, presence of farmland, and travel time to the nearest city; ω_i and ψ_j are fixed effects by 0.5-grid cell and country, respectively; and ε_i are standard errors clustered by grid cell to adjust for local heterogeneities. As we discuss below, we conducted a wide set of robustness checks – including varying disaster measures, disaggregating conflict types, adding political controls, standardizing disaster indicators, and estimating system GMM models, and distinguishing the effects of disasters in the capital from other areas – to test the sensitivity of our OLS findings. To this end, in the latter (Table S14), which climate-induced disasters and temperature across urban and rural areas, we adjusted equations 3 and 4 as follows:

$$y_{it} = \beta_1 sd_{it} + \beta_2 u_{it} + \beta_3 sd_{it} \,\mathrm{X}\, u_{it} + \beta_4 y_{it-1} + \beta_5 t_t + \beta_{6-8} Z_{it} + \boldsymbol{\psi_i} + \epsilon_i \quad (5)$$

$$y_{it} = \beta_1 sd_{it} + \beta_2 u_{it} + \beta_3 sd_{it} + \beta_4 y_{it-1} + \beta_5 t_t + \beta_{6-9} X_{jt} + \beta_{10-12} Z_{it} + \boldsymbol{\psi_i} + \epsilon_i \quad (6)$$

Here, sd_{it} is the aggregate indicator of all climate-induced disasters, u_{it} is an indicator of whether a city was a capital, and sd_{it} X u_{it} is their interaction. Otherwise, equations 5 and 6 are identical to equations 3 and 4, respectively. All resource-driven subsample models similarly use equations 1-6.

Additionally, as discussed above, we are aware that using fixed effects at different geospatial levels as well as the country level variables and cell-level variables that measure similar features, can all introduce multicollinearity and serial autocorrelation in 0.5 grid level analysis that may lead to falsely observing null results, we also conduct two sets of tests specifically designed to assess the impact of collinearity on our estimates. First, we report models where the 0.5-grid cell or country fixed effects (which induce multicollinearity) are removed (Table S15). Second, we conduct variance inflation factor (VIF) tests, finding that multicollinearity is not a concern in all 0.5-degree grid level variables (VIF<5), including in our two disasters variables (VIF = ~1) (Table S16). However, we also find that our country-level variables do exhibit, unsurprisingly, noticeable (VIF>5) multicollinearity.

Therefore, to systematically assess the impact of multicollinearity, we additionally estimate a set of ridge regressions corresponding to each equation. Ridge regressions seek to minimize the residual errors caused by multicollinearity by "penalizing" (i.e., reducing or fully omitting the effect of) specific independent variables (Hoerl & Kennard, 1970). To this end, it introduces a "shrinkage penalty" term, λ, into the residuals. When λ = 0, this penalty term has no effect and ridge regression produces the same coefficient estimates as the OLS models; as λ approaches infinity, the shrinkage penalty becomes more influential, and the ridge regression coefficient estimates approach zero.

The estimation process takes place in two steps. In the first step, we use *k*-fold cross-validation, where *k*=10, to estimate the λ value that minimizes the test mean squared errors. For this purpose, we standardize all our predictors to ensure no single predictor variable is overly influential when performing ridge regression. In the second step, we take the best λ values, and then estimate the coefficients of each of our (standardized) predictors using this value, to identify each variable's "multicollinearity penalized" coefficient, and compare if it statistically different from zero using standard errors from each corresponding OLS model, which would suggest multicollinearity is driving our key results.

4 Results

To assesses this relationship, Figure 1 visualizes the OLS coefficients of climate-related disasters for a change in the expected number of social conflicts, after (1) adjusting for 0.5

grid fixed effects, conflict lags, and time trends (eq. 1); (2) then adding country controls for temperature, life expectancy, population, and GDP per capita (eq. 2); (3) adding country fixed effects with local level controls for nighttime light, farmland, and distance from big cities (eq. 3); and then (4) adding the country controls from (2) (eq. 4) (Table S2 reports estimates for all models).

Even after adjusting for socioeconomic, political, and other environmental confounders, the results demonstrate that climate-related disasters have no statistically significant impact on conflict risks in Fiji, Solomon Islands, and Vanuatu. Equality of coefficient tests suggest there is no statistical difference in effect size between storms and other types of disasters (with χ^2 tests *p*-values ranging from 0.2 to 0.99). Adjusted R^2 values across the models range from 0.45 (CFE full) – 0.64 (GFE country), suggesting the models provide overall good fit for these observational data. Moreover, while some studies highlight temperature as a key driver of conflict risks (Helman & Zaitchik, 2020), we find it has no noticeable effect on social conflict in our sample (Figure 2).

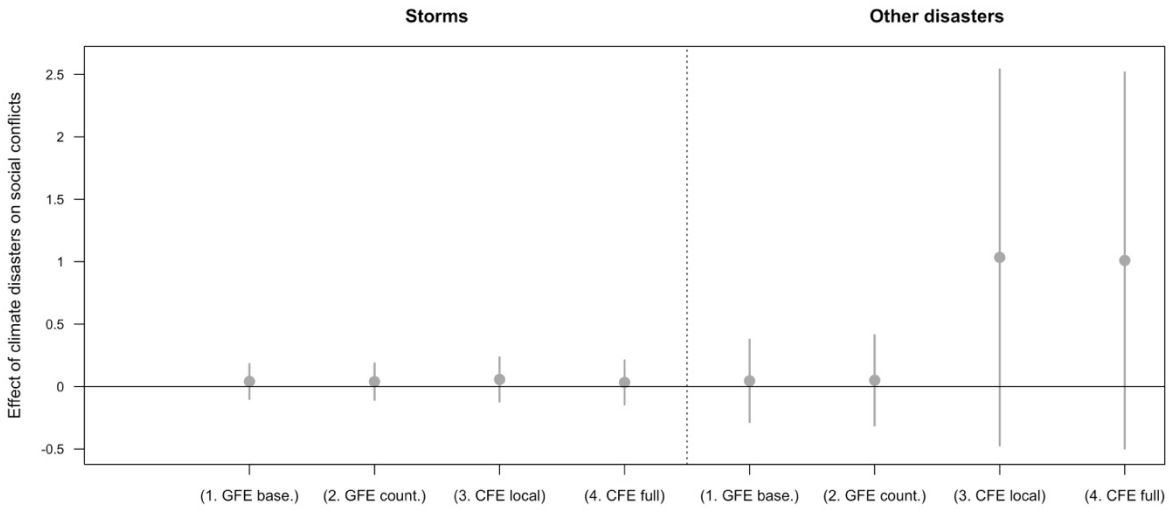

Figure 1: The effect of climate-related disasters on social conflict in three Pacific Islands. Coefficient estimates for storm (left) and non-storm (right) disasters are reported with 95% confidence intervals for each respective model. Numbering corresponds with specifications discussed above.

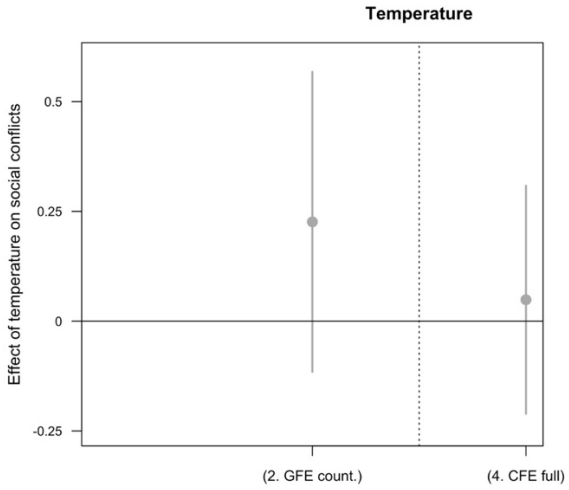

Figure 2: T effect of temperature on social conflict in three Pacific Islands. Coefficient estimates for temperature are reported with 95% confidence intervals for each respective model. Numbering corresponds to the specifications discussed above.

To test the sensitivity of our OLS results, we conduct a range of robustness checks (Tables S3–S14 in the SI). We begin by varying the disaster indicators. Because storms are the most frequent disasters in our sample (28 compared to 11 non-storm events), we re-estimate the models (1) using only storms, (2) using only non-storm disasters, and (3) using an aggregated disaster indicator. In addition, we estimate models with (4) country-by-year fixed effects, which absorb all confounders that vary at the country-year level. These checks confirm that our findings are not sensitive to how disasters are measured or to unobserved national shocks (Tables S3–S6).

Next, we assess whether the null findings are driven by the aggregation of different types of social conflict. We re-estimate the models disaggregating the dependent variable into (5) protests, (6) riots, (7) assaults, and (8) violence against civilians. In all cases, the results remain substantively unchanged (Tables S7–S10), indicating that our conclusions are not an artifact of outcome aggregation.

We also consider institutional and political context. Adding an indicator for democracy and removing fixed effects does not alter the results, suggesting they are not contingent on a particular modeling choice for regime type or on the inclusion of fixed effects (Table S11).

There is also a possibility that a better way to account of the impact of disaster is by standardizing them based on anomaly in disaster frequency. To this end, we estimate models where each disaster indicator is constructed by standardizing their effect time, subtracting the mean of storm and non-storm disaster for each 0.5-degree cell from the annual indicator, and then dividing it by the standard deviation (Table S12).

A further concern is potential endogeneity between disaster onset and conflict, which could bias OLS estimates toward zero. To address this, we re-estimate equations (1) and (2) using generalized method of moments (GMM), specifically system GMM models (Blundell & Bond, 1998). These models generate internal instruments that help "exogenise" the effect of climate-related disasters on conflict. They also incorporate two-way effects that adjust for both year and grid-cell heterogeneity, obviating the need for fixed effects or linear time trends, and employ two-step robust standard errors for additional reliability. The system GMM estimates (Table S13) corroborate the OLS results, indicating that the null findings are not artifacts of simultaneity between disasters and conflict.

Finally, we explore whether disasters affect capital cities differently from other areas, given potential dynamics of migration, land pressures, or institutional capacity. We interact the disaster indicator with a capital-city dummy (Suva, Honiara, Port Vila). Results (Table S14) show no significant interaction effect: disasters do not increase or decrease conflict risk in capitals relative to other areas. However, the constitutive capital term is positive and significant, indicating that capitals were generally more conflict-prone during disaster periods, regardless of disaster shocks themselves.

Turning to our collinearity effect tests, we first find that removing 0.5-degree grid (eq. 1-2) or country (eq. 3-4) fixed effects have no impact on our disaster coefficients' size and significance (Table S15). Moving on to our ridge regressions, designed to specifically account for the risk that some highly autocorrelated country level variables affect the results (Table S16), we find that in all four ride specifications, the most penalizing value of λ is practically =0, meaning that the OLS estimates are unaffected by multicollinearity (Figure S4).

Moreover, to test whether the coefficients of both our storm and non-disaster coefficient become statistically significant after adjusting for multicollinearity in these ridge regression models, we first estimate our four OLS models using the same standardized data used in the ridge regression to create a standardized version of the OLS coefficients. We then use the

formula $\beta_{\text{ridge regression}} - 1.97 \times SE_{\text{OLS regression}}$ to test whether the penalized coefficients are now statistically significant. As long as this formula is < 0, which happens in every case, the penalized/adjusted coefficient fails to reach statistical significance. This means that the (lack of) effect of each storm and non-storm disaster coefficient is practically the same across the ridge regression and OLS models, and hence multicollinearity is not inducing lack of significance in these models.

5 Discussion

Using a new database on localized social conflict events – and accounting for key confounders, endogeneity, and multicollinearity – our analysis finds no impact of climate-related disasters and temperature extremes on social conflict risks in three Pacific Island countries: Fiji, Solomon Islands, and Vanuatu. If these results are illustrative of a broader trend, they have several relevant implications.

Many experts argue that climate change tends to (indirectly) increase conflict risks in settings characterized by high climate vulnerability and pre-existing tensions, with low-level (community) conflicts being particularly sensitive to climate-related disasters (Boege, 2022; Mach et al., 2019; Unfried et al., 2022). Our study casts doubt on these claims. Even when focusing on most-likely cases (Pacific Island countries), potential hotspot areas within such contexts (urban centers), and conflict types particularly sensitive to climate change (low-intensity conflicts), we do not detect a significant relationship between climate extremes and conflict risks.

Granted, while theory emphasizes mechanisms such as grievances, land competition, and migration, our empirical leverage is necessarily limited to identifying aggregate associations rather than directly observing these causal pathways. We are also aware that our time series may appear relatively short and that the number of disasters is limited, which could raise concerns about statistical power explaining the null results. Nevertheless, the dataset still spans 13 years across 72 distinct grid cells (n=936), providing substantial variation over time and space that allows us to detect more than just very large effects. Therefore, despite these concerns, we believe our finding at least indicate that claims about a climate-conflict nexus should be moderated. This is in line arguments made by Daoust and Selby (2023) for Western

Africa, Koren and Schon (2023) for the Sahel, and Dinc and Eklund (2023) for the Middle East, among others.

Moreover, if our findings are correct, this does not mean that climate change will have no conflict implications for the Pacific in the future. Precipitation extremes (which are not completely covered by our analysis due to data availability issues) might drive higher conflict risks. Likewise, climate-related inter-island migration and slow livelihood deterioration could increase conflict risks over large distances or long time periods, going beyond the spatial and temporal lags employed here. Future sea-level rise will further aggravate the situation. Finally, the climatic changes projected for the next decades will very likely be more severe than what the region experienced in recent years. We also did not study all Pacific Island countries and all potential climate-conflict pathways. Nevertheless, our results caution against calls that climate change significantly increases conflict risks in contexts of high climate vulnerability and pre-existing conflict risks.

Our findings further indicate that the Pacific Island countries are not destined to face a protracted conflict crisis in a climate-changed world. This is in line with other evidence that communities in the Pacific region are remarkably resilient and can successfully adapt to climate change in-situ, suggesting they may be more resilient than expected (Farbotko & Campbell, 2022; Keen et al., 2022).

In Vanuatu, for instance, Community Climate Change Committees were effective in preparing locals for cyclone Pam in 2015 and played a key role in delivering relief and recovery support. Such community cooperation often cut across local cleavages and therefore likely even lowered conflict risks (this is in line with other success stories of disaster diplomacy in the Pacific; see Ide et al., 2025). The government's response to the cyclone was also swift and well-coordinated, hence boosting citizen's support of state institutions (Keen et al., 2022). Understanding the drivers of such peaceful responses despite major climate impacts will be a key task for future research that could focus on, for instance, local social capital and the response strategies by governments and donors.

In these regards, future qualitative case studies or mixed-methods designs could complement our aggregate quantitative analysis by illuminating specific micro-level mechanisms – such as community cooperation, institutional trust, or donor engagement – that can explain when and where societal impacts of climate disasters are mitigated. These mechanisms currently

remain hidden in large-N data. Another approach would be to improve large-N data coverage and resolution. Finer-grained measures – such as monthly climate anomalies (building on our robustness model in Table S12 that uses annual local anomalies), cyclone track data, and displacement records – would sharpen estimates and allow for a closer alignment between theoretical mechanisms and observed outcomes. Together, these approaches would provide a fuller picture of why disasters sometimes mitigate rather than exacerbate conflict risks in the Pacific Island countries.

If climate-change related livelihood loss, social disintegration, and conflict in the Pacific are assumed to be inevitable, incentives to support local adaptation efforts and peacebuilding processes are likely to decline (Barnett, 2017). Our findings instead provide room for cautious optimism: If even highly vulnerable countries with a history of political instability do not yet experience climate-related conflicts, there is still room to build climate-resilient peace (Barnett, 2019)

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