

Quantifying Assets for the Geopolitics of Climate Change Mitigation

Corresponding Author: Dr Guanna Pan

Attachments originally included by the reviewers as part of their assessment can be found at the end of this file.

Version 0:

Reviewer comments:

Reviewer #1

(Remarks to the Author)

Review of NATSUSTAIN-25106791

The paper presents an interesting analysis. I have some concerns and suggestions for improvement, expressed below, which I expect the authors can address. The main concern is that the implications of this work should be discussed more thoughtfully. I was expecting more analysis of how the country groupings match with countries' climate ambitions, but the paper didn't deliver very well in that aspect. Deepening the analysis and/or the discussion towards this direction would strengthen the relevance of the paper considerably.

Major comments:

1. I think the authors should articulate better the implications of their work, i.e., answer better the 'So what?' question. There is some discussion to that direction in the final section, but this should be improved. Now the implications are a bit hanging up in the air.
2. Somewhere in the manuscript should be more clear indication of past work of similar scope. There exist also literature using game theory for coalition formation, which could be referenced here.
3. On the selection of scenarios, I was thinking would RCPs 4.5, 2.6 and 1.9 make a more representative set of the current situation rather than the ones used by the authors. My understanding is that RCP6.0 is unlikely to be realized without a major rollback of climate commitments. Conversely, it would be interesting to have a scenario more aligned with 1.5C. I would like to hear what the authors think about this.
4. The measures for fossil asset losses, renewable energy and land carbon sequestration are concrete and easy to understand. However, climate damages are a broad and bit abstract term. Please discuss briefly what kinds of damages the used estimates entail in concrete terms. Also, is there potential overlap in this measure with the other measures considered?
5. The value of renewable electricity is calculated by multiplying avoided emissions with the carbon price in the mitigation scenario (equations 7 and 8). However, the difference between baseline and mitigation scenario spans a wide range and the marginal value of carbon increases from zero to the used carbon price value across that range. I think the value should not be calculated with the mitigation scenario carbon price, but integrated from baseline to the mitigation scenario. As a rough approximation, if one assumes a linear increase in the marginal value, this would lead to 50% of the value that you present here. This might not affect your clustering or conclusions, but the renewable electricity value would be lower overall.
6. Regarding land carbon sequestration, it remained unclear to me what the land-use projections were based on, as this was not stated on lines 125 to 137. Also, was there active land-based mitigation assumed on these scenarios and were there possibly some constraints present for this (e.g. land availability, self-sufficiency etc.)? Please clarify.
7. What is the relevance of the cross-country analysis (line 205 onwards)? I did not understand what question this tries to answer, although the section seemed somewhat interesting nevertheless.
8. There is potential for condensing the text throughout the manuscript, which I advise the authors to do. For example, there is bit too much of listing each impact by country.

Specific comments:

Figure 1: It would be better to move the scale legend next to the row description on the left. Now the cryptic acronyms ('ffot' etc.) are not clear enough to interpret the scale. Additionally, the scale would be then next to the subfigure which presents the most variations. Figure resolution should be improved, as well.

Figure 2: It would be good to have the full names of the country groupings next to the cluster legend in panel b). I'm not very

sure if panel a) is necessary here, as it doesn't even show what the optimal number of clusters is.

Figure 3: It would be good to avoid 'programming code' in the radar plots. (E.g., what does 'cs_managed' mean?) In the caption, perhaps 'home country' -> 'representative country'.

L110: The IAMs used here do not portray specific countries. How the authors came from the regional representation of IAMs to country level results?

L190: This section starts suddenly a new topic: possible alignment between countries. This should be introduced and discussed better throughout the manuscript (please see major comment #1).

L249: 'which may expect differences' But does it actually explain? Please discuss this more thoroughly (also see major comment #1). Lines 283-285 seem to indicate the contrary.

L258-267: This part seems like random collection of recent developments, and is too Eurocentric to be relevant for a paper like this. Please consider removing or revise thoroughly.

(Remarks on code availability)

Reviewer #2

(Remarks to the Author)

Please see the attached pdf

(Remarks on code availability)

Reviewer #3

(Remarks to the Author)

Major Comments

The paper addresses a timely topic, aiming to unpack potential coalitions among countries based on their asset portfolios. The manuscript is well-written, and the analysis and results are generally presented clearly. However, despite the potential of the topic, I have significant reservations regarding the study's framing and its alignment with political reality.

First, the analysis appears somewhat detached from political reality. This is because it overlooks the issue of "scale," although the authors acknowledge this limitation at the end of the manuscript (L303). This is exemplified by the "closest pairs" identified in the study: the US and Thailand; China and Bosnia & Herzegovina. Treating these nations as pairs – given the large difference in economic size, geopolitical influence, and population – makes it difficult to draw actionable conclusions.

Second, the paper lacks the perspective that the climate problem is fundamentally a development problem. The analysis does not account for the state of development (e.g., GDP per capita), nor does it address historical responsibility – an underlying, persisting source of friction in climate negotiations. While I recognize that not every aspects can be included in a quantitative analysis, the utility of study's outcome would be limited without contextualizing the results also considering these dimensions.

Third, the introduction frames the climate mitigation largely as an arena for self-competition and maximization. While trust may be eroding, the Paris Agreement's original spirit was for the global commons. Incorporating this contrast between national self-interest and the collective goal would allow the analysis to better illuminate conflicts that should be resolved.

Furthermore, the statement on novelty (around L38) is insufficient. The paper should elaborate on what exactly the novel aspect(s) of this study are.

Specific Comments

L23-25: I am not sure what this finding means, given the political reality where the US, EU, and China take very distinct positions from each other.

L36-37: It is important to know if and how each country's economic scale is considered in the analysis. Please be more specific about how the country's share in current global GDP entered into the analysis.

L38: refs 9 and 10 are a global study.

L49: Remove the hyphen between "we" and "quantity"

L85: Please define "fossil fuel rent losses" here. While it is described in the Methods, a short description here would help.

L104: Growth-based damage functions are known to yield large impact estimates. Why are the impacts estimated here generally modest? A brief explanation of how the growth-based damage functions affect the results would be helpful.

L119: Fig 1 is too small to distinguish individual countries. For example, Bhutan mentioned in the text is invisible.

L134: Although not central to the analysis, it would be interesting to indicate countries with net carbon sources in a different color. They are hard to distinguish from others in Fig. 1.

L157: Please discuss why the k-means clustering results were retained. The authors could also mention that the clustering results are highly uncertain, as the three methods yield consistent results only for a small subset of countries

(Supplementary Fig. 3).

L191: What do "natural emission assets" mean? Are they equivalent to carbon sequestration capacity?

L193: But the GDP share of CSE and REE is even lower than 4.4%.

L201: The interesting interpretation is more clearly stated here than in the Abstract. I recommend revising the corresponding

statement in the Abstract based on this sentence.

L215-217: The identified pairs (US and Thailand; China and Bosnia & Herzegovina) are unexpected. However, as noted in my main comments, given the vast differences in area, population, GDP, and development state, it is difficult to extract realistic insights from this finding.

L361-362: Please add an explanation for 0-lag and 5-lag. How are they defined to address immediate and delayed economic response?

Table 1: Typo for “They most of them” in Key Characteristics of ADE.

Fig. 3: Please spell out the full country name either in the figure or figure caption.

(Remarks on code availability)

Version 1:

Reviewer comments:

Reviewer #1

(Remarks to the Author)

The authors have responded to my comments and improved the manuscript diligently. The comments from the other two reviewers very complementary to my observations, and these seem to be addressed appropriately, as well. This is an interesting analysis and I'd like to recommend it for publication.

(Remarks on code availability)

Reviewer #2

(Remarks to the Author)

The authors have carefully revised the manuscript and provided detailed responses to the reviewers' comments. Considerable effort has been made to address both the conceptual and methodological concerns raised during the first round of review. The revised manuscript is substantially improved in terms of methodological transparency, robustness analysis, interpretation of the asset indicators, and discussion of the limitations of the proposed framework.

In particular, I found the revisions concerning the country-level disaggregation of fossil-fuel revenues, the expanded documentation and sensitivity analysis of the climate-damage estimates, the clarification of the relationship between the clustering and similarity analyses, and the treatment of spatial-resolution limitations to be convincing. The additional analyses and more cautious framing of the geopolitical implications also strengthen the manuscript considerably.

I have only a few minor points that the authors may wish to consider in the final version. First, it would be useful to consistently emphasize that the four asset dimensions are conceptually heterogeneous and that the clustering should be interpreted as a descriptive multidimensional typology rather than as an aggregation of economically equivalent gains and losses. Second, the renewable-energy indicator should be described carefully as a monetized or policy-valued technical mitigation potential, rather than as an expected realized economic benefit, since actual deployment also depends on infrastructure, investment capacity, institutions, and other country-specific constraints. Finally, GDP may be more appropriately described as a proxy for economic weight or potential leverage rather than as a comprehensive measure of geopolitical or negotiating power.

Overall, however, I consider that the authors have responded appropriately and constructively to the reviewers' concerns, and I find the revised manuscript substantially stronger.

(Remarks on code availability)

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Additional Revisions Made During Revision

In addition to the revisions made in response to the reviewers' comments, we conducted additional consistency checks and made several updates to improve the robustness, accuracy, and transparency of the analysis. Importantly, the main climate-asset patterns, the defining characteristics of the identified clusters, and the central conclusions of the study remain robust to these updates.

We excluded countries with a land area smaller than 2500 km² from the final clustering and similarity analyses because the climate-model and land-use-model outputs used to estimate renewable energy mitigation potential are processed at 0.5° spatial resolution. We used 2500 km² as an approximate threshold because it is comparable to the area represented by a 0.5° × 0.5° grid cell at low to mid latitudes; countries below this threshold are represented by very few grid cells and are therefore less reliable for grid-based renewable energy estimates. This exclusion affects the country coverage of the renewable energy mitigation potential indicator, but does not affect the fossil fuel rent loss disaggregation, which does not rely on climate or land-use gridded data. The final similarity and clustering analyses are therefore conducted for 169 countries.

We also corrected several implementation and input-related inconsistencies identified during additional checks. These include updates to the renewable energy mitigation potential calculations (land suitability for onshore wind and rooftop PV potential), the input file for the Kalkuhl-Wenz damage estimates, the aggregation of avoided damages from a pooled median to a two-step function median across climate models and damage functions, and the carbon price input used to estimate land carbon sequestration capacity. The carbon price update primarily affects the absolute magnitude of land carbon sequestration capacity values and doesn't change the normalized indicators used in the clustering and similarity analyses.

Together with the revisions made in response to the reviewers' comments, these updates lead to revised country-level climate-asset indicators and consequently to updated similarity relationships and cluster assignments. We have updated the corresponding figures, tables, and text accordingly. While country assignments and asset-profile-similarity counterparts are updated under the revised indicators, these changes do not affect the overall interpretation of the identified climate-asset patterns or the study's main conclusions.

Point-by-Point Responses

Reviewer #1 (Remarks to the Author):

Review of NATSUSTAIN-25106791

The paper presents an interesting analysis. I have some concerns and suggestions for improvement, expressed below, which I expect the authors can address.

We thank the reviewer for the detailed comments. We believe addressing all of them has significantly improved the manuscript.

The main concern is that the implications of this work should be discussed more thoughtfully. I was expecting more analysis of how the country groupings match with countries' climate ambitions, but the paper didn't deliver very well in that aspect. Deepening the analysis and/or the discussion towards this direction would strengthen the relevance of the paper considerably.

We thank you for this comment which prompts us to better clarify what the purpose of our analysis is. As we detail below, our paper is mainly aimed at providing information about changes to environmental and energy assets through the low carbon transition, and resulting geopolitical challenges and opportunities that arise from heterogeneity.

While we are confident and argue that our indicators shape incentives for domestic climate action and international strategies taken by countries, we believe that the factors that determine final incentives go beyond our partial analysis so that we do not provide this analysis as a main contribution in the paper.

To address your comment, we emphasize more clearly that our analysis is partial, add evidence where our data maps to actions directly as the interests we describe are so dominant (such as: losses in fossil fuel rents leading to resistance; damages from climate change leading to ambitious positions) and contextualize our findings when our indicators are clearly a partial picture to full incentives for climate action. Furthermore, we provide some quantitative insights into how our indicators map to climate policy ambition in Supplementary Section S7 that we discuss in the main text when describing our indicators and clusters. The quantitative analysis provides support that larger fossil fuel rent losses are associated with lower climate policy ambition across six different measures of climate policy. There is also some support that higher avoided damages map to more ambitious/stringent policy, but the evidence is not strong as avoided damages are negatively associated with ambition for 1 of the policy indicators at a statistically significant level. Evidence on a positive relationship between policy ambition/stringency and both renewable energy's mitigation potential and carbon sequestration potential is even lower, but the data does not speak against this relationship.

We opt not to emphasize the quantitative analysis in the main text because it is partial (only our four asset indicators enter as explanatory variables) and because there is little data available to measure climate policy ambition on a global scale. The NDC ambition score by (Tørstad, V., Sælen, H. & Bøyum, L.S. (2020): The domestic politics of international climate commitments: which factors explain cross-country variation in NDC ambition? *Environmental Research Letters* 15:024021. DOI: 10.1088/1748-9326/ab63e0) covers many countries but has normative assumptions behind it to rate the ambition of a country's NDC. The other three databases (the CCPI score, the CAPMF database and the Climate Action Tracker score) cover much fewer countries (61 or less) that are often biased toward high-income countries.

Major comments:

1. I think the authors should articulate better the implications of their work, i.e., answer better the 'So what?' question. There is some discussion to that direction in the final section, but this should be improved. Now the implications are a bit hanging up in the air.

In response to this and other comments from you and the other two reviewers, we have made significant changes throughout the manuscript. All these changes highlight that our analysis focuses on quantifying country-specific gains and losses from climate change mitigation in the assets we consider, thereby creating varying interests in climate change mitigation across countries. To explain why these differences matter for interests in climate change mitigation, we have added further literature from political science and economics in the main section.

We then identify three geopolitical implications from the dataset. First, conflict that emerges when some countries predominantly experience losses from decarbonization. Second, we identify similarities across countries to identify shared interests. Third, we identify complementary asset profiles such that interests can be aligned through nuanced international strategies. Through our analysis, we attempt to explain why cooperation is difficult even when mitigation is globally beneficial, and figure out potential complementarities and tensions under climate mitigation that can aid

stakeholders to identify fruitful partners and strategies to enhance climate action. We have especially revised the first paragraphs in the main section to clearly explain our contribution to the reader.

2. Somewhere in the manuscript should be more clear indication of past work of similar scope. There exist also literature using game theory for coalition formation, which could be referenced here.

Thank you for this comment as a clear distinction to the literature was lacking in the manuscript. In response to this and a comment by another reviewer, we have added further literature and a clear distinction at the start of the main part of the paper.

3. On the selection of scenarios, I was thinking would RCPs 4.5, 2.6 and 1.9 make a more representative set of the current situation rather than the ones used by the authors. My understanding is that RCP6.0 is unlikely to be realized without a major rollback of climate commitments. Conversely, it would be interesting to have a scenario more aligned with 1.5C. I would like to hear what the authors think about this.

We thank the reviewer for this suggestion on scenario selection. Our intention in choosing SSP2-RCP6.0, SSP2-RCP4.5, and SSP2-RCP2.6 was to maintain a consistent socioeconomic pathway (SSP2) while varying the level of mitigation ambition. This design allows us to isolate how different mitigation levels influence the distribution of climate mitigation-related gains and losses across countries, without conflating these effects with differences in socioeconomic development pathways.

Within this framework, SSP2-6.0, SSP2-4.5, and SSP2-2.6 are used to span a range of mitigation levels, from limited to relatively stringent climate mitigation. In this context, SSP2-6.0 serves as a higher-emissions reference pathway representing limited mitigation, rather than a projection of current efforts into a future trajectory.

We agree that scenarios aligned with the 1.5°C target (e.g., SSP1-1.9) are highly relevant. However, such scenarios are typically associated with a different socioeconomic pathway characterized by stronger sustainability transitions, technological shifts, and higher levels of international coordination. Including SSP1-1.9 would therefore introduce simultaneous changes in both mitigation ambition and socioeconomic structure, making it more difficult to isolate the effects of mitigation intensity itself, which is the primary focus of our analysis.

To clarify this rationale, we added a dedicated explanation in the Methods section (Main text: Lines 335-348), where we explicitly state that the three SSP2-based pathways were selected to attribute differences across scenarios primarily to mitigation ambition rather than to broader socioeconomic transformations.

4. The measures for fossil asset losses, renewable energy and land carbon sequestration are concrete and easy to understand. However, climate damages are a broad and bit abstract term. Please discuss briefly what kinds of damages the used estimates entail in concrete terms. Also, is there potential overlap in this measure with the other measures considered?

We thank the reviewer for this comment. Compared with fossil fuel rent losses, renewable energy mitigation potential, and land carbon sequestration capacity, climate damages represent a broader macroeconomic measure. The damage estimates used in

this study are derived from multiple empirical damage functions, including both growth-based and level-based approaches. These functions estimate the impacts of temperature change on GDP and therefore represent aggregate macroeconomic climate damages across multiple sectors and activities, including agriculture, labor productivity, health, and energy demand, rather than a specific physical damage category. To make this more explicit, we added the following sentence in the Methods section: *"These damage functions estimate the impacts of temperature change on GDP and thereby represent aggregate macroeconomic climate damages."* (Main text: Lines 408-410).

We acknowledge that some conceptual overlap with other indicators may exist because climate change can influence multiple economic activities simultaneously. However, the other three indicators quantify mitigation-related assets or transition-related losses rather than aggregate temperature-related macroeconomic damages. In particular, the land carbon sequestration indicator values mitigation-driven changes in carbon stocks rather than avoided damages to land productivity, while fossil fuel rent losses represent transition-related producer revenue losses. We report all four indicators separately and do not aggregate them into a single benefit measure, thereby avoiding arithmetic double-counting.

To clarify these issues, we have added a new Supplementary Section S2.5 discussing the potential overlap between climate damages and the other indicators, and we now provide a detailed description of the damage functions and their associated parameters in Supplementary Section S2.

5. The value of renewable electricity is calculated by multiplying avoided emissions with the carbon price in the mitigation scenario (equations 7 and 8). However, the difference between baseline and mitigation scenario spans a wide range and the marginal value of carbon increases from zero to the used carbon price value across that range. I think the value should not be calculated with the mitigation scenario carbon price, but integrated from baseline to the mitigation scenario. As an rough approximation, if one assumes a linear increase in the marginal value, this would lead to 50% of the value that you present here. This might not affect your clustering or conclusions, but the renewable electricity value would be lower overall.

We thank you for your thoughtful comment. However, we chose not to use the integral for this measure because we aim to give an economic value of the natural solar and wind resources of each country that can be used for climate change mitigation through this measure.

We conjecture your suggestion could be due a poor explanation of how we quantify this indicator, for which we apologize. We have therefore made several changes to our manuscript in response to your comment.

Most importantly, while keeping the computation as is, we have rephrased the explanation of the indicator in the section "Identifying country-specific gains and losses across assets" entirely (Main text: Lines 117-124). There we explain that we measure a country's natural resources in renewable energy through their technical potential to generate energy from wind and solar. We connect the technical potential to generation needed for mitigation by scaling it with the share of a regional's total potential that is utilized for electricity generated in the mitigation scenarios compared to the baseline. Monetization of the technical potential is done by multiplying the scaled technical potential with the carbon intensity of the baseline (giving a measure

of potential emissions avoided through the natural resources of a country) and the regional carbon price.

We also emphasize now that our measure does not represent an actual change in GDP but represents an economic value of the emissions that can be avoided by the country's natural resources under climate change mitigation.

Lastly, we have also rephrased the explanation of the indicator in the Methods section (Main text: Lines 444-495) to emphasize that we start from the natural resources of a country, and then give a monetary measure of the value of the natural resource of the country under mitigation.

6. Regarding land carbon sequestration, it remained unclear to me what the land-use projections were based on, as this was not stated on lines 125 to 137. Also, was there active land-based mitigation assumed on these scenarios and were there possibly some constraints present for this (e.g. land availability, self-sufficiency etc.)? Please clarify.

We thank the reviewer for this comment. We have revised the manuscript to clarify the basis of the land-use projections, the representation of active land-based mitigation, and the constraints governing mitigation uptake. We added a brief clarification in the Results section (Main text: Lines 135-137): *"In these scenarios, active land-based mitigation is included in MAgPIE and constrained by land availability and competing land demands, see Methods."* We also added a more detailed description in the Methods (Main text: Lines 503-512; second paragraph of Section "Land carbon stocks and sequestration capacity"):

"Land-use projections are generated by MAgPIE, while LPJmL provides biophysical constraints on land productivity, water availability, and carbon stocks. In the two mitigation scenarios, MAgPIE simulates active land-based mitigation in response to carbon prices, bioenergy demand, and land-use targets. The moderate and stringent mitigation scenarios apply NDC-based land-use targets for afforestation, deforestation, and land conversion. They differ in their GHG-price and second-generation bioenergy-demand pathways, which are aligned with cumulative emission budgets of 1000 and 650 GtCO₂, respectively. Mitigation uptake is constrained by competing demands for land from food, feed, seed, and timber production, as well as by protected areas, conversion emissions, opportunity costs, and temporal lags in carbon-stock development¹¹⁰."

7. What is the relevance of the cross-country analysis (line 205 onwards)? I did not understand what question this tries to answer, although the section seemed somewhat interesting nevertheless.

As already outlined in the response to your first comment, we early on state that we look for similarities across countries to identify shared interests.

To address your comment further, we revised the start of the similarity analysis to clearly state why it is important: *"To inform stakeholders about countries with shared interests in climate action that could be relevant for forming strategic alliances, we perform a similarity analysis which identifies common asset profiles between any two countries."* (Main text: Lines 216-218).

8. There is potential for condensing the text throughout the manuscript, which I advise the authors to do. For example, there is bit too much of listing each impact by country.

We thank the reviewer for this suggestion and agree that several sections of the manuscript could be made more concise. In response, we carefully revised the text throughout the manuscript to reduce repetition and streamline the presentation of the results. In particular, we reduced the number of country-specific examples in the result descriptions, shortened several discussions of individual clusters and similarity relationships, and refer readers to the Supplementary Information for additional details where appropriate.

Specific comments:

Figure 1: It would be better to move the scale legend next to the row description on the left. Now the cryptic acronyms ('fftot' etc.) are not clear enough to interpret the scale. Additionally, the scale would be then next to the subfigure which presents the most variations. Figure resolution should be improved, as well.

The scale legend has been moved next to the row descriptions on the left, the indicator labels have been clarified, and the figure resolution has been improved.

Figure 2: It would be good to have the full names of the country groupings next to the cluster legend in panel b). I'm not very sure if panel a) is necessary here, as it doesn't even show what the optimal number of clusters is.

We added the full names of the country groupings next to the cluster legend in panel b) of Fig. 2 (now panel a)). We also removed the original panel a).

Figure 3: It would be good to avoid 'programming code' in the radar plots. (E.g., what does 'cs_managed' mean?) In the caption, perhaps 'home country' -> 'representative country'.

We replaced the abbreviated variable names in the radar plots with more descriptive labels and changed "home country" to "representative country" in the caption.

L110: The IAMs used here do not portray specific countries. How the authors came from the regional representation of IAMs to country level results?

We have revised both the beginning of this paragraph (Lines 117-124) and the Methods section (Lines 444-495) to clarify how country-level renewable mitigation potential is derived from regional IAM outputs. We now explicitly state that "*each country's renewable energy's mitigation potential is proportional to its technical potential to generate energy via PV, onshore and offshore wind, estimated using CMIP6 climate projections and land-use suitability from 3 IAMs*" (Main text: Lines 117-120). We further clarify that "*technical potential translates to each country's potential to avoid emissions by scaling with the avoided emissions using renewable energy generation at the regional level in the mitigation scenarios compared to the baseline*" (Main text: Lines 120-122).

Thus, country-level estimates are based on country-specific natural renewable resources, while regional IAM outputs are used to translate these resources into mitigation potential under different climate scenarios.

L190: This section starts suddenly a new topic: possible alignment between countries. This should be introduced and discussed better throughout the manuscript (please see major comment #1).

We now introduce the lense of aligned asset profiles in the second paragraph of the main section and cite literature that discusses in more detail how alignment can create shared interests and possibilities for resolving trade-offs in international governance.

In addition, we have rephrased the discussion in this paragraph to explain in more detail that countries in the ADE and CSE/REE clusters have complementary asset profiles that can be aligned to create climate action (Main text: Lines 192-204).

L249: ‘which may expect differences’ But does it actually explain? Please discuss this more thoroughly (also see major comment #1). Lines 283-285 seem to indicate the contrary.

In response to this comment, together with the overall revisions about the contribution of our analysis, we have emphasized the partial character of our analysis. To make this clear, we have added indications (Main text: Lines 293-304) about why the US and China differ in their positions, even though they belong to the same cluster from our analysis. Due to space, the discussion remains limited but we hope that this clarifies our contribution and limitations better.

L258-267: This part seems like random collection of recent developments, and is too Eurocentric to be relevant for a paper like this. Please consider removing or revise thoroughly.

We agree that these examples were too specific and not sufficiently relevant to the broader scope of the paper. We have therefore removed them and revised the discussion to focus on the more general mechanisms identified in the literature, including research and development cooperation, trade agreements, and transfers.

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Reviewer 2: Comments on the paper: “Quantifying Assets for the Geopolitics of Climate Change Mitigation”

The manuscript proposes an asset-based quantification of how climate mitigation reshapes countries’ incentives by combining (i) fossil-fuel revenue losses under mitigation vs baseline; (ii) avoided macroeconomic damages from reduced warming; (iii) monetized renewable electricity mitigation potential (PV, onshore and offshore wind) based on avoided CO₂ times a carbon price; and (iv) monetized land carbon sequestration based on annual land carbon stock changes times a carbon price. All quantities are discounted and expressed as shares of discounted cumulative GDP, evaluated for mid-century (2020–2050) and end-century (2020–2100) under SSP2-2.6 and SSP2-4.5 relative to SSP2-6.0 (baseline). The authors then use k-means clustering on four aggregate indicators to define five country types (FFE, ADE, CSE, REE, MIE), validate clusters with alternative clustering approaches, and add a distance-based similarity analysis on eight sub-dimensions (coal/oil/gas; damages; solar/wind; natural/managed sequestration) to highlight potential alignments in negotiations. Overall, while the manuscript is timely and promising, my comments below emphasize the need to strengthen conceptual consistency and methodological robustness, especially the downscaling assumptions, documentation of the damage calculations, interpretation of the clustering results, and limitations from coarse spatial resolution, to better support the geopolitical conclusions.

We thank the reviewer for agreeing with us about the importance of our analysis. We believe that addressing all your valuable comments has significantly improved our paper.

Comments

1. Conceptual comparability of the four assets

The manuscript combines: (i) fossil-fuel revenue losses, which is a mostly private sector/terms-of-trade concept; (ii) avoided macro damages, which is a social welfare/aggregate output concept; and (iii - iv) carbon-price-valued mitigation/sequestration, which are policy-shadow-price $\times$ quantity concepts. Treating these as comparable assets risks category errors when you later interpret clusters as reflecting stakes and bargaining incentives.

We very much agree that our four indicators are of different nature and have now added your categorization of their role (private/public/policy-induced) when we introduce the four indicators in the main section. In this paragraph we also argue why country's interests are influenced by the four assets we quantify.

In our later analyses of clustering and similarity, we aim at identifying similarities in asset-based profiles, that is we assert that countries are comparable along individual dimensions. Thus, we do not directly offset one indicator's value with that of another. You are however of course right that we do assert that a higher value of a country's renewable mitigation potential is meaningful compared to its lower indicator for fossil fuel rent losses when interpreting that the country belongs to the REE cluster. Thus, for interpreting the cluster analysis, we are now more careful to discuss that when a high value of, for example, renewable mitigation potential is decisive for a country to belong to the "High-potential renewable economies" cluster, the countries in this cluster have particular interests in climate change mitigation compared to other countries but this interest is shaped by the renewable mitigation potential indicator being economic value that is induced by climate policy.

We have added this clarification at the end of the third paragraph in 'Main' (Lines 79-82) and made sure that we discuss our findings with this understanding in mind.

To strengthen our indicator's relevance for climate policy, we perform cross-country regression analysis across 6 climate policy indicators and add average climate policy ambition by cluster in a new Supplementary Section S7. The quantitative analysis provides support that fossil fuel rent losses are negatively associated with climate policy ambition/stringency when considering 6 different scales of climate policy. There is also some support that higher avoided damages map to more ambitious/stringent policy, but the evidence is not strong as damages are negatively associated with ambition for 1 of the policy indicators at a statistically significant level. Evidence on a positive relationship between policy ambition/stringency and both renewable energy's mitigation potential and carbon sequestration potential is even lower, but the data does not speak against this relationship.

We opt not to emphasize the quantitative analysis in the main text because it is partial (only our four asset indicators enter as explanatory variables) and because there is little data available to measure climate policy ambition on a global scale. The NDC ambition score by (Tørstad, V., Sælen, H. & Bøyum, L.S. (2020): The domestic politics of international climate commitments: which factors explain cross-country variation in NDC ambition? *Environmental Research Letters* 15:024021. DOI: 10.1088/1748-9326/ab63e0) covers many countries but has normative assumptions behind it to rate the ambition of a country's NDC. The other three databases (the CCPI score, the CAPMF database and the Climate Action Tracker score) cover much fewer countries (61 or less) that are often biased toward high-income countries.

2. Use of regional IAM outputs downscaled to countries may drive some geopolitical conclusions (Fossil Fuels)

The authors downscale regional revenues to countries using historical production shares (obtained from GEM), implicitly assuming future production shares do not shift with mitigation. This is a strong stationarity assumption, particularly for the end-century horizon (to 2100). In reality, the within-region distribution of production is likely to evolve endogenously with: (i) resource depletion and reserve exhaustion (shifting comparative advantage over time); (ii) heterogeneous extraction costs and carbon-intensity of supply (mitigation pathways tend to concentrate remaining production in lower-cost segments) (McGlade and Ekins, 2015); and (iii) geopolitical disruptions (conflicts, sanctions, trade rerouting, and shifting energy-security strategies) that can change long-run supply patterns and access to markets (Aizenman et al., 2024), weakening the plausibility of extrapolating pre-2020 shares to 2100, particularly for countries whose exports are exposed to geopolitical risk. Therefore, this assumption can potentially affect who is a winner and loser (e.g., Middle East vs higher-cost producers). Although the authors briefly mention this as a caveat in their discussion, my suggestion is to: i) more explicitly acknowledge this limitation in the Methods/Discussion; and (ii) add a sensitivity analysis by reallocating regional production/revenues using alternative rules, such as reserve-weighted shares (proved reserves or remaining resource base) or cost-curve-weighted shares (greater weight to lowercost producers) or other rule.

We thank the reviewer for this insightful comment. We agree that allocating regional production to countries solely based on recent production shares implies a strong stationarity assumption, particularly over long-time horizons. We also note that the previous version of the manuscript inaccurately referred to these as "historical production shares". In fact, the allocation was based on recent country-level production data, and we have corrected this terminology throughout the revised manuscript.

In response, we revised our methodology to introduce a time-varying allocation scheme in which country-level production shares gradually shift from production-based shares toward reserve-based shares over time. This approach allows the within-region distribution of production to evolve, partially reflecting changes in comparative advantage associated with long-term supply potential. Specifically, the allocation parameter (λ_t) determines the relative contribution of production- and reserve-based shares and increases over time. We evaluate multiple specifications of λ_t , including alternative functional forms (linear, exponential, and logistic) and different terminal reserve weights (λ_{2100}). We additionally conduct stress tests under extreme assumptions in which allocation becomes fully reserve-based by 2050, 2075, or 2100. Across these alternative specifications, results remain highly consistent with the main specification. Country-level total fossil fuel rent losses exhibit strong correlations across alternative allocation schemes, indicating that the main conclusions are robust to plausible variations in allocation assumptions. At the level of individual fossil fuel types, coal and oil remain highly robust across specifications, while natural gas exhibits greater sensitivity under extreme early reserve-based allocation, particularly in end-century scenarios. However, this sensitivity declines substantially under more gradual transitions toward reserve dependence (Supplementary Table 8).

We now discuss the remaining limitations of the allocation framework in the Methods (Main text: Lines 396-403). We acknowledge that this framework does not fully capture all potential drivers of within-region production shifts, including endogenous cost-based reallocation, geopolitical disruptions, trade restructuring, or explicit reserve depletion dynamics. These processes are difficult to represent consistently at the global scale due to limited and heterogeneous data availability. In particular, globally consistent estimates of remaining recoverable fossil fuel resources are not available

across countries. We therefore use proved reserves as a proxy for long-term supply potential. However, proved reserves reflect current economic and technological conditions rather than the full geological resource base.

Nevertheless, the revised framework captures a broad range of plausible allocation pathways between production-based and reserve-based distributions. The high consistency of results across both sensitivity analyses and extreme stress-test scenarios suggests that our conclusions are not driven by a specific allocation rule and remain robust to substantial uncertainty in future within-region fossil fuel production dynamics. The revised methodology is described in the Methods section (Main text: Lines 361-403), while detailed sensitivity analyses and stress-test results are provided in Supplementary Section S1.

3. Use of regional IAM outputs downscaled to countries may drive some geopolitical conclusions (Renewables)

Similar to the above, the authors downscale regional renewable generation to countries using a deployment index equal to (regional generation / regional technical potential), then apply it uniformly within a region proportional to each country's potential. This can strongly overallocate generation to high-potential but low-demand, weak-grid, or politically unstable countries, consistent with extremely large shares of GDP reported for some countries (e.g., Somalia ~110% renewable mitigation potential by end-century). My suggestion is to add a sensitivity analysis with alternative downscaling rules (e.g., proportional to projected electricity demand or to current installed capacity or using country-level IAM outputs where available).

We thank you for your thoughtful comment, which is a correct observation. However, we chose not to implement this sensitivity analysis because we believe it would not be consistent with our aim to estimate country's natural assets in renewables.

We conjecture your suggestion could be due a poor explanation of how we quantify this indicator. We have therefore made several changes to our manuscript in response to your comment.

Most importantly, we have rephrased the explanation of the indicator in the section "Identifying country-specific gains and losses across assets" entirely (Main text: Lines 117-124). We emphasize that we measure a country's natural resources in renewable energy through their technical potential to generate energy from wind and solar. We connect the technical potential to generation needed for mitigation by scaling it with the share of a region's total potential that is utilized for electricity generated in the mitigation scenarios compared to the baseline. Monetization of the technical potential is done by multiplying the scaled technical potential with the carbon intensity of the baseline (giving a measure of potential emissions avoided through the natural resources of a country) and the regional carbon price.

We also emphasize now that our measure does not represent an actual change in GDP but represents an economic value of the emissions that can be avoided by the country's natural resources under climate change mitigation (Main text: Lines 115-117).

Lastly, we have also rephrased the explanation of the indicator in the Methods section to emphasize that we start from the natural resources of a country, and then give a monetary measure of the value of the natural resource of the country under mitigation (Main text: Lines 444-495).

Thus, we do not consider GDP or observed electricity demand for downscaling because this would not represent the potential contribution of each country's natural resources to mitigation.

4. Economic damage methods need clearer documentation

You compute population-weighted annual temperature at country level and apply both growth- and level-based damage functions, combining BHM, DJO, Kalkuhl & Wenz, Tol, and Hope. For growth-based functions, results are highly sensitive to whether you use absolute temperature vs anomalies, how you treat model bias, and how lags are implemented. The Methods section currently does not make these steps fully transparent (there is just a reference to coding materials). The authors should add the relevant equations/steps explicitly in the SI (or main text).

We thank the reviewer for highlighting the importance of transparency in the implementation of the damage functions.

To address this, we have expanded the Supplementary Information to explicitly document the functional forms and implementation steps for each damage function. We now provide the full equations and clarify key differences across specifications, including the use of temperature levels relative to a baseline (e.g. BHM), year-to-year temperature changes (e.g. DJO), interaction effects with lagged temperature changes (e.g. Kalkuhl-Wenz), and level-based damage formulations (e.g. Tol and Hope).

We also clarify the construction of temperature inputs, the treatment of heterogeneous responses, and the implementation of lag structures. Importantly, we now explicitly describe how climate impacts are incorporated into GDP projections, distinguishing between additive growth effects, multiplicative formulations, and direct level impacts across models. For models originally estimated at sub-country resolution (e.g. Kalkuhl-Wenz), we clarify that temperature is computed at the sub-country level and aggregated using population weights to country level.

Finally, we clarify how discounted damages are aggregated, how avoided damages are computed across scenarios, and how results are combined across damage functions. In the revised manuscript, the main estimate is obtained using a two-step function-median aggregation: climate-model uncertainty is first summarized within each damage function by taking the median across climate models, and the final estimate is then obtained as the median across damage functions. Uncertainty is therefore represented through multiple damage functions and climate-model ensembles. A pooled median across all climate-model and damage-function estimates is additionally reported as a sensitivity analysis. We further evaluate the sensitivity of the results to the choice of damage functions by comparing the main estimate with alternative subsets, including growth-based only, level-based only, and specifications excluding BHM or Kalkuhl-Wenz. These additions ensure full transparency and reproducibility of our results (see Supplementary Section S2).

5. GDP share as a proxy for negotiating power

Using a country's share of current global GDP as a first-order proxy for negotiating power is defensible (it captures material resources, ability to fund large delegations, and capacity to offer side-payments). However, GDP is not the only (or always the dominant) determinant of influence in climate bargaining, and treating it as the sole proxy risks overstating the link between economic size and leverage. Empirically, work on climate negotiations finds that bargaining success and positions reflect multiple drivers, including vulnerability to climate

impacts, domestic political constraints, and institutional features, not only GDP. For example, Weiler (2012) reports that external power (operationalized with GDP) and vulnerability both help predict bargaining success, while other factors also matter. Similarly, Bailer and Weiler (2015) argue that countries' negotiation positions are shaped by structural/economic factors, vulnerability, and domestic political characteristics (e.g., democratic status). My suggestion is to add a short sensitivity/robustness section where negotiating power is proxied not only by GDP share, but by a small set of complementary indicators (or a composite index/PCA), such as GHG emissions, fossil-fuel export dependence, climate vulnerability/exposure, etc.

In response to this comment, we have added the indicator of share of global greenhouse gas emissions by cluster as an indicator for influence on international climate policy ambition in Table 1. Here we found conflicting explanations in the literature: countries with higher emissions were shown to have less success in bargaining (Weiler 2012) while Victor (2011) discusses countries with higher emissions to be more powerful because they control most emissions that cause the global externality. We have added this lense to our discussion about power in the main text (Lines 180-182, 205-207, 277-289).

We opted not to include vulnerability and fossil-fuel export dependence as both indicators are very close to our main indicators of avoided climate change damages and fossil fuel rent losses. See the table below that shows these indicators by cluster. Instead, we discuss that higher vulnerability can lead to more bargaining success and that a larger fossil fuel industry can decrease climate policy ambition when we discuss the role of our two indicators on interests in climate action throughout the text, also citing the new literature.

Table: Share of GDP, GHG emissions and population by cluster, average fossil fuel export share and average ND-GAIN vulnerability score. N is the number of countries in the cluster. Fossil fuel export share is the share of fuel exports in total merchandise exports (%). ND-GAIN vulnerability score is the 2023 country vulnerability index from the Notre Dame Global Adaptation Initiative (higher values indicate greater vulnerability).

Cluster	N	Avg. fossil fuel export share (%) ^{cd}	Avg. ND-GAIN vulnerability score	Share of global GDP ^a	Share of global GHG ^b	Share of global pop.
ADE	51	10.9	0.470	10.7%	14.5%	29.2%
CSE	17	21.7	0.491	0.4%	0.7%	2.9%
FFE	19	66.5	0.384	6.0%	10.4%	5.9%
MIE	69	8.9	0.368	81.0%	72.2%	59.0%
REE	13	17.3	0.535	2.0%	2.2%	2.9%

^aGDP totals extrapolated to 2024 using regional growth rates for Afghanistan, Bhutan, Eritrea, Lebanon, New Caledonia, Syria, Yemen.

^bGHG emissions of Serbia and Montenegro were derived by taken the aggregate from the EDGAR database and splitting by GDP.

^cFossil fuel exports as a share of merchandise exports (World Bank TX.VAL.FUEL.ZS.UN). The following countries use data from a year other than 2024 (year in brackets): ADE: Burundi (2023), Cuba (2021), Djibouti (2023), Equatorial Guinea (1983), Eswatini (2023), Ethiopia (2023), Guinea (2016), Guinea-Bissau (2015), Haiti (2016), Israel (2022), Palestinian Territories (2023), Rwanda (2022), Sierra Leone (2018), Sudan (2018), Syria (2010), Timor-Leste (2023), United Arab Emirates (2023), Yemen (2019); CSE: Cameroon (2023), Congo - Brazzaville (2023), Congo - Kinshasa (2023), Gabon (2023), Laos (2023), Liberia (2023), Papua New Guinea (2023), Solomon Islands (2017), Vanuatu (2011); FFE: Algeria (2017), Botswana (2023), Iran (2022), Iraq (2016), Libya (2019), Turkmenistan (2000), Venezuela (2013); MIE: Bangladesh (2018), Belarus (2020), Cape Verde (2023), French Polynesia (2022), Ghana (2023), Jamaica (2023), Nepal (2022), New Caledonia (2015), Russia (2021), Serbia (2007), Tajikistan (2023), Vietnam (2023); REE: Afghanistan (2019), Bhutan (2023), Chad (1975), Eritrea (2002), Mali (2023), Mongolia (2023), Somalia (1982).

^dCountries with no fossil fuel export share data available (excluded from cluster average): MIE: Puerto Rico.

6. Cross-cluster nearest neighbours indicate typology limitations

The observation that some countries' closest counterpart lies outside their assigned cluster (e.g., Mexico in ADE and Thailand in MIE being each other's most similar) is not necessarily an error, but it highlights a limitation of the typology: clustering on four aggregate indicators can mask heterogeneity within clusters and create boundary cases where countries are closer to members of another cluster once the underlying sub-components are considered. In other words, the partition is driven by the compressed 4D representation, while meaningful similarity is assessed in a richer 8D feature space. This is an important issue since the paper uses the clusters for interpretive claims about country types and negotiation-relevant alignments. If a non-trivial number of countries are nearest to countries in other clusters, readers may (rightly) worry that cluster membership is being over-interpreted as a definitive descriptor rather than a coarse categorization.

We thank the reviewer for this important comment and agree that the clustering results should be interpreted as a broad country typology rather than a definitive description of countries' climate interests. In response, we revised the manuscript to emphasize more clearly the complementary roles of the clustering and similarity analyses. In the revised manuscript, we explicitly state that the clustering results *"show which countries experience structurally similar changes across the four assets, indicating common patterns in their potential interests in mitigation"* (Main text: Lines 156-158); and the similarity analysis *"complements the clustering analysis by revealing similarities in the underlying asset structure that may not be fully captured by the four aggregate indicators used for clustering"* (Main text: Lines 222-224). Furthermore, we clarify that *"these cross-cluster relationships suggest that countries assigned to different broad asset-profile clusters may nevertheless share specific climate-related interests, highlighting potential alignments that extend beyond the broader country typology"* (Main text: Lines 227-230).

Quantitatively, across all 169 countries, 146 (86.4%) have their closest counterpart within the same cluster. Among the remaining 23 countries whose closest counterpart belongs to a different cluster, 18 are linked to countries in their second-nearest k-means cluster (Main text: Lines 224-227; Supplementary Section S5). These results suggest that cross-cluster nearest-neighbour relationships are mainly concentrated near cluster boundaries and do not undermine the broader country typology.

Following this comment, we also revised the Abstract and "Discussion and limitations" section of major economies such as the US, China, and the EU to avoid over-interpreting cluster membership and to emphasize that countries belonging to the same broad cluster may still differ substantially in climate strategies and policy positions because of factors outside our framework (Main text: Lines 290 -305).

7. Spatial resolution of climate inputs

The use of $0.5^\circ \times 0.5^\circ$ gridded climate data raises a non-trivial concern for small countries, and especially small-island states, where strong coastal gradients, steep topography, and land-sea mask issues can make grid-cell averages poorly representative of conditions experienced by local populations. This is not only a precision issue: if country-level temperatures are used to drive damage functions (and potentially land-carbon outcomes), coarse resolution can introduce systematic biases (e.g., smoothing extremes or misplacing land points). In addition,

with 0.5° grids for PV/wind and land carbon, some small states may be poorly represented. The IPCC explicitly notes that downscaling is pivotal for small islands because their diversity makes generalization invalid and data limitations constrain robust impact modelling. The authors should add a brief discussion on that issue.

We thank the reviewer for raising this important point regarding the use of 0.5° × 0.5° gridded data, particularly for small countries and small-island states. We agree that coarse spatial resolution may not fully capture local climatic conditions in such regions. Coastal gradients, complex topography, and land-sea mask effects can reduce the representativeness of grid-cell averages and may smooth climatic extremes. We also acknowledge that these issues are especially relevant for small-island contexts, where spatial heterogeneity is high relative to country size.

To reduce potential biases associated with coarse-resolution gridded data, we exclude countries with land areas below 2500 km² from the analysis. We now explicitly state this in the Methods section: *"All spatial datasets were harmonized to a common spatial resolution of 0.5° × 0.5°. To reduce potential biases associated with coarse-resolution gridded data, we excluded countries with land areas below 2500 km² from the analysis."* (Main text: Lines 357-359). The threshold was chosen because a 0.5° × 0.5° grid cell corresponds to an area of approximately 2500 km² at low and mid latitudes.

Nevertheless, we acknowledge that some uncertainty remains due to spatial aggregation, particularly for geographically heterogeneous countries. We therefore added the following limitation to the Discussion section: *"Additionally, downscaling of AR6 regional data may omit national heterogeneity, and the coarse resolution of climate data may limit accuracy for small or geographically heterogeneous countries, despite the exclusion of countries smaller than 2500 km²."* (Main text: Lines 329-332).

We agree that higher-resolution or downscaled climate datasets would be valuable for future work, particularly for small-island states and countries with complex geographic features.

8. List of all models

The authors obtain data for several variables from the AR6 database. These data are the median of the outcomes of several models. For clarity, I would strongly recommend the authors to provide a list (in the SI) of all models used.

Thank you for this suggestion. We have added a new supplementary table listing the regional AR6 IAM model runs used in this study and indicating which models contributed to each AR6-derived variable group: fossil fuel production, fossil fuel price, renewable electricity generation, baseline fossil fuel electricity generation, and carbon price. The new table has been added as Supplementary Table 2.

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Reviewer #3 (Remarks to the Author):

Major Comments

The paper addresses a timely topic, aiming to unpack potential coalitions among countries based on their asset portfolios. The manuscript is well-written, and the analysis and results are generally presented clearly. However, despite the potential of the topic, I have significant reservations regarding the study's framing and its alignment with political reality.

We thank the reviewer for the detailed comments and overall positive assessment of our manuscript. We believe that our manuscript has significantly improved through the review process.

First, the analysis appears somewhat detached from political reality. This is because it overlooks the issue of "scale," although the authors acknowledge this limitation at the end of the manuscript (L303). This is exemplified by the "closest pairs" identified in the study: the US and Thailand; China and Bosnia & Herzegovina. Treating these nations as pairs – given the large difference in economic size, geopolitical influence, and population – makes it difficult to draw actionable conclusions.

We thank the reviewer for this comment and agree that similarities in climate-related assets should not be interpreted as overall geopolitical or socioeconomic similarity. Following the methodological revisions and code implementation corrections described above, several country pairings in the similarity analysis have changed. In particular, the US is now most similar to Mexico rather than Thailand. Nevertheless, we agree with the reviewer's broader point that countries with similar climate-asset profiles may still differ substantially in economic size, geopolitical influence, population, and development level.

To make this distinction clearer, we substantially revised the similarity section in the main text. We now explicitly state that the similarity analysis "*complements the clustering analysis by revealing similarities in the underlying asset structure that may not be fully captured by the four aggregate indicators used for clustering*" (Main text: Lines 222-224). We further clarify that pairings such as China and Bosnia and Herzegovina or India and Bulgaria "*should not be interpreted as indicating broader socioeconomic similarity. Rather, they suggest that countries with different development contexts may nevertheless face comparable configurations of climate-related assets*" (Main text: Lines 252-254).

Economic size, population, geopolitical influence, and geographic proximity may provide additional context for assessing the political relevance and feasibility of potential partnerships. We therefore added Supplementary Section S6, which reports alternative climate-asset counterparts under GDP-, population-, and region-constrained comparisons. These analyses complement the main similarity analysis by identifying countries that share similar climate-asset profiles while also exhibiting comparable economic scale, population, or geographic proximity.

However, we retain the original similarity metric as our main analysis because our objective is to characterize similarities in climate-related assets themselves. The constrained analyses are therefore intended as additional context rather than a replacement for the main framework.

Second, the paper lacks the perspective that the climate problem is fundamentally a development problem. The analysis does not account for the state of development (e.g., GDP per capita), nor does it address historical responsibility – an underlying, persisting source of friction in climate negotiations. While I recognize that not every aspects can be included in a quantitative analysis, the utility of study's outcome would be limited without contextualizing the results also considering these dimensions.

We have tried to address this very important point throughout the manuscript.

First, we have emphasized in the Abstract that our framework captures only one dimension of countries' interests in climate mitigation: *“While our framework remains limited, our data provides input for integrating other ethical, governance, and socio-economic factors in future analyses of opportunities and challenges for ambitious climate change mitigation.”*

Second, we revised the Introduction to emphasize that our dataset should be interpreted together with these broader dimensions: *“Thus, our dataset establishes a foundation that future research can link with ethical, governance, and socio-economic contexts, and provides stakeholders with information to identify potential strategic allies and design nuanced strategies to enhance climate action.”* (Main text: Lines 54-57)

Finally, we have made this dimension explicit in the second paragraph of "Discussion and limitations" section (Main text: Lines 277-289). The paragraph highlights that low-income countries are the most vulnerable and often the most renewable-rich countries but lack capacity to take climate leadership and additionally have low historical responsibility for past global climate change. We have added further literature on this development dimension that the interested reader can refer to.

We acknowledge that we have to remain limited in our analysis with respect to this important issue and hope that our edits to the text are satisfactory to contextualize our analysis in this regard.

Third, the introduction frames the climate mitigation largely as an arena for self-competition and maximization. While trust may be eroding, the Paris Agreement's original spirit was for the global commons. Incorporating this contrast between national self-interest and the collective goal would allow the analysis to better illuminate conflicts that should be resolved.

We agree and have revised the framing accordingly. We now begin the Introduction by emphasizing the collective benefits of climate mitigation: *“Humanity will significantly benefit from ambitious global climate change mitigation, creating a collective interest in climate action.”* We come back to this framing also at the start of "Discussion and limitations" section by discussing how heterogeneous national interests create challenges for international cooperation on climate change mitigation and by highlighting possible pathways to strengthen ambitious climate action (Main text: Lines 259-276).

Furthermore, the statement on novelty (around L38) is insufficient. The paper should elaborate on what exactly the novel aspect(s) of this study are.

Thank you for this comment as a clear distinction to the literature was lacking in the manuscript. In response to this and a comment by another reviewer, we have added further literature and a clear distinction in the first paragraph of the main text.

We now explicitly state that: *"We extend the literature in two main ways. First, we provide a new country-level dataset that combines indicators of previous literature in a common framework, which is unique while there are only few existing studies that combine estimates of multiple indicators. Second, we take an asset-based perspective by quantifying how four broad and eight detailed underlying energy and environmental assets change under ambitious emission-reduction pathways."* (Main text: Lines 40-46).

Thus, the novelty of our work lies not in introducing a new model for a single climate-related dimension, but in developing an integrated country-level framework that combines multiple climate-related assets and examines how their heterogeneous distribution shapes opportunities and challenges for ambitious climate mitigation.

Specific Comments

L23-25: I am not sure what this finding means, given the political reality where the US, EU, and China take very distinct positions from each other.

As you suggested in your comment below, we have changed the framing of this finding in the abstract to how we phrased it at the end of the cluster section.

As another reviewer requested more clarity on this point, we expanded the Discussion to explain why the US and China belong to the same cluster in our asset-based analysis, even though they adopt different climate policy positions. In the third paragraph of "Discussion and limitations" section, we now state: *"As a case in point, China, the EU, and the US belong to the moderate-impact cluster according to our four indicators, but their current climate strategies differ in many important aspects..."* (Main text Lines 293-304). We then discuss how the US and China differ in terms of domestic political economy, historical development, and governance, illustrating that our clustering reflects similarities in climate-related asset profiles rather than overall climate policy. Due to space, the discussion remains limited but we hope that this clarifies our contribution and limitations better.

Furthermore, we clearly discuss that our analysis is partial as there are more dimension that determine climate policy than our four asset categories.

L36-37: It is important to know if and how each country's economic scale is considered in the analysis. Please be more specific about how the country's share in current global GDP entered into the analysis.

We use the share of global GDP only to characterize the power of different clusters, where power is meant as country's capability to influence global climate action, supporting this relationship by additional literature. Since this is only a minor part of our analysis, we have moved this explanation to when we discuss our clusters. Additionally, we highlight that influence on global climate action is shaped by more than a country's GDP, in particular that countries with high vulnerability to climate change are vocal players within international climate negotiations and that countries rich in fossil fuels are often blocking progress.

L38: refs 9 and 10 are a global study.

We have completely revised this part of the paper and the two references have been removed in the process.

L49: Remove the hyphen between “we” and “quantity”

Thank you for pointing this out. The hyphen and strikethrough formatting were unintended remnants from the revision process. We apologize for the confusion. The formatting has now been removed and the text has been corrected in the revised manuscript.

L85: Please define “fossil fuel rent losses” here. While it is described in the Methods, a short description here would help.

We added a short description in the paragraph of fossil fuel rent losses (Main text: Lines 94-96): *“Country-level fossil fuel rent losses are defined as reductions in discounted fossil fuel revenues under mitigation scenarios relative to the SSP2-6.0 baseline, summed over mid- and end-century periods and expressed as a share of discounted cumulative GDP”*.

L104: Growth-based damage functions are known to yield large impact estimates. Why are the impacts estimated here generally modest? A brief explanation of how the growth-based damage functions affect the results would be helpful.

We thank the reviewer for this helpful comment. We have clarified in the Methods (Main text: Lines 431-442) and Supplementary Information why the estimated avoided-damage values are relatively modest despite including growth-based damage functions. First, our indicator measures avoided damages from mitigation relative to SSP2-RCP6.0, rather than total climate damages relative to a no-warming counterfactual. Second, damages are expressed as a share of cumulative discounted reference SSP2 GDP over 2020-2050 or 2020-2100, which gives a substantially larger denominator than annual GDP in a single future year. Third, the main estimate uses a two-step function-median aggregation across heterogeneous damage functions after matching climate models across scenarios, so high-impact growth-based estimates do not dominate the final indicator. The aggregation approach is described in Supplementary Section S2.3.

To make the role of growth-based functions transparent, we added a sensitivity analysis in Supplementary Section S2.4, comparing the main function-median estimate with growth-only, level-only, pooled-median, excluding-BHM, and excluding-Kalkuhl-Wenz specifications. For the end-century stringent mitigation case, growth-based functions produce a larger median avoided-damage estimate of 2.86% of cumulative discounted reference SSP2 GDP, compared with 1.31% for the main function-median estimate and 0.81% for level-based functions only. This confirms that growth-based functions yield larger estimates, while the main estimate remains more moderate because it combines growth- and level-based specifications using the function median.

We revised the main text (Lines 431-442) and added Supplementary Section S2 to explain the metric definition, aggregation approach, and sensitivity results.

L119: Fig 1 is too small to distinguish individual countries. For example, Bhutan mentioned in the text is invisible.

We thank the reviewer for this suggestion. We increased the resolution of Fig. 1 to improve the visibility of individual countries. In addition, the revised text focuses on the broader spatial patterns shown in Fig. 1 rather than discussing very small individual countries.

L134: Although not central to the analysis, it would be interesting to indicate countries with net carbon sources in a different color. They are hard to distinguish from others in Fig. 1.

We indicated the countries with net carbon sources in red in Fig. 1, making them easier to distinguish from others.

L157: Please discuss why the k-means clustering results were retained. The authors could also mention that the clustering results are highly uncertain, as the three methods yield consistent results only for a small subset of countries (Supplementary Fig. 3).

We thank the reviewer for this suggestion. In the revised manuscript, we summarize the robustness of the clustering in the main text (Main text: Lines 163-170) and provide a more detailed sensitivity analysis, together with the rationale for retaining the k-means ($k = 5$) framework as the primary classification approach, in Supplementary Section S4.

First, we expanded the sensitivity analysis for the number of clusters by evaluating k-means solutions with $k = 4-7$ and comparing their diagnostics, spatial patterns, and cluster characteristics. The within-cluster sum of squares declines smoothly rather than exhibiting a sharp elbow, and the average silhouette score is highest for $k = 4$. However, the $k = 4$ solution is highly imbalanced, assigning 116 of 169 countries to a single cluster and accounting for only 56.7% of the total sum-of-squares variation. It also largely merges the avoided-damage economy (ADE) group into the broader moderate-impact economy (MIE) group, reducing the ability to distinguish countries characterized primarily by avoided climate damages. Increasing the number of clusters beyond $k = 5$, on the other hand, produces several very small clusters without identifying additional broadly representative mitigation-asset groups. We therefore retain the $k = 5$ solution, which explains 65.3% of the total sum-of-squares variation and provides a more balanced, interpretable, and policy-relevant representation of mitigation-asset heterogeneity.

Second, we evaluated robustness using hierarchical clustering with Ward's method. We now explicitly report that hierarchical clustering assigns 146 of 169 countries (86.4%) to the same cluster as k-means, while 23 countries are assigned differently. Among these, 19 correspond to their second-nearest k-means cluster, indicating that disagreements are concentrated among countries located near cluster boundaries rather than reflecting fundamentally different global clustering structures (Main text: Lines 163-170).

We also clarify that the two approaches reflect different clustering philosophies. Hierarchical clustering constructs nested groupings based on distances among countries and clusters, whereas k-means partitions countries around global centroids in the multidimensional mitigation-asset space. Although hierarchical clustering recovers broadly similar patterns, it produces less clearly separated group characteristics in some cases, particularly by assigning many countries classified as MIE under k-means to the ADE group. This broadens the ADE group and weakens the

distinction between countries primarily characterized by avoided damages and those with more mixed mitigation-asset profiles. Given the objective of identifying broad, economically meaningful, and policy-relevant mitigation-asset groups at the global scale, we therefore retain the k-means $k = 5$ solution as the primary classification framework and report hierarchical clustering as a robustness check.

We also reconsidered the Gaussian mixture model (GMM) comparison cited in the previous Supplementary Fig. 3. In the revised manuscript, we no longer include GMM as a robustness check because it is not well suited to the structure of the dataset: the analysis contains 169 countries, the clustering space is relatively high-dimensional, and several mitigation-asset indicators are bounded, skewed, and non-Gaussian. Since GMM assumes multivariate Gaussian component distributions, we consider its assignments less appropriate for evaluating the robustness of the mitigation-asset groups. We therefore focus the revised sensitivity analysis on hierarchical clustering, which provides an alternative distance-based clustering framework without imposing Gaussian distributional assumptions.

L191: What do “natural emission assets” mean? Are they equivalent to carbon sequestration capacity?

We have reformulated the old wording “natural mitigation assets” to “natural resources for mitigation”.

L193: But the GDP share of CSE and REE is even lower than 4.4%.

Thank you for this observation, which we now discuss in this paragraph as well. Note that our clustering results changed somewhat because of the methodological revision to the fossil fuel rent-loss allocation and the additional implementation corrections identified during the consistency checks described at the beginning of this response.

L201: The interesting interpretation is more clearly stated here than in the Abstract. I recommend revising the corresponding statement in the Abstract based on this sentence.

The corresponding statement in the Abstract has been revised accordingly.

L215-217: The identified pairs (US and Thailand; China and Bosnia & Herzegovina) are unexpected. However, as noted in my main comments, given the vast differences in area, population, GDP, and development state, it is difficult to extract realistic insights from this finding.

As discussed in our response above, we revised the manuscript to clarify that the identified counterparts refer specifically to similarities in climate-asset profiles and should not be interpreted as broader socioeconomic similarity (Main text: Lines 252–257). We agree that dimensions such as economic size, population, and development are important for assessing the political relevance of potential partnerships. However, because the aim of the similarity analysis is to identify countries with similar climate-related asset profiles, we chose not to incorporate these dimensions into the main similarity metric, as doing so would shift the focus away from the specific research question addressed here. Additional GDP-, population-, and region-constrained counterparts are provided in Supplementary Section S6.

L361-362: Please add an explanation for 0-lag and 5-lag. How are they defined to address immediate and delayed economic response?

We now explicitly explain the definitions of the BHM 0-lag and 5-lag specifications in Supplementary Table 9 of Section S2.2. We clarify that the BHM 0-lag specification captures the contemporaneous effect of annual temperature on GDP growth, whereas the BHM 5-lag specification incorporates temperature effects from the current year and the previous five years, allowing for persistent and delayed economic responses to climate change. We also clarify the cumulative lag structure used in DJO and the lagged temperature terms included in the Kalkuhl-Wenz specification.

Table 1: Typo for “They most of them” in Key Characteristics of ADE.

We thank the reviewer for catching this issue. We corrected the typo and revised the corresponding description in Table 1 for clarity.

Fig. 3: Please spell out the full country name either in the figure or figure caption.

We spelled out the full country names in the figure caption.

Point-by-Point Responses

Reviewer #1:

Remarks to the Author:

The authors have responded to my comments and improved the manuscript diligently. The comments from the other two reviewers very complementary to my observations, and these seem to be addressed appropriately, as well. This is an interesting analysis and I'd like to recommend it for publication.

We thank the reviewer for the positive assessment of the revised manuscript and recommendation for publication. We greatly appreciate their constructive comments throughout the review process.

Reviewer #2:

Remarks to the Author:

The authors have carefully revised the manuscript and provided detailed responses to the reviewers' comments. Considerable effort has been made to address both the conceptual and methodological concerns raised during the first round of review. The revised manuscript is substantially improved in terms of methodological transparency, robustness analysis, interpretation of the asset indicators, and discussion of the limitations of the proposed framework.

In particular, I found the revisions concerning the country-level disaggregation of fossil-fuel revenues, the expanded documentation and sensitivity analysis of the climate-damage estimates, the clarification of the relationship between the clustering and similarity analyses, and the treatment of spatial-resolution limitations to be convincing. The additional analyses and more cautious framing of the geopolitical implications also strengthen the manuscript considerably.

I have only a few minor points that the authors may wish to consider in the final version. First, it would be useful to consistently emphasize that the four asset dimensions are conceptually heterogeneous and that the clustering should be interpreted as a descriptive multidimensional typology rather than as an aggregation of economically equivalent gains and losses. Second, the renewable-energy indicator should be described carefully as a monetized or policy-valued technical mitigation potential, rather than as an expected realized economic benefit, since actual deployment also depends on infrastructure, investment capacity, institutions, and other country-specific constraints. Finally, GDP may be more appropriately described as a proxy for economic weight or potential leverage rather than as a comprehensive measure of geopolitical or negotiating power.

Overall, however, I consider that the authors have responded appropriately and constructively to the reviewers' concerns, and I find the revised manuscript substantially stronger.

We thank the reviewer for the positive assessment of the revised manuscript and for the remaining constructive suggestions. We have carefully considered the three points raised and have further clarified the manuscript accordingly.

1. We agree and have further clarified this point at the beginning of the section of "Country Clusters". We now explicitly state that the clusters describe multidimensional asset profiles rather than aggregate gains and losses across the four indicators (Lines 158-160).

2. We agree and have clarified this point in the section of “Country-Level Asset Gains and Losses”. We now explicitly state that the renewable energy and carbon sequestration indicators do not estimate changes in GDP or realized economic benefits (Line 117).

3. We agree and have revised the relevant text to describe GDP as a proxy for countries’ economic weight and potential leverage in international negotiations (Lines 181-182 & Lines 323-324), rather than as a proxy for negotiating power.

Reviewer #3:

None

Reviewer #2

Comments on the paper: “Quantifying Assets for the Geopolitics of Climate Change Mitigation”

The manuscript proposes an asset-based quantification of how climate mitigation reshapes countries’ incentives by combining (i) fossil-fuel revenue losses under mitigation vs baseline; (ii) avoided macroeconomic damages from reduced warming; (iii) monetized renewable-electricity mitigation potential (PV, onshore and offshore wind) based on avoided CO₂ times a carbon price; and (iv) monetized land carbon sequestration based on annual land carbon stock changes times a carbon price. All quantities are discounted and expressed as shares of discounted cumulative GDP, evaluated for mid-century (2020–2050) and end-century (2020–2100) under SSP2-2.6 and SSP2-4.5 relative to SSP2-6.0 (baseline).

The authors then use k-means clustering on four aggregate indicators to define five country types (FFE, ADE, CSE, REE, MIE), validate clusters with alternative clustering approaches, and add a distance-based similarity analysis on eight sub-dimensions (coal/oil/gas; damages; solar/wind; natural/managed sequestration) to highlight potential alignments in negotiations.

Overall, while the manuscript is timely and promising, my comments below emphasize the need to strengthen conceptual consistency and methodological robustness, especially the downscaling assumptions, documentation of the damage calculations, interpretation of the clustering results, and limitations from coarse spatial resolution, to better support the geopolitical conclusions.

Comments

1. Conceptual comparability of the four assets

The manuscript combines: (i) fossil-fuel revenue losses, which is a mostly private-sector/terms-of-trade concept; (ii) avoided macro damages, which is a social-welfare/aggregate output concept; and (iii - ivv) carbon-price-valued mitigation/sequestration, which are policy-shadow-price × quantity concepts. Treating these as comparable assets risks category errors when you later interpret clusters as reflecting stakes and bargaining incentives.

2. Use of regional IAM outputs downscaled to countries may drive some geopolitical conclusions (Fossil Fuels)

The authors downscale regional revenues to countries using historical production shares (obtained from GEM), implicitly assuming future production shares do not shift with mitigation. This is a strong stationarity assumption, particularly for the end-century horizon (to 2100). In reality, the within-region distribution of production is likely to evolve endogenously with: (i) resource depletion and reserve exhaustion (shifting comparative advantage over time); (ii) heterogeneous extraction costs and carbon-intensity of supply (mitigation pathways tend to concentrate remaining production in lower-cost segments) (McGlade and Ekins, 2015); and (iii) geopolitical disruptions (conflicts, sanctions, trade re-routing, and shifting energy-security strategies) that can change long-run supply patterns and access to markets (Aizenman et al., 2024), weakening the plausibility of extrapolating pre-2020

shares to 2100, particularly for countries whose exports are exposed to geopolitical risk. Therefore, this assumption can potentially affect who is a winner and loser (e.g., Middle East vs higher-cost producers). Although the authors briefly mention this as a caveat in their discussion, my suggestion is to: i) more explicitly acknowledge this limitation in the Methods/Discussion; and (ii) add a sensitivity analysis by reallocating regional production/revenues using alternative rules, such as reserve-weighted shares (proved reserves or remaining resource base) or cost-curve-weighted shares (greater weight to lower-cost producers) or other rule.

3. Use of regional IAM outputs downscaled to countries may drive some geopolitical conclusions (Renewables)

Similar to the above, the authors downscale regional renewable generation to countries using a deployment index equal to (regional generation / regional technical potential), then apply it uniformly within a region proportional to each country's potential. This can strongly over-allocate generation to high-potential but low-demand, weak-grid, or politically unstable countries, consistent with extremely large shares of GDP reported for some countries (e.g., Somalia ~110% renewable mitigation potential by end-century). My suggestion is to add a sensitivity analysis with alternative downscaling rules (e.g., proportional to projected electricity demand or to current installed capacity or using country-level IAM outputs where available).

4. Economic damage methods need clearer documentation

You compute population-weighted annual temperature at country level and apply both growth- and level-based damage functions, combining BHM, DJO, Kalkuhl & Wenz, Tol, and Hope. For growth-based functions, results are highly sensitive to whether you use absolute temperature vs anomalies, how you treat model bias, and how lags are implemented. The Methods section currently does not make these steps fully transparent (there is just a reference to coding materials). The authors should add the relevant equations/steps explicitly in the SI (or main text).

5. GDP share as a proxy for negotiating power

Using a country's share of current global GDP as a first-order proxy for negotiating power is defensible (it captures material resources, ability to fund large delegations, and capacity to offer side-payments). However, GDP is not the only (or always the dominant) determinant of influence in climate bargaining, and treating it as the sole proxy risks overstating the link between economic size and leverage.

Empirically, work on climate negotiations finds that bargaining success and positions reflect multiple drivers, including vulnerability to climate impacts, domestic political constraints, and institutional features, not only GDP. For example, Weiler (2012) reports that external power (operationalized with GDP) and vulnerability both help predict bargaining success, while other factors also matter. Similarly, Bailer and Weiler (2015) argue that countries' negotiation

positions are shaped by structural/economic factors, vulnerability, and domestic political characteristics (e.g., democratic status).

My suggestion is to add a short sensitivity/robustness section where negotiating power is proxied not only by GDP share, but by a small set of complementary indicators (or a composite index/PCA), such as GHG emissions, fossil-fuel export dependence, climate vulnerability/exposure, etc.

6. Cross-cluster nearest neighbours indicate typology limitations

The observation that some countries' closest counterpart lies outside their assigned cluster (e.g., Mexico in ADE and Thailand in MIE being each other's most similar) is not necessarily an error, but it highlights a limitation of the typology: clustering on four aggregate indicators can mask heterogeneity within clusters and create boundary cases where countries are closer to members of another cluster once the underlying sub-components are considered. In other words, the partition is driven by the compressed 4D representation, while meaningful similarity is assessed in a richer 8D feature space.

This is an important issue since the paper uses the clusters for interpretive claims about country types and negotiation-relevant alignments. If a non-trivial number of countries are nearest to countries in other clusters, readers may (rightly) worry that cluster membership is being over-interpreted as a definitive descriptor rather than a coarse categorization.

7. Spatial resolution of climate inputs

The use of $0.5^\circ \times 0.5^\circ$ gridded climate data raises a non-trivial concern for small countries, and especially small-island states, where strong coastal gradients, steep topography, and land-sea mask issues can make grid-cell averages poorly representative of conditions experienced by local populations. This is not only a precision issue: if country-level temperatures are used to drive damage functions (and potentially land-carbon outcomes), coarse resolution can introduce systematic biases (e.g., smoothing extremes or misplacing land points). In addition, with 0.5° grids for PV/wind and land carbon, some small states may be poorly represented. The IPCC explicitly notes that downscaling is pivotal for small islands because their diversity makes generalization invalid and data limitations constrain robust impact modelling. The authors should add a brief discussion on that issue.

8. List of all models

The authors obtain data for several variables from the AR6 database. These data are the median of the outcomes of several models. For clarity, I would strongly recommend the authors to provide a list (in the SI) of all models used.

References

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