Summary

This paper presents AD-MERGE 2.0, a substantially updated integrated assessment model extending the AD-MERGE 1.0 with seven enhancements: updated base year (2015), expanded regional disaggregation (9 to 15 regions), explicit VRE dynamics, hydrogen pathways, DACCS, recalibrated damage and adaptation functions, and alignment with SSP2 version 3.0. The model is applied across five scenarios ranging from a no-policy Baseline to an Announced Pledges scenario with optimal adaptation (APS-OA). The paper is well structured and represents a genuine technical contribution — the simultaneous treatment of adaptation and mitigation within a welfare-optimizing framework with an endogenous climate module is relatively rare in the IAM literature. I am not an energy system modeller and will not comment on the VRE, hydrogen, or DACCS components. My comments focus on the damage and adaptation framework, which the paper presents as a central contribution.

Major Comments

1. The adaptation and damage framework: a prior justification is required

Adaptation is presented as a key contribution of this paper, and the authors draw substantive policy conclusions from adaptation results, including claims about 'region-specific policy design' (abstract, l. 11) and 'optimal policy approaches tailored to the unique needs and capabilities of each region' (l. 566-567). However, the paper does not address a prior question: why does modelling adaptation as an aggregate welfare-optimizing choice variable over regions like 'Africa' or 'Other Asia' produce scientifically credible or policy-relevant outputs?

A central and well-established insight in adaptation science is that adaptation is inherently local, context-specific, and institutionally embedded (Smit & Wandel, 2006; Adger et al., 2007; IPCC AR6 WG2). There is no universal optimal adaptation level derivable from aggregate economic parameters. The UNEP Adaptation Gap Report (2025) documents a 'widening gap' between adaptation needs and realized action in many developing countries, which are precisely the regions where the model predicts the largest benefits (Africa, Other Asia). The calibration procedure (Appendix D) implicitly assumes that adaptation costs and effectiveness can be meaningfully expressed as a fraction of GDP at the regional scale, and that 'optimal adaptation' is achievable given sufficient investment. This follows the approach of earlier IAM-based adaptation models in the literature (de Bruin et al., 2009; Agrawala et al., 2011), and as such represents a reasonable starting point. However, to produce genuinely meaningful policy implications, the modeling community needs to start moving beyond this type of representation toward frameworks that are connected with empirical findings on how adaptation actually happens — taking into account characteristics such as adaptive capacity, institutional context, path dependency, and local governance conditions. I am aware this is a difficult challenge, but there are promising avenues in this direction, including adaptation pathways approaches new collection of papers coming in (https://www.nature.com/collections/gfebjgbjid) and efforts to couple IAMs with more bottom-up behaviourally rich simulation models (Filatova et al., 2025)

To strengthen the contribution of this paper, the authors should either: (a) demonstrate, with explicit reference to the empirical literature, that the structural assumptions are reasonable approximations and constrain policy conclusions accordingly; or (b) reframe contribution as modelling exercise looking at theoretical upper bounds under idealized conditions, not as achievable policy targets. The macro-level outputs the model can legitimately support — aggregate damage cost comparisons across scenarios and the identification of highly vulnerable regions — are genuinely valuable. But the claims to region-specific policy design cannot be sustained. This concern is actually acknowledged at l. 563-564 ('these results are obtained on the assumption of optimal adaptation, an idealized scenario in practice'), but a single end-of-paper caveat is insufficient given the policy framing used throughout Sections 4.3 and 5. This caveat needs to be made more explicit in the framing of the paper and its contribution.

A related issue: Figure D1b — the adaptation cost curve — is explicitly described in its own caption as 'conceptual' with 'shapes of the curves theoretical and not reflecting specific empirical estimates.' If the figure used to illustrate the adaptation calibration logic has no empirical grounding, the paper's claim to have empirically recalibrated adaptation is weakened. The paper needs to be transparent about what is empirically derived versus theoretically assumed throughout the adaptation framework. Where empirical adaptation investment data exist — for instance for European countries (Cortes-Arbuez et al., 2025) — these should be used to benchmark whether modeled adaptation expenditure levels are plausible.

2. Lack of model documentation

A broader concern across the paper is that key model components are insufficiently documented for a model description paper. The main text frequently defers to earlier publications (Bahn & Kypreos, 2003; Bahn et al., 2019) for core structural elements, and the appendices, while useful, do not fully compensate. The paper is clearly written with appropriate conciseness, and I am not asking for an exhaustive technical manual in the main text. However, at minimum, the appendices — or a linked online documentation resource — should provide enough information for a reader to understand the main modeling without having to track down earlier papers.

The adaptation and damage framework is a case in point. Equation D2 governs the entire adaptation structure via parameters σ, β1,r, β2,r, and β3,r (l. 679–680), yet none of these values are reported anywhere. The 'maximum adaptation level' mentioned at l. 301 is stated but neither quantified nor explained. A reader cannot assess the plausibility of the adaptation results, connect them to current literature, or understand their sensitivity without access to these parameters. The same issue applies more broadly: the welfare function parameterization and the Negishi weights are not reported, and the climate module equations are not provided. The authors should either expand the appendices or provide a clearly signposted link to online technical documentation that fills these gaps.

Minor Comments.

1. Table 1: The selection of IAMs for comparison is not justified. Why these specific models? Some (e.g. GCAM, REMIND) have been substantially updated and the version used for comparison is not stated. Please add a brief justification of the selection criteria.
2. Appendix D / Table D1: The paper estimates its own regional damage functions rather than using existing sector-specific, country-level approaches such as COACCH (Van Der Wijst et al,. 2023) . Given that Table D1 draws on many of the same underlying data sources that such frameworks use, why was an existing damage assessment not used or more systematically benchmarked against? This choice should be explicitly justified.

3. Scenarios / Figure 10: APS-NA does not appear in the energy figures (Figs. 7-9), which is justified at l. 367-369 because APS-NA and APS-OA share identical energy trajectories. However, showing APS-NA explicitly alongside APS-OA in Figure 10 and the adaptation decomposition figures (10b-10c) would allow clearer isolation of the marginal effect of optimal adaptation under the more ambitious mitigation pathway. It is not evident why this comparison is currently absent.
4. Section 3.1: The model is calibrated to 2015 and validated against observed 2015 and 2020 Baseline emissions (l. 326-330). It would strengthen the paper to also assess whether other outputs, such as GDP, align with historical data.

References Cited in This Review

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