Title: A new sediment budget for the Congo River Basin reveals underestimated tributary contributions and large-scale deposition

overall evaluation

The paper presents a compelling and timely exploration of a crucial Earth-surface system. Its primary strengths lie in its basin-scale scope, the integration of remote sensing with a "sediment-budget" framework, and the provision of new quantitative insights into tributary contributions and net deposition in the Cuvette Centrale. It is relevant to the Journal scope and has the potential to make a significant contribution.

Overall, my assessment is positive, but I believe the manuscript requires more refinement before it is ready for publication. The central conclusions are plausible and generally well-supported, but there are methodological and interpretive issues that need to be elaborated on and presented more clearly. Specifically, the authors should show how uncertainty is propagated, the effects of attrition in calibration data, the methods used for spatial and temporal validation and support the inferred deposition with some evidence. I am also apprehensive about the use of the term "sediment budget," given that the study quantifies washload-dominated suspended fluxes.

Review against the detailed ESurf criteria

S/N	CRITERIA
1	**Does the paper address relevant scientific questions within the scope of ESurf?**
	Yes, the paper falls within the journal's scope. It examines the sediment budget of the world's second largest river basin and directly contributes to ongoing research in geomorphology, sediment connectivity, and source-to-sink dynamics.
2	**Does the paper present novel concepts, ideas, tools, or data?**
	Yes, it does: • It develops a region-specific remote-sensing/machine-learning workflow for TSS in the Congo Basin, • outputs an updated "sediment budget" for the entire Congo Basin, including secondary tributaries, and presents the first "sediment budget" for the Kasai Basin. However, the methodological novelty is more applied than conceptual, and the authors should avoid overstating the approach's broad transferability without clearer validation across diverse hydro-sedimentary conditions.
3	**Are substantial conclusions reached?**
	1. The Congo exports approximately 33 Tg/year. This is likely the minimum exported given the study's focus on washload-dominated suspended fluxes. 2. The Kasaï River contributes more to these exports than previously estimated. 3. Secondary tributaries play a notable role and should not be overlooked.

	4. The Cuvette Centrale is likely a major zone for net deposition. Again, this is likely true, but it's important to approach it with caution, as the paper addresses net depositional losses rather than gross deposition, and the supporting evidence is based on inference rather than direct observation.
	Are the scientific methods and assumptions valid and clearly outlined?
	Partly. The general framework is valid, but some assumptions need more justification • The cloud cover problem in the Congo is well known. However, the 11-day matching window between field observations and satellite images may be too long, even for larger, less dynamic rivers, leading to temporal mismatch. Additionally, because sediment transport is event-dominated, with a small number of high-flow events carrying the bulk of the annual sediment load, peak sediment loads are missed. • The treatment of spectral saturation is somewhat ad hoc. The authors remove values above 100 mg/l a priori, yet later suggest that saturation may begin around 70 mg/l in the Congo and possibly around 45 mg/l in some tributaries. This materially affects interpretation in high-TSS rivers and needs a more formal analysis. • The independence of the validation in space and time is unclear. If training and test data overlap across stations or dates, the performance results may be overly optimistic. Conducting a grouped validation by station, tributary, or campaign would enhance confidence in the results. • The process of converting predicted TSS and discharge into sediment yield is simple, but it's important to explicitly describe the uncertainty propagation throughout the entire workflow.
5	**Are the results sufficient to support the interpretations and conclusions?**
	Mostly, but only after more careful qualification The sediment yield estimates are derived from satellite-based TSS predictions combined with discharge and temporally integrated; however, the methodology is inherently limited in its ability to capture event-scale sediment dynamics. Given the reliance on Landsat imagery, persistent cloud cover, and the use of a ±11-day matching window, high-flow, high-sediment events, likely responsible for a substantial proportion of annual sediment transport, are systematically undersampled or excluded. As a result, the derived sediment yields may be biased toward lower values, reflecting temporally averaged conditions rather than true flux magnitudes. This limitation should be more explicitly acknowledged, and its implications for both total yield and inferred deposition should be discussed Further, the precision of the quantified depositional estimates and the confidence with which along-river decreases can be attributed specifically to deposition rather than to compounded model error, changes in bias along the mainstem, floodplain exchange, or unresolved tributary/bank inputs is unclear. The paper acknowledges some of this, but the discussion should be strengthened.
6	**Is the description of experiments and calculations sufficiently complete and precise to allow reproduction?**

	Not yet fully, conceptually, yes, but they could include: • fuller details on train/test splitting and cross-validation, • hyperparameter settings for the random forest model, • the exact variable set retained after RFE in a machine-readable table, • a more explicit uncertainty propagation workflow, • criteria for defining virtual stations and extracting cross-sectional reflectance, • clearer documentation of discharge inputs and how missing data were handled.
7	**Do the authors give proper credit to related work and clearly indicate their own contribution?**
	Yes
8	**Does the title clearly reflect the contents of the paper?**
	The title broadly reflects the scope and findings of the manuscript; however, the use of the term 'sediment budget' may overstate the completeness of the analysis. Given the methodological limitations, more qualified wording would improve alignment between the title and the underlying analysis, e.g., "Satellite-derived suspended sediment fluxes in the Congo River Basin indicate underestimated tributary contributions and net downstream sediment losses."
9	**Does the abstract provide a concise and complete summary?**
	Mostly yes. A minor concern is that it reads slightly more certain than the body of the paper justifies, especially regarding model accuracy and depositional interpretation
10	**Is the overall presentation well-structured and clear?**
	Yes.
11	**Is the language fluent and precise?**
	Yes
12	**Are mathematical formulae, symbols, abbreviations, and units correctly defined and used?**
	Mostly yes.
13	**Should any parts of the paper be clarified, reduced, combined, or eliminated?**
	• a more comprehensive presentation of the model calibration workflow, • a more explicit limitations subsection focused on validation, saturation, and uncertainty, • The conclusion should frame results as indicative rather than conclusive. In particular, statements regarding tributary contributions and large-scale deposition appear more definitive than the underlying data support.
14	**Are the number and quality of references appropriate?**
	Yes. The references appear appropriate and sufficient.
15	**Are the amount and quality of supplementary material appropriate?**
	They are ok.