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Peer Review

# Review of: "Is There a Direct Relation Between EEG Band Spectrum and DMN Activity in fMRI? A Multivariate Exploratory Study"

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## *1. Overall Summary*

The manuscript titled *"Is There a Direct Relation Between EEG Band Spectrum and DMN Activity in fMRI? A Multivariate Exploratory Study"* investigates the relationship between EEG spectral features and fMRI BOLD signals in the Default Mode Network (DMN) across various sleep stages. Using an open-access EEG-fMRI dataset, the authors apply Random Forest regression to predict DMN activity from EEG band powers across 28 electrodes and seven frequency bands. The study reports that midline EEG features are most predictive of DMN activity, with stage-specific variations—particularly a divergence in N3 sleep.

The topic is timely and technically relevant, especially for researchers interested in multimodal neuroimaging and sleep neuroscience. However, the manuscript currently lacks the conceptual clarity, methodological transparency, and neuroscientific framing required for a rigorous scientific publication.

## *2. Recommendation Improvements*

**Please redo and detail further.** The manuscript requires substantial revision before it can be considered for publication. Below are the key areas that need attention:

### *Conceptual Clarity*

- There is a **conceptual mismatch** between the stated motivation and the actual implementation. The authors claim EEG is used because it captures "network-level activity," yet the modeling is based solely



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on **electrode-level features**. This contradiction should be addressed explicitly, and the rationale for using channel-wise spectral power as a proxy for network-level dynamics must be justified or revised.

### *Methodological Transparency*

- The **Methods section lacks critical detail** necessary for reproducibility:
  - No information is provided on the number of subjects, sessions, or basic demographics.
  - EEG preprocessing is vaguely described as "automated fMRI artifact removal." Specifics such as gradient artifact correction (e.g., FASTR), BCG removal (e.g., OBS), line noise filtering, bad channel detection, and re-referencing are missing.
  - CONN preprocessing steps (e.g., slice timing correction, normalization, smoothing kernel size, denoising strategy) are not described.
  - Sleep scoring methods are not explained—who scored the data, using what criteria
  - The Random Forest model lacks essential details in terms of model tuning and evaluation.
  - The leave-one-out cross-validation (LOOCV) strategy is ambiguous. Was it across subjects or time points?

### *Statistical Rigor*

- The manuscript does not address **multiple comparisons** in interpreting feature importance. With 196 features, the risk of false positives is high. Consider applying FDR correction or permutation testing.
- Feature importance is presented descriptively, but no statistical validation (e.g., bootstrapping, SHAP values) is provided to support claims.

### *Neuroscientific Relevance*

- The **Discussion lacks depth** in terms of neuroscientific interpretation. The manuscript does not clearly articulate why this investigation matters beyond its technical novelty.
  - What do the findings imply about DMN function across sleep stages?
  - How do the results align with or challenge existing theories of consciousness, sleep, or EEG-fMRI coupling?
  - Why is it important that midline alpha and gamma power predict DMN activity in wakefulness?
  - Why is electrode-level feature importance selected, and how can this be applicable to the field? Do the authors imply that DMN activity can be derived from less complex electrode placement?

- Without this framing, the study risks being perceived as a technical exercise rather than a contribution to neuroscience.

### *3. Overall Assessment and Evaluation*

This manuscript presents a technically interesting approach to modeling DMN activity from EEG features. However, the current version falls short of the standards expected for scholarly publication. The conceptual framing is inconsistent, the methods are under-specified, and the discussion lacks neuroscientific grounding.

I encourage the authors to:

- Reconcile the conceptual claims with the modeling approach.
- Provide full methodological transparency.
- Deepen the discussion to highlight the neuroscientific significance of the findings.

Once these issues are addressed, I would be happy to review a revised version.

## Declarations

**Potential competing interests:** No potential competing interests to declare.