**Review of Proposal 2**

**Summary:** With combination of MODIS and CERES data, can a neural network trained on MODIS data be used to predict sky fluxes at a CERES resolution.

**Scientific Merit:**

*Major Strengths:* Novel idea that seemingly hasn't been done in the literature, at least the literature presented.

*Minor Strengths:* Literature review was in depth, helping show the gap in knowledge in regards to MODIS and CERES.

*Major Weaknesses:* Overall, I thought this proposal was too technical for a proposal, and seems better suited for a paper. Even as someone with an atmospheric science background, I found myself getting lost in this proposal and am still unsure what the goal of the project ultimately is and what the science is.

*Minor Weaknesses:* N/A

**Methodology:**

*Major Strengths:* Even if the methodology is too technical for a proposal (see weaknesses), it is very clear what the model is going to be doing for this project.

*Minor Strengths:* N/A

*Major Weaknesses:* Using MODIS and CERES data, however, no other information on the data. What is the timeframe being used? What is the region being studied?

*Minor Weaknesses:* Methodology is far too technical for a proposal. Don't need information on how the model is going to work, want more surface information for a proposal.

**Feasibility:**

*Major Strengths:* Clear agenda on what is wanted to be accomplished from this project. Detailed list of objectives, clear what is wanted out of the project at each point.

*Minor Strengths:* N/A

*Major Weaknesses:* No true timeline. Lists the agenda and tasks that need to be completed, does not mention even soft deadlines of when to try and finish these objectives by.

*Minor Weaknesses:* Tasks include validation and verification, what happens if verification fails?

# Review of Proposal 2

*Summary:*

This project aims to use machine learning to emulate an Angular Distribution Model to convert from satellite directional radiance measurements to estimate the full sky flux. The machine learning model will be used to reduce the dimensionality of the model and force flux predictions to utilize only the most important factors of the satellite viewing geometry. Additionally, the CERES satellite data will be downscaled to the MODIS resolution to obtain higher resolution products.

*Scientific Merit*

Coming up with effective ways to avoid the extreme computational expense of atmospheric models is incredibly important, especially for radiative transfer models. This allows parameters that were initially difficult to calculate to be much more readily available, which can be incorporated into new and meaningful near-real-time products. There is plenty of work currently ongoing on emulating radiative transfer models, but up to this point, there has been relatively little success, especially when attempting to couple the emulated model to operational weather forecast models. Work in this area is needed to advance the application of science to operations. Additionally, the literature used to contextualize the project was extremely relevant and provided fundamental scientific background knowledge.

*Methodology*

The methodology presented here was extremely detailed. As a reviewer with a relative lack of machine learning knowledge, a lot of the detailed methodology discussion goes over my head, but there was a clear connection back to the physical relevance and main objectives of the research.

The proposer did not identify any risks regarding the proposed work. One slight risk that I found was when the proposer stated that part of the methodology was contingent on "if this approach is successful." The proposer does not provide an alternative method or objective if the approach is not successful.

*Feasibility*

The machine learning models described here seem extremely complex, so I am unsure of how easy they would be to construct, tune, and test. The timeline proposed seems reasonable if the proposer is extremely well versed in machine learning, but if they have limited experience, I have doubts about the ability for this project to be completed before the end of the semester.

## Summary

The author of this proposal describes the current state of estimating Earth's outgoing radiation flux using satellite data and the limitations that are occuring in the field. The satellites EOS Terra and Aqua are hugely important for understanding the processes related to Earth's surface flux, but are limited by the capabilities of the bolometer CERES. The CERES has coarse spatial resolution and the conversion from directional radiance to full sky flux depends on ADMs which then depend on MODIS. It is therefore this author's proposition to use machine learning to predict fluxes from MODIS data alone in order to improve spatial resolution and accuracy. Their hypothesis states that a neural network trained on MODIS pixels can predict CERES flux values over multiple different land surface types and geometries An encoder will be used to produce latent vector embeddings for each MODIS pixel, which are then aggregated and passed through a decoder to emulate ADMS and estimate CERES-observed fluxes. If successful, the model will be extended to predict flux contributions for individual MODIS pixels to further refine the spatial resolution.

## Scientific Merit

Overall, I think this proposal is really solid. It takes a recognized shortcoming from current instrumentation and data analysis and comes up with a feasible solution. While much of the information from this proposal is new to me, their claims are backed up with plenty of references and documentation. I really appreciated how detailed the background section was as it did a great job familiarizing myself with the topic before presenting the hypothesis.

## Methodology

The methodology portion of the proposal is well thought out and appears to address the objectives. I particularly liked that you included a paragraph discussing what could happen next if your initial approach is successful.

## Feasibility

Overall, this project seems like a large undertaking and the only concern I might have is that it might be too much to accomplish for the short timeline we have for this class project, especially when taking into consideration projects and work for other courses/research/thesis. However, the author appears to have a lot of experience in this topic and is very well versed in what they plan to do, so this concern is most likely unwarranted.