Review of:
"**Direct Measurement of Forest Carbon Sequestration: A Commercial System-of-Systems to Incentivize Forest Restoration and Management**",
by Marino, Truong, Munger and Gyimah.

Reviewed by: David Fitzjarrald, Atmospheric Sciences Research Center, SUNY Albany
(N.B. I do not submit anonymous reviews.)

General comments.
My experience with direct measurements of carbon flux is that of a micrometeorologist. I've been around this work for some decades now. However, I have no background in the economics of financing carbon sequestration. My main reaction in reading this material is that the first author has the inverse experience, and those co-authors who have the technical background have not fully argued the case for understanding a number of limitation to the method.

Most of the issues that come to mind have to do with repeated claims that the eddy flux approach delivers values at half-hourly intervals. *That* is the way the tables that the authors downloaded do present the data, but to assure that these numbers represent *net* ecosystem exchange, corrections must be made before the final data set is released. This is what rankles about the statement that the method is (line 247) "high precision, high frequency…" It will be more convincing when the authors outline how much better this approach is than competing ones, whatever they are.

These include: Correction for the chronic underreporting of nocturnal fluxes, which bias the results to excessive overall uptake, an error memorialized for all time by a paper by Grace et al. in 1990's extolling exaggerated C uptake in the Amazon forest. The correction in force for some years now has been simply to assert that the respiratory emission on windy, well-mixed nights holds for the other nights. Nearly twenty years ago we quantified that 10-20% of the CO_2 escapes horizontally at Harvard Forest (Staebler and Fitzjarrald, 2004). There is another bias in filtering to fit into the half-hourly data window which can, on certain kind of days, *underestimate* daytime fluxes (Sakai et al., 2001). I am not certain how the error limits on the Harvard Forest data are estimated, but I would hope that these effects are included. There is the 'science' of gap-filling—making up data when the instruments fail—introduces another problem. Finally, the authors *imply* that the eddy flux method applies to entire enormous forests, but they are certain to know that the 'footprint' for flux measurements is only a small area—at Harvard Forest it has been reported to be 0.23 km^2 (Kim, 2015). (The species composition at Harvard Forest For depends on wind directions, for example.) *Where* is the discussion of the degree of homogeneity of the forest area being considered? Much of the discussion of the Ghanaian forest has to do with the species richness and paucity of commercial timber. These are truly important considerations but largely outside the purview of carbon uptake measurements. Perhaps the authors do not take these issues to be 'show-stoppers' given their urgency to commercialize the product. However, since critics of their commercial proposal could easily be as adept as am I, the authors should tighten their case. As it stands, much of their text resembles what one might hear from a manufacturer of eddy flux equipment, Dr. Burba, for example. Optimism is only sometimes a virtue.

Another difficulty with this paper is the comparison of four years of data from the tower in Ghana with the longest eddy flux record anywhere, at Harvard Forest. I believe that this paper would be greatly served by a careful stating of the uncertainties in the eddy flux method, a plan for assessing the similarity of the forest being considered with the 'footprint' of the eddy flux measurement being made. Most convincingly this would be done with some remotely sensed data.

Please help me to see how the remuneration plan works on years, possibly long stretches of time during which a measured forest *loses* carbon over time. It appears that C uptake at Harvard Forest can be quite variable. Last year, coauthor Munger et al. (2018) noted:

> At Harvard Forest: "Annual NEE ranges from -0.4 to -6.1 Mg-C $ha^{-1}y^{-1}$ (always carbon uptake), with a mean of -2.9 (±1.5) Mg-C ha^{-1} y^{-1} . Here we focus on the highest and lowest carbon uptake years, 2008 and 2010, respectively in order to examine how forest carbon balance can shift so dramatically over a 2 year interval."

If the system were implemented, would the landowner have *to pay someone back* if the forest is a net carbon emitter? What happens in the case of a wildfire? (Actually, one could follow the other should there be a prolonged drought.) Please try to address these issues.

Specific comments.

1. Line 141 "Genetic Heat Index" not common knowledge. A reference is needed.
2. Li
ne 181. The absurd number of citations to patents fills up the bibliography. Kindly explain to the naïve reader just what is being patented here. Put this information into Appendix 1.
3.

References.

Kim J., 2015. Carbon and water cycles in mixed-forest catchments: ecohydrological modeling of the influence of climate variability and invasive insect infestation, PhD diss. Boston University.

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Sakai, R.K., Fitzjarrald, D.R. and Moore, K.E., 2001. Importance of low-frequency contributions to eddy fluxes observed over rough surfaces. *Journal of applied meteorology*, *40*(12), pp.2178-2192.

Staebler, R.M. and Fitzjarrald, D.R., 2004. Observing subcanopy CO_2 advection. *Agricultural and Forest Meteorology*, *122*(3-4), pp.139-156.