Referee comment on "Rates and timing of chlorophyll-a increases and related environmental variables in global temperate and cold-temperate lakes" by Hannah Adams et al., Earth Syst. Sci. Data Discuss., https://doi.org/10.5194/essd-2021-329-RC2, 2022

This manuscript describes a valuable data set on chlorophyll-a dynamics and associated environmental variables for lakes covering a wide range of limnological conditions and localities (mostly north temperate). Specifically, the authors identified periods of increasing chlorophyll-a concentrations, and they calculated the increment rates for such periods in each lake. This approach and the compilation of calculated rates has resulted in a derived dataset that will be of interest to many researchers.

The description of this dataset would likely be appropriate to ESSD, but I have two concerns:

1. Although ‘growth rates’ appears in the title and manuscript, the estimates produced are not growth rates per se, but are net increment rates. In the phytoplankton literature and the ecological literature in general, the term growth rate refers to the parameter r in the exponential growth of populations: dN/dt = rN. For phytoplankton, N = cell number or a proxy (such as chlorophyll), and r is the specific growth rate with units of inverse time (d⁻¹). In this study, I don’t think that the authors fitted curves to the data (except for smoothing), but instead took chlorophyll values at the start and end of each growth window, subtracted the values, and divided by the time interval (this my interpretation; the exact calculation method needs to be more explicitly stated in the manuscript). The resultant parameter is therefore in linear increment units (µg Chl a L⁻¹ d⁻¹) not growth rate units (d⁻¹), and it is misleading to call this a phytoplankton growth rate. In fact, sections of illustrative Fig. 3 do look more like exponential growth rather than linear increments, notably March-April and July-August.

Additionally, this calculated rate measures not only phytoplankton growth but also losses, and it is therefore a net rate of increase. This may be why the changes may be near-linear (e.g., August-September) rather than exponential (also, these are averages, with phytoplankton having different net growth rates down though the mixed layer, different
parts of the lake, etc.).

Without making this distinction between net rates of phytoplankton change (as estimated) versus phytoplankton growth rates (not estimated), it is easy to be led astray in interpreting the data. For example, the authors find that the net increment rate is lower in higher latitude waters:

Line 331: "Chlorophyll-a growth rates increase with nutrient availability while they decrease at higher latitudes due to cooler temperatures and lower SSR."

But higher latitude waters are largely ultra-oligotrophic. This means that phytoplankton increment rates in absolute terms (µg Chl-a L⁻¹ d⁻¹) can never be large; there is not enough standing stock (nor available nutrients) to allow a large absolute increment, as opposed to a southern eutrophic lake where even a 5% increase would be huge in absolute terms (this also biases the analyses towards eutrophic waters, with the cutoff expressed in absolute rather than relative terms; line 180).

On the other hand, the specific growth rate of high latitude, cold-adapted phytoplankton could be rapid (as in algal blooms in the polar oceans) with the growth supported by nutrient recycling processes, and population size kept in check by grazing and other loss processes, as well as capped by TP and other nutrients.

I do think that the estimates and window approach are very interesting, as are the trends, but the terminology needs to be re-thought. The flip side of the question is also interesting, the net rates of chlorophyll decrease. This same approach (but for periods of decreasing chlorophyll) could be used to identify periods of sedimentation (storage fluxes) and/or high grazing intensity. The paper could be retitled "Net rates of chlorophyll-a change and..." with the abstract explaining that these are net rates of linear increase or decrease. Or the application to net loss rates could be just mentioned in the Discussion, without the need to update the database.

2. Several sections of this manuscript read more like a scientific research article than a data description paper. For ESSD, it seems like it would be better to focus on the rationale (the current Introduction, which reads very well), the methods, and the resultant dataset, and leave the questions, hypotheses, trend analyses and interpretations to a paper for publication in a limnological journal that then refers to this article for the dataset methodology and to Adams et al. (2021) for the complete compiled data (which I verified to be available for download and well organized; I see the data sources are given in the readme file but it would be useful to have the citations for the original limnological data as the final column in the lake_summary data file).

This question of how much interpretation to include would be best to discuss with the ESSD...
editors. In checking the website, I see that some recent articles go beyond a description of the data to include trend and spatial analyses, for example:

https://essd.copernicus.org/articles/14/517/2022/

https://essd.copernicus.org/articles/14/463/2022/

while others are more exclusively focused on describing a dataset, for example:

https://essd.copernicus.org/articles/14/449/2022/