General assessment:

The authors of this paper attempt to synthesize the satellite imagery record, local weather station data, firn model output, climate reanalysis model output, and CMIP future climate model output in order to better understand the future expansion of the George VI’s surface meltwater drainage system. This is motivated by the recent findings of unprecedented melt in the 2019-2020 melt season. The motivations behind this study, to use the available data to give an assessment of present-day conditions of the surface drainage system to better predict its future evolution, is well-thought out, valuable, and promising for future study. Other studies have done similar multi-year assessments of Antarctic ice shelf surface hydrology systems (Langley et al., 2016, Stokes et al., 2020, Spergel et al., 2021). However, the authors present many different means of showing a disconnect between climate and surface conditions with lake coverage, but do not discuss that lake coverage, i.e where melt is observed ponding in satellite imagery, is mainly controlled by surface topography. The pre-existing surface depressions (discussed in Reynolds, 1981) must be filled to overspilling before water can drain over the surface into new areas/depressions. I am not familiar with the surface topography of GVIIS, but other ice shelves have more-or-less U-shaped depression cross-sections, so the addition of more meltwater does not change the surface area of the water body. It is only when all available space is filled with water that the surface area of water coverage expands via over-surface drainage. What the authors seem to describe with their analysis of similar meltwater lake coverage between 1989 and 2020 is not a dampened response to climatic forcing, but the fact that meltwater pond coverage increases as water flows and partially fills depressions nearby to meltwater production, but meltwater coverage plateaus as the partially-filled depressions fill, and only once overspilling occurs does lake coverage increase again significantly.

I would recommend refocusing this paper on the changes observed in melt pond distribution on GVIIS between the 1980s, as described in Reynolds, 1981, and where melt is observed today. There is a lot of value to giving a base-line and a detailed description of
the inter-annual variability in the ice shelf’s hydrology. I would recommend a thorough search of the literature to give a broader context to the authors’ findings on GVIIS.

Originality:

The purpose of the paper, to assess the decadal trends in a persistent surface drainage network has a lot of merit.

Scientific Quality:

The oversight of topography controlling where melt forms, and what that means for measuring meltwater lake coverage with satellite imagery, makes a lot of the analysis done in the paper unsuccessful in proving any climate-surface hydrology mechanisms. I also have many questions about methods that are not addressed in the paper. The results are reliant on the threshold of NDWI, the comparison of imagery coverage across the years, and uncertainty in manual mapping. These three issues and the uncertainty they contribute should be discussed.

Significance:

As the paper is now, the points that are presented successfully (persistent, widespread melt on GVIIS; inter-annual variability in melt production leads to variability in meltwater lake coverage; meltwater being divided between ponds and firn pore spaces) are not novel enough to be significant. In its current form, the paper is unsuccessful in supporting a new mechanism for climate-surface hydrology interaction, the “dampening effect” of increased firn air content on lake coverage.
Presentation Quality:

Much of the paper is well written, but there are a few issues in the paper’s presentation: 1) there remains a number of passages that could use scientific, quantitative terminology instead of conversational. 2) quantities such as averages, sums, etc, should be precise in what they are describing to avoid ambiguity. 3) The figures should be revised to be more readable, especially the time-series plots. 4) Much of the material presented in the supplementary materials is critical to assessing the paper, and should be brought into the main text.

Main Comments:

- The paper needs to be reassessed after considering how surface topography affects where water pools. Several of the proposed mechanisms and causal relationships between climate, firn, and meltwater lake coverage need to be reconsidered, and revised if still true or removed if no longer true.
- The methods by which lake pixels are selected need to be further explained. Moussavi et al. (2020) would be a good reference if the new method is to be kept, but I would recommend using Moussavi et al.’s available code for Landsat 8 imagery and discuss the process used to select the NDWI thresholds for Landsat 1-7. I also don’t understand what the scaling of lake pixels derived from non-Landsat 7 imagery includes, but the uncertainty introduced by this needs to be discussed.
- Many of the assertions about climate effects on meltwater lake coverage presented in the discussion/conclusion need to be supported by data or citation of the literature. The choice of MAR is discussed in the supplementary materials, but the authors also seem to use MAR output as a single point rather than a spatially-varying raster dataset.
- Some sections need to be rewritten to clear up ambiguity in what was done, what is being extrapolated, etc.

Line-by-line comments are included in the attached pdf

Please also note the supplement to this comment: https://tc.copernicus.org/preprints/tc-2021-214/tc-2021-214-RC1-supplement.pdf