Authors claim to "… provide the most granular, contemporary, comprehensive, high-quality, and robust data currently available to assess temporal and spatial trends of global human modification." This reviewer finds no reason to doubt their motivation, few reasons to doubt skill and tools applied, but only small evidence that authors achieved their goal. Without substantial changes and improvements one suspects they will miss that goal. They include a useful and thoughtful 'Caveats' section (Section 4.2) but without sufficient information to allow this reviewer to determine whether caveats outweigh results.

Data access: Dryad requires too much time to deliver 4 GB. Could the authors provide some kind of teaser data product, demonstrating their tools and techniques, that does not require reviewer / user to commit to 4 GB? Something labelled and protected under a separate DOI, accessible via a trusted competent repository? Perhaps the 2017 data? Or a monthly portion of it? As I work through this review, Dryad has not delivered notice of availability for this data product for more than nine hours. By any standards, this represents failure to meet my, much less ESSD's, expectations for data accessibility.

I worry very much about source data. Authors have provided a list in Table 1, but the more-useful reference list - with DOI! - occurs in Data Availability Section 4.3. Authors (page 12, section 2.4) refer to use of GE Engine. Because I know that several of their data sources did not (do not?) exist on GE Engine, I conclude that these authors uploaded some of their required products into GE Engine in order to then use GE Engine tools for analysis and display. Note reference to GE Engine version of Figures 1 and 3! Knowing Google, we have no confidence that GE Engine will have same version of same data products or the same tools available for a subsequent user? Section 4.3 implies reliability, while GE Engine - for all its strengths - implies proprietary changes? In other cases, I believe ESSD has asked authors to archive a snapshot in a repository separate from GE Engine to ensure a stable version matched to their description. Here we would at least need to know which and how many of the data products listed in Section 4.3 remain accessible via GE Engine or, more likely, whether subsequent users would need to track and obtain individual sources to replicate this work. From ESSD guidelines (https://doi.org/10.5194/essd-10-2275-2018) one reads "The journal expects that a future user, 5 or more years after date of publication, will find exactly the data, the tools and the recipe (description) that allow her or him to completely and reliably reproduce any figure from the original data description or accompanying research paper." These authors fail to convince us that they have met that ESSD expectation?

Manuscript fails to present comprehensive estimates of uncertainty. Throughout a reader encounters percent changes to four sig figs (e.g. 15.04% to take but one example of dozens), apparently precise to 0.01%. But, if these overall estimates instead carry an uncertainty larger than 0.01% (likely!), then single estimates of 15.04% or time changes of 0.60% need plus/minus uncertainty estimates? This reviewer doubts, for example, that authors can reliably report time-dependent changes from e.g. 2000 to 2015 for most regions. Instead, convert all data to area numbers (km^2) and then provide uncertainties in $\pm$ km^2? In some cases authors seem to calculate percent changes of percent data? At page 13 line 452, authors claim *with no basis or justification* "estimate of the level of precision (~0.00001) [for H] given the data inputs". Where does this unlikely precision come from? Later still, page 14 line 494, the authors claim "our best-estimate of 0.14605: [for H]. Please explain? The first based purely on high n count of H estimates while the second based on 50 randomized map (all pixels?) comparisons? Authors provide no basis for confidence, nor any discussion of larger uncertainties due to other complicating factors (e.g. wildfires, climate warming, etc.). Present Section 3.4 focuses entirely and only on statistical uncertainties of H but misses larger questions of reliability and accuracy. Neither do the authors assign any uncertainty to so-called validation products HF or THPI. This reader gains no confidence from those comparisons; improvements such as exist occur entirely due to higher spatial resolution (e.g. page 14 line 504, 505)? Manuscript needs to

provide readers and users an expanded detailed section conveying accurate dissection of uncertainties in H - including cumulative uncertainties propagated forward from source data - and of overall uncertainties of change estimates with reference to factors (stressors) not included here. For calendar year 2019, combination of fires in Australia, Amazonia, Siberia, California, etc. represented an equal or larger stressor than any itemized here? True, or not true? Small areas with high intensity values? Cumulative impact over 15 years? Authors give no hint. Caveats (Section 4.2) addresses these issues but in qualitative narrative rather than quantitative form. I especially worry about this statement (page 16 line 550, 551: "estimates of H generated for areas less than roughly 100 km 2 should be used with caution"! How then does a reader have any confidence in 0.09 km² resolution?

Specific comments, suggestions, complaints

Page 2 lines 23-24, "natural lands were lost (~17 per minute)" 17 pitches per minute? At 8 pitches per breath that means only 2 very slow breathes per minute? Back of the envelope: 5 slow breathes per minute, 7k m² per pitch, gives 5 x 7k x 8 = 280k m², = 0.28 km² per minute? Times 1440 minutes per day, would give 400 km² per day. If I work backward from their number - 178 km² per day - that gives .12 km2 per minute, or - at 7k m² per pitch and 8 pitches per breath, closer to 2 breathes per minute? Can the authors sustain fewer than 6 breathes per minute? Sorry to waste my and the authors time on this triviality, but unless they declare units more clearly and accurately, this sentence represents a not-useful diversion? Unfortunately, despite enthusiasm expressed in these 'real world' areal estimates, uncertainty here early in the manuscript persists throughout.

Page 3 line 49: Interestingly, the HYDE 3.2 product (by same author - Klein Goldewijk - cited here, also in ESSD at https://doi.org/10.5194/essd-9-927-2017), attempts similar assessments of total land use impacts from an inventory approach different to the remote sensing approach applied here. That group aspires to much longer (10k years) time coverage at much lower spatial resolution but justifies the trade-off of low spatial resolution for extended temporal coverage. Not 'high-resolution" but potentially "temporally comparable" at least over past decades? Have these authors have dismissed too many prior studies or contemporary work on human impacts issues?

Page 3 line 60: "obstructions by vegetation canopy (e.g., some roads, trails)" - Strange wording here. I think the authors mean that remote sensing might miss development features such as roads or trails hidden by vegetation canopies? As written, however, the sentence implies that roads and trails represent examples of vegetation canopies?

Page 3 line 69: "assumes additive but monotonic relationships" additive but monotonic? Some inconsistency here? Authors need to provide better explanation?

Page 8 line 264: "silver, tin, U 3 O 8 , and zinc" - strange to find uranium represented by chemical formula of uranium oxide while authors list all other metals by common names?

Page 10 line 337: I understand why authors felt the need to not include wildfires, but this exclusion should show up as an uncertainty later? (ESSD published a summary of global fire emissions, which must have had some satellite-based global fire product as it basis?)

Page 12 line 418: here 177 while the abstract had 178 (if "roughly", use 175?) and again the 17 football pitches not useful.

Page 12 lines 423, 424: absence of climate-induced warming as one of their stressors becomes acute for high latitude regions: Arctic greening (one of the predominant satellite-detected changes on the planet even if now waning); permafrost thaw; loss of snow cover

(global gridded product does exist going back two or three decades)? At lower latitudes, sea-level driven salt water intrusion of mangroves or into island freshwater aquifers, etc? In Oceania, large scale consumption-driven conversion to palm oil plantations (I think I saw a recent ESSD data product on this), possibly a more-useful validation point? Invasive species, food security, social health likewise, even if global data for those features does not exist? Despite skill with selected data products, authors have not convinced this reader that they captured the most important stressors. Despite basic oft-repeated excuse - that global data sets of not-included stressors do not exist - they have not convinced us that compilation and analysis of global data products that do exist represents a reliable reproducible useful product