Earth Syst. Sci. Data Discuss., referee comment RC4
https://doi.org/10.5194/essd-2022-336-RC4, 2022

Comment on essd-2022-336

Anonymous Referee #3

Referee comment on "Estimating local agricultural gross domestic product (AgGDP) across the world" by Yating Ru et al., Earth Syst. Sci. Data Discuss., https://doi.org/10.5194/essd-2022-336-RC4, 2022

This paper extends the authors' previous Brazil-focused work on gridded agricultural GDP to the entire world. It uses cross-entropy optimization to disaggregate an impressively large number of national and subnational datasets to 5 arc-minute grid cells. The authors previously applied this method to the development of gridded global crop data ("MapSPAM"). Their development of gridded agricultural GDP in this paper is a valuable advance given that agriculture is essential to human survival, remains the dominant economic sector in rural areas of most countries (especially low-income countries), and is threatened by climate change. The authors necessarily make some strong assumptions to construct this new dataset, but I don't view their assumptions as being any less tenable than those that underlie standard national accounts statistics.

My overall reaction to the paper is favorable, but I have two general suggestions for making it easier to understand and more convincing. First, it can be organized better. It flows naturally through section 2.3. After that point, I suggest reorganizing it as follows:

- Create a new section 3, titled "Results and Validation." This section would begin with the presentation and interpretation of the new dataset on gridded agricultural GDP, which is displayed in Fig. 7. It would then compare the new dataset to the night-time lights (NTL) data, the point of which (as I understand it) is to demonstrate that NTL is not a good proxy for gridded agricultural GDP. Nor are gridded total GDP or gridded population (Table 2). Hence, the new dataset does indeed provide new information. The material in current section 2.5 would be integrated into this new section.
- Confidence in these findings depends on the validity of the new dataset, so new section 3 would next cover validation. This subsection would begin with the acknowledgment of limitations of the new dataset presented in current section 3.2 (including 3.2.1 and 3.2.2) and wrap up with the presentation of the validation findings in current section 3.1 (including Fig. 10).
- A new section 4 would follow and would be titled something like "Illustration of use: drought risk." It would integrate information from sections 2.4 and various parts of

section 3 and would include Fig. 5, 6, 8, and 9 and Table 2.
- The paper would finish with the existing concluding section.

Second, and more substantively, the authors need to address several issues with the construction of the wood production component in section 2.1.3:

- The Lebedys and Li (2014) estimates used by the authors are, to my knowledge, the best available estimates of forest sector GDP, but they focus on industrial roundwood (and products derived therefrom) and largely exclude fuelwood, which accounts for half of global wood harvests. As a result, even allowing for fuelwood's unit value being much lower than industrial roundwood's, the current wood production component underestimates the contribution of wood harvests to agricultural GDP. The easier option for the authors would be to stick with the current estimated component but acknowledge that it underestimates the wood harvest value. The harder option, but the one I encourage the authors to consider, is to figure out a way to add the value of fuelwood harvest to the component. Fuelwood harvests are usually correlated with the collection of nonwood forest products, so perhaps the authors can use information in Siikamaki et al. (2015) to impute gridded values for fuelwood harvests. I note that Siikamaki et al. refer to some of the studies they reviewed as having included information on fuelwood values. Annual data on national harvests of fuelwood from FAOSTAT-Forestry might also be useful in the imputation.
- The authors write, "The value of wood products per pixel is calculated based on forest loss from year 2010 to year 2011 …." This statement requires qualification and, ideally, some additional analysis. The MODIS dataset the authors use to calculate "forest loss" measures tree cover, which includes perennial tree crops such as oil palm plantations, cocoa plantations, orchards, etc. in addition to wood-producing forests. This is a well-known deficiency of satellite-based "forest cover" datasets (Tropek et al. 2014; https://www.science.org/doi/10.1126/science.1248753). The authors' estimate of "forest loss" thus includes the replanting of perennial tree crops that occurs when the trees have reached the end of their economic lifetime. The resulting upward bias in "forest loss" can be substantial. For example, oil palm is replanted every 20-30 years, which implies a 3-5%/year "deforestation rate" that is many multiples of the annual loss rate for true forests reported in standard sources (e.g., FAO's Global Forest Resources Assessment). Fig. 3 in the paper illustrates this problem, as it shows wood production occurring in parts of Malaysia and Indonesia that are virtually 100% oil palm plantations. I know there are remote sensing products that show the locations of oil palm plantations, and perhaps there are ones for other non-forest tree crops too. I encourage the authors to use these products to estimate forest loss more accurately by masking out areas with tree cover that are not forests.
- Also requiring qualification is the statement, "forest loss due to fire should be removed because it does not result in wood products." Land clearing often involves a first stage of wood harvests followed by burning to eliminate remaining vegetation and woody debris. The authors' assumption that wood harvests do not occur in areas with fires thus results in underestimating the area harvested for wood products. I can't think of a way to fix this problem, but the authors should acknowledge it.

Specific comments

- Abstract: State the year of the new gridded dataset, i.e., 2010. Precede the penultimate sentence on the drought analysis with a phrase like, "To illustrate use of the new dataset, the paper …." Such a phrase would clarify that the paper is not primarily about drought risk. The paper would need to be completely rewritten if that were the case.
- Line 22: The authors could note that detailed agricultural data are also needed to evaluate forest restoration opportunities (e.g., P. Shyamsundar et al., "Scaling smallholder tree cover restoration across the tropics," *Global Environmental Change* 76, 2020; https://doi.org/10.1016/j.gloenvcha.2022.102591), which have become a focus of "nature-based" climate solutions (B. Griscom et al., "Natural climate solutions," Proc Natl Acad Sci USA 114, 2017; https://doi.org/10.1073/pnas.1710465114) since the launch of the UN Decade on Ecosystem Restoration (https://www.decadeonrestoration.org/). Agriculture is the main land-use competitor for forestry. The dataset developed in the paper will help researchers and policymakers better understand the opportunity cost of converting land from agriculture to forest.
- Line 45: Given that the paper is about GDP, "income" would be better than "wealth."
- Line 50: The phrase "the uniform distribution of labor in agriculture is another key concern" is vague and should be clarified.
- Line 65: The authors refer to two main contributions of the paper, with one being the drought analysis. I view that analysis as an illustration of the use and value of the new dataset, not as a main contribution. For the latter to be the case, the authors would need to provide more context for the drought analysis and evaluate it more directly against prior analyses. Constructing the new dataset is a sufficient contribution to justify publication of the paper in my view.
- Line 92: State the year of the producer price data. 2010? Mean of 2009-2011? Relatedly, the authors need to explain somewhere whether their agricultural GDP estimates are purchasing power parity (PPP) estimates or market-price estimates. Information in footnote 9 is pertinent to this point. The authors should explain the implications for interpretation of the dataset if the prices that underlie it are not using consistently defined (i.e., some prices are in PPP terms while others are market prices).
- Footnote 3: This point should be incorporated into the text. Prices for agricultural, forestry, and fishery products can vary greatly within countries and their subdivisions. The use of national prices is unavoidable given current data limitations, but it is a shortcoming of the new dataset that the authors should acknowledge in the text.
- Line 116: The use of uniform livestock conversion factors across countries seems like an unnecessary simplification. Why not use country-specific FAOSTAT data on the value of products from each type of animal?
- Lines 130-132: The authors' use of forestry terms is unconventional. I recommend the following rephrasing: "The trees are harvested for fuelwood and industrial roundwood, which is processed into a variety of products including lumber, plywood, furniture, and paper products." Mentioning fuelwood is necessary given that it accounts for half of global wood harvests.
- Footnote 5: The MODIS land cover data used by the authors is quite coarse, ~500 m at the Equator. I doubt it reliably measures selective harvesting or forest degradation. I recommend rephrasing the footnote as follows: "The measurement is limited to detection of land cover change from satellites and might not fully account for selective harvesting or forest degradation."
- Lines 188-189: Mention the typical level of the subdivisions in the dataset here or earlier. Lines 230-231 imply they are mostly Level 1 subdivisions (i.e., states or provinces).
- Lines 214-219: The authors state, "Theoretically, the sum of these components should be close to the official values obtained from the World Development Indicators." This statement prompts two thoughts. First, as part of the validation of the new dataset, I recommend presenting information on the ratio of the sum of the components to the official values and interpreting any systematic discrepancies that are observed across

regions, countries, or subdivisions. Second, I wonder whether the components the authors have constructed actually correspond to GDP components in all cases. GDP refers to value added, i.e., output value minus expenditure on intermediate inputs. I believe that some of the authors' components refer to output value (e.g., the crop and livestock estimates) whereas others refer to value added (e.g., the forestry estimates). If I am correct, then there is a conceptual inconsistency across the components that the authors must acknowledge and whose implications they must discuss.

- Line 241: The authors need to explain why they have chosen two drought indicators instead of one. Are two indicators necessary? If the purpose of the drought analysis is to illustrate the use and value of the new agricultural GDP dataset, then why not use only one? Moreover, given global concerns about climate change, why not illustrate use of the new dataset by using a forward-looking indicator of climate-change risks? The SPEI and WCI indicators are backward-looking, which makes them of dubious value given that climate change is altering drought risks.
- Table 2: I suspect that the correlations are not significantly different within some of the regions. I recommend adding information on the significance of the differences between the following pairs of correlations within each region: AgGDP/NTL vs. GDP/NTL, and AgGDP/NTL vs. POP/NTL.
- Figure 7: Given that grid cells become smaller at higher latitudes, shouldn't the map show $/km2 instead of $?
- Lines 306-307: The authors state, "One advantage of the cross-entropy is the volume preserving pycnophylactic property, which ensures the sum of the gridded data is the original value …." Spatial regression presumably violates this property. Does the analysis of predictive accuracy in the Brazil study by Thomas et al. (2019) indicate how much spatial regression violates it? In the current paper, the authors' comparison of the cross-entropy dataset to the naïve dataset based on rural population would be more compelling if Thomas et al. find that spatial regression violates the property a lot and thus is internally less consistent than the cross-entropy dataset.
- Line 317: The authors state, "Since we cannot perform an evaluation of prediction accuracy for all countries …." Why not? I'm not saying they should perform such an evaluation. I am just unclear as to why they cannot perform it. Can they perform it for a subset of countries?
- Lines 320-324: Doesn't the finding that the naïve and cross-entropy maps are not significantly different imply that one might as well use the (presumably) simpler and more transparent naïve approach instead of the cross-entropy approach? I.e., what is the advantage of the cross-entropy approach over the naïve approach if the two approaches yield statistically indistinguishable results? Preservation of the pycnophylactic property? If so, can the authors provide information on the degree to which the naïve approach violates that property?
- Line 334: The authors need to define "MAUP."
- Lines 336-337: The authors write, "The data are most appropriate for applications at global, continental and regional scales (You and Wood, 2006)." Aren't the data also appropriate for applications in countries that contribute data from a relatively large number of subdivisions to the cross-entropy optimization (e.g., Thailand)?
- Line 381: Starting the "Conclusions" section with discussion of the drought analysis is odd given that the main contribution of the paper is the construction of the new gridded dataset. The current second paragraph in the section would work better as the starting paragraph.
- The manuscript includes an Appendix B but no Appendix A. Is Appendix A missing, or is Appendix B mislabeled?