Response to CC1

General Comments:

The high-resolution grazing maps are of significant important to researchers and grassland managers, as they substantially improve our understanding of the threats posed to grasslands by overgrazing. Moreover, these maps have potential applications in various areas. I think this study makes very useful contribution to the field, the maps provided are the first of their kind for the Tibetan Plateau, making them a crucial resource for studying grazing intensities in this region. As such, their publication is highly recommended. Overall, the study's novelty is appreciable, and its quality aligns with the standards of ESSD. However, the manuscript will need some minor revisions before it can be accepted for publication.

Response: Thank you very much for your overall positive words about our paper. We are delighted to hear that our manuscript received such feedback. We have improved the manuscript according to you and another reviewer's suggestions. In the following sections, you will find our detail responses to each of your points and suggestions. We are grateful for the time and energy you expended on our behalf.

Specific Comments:

Line 48: There are a number of typos need to be carefully revised in the references. For example, it is unclear whether there should be a comma after 'et al.' in the author citations.

Response: Thanks for raising this point. We have checked and revised all the references one by one in the revised manuscript with reference to the requirements of ESSD journal (see through the text).

Line 94: The text incorrectly states thirteen factors, whereas only four are discussed. This discrepancy should be corrected.

Response: We have reconfirmed that Li et al. (2021) did select 13 environmental factors from four aspects: land use practice, topography, climate, and socioeconomic. In the revised version, we have modified the vague expressions (see L85~88).

Lines 142-143: I think although this study makes important contributions to the grazing intensity mapping, the methodological framework is generally not new. This needs to be addressed if the manuscript is to be revised.

Response: Thanks for raising this point. We have deleted the word of "novel" in the revised version (see L138).

Line 166: Figure 1 lacks a compass.

Response: Since the WGS_1984_Albers projection was used for Figure 1, placing a compass would contradict the direction indicated by the latitude lines, so the direction was indicated by the latitude and longitude lines in Figure 1.

Line 176: A comma is needed in the number 4998 for consistency with standard numerical formatting.

Response: Thanks for raising this point. We have corrected these numbers for consistency with standard numerical formatting in the revised manuscript (see through the text).

Lines 228-231: The descriptions of the methodology, particularly the specific machine learning algorithms used is poor and needs to be improved.

Response: Thank you for this great suggestion. We totally agreed with you that the methodology section need to be improved. In light of your feedback, we have introduced more detail information about the five machine learning algorithms in the appendix (see L148~182 in the supplement file for details).

Lines 382-415: This section would benefit from referencing additional literature related to grassland management on the Tibetan Plateau. For instance, including Wang et al. (2023) published in Nature Reviews Earth & Environment could provide more insights into the novelty and contribution of the GDGI dataset.

Response: We appreciate this valuable comment. In the revised version, we have had an in-depth discussion related to grassland management in the Tibetan Plateau by referencing more literatures, including the paper you mentioned (see L425~487). We hope you find these revisions rise to your expectations.

Response to RC1

General Comments:

Livestock is a crucial contributor to global food systems through the provision of essential animal proteins and fats, and plays a significant role in supporting human survival and socio-economic development. This study developed a long-term and high-resolution grazing intensity dataset in the Qinghai-Tibet Plateau from 1990 to 2020 by integrating machine learning algorithms, census data, and multiple environmental variables and socio-economic factors. The manuscript is well structured, but the discussion section needs to be improved. The machine learning model performs well. The spatial pattern of the grazing intensity map is also consistent with previous studies and looks more reasonable. The dataset is valuable for the research focusing on climate change, human activities, and their interactions with ecosystem dynamics in the Qinghai-Tibet Plateau. However, I have some concerns in the data preparation, model training and validation, which are provided blow.

Response: Thank you for these overall positive words. We have thoroughly addressed all of your comments and suggestions, especially for your concerns in the data preparation, model training and validation, as well as the discussion. Please see our detail responses to the individual comments in the following sections. We hope that our revisions rise to your expectations.

Major comments

1) The first major concern is the input data for spatialization. The key input datasets for the grazing intensity spatialization are population density, climate data, terrain, and HNPP.

Population density data:

Line 66-68 in supplementary materials. WorldPop data at 100 m resolution from 2000 to 2020 and Population at 1km resolution from 2000 to 2015 was used to reconstructed the population at 100 m during 1990 to 2000. For each pixel, only four records were used to fit the linear model, there is large uncertainty using such as model in estimating the population in 1990 and 1995. The downscaling method or bias correction of population data (1990-2000) is not reasonable and can be improved, check the bias correction method from He et al. (2022). He, S., Zhang, Y., Ma, N., Tian, J., Kong, D., and Liu, C.: A daily and 500 m coupled evapotranspiration and gross primary production product across China during 2000–2020, Earth Syst. Sci. Data, 14, 5463–5488, https://doi.org/10.5194/essd-14-5463-2022, 2022.

Response: Thanks for raising this important point. We totally agree with you that fitting a linear model using only four pairs of records for each pixel may introduce uncertainty. Nevertheless, we have no other choice because the ChinaPop dataset at 1-km resolution is at 5-year intervals. In light of your feedback, we address the uncertainty that this shortcoming may introduce in the revised discussion section (see L488~533). As for the bilinear interpolation and delta change method you mentioned, which were used in the He et al. (2022) article, we have actually paid attention to it before. The reason why they were not adopted as the downscaling and bias correction method in our manuscript is mainly based on the following reasons:

Firstly, as the bilinear interpolation method automatically generates new intermediate values during the interpolation process, it is very suitable for indicators with continuous values, like surface temperature in the He et al. (2022) article. It is obvious that population density is not a continuous variable. As for the nearest neighbor algorithm, it does not change any pixel values in the resampling process, thus avoiding the unanticipated bias caused by pixel value changes, and is suitable for indicators with discontinuous values. Therefore, in this study, the nearest neighbor method was used instead of bilinear interpolation method for downscaling process.

Secondly, another reason we used the nearest neighbor algorithm is to be consistent with the WorldPop dataset, which also used the nearest neighbor method for their resampling process.

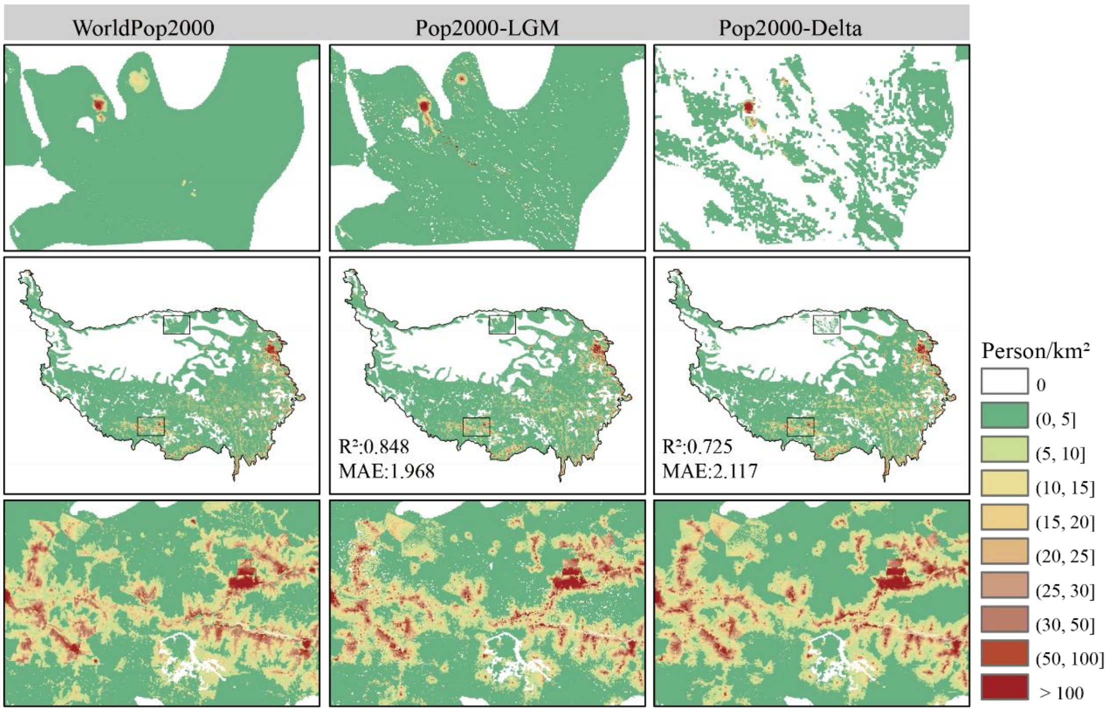

Figure R1. Comparisons of the reconstructed population density maps and the WorldPop dataset in 2000.

Thirdly, since the 100-m WorldPop dataset is one of the basic data sources in this study, the criterion for determining the effectiveness of the downscaling and bias correction method, should be based on the compliance of the simulation results with the WorldPop dataset. In other words, the more similar the reconstructed data and the WorldPop dataset, the better performance of the methods are considered to be. Taking the population density map in 2000 as an example, we used the bilinear interpolation method to resample the 1-km population density data to 100 m, and then corrected the population distribution data with Delta change method. In addition, we also used the nearest neighbor and liner regression methods to resample the 1-km population density data. Last, we compared these two 100-m population density data with the WorldPop dataset. It can be seen that the reconstructed population density data by using the nearest neighbor and liner regression methods are more consistent with the WorldPop data (see Figure R1 in the attached response file).

- Moreover, the WorldPop dataset is not reliable in the QTP. On the contrast, the 1km population from obtained from the Resource and Environment Science and Data center of the Chinese Academy of is more accurate, because it is developed by using the county-level census data.

Response: This is another good point. In this study, the reason we chose the 100-m WorldPop dataset instead of the 1-km ChinaPop dataset are mainly based on the following reasons:

First, the WorldPop dataset has higher spatial and temporal resolution (100-m and annual), which provides important support for this study to eventually provide annual grazing dataset at 100-m resolution. While the ChinaPop dataset has a spatial resolution of 1 km and a temporal resolution of five years, which can not serve this study well.

Second, the WorldPop dataset is actually more accurate than the ChinaPop dataset on the QTP. It is true that the total population number of the ChinaPop dataset does better match the actual statistics than the WorldPop dataset, because it was developed based on county-level census data. However, the spatial heterogeneity of the ChinaPop dataset is coarse, whereas the WorldPop dataset is able to show more details (Figure R2a,2b in the attached response file). For example, the ChinaPop dataset has populations in lakes and glaciers, which is obviously unreasonable. Moreover, the WorldPop dataset was able to successfully identify the uninhabited areas in the northwestern part of the plateau, whereas the ChinaPop dataset can not (Figure R2a,2b in the attached response file). In addition, the WorldPop dataset can identify 204 urban areas on the QTP, while the ChinaPop dataset can only identify 116 urban areas with fuzzy boundaries (Figure R2c in the attached response file).

Third, other scholars have also shown that the WorldPop dataset is more accurate than the ChinaPop dataset on the Tibetan Plateau (Li et al., 2020). Li, L.

H., Zhang, Y. L., Liu, L. S., Wang, Z. F., Zhang, H. M., Li, S. C., and Ding, M. J.: Mapping Changing Population Distribution on the Qinghai–Tibet Plateau since 2000 with Multi-Temporal Remote Sensing and Point-of-Interest Data, Remote. Sens., 12, 4059, https://doi.org/10.3390/rs12244059, 2020.

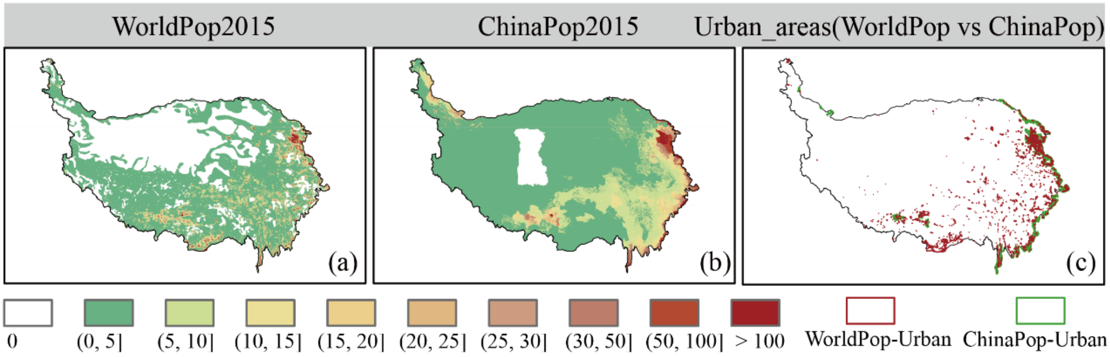

Figure R2 Comparisons between the WorldPop and the ChinaPop dataset

Besides, there are only 7 snapshots for population after data harmonization. How did you get the population density data for rest years to train the machine learn models and conduct the grazing intensity spatialization?

Response: Thanks for raising this important point. After obtaining population density maps for 1990 and 1995, we generated maps for the remaining years in 1990-1999 by linear extrapolation. We have added this information to ensure clarity according to your comments in the revised version (see L34~37 in the supplement file for details).

I did check the grazing intensity dataset and found that pattern of grazing intensity near the urban/built-up area was quite similar with WorldPop data. The influence of population density also resulted in some abrupt changes in the southeast of Qinghai Lake.

Response: Many thanks for your comment. In this study, we assumed that grazing activities are confined solely to grassland, thus have excluded urban areas. Nevertheless, livestock grazing activities can still be influenced by a variety of social and environmental factors, which also including population distribution. Therefore, it is reasonable for grazing intensity patterns to be similar with the WorldPop data. In fact, other studies also showed that grasslands closer to settlements face more degradation, because of more grazing intensities. Li, C. X., de Jong, R., Schmid, B., Wulf, H. &Schaepman, M. E. Spatial variation of human influences on grassland biomass on the Qinghai–Tibetan Plateau. Sci. Total Environ. 665, 678–689 (2019). https://doi.org/10.1016/j.scitotenv.2019.01.321.

In fact, the pattern of grazing intensity in this study are not only similar with population data, but also with other factors used in this study. Taking the

population density and slope as example, it can be seen that the grazing intensity pattern is not only similar with the population density pattern, but also similar with the slope pattern (Figure R3a, 3c, 3d in the attached response file).

It is true that there is an abrupt change in grazing intensity in the southeastern part of the Qinghai Lake in our GDGI map. In fact, this pattern is consistent with the real condition in this region, as the census data has showed that several counties in the southeastern part of Qinghai Lake have high livestock numbers and relatively low grasslands, which in turn leads to higher grazing intensity (Figure R3b in the attached response file). In addition, this pattern can also be proved by other grazing intensity maps (see Figure 8 in the main text).

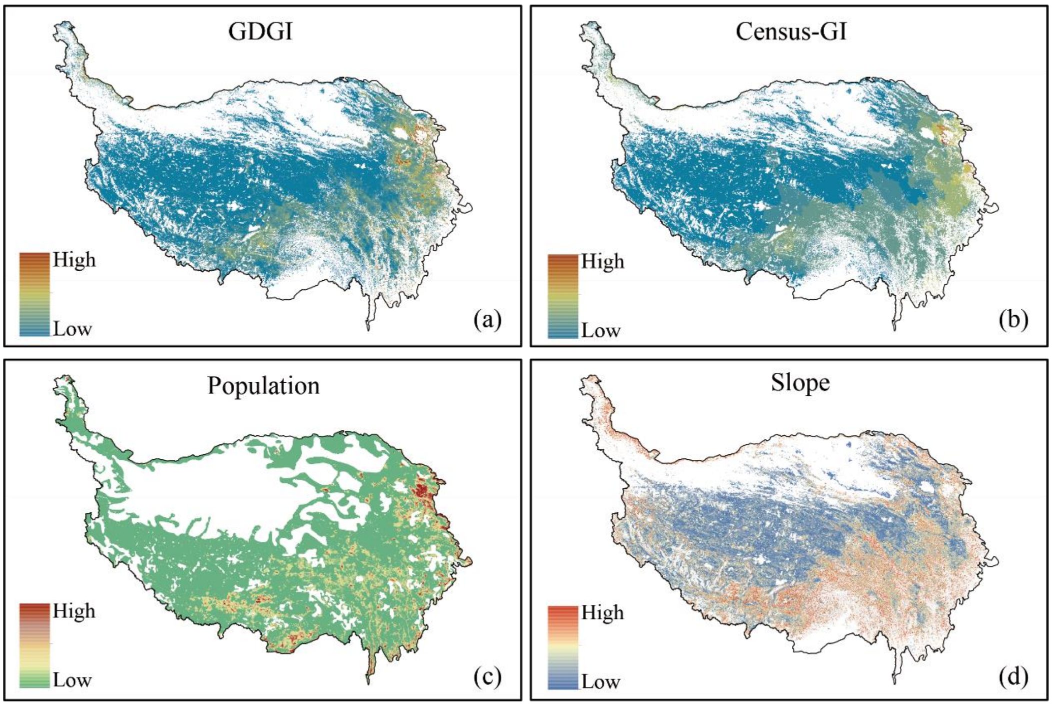

Figure R3. Comparisons for the distribution patterns of grazing intensity maps, environmental variables, and census livestock data.

HNPP data：

- MODIS terra was launched in 1999, it is impossible to get the NPP product before 2000. How did you the MOD17A3 NPP data from 1990 to 2000. Please check the data source carefully.

Response: Thank you for pointing out this error. We sorry about this mistake. In this study, the NPP data for the 2000-2020 period (NPP-I) is obtained from the Land Processes Distributed Active Archive Center (https://lpdaac.usgs.gov), and the NPP data for 1990-2015 (NPP-II) was obtained from the Global Change Research Data Publishing and Repository (http://www.geodoi.ac.cn). Chen Pengfei. Monthly NPP Dataset Covering China's Terrestrial Ecosystems at North

of 18°N (1985-2015), 2019, 3(1): 34-41. DOI: 10.3974/geodp.2019.01.05. In the revised manuscript, we have corrected this error (see L119~120 in the supplement file for details).

The downscaling or bias correction method of NPP data (1990-2000), same as population density data, is not reasonable and can be improved.

Response: We appreciate this valuable comment. The reason we did not downscale the NPP data using a combination of bilinear interpolation and Delta change correction is similar as the population density downscaling process. Taking the year of 2000 as an example, we compared the estimated NPP data obtained using bilinear interpolation and Delta change correction with the results obtained using the nearest neighbor method. It can be seen that the estimated NPP by using the nearest neighbor method are more similar with the real Modis NPP values (Figure R4 in the attached response file). The reasons for such a result are similar as the population density downscaling process above, please refer to the responses in that section.

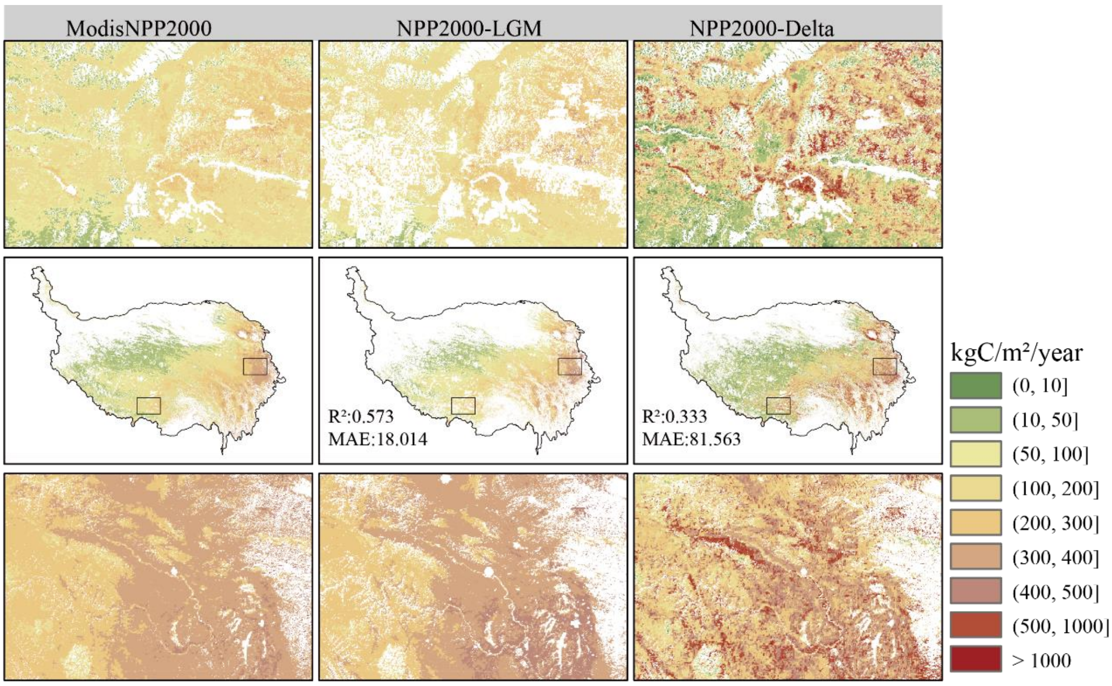

Figure R4. Comparisons of the accuracy by different downscaling methods in predicting NPP distribution.

The Thornthwaite Memorial model to estimate the potential NPP in QTP, is it suitable for QTP? I suggest add several figures to show the spatial pattern of HNPP and reflect the contribution of human activities on grassland in the QTP. How is the relationship between HNPP with population density?

Response: Thanks for raising this important point. Our previous literature statistics have showed that there are at least more than twenty papers using the

Thornthwaite Memorial model to estimate PNPP on the Tibetan Plateau after 2010. Therefore, it is reasonable to believe that this model is appropriate for the QTP. In addition, based on your suggestion, we have added one map to show the spatial pattern of HNPP (see Figure S2 in the supplement file for details). As for the relationship between HNPP and population density, we calculated the relationship between them by using the Bivariate Moran's I index. The results showed that Moran's I was 0.2, which indicate there is positive relationship between population density and HNPP . To verify the reliability of the results, we further performed randomization with 999 permutations, which showed a p-value of 0.001 and a Z-score of >2.58. This indicates that there is a significant positive correlation between population density and HNPP. This means that where population density is high, HNPP are also high (see Figure S2 in the supplement file for details).

2) Another major concern is the spatialization model. Because the machine learning models were trained and validated by using county-level data, it should be very careful before applying to the prediction at grid scale. Specifically, the data you use to train the model may not be representative all the grids because it only reflects an average condition of each county. If the county-level model was applied to grid scale, there would be some bias in the spatial prediction though the model results look good at county-level, such as low estimation at high grazing intensity area and high estimation at low grazing intensity area. Thus, a statistical distribution comparison between the training data and grid data should be conducted to prove the model is transferable from county-level to grid-level.

Response: This is another important point. We totally agree with you that there may be some bias when the model trained at county-level was applied to grid scale. Therefore, a statistical distribution comparison between the training data and grid data is necessary. In fact, we do show this comparison in our manuscript (see Figure 4 in the main text). Unfortunately, we lacked the statistical analysis. In light of your feedback, we have given the statistical analysis results in the revised version (see L278~282 in the main text), which show that the model trained at county scale can be downscaled to the pixel scale. We hope you find these revisions rise to your expectations.

3) The data validation (section 3.2 & 3.3) is quite weak. Especially, the validation in section 3.2 is not convincing and meaningless, because the grid data already corrected using the census data. Thus, the above validation is not enough because the newly grazing intensity data is grid-based and has very high resolution (100 m). Validation by using site observed grazing intensity is necessary as that in Meng et al. (2023).

Response: Thanks for raising this important point. We totally agree with your comment and have deleted the validation analysis at county scale in Section 3.2. Moreover, we have added the validation of the GDGI dataset at the pixel scale (see L 289-298 in the main text). Also, we have provided detail information on these ground-truth validation data in the appendix (see Figure S3 and Table S3 in the supplement file for details).

Minor comments

1) Line 16: “socio-economic”, “social economic” should be consistent in the whole manuscript.

Response: Done. Thank you (see L15, L189 and L413).

2) Line 16: replace “land cover” as “vegetation index”. In section 2.2, “land cover” is not included in the factors affecting grazing activities.

Response: We apologize for the confusion caused by the description as you mentioned. In the revised version, we have replaced “land cover” as “vegetation” (see L15).

3) Line 143: The methodological framework is not novel. Figure 2 and the four major steps in section 2.3 look quite similar with the method in Meng et al. (2023).

Response: Many thanks for your comment. We have deleted the word of “novel”, and revised the flowchart in the revised manuscript (see L137 and Figure 2 in the main text).

4) Line 171-178: Census livestock data in some counties were not available, how did you process these counties when conduct spatialization in the missing-data year?

Response: Thanks for raising this important point. In this study, these missing data were processed by using the following two methods. Specifically, for these counties belonging to the same prefecture, including counties in Ganzi and Aba prefectures in Sichuan Province, we used the livestock census data at the prefecture-level to carry out spatialization. For these counties in Yunnan Province, since they belong to different municipalities, it is not reasonable to replace them with municipal-level data. For these counties without livestock census data for some years, we supplemented the missing data by linear interpolation with grazing density data in available year. We have stated the process clearly in the revised manuscript (see L169~175).

5) Line 179-182: A spatial distribution map of the towns with available livestock data could add in the supplementary materials.

Response: Thank you for your suggestions. A spatial distribution map of the townships with available livestock data, as well as the site of ground truth validation data have been added in the revised version (see Figure S3 and Table S3 in the supplement file for details).

6) Line 209: revise “mappingd”.

Response: Done. Thank you (see L221).

7) Line 213: The author used the 30 m annual land cover dataset (CLCD) to derive grassland extent. The CLCD data ranged from 1985 to 2020, is the grassland extent in this study dynamic or static? Make it clear.

Response: Thank you for your valuable comment. We used the 1990~2020 grasslands from the CLCD dataset, which has a dynamic range. We have clarified this clearly in the revised version (see L212).

8) Line 235-239: How the adjustment was conducted?

Response: Thank you for your question. In this study, grazing intensity is adjusted for the total amount of livestock in each county using the formula below. We have added this information in the revised version (see L240~246).

9) Line 282: The determination coefficient (R2) is 1 at county-level, indicating your model is really good. This was because you corrected the grazing intensity map (section 2.3.4). How was the model performance if there was no correcting step?

Response: We appreciate this valuable comment. As you have noted in your above comment, the main reason for the determination coefficient of 1 is that we carried out the correction at the county scale. In the revised version, we have deleted this section and the related statement. In addition, the model performance with no correction was also showed in the second paragraph of Section 3.1 (see L278~282 in the main text).

10) Line 379-381: Figure 7: The grazing intensity data from Meng et al. (2023) is not consistent with the original figure, especially the spatial extent. Please check it carefully.

Response: Thanks for raising this point. We think that for grazing maps, only areas with grazing intensity greater than zero are meaningful. Therefore, for the grazing intensity data provided by Meng et al. (2023), we extracted only those areas in which the grazing intensity was greater than zero. As a result, the maps appearing in our text are not consistent with their original maps. We hope you find these explanation rise to your expectations.

11) Section 4.2 Implications for grazing management. This section needs to be reorganized and add more findings (e.g., the hotspots of grazing) from the newly developed grazing intensity datasets rather than just talked the driving forces of spatial heterogeneity of grazing intensities.

Response: Thanks for raising this important point. We totally agree with you. In response, we provide an in-depth discussion of grazing management based on our newly developed grazing intensity maps on the QTP in the revised version. We hope you find these revisions rise to your expectations (see L427~489).

12) Line 423-425: A research “High-resolution livestock seasonal distribution data on the Qinghai-Tibet Plateau in 2020” already incorporated the seasonality into livestock spatial distribution mapping in the QTP.

Response: Thanks for your comment. Yes, there have been studies that have incorporated seasonality into livestock spatial distribution. Nevertheless, we discuss here the limitations of this study because we did not distinguish between warm-season and cold-season pastures.

13) Line 428-430: Agree with your discussion. See the second major comment.

Response: Thanks for this comment. We agree that due to the lack of enough training samples, the accuracy of model simulations may be compromised in this study. However, as we noted earlier in response to your second major comment, the ET model yielded relatively good results in livestock spatialization (see our response to your second major comment). We think that these limitations stated in section 4.4 are exactly where future research should focus.

Response to RC2

General Comments：

This manuscript presents a study utilizing the Extreme Trees (ET) model and detailed census data to produce annual Gridded Dataset of Grazing Intensity (GDGI) maps spanning from 1990 to 2020 in the Qinghai-Tibet Plateau (QTP) at a resolution of 100 meters. The authors compare the performance of five machine learning algorithms in delineating spatial patterns of grazing intensity in the QTP, concluding that the ET model offers the most accurate estimation. Overall, the manuscript is well-written with clearly defined objectives, and the study itself is both interesting and well-executed. However, there are some concerns regarding the lack of clarity and justification for uncertainties in data and results, as well as a few minor points that could be addressed.

Response: Thank you very much for your overall positive words about our manuscript. In the revised version, we have improved the manuscript according to your suggestions, especially your concerns regarding the lack of clarity and justification for uncertainties in data and results. In the following sections, you will find our detail responses to each of your points. We are grateful for the time and energy you expended on our behalf.

Specific comments

The main shortcomings of this study include:

1. The description of parameters for the machine learning models you trained is lacking.

Response: Thanks for raising this important point. In the revised version, we have introduced more detail information about the five machine learning algorithms as well as the description of parameters in the appendix (see L148~182 in the supplement file for details).

2. What is the optimal model you used in Line 235?

Response: Thank you for your question. When introducing the methodological framework for mapping high-resolution grazing intensity, we did not know which of the five models performed best. Thus, we can not specify which model was optimal at this section. In section 3.1 (Performances of models), according to the simulation verification results, it can be seen that the ET model performs best among the five models.

3. The authors have made efforts to address uncertainties and limitations in the discussion section. However, I find that the treatment of these aspects is somewhat superficial and lacks depth. There are uncertainties from input data,

model, and the framework. I expect when you offer the final predictions with uncertainties.

Response: Thanks for raising this important point. We totally agree with you. In response, we provide an in-depth discussion of uncertainties and limitations in the revised version, from input data, model as well as the framework (see L490~535 in the main text). We hope you find these revisions rise to your expectations.

4. In Table 1 and Figure 3, the R2 is too high, which may not accurately reflect real-world conditions. To ensure reproducibility and transparency, it would be beneficial to provide access to the model training and validation data, as well as the code used in the study.

Response: We appreciate this valuable comment. In the revised version, we have provided both the training and validation data in the appendix (see Table S4 in the supplement file for details). Nevertheless, due to the confidentiality requirements of the relevant departments providing the livestock census data, we can not specify the exact name of each county. However, this does not affect the validation process of the model. Moreover, the code used in this study can be found in https://figshare.com/s/ad2bbc7117a56d4fd88d. We hope you find these revisions rise to your expectations.

Minor issues:

1. "Random Forests" is the right name for the algorithm.

Response: Thanks for raising this point. We have correct "Random Forest" as "Random Forests" in the revised version (see through the text).

2. Line 443 Conclusions should be 6.

Response: Done. Thank you (see L544).

3. 5 Data availability could be moved after the conclusions.

Response: According to the requirements of ESSD, the section of Data availability is usually placed before Conclusions.

Response to RC3

General Comments：

Grazing has been an important human activity in the grasslands of the Tibetan Plateau for thousands of years. If grazing intensity is not considered as a factor, then most ecological problems cannot be solved. Therefore, this study has made an important attempt to do so, and in particular, has made a commendable effort by providing a map of grazing intensity on the plateau. However, I have several reservations about the research methodology used in the paper, which is directly related to the results of the study.

Response: Thank you very much for recognizing the value of our paper and for your overall positive words about it. We are delighted to hear that our manuscript received such feedback. In the revised version, we have improved the manuscript according to your comments and suggestions, especially your concerns regarding the research methodology. In the following sections, you will find our detail responses to each of your points and comments. We are grateful for the time and energy you expended on our behalf.

Specific comments

First, discrepancy in spatial resolution across nodel phases. This approach translates county-level analysis in model training and validation to grid-level (100 m resolution) prediction. One problem with this approach is the applicability of county-level training models to grid-level predictions without rigorous validation at the same finer resolution. Whilst the challenge of obtaining grid-level validation data is recognised, it is essential to ensure the reliability of model predictions at this scale. I recommend a detailed revision of the methodology to address or justify this discrepancy. This is where the study comes to life.

Response: Thanks for raising this important point. We totally agree with your comment and have incorporated pixel-scale validation of the GDGI dataset to ensure the model's predictive accuracy at this granularity (see L 289-298 in the main text). The pixel-scale validation dataset comprises a total of 112 records across 68 locations, which were derived from literatures, questionnaires, and field investigations. Furthermore, comprehensive details about these ground-truth validation data are provided in the appendix (see Figure S3 and Table S3 in the supplement file for further information).

Second, issues with averaging ecological factors at the county level. The study's approach of averaging environmental and other factors at the county level to infer grazing intensity raises questions about its validity and accuracy. Given the variability in county size and environmental conditions, this approach may oversimplify the complex dynamics of grazing intensity. A more nuanced approach, which might include feature selection and evaluation, might better capture the different influences on grazing intensity in different counties. In addition, the hypothesised correlation between average factors and grazing intensity deserves further research to confirm its validity.

Response: This is another important point. We fully acknowledge the legitimacy of the concern that averaging environmental factors might oversimplify the intricate dynamics between grazing intensity and lead to a certain degree of estimation inaccuracies. In our research, the rationale behind employing county-scale averages of environmental factors is to align with county-scale livestock census data. Although theoretically, it would be ideal to utilize sub-county scale livestock census data to cover the entire Tibetan Plateau, practical limitations currently render this approach nearly unfeasible. Consequently, grazing maps have widely been modeled through the averaging of environmental factors, predicated on the hypothesis that a consistent causal relationship between livestock intensity and environmental factors persists across various scales (Robinson et al., 2014; Nicolas et al., 2016; Li et al., 2021; Meng et al., 2023). Nonetheless, as far as the actual situation is concerned, the influence of environmental factors on livestock distribution varies across different scales, a fact we have addressed as a potential limitation in the revised manuscript (refer to L505~510 in the main text for details).

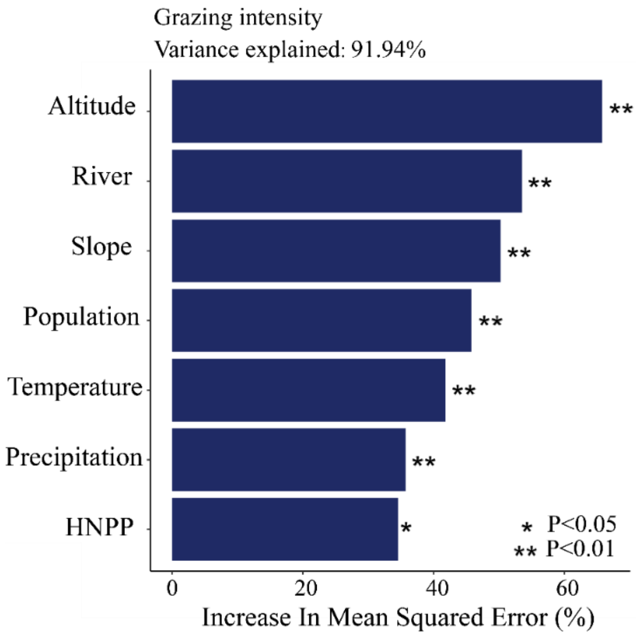

Figure R5 Importance of environmental factors influencing the spatial distribution of grazing intensity.

Indeed, we had previously identified this limitation. To mitigate it as effectively as possible, we assessed the relative significance of each environmental factor in predicting livestock distribution at the county scale before initiating the

estimation process (see Figure R5 in the response attachment). The analysis revealed that the seven environmental factors collectively account for 91.94% of the variance in grazing intensity distribution, with all factors achieving statistical significance ($p<0.05$). This demonstrates a specific correlation between the selected environmental factors and livestock distribution, underscoring their potential predictive power regarding grazing intensity.

Further validation at the pixel scale corroborates our approach. The validation results demonstrate that the GDGI dataset maintains high accuracy at this finer resolution (see L 289-298 in the main text), further validating the use of county-scale averaged environmental factors in predicting livestock distribution.

Third, exclusion of vegetation productivity factors: The omission of productivity indicators such as grass biomass or grass quality from the model is a serious flaw. These factors are intrinsically linked to grazing intensity and could greatly improve the explanatory power of the model. Without quality control of the grassland, all grazing is not possible. This may be the determining factor in whether the paper is published. It is therefore recommended that the relationship between grassland quality and grazing intensity be assessed to gain a fuller understanding of the determinants affecting grazing intensity and patterns in alpine grasslands.

Response: Thanks for raising this point. We apologize for any confusion. Indeed, our study has taken into account factors of vegetation productivity, opting for the Human-activity-induced Net Primary Productivity (HNPP) indicator over the traditional Net Primary Productivity (NPP). Numerous studies have validated NPP as a crucial metric for assessing grassland biomass and quality (Scurlock et al., 2002; Cheng et al., 2023; Adam et al., 2024). Our preference for HNPP over NPP is twofold.

Initially, it is critical to acknowledge that grassland biomass or quality merely indicates potential rather than actual grazing intensity. In other words, while grassland NPP may theoretically correlate with potential grazing capacity, it does not have a direct relationship with actual grazing activities. For instance, grasslands within nature reserves on the Tibetan Plateau often exhibit higher NPP compared to those in non-protected areas, yet due to strict restrictions for grazing activities, leading to lower grazing intensities (Zhu et al., 2023).

Furthermore, on the Tibetan Plateau, grazing activities represent the predominant form of grassland utilization. As mentioned above, HNPP quantifies the actual primary productivity harvested through human activities, thus reflecting the real extent of biomass utilization by humans. In addition, evidence suggests that HNPP bears a closer association with grazing activities than does NPP (Huang et al., 2022; Zhou et al., 2024).

For these reasons, we employed HNPP as a proxy indicator for grass biomass and quality in this study. Our model, integrating the HNPP indicator, exhibited commendable performance, elucidating 95.4% of the grazing intensity with

notable robustness (Figure 3 in the main text). In response to your feedback, we have further clarified our rationale for selecting HNPP over NPP in the revised manuscript (see L122-130 and L390-392 for detailed explanation). We hope you find these revisions rise to your expectations.

Other issue. From Fig. 5, around 1996 to 2000, government statistics show a dramatic reduction in grazing intensity, which may be caused by a change in statistical standards or policy, not necessarily a real reduction in grazing intensity. This needs to be explained in detail, otherwise there would be such a sharp change and the predictive model should be able to catch it.

Response: We appreciate this valuable comment. Yes, as shown in Figure 5 (Figure 7 in the revised version), the average grazing intensity on the QTP experienced a pronounced decrease in around the 1996-2000 period. Our investigation confirms that this significant reduction is not attributed to changes in statistical standards but rather to a substantial decrease in livestock numbers. Specifically, government livestock census data indicate a reduction of approximately 1,556,000 sheep units in livestock populations across the Tibetan Plateau during this period. The observed sharp decline in around 1996-2000 also aligns with other studies (Ye et al., 2020; Sun et al., 2022; Meng et al., 2023). Encouragingly, our GDGI maps have accurately captured this sharp change (refer to Figure 7d in the revised version). In light of your feedback, we have provided a thorough analysis of this decline in the discussion section (see L417-426 in the main text for details).

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