We thank the reviewer again for the detailed and constructive comments that help improve our manuscript greatly in several aspects.

RC1: *The quality of the revision is not satisfactory. I recommend major revision rather than rejection mainly because of the relatively strong yearly correlation between NDVI and GRACE TWS (R^2~0.59), which if carefully analyzed has the potential to lead to informative conclusion regarding regional ecohydrology in a humid area. Claiming water limitation in such a humid area needs caution. The Pearl River basin features strong rainfall that is generally higher than the potential evapotranspiration (i.e., rainfall outweighs water demand) and the river runoff is large, too. The claim about water limitation therefore needs justification and explanation. When and where exactly does water limitation come from, is there a period in each year when the water supply cannot meet the demand of plant growth? The authors have multiple datasets (both water supply and demand) available to answer these questions.*

AC1: We have adopted the advice of using EVI and SIF instead of NDVI for the analysis, and therefore, rewrote some parts of the manuscript in each section. Discussion has also been rearranged with the uncertainty moved to Methods section. With some thoughts and trials, we decided not to adopt the suggestion of using the entire study period as the baseline to calculate the anomalies though, and reasons are given in the response to that specific comment. We want the reviewer to know that we take the comments and suggestions with gratefulness, and carefully addressed them to the best of our knowledge.

The critical questions mentioned above are important in this study and have been carefully addressed where possible with support from the data and stated in the relevant sections in Results and Discussion. The annual water availability is relatively high in this monsoonal humid subtropical basin, but this does not guarantee no water stress to plants due to the obvious seasonality of both water availability and vegetation growth. Our best guess is that the water stress could be a result of long growing seasons since majority of the forests are evergreen and the crops are planted on rotations throughout the year. Irrigation water needs to be added in croplands to supplement water supply in addition to rainfall during dry seasons. In addition, radiation energy is intermittently available to plants because of the cloudy/rainy conditions during the growing seasons and its periodic increases will improve vegetation productivity. Overall, using EVI, SIF and GPP for analysis instead of NDVI, we found the water supply did not influence greenness as much as productivity, supported by the long-term mean monthly data in Fig. 8c-d. Please refer to the revised m/s.

RC2: *I suggest the authors use EVI and SIF both of which are easily available. The authors' argument is problematic as most of the significant trends in NDVI occur in the central portion of the study domain where notable forest cover exists (Fig. 1c & 4b). Using EVI can avoid the saturation issue associated with NDVI. In addition, the adopted GPP products may not represent well the soil moisture constraint. Using SIF will provide an observational metric that represents GPP.*

AC2: While EVI can avoid the light saturation problems, taking the advice, we use EVI, GPP and SIF datasets in the analysis in this version. MODIS EVI shows difference in dynamics compared to GIMMS NDVI, showing better concurrency with GPP and SIF overall. Because of the GOSIF algorithm and input data (OCO-2, MODIS, etc.), it has high correlation with MODIS EVI (R^2=0.95), which leads to similar relationships between hydroclimate and EVI and SIF.

Using the new datasets in the analysis and taking out the analysis with NDVI, we have updated the results and discussion accordingly. We think the different vegetation indices are worth a sophisticated comparison regarding phenological indication and ecohydrological applications, and we found some papers have done some work on it. We will pursue farther studies in separate works on this matter.

RC3: *Line 20. The authors conclude that "the degree of water restriction on vegetation was higher than that of water consumption by vegetation even in this rain-abundant region." It is still unclear to me how water restriction (which is quantified here by correlation) can be directly compared with plant water consumption (which is often quantified by transpiration).*

AC3: This statement was a bit confusing and may not be expressed properly by 'water restriction' and 'water consumption'. We have rewritten it.

This conclusion originates in the assumption that if vegetation parameters' series change ahead of water supply (e.g., TWS) then the negative impacts of vegetation on water supply outweigh the positive impacts of water on vegetation growth, and vice versa. From the analysis we observed the leading role of water supply variations rather than vegetation parameters, based on which we concluded that water supply leads vegetation development, and that vegetation is not dominant in reducing water availability. Therefore, it is more of a qualitive conclusion. Using plant transpiration to compare with either P or TWS can show the proportion of water consumption by vegetation, and the temporal variations can demonstrate which one leads the changes. As in Kirchner et al., (2020) and Deutscher et al., (2016) who showed the diurnal variations of sap flow, groundwater and streamflow and attributed hydrologic variations to plant water use. The idea in this study follows their conclusion in that way. We hope the discussion part makes sense to you.

Kirchner, J., Godsey, S., Osterhuber, R., McConnell, J. and Penna, D.: The pulse of a montane ecosystem: coupled daily cycles in solar flux, snowmelt, transpiration, groundwater, and streamflow at Sagehen and Independence Creeks, Sierra Nevada, USA, Hydrol. Earth Syst. Sci. Discuss., 1–46

Deutscher, J., Kupec, P., Dundek, P., Holík, L., Machala, M. and Urban, J.: Diurnal dynamics of streamflow in an upland forested micro-watershed during short precipitation-free periods is altered by tree sap flow, Hydrol. Process., 30(13), 2042–2049

RC4: *Line 118-119. The description about GRACE uncertainty is not accurate. The leakage error depends on the specific basin of interest and needs to be calculated case by case, and I don't think they are released by the processing centers. Wiese et al 2016 documented the recommended procedure to estimate the leakage error (although the values provided in that paper were for RL05). How did the authors quantify the leakage error for the Pearl River basin? Also note that the mascon uncertainties provided by JPL are uncertainties associated with each mascon estimate (not the uncertainty associated with a single 0.5 degree pixel). Based on the short description provided by the authors, it is difficult to assess whether the GRACE uncertainty is treated properly.*

AC4: We totally agree it is important to estimate data uncertainties for any quantitative studies. The JPL provides both the measurement errors and leakage errors for the 1-degree GRACE data. Save et al., (2016) stated that quantifying leakage errors does not impact CSR mascon solutions as much as it affects JPL mascon estimate due to the native estimation resolution of 1° for CSR mascons versus 3° for JPL mascons. Anyway, unluckily no such error attributes are provided in the 0.5-degree GRACE data based on the mascons solutions.

We do not intend to estimate leakage errors following Wiese et al., 2016 method, because we primarily used TWSA to infer water availability changes, so it is the dynamics and trends, not the absolute values, that really matter. In the case of water balance studies or other quantitative hydrologic studies such as using it to verify water storage simulation results, it is very necessary to estimate the data errors. That is not the case of this study.

Here, we simply used the standard deviation of the time series to represent the uncertainty of the data, i.e. the standard deviation of TWSA in each year represents the spatial variability of TWSA in that year across the basin. We have made our point regarding the data uncertainty more clearly in the Methods section to avoid further confusion and argument.

Save, Bettadpur, and Tapley, 2016. High-resolution CSR GRACE RL05 mascons, JGR-Solid Earth, 121 (10): 7547-7569

RC5: *Line 175-176 and Fig 2a-c. As I pointed out in my previous comments, the TWSA depends on the choice of the reference period. The common practice is to use the entire analyzed period to represent the "normal" condition. I suggest the authors do the same because using the 2004-2009 as the reference period is not the best effort practice when longer baseline exists, unless the authors have specific reasons to believe the 2004-2009 period better represents the normal condition. This would potentially affect Fig 2a-c in the manuscript and also the classification of dry and wet years in Figs 9 and 10. The authors' response to my original comment stressed that the reference period wouldn't affect the trend analysis, which was obvious and not relevant to my comment.*

AC5: There are two ways of processing other data series to comply with GRACE data. One is to use 2004-2009 as the baseline period suggested by JPL Tellus, and the other is to use the entire study period as suggested. We adopted the first one.

We tried with the entire period (2002.04-2015.03). We found firstly that the dynamics (highs and lows) and the linear trend of the newly calculated 'anomaly' are the same with that using 2004-2009; and secondly, the mean value of the new annual 'anomaly' for each pixel is nearly zero when we used the already anomaly data provided by JPL and CSR to subtract their means. This alters the original concept of data anomaly, and it is more of the anomaly of anomaly. Similar studies commonly use the level-3 anomaly products. If we had the absolute mass field, then we can calculate anomaly regarding any baseline period, because it then would be essentially just a matter of different subtractors. But the time-mean fields that are removed in the processing represent essentially the Earth's mean gravity field. As such, there isn't really a 'time-mean mass field', particularly not in terms of average water storage height (https://grace.jpl.nasa.gov/about/faq/).

Using a different baseline does not affect the identification of dry and wet years because we considered the annual variations of TWSA, vegetation index as well aridity index when doing this. It will change the TWSA magnitude little since the period 04/2002-03/2015 is not significantly longer than 01/2004-12/2009. However, the pixelwise mean annual newly calculated anomaly is close to zero, which is not helpful in detecting the hotspot of water availability changes.

For the reasons above, we kept the 2004-2009 baseline period to calculate the anomalies of other data series.

RC6: *Figure 7. It is not appropriate to calculate correlation with TWS at pixel level as the TWS resolution is intrinsically coarse (~ 3 degree) and going beyond the mascon resolution will rely on scaling factors that are not based on pure observations. This should be either noted or avoided. The basin scale results shown in Fig 6 are more appropriate.*

AC6: Thanks for pointing this out. We agree. A few sentences regarding this matter have been added in the first paragraph of section 3.1 and 3.3. We keep it mainly because it can show that the significant relationships mostly exist in the central cropland areas.

RC7: *Line 226. As I asked in my previous comment, please clarify if the seasonality has been removed when calculating correlation using the monthly time series. Note that to quantify water limitation, the seasonality should be removed from the monthly time series.*

AC7: Description has been made clearer in the Data and Methods section 2.4. We have firstly removed the linear trend from the data, and then removed the seasonality by subtracting the seasonal signals (i.e. the monthly climatological means) from the detrended data.

RC8: *Figure 8. Is each point corresponding to a pixel? Note that this is not appropriate for TWS. See my comment as above.*

AC8: Each point represents a monthly data averaged over the entire basin, not each pixel. We have made it clear in the caption and the text related with the figure.

RC9: *Line 188-190. It is difficult to infer causality between two variables simply based on temporal lag. The drop in NDVI occurs after Nov. Is this period also part of the growing season? Is there any energy limitation? When the NDVI starts to decline, does water demand outweigh water supply?*

AC9: Please refer to the response to comment RC3 and RC2 above. Monthly climatological mean EVI shows different characteristics from NDVI in a way that it synchronizes better with other vegetation parameters and water components. Some of the phenomena mentioned is gone.

RC10: Figure 9c-d. The range of the right y-axis is way too large for AI and NDVI. -0.5 to 0.5 is likely more appropriate.

AC10: Thanks for the advice. We have modified the y-axis range to make the variations more visible. Now it is within the range of -0.4 to 0.5.

RC11: *Line 240. If the difference in NDVI between the dry and wet years is mainly attributed to the difference in the non-growing season, does that mean the growing season NDVI does not show significant difference between dry and wet years? That would indicate that water is not a limiting factor to growth in the study domain.*

AC11: With EVI in the analysis, the difference between dry and wet years is not as obvious as indicated by NDVI, although the EVI anomaly is still slightly lower in dry years than in wet years in both growing and non-growing seasons.

Previously, analysis with NDVI indicates that the difference lies primarily in non-growing seasons between dry and wet years, while the difference is not so obvious in growing seasons. This does not infer that vegetation is not limited by water, and the main reason could be that in dry years irrigation water is often supplemented to secure food production in the croplands as we argued. The current analysis did not account for the additional water supply but only the natural precipitation and water storage.

In this revision, we have carefully rewritten the relevant part in the results and discussion to make sure them consistent.

RC12: *Line 246-247. That seems contradictory to the notion of water limitation.*

AC12: This can be partly referred to the response to the comment RC3. After a second thought, we feel the expression should be improved in the revision. Our intention was to state that under dry conditions (with less precipitation/storage), water surplus by irrigation plays an important role in boosting plant production.

RC13: *Line 248 vs. Fig 10. Is it "monthly climatological mean" (Line 248) or is it "monthly anomalies" (Fig 10 caption)?*

AC13: After doublechecking the code, we confirm that it should be monthly climatological mean of the data anomaly. The caption has been corrected. Thanks!

RC14: *Section 4.1 As I pointed out in the original comment, this section should go to the Method (some of them should go to introduction, e.g., Line 259-261, 272-280). The discussion should focus on the scientific hypothesis that the authors aim to address. I believe the other reviewer pointed out the same issue as I did.*

AC14: We have now moved this part to Methods and Introduction where appropriate. By doing that, we edited the Data and Methods section to avoid repetition and for a better logic of flow.

RC15: *Line 314. The reason that I pointed out Tong et al., 2018 in the original comment is to remind the authors that there is an active human intervention in the study (with respect to land use and land cover change), which would very likely complicate the explanation of plant-water relation – in this case, the increase in growth is partly attributed to human management rather than plant response to water supply changes.*

AC15: Yes, in this version of discussion, we also directed our findings to human intervention in section 4.1 corresponding to your kind reminder. Presumably, these interventions are mainly the irrigation activities and possible planting structure adjustment during dry periods. Thus, the plant-water relationships might not be under purely natural conditions but also exposed to anthropogenic influences.