We would like to sincerely thank the three reviewers for their valuable comments. These comments have help improve the manuscript and clarify the focus of the paper. Responses to the reviewers' comments are shown below each comment.

Reviewer #1:

The present analysis of the post period is inherently constrained by two factors: (i) the two records are not well comparable because of unequal temporal division and lack of data in the post period, (ii) the considerable impacts of dams, if any, is most likely in the last 10 years of data. Furthermore, these two factors are not independent from each other, which further complicate a straightforward analysis and description of the results. Therefore, a proper analysis of these two factors is essential in order to put the results in a proper context.

Response:

(i) Similar results for all parameters were obtained when the analysis was done between two equal periods (the pre dam period of 1971-1990 and the post period of 1991-2010). The period 1960-1970 was included in the analysis as the data was available for that period, but does not affect overall results/conclusions on the analysis of water levels. The analysis was also run using 1985, 1986… 1994, 1995 as the division of pre and post years and each shows little difference in the overall results. The division at 1991 was selected as it reflects a key point in history in the development of dams and water infrastructure in the region.

(ii) Indeed, the greatest changes in dam development have occurred in the last 10 years. The implications of this can clearly be seen in the water level fluctuation results when plotted on a yearly basis for all stations analysed (Figure 5). Our summary statistical results when comparing pre- and post- 1991 water level can be considered conservative in this regard. However, we believe that we have achieved the right balance (in terms of quantity of data and verifiable level of changes) for providing evidence for water level changes with respect to dry season water levels, water level fluctuations, and water level rise/fall rates. In addition to dam development, it is also important to emphasize that infrastructure (irrigation) development in the Chi-Mun basin, which developed rapidly during the post 1991 period, is of importance in the alternation of key water level indicators (fluctuations and decrease in rise rate).

We have added text to the manuscript to ensure better clarity in the understanding of our data analysis and focus.

At the moment analysis on climate is missing. Authors give section 4.2 where issue of climate variability and change is discussed from previous studies, but, this section does not give any clear explanation of the presented results. Therefore, it is recommended that authors should conduct an analysis of climate for the corresponding study period (1960-2010 including an analysis of pre-and-post 1991 periods). Ideally this should be done using areal precipitation for each catchment upstream of the studied gauges. However, given the limitations, at least, this should be done for few climatic stations across the Mekong basin where long-term records are available. This analysis should at least cover monthly precipitation, if possible other parameters like peak rainfall, dry spells etc.

Response: The reviewer suggests doing climatic analysis to explain wet season changes. Most of these analyses have been done in the past by others and their findings do not conflict with our findings. This is now better clarified in the manuscript text and we have added more description of previous climatic studies. Wet season flows and levels (and mean yearly values as well, because mean flows are impacted by the magnitudes of wet season flows) have been related to climatic changes in various studies. In doing our study we

identify and focused on water level indicators which are unlikely to have been impacted by historical climate variability, but which can be directly related to infrastructure development. We provided a summary of over 30 parameters (Table 3) for Chiang Sean as an overview/example of all the analysis done. We do not argue that all these parameters are affected by infrastructure only. What stands out is the significant difference in dry season levels, fluctuations, and fall rates which can be explained by infrastructure changes. Our paper then quantifies these changes for key stations along the Mekong River.

The analysis on temperature records will shed light on snow flow and snow melt processes as well as changes in the evapotranspiration.

Response: We have added references to studies by Cook et al. (2012) on the issue of snow melt. Their study concludes that contemporary and future changes in the lower Mekong flows between March and May are negligible as a result of the conflicting effect of melting snow cover and increasing local precipitation.

Monthly precipitation assessment for the pre-and-post 1991 period is inevitable to explain the results given in Table 3 and Figure 3. For instance, mean monthly flows in the post period (1991-2010) are consistently higher for all months compared to pre-period (1960-90). This is not expected result of regulation by dams as well as abstractions for the irrigation. Similarly, high flow (1, 3, 7, 30 and 90 days) values for the post-period (1991-2010) are higher than pre-period (1960-90), which is also not expected with reservoir operations alone.

Response: Table 3 represents the analysis results from the upmost station (Chiang Sean) only, and it wasn't our intent to imply that all observed changes are from hydropower. Climate variation is likely responsible for some of these changes, particularly the wet season ones as reported by others who studied climate variability. It is not our intention to repeat those studies, but they have now been reported in greater detail in our manuscript. However, it is also important to note that previous studies at this location (Lu et al 2014, Li and He, 2008, etc.) have concluded that dams are the main factor in impacting dry season water levels and fluctuations. Furthermore, the tables shows that mean level increases (magnitude) are not consistently higher for all months. The analysis shows that mean monthly levels are not always large and significant for the wet months. The wet season months of July – Sept have values with low significance. Figure 3 also shows that there is very little variation in wet season months.

Authors have attempted to explain increase in dry season flow attributed to dams, which seems logical. But the discussion on increase in wet season flows (monthly and floods) is missing. This warrants proper investigation of climate for both periods. It is likely that period 1991-2010 is wet compared to 1960-90, which resulted in increase in mean monthly flows during wet season as well as high flows. This is an important aspect, which should be properly analyzed and included in this paper in order to substantiate or modify the conclusions on the hydrological alterations in the Mekong mainstream and the Tonle Sap system.

Our focus is on explaining the changes in water levels that result from water infrastructure development. We agree that wet seasonal flows are less influenced by water management, but can be related to climatic change as has been shown in previous papers on this topic (a more detailed review/discussion of these has now been included). Explaining climate changes in wet season flows is not the focus of our paper, but we agree it could be studied in greater detail by others. It is undeniable that the current effect of water infrastructure on wet season levels would be minimal and this is more likely to be as a result of climatic variability.

Another major comment is related to the storage capacity and flow regime. Authors should explain in the results section 3.1 how the increase in storage capacity relates to the flow regime. For instance, by estimating mean annual flows from Table 1 (this column could be added to Table 1) and then comparing it with the storage capacity upstream of these gauges, it is evident that the storage is very less, especially before 2005, compared to the flow volumes. Only during 2006-2010, the substantial increase in the storage capacity could be noted from Table 2. Another important factor is to discuss the residence time of water in the dams. The operational strategies of the dams should also be discussed, especially in relation to meeting peak electricity demand, which might have high influence on altering the flow regime (e.g. rise/fall rates, reversals).

Response: Given the large wet season flows, annual total flow volume values are significantly larger than active or total storage capacity of the dams and would shed little light into the links. Releases of flows during the dry season, however, are important in terms of dry season water levels, as discussed in the results and discussion sections. Unfortunately there is limited data available on operation strategies of individual dams as this information is often kept confidential. If we were analysing 1 dam, as in the study of the Yali Falls dam in the Sesan River, a direct cause and effect of flows/water level to electricity generation and reservoir operation would be possible, but in a large scale study like this, with multiple dam and irrigation operations, it would be difficult to find a direct cause and effect. In general the peak electricity demand would be dictated by requirements in large urban areas throughout Southeast Asia. Detailed hydropower and hydraulic modelling of the system could be attempted to pin point cause and effect of individual operations.

Minor Comments:
In the introduction, several studies are referred and many of them with similar subject. Authors should briefly review some of them and clarify the limitations of the related studies and what additional contribution this paper is making.
Response: All relevant studies have been references and discussed. Greater emphasis has been placed on studies related to climatic variation.

Authors attempt to link the studied IHA indicators to river ecology and fisheries. But these statements are not well elaborated. Please consider improving these parts.

Response: We have added some additional explanation of the relationships between the water level indicators and river ecology in the introduction and discussion. We have also included a reference to a study conducted on ecological and social impacts at the Yali Falls dam on the Sesan river.

The conclusions may be different after addressing the review comments. In either case, please consider adding quantitative figures on the key indicators. At the moment only percentage changes are given for some indicators. It would be valuable if changes in water levels are also indicated (e.g. in centimeters). This can also be done in the results and discussion section.

Response: Numerical values for the key parameters have been provided in the tables and we have also now added some additional values to the discussion.

As indicated previously, please consider adding a column in Table 1 which gives the mean annual flow for each of the study gauge (e.g. in Billion cubic meters, BCM). It seems that Table 2 is not correct for LP and VT. Please re-check the given storage values.

Response: Mean annual flows are already provided in Table 1 and can be provided as total flow volume, but as mentioned previously it may not provide the intended comparative effect.

Values in Table 2 between LP and VT are correct.

In Table 4, percentage values could be rounded to integer values.

Response: Percent values in Table 4 have now been rounded to integer values.

Reviewer #2:

This paper intends to assess the effect of recent hydropower dam and irrigation developments on the water levels of the Mekong and Tonle Sap rivers. In addition to the issues raised by the first referee (Ilyas Masih), I have two additional criticisms:

A/ In the context of the Mekong River, water level variations do not necessarily reflect upstream hydrological changes and thus, cannot be used alone to evidence and quantify the magnitude and statistical significance of hydrological changes.
This analysis primarily uses water level data of the Mekong mainstream to investigate hydrological impacts. Using water level instead of discharge data is tempting because water levels records of the Mekong River are of good quality, compared to flow data: daily time series are virtually uninterrupted since automatic water level recording stations were installed in the early 1960s. In contrast, discharge values are missing for many years at several stations and often include errors which are difficult to detect, quantify and correct. The main source of uncertainty in discharge values originate from the rating curve (stage – discharge relationship) which used to be updated every year at each gauging station to account for possible changes in the river cross section (because of sedimentation and/or erosion). Where the cross section is stable (rocky section), flow time series can be confidently estimated from water levels over multiyear periods using one single rating curve. However, at some gauging stations of the Mekong River, especially in the downstream part of the Basin (e.g. Mukdahan), sandy banks continuously modify the relationship between water levels and discharge. In this situation, flow estimation using outdated rating curves may lead to non-negligible bias.
I would therefore recommend the authors to carefully assess possible bias caused by changes in river cross section when attempting to detect hydrological changes using water level data. Slights changes in the river cross section can accentuate or moderated water level variations caused by upstream catchment modifications. One option could consist in comparing the rating curves before and after 1991 at each station. For example, the authors could plot the measured discharges corresponding to each water level (1m, 2m, 3m, etc. . .) against time (i.e. 1 curve per water level), using the rating curve data available in the MRC hydrological year books and maybe in the MRC database. Another informative graphic could include one “average” rating curve (discharge on Y axis and water level on X axis) for the period before 1991 and another for the period after 1991, with the multi-year variability depicted by standard deviations at each plot.

Response: As the reviewer states, the reliability of water level data is much higher than flows (because of potential variability in measuring velocity and cross sections) and thus water levels were used for the analysis. The focus of our study has been on identifying those parameters that can be linked to water infrastructure (fluctuations, rise/fall rates, and seasonal levels). The impact of cross-sectional changes to the analysis of the difference between pre and post 1991 in fluctuations, rise/fall rates, and dry season values is negligible. Based on the cross-sectional data we have at the stations, the changes in those cross-sections cannot explain the changes in fluctuations, rise/fall rates, and dry season levels. An analysis of rating curves for estimating flow rates, although interesting, would be out of the scope of this paper.

B/ Due to the limited hydrological contribution of the Chi-Mun Basin to the Mekong River, water infrastructure developments in this sub-basin are not likely to have significantly altered the mainstream flow regime.

The authors indicate that hydrological alterations at Pakse can be attributed to water infrastructure development in the Chi-Mun basin, including hydropower and irrigation development. This explanation is not supported by rigorous scientific demonstration. One impediment to this hypothesis is the relatively low flow contribution of the Chi-Mun Basin to the Mekong River. Using the numbers provided in the manuscript (page 4410, line 24: average annual flow of 32,280 Mm3 in the Chi-Mun Basin; page 4424, Table 1: mean annual flow of 9,700 m3/s in the Mekong River at Pakse), it can be estimated that the relative contribution of the Chi-Mun Basin to the Mekong flow is about 10%. This low percentage indicates that possible hydrological alterations in the Chi-Mun Basin are considerably attenuated downstream of the confluence with the Mekong River. In addition, the reservoirs that were built in the Chi Mun Basin have most likely very little influence on the river flow regime at the basin outlet for two reasons: there are either located in the headwater catchment areas with relatively low drainage areas or have very little active storage capacity: the storage capacity of Pak Mun dam, 225 Mm3, is equivalent to less than 1% of the Chi-Mun basin water yield. Another problem is: page 4410, line 17: “Seventeen out of the 39 dams in the Mekong basin became operational between 2006 and 2010”. This 5-year period is very short compared to the duration of the second tested period of 20 years. The possible effect of irrigation development on the Chi-Mun Basin hydrology should also be assessed with more accuracy. Page 4410, line 27: “the irrigated area is close to 1,266,000 ha with an annual water demand of 8,963 Mm3”. This water demand is equivalent to less than 3% of the Mekong flow at Pakse and not expected to have a significant effect on the Mekong mainstream water levels. However, the authors should verify the figures for the dry season only when the Mekong flow reduces and the irrigation water demand may increase. Page 4415, line 9: “during reservoir flood control operations” this explanation is not ascertained. Need for more references/evidences. Line 18-19 “thus, an increase of this indicator in such a large river is most likely a direct function of reservoir fill and release operations”. This is speculative again.

Response: Although the Chi-Mun subbasin only contributes 5-10% of the total Mekong’s discharge at Pakse (MRC, 2005), it is not the quantity of water but rather the intensity and frequency of water management operations that is reflected in the large increase of water fluctuations at Pakse. Section 4 (Discussion) has been improved to substantiate and address the perceived issues described by the reviewer. Furthermore, we have clarified in the manuscript that the analysis reported is not all about flows, but rather about dry season levels, water level fluctuations, and water level rise/fall rates. We have also provided a figure to explain these key indicators and added references to other studies which demonstrate the impact of dams and irrigation schemes on downstream water levels. In terms of irrigation management and operations, we have made it clear that the irrigation occurs (as expected) in the growing (dry) season and that given the type of irrigation and agriculture that occurs in the basin (largely flooded rice), it is both plausible and likely that the management will have an effect on water level fluctuations and rise rates downstream in the Mekong at Pakse.

Minor comments:
Page 4413, Lines 4-6 at the beginning of section 3.5 and lines 17-19 at the beginning of section 3.6 should be included in the method section, not in the discussion.
Response: We believe this adds clarity to the discussion of results and thus it has been kept.

Page 4414, line 4: “Fig 4” instead of “Fig 5”.
Response: This has been addressed.

Page 4416, line 26: Kummu et al. 2014 is not in the reference list.
Response: This has been addressed.

Page 4424, Table 1: it would be interesting to add in this table the % of area ratio and dry season flow contribution to total flow.
Response: Percentage values of the area ratio and dry season flows (and wet) have been added to the table.

Page 4426. It would be clearer to put the values of table 3 in a graph.
Response: The results for this table are only for Chiang Sean and a graph made out of this table would have too much data. Additionally, relevant data of water levels in the wet season and dry season for Chiang Sean have already been graphed in a Figure.

Page 4430, Fig 3. It would be useful to provide the p-value of the statistical test (that is used to assess the significant of the change between the two periods) for each month of the year. Using the same scale on the Y axis for the 3 graphs would allow an easier comparison of the graphs.
Response: Significant levels are shown in Table 4. The figure was changed to use the same scale for all graphs. Adding other statistical values (apart from the standard deviation values already there) will complicate the graph.

Reviewer #3:
The paper aims to quantify river water level changes in the Mekong caused by hydropower dam and irrigation development by analysing indicators calculated from the water level data from seven gauging stations over the period 1960-2010. The impacts of the water resources development in the Mekong have been poorly documented and published and therefore this paper can provide valuable information on those impacts. The findings of the paper can benefit Mekong region as well as other large river basins of the world. The paper is in the scope of HESS and it is well written, but needs to be revised before publication, especially regarding the potential effects of climate on the findings. The detailed comments are given below.

Major comments:
I agree with Masih I. (Referee) that the influence of climate needs to be addressed better in the paper. Currently it is not possible to exclude adequately the effects of climate on the findings. For example, Mekong's mean hydrological conditions have varied during the study period of the paper and the effects of this on the results have not been properly excluded. In particular, the mean water level comparisons in Table 3, Fig. 2, Fig. 3, are most likely affected by the changes in mean hydrological conditions. In addition, the lack of evidence of climate induced hydrological alterations does not mean that there has not been climate induced hydrological alterations, contrary to what the paper implies on page 4417, line 27. The lack of evidence in this case is largely due to lack of research. Especially the variability in precipitation patterns and intensities in the Mekong is not well researched. However, the authors are strongly encouraged to resolve this issue as the paper contains valuable information.

Response: As discussed previously, a more thorough and clear background and discussion on climate change has been incorporated in the paper. Additional references and discussion on the climate issue have been added to clarify the point being made by this paper.

Minor comments:
The paper should link more clearly the observed water level variations to the actual hydropower and irrigation operations. The current discussion in Section 4 does not give clear overall picture of the hydropower and irrigation operations. It is suggested that the hydropower and irrigation operations are introduced in detail before presenting and discussing the results. Please use references when introducing the operations as much as

possible. In this way the findings can be discussed in more structured way and linked better to the actual hydropower and irrigation operations. I believe this approach could also help in excluding the effects of climate on the findings, if not fully at least partially.

Response: Section 4 has been re-structured to clarify the discussion on the key parameters related to hydropower and irrigation operations. We have also included reference to other studies (in the Introduction and discussion) who have also shown specific impacts which are directly related to changes in specific water level indictors.

The literature review on the observed and estimated impacts of hydropower and irrigation operations on the Mekong's water levels/flows could be improved. This comment links to the first minor comment. First, it is suggested that the findings in the literature that are related to the findings of this paper are discussed in more detail. Second, the literature review is missing papers that are relevant and potentially useful in discussing the findings (e.g. Lu et al. In Press, Observed changes in the water flow at Chiang
Saen in the lower Mekong: Impacts of Chinese dams?, Quaternary International; Wyatt and Baird 2007, Transboundary Impact Assessment in the Sesan River Basin: The Case of the Yali Falls, Water Resources Development). Please see if there are also other relevant papers that could be included into the literature review.

Response: Additional references have been added to the introduction and discussion, in particular Lu et al. 2014 and Wyatt and Baird 2007.

The paper would benefit from a figure presenting the Mekong's annual flow/water level regime that illustrates also the most important hydrological indicators. The paper addresses various types of water level variations from various operational sources and currently it is challenging for the reader to keep track of all variations.

Response: A figure describing the key water level indicators related to hydropower and irrigation operations has been included.

Could you reflect how your findings on water level alterations agree with model based estimates done in the Mekong? Various modelling applications have estimated the future river flow changes caused by hydropower and irrigation, but so far no published comparison has been done between the model estimates and the observations. The model estimates could also potentially support your findings. This comment can be considered as a suggestion.

Response: comments on modelling have been added to the discussion and conclusions.

Technical corrections:
- Abstract is relatively long. Please consider shortening it.

Response: The abstract is only 365 words long and although we have only made minor reductions we believe it reads well.

- Please take care that the font sizes in figures are large enough.
- In Table 4 the percentage change for 7-day minimum mean at Prek Kdam is missing +/- sign.

Response: The +/- signs were added to all values. Figure text was checked for size.

- The discussion section starts with justification for the importance of analysing alterations in water level fluctuation. This is partially repetition from the introduction section. Please consider merging it with introduction.

Response: As stated previously, we restructured the discussion and the first part of the discussion provides a basis for that discussion.

- Figure 4 presents the findings in more easy and comprehensive way than Table 4. Would you like to consider presenting the other results in figures as well?

Response: We have added another figure to the manuscript and Table 4 is now discussed in greater detail in the discussion section.

Response: All technical corrections have been addressed as appropriate in the manuscript.