Response to Anonymous Referee #1
The paper, "A web-based software tool to estimate unregulated daily streamflow at ungauged rivers" seeks to present "the first publically-available, map-based regional software tool to interactively estimate daily streamflow time series at any user-selected ungauged river location." The paper is well-written and relevant to the readership base of the journal. I generally support publication of this manuscript subject to my general and specific comments listed below.

Thank you for your comments. We found that your suggestions greatly improved the clarity of the manuscript and further aid in the understanding of the methods and software tool. We have revised the manuscript substantially to address these concerns, including adding new sections to the text, expanding existing sections and revising the figures to provide more detail and clarity. Responses to your individual comments are found below.

General Comments:

1. The manuscript title states that the tool presents a way to estimate "unregulated" daily streamflow. This could be interpreted in two ways: (1) in a given record that has been subject to some regulation (such as downstream of a dam) the tool can remove some aspects of this regulation (2) the tool is only valid in unregulated basins, since it cannot remove regulations. I believe #2 is what was intended. In general this point should be clarified, though, to avoid confusion. I would even go so far as to suggest removing "unregulated" from the title to avoid confusion.

The reviewer is correct that #2 was intended. We clarified this point throughout the manuscript, including the abstract, introduction, and conclusion sections by adding the phrase "streamflow not affected by human alteration such as dams or water withdrawals" when the term "unregulated" was used in the text.

2. In methods such as this, does one need to consider routing? Let's assume that we are working in a basin that is ungauged to a large degree, and the only index basin is far upstream of the ungauged point. Ostensibly there would be a timing lag between the index point and the ungauged point. If the authors' proposed method is invalid in such situations, a comment should be made to this effect.

This is an excellent point. The catchments were selected to be generally small enough that routing is not an issue, although this is a limitation of the methods. We have added a new Discussion section the manuscript to discuss the limitations of the method and we include a statement about routing here.

3. From a stylistic perspective, this manuscript assumes that the reader has very good knowledge of statistical hydrology and some of the sub-methods employed in the framework. When discussing the modeling components, the authors use statements such as, "Method A was chosen, it was shown to perform better than B and C." Much of these methods have been published previously, though, so the justifications (comparisons to B and C) may not be necessary. Instead, I'd like to see some more

description of the conceptual basis behind the techniques, especially the local regression to estimate the middle of the quartiles, for example. This is important for readers to understand, given that the framework will do these calculations for them and they will not have to do the calculations on their own. Additionally, more explanation would allow the manuscript to have a greater appeal to those readers who may not be intimately familiar with statistical hydrology but need to use the results, such as water utilities or managers. In general there are non-hydrologists in the readership base of this journal so it's a relevant issue. There are some additional specific comments relating to this point below.

We believe this comment greatly improved the clarity of the manuscript and agree that there was not enough detail in the manuscript. We have since substantially expanded Section 2.1, which describes the methods and rationale for how the flow-duration curve was estimated. We also added a new section (now named Section 3.2) to describe precisely how these methods were implemented for the regional tool.

4. As far as I can tell, the main problem to be solved is to provide ungauged streamflows for a specific period in the historical record at a specific site. This explicitly assumes stationarity in the record. I could think of three other relevant problems that hydrologists or water managers may be interested in: (1) calculation of streamflows in which there is a trend or changing moments, (2) hydrologic forecasting (of future periods) at an ungauged site, or (3) generation of synthetic streamflows that match the statistics of the historical record. I realize that these three applications are out of the scope of this paper. However, I think the use of StreamStats for delineating a catchment, as shown in Figure 1, the resampling of historical low-flows, the area-weighted method, and other components here could help solve problems (1)-(3). To increase the impact of the manuscript, I really think there should be some discussion of this (i.e., the applicability of some of these concepts to broader water management problems).

We agree with this point and have added additional text to the new Discussion section to address these points.

5. There is a lot of great information in the figures, but I have two comments regarding their presentation. (1) they are probably way too small, especially figures 3-4. It was very hard to read the text in the figures, and I'm not sure how big they will be in the final manuscript. (2) When discussing the methods, the authors refer to the figures without walking through and explaining each panel. Especially regarding figure 1, it seems like the treatment of text around the figure is incomplete and should be revised. Further specific comments along these lines will be provided below.

We agree with these comments. (1) We have broken figure 3 into two figures so that the details of the text can be shown. We also added additional descriptive text to these figures to highlight the functionality of the software tool. Figure 4 has been reconfigured so that the hydrographs are much larger and easier to read. (2) We have completely reworked Figure 1 to show more details of the estimation process both visually and as text in the figure and in the figure caption. We

have also modified the text in Section 2 to correspond the related sub-sections of the text as well as to the new Figure 1.

6. I'm not sure what the policy of this journal is, but there should be a more formal software disclosure section that tells readers where they can download a copy for themselves. The material is already in the manuscript, but it would be nice to reiterate it at the end. Also, is there a general version of the code or was the model only developed for Connecticut? This seems like a disparity from the title/abstract and the case study treatment later in the manuscript. In fact the authors mention it is a basin-specific tool [page 2512, line 11].

We agree with this comment. We have added additional text to the software tool section (Section 3) regarding the development of a similar tool as well as added text to the new Discussion section about the development of such a tool outside of the Connecticut study area. Specifically, the text added in Section 3 reads:

"The software tool can be considered a general framework to provide daily streamflow time series at ungauged locations in other regions of the United States and possibly other areas. Furthermore, all data and methods underlying tool are freely available. Whereas the tool is a general framework for providing a map-based, "point-and-click" approach to estimate daily streamflow at an ungauged river location of interest, the underlying data, including the river network and catchment characteristics, are specific to the region of interest. Much like other modeling frameworks, the software tool must be calibrated based on the data available in the region of interest. Details of the functionality of the regional tool presented in this study follow. Additional details on the customization of the catchment delineation for application to other regions is discussed in Section 4."

Specific Comments:
1. [page 2504, line 12] I don't think "efficiency" should be discussed in the abstract without any specific mention of what metric is used. I suggest removing this result from the abstract; I don't think it adds much to the discussion.

We agree and removed mention of the term in the abstract.

2. [page 2505, lines 8-11] I was confused between method (1) about rainfall-runoff modeling and (2) empirical approaches. I'm assuming #2 was intended in this manuscript, but it was unclear. This should be rewritten for clarity.

We have expanded the introduction section to be clear about the general modeling approaches that can be taken, their advantages and limitations, and which method (#2, as the reviewer correctly points out) is used in the manuscript.

3. [page 2506, lines 18-25] This paragraph was unclear. I suggest the authors introduce the steps of figure 1 explicitly. Then, they should link to the specific sections by number (i.e., "Section 2.1 discusses...").

We agree and took this suggestion. Each step of the estimation process in Section 2 is now linked to the sub-section and the new Figure 1, which has been completely reworked to show more details of the estimation process.

4. [page 2507, lines 7-10] I would like to see more discussion of how the quantiles from 0.02 to 0.85 were derived. This relates to my general comment #3 above.

We agree. We have now added the general form of the regression equation as well as additional text on how the regression equations are developed. We have also added a new section for the demonstration area (now Section 3.2) that specifically details how the regression equations were developed for the demonstration area.

5. [page 2509, lines 8-9] The authors argue that the map-correlation method is better than a method that includes drainage-area ratio, because the drainage-area ratio is already used in another part of the method. Why is this an advantage?

We agree this was unclear in the text. The point that we were attempting to make is that the map correlation is used to select the donor streamgauge and was tested for the drainage-area ratio in *Archfield and Vogel* [2010]. We also added text to discuss other methods of donor streamgauge selection, with reference. We clarified the text to reflect this:

"The donor streamgauge is used for two purposes in the streamflow estimation approach: 1) to estimate streamflows that have less than a 1-percent chance of being exceeded, and 2) to transform the estimated FDC into a time series of streamflow at the ungauged location. For the direct transfer of streamflow time series from a gauged to an ungauged location, several methods have been used to select the donor catchment. The most common method is the selection of the nearest donor catchment [*Mohamoud*, 2008; *Patil and Stieglitz*, 2012; *Shu and Ourda*, 2012]. Also recently, *Archfield and Vogel* [2010] hypothesized that the cross-correlation between concurrent streamflow time series could be an alternative metric to select the donor streamgauge. For one streamflow transfer method – the drainage area ratio – *Archfield and Vogel* [2010] showed that the selection of the donor streamgauge with the highest cross-correlation results in a substantial improvement to the estimated streamflows at the ungauged location. Using this result, *Archfield and Vogel* [2010] introduced a new method – the map correlation method – to estimate the cross-correlation between an ungauged location and a donor streamgauge."

6. [page 2510, lines 9-10] "the software tool can be considered a general framework to provide daily streamflow time series at ungauged locations in other regions". I don't understand how it logically follows that because data is available in the US, the authors' method is general. Furthermore, it looks like the software is specifically derived for Connecticut, so the point here is unclear. On the other hand, I do believe the authors have presented a general approach – it's just that the discussion here in the text is confusing.

We agree this is not clear. We have added text to the section to more precisely describe how the tool is a general framework (see response to General Comment #6) as well as how the tool is

specific to the Connecticut River Basin. We also added a paragraph to the new Discussion section to describe the data needed to customize the framework to their study region.

7. [page 2511, line 10] "The Microsoft Excel spreadsheet..." In this section the authors are talking about some components of the software as if we already know what they are, even though we are hearing about them for the first time. I recommend some explanatory text in the beginning of this section such as "Software package X consists of the StreamStats web applet and an Excel spreadsheet." Then continue with these explanations. I had to parse through this several times to understand what was really happening. Also it's really difficult to understand what's going on with Figure 3 in my copy of the manuscript, so it should probably be enlarged or broken across multiple figures (see general comment #5).

As suggested, we added more introductory comments and reorganized the section so that the components of the software are clear. We also broke figure 3 into two figures so that each portion of the software tool is more clearly visible.

8. [page 2513, line 1] "dam management": Here, as per general comment #1, it is important to distinguish between regulated vs. unregulated flows.

We agree this was not clear in the manuscript. We have clarified this issue by revising the text as follows:

"The CRB has thousands of dams along the mainstem and tributary rivers that are used for hydropower, flood control, and water supply just as the CRB is home to a number of important fish species that rely on the river for all or part of their life cycle. To understand how dam management can be optimized to meet both human and ecological needs for water, unregulated daily streamflows are needed to provide inflow time series to dams that can be routed through operation and optimization models being developed in the CRB."

9. [page 2513, line 23] "leave-one-out cross validation" Is there a citation for this method? In general, the case study is so quick here that the authors don't have a lot of time to discuss the specifics behind their method. I would be careful to explain as much as possible in the space allotted. Unfortunately part of the case study reads as if we already understand what the authors did, even though we are obviously reading it for the first time here.

We agree with this comment. We have added a new section (now Section 3.2) to the text titled "Estimation of daily streamflow in the demonstration area." This section describes how the methods introduced in Section 2 were implemented for the Connecticut River Basin.

We have also added a new paragraph to explain the leave-one-out cross validation. We reviewed several other papers that use this method and references are not given when describing the approach; we believe this is due to the common use of leave-one-out cross validation in modeling studies. Therefore, we only added additional description and did not provide a reference:

"To evaluate the utility of the underlying methods to estimate unregulated, daily streamflow at ungauged locations, a leave-one-out cross validation for 31 study streamgauges (fig. 6) was applied in conjunction with the methods described in Sections 2 and 3.2. These 31 study streamgauges were selected because they have observed streamflow covering the entire 44-yr historical period of streamflow estimated by the CRUISE tool. In the leave-one-out cross validation, each of the 31 study streamgauges was assumed to be ungauged and removed from the methods described in Sections 2 and 3.2. The methods were then reapplied without inclusion of the removed site. Using the catchment characteristics of the removed site, daily streamflow was determined and compared to the observed streamflow data at the removed streamgauge. This cross-validation procedure ensured that the comparison of observed and estimated streamflow at each of the study streamgauges represented the truly ungauged case because the streamgauge was not used in any part of the methods development. This procedure was repeated for each of the 31 validation streamgauges to obtain 31 estimated and observed streamflow time series from which to assess the performance of the study methods."

10. [Conclusion] It may be helpful to discuss some issues such as the ones presented in general comment #4 here. Right now the conclusion is a summary of the paper, but the paper was short and straightforward enough where I don't think a straight summary is warranted. I think the readers would benefit from a broader discussion of some of the key issues here, beyond what is written already.

We agree and added a new Discussion section to the manuscript to discuss the points raised here and by another reviewer, including a discussion of routing, non-stationarity, data required to extend the tool to other regions and limitations of the current methods. We did leave the conclusion section intact because the paper is now longer and, based on another reviewer's comments, we reiterated the location of the software tool in the Summary and Conclusions section.

*11. [Table 2] What is the ** in the caption? I don't see this in the table at all.*

We agree this was unclear. The ** was intended to show this catchment characteristic was not used in the regression equation for this quantile. Instead, we changed ** to '0' which indicates that the coefficient for this term in the regression was zero.

12. [Figure 1] There should be some discussion of the StreamStats tool in the caption as well. There's a gray box in the legend but it's not clear what StreamStats is, if you're only looking at the figure in isolation.

We completely redid Figure 1 so that the emphasis is on the streamflow estimation process and provided much more text and visual detail about the methods in the study. We have also changed the figure caption and text to compliment the new Figure 1.

13. [Figure 2] What do the multiple points represent in each log-streamflow plot? Multiple sites? Also there are some typos in the text; "interpolated" and "quantiles" are spelled wrong. Finally, I noticed there's a point in the FDC that does not lie on the

interpolation. What type of interpolation is used, and are there allowed to be outliers here? (In other words is the interpolation like a least squares approach where the interpolation line doesn't have to go through all the points, or a piecewise method where every point is preserved?

Thank you for carefully reviewing this figuring and pointing out these issues. We have now broken Figure 2 into two figures – the first is a more general figure used in the methods section to describe how the flow-duration curve is estimated. The second figure is placed in the new section (Section 3.2) titled "Estimation of daily streamflow in the demonstration area," which describes how the methods in Section 2 were implemented for the demonstration area. We describe in more detail in Figure 1 and in the text how the interpolation is completed. We also corrected the typos in the figure and also removed the extra point on the figure. This appeared to be an artifact of the publication process and did not belong in the figure.