

February 4th, 2015

Revision of Journal Paper

Title: “Performance and Robustness of Probabilistic River Forecasts Computed with Quantile Regression based on Multiple Independent Variables in the North Central U.S.A.”

Authors: Frauke Hoss, Paul Fischbeck

Dear Reviewer,

This letter outlines the changes we have made to our journal paper “Ten Strategies to Systematically Exploit All Options to Cope with Anthropogenic Climate Change”.

General Comments

1) The authors are apparently unaware of the first presentation of QR, which pertained to an American river, and predated the Weerts et al (2011) paper by several years. Wood et al (2009) is a citable conference presentation and is available online through the Amer. Met. Soc. (note the paper currently cites one conf. presentation). It is notable because the presentation also presents the rationale for using river rise as a predictor in QR, and demonstrates the application to operational river forecasts. This paper claims repeatedly to be the first application in an American context, which it is not given the earlier work, and also claims to introduce the concept of the additional predictors. I recommend that the paper recognize both Wood et al (2009) and Weerts et al (2011) as introducing the QR method for streamflow post-processing (until another earlier ref. can be found!), recognize the Wood et al inclusion of predictors such as river rise, and remove the framing of Weerts et al as the ‘original’ method versus this papers ‘new additions’. The authors make a substantial contribution in their detailed examination of river rise together with the new predictor – trailing error – and the use of QR to estimate exc. probs. The presentation by Wood et al. is great, because it is exactly the type of application that motivated me to do this research. I think, it is very valuable that small business provide uncertainty estimates as long as the NWS doesn’t. Additionally, they can provide more localized services when there is a need/market for it.

Thank you for referring me to that presentation, I was definitely not aware of it. It is a pity that that presentation is so hard to find and watch. It took a while before I found the right browser to watch it; on the university network I wasn’t able to watch it at all.

Throughout the paper, I added in references to the presentation, removed the “first application” references, and reworded the “original” additions vs. method “with additions”.

2) *Though the authors highlight several interesting characteristics about the varying performance of predictor combinations, they currently offer little physical explanation for outcomes such as the (1) forecast itself being a poor predictor in some cases, or (2) multiple predictors faring worse at high thresholds. Physical reasoning would help dispel the possibility of simple overtraining, or perhaps mis-aligned training given the sample. I think the paper needs a stronger physical or at least statistical discussion to provide insight into the cause of such findings.*

I can give more statistical discussion:

- (1) The forecast is not a poor predictor. He just cannot be combined well with the other predictors. I explain more in response to your comment 298,7.
- (2) I discuss overfitting in response to your comment 294,7.

3) *The paper argues in several places that the exc. prob. forecasts are somehow ‘more useful’ for decisions than confidence intervals on forecasts (a widely used output). This arguably depends on the user. The position is taken to bolster the author’s claim of an ‘advance’, but it’s unnecessary – both are useful, and the author’s can simply note that they have taken a different tack than in earlier uses.*

I removed this claim throughout the paper.

4) *The results section is somewhat long, and I think the paper could still be effective if the figures and tables were trimmed somewhat – but I leave this to the author to decide.*

Given that this isn’t a print journal, I would like to keep the descriptions of all types of analysis that we have done. I think, they all add a new insight and their descriptions are concise.

Additionally, the other reviewer has asked for more figures rather than less.

Specific Comments

282,2 – *awkward first sentence: ‘further develops [QR]’? or just ‘applies’, or perhaps ‘further develops an application of QR’. I don’t think QR itself is being further developed. also, suggest rephrasing “. . .to predict flood stage exceedance probabilities based on post-processing single-value flood stage forecasts.”*

I revised sentence to be:

“This study applies quantile regression (QR) to the prediction of flood stage exceedance probabilities based on post-processing single-value flood stage forecasts.”

282,5 – *it was not the first, actually – see comment below for 285,6.*

True. I deleted that sentence.

282,8 – *suggest avoiding references in the abstract. Also, this statement is not correct – see comment on 285,6 below – the first implementation did use additional variables. The Weerts*

implementation was far more comprehensive, leading to an article, and also added the nice feature of flow normalization as an innovation to the approach.

I agree. I revised this section of the abstract to be:

“Besides the forecast itself, this study uses the rate of rise of the river stage in the last 24 and 48 hours and the forecast error 24 and 48 hours ago as predictors in QR configurations. When compared to just using the forecast as independent variable, adding the latter four predictors significantly improved the forecasts, as measured by the Brier Skill Score (BSS).”

282,17 – I suggest adding one more sentence to the abstract to state the value of the approach – ie, that it helps quantify forecast uncertainty for the outputs of a deterministic forecasting process, which is currently common practice in many national flood forecasting services.

Good idea. I made this the second sentence in the abstract:

“A computationally cheap technique to predict forecast errors is valuable, because many national flood forecasting services, such as the National Weather Service (NWS), only publish deterministic single-value forecasts.”

283,3 – “quantify ‘forecast’ uncertainty”

Added “forecast” there.

283,13 – perhaps mention that the HEFS system described in Demarge also includes a method for post-processing total uncertainty.

Please see my response to your comment 284,10 below.

283, 23 – ‘serves as’ – perhaps, but who knows? It’s never been verified. Better to say ‘may serve as’

The other reviewer suggested removing this section on QPF forecasts, so I did.

284,3 – this is true in the eastern US – in the west, ensemble forecasts go out as long as 2 years. This figure could be trimmed to reduce paper length.

The other reviewer suggested removing this section on outlooks, so I did.

284,10 – NWS also has a technique called HMOS which is applicable to postprocessing single value forecasts. HEFS also includes the EnsPost module, which post-processes total forecast uncertainty, and these both should be mentioned.

Thank you for pointing me to HMOS, I had not considered it yet. I added the text below. On a side note, I really don’t understand why NWS does not publish the valuable uncertainty information produced by HMOS alongside the deterministic forecast, even though it is a standard product of CHPS. Do you know?

“HEFS includes two types of post-processors. The Hydrologic Model Output Statistics (HMOS) Streamflow Ensemble Processor – which is also a module in NWS’ main forecast tool, the Community Hydrologic Prediction System (CHPS) – corrects bias and evaluates the uncertainty of each ensemble, while Hydrologic Ensemble Post-Processing (EnsPost) corrects bias and lumps the set of ensembles into one uncertainty estimate (Demargne et al., 2013; Seo, 2008). HMOS performs a similar task as the QR approach presented here, but with two major differences. First, it relies on linear regression based on streamflows at various times as predictor, instead of using QR with several types of independent variables. Second, it does not compute distributions of water levels from which confidence intervals or exceedance probabilities of flood stages can be derived, but generates ensembles (Regonda et al., 2013).”

284,13 – again, ‘further developed’? What does this mean exactly? perhaps just use ‘applied’ or clarify what aspect of R. Koenker’s method is being ‘further’ developed.

That part is not essential to the sentence. So I shortened the sentence to be:

“In contrast to an ensemble approach such as HEFS, the statistical post-processing in this paper does not distinguish between sources of uncertainty, but studies the overall uncertainty in a lumped fashion.”

I also included the following disclaimer:

“The study does not add to the mathematical method of quantile regression itself.”

285,12 – this view is a bit narrow; certainly many users are concerned with low flow thresholds as well, and in any case, confidence bounds on forecasts are directly relatable to risk of threshold crossing (high or low).

Please see answer to general comment 3.

285,6 – QR for streamflow post-processing was introduced both by Wood et al (2009) and Weerts et al (2011). The former reference described what was likely the first application of QR to streamflow in the ‘US American context’, and possibly anywhere: Wood, AW, M Wiley and B Nijssen, 2009, Use of quantile regression for calibration of hydrologic forecasts, 23rd Conf. on Hydrology, Phoenix, AZ, Amer. Meteor. Soc., 11.3 [available online at: <http://ams.confex.com/ams/89annual/wrfredirect.cgi?id=10049>] Wood et al. described using QR to provide confidence limits for deterministic forecasts of the Lewis River in Washington State (e.g., Figure 1). The work emphasized the need for determining the QR error models as a function of the rise rate of the river as well as lead time (e.g., Figure 2), and then demonstrated the application. An earlier version of this presentation had been given by the same author at the 2008 HEPEX workshop in Delft, NL on Hydrological Ensemble Post-processing Methods, and this was acknowledged as the inspiration for Weerts et al (2011). It is likely that the work was not submitted to a journal because the authors worked in the private sector, where publication is typically less encouraged than conference presentation. Incidentally, the Wood et al streamflow

QR work had in turn been inspired by the application of QR for calibrating temperature forecasts, as described by Hopson and Hacker (2008), as well as by applications in the wind forecasting industry. Hopson, TM and JP Hacker, 2008, Combined approaches for en-semble post-processing, 19th Conference on Probability and Statistics, New Orleans, LA, Amer. Meteor. Soc., 3.1 [available online at:

<http://ams.confex.com/ams/88Annual/wrfdirect.cgi?id=7501>]

I revised this bit to be the text below. Additionally, I re-worded all references to the “original” approach throughout the paper. Please see also my answer to general comment 1.

“This paper further develops one of the techniques mentioned above: the Quantile Regression approach to post-process river forecasts first introduced by Wood et al. (2009) and further elaborated by Weerts et al. (2011) and López López et al. (2014). The Weerts study achieved impressive results in estimating the 50% and 90% confidence interval of river-stage forecasts for three case studies in England and Wales using QR with calibration and validation datasets spanning two years each.”

285, 23 – given the previous comment, this statement is incorrect and should be removed. The paper should recognize the earlier work and related ideas therein.

Done.

285,25 – this paragraph summarizes results, and seems out of place. Better to state that QR is conditioned on several factors in the study, and say what those are and why they are considered, than to tell the outcome (here) of doing so.

I agree. I re-wrote that paragraph:

“Identifying the best-performing set of independent variables is central to this paper. All possible combinations of the following predictors have been studied: forecast, rate of rise of water levels in past hours, and the past forecast errors. The performance of these joint predictors has been measured and compared using the Brier Skill Score (BSS). This exercise has been repeated for various water levels and lead times. Additionally, the robustness of the resulting QR configurations across different sizes of training datasets, locations, lead times, water levels, and forecast year has been assessed.”

286,10 – having established earlier that Weerts, and I suggest also Wood, introduced QR, it is not necessary to return to it repeatedly in the paper (eg 286, 15, 19 etc). Overall, I think the paper should de-emphasize the verbiage about ‘additions’ and ‘further development’ in contrast to an ‘original method’, especially since the rise-conditioned error approach actually was the first method introduced at a national scientific meeting. Instead, just emphasize what has been done, as it is good work, and the paper can stand on its efforts alone, without requiring the label of being ‘new’ or ‘first’.

Thank you for the compliments. Please see my answer to general comment 1. The paragraph now reads as follows:

“The paper is structured as follows. The Method section reviews quantile regression, introduces the performance measure, and discusses the performed analyses and data. The Results section first reviews the overall forecast error for the dataset. It then describes the results of identifying the best-performing set of independent variables. Finally, it discusses the robustness of the studied QR configurations. The fourth and last section presents the conclusions and proposes further research ideas.”

286,20 – *I would just write here that the work combines elements of Weerts et al and Wood et al, and also does [b] and [c] (though take out the word ‘more’ – not needed, and perhaps debatable).*

Please see my answer to general comment 1. The paragraph now reads as follows:

“The use of quantile regression to estimate the error distribution of river-stage forecasts has first been introduced by Woods et al. (2009) for the Lewis River in Washington State. Later, Weerts et al. (2011) applied it to river catchments in England and Wales. In this paper, elements of both studies are combined. However, our predictand is the probability of exceeding flood stages rather than confidence bounds. Additionally, this study tests the robustness of the technique across locations, lead times, event thresholds, forecast years, and the size of training dataset is tested. To develop the different QR configurations and to compare their performance, the Brier Skill Score (BSS) is used.”

287, 21 – *Here and throughout the rest of the paper, please reframe the presentation of Weerts et al (2011) as the ‘original’ implementation focusing only on the forecast as predictor, with an ‘addition’ being the use of other predictors or conditioning factors – as this addition is quite clearly described in the earlier Wood et al (2009). Both work should be recognized, as they are citable/viewable by the field, and assigning the term ‘original’ to the second reference is misleading. Your paper, as noted above, makes other valuable contributions in addition to exploring these ideas, and does not need to work so hard to distinguish itself. Perhaps call the Weerts version the ‘forecast-based’ or ‘W11’ approach, versus multiple predictor approaches, or any other labeling that seems better.*

Please see my answer to general comment 1. The paragraph now reads as follows:

“When applying QR to river forecasts, Weerts et al. (2011) transformed the forecast values and the corresponding forecast errors into the Gaussian domain using Normal Quantile Transformation (NQT) to account for heteroscedasticity. Detailed instructions to perform NQT can be found in Bogner et al. (2012).”

289, 16 – *again, I object to the characterization that exceedence prob. is ‘more useful’ for decisionmaking than confidence intervals. This really depends on the decision, and I have actually more often, in forecast office settings, heard users ask about confidence than risk of exceedence, though again, it depends on the use. There is no reason to argue this point in the paper. Both uses of the uncertainty are valuable, and I support the authors focusing on the risk*

of exceedence predictand, and stating that is ‘also important’ or even ‘more useful for some users’. But the assertion that it is somehow categorically more useful is needlessly provincial, and can be removed.

Please see my answer to general comment 3. I changed this sentence to:

“First, to be able to optimize model performance it is best to choose a single measure.”

Section 2.3 – as per earlier comments, suggest retitling this ‘Inclusion of additional independent variables’. Please reference Wood et al (2009) as described earlier in recognizing the value of including rise rate and lead time as variables (this can be done obliquely, eg, “. . .as noted earlier, rise rate and lead time have been previously shown to be informative independent variables. We assess these factors as well as . . .” etc. Also, please give more detail (ie, an update of equations 1 &/or 2) to show mathematically how the additional predictors were included.

The section now reads:

“2.3 Identifying the best-performing sets of independent variables

The challenge is to identify a well-performing set of predictors that is both parsimonious and comprehensive. Wood et al. (2009) found rate of rise and lead time to be informative independent variables. Weerts et al. (2011) achieved good results using only the forecast itself as predictor. Besides these variables, the most obvious predictors to include are the observed water level 24 and 48 hours ago, the forecast error 24 and 48 hours ago (i.e., the difference between the current water level at issue time of the forecast and the forecast that was produced 24/48 hours ago), or the time of the year, e.g., using month or season as categorical predictors. Additional potential independent variables are the water levels observed up- and downstream at various times, the precipitation upstream of the catchment area, and the precipitation forecast. However, requesting the corresponding precipitation and precipitation forecast requires an extensive effort or direct access to the database at the National Climatic Data Center (NCDC).

In preliminary trials on two case studies (gages HARI2 and HYN12), it was found that the rates of rise and the forecast errors are better predictors than the water levels observed in previous days. After all, the observed water levels are used to compute the rates of rise and forecast errors, so that these latter variables include the information of the former variable. It was also found that season and months are not significant in quantile regression configurations to predict the quantiles of the forecast error. Probably, the time of the year is already reflected in the observed water levels and forecast errors in the previous days.

To determine which set of predictors performs best in generating probabilistic forecasts, all 31 possible combinations of the forecast (fcst), the rate of rise in the last 24 and 48 hours (rr24, rr48), and the forecast error 24 and 48 hours ago (err24, err48) – see Equation 5 – were tested for 82 gages that the NCRFC issues forecasts for every morning (**Error! Reference source not found.**). Based on the Bier Skill Score, it was determined which joint predictor on

average and most often leads to the best out-of-sample results for various lead times and water levels.

Equation 5: QR configuration without NQT, with percentiles of the forecast error as the dependent variable and varying combinations of the five independent variables. This equation was used to predict the water level distribution for each day at 82 gages with different lead times.

$$F_{\tau}(t) = fcst(t) + a_{fcst,\tau} * fcst(t) + a_{rr24,\tau} * rr24(t) + a_{rr48,\tau} * rr48(t) + a_{err24,\tau} * err24(t) + a_{err48,\tau} * err48(t) + b_{\tau}$$

with	$F_{\tau}(t)$	– estimated forecast associated with percentile τ and time t
	$fcst(t)$	– original forecast at time t
	$rr24(t), rr48(t)$	– rates of rise in the last 24 and 48 hours at time t
	$err24(t), err48(t)$	– forecast errors 24 and 48 hours ago (e.g., the original forecast) at time t
	$a_{xx,\tau}, b_{\tau}$	– configuration coefficients; forced to be zero if the predictor is excluded from the joint predictor that is being studied.

Table 1: Joint predictors. “

293,16 – again, this is a needlessly narrow view, as what aspects of the forecast PDF are required entirely depends on the decision model to which the forecasts may be input. For hydropower optimization, for instance, the full PDF of the forecast would be desired, and is ‘decision relevant’ input. I think all but the last sentence of this paragraph should be removed, and the remaining sentence added to the preceding paragraph.

Please see my answer to general comment 3. That motivation was superfluous in the method section anyways, so I shortened the sentence:

“Then, we calculate the probability with which various water levels (called event thresholds hereafter) will be exceeded.”

294,7 – (1) this is clearly quite a lot of work (which would lessen its operational applicability), and a somewhat brute force approach to determining the best functions. Can the authors suggest any more expedient alternatives to the more or less ‘trial and error’ search for the best predictor combinations? Is there an analogue to ‘stepwise regression’ here, perhaps? In stepwise fit approaches to MLR, there is typically a stopping criterion that discourages the addition of new predictor variables – would any similar measure be useable here? (2) Later comments in Section 3.2.2 suggest that there may be overfitting with larger predictor sets. (3) Overall, the results presentation is quite long, although the figures do support a range of conclusions of the paper, and most are of interest. I suggest the authors look for chances to remove a few of the figures

and/or tables, which may be overkill, especially if they jointly support a conclusions, but I leave it up to them.

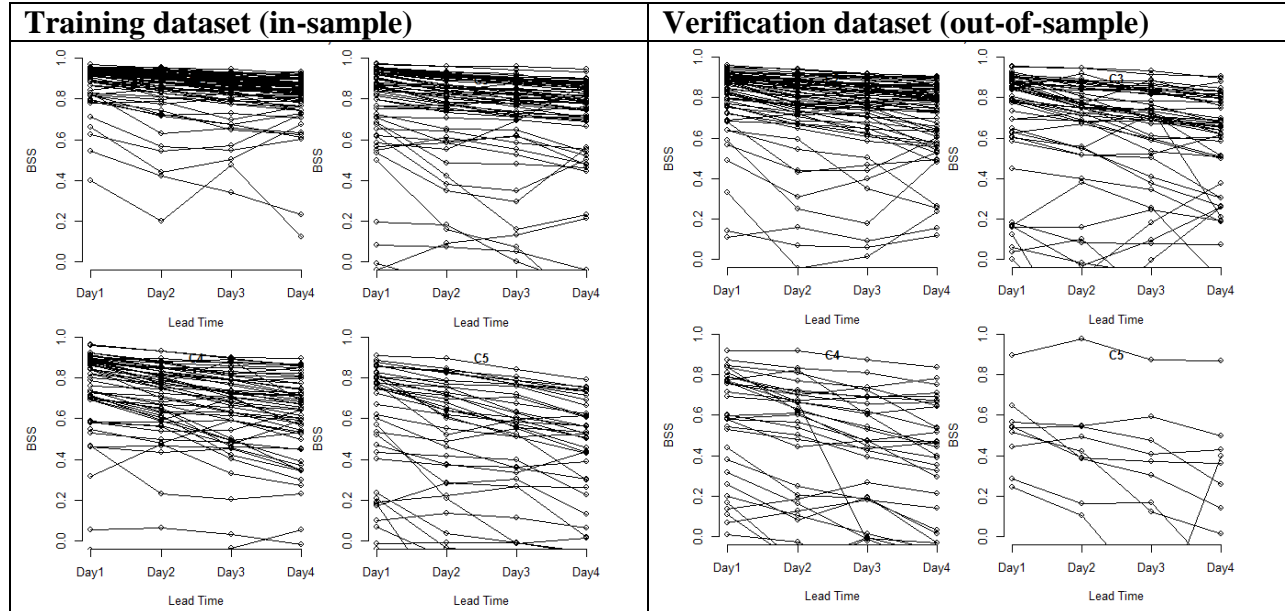
(1) Yes, this is definitely a brute force approach. Of course, stepwise regression would be a much more elegant method. However, we felt that applying stepwise regression to QR in a mathematically responsible way would require too much of our time and resources. Additionally, we are not aware of a R-package or example for stepwise QR. Considering the costs and the benefits of figuring implementing stepwise QR, we felt that we could get a good idea of how different sets of independent variables compare by using the theoretically much simpler brute force approach. Our ambition was to improve the application of QR to estimating forecast errors, rather than further developing QR itself. Stepwise QR would probably warrant the subject of a stand-alone paper, rather than be a detail in a paper on river forecasting. We included this suggestion in the section “Further Work”:

“Finally, this paper uses a brute force approach by simply calculating and comparing all possible combinations of independent variables. Mathematically more challenging stepwise quantile regression would not only be more elegant, but also provide better safeguards against overfitting the data. ”

(2) Regarding overfitting, especially for extreme events, I added the following text in section 3.2.2.:

“For moderate and major flood stage, combinations with fewer independent variables rank higher on average. The most likely explanation is that extreme events like major and moderate flood stage are infrequent. After all, major flood stage equals 90th to 100th percentiles at the various gages. This data scarcity can lead to overfitting when using more predictors.”

I re-ran some of the analysis in-sample, and indeed the model does perform much better for the training than for the verification dataset, see figures below. That is sure sign of overfitting.



(3) Finally, please refer to my answer to the general comment 4.

298, 7 – (1) Please provide greater insight into why the inclusion of the forecast itself might degrade the performance of the post-processed forecasts. (2) Elsewhere, findings that, eg, more variables lead to worse performance at higher stages, also bear more physical explanation. What aspect of the variables could make them damaging to the high threshold models? (3) Also, it's not entirely clear that figures 11-12 support the assertion that “Without a transformation into the normal domain, the forecast does not provide a lot of information for the QR model” – giving metrics of these relationships (r^2 for instance) may help show that in fact, they are significantly different with normalization. There is a lot of scatter in both figs 11 & 12.

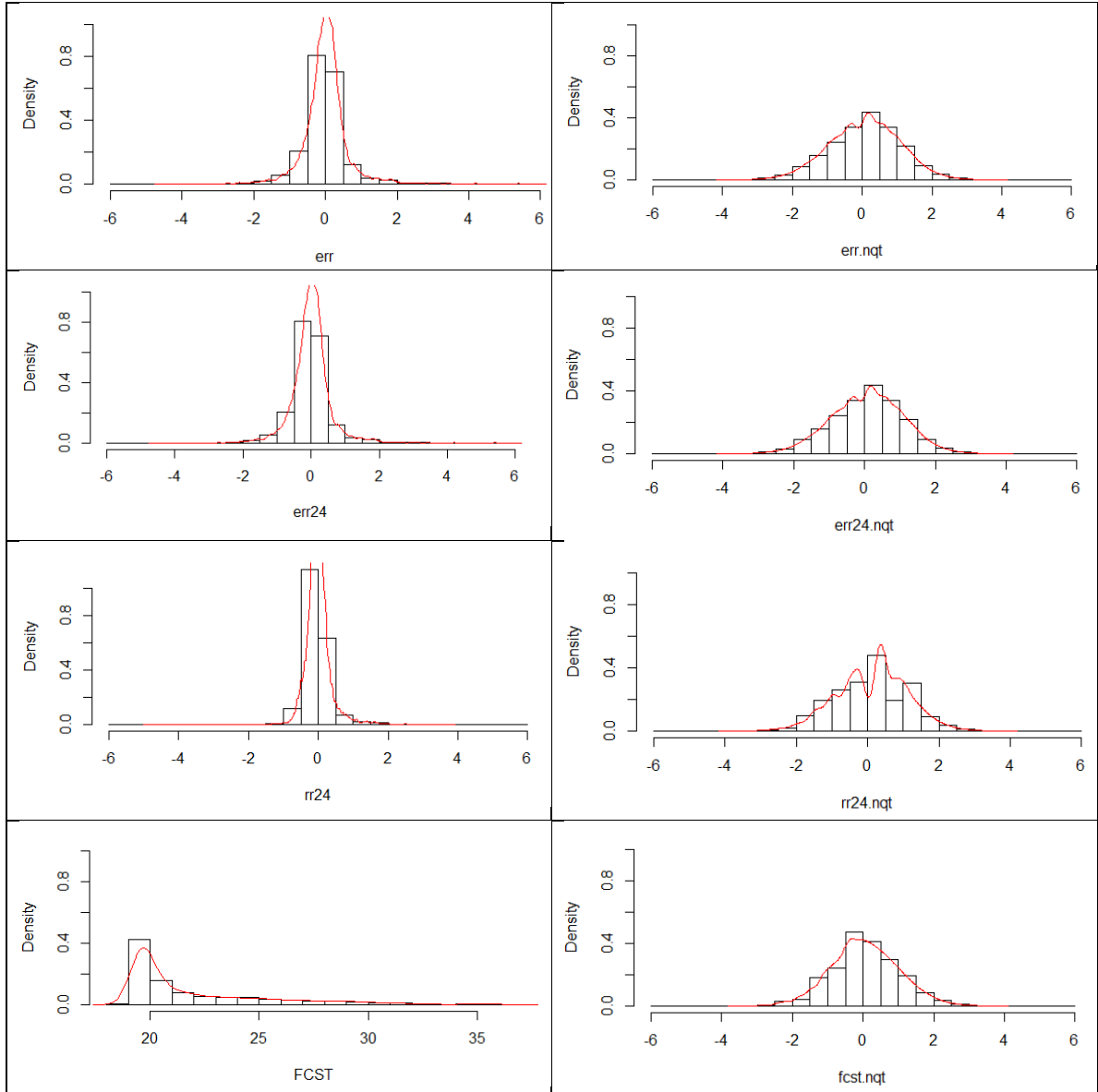
(1+3) I clarified the explanation a bit:

“Without a transformation into the normal domain, the scatterplot of forecast and forecast error does not show a trend. After NQT, the percentiles show trends laid out like a fan. In contrast, the other four predictors become uniform distributions after NQT transformation. There is no trend detectable anymore. Further research is necessary to reconcile these two types of predictors. A possible solution could be to define QR configurations for subsets of the transformed dependent and independent variable. “

The variables other than the forecast have very similar distributions (see plots below). Mapping them onto normal distributions (i.e., “smearing them out more equally”), removes the

bit of trend that is visible in the untransformed scatterplots. However, the forecast is very differently distributed. Transforming the data does obviously not change the physical relationship between these variables, but it does change the statistical relationship bringing out a relationship between forecast and forecast error that was not seen before.

Interestingly, Weerts et al. originally transform the forecast to account for heteroscedasticity, but it turns out that quantile regression would have been much more difficult, if not impossible, without NQT. So accounting for heteroscedasticity made the approach possible at all.



Showing the R2 of the relationships would not cover what I am trying to say. Even after NQT, linear regression would not detect a correlation between forecast and forecast error. But there are trends in the percentiles. They “stick out” from the origin in a fan-like fashion.

And yes, there is a lot of scatter in these scatterplots. This partly explains the mediocre performance of the forecast as shown in Figure 13-17.

(2) Please see my response to comment 294,7 (2).

300,7 – I may be misinterpreting the figures (19,20), but it appears that length of record does matter (longer is better) somewhat more than the authors suggest, and more for the lower thresholds, which is surprising – I'd think those were better represented in any length record than high extremes, given the typical skew of flow distributions. Please comment or provide a more nuanced assessment.

I was not referring to Figure 19 and 20 here, but rather to the regression summarized in Table 4 (now Table 2). That regression generalizes the results that were illustrated in Figure 19 and 20 for just two gages. The size of the dataset was the only independent variable that was not statistically significant in this regression, meaning that the size of the dataset therefore has at the very least much less impact than gage, lead time, and event threshold etc. on forecast performance. However, your comment is still valid: Event thresholds are statistically significant in that regression, meaning that forecasts for low thresholds perform less well. As a likely explanation, I mention that the forecast error is very small for low water levels, so that there is little variability to run a regression on. You are right though, that forecast for low event thresholds seem to be particularly disappointing when the training set was short. While I did not explicitly study these interactions, I qualified my statements a bit. Here are the following relevant, updated text experts:

“Figure 21 and Figure 22 show that training datasets shorter than three years result in very low BSSs for low event thresholds (Q10) at Henry and Hardin. For the other event thresholds, it barely matters for the BSS how many years are included in the training dataset. That is good news, ...

...

To generalize the result, the same analysis as just described for Hardin and Henry was repeated for all 82 gages. Following that, a regression analysis was executed with the BSS score as the dependent variable and the river gages and forecast years as factorial independent variables and the lead time, event thresholds, and number of training years as numerical independent variables (Table 2). The forecast performance was found to vary statistically significantly across all those dimensions except the number of training years.

...

A closer look at the regression coefficients (Table 2) provides interesting insights. For low event thresholds, the BSSs are much worse than for high thresholds. The QR configurations might be performing less well for low event thresholds, because the variance in the dependent variable – the forecast error – is smaller. After all, river forecasts have much smaller errors for lower water levels. The illustrative cases of Henry

and Hardin, described above, indicate that using longer time series to predict exceedance probabilities of low event thresholds improves forecast performance.”

301,22 – as per earlier comments, Wood et al (2009) preceded this study in the American context, and further argued for and demonstrated the use of the ‘additional’ variables of both river rise and lead time. Please adjust text appropriately.

Please see my answer on general comment 1. I deleted that sentence and updated the Conclusions section accordingly. For the sake of brevity, I did not copy the whole Conclusions section into this letter. Please refer to the new version of the paper.

301,26 – Instead: “This work confirms a prior finding that including additional predictors such as rise rates in the past 24 and 48 h benefits the resolution of the resulting probabilistic forecasts. In the first comprehensive assessment of various combinations of. . . , we found that . . . ”

This paragraph now reads:

“When compared to the configuration using only the forecast, it was found that including rates of rise in the past 24 and 48 hours and the forecast errors of 24 and 48 hours ago as independent variables improves the performance of the QR configuration, as measured by the Brier Skill Score. This confirms Wood et al.’s (2009) finding that QR error models should be a function of rate of rise and lead time. The configuration with the forecast as the only independent variable, as studied by Weerts et al. (2011), produced estimates with high reliability. Including the other four predictors mentioned above mainly increases the resolution.”

302,10 –It’s inaccurate to call these ‘the new independent variables’ as rise rate was used earlier.

Updated:

“When forming a joint predictor, the independent variables rates of rise and forecast errors do not combine well with the forecast itself.”

302,14 – it’s not clear why these variables do not lend themselves to transformation – please be more specific and speculate as to why you are finding this. Are they distributed such that the transformation reduces their correlation with the predictand? It’s an interesting result, but not intuitive why it should be.

Also, see my answer to comment “298, 7”.

We hope that you find that these changes to have satisfactorily addressed the reviewer's concerns. If there are additional changes that you believe are needed, please let us know.

Regards,

Frauke Hoss, Paul Fischbeck