Review of: Advances in ungauged streamflow prediction using artificial neural networks

By Besaw et al.

This manuscript discusses a new method for predicting streamflow in ungauged streams and this method is compared to other methods that have been used for predicting streamflow. More recent neural network techniques are compared with more traditional regression and moving window average approaches. The use of data driven models are appealing because they can provide real-time predictions, and do not require the same level of development as physical based models. The background section is an in-depth literature review on the data driven models that have been used to date for predicting hydrologic events. The rivers being analyzed in this study are within the Winooski River Basin. Data from the Dog River and the Winooski River were used in this study and the variables P, T, and Q are used as inputs for the data driven models analyzed. The components of the different predictor methods are described as well as the evaluation criteria for the predicting models. The results and discussion are separated, probably because within the results there was further discussion on some of the methodology. The discussion section compares the different techniques and provides explanation for the significant findings. The transferability and scaling section provides a good discussion on the effects of predicting on a different basin scale and with data that is on an hourly or daily scale. Overall this paper reads fairly smoothly and the conclusions do a good job of stating that this is a good method of forecasting streamflow in ungauged streams.

This paper should be accepted after some revision, because the comparison of the methodologies for predicting stream flow is very well done and this technology is important in water resources. I have a few comments I will discuss regarding the paper, you can find more commentary on the annotated paper copy of your manuscript. The issues of using parameters taken on different time scales was supplemented well with figures and with an explanation of how small scale temporal factors are not considered with daily measurements. I think that further discussion on the transferability of these techniques amongst rivers and watershed should be discussed. Are these methods applicable if trained on a river on the east coast and implemented in the west coast? Is it just assumed that someone will use the measurements from the closest stream?

- I think that you could have a table that lists the different methods used. I would sometimes forget the method that was represented by an acronym.

- The issue of different scales comes up frequently: is it fitting for the time series correlation analysis to be in the background section. On pages 10 and 11 you are performing some preliminary data analysis so I was wondering if it should go somewhere else.

- You discuss how your goodness of fit values for common predicting methods are well below typical values in the literature. I think that this point could be made clearer with a possible explanation why this is happening.

I look forward to discussing your paper further in class. Good luck.

Martin

Advances in ungauged streamflow prediction using artificial neural networks

Besaw, Rizzo, Bierman and Hackett

Review by Andrea Pearce
3/25/09

This manuscript describes the application of 4 different methods in streamflow forecasting and applies the methods to sites with known discharge in the Winooski River basin. The author introduces two recurrent ANNs, trains them on one site and tests them on another simulating an ungauged basin. All of the methods provided reasonable predictions. Shorter time increments between flow and cumulative precip measurements can produce more accurate flow forecasts, particularly in smaller basins where response times are shorter. The dominance of lower flow measurements in the training data biased the ANN predictions, resulting in poorer prediction of high flow events.

This manuscripts should be accepted with minor revisions. These methods describe advancement in streamflow predction in ungauged basins and are appropriate to be published in the proposed journal. The descriptions of the ANN's are clear and well documented. The introduction and background section presents a very thorough documentation of the evolution of streamflow forecasting, but could benefit from shifting the focus from the details of this past work to the importance of past work. Perhaps by picking a couple key themes or major steps forward and discussing how these improved the forecasts, you could better introduce why this is a significant advancement.

Specific Recommendations

- Lines 60-76 – Some of these statements could use references, ex: lines 64-65
- Lines 95-113 and 121-142 – You provide a cross section of examples throughout the evolution of streamflow modeling. If this review focused more on a couple big picture developments in stremflow modeling rather than the details, you could give the reader a better feel for the difficulties and problems solved by the incremental progress.
- Lines 179-197 –You refer to USGS sites, USGS stations, USGS gauging stations, etc. Would be clearer if you used consistent language.
- Lines 249 – 253 – Do you need to include this much information about multiple linear regression?
- Line 402 – This is the first you mention of using MLR in your comparison of methods. Maybe at the start of the methods section you could list the methods you will use.
- Think about presenting the methods and the results in the same ordering consistently throughout the paper. A little detail, but it might be clearer for some when the names don't mean much.
- Lines 590-596 – This could be clearer. Try to explain what you mean here in 1 sentence. Ex, " When we applied the recurrent ANN methods (trained on data from the Dog River) to a location on the Winooski River the CPN outperforms the GRNN (Table 4)." Then go on to discuss.

- When describing scaling streamflow, sometimes you refer to ‘area’ and sometimes to ‘drainage area’. Would be clearer if you consistently used the latter.
- Portions of the discussion might fit better in the results section.
- Lines 614-620: This is the only place you bring up processing speed and without more specifics such as CPU time required and how the flexibility of the GRNN makes better predictions, it seems out of place.

Lee Corbett
GEOL 371
3/18/09

Review of: Advances in ungauged streamflow prediction using artificial neural networks
Authors: L.E. Besaw, D.M. Rizzo, P.R. Bierman, and W.R. Hackett

In this manuscript, the authors present data from a study that uses ANNs to predict streamflow in the Winooski River Basin, Vermont. While most similar work has focused on predicting streamflow in basins that already have stream gauges (therefore using the measured stream flow as an input parameter), this study seeks to develop a method for predicting streamflow in basins without gauges. This work appears to be unique and has important implications for communities and community planners who live and work in ungauged stream basins.

Overall, the manuscript is well written and polished. The authors do a very good job balancing the highly technical method-oriented approach of their research with the larger questions and implications surrounding the topic and their results. The material is presented in an organized and logical fashion and the manuscript as a whole is enjoyable to read.

Please refer to the hard-copy edited version of the manuscript for small comments regarding structure and rhetoric. In addition, I have several broader comments:

1.) Your paper has a lot of really technical information, which most typical Journal of Hydrology readers won't be familiar with. In general, you do a good job making the material accessible to the reader. I have a couple additional suggestions on this topic though:

- You never really define what an ANN is. I'd expect that most of the journal's readership won't be familiar with ANNs, so it might be helpful to give a brief definition in your introduction to set the stage for the rest of the paper.
- You have a lot of acronyms floating around in your text and it sometimes has that "alphabet soup" feeling. Can you eliminate some of these? As someone not familiar with this type of work, I found it frustrating to keep having to flip back to figure out what some of the acronyms meant.

2.) I really liked your introduction and I think it was effective at presenting the big picture idea and the motivations behind your research. It needs some supporting citations though. Many of the ideas you raise are probably "common knowledge" to the readership of the journal, but citing a few well-known papers will add weight and credibility to your arguments.

3.) I found that your Results section was more of a mixture between method and results (i.e. you describe what you did and then how it turned out). It flowed well and I don't think you should change how it's organized. However, using a more descriptive section heading will help the reader feel prepared for the material that is in the section.

Good luck with edits and publication. Well done!

Lee Corbett (abcorbet@uvm.edu)

Review by Carrie Pucko

Title: Advances in un-gauged streamflow prediction using artificial neural networks

Authors: Lance Besaw, Donna Rizzo, Paul Bierman and Will Hackett

Summary:

This paper set out to examine the current state of streamflow predictions and also to improve on current models for the purpose of predicting streamflow in small unmonitored stream reaches. The review of current models included many types of statistical models which incorporate a range of variables for the purpose of predicting stream flow. What the current models do not do however is model streamflow in un-gauged streams, giving them limited utility for a vast majority of streams and rivers worldwide (or so I'd guess). The improvements to current models were done using two rivers in Northern Vermont, the Winooski River and the Dog River. They differ in size and in control (Winooski is damned). Results show that it is possible to design ANNs that can fairly accurately predict streamflow in these rivers, even though the confidence in that prediction was lower in the un-damned river and was lower than shown in other studies. However, these other studies incorporated flow data into the predictions unlike the model presented here.

Overall Impressions:

I found that although I know virtually nothing about ANNs, I was able to understand what you were doing, your basic methods and the implications of your results. I thought that the abstract, introduction, and discussion were very well written (Not that the other sections weren't). I did think however, that the fusion of a fairly extensive literature review with the amount of data you have and the clear story you're able to present with it, made the paper a bit confusing. I think you may be better served by eliminating the bulk of the Background section and cutting back on the direct evaluation of the literature. The one way I could see the background section benefiting the overall paper is if you make it clear how each section directly impacts the study you did.

More specific comments:

I thought the abstract of your paper was very well written and gave a very clear and interesting picture of what your study was about and the reasons why it was important. It also pointed me in the direction of a paper focused on data and the development of a model used in a novel way rather than a review paper. I think this is another reason why your paper may be a little more cohesive without that background section existing in its current state.

In terms of your introduction, I think that this could take the place of the more extensive background section simply by providing more references. I think this may be effective, particularly in the second paragraph. Given the flow of the introduction though, which I found to be quite easy to read, I think the addition of information into this section, although it may be necessary, should be selected carefully and well organized. For instance, if you want to include information on the variety of input variables that different models incorporate (paragraph 5 of the background section), organize them into more meaningful groupings such as environmental variables, temporal variables (periodicity) or spatial scale variables. In general I think that the link between any other information included in the introduction from the background section and your study should be made clear. I would hate to see the cohesive picture you've made in your intro be overrun by too much information.

I had no problem with the study site section and found it clear. The one question I may have from this section is in relation to the first sentence of the last paragraph on page 8. "This study uses hourly and daily streamflow data from three USGS gauging stations..." to do what? In the next section (2.1.1) was just confused by what "this basin" was referring to on line 187. It becomes clear by the end of the paragraph, but clarification of basin and sub-basin earlier on may help a little. In line 197, I was also a little bit confused by which USGS stations this was referring to.

Although I did not make it too far into the methods, there was one thing I just wanted to mention. On page 13, in line 280 you define x, but it's not in the model diagram. Is it supposed to be? Or is it incorporated into W?

I thought your results and discussion sections were very well done and nicely written and although I will discuss figures in a minute, I felt like they were all appropriate and for the vast majority of them, they were clear and added to your overall message. In

section 5.1 I had a question based on something you told the class last week. You report on line 514 that the R2 and E that are usually observed in these models are far better than what your model came up with, but based on what I understood last week, aren't the common model fits based on gauged streams and the primary reason you think yours aren't as good is because they are based on un-gauged streams? I just don't feel like you emphasize that enough if that's the case and that you're selling yourself short in terms of the strength of your results. My final comment on the text is in reference to a point made in your conclusions that I think should be emphasized more in your intro, which is the fact that most streams don't have gauges, so the ability of traditional methods to model streamflow in these streams hasn't been shown. Yours however, can do this with pretty good accuracy.

Figure comments:

1) Figure 1: Do the different symbols (squares, circles, triangles) simply refer to the organization the data is collected by at each of these stations? Are they all stream gauges? You may want to think about including climate stations on the map to show proximity to the different drainages.
2) I really liked the clarity of figure 2. It's clear a lot of thought went into this figure. I particularly liked that you pointed out the differences in the range represented in panels c and f.
3) Could you include in the caption to Table 1 how you generated the lag times.

Advances in ungauged streamflow prediction using artificial neural networks
By Lance E. Besaw, Donna M. Rizzo, Paul R. Bierman and William Hackett

Review by Christina Syrrakou

In this paper the writers present the use of artificial neural network methods in order to predict streamflow. For training the ANNs precipitation and temperature data are used as input together with flow predictions. The results from the ANNs are also compared to traditional data-driven flow forecasting models. According to the writers the new aspect that the specific study has to offer in comparison to preexisting literature is the use of predictions as input to the ANN and not observations as it is usually done. Although this method provides less accurate data the writers emphasize that it is a method that corresponds more to reality as usually the observation data do not exist.

The paper overall is well written. The use of language is very careful and although there are many technical terms the text is quite easy to follow. More specifically, the Introduction is well presented, containing only the necessary info and not tiring the reader with exhausting and not needed generalities. The first part of the Background provides the reader with information on all the important studies that have been made so far and the reasons why the specific ANN methods are used. This part is in general easy to follow with some small exceptions (difficult terminology) that if revised would be very helpful (refer to it later). Also, I think that a separate title for this section would be also helpful for the reader as for example "Existing literature" or something similar. The rest part of the background is quite good as well.

At the Methods, although it seems that the writers tried to present a rather simple approach in explaining these complex methods I felt that it might draw the attention from the main points on this paper and make the reader lose focus. In my opinion a less detailed description of the methods and perhaps a small comparison section would be more helpful to the reader. As for the results and the discussion they are presented in a satisfying way. Finally, the conclusions summarize well all the important points that a reader needs to take from this paper.

Additionally, a point worth mentioned, is that the writers throughout the text and especially at sections were the main ideas are more complicated, try to keep the attention of the reader by repeating concluding phrases. Also, they try to make clear how the specific study is different to what exists so far.

So, I recommend that this paper should be published with minor revisions which include the following.

-l50 The term "not statistically different" is a little vague to me.
-l71 Can you give some examples of physics-based models?
-l132 What is "an adaptive neuro-fuzzy inference system"?
-l141-142 The formula seems a bit confusing. Maybe consider omitting this sentence.
-l148 What do you mean by "black box approach"?
-l183 Which are the trends you refer to?

-l236-238 Which criteria did you use to separate the data in training and prediction sets?
-l445 Was there a specific reason that the training data were taken from the Dog river?
-l475 Are there other parameters that need to be scaled? For example the vegetation, elevation etc.
-l582 Why is it so certain that although the “training” was made for summer data it would also work for winter data as well? According to p.11 l239-242 you state that for different periods (summer-winter) it would be better to create a specific ANN.
-Table 2. You might want to add in the caption that Q refers to observed values.

In conclusion it is a very good paper and very well-written. Good luck with everything!

Review of

Advances in ungauged streamflow prediction using artificial neural networks

By Lance Besaw

This paper looks at using artificial neural networks to predict streamflow on ungauged rivers. This paper is important because most streams throughout the world are ungauged and we do not have an accurate way of predicting streamflow on most of these streams. The ANNs were used to predict steamflow on a gauged stream so that they could compare the predictions to reality. In addition, the ANN method was compared to a few other methods to determine if it was better. They determined that at a gauged station using previous observation were better for the ANN model then using previous streamflow data. Since the majority of streams are ungauged, these observations cannot be used in most cases. For ungauged streams, they found that using precursory predictions to predict the future was much more accurate then just using climate data.

This is a very well written that is very easy to read and understand for the most part a few things do need to be clarified but that is understandable given the recent rewrite of the paper. The data used in the paper is very high quality and is used and analyzed in what appears to be the best ways possible. The interpretations were very good and what I expect from the beginning, although I wonder what researchers of other predictive models would think. The illustrations and tables are nice and easy to understand, I think you need to a map of Vermont or the Northeast to better show people where it is.

I believe this paper should be accepted with minor revisions. This is a very important paper for better understanding future streamflow on ungauged streams. In addition, the research

was well done and the paper was written very well. I just think the author needs a little more time to revise it and it will be ready for publication. The few things I would change are;

1. Add a zoomed out map.
2. Use fewer acronyms or reintroduce a word after it has not been used for a while.
3. I think more conclusions need to be in the abstract, and the abstract as a whole does not do the paper justice.
4. Simple errors need to be fixed.
5. Clarify your use of gauged streams.

Eric Portenga
3/25/09

Review of Besaw et al.

This manuscript, being submitted to *Journal of Hydrology*, uses modeling techniques to test the ability to predict stream flows in ungauged river basins. In order to test the ability of the models to do this, they were first tested to predict stream flows in a gauged river basin and then compared to observed data. This latter test provided results that showed it is possible to predict stream flows on a basin if it is similar to the basin from which the data used in the model is from.

I cannot speak in regards to the usefulness of the models chosen as my knowledge in modeling is *extremely limited*. What I was able to glean from this manuscript was that using models to predict stream flows worked remarkably well – this coming mostly from Figure 5. I think that after listening to the discussion of this manuscript and what the study involved, the methods used will become clearer to me as I am not used to reading about them.

I think the journal this manuscript is being submitted to is a likely pick and based on what they say they are looking for in original research, this manuscript would be a perfect candidate for publication. I thought the layout of this paper was well organized though there were a few grammatical things I would take a look at:

- The style in which in text citations within parenthetical expressions is not consistent throughout. At times the year of publication is set in parentheses within parentheses, which I found to be a bit redundant and could be simplified by the use of semi colons between citations.
- The general use of semi colons should definitely be looked at. Clauses on both sides of a semi colon should be complete thoughts and often in this manuscript, they are not. For

example, the first sentence of the Abstract includes a semi colon where the phrase following it is not a complete thought.

- I felt a few sentences were misplaced within paragraphs and tried to make note of how I thought they could be rearranged. For instance, the second paragraph of the Study Site section is where the location of the Winooski River basin is located (in northwestern Vermont, USA), though the Winooski River basin is mentioned in the first paragraph

Joe Bartlett

3/25/09

Review of Besaw et al.

The purpose of this study was to develop and test models for forecasting streamflow in ungauged basins. Counterpropogation and generalized regression artificial neural networks (ANNs) were used due to advantages over other types of ANNs. Real world precipitation and streamflow predictions were also input into the model. This model was tested and compared with data driven flow forecasting models based on the streamflow data from USGS gauging stations on the Winooski River. The results of the model were compared for gauged and ungauged basins that were both trained off of the same gauged basins hydrologic data. No statistical difference was found thereby supporting this method for predicting streamflow in ungauged basins.

This article is very well written and organized and is almost ready for publication. A few grammatical errors and tense changes were present. There were also a few wording issues. The abstract gives a good summary of the article and is easy to understand. The introduction and background information are very thorough and do an excellent job framing the purpose of the study. The paper could benefit a paragraph that summarizes the AAN, for less knowledgeable readers. Unfortunately the bulk of the methods and results section make very little sense to me, but I have almost no experience with hydrologic modeling. The discussion was somewhat easy to understand as a lay person. The conclusions were good and wrapped up the paper well.

This paper should be accepted with minor revisions to the Journal of Hydrology. Specific recommendations for editing are as follows:

- L33: Good opening idea, would be better as 2 sentences
- L57: Need citations in the Introduction
- L159: Awkward sentence
- L161: Could use a clearer purpose statement, its basically there in L149 but could be clearer
- L174: This sentence should probably be at the start of the study site section
- L182: Why did you only use 3 of the 6 USGS stations?
- L187: Confusing sentence, what are you trying to say here?
- Figure1: What does the blue shading mean, also dots 5 and 6 are reversed
- L285: Should you number the equations?
- Watch tense, you switched to past tense in the Results section.

- L466: Figure 6: Would benefit from more vertical relief to show detail

March 25, 2009
UVM internal review of:

Advances in ungauged streamflow prediction using artificial neural networks
Authors: Lance E. Besaw, Donna M. Rizzo, Paul R. Bierman, and William Hackett

It has been known that to significantly improve understanding of hydrological systems knowledge of surface water flow is necessary. Currently only a small percentage of streams are sufficiently instrumented to monitor streamflow. To further understand our knowledge of these systems work has been conducted in advancing flow forecasting methods. In this paper the author displays how artificial neural networks (ANN), particularly recurrent ANNs, may be used to predict streamflow in ungauged streams using training data from a stream located in a similar climate. The author also demonstrates the advantages of recurrent ANNs when compared to the over smoothing autoregressive moving average with exogenous input systems. Initial investigations in this paper were used to validate the ANNs by predicting streamflow on the training stream. The results of this showed that for low flows (e.g. base flows) the models did a reasonable job of predicting flow. During this testing phase it was shown that the recurrent ANNs outperformed other ANNs. The models did a reasonable job of predicting the time of peak flow, however often the peak flows were underestimated. In order to account for the under prediction of peak flows a second training data set was constructed consisting only of storm events. Estimations of the ANNs that used the storm event training data set showed similar correlation values as those that were trained using the more inclusive training data; however these ANNs had better predicting capability of peak discharge while having greater inaccuracies when compared to times of lower flow.

The major data component for this paper was from readily available sources making the methods used quite robust. While well laid out tables made interpretation of the results easy for the reader to view and draw their own conclusions without the author's explanations. This said the interpretations of the author did well at clarifying the limitations of the models used, while showing the logical progression behind it. The figures in this report added a wealth of information for the reader, although it may have been more than necessary; distracting from the clarity of at least one of the figures. As I have little ANN theory or systems understanding the algorithms used was quite difficult, and showed that there is room for improvement in the methods section of the report. The results and discussion portions of this paper could have been combine but keeping them as two distinct sections did not seem to affect the flow of the paper, as little additional cross referencing on behalf of the reader was necessary for understanding.

This paper should be accepted with minor revisions. Overall this paper presented a high quality method that allows for reasonable predictions of streamflow in ungauged streams where no training data may be maintained. The methods that were presented in this report may seemingly be applied to stream reaches wherever data may be obtained from proximal streams within the same climate region. That said some areas of the paper need improvements that would allow readers of various backgrounds and specialties to better understand the methods that were used in this report.

1. This report does a wonderful job of detailing the different ANNs that were used to make streamflow predictions. However if the reader is not familiar with ANN systems the actual point of this paper is lost in this section. As developing and understanding ANNs is a quite complex process the readership that may grasp all of the information is presumably small, and

most likely capable of developing their own ANNs and would not need to read the entire methods section. At the very beginning of the section it may be advantageous to only explain the basic information of the GRNN, in layman's terms as the technical information leads to some confusion here. If a nice flow can be maintained in this section it would be nice to have an early explanation the statistical analyses that will be used to determine the validity of each model. Then the report may progress to the different variations that were used, where more technical information on the variations used could be explained. This would allow the non-technical inclined reader to skip to the evaluation and then to the results sections with an understanding of what is to be expected. The more technically inclined reader at this point may already know how the specific ANNs work and skip ahead or pick up any missing details in the following methods sections.

2. The results section does a great job of detailing the analysis results with clear figures showing the variations between the predictions and observed values over time. There is one stumbling block in this section which could have been placed in the methods section of the report. In section 5.1(predicting ungauged streamflow) is the first mention of how the model was scaled to the ungauged watershed, which does bear mentioning here. It would be nice however to keep the technical information in the methods section of the report, just as a formality.

3. Over all the clarity of the figures in this report is quite good. However there are a few changes that may help the reader in the understanding of the report and keep it from being cluttered. Figure 1. is well laid out and clearly marks each piece of instrumentation that is in the study area. There seems to be erroneous information on the stations used here though. Presumably each symbol represents not only different agencies instrumentation but a different type of instrument as well. The legend could be narrowed down to only the three symbols that represents the different gauge types, while excluding the id numbers. Doing this would also allow the actual figure to be expanded giving more geographic detail to the reader while maintaining information on the gauge type.

After closely reviewing the instructions to the author there seems to be minimal infractions on the format requested from the journal for submission. The exceptions that were noted are most likely known already by the author. These included removing figures, tables, captions and references to separate pages. Overall great work and best of luck with your submission

Jaron Borg

Jared Nunery
GEOL 371
March 25th, 2009

Review of:
Besaw et al., **Advances in ungauged streamflow predictions using artificial neural networks**

For submission to:
Journal of Hydrology

In this manuscript the authors describe, compare, and validate a new technique for predicting streamflow patterns in small ungauged drainage basins. Methodologies used with counterpropagation and generalized regression artificial neural networks (ANNs) were tested, as a means of developing a new methodology for forecasting streamflow in ungauged drainage basins.

I must start my review with the explicit confession that this manuscript is beyond the scope of my field, so my comments will largely be restricted to stylistic and small grammatical concerns. In particular, the methodology section, though very well written, was difficult for me to follow; however, I do not feel that this was the fault of the authors but rather of my own lack of understanding of the subject. Overall, I thought the manuscript was extremely well written, concise, and informative. In particular, the sub-section organization throughout the paper provides an excellent framework that was easy to follow. There are only two major comments that I have in regards to the larger picture of the manuscript. First, be sure to clearly state the goals and/or objectives of the study early on in the paper. You do an excellent job of starting both the discussion and the results sections with a re-visit to the study goal, however, this clear problem statement definition and goals/objective is lacking from the introduction. Also, at times the results and methods are slightly intermingled when the individual analyses are described in the results. This could be alleviated by adding in an additional subsection to the methodology section that focuses on model comparison or data analysis, or by incorporating them into the evaluation criteria subsection of the methods.

As I am not familiar with the Journal of Hydrology, I assume this is a very technical journal, with a focused audience. If this is the case, I feel that this is a very appropriate journal choice based on the manuscript in its present form.

Below I have listed specific comments, separated by section (additional comments can be found on the hard copy of the manuscript).

Abstract:

-While you do a very good job describing ANNs and why they are chosen, I feel that this is one part of the manuscript that could use the most work. The opening sentence is very long, and might work better as two sentences. Lines 40 through 48 go heavy into the methods,

substituting more of the results for one or two of the sentences here might offer the abstract more meat. Making the abstract more substantive would reflect the true meatiness of this study.

Introduction:

-While you do a great job citing multiple studies in the latter half of the background section, the introduction lacks this level of detail. Is it possible to incorporate the introduction and the background together, as this would allow you to more quickly jump into a more technical discussion supported by the literature?

-Lines 80-84 begin to describe the methodology, this might be more appropriate to include later in the methodology section.

Background:

-It is clear that you have done a thorough review of the literature; however, I feel that this effort is overshadowed at times in the writing. By using a listing format as in line 107, you are foregoing the opportunity to discuss how each of these studies used ARMA models for hydrologic estimation applications. If one study is about small drainage basins, it might be more valuable to highlight this one study, and include more information as to why it is pertinent to your study, and also how your study is different.

-In closing the background section on line 160 you do an excellent job of framing the study and setting the stage for a slam dunk closing sentence of the importance of your study, but instead it abruptly ends with the however statement describing other studies. This would be a great time to highlight your study, and state the goals/objectives of this study.

Study site:

-The last two sentences on lines 183-185 are very important, however, I wonder if they might fit better in the discussion section.

-Figure 2: Great figure! As I am not sure how regionally focused the journal is, an inset of either Vermont or the Northeast might be helpful for geographic reference of where your watershed is.

Methods:

-This section was written with a great deal of technical rigor. As it is out of my field of understanding, I will not attempt to evaluate, though I will say it seemed to flow logically.

-On page 16 the section from lines 328-335: this might be able to be paraphrased in a table.

Results:

-The first sentence of this section stating the goals of the study is great, but I think it might be more appropriate for wrapping up the background section than here. The following two

paragraphs describe the methods used for evaluate varying methodologies. These paragraphs are important, but might flow better under the evaluation criteria subsection in the methods.

Discussion/Conclusion:

-Great opening paragraph! This sets up the discussion nicely, and helps rope the reader back into the manuscript after a lengthy, but well written methods section.

-I was waiting for the last sentence of the conclusions to come out sooner as I was reading the conclusion. It might be useful to elaborate this sentence a little more, how could this be useful? What are some potential applications? Is this restricted to Vermont, the Northeast, or North America? This would really help to strengthen the justification as to why this new technique is useful.

Meredith Clayton
GEOL 371
25 March, 2008

Advancements in ungauged streamflow prediction using artificial neural networks

This paper presents the results of a study conducted to develop and test methods of forecasting streamflow in ungauged basins. This study involved forecasting through the use of counter propagation and generalized regression artificial neural networks (ANNs). The ANNs were selected for training speed and guaranteed convergence. Local climate records and antecedent streamflow predictions are used as drivers for the model. The streamflow predictions are input through a recurrent feedback loop allowing for the ANNs to forecast flow in ungauged basins. The methods were tested through comparison with climate and USGS streamflow records from three basins in Northern Vermont. In order to validate predictions made in ungauged basins, the models were trained on climate-flow data from one basin and to forecast streamflow in a nearby basin with a different climate record. The results of this study suggest that this process of training and predicting produces results that are not statistically different than those obtained when training and predicting in the same basin. Comparisons of the prediction accuracies of the models are also presented using both daily and hourly data collected.

I would like to begin by saying that this piece is very well written and appears to be about as close to being ready to publish as you can get. I don't feel like I am qualified to critique the methods used in this study as I have little to no experience with modeling. I can say that for someone who is familiar with hydrologic processes with no knowledge of modeling I found your discussion section to be extremely useful for interpreting the results. Without the discussion section I would have gathered relatively little from the results presented in the paper. If this paper is intended to be read by people who understand modeling than the content of this paper is great. On the other hand, if this is intended to be read by multiple audiences than I would recommend adding some additional details surrounding the concept of complex modeling. I found only minor grammatical/typo errors throughout the paper, more heavily concentrated toward the end. The conclusion definitely needs to be edited a bit before submission for these errors. And a thorough examination of the conclusion will ensure your paper ends on a strong note. I also noted that there are a few places in the discussion where you repeat things from previous paragraphs in the section. I did not think that it was imperative that any of them be removed, but you may consider it if you are hoping to tighten the section a bit. Specifically, you repeated your explanations of how to you accounted for differences in watershed size and the differences in accuracy when using predicted versus observed data. I realize this will not be your most useful review but my limited experience with this topic inhibits my ability to review this paper more critically. Best of luck with the publication, I have no doubt that you will nail it!

- Proofread and correct spelling errors and typos
- Focus on polishing conclusion
- Work to make methods and introduction more accessible to the average scientist, or one without extensive experience with modeling

Mark Isselhardt

Critical Writing (Geology 371)

3/25/09

Besaw L., et al., 2009 Advances in ungauged streamflow prediction using artificial neural networks **for submission to the Journal of Hydrology**

This paper presents a method of predicting streamflow using artificial neural network(ANN) mathematical modeling. The authors use these variations of these sophisticated models to incorporate streamflow, precipitation and temperature observations in one gauged basin to predict the flow in another, ungauged basin. The results were compared to those generated from more traditional data models that include streamflow observations. The results suggest that although the model cannot produce prediction accuracies identical to the ones forecasted on the gauged basins they do capture enough of the flow in ungauged basin as to be meaningful.

This paper presents a large amount of compelling, original work that is clearly written with very few grammatical mistakes. This manuscript deserves to be published with only minor changes. If successful, the prediction models described would have wide applicability for natural resource managers. The abstract does a good job of summarizing the complex modeling that was done. One notable omission in the abstract was the lack of actual result values. This may have been designed to draw the reader in without spoiling the ending. It might also be helpful to include a sentence that states something like "Data from the gauged sections of the Dog River and Winooski River were used to predict the flow in an ungauged section of the Winooski….." The introduction offers a brief background on the use of data models in forecasting streamflow and makes the case clearly that previous work with ANNs all benefited from actual streamflow observations on the basin in question. The background section continues from the introduction with more detail regarding past attempts at streamflow prediction. This section (combined with the introduction) could be condensed without losing the key points. The study site section was helpful to set the scene for the reader but it would be helpful to more clearly state the location of the ungauged section of stream (perhaps on the updated Figure 1?). Could there be room for some general comments about the precipitation events during the years in question? Where the models testing on droughty or wet years? The methods section is written to a level of detail that appears to give ample knowledge to a reader who wishes to reproduce the work. The results section provides the necessary information for the reader to see the outcome of the various models. The big question leading up to this section is how well the model predicts the flow both with and without the benefit of previous observational data. The results appear to show that the geomorphic character of a basin and the temporal precision of data heavily influence the models predictive abilities. It would be helpful to put the error metrics for the Dog River, Winooski River (Wrightsville) and the Winooski River (Montpelier) on a single table. The discussion section covers the findings well but could be fleshed out a bit to include issues of seasonal precipitation, extreme events and at what point would a models predictive ability be considers too low. The conclusion section makes a strong case for the ANN model as being better then climate data alone.

The figures are clear and provide helpful illustration to the results. In some places it feels like there is more information than one figure can handle. Consider breaking out the qq plots.

A few random thoughts regarding the paper:

- Could you clarify how the model was tested on the Winooski (Montpelier) stretch?
- What influence does the timing (day or night) play in the streams response to a storm event?
- Why didn't you test the model on the ungauged section of the Huntington River?
- Would you expect to see the same results if you used the winter data? Or would the models do a better job predicting flow?

There are a few minor changes noted on the hard copy.

Great job Lance, good luck.

Paper: 'Advances in ungauged streamflow prediction using artificial neural networks' by Lance E. Besaw, Donna M. Rizzo, Paul R. Bierman and William Hacket

Reviewer: Nikos Fytilis – 03/25/09

This paper's goal is to develop and test ANN's methods to forecast streamflow in ungauged basin. Several different data are used to train the ANNs such as local climate records and antecedent streamflow records. The selected ANNs were selected due to their training speed and guaranteed convergence. These models were trained on climate-flow data from a gauged basin and afterwards used to predict streamflow in a nearby basin, with a different climate record. The ability of incorporating a feedback loop allows the ANNs to predict flow where no flow observations are available. The two streams used in this research are located in the Winooski Basin, in northwestern Vermont and both of them evaluated on forecasting summer streamflow resulting from rainstrorm events. The results showed that future predictions based to antecedent predictions is more accurate and reliable in an ungauged basin than using climate data alone.

The authors present in great detail their methods and results. Overall, I believe this is an excellent paper and should be published with minor revisions. Writing clarity is a strong point for this manuscript. The structure of the paper helps the readers to digest it very easily. There are many citations of relevant work but I think it would be a good idea to add citations to your introduction also. I believe that in the abstract you could add the abbreviations for the methods you used because it will help the scientific readers of the journal to get to the point immediately. For the two first parts of this paper which I find well organized, I would like to make some minor suggestions:

- I was confused with what tense we should use when we cite relevant research? You weren't consistent with the past or present tenses.
- Although you mention in the background past work done using ANN, I wanted to see a clear point with what the previous ANN research used as inputs for example. I think this part is too long and that is why I maybe lost this information. Also, I believe that the background ends suddenly.
- In subsection 2.1.1. you mention that the temperature data was adjusted for elevation and I think it will be great if you could add the equation you used to do that. Additionally, in this subsection the figure you use to show the Winooski River basin doesn't help at all. I was confused especially on what the two lines show on the map.
- Finally, I loved Figure 2 and how you saved a lot of space by putting the Figures 2 b and c inside 2.a and d respectively. I don't know if you have enough space to add your hydrograph analyses. If you have, they could strengthen your paper.

The first paragraph of the results could easily be the last paragraph at section 3 before subsection 3.1. It is a good summary if what you are going to describe and also includes general useful information about your project. The rest part of the methods is well written and the figures included in this part are the strongest part of the paper. In the results, I liked Table 2 where you compare all the models for the

two rivers. My only suggestion is to clarify what f value you used in equation showed in line 477 so the experienced readers of the journal could easily replicate your results from the data you used. Also, one other interesting point that you didn't discuss much is the result in Table 4 for the GRNN method and the E error metric (-0.35). Finally, the discussion part is well organized providing at the same time some of the possible implications.

Lance, I wish you good luck with all the revisions. Excellent work. I liked especially your sixth reference.