Scientific Peer Review of the Natural System Regional Simulation Model (NSRSM) v2.0

Panel Final Report

July 9, 2007

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1.0 Introduction

This review has been carried out per the request of the South Florida Water Management District. The project name was Scientific Peer Review of the Natural System Regional Simulation Model (NSRSM) v 2.0 and the tasks are specified in a Statement of Work dated December 8, 2007.

The peer review goals were:

1. Determine if proper and sound scientific approaches were used in the implementation of the NSRSM.
2. Identifying weaknesses and potential enhancements in the conceptual framework of the model and investigating if the model contains all of the important hydrologic processes necessary to perform regional scale natural system modeling in south Florida.
3. Determining if the model is suitable to south Florida pre-drainage conditions, including specifying if there are any fatal flaws apparent in its implementation.
4. Recommending improvements in performance metrics.

The panel members and authors of this report were:

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Prof. Rafael L. Bras, Chair
Prof. Wendy Graham
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Mr. Peter Stone

The review was based on the report entitled "Natural System Regional Simulation Model (NSRSM) V2.0 Implementation Report" by Said, Brown and Newton that was published in draft form in December 2006. Elements of this report were not complete at initial release. The final draft version of Results and Evaluation chapter became available April 2007. In addition the panelists used other documents made available through a project web site. Frequent teleconferences were scheduled, with many such meetings held to discuss the progress. Most panelists participated in a field trip to sites of interest in south Florida. A workshop was held in West Palm Beach last March 1-2 2007. All panelists and SFWMD staff attended. Many of the panelists had prior experience with the area and/or models of the area. This knowledge was invaluable in performing tasks that would otherwise been nearly impossible.

The panel understands that previous reviews have commented on the theory behind the model and that this effort was to focus on implementation. Nevertheless, the panel has found it necessary to comment on elements of the model that could be attributed to theory. It is impossible to speak of implementation divorced from an understanding of what is being implemented.

By charge and necessity the panelists had to rely on the draft implementation report cited previously. The panel was asked to comment on the report and was provided with sample forms that outlined the types of issues to be addressed. Hence, a good part of the effort leading to the workshop was to critique the report and formulate questions generated by reading the report and other material. The panel also worked to succinctly answer the questions posed in the list of goals provided in a previous paragraph.

Section 2 addresses “The Conceptual Model”. The goal is to discuss the soundness of the representation of south Florida prior to drainage. The conceptual model consists of the geographical, topographical, ecological and hydrologic assumptions of how the system operated in the mid 1800’s. Section 3, “Evaluation of Hydrologic Processes” discusses whether the hydrology of the conceptual model is appropriate. Section 4 speaks to “Calibration, Verification, Performance and Metrics”. Section 5 discusses “Uncertainties and Model Use”. Section 6 summarizes the comments and questions on the implementation report as provided prior to the March workshop. The detailed questions and comments of each panelist are provided in Appendix A. These comments and questions constitute raw data used in the process of formulating this report and hence are provided as reference and context. They are not edited. Section 7 is a brief “Conclusion”.

2.0 The Conceptual Model

Creation of a conceptual model is the first step in developing more detailed quantitative models. In general, a conceptual model is a theoretical construct that represents some process or processes, with a set of variables and a set of logical and quantitative relations among the variables. Conceptual models are constructed to enable reasoning within an idealized logical framework about the processes. Conceptual models generally include explicit assumptions that are not satisfied in the real system, but these assumptions may be justified because they simplify the model while simultaneously producing acceptable solutions. Models of the same phenomenon may, then, be essentially different because of differing requirements of the end users or because of differences in assumptions used to develop the model.

The District's view of a conceptual model differs somewhat from that of the panel. As indicated in Appendix A of the implementation report, the District views the understanding of the pre-drainage natural system, developed through interpretation of historical data and information, as the conceptual model. There is nothing inherently wrong is this view. The panel strongly believes, however, that the relations among the components of the hydrologic system and assumptions about how the system operates needs to be provided, whether this is termed *conceptual model* or something else.

The current understanding of the natural system hydrology is based on an extensive body of research. This is exemplified by the 41 pages of citations in the report *Pre-Drainage Everglades Landscapes and Ecolog*y (in prep.), from which Appendix A in the implementation report is drawn. Current understanding, although limited by historical data, is grounded in solid research, insight into hydrologic and ecological processes, and extrapolation of current conditions in relatively unmodified parts of the system to pre-drainage conditions. The panel recognizes that 'understanding' is generally incorporated into the model implementation, and, in the panel's view, is used to parameterize the model. Hence a large part of the criticism of the conceptual model that follows is based on presentation rather than substance.

The NSRSM version 2.0 Implementation Report does not provide a clear description of the NSRSM conceptual model, as defined above. Some information on the conceptual model is presented in Chapter 3 and in Appendix A of the Implementation Report, but the discussion is somewhat disjointed. Chapter 3 is a description of natural system hydrology, based on current understanding of the best available information. This 'understanding' of the natural system hydrology is never fully developed into a conceptual model that *demonstrates relations among various components of the hydrologic system and assumptions about how the system operates*. A framework for discussion of the conceptual model could be landscape, boundaries, inputs and losses, and exchange processes.

The NSRSM conceptual model includes rainfall, evapotranspiration, ground-water flows, river flows, and overland flows. Reasonably good descriptions of individual pieces of the model are provided, but, again, it is not at all clear how the pieces fit together. A flow chart(s) and schematic diagram(s) that shows how water moves through the system, and which model components are used to simulate this movement, would be a useful addition to the documentation of the conceptual model.

A second important aspect of the conceptual model that is missing from the discussion is a clear presentation of assumptions. Enumeration of assumptions associated with each of the aspects of the conceptual model is critically important for an informed presentation and discussion of the model. For example, groundwater and surface water flows are constrained in the model to be unidirectional in any single computational cell. This fact was not evident from available documentation, and only was revealed in discussions during the workshop. As previously noted, assumptions are generally required for the development of all models, so assumptions are not to be viewed as a shortcoming. Rather, documentation of assumptions leads to transparency in the modeling process and improved credibility of modelers. Note, also, that the discussion here is about assumptions in the conceptual model. Additional assumptions about model input data, methods for evaluating model performance are required for model implementation, but are not discussed here.

A concise presentation of assumptions inherent in the conceptual model serves a second important function—assumptions are a logical starting point for uncertainty analysis. The effects of a number of assumptions on simulation results could and should be evaluated through sensitivity testing, or uncertainty analysis.

Another significant assumption of the conceptual model is that "the vegetation and peat micro-topography of the pre-drainage Everglades were in equilibrium with, or closely "tuned to" the hydrologic driving forces originally present" (Appendix A). This assumption seems to preclude consideration of the effects of fire on micro-topography, as well as the interaction between hydrology and vegetation. In other words, topography and vegetation are assumed to be constant during the period of the simulation.

Yet it is well known that there is indeed a dynamic relationship between hydrology, topography and vegetation. For example, several years of lower water levels quickly lead to spreading of drier species such as willow, elder and careless weed into a previously monotypic sawgrass marsh. It seems likely that this type of succession could occur during naturally dry periods. Hence, a significant enhancement to the model could be some linkage between hydrology and vegetation. This linkage need not be fully deterministic, but could include an iterative process whereby vegetation is modified following some selected number of years of lower than normal (or higher than normal) water levels. The effects of fire on micro-topography might also be included through a stochastic component, in which fire occurs at some stated frequency over some stated area. These linkages could become more important if the simulation period increases from a few decades to almost a century. That is, a static vegetation pattern may be a reasonable assumption for two to three decades, but it not over time periods of fifty years or more.

These enhancements would allow the NSRSM to move from being a simulation tool to a more powerful tool for assessing and understanding long-term hydrologic regimes and regime shifts. The model could be used to generate various meteorological and hydrologic scenarios, and, using multiple simulations, produce probability distributions of potential effects on vegetation.

The NSRSM modeling effort is unprecedented and highly impressive. The development of historical data sets and the creation of an entirely new modeling code have been conducted efficiently and in a scientifically rigorous and open manner. For the most part, issues relating to the conceptual model are related to elucidation, documentation, and explanation. The conceptual model needs to be presented in a concise set of figures and tables. Description of 'understanding' of pre-drainage hydrology, topography, and vegetation conditions is not a conceptual model. Assumptions, rationale for the assumptions, and possible impact of the assumptions on simulations need to be summarized in one place.

3.0 Evaluation of Hydrologic Processes

All significant processes that describe the regional hydrology of pre-development south Florida are represented in the NSRSM. The methodologies used to represent these processes range from quite empirical (e.g. the treatment of land surface and unsaturated zone processes) to more physically-based (e.g., groundwater flow and overland flow). Decisions regarding the level of sophistication required for modeling different hydrologic processes in different regions seem to have been made based on intuition, data availability and experience to improve computational efficiency. While the panel accepts the judgment of the modelers at the SFWMD, a more rigorous discussion and justification of the level of complexity chosen for each process should be included in the written documentation.

3.1 Topography and Land Cover

The District has used a variety of sources of topography data, photographs and other historical knowledge to describe land surface elevations and land cover in pre-development south Florida. The resolution and accuracy of the topography data used depends on the technologies used to gather and process the data. Methods used to match and merge topography data are described very cryptically, i.e. in terms of the names software algorithms used, rather than in terms of the procedures implemented in the software algorithms. These methodologies should be described more clearly when the report is revised.

Although some plots presented in the March workshop showed spurious land elevation peaks in some parts of the domain, in general the panel believes the final product represents a plausible best estimate of the pre-drainage topography. Nevertheless the panel recommends that the model be exercised to reveal sensitivity in key model predictions and performance measures to alternative topographic and land cover representations. Geostatistical and other spatial random field methodologies coupled with Monte Carlo analyses should be explored to evaluate perturbations in predictions and performance measures to perturbations in topography and land cover.

3.2 Precipitation

The results that are included in the NSRSM Implementation report, and discussed during the March workshop, are based on the historical measured rainfall from 1965 through 2000. The methods by which the rainfall measurements were screened for outliers and interpolated over the modeled domain seem generally appropriate. However the patterns of rainfall and the choice of input periods need further analysis, justification and discussion. The use of the PRISM precipitation data is discussed in the report and appendices but apparently has not been used for NSRSM simulations. If this is indeed the case, the panel recommends the discussion of the PRISM data be removed from the entire report.

The panel supports the on-going efforts to develop a longer precipitation record that would exhibit a wider variety of extreme precipitation forcing and the panel understands that PRISM data may be useful in this endeavor. Using historical data as forcing is always the ideal. Developing synthetic time series of precipitation as a way of studying system behavior with longer and possibly more variable input is an option that requires careful consideration and justification.

3.3 Reference Evapotranspiration (ET)

The procedures used to generate Penman-Monteith Reference ET over the NSRSM domain using NARR data are innovative and defensible. The spatial patterns generated by these procedures appear to be physically reasonable at the large scale, however some of the small-scale perturbations appear to be spurious results of the spatial interpolation of the 32 km NARR grid onto the higher spatial resolution NSRSM grid. Alternative interpolation schemes (including nearest neighbor "non-interpolation") should be investigated to eliminate this problem.

3.4 Actual Evapotranspiration

Methods used to estimate actual evapotranspiration from reference evapotranspiration in the Hydrologic Process Modules (HPMs) are incomprehensible based on information included in the report. More information was given at the March workshop, but the rationale used to select the various methodologies implemented and the details of these methodologies are still unclear. Two different methodologies are apparently implemented in separate HPMs (Layer1nsm and Unsat). The various coefficients used in these methodologies (e.g. Kc, Kveg, Kw, Rd, Xd, Pth, Xth) apparently have little real physical meaning and are primarily tuning parameters. In fact, based on the information presented, the two sets of parameters for the different methods seem completely analogous in their impact on actual ET calculations. If the physics of the natural system requires alternative ET estimation algorithms to be used in these HPMs to accurately simulate the system, this information should be presented in the report to justify the additional complexity.

Another fundamental issue is the lack of representation of the two-way relationship between the hydrology and the vegetation. Changes in hydrology would be expected to change vegetation composition, which in turn should feedback to changes in the water balance through changes in ET. In the future, the District should consider adapting NSRSM to allow the adaptation of vegetation in response to hydrology (and vice versa, see Conceptual Model discussion).

The panel recommends that the use of the term "crop coefficient" for natural system ET coefficients be eliminated to avoid confusion. The panel also questions the why the rate of ET from a lake should depend on depth, and how the cutoff between "deep areas" and "shallow areas" in lakes is determined.

3.5 Overland Flow and Groundwater Flow

Both overland flow and groundwater flow are simulated using a diffusive wave equation in NSRSM, which is probably valid over most of the domain but may be questionable near tidal boundaries. The NSRSM currently assumes no vertical gradient of head between the overland and ground-water domains since there is only one head value computed for each cell. This raised concerns with panel members because of the significant differences in response times between surface and subsurface systems, and extreme differences in resistance to flow in the two regimes. Coupling the overland and ground-water domains more realistically, perhaps with a flux coupling term between the overland water store and the groundwater store, may produce more realistic interactions between the overland flow domain and the groundwater domain.

It is the panel's opinion that even if the water stage and integrated lateral fluxes are predicted adequately using this continuity assumption between the overland flow and groundwater systems, the methodology cannot predict actual flow paths and thus serious problems may occur when solute transport and transformation algorithms are added to the model. The current model will assume that any solute is completely mixed over the overland flow and the entire surficial aquifer volume, which will not be a valid assumption, particularly for surface contaminant sources.

The panel is also concerned that the integrated treatment of the overland flow and groundwater flow systems may produce problems at the tidal boundary, or in regions where water is ponded in certain cells but not in neighboring cells. Unfortunately insufficient information is given in the report to really understand how the tidal boundary condition is implemented or how within-time-step changes to the non-linear integrated surface-subsurface "transmissivity" are handled. Furthermore the panel recommends that the district avoid the use of the term "transmissivity" to describe this integrated flow resistance factor.

A final concern with this integrated treatment of overland and groundwater flow is that it does not allow for surface and groundwater to flow in different directions, as has been observed in the Everglades system. The panel recommends the District investigate incorporating anisotropic flow resistance terms into the surface and groundwater flow equations to allow for this possibility. It seems that this would be particularly important for surface flow in the ridge and slough system, and for subsurface flow in the vicinity of the peat layer-limestone interface.

Neither the report provided to the panel, nor the March workshop, clearly described the processes simulated in the HPMs or how the HPMs interact with the overland/groundwater flow system. Thus it is impossible to evaluate the accuracy of the assumptions required to decouple these systems, or whether mass is conserved during the sequential solution.

An additional concern is the methodology used to convert stage to volume in the ridge and slough system. Analyses should be conducted and information given to show that this method of converting stage to volume is better than using an equivalent flat ground model. It seems important to evaluate how sensitive the results are to the relative land surface elevations and landscape area percentages that must be assumed. Furthermore the panel had questions about whether the Manning's and Kadlec coefficients used to estimate flow resistance were calibrated for this type of depth-volume relationship.

A final concern regarding surface/subsurface interactions is the way in which river seepage into groundwater occurs (and vice-versa). The equation used to represent this interaction assumes a relatively low conductivity sediment layer exists between the sides and bottom of the river and the limestone aquifer, which may not always be the case in this system.

3.6 Mathematical Representation of Processes

NSRSM uses a very efficient and innovative numerical solution to the representation of the hydrologic processes. But, as discussed previously, it is assumed that a single hydraulic head is valid for a unified surface and subsurface representation. Essentially it operates like a two layer model where the vertical connectivity is perfect but the horizontal hydraulic conductivity (ability to transmit water) is different: very high for the surface processes and lower in the soil. This solution implies that in the model the subsurface and the surface flows must move in the same direction. This may be a reasonable assumption in much of the historical Everglades but it is a major assumption that is not clearly stated or explained in the text. This needs to be done. What is needed is not a mathematical discussion but providing a physical argument for the assumption.

The discussion of numerical errors and computational constraints remains very confusing. Although the panel believes it is correct, the fact is that we cannot verify the analysis and numerical results. This numerical error analysis is complicated by the assumptions discussed in the previous paragraph.

4.0 Calibration, Verification and Performance

4.1 Calibration and Verification

The material available to the panel did not provide a thorough discussion of how calibration was performed. Calibration was made to reproduce ranges of pre-determined "historical" values of stage or flow. Where other reasonable alternative interpretations of history or different ranges of variables could be assumed a discussion on possible impact on calibration is warranted. It is understood that many of the parameters are obtained from other sources, but there were, like with any model, adjustments to some of the parameters. How this was done and whether it involved major or insignificant "tweaking" is not clear.

It is accepted and understood that by necessity this model requires a "soft" calibration and verification. Nevertheless, the individual processes are testable. To carry these tests elements can be partitioned geographically or by hydrologic process. Again, there is no real discussion of testing the various elements so as to definitively conclude that the best representation has been attained. The panel does understand that this model can, should and will be constantly refined.

4.2 Performance and Performance Indicators

Performance indicators are typically used to quantitatively describe the agreement between model simulations and measurements. Some simulation model performance indicators that have been used include absolute mean error, root mean square error, correlation coefficient, range, variance of errors, weighted residual, Akaike Information Criterion, Cook's D, and the Nash-Sutcliff coefficient. Application of these performance indicators requires a good set of measurements with which to compare simulations. No such data exist for the pre-drainage Everglades. Because of the absence of pre-drainage Everglades data, the use of model performance indicators of the type described above is not appropriate for NSRSM. Rather, it is more appropriate to evaluate model performance by using a more qualitative approach that addresses acceptability of the conceptual model and associated assumptions, validity of input data sets and the sensitivity of model results to changes in these data, and plausibility of the simulations based on understanding of the natural system.

The simulation results that have been provided are very encouraging. They are almost surprisingly good, for the Everglades region at least, especially considering the complexity of the modeled region and the possible multiplication of errors downstream.

There is general agreement between the model output and gross estimates of depths, minimum, maximum and range in depths for different land cover types. In some ways, this reflects the ability of the staff to 'tune' the model to *a priori* estimates of hydrologic regimes in different land cover areas, although precise tuning is not possible because exact values of pre-drainage water depths are not known. This also reflects assumptions about rainfall patterns during the simulation period, as discussed below.

The performance indicators focus on specific scale ranges in space and time. The model output is presented as 1) hydrologic measures (primarily water depths, depth ranges, length of inundation and flows) at a specific geographical point, 2) flows across specific transects or flow sections and 3) water budgets for landscape units. In all of these cases, a time series of output (water level and overland flow) from the NSRSM are used to calculate a statistical summary. In most cases it is an annual mean of water depths, range of depth, flow or hydroperiod. These summary statistics are calculated for a specific cell or group of cells. These cells are located around the Everglades and appear to correspond to existing gauging stations that have been identified and selected as key indicators for CERP. The intra and inter-annual patterns are treated as variance around an annual mean. The annual statistic reflects a single time frame of the system dynamics. The system however, operates at multiple time frames, and the output statistics should reflect both longer-term dynamics (decadal) scales and shorter-term (monthly) scales. This can be readily done from the existing output.

The use of indicator cells is problematic. Such punctual comparisons are fraught with error. It is really wishful thinking that the model could reproduce a particular point, even if pre-drainage values were available for that point. It would be more reasonable to make comparisons over regions, averages over large spaces. The panel is pleased that this approach is being followed in the latest version of the results. A much larger assemblage of cells that more assuredly cover the range of results in this validation or demonstration data is needed for each major landscape. It is obvious in presented results (mainly from outside the Everglades) that individual cells can give extreme and implausible results (perhaps impossible). Obviously the specific reasons for such results need to be tracked down and that knowledge reapplied to the model as a whole. If these are not spurious and simply errors that can be corrected, then--very importantly--they begin to tell at what spatial scales the model can be given confidence or little confidence. How will it be eventually determined whether 4, 16, or many more cells should be averaged (by some criterion) to give predictions for specific areas of high interest?

The chosen simulation period may or may not be reflective of long-term rainfall patterns and other climatic factors. The report says that the base simulation is “representative of a drier than average decadal climate oscillation” and then tries to attribute lower than reference values model results to this drier period. As stated in the discussion of precipitation input, the panel encourages the extension of the period of record used as precipitation input and supports careful assessment of the possibility of using longer synthetic records that properly represent variability at various time scales.

One missing piece of information is an understanding of how the hydrology of specific areas of the Everglades may have changed since pre-drainage conditions. That is, it may be more important to use the NSRSM to indicate relative changes in hydrology (depths, hydroperiods, ranges, flows) under identical climatic conditions (historically derived time series) between the pre-drainage system and current conditions than to use it to reproduce set target ranges.

4.3 Comments on Performance

In April 2007 the panel received the final version of the chapter on "Natural System Regional Simulation Model v2.0 – Results and Evaluation". This is a good compilation of results, in the form of Figures and Tables. It is not a good discussion. Critical analysis is lacking. The panel recommends that a significant amount of time and effort be given to evaluating and discussing the meaning of these results. Are they good enough for the use? Can unexpected behavior be explained? Is it possible to improve some of the result? How sensitive will the results be to changes in parameters? Are there any unexpected behaviors? What can be learned from the results, do they point to weaknesses, to areas requiring more study? How confident is the district about the results? These are just a few of the questions that a good discussion could address.

Having stated the above, it is clear that the model is performing well within expectations. Appendix A provides a catalog of comments by figure and table of the results chapter.

5.0 Uncertainties and Model Use

By definition modeling the Everglades as they were in 1850 is an exercise that involves difficult educated guesses based on incomplete information. Furthermore, it is impossible to ultimately prove or disprove the results, it is only possible to argue reasonableness based on the best information and scientific deduction. Hence, uncertainty analysis should be the tool to identify the elements of the model and parameters that should receive attention in order to reduce uncertainties. In essence uncertainty analysis is the only tool available, in this situation, to prioritize where to engage in refinements and how to spend resources. It is also crucial that decision makers realize that uncertainty is unavoidable and can only be incrementally reduced, although never eliminated because of limits imposed by the scarcity or the absence of pre-drainage hydrologic, climatological, topographic, and vegetation data. This idea is certainly consistent with adaptive management as adopted by the SFWMD. The District has defined very good methodology to begin uncertainty analysis of the NSRSM. The panel read and heard a proof-of-concept to generate quantitative measures of uncertainty. This must be extended and integrated into the final report. More importantly a discussion is needed about how this information will be used to direct calibration and efforts to refine the system representation.

As noted in the implementation report, the NSRSM provides simulations that "reasonably represent pre-drainage (ca. 1850) hydrology in south Florida. The obvious question is "To what end?" Elsewhere, the report states that the model will be used in combination with other adaptive management tools in restoration plan formulation and target setting. Again the question is "How?" Much effort, and the effort is admirable and scientifically sound, has been given to 'reasonably representing pre-drainage hydrology, but there now needs to be a context in which to apply the model.

As a first step, the report should explicitly give some guidance on the expected accuracy of the results . . . "hydroperiods are likely reasonable to within 14 days; water levels are likely reasonable to with 0.3 m, etc." Recognizing that any such guidance will be largely speculative, it will, at least, give those less familiar with the model than SFWMD staff some benchmarks for interpreting and using model results. Such guidance can be based on (1) current understanding of the pre-drainage system and (2) results of model sensitivity testing. In fact, the sensitivity tests proposed in the document *NSRSM v2.0 Results and Evaluation* are critically important to establishing guidance on expected reliability of simulations.

A second issue to be considered when applying the model is the extent to which pre-drainage south Florida and current conditions differ and how this will affect the manner in which the model is used. Pre-drainage conditions may be very well represented in the NSRSM. But, because of differences in micro-topography, vegetation, precipitation, and evapotranspiration between pre-drainage and modern conditions, consideration must be given to the manner in which the NSRSM results are extrapolated to current conditions. The NSRSM is most appropriately applied as a tool to examine the effects of perturbations to the natural system on hydrology. Assuming that the calibrated NSRSM reasonably simulates pre-drainage hydrology, the model can be used to investigate how changes in various processes or conditions would alter pre-drainage conditions. Perturbations might include the effects of changes in micro-topography, climate (both rainfall and temperature), vegetation type and distribution, timing of inflows, etc.

One concern of the panel is the use and application of output from the NSRSM for restoration purposes. At least two different applications for the model output have been mentioned. The NSRSM report states that the model will be used in combination with other adaptive management tools in restoration plan formulation. Another application is that the NSRSM will be used to develop target settings, or best estimates of a range of hydrologic conditions at various locations around the Everglades. This will engender a set of planning and management activities to attempt to meet those targets.

The panel strongly urges careful consideration of the use of model output, and it should not be used to set targets or any other such prescriptions for restoration. Rather it should be used to help estimate how the hydrology has changed and help design restoration experiments. As an example, one group might propose that the NSRSM be used to establish mean annual flows that would have crossed the Tamiami Flow section, given the rainfall conditions during the simulation period, or another group might suggest the use the NSRSM to calculate an average hydroperiod and water depth in the marl prairies north of Taylor Slough. The panel recommends that these sorts of applications are not appropriate uses of the model. Rather, output from the NSRSM should be used in conjunction with other models, studies and information to suggest how flows across Tamiami Trail or hydrologic patterns in marl marshes might have changed. Almost all of the models, including the NSRSM, NSM, and others, calculate that actual flows at the upper end of Shark Slough during the simulation period were much higher than measured flows during the same time interval. This conclusion seems rather robust, but it is not definitive. So rather than use the model to predict flows into the upper end of Shark Slough, the a prudent next step should be a set of activities that would design an experiment or set up a pilot study on how to get more water into this flow section.

As in the previous example, perhaps one of the most useful applications of the NSRSM will be as a tool to help guide future inquiries about the Everglades system. For example, in the course of this review, numerous questions arose because some model output was different than someone's preconceived idea about that output. That is, the model produced an unintended result, whether it was the depth of water in an upland pine forest, or the amount of water flowing out of the simulated Loxahatchee estuary/slough. In both cases, these contradictions were used to foster or stimulate an activity to resolve those differences. In some cases, a programming or input error was noted.

NSRSM should be used in an adaptive management framework to help guide management experiments aimed at restoring hydrologic regimes, and more importantly ecological function. It is not reasonable to use NSRSM to set hard targets for hydroperiod or water levels because of uncertainty in model results and because aspects of the ecology (fire impacts, topography, among others) have been altered between pre-drainage and modern conditions.

An unresolved issue is how the output from this model could and should be used to deal with other restoration performance metrics. There is little or no discussion of how the output from this model could be used to 'drive' ecological models of vegetation dynamics, of wading bird or aquatic organisms.

6.0 Summary of Comments and Questions on the Implementation Report

In advance of the Workshop of March 1-2, 2007 the panel prepared a series of comments and questions, focused on the draft implementation report. Following is a summary of key points. Appendix B includes all the questions submitted by individual panelist. This material is raw data, not edited or modified for this report and should be read and used accordingly. Some of the questions in the Appendix were indeed cleared or clarified in the Workshop and hence not necessarily applicable. This summary focuses on issues that are still valid. Many of these issues have been addressed in the previous sections.

1. It is important to provide a clear indication of how is the model to be used. What is (are) the purpose(s) of the model. It has been suggested that the model could be used for integration of ideas and knowledge, to explore uncertainties, or to develop better questions or hypotheses about the behavior of the natural eco-hydrologic system. How are the model results to be used in planning?
2. There is a need of a better "big picture" description. The main value of this model is the conceptualization of the various parts of the ecohydrologic system and their links.
3. It is acknowledged that calibration cannot be carried out in the traditional sense. The nature of the model testing deserves a better discussion. Although indeed the process of testing may differ from traditional model uses the panel believes that:
    a. Elements of the model could be tested independently and more thoroughly than presented.
    b. The assumptions going into setting the ranges of values for the different variables need more justification, particularly the key assumption of the amplitude of stages.
    c. Given the difficulty of inferring past behavior, why not test for sensitivity to various assumptions?
4. Analysis of uncertainties and sensitivity are critical for credibility of results and for testing of model behavior. Different types of uncertainties can be addressed (i.e. input, parameters, structural, etc.). Sensitivity analysis and propagation of uncertainty are tools that should be helpful. The report for the district by Intera on the tools to carry on this exercise was well received. Uncertainty may not be completely eliminated. There is the need to be very clear about assumptions made and clear about where knowledge is incomplete and not sufficient. All panelists addressed uncertainty from a slightly different perspective. The district should consider all these independent comments.
5. The results need far more analysis and interpretation than presently provided. Although generally the results seemed promising, there seem to be some implausible results that need explanation. The water balance and fluxes are important results that need additional discussion and far more explanation.
6. The hydrogeology needs more attention. There are significant questions about groundwater flows in and out of the system and about connectivity between sub-surface and surface waters. The parameters and the values used to describe hydrogeology and flows need better justification.

7. The discussion of numerical errors seems to be a source of a lot of confusion, particularly with statements of key sensitivities and the definition of terms.
8. Patterns of rainfall and the choice of input periods need discussion. The use of the PRISM data and the statistical analysis related to it raised questions.
9. Sensitivity to potential evapotranspiration (ET) should be explored. The patterns of ET need further discussion. The conversion of the reference ET to actual evapotranspiration is very important and its parameterization not as clear as it should be.
10. A fundamental question is the two-way relationship between the hydrology and the vegetation. One way of rephrasing this issue is the following. Presuming that the conditions of flow prior to drainage are reproduced and reconstructed, then it is expected that shifts in vegetation patterns will occur with concurrent impact on the hydrology and hence on the flows themselves.
11. The report needs considerable organizational and editorial work.

7.0 Conclusions

The NSRSM model has a long legacy and many of its elements have been previously reviewed. Not surprisingly the overall conclusion is that the model and implementation is based on sound scientific ideas. Nevertheless, this conclusion is based on the integration of the reading material, presentations and panel members' knowledge of work by the District on related issues. Judgment would have been impossible and inconclusive based on the NSRSM V2.0 implementation report only. The draft report needs a lot of work to make it a complete and appropriate document for the task.

The panel was generally impressed by the bases and formulation of the conceptual model; particularly as conveyed during the 2-day interactive workshop. Clearly a lot of literature has been reviewed and a coherent image of the region prior to development has been created and quantified. However the current structure and content of the report make it difficult for an outsider to independently come to that conclusion. Significantly more information regarding the conceptual underpinnings of the model, the integration and linking of hydrologic elements and the numerical implementation, need to be incorporated into the report so that stakeholders can independently access the strengths and weaknesses of the modeling effort.

The report needs major restructuring to help independent reviewers fully understand 1) the conceptual model (as defined in this report) behind the NSRSM; 2) the numerical implementation of the conceptual model; 3) the parameterization of the model to simulate pre-drainage conditions; and finally 4) an analysis of numerical accuracy of the model as parameterized. All chapter headings and sub-headings should be given a numbered hierarchy so that it is easy for the reader to determine where he/she is in the chapter, and what level of importance the current details possess. As it is the subsections seems to present a hodge-podge of information and there does not seem to be any parallel structure between them.

Information on the methods used to determine numerical stability, accuracy and mesh evaluation needs to be expanded considerably into a separate stand-alone section that incorporates necessary material from previously published reports, and technical memos and peer reviewed journal articles.

The assessment of the approach may be organized by looking at: the conceptual model, the inputs, the representation of the hydrologic processes, the mathematical solutions, the calibration, the verification, the results and finally the analysis of uncertainty. Following are brief concluding statements on each on them.

7.1 The Conceptual Model

The inference of the pre-drainage conditions in south Florida seems to be based on sound historical inference and data analysis. Much of it is covered in Appendix A and Appendix B of the implementation report. Both need significant editing for improved readability and integration into a final NSRSM report. Another section of this document addresses the conceptual model and its representation in more detail. Some suggestions are: contrast different interpretations of the state of the pre-drainage system; assign a value judgment on credibility of the various sources and show how it is used in making inferences and decisions; pay more attention to the continuity of contours in the borders of the domain; discuss how different assumptions or inferences may affect results.

7.2 The Inputs

The major inputs are the topography, the precipitation, the evaporation and the boundary conditions. The topography is stitched from various sources and adjusted to fit the conceptual model as needed. As mentioned previously Appendix B of the implementation report needs to be clearer in discussing the creation of the topography. The use of the topography information and the data is sound. The main concern is the lack of discussion of how the errors of the various sources are taken into account in producing the overall, "stitched", product. Analyzing the uncertainty introduced by the topography remains a challenge.

The precipitation product again brings a lot of legacy. It seems to be based on sound data analysis and it seems to have resolved peculiarities of past representations of the annual rainfall distribution.

The district embarked in a completely new approach, relative to old products, of the evaporation. The discussion in appendix D needs to be refined, nevertheless the results are sound and use the best available information and data, The only recommendation is to expand on the discussion of the patterns of reference evaporation obtained.

The main questions about the boundary conditions are: the assumption of no-flow (subsurface in particular) on the western edge of the domain and confusion about the nature of the tidal boundary condition. The panel interpreted the latter as a variable head condition from where flow is computed. How this variable head interacts with the integrated sub-surface and surface waters solution remains unclear.

7.3 The Hydrologic Processes

All significant processes that describe the regional hydrology of pre-development south Florida are represented in the NSRSM. The methodologies used to represent these processes range from quite empirical (e.g. the treatment of land surface and unsaturated zone processes) to more physically-based (e.g., groundwater flow and overland flow). Decisions regarding the level of sophistication required for modeling different hydrologic processes in different regions seem to have been made based on intuition, data availability and experience to improve computational efficiency. While the panel accepts the judgment of the modelers at the SFWMD, a more rigorous discussion and justification of the level of complexity chosen for each process should be included in the written documentation.

7.4 Calibration and Verification

The material available to the panel did not provide a thorough discussion of how calibration was performed. It is understood that many of the parameters are obtained from other sources, but there were, like with any model, adjustments to some of the parameters. How this was done and whether it involved major or insignificant “tweaking” is not clear.

It is accepted and understood that by necessity this model requires a “soft” calibration and verification. Nevertheless, the individual processes are testable. Again, there is no real discussion of testing the various elements so as to definitively conclude that the best representation has been attained. The panel does understand that this model can, should and will be constantly refined.

7.5 Results and Uses

The simulation results that have been provided are very encouraging. They are almost surprisingly good, for the Everglades region at least, especially considering the complexity of the modeled region and the possible multiplication of errors downstream. This reports provides guidelines on how to define performance indicators. The main problem with the results so far is the lack of analysis, discussion and interpretation.

The panel issues a caution against misuse of the model. This report discusses potential uses in some detail. NSRSM should be used in an adaptive management framework to help guide management experiments aimed at restoring hydrologic regimes, and more importantly ecological function. It is not reasonable to use NSRSM to set hard targets for hydroperiod or water levels because of uncertainty in model results and because aspects of the ecology (fire impacts, topography, among others) have been altered between pre-drainage and modern conditions.

7.6 Uncertainty Analysis

The District has defined good methodology to begin uncertainty analysis. The panel read and heard a proof-of-concept to generate quantitative measures of uncertainty. This must be extended and integrated into the final report. More importantly a discussion is needed about how this information will be used to direct calibration and efforts to refine the system representation.

Appendix A: Specific Commentary on Figures and Tables of Results Chapter

Following is a commentary of figures and tables appearing in the latest version of the results chapter provided to the panel. It is mostly due to panelist Peter Stone. The commentary points out areas of unexpected behavior or unexplained discrepancies in the results. The panel emphasizes the need for discussion and analysis in the chapter, which is completely missing. Nevertheless, the District should address question brought up in the following.

Fig. 4. Sawgrass marsh hydroperiods: Very good agreement is observed. A discussion of why zones 1 and 7 are slightly below would be useful.

Fig. 5. Marl marsh hydroperiods: Clearly acceptable but very different behavior between zones 35-39 and 40-46, why? The latter are southeastern Everglades, the former do not show up in figure 5, where are they?

The low values are found where (as mentioned in the document elsewhere) they would be expected (up the slight flanks of Shark Slough), while others are in the southeastern "saline" Everglades (of Egler) and may simply represent a slightly different type of marsh.

The trend of zones 38 to 40 shows the up-flank condition as expected, toward shallower/lesser flooding.

But the trend for zones 35 to 37 shows the opposite of what might be expected, unless there are topographic peculiarities there other than the trend out of Shark Slough.

Fig. 6: Ridge and slough: Good agreement except for few zones

Fig. 7. Long-term average seasonal water depths: Overall there is very good agreement. The Ochopee marl marsh hydroperiod does not occur low in its reference range, despite less rainfall, which might be telling us something about that lesser-known area.

Table 4.: Seasonal amplitudes: These are smaller than interpreted for the natural system, again plausibly because of drier years being modeled.

PM-4 (Performance Measure 4): Computed ET: The quote is a worthy addition: this major factor (ET) needs greater understanding and confidence in its estimation: more research is needed.

Table 7. Evaluation Area 3: Area east of the Everglades proper. Cell 3189 Cypress swamp, hydroperiod is apparently too long. Cell 1924 Mesic pine flatwoods shows a much too long hydroperiod (~11 months) and water much too deep (~3.75 feet maximum) especially considering that these are averages. This cell is mapped to the

marshy back-barrier swale parallel to the coast, which was not pine. Cell 37988 Mesic pine flatwoods showing an average of ~-9.5 ft minimum water level seems unlikely.

Table 8. Evaluation Area 1, Lower Kissimmee & Northern Okeechobee basins: Cells 27450 and 34098, Non-forested wetland, show depths of ~4 ft, which as an average seems quite high. The Kissimmee marshes would not have reached those depths on average.

Table 9. Evaluation Area 2: St Lucie watershed east of Lake Okeechobee: Depth ranges (difference max to min) of ~8.5 to ~12.5 feet in over half the mesic pine flatwoods sites seems extremely unlikely, particularly as averages. These values are far higher than one might guess for 34-year modeling extreme ranges. What are the water table ranges in the area?

Table 10. Evaluation Area 4: Big Cypress basin: Most wet prairie cells (including those with trees) show hydroperiods too long compared to the reference range. Cypress swamp and mesic pine flatwoods are modeled with hydroperiods a bit too long as well

Table 11. Evaluation Area 5: Caloosahatchee area: Several cells have conspicuously wide ranges, ~7 ft or more, even as averages. Cell 26865 has a clear error on the “max” value and probably an error on the “min” value. This could be the result of anomalous topography, but needs explanation. Cell 22561, along lower Fisheating Creek shows a depth of 5.6 feet as average maximum depth. Is this possible?

Table 12. Natural system rivers: Kissimmee River flow is distinctly low in the range. Being such a major factor in water delivery to the whole downstream system, this really needs better understanding. The table should use consistent units.

Figs. 8, 9, 10: Southwestern rivers: Is the unit “cfs” the correct one? (An observed flow of ~3500 cfs for a small low gradient river seems pretty high.) Are there really some small net upstream flows on these rivers (i.e., not merely tidal) as are shown by the “observed” lines? There is very good flow agreement early in the records, albeit for near-zero flows. These are negated in strength of evidence by the high divergence after May 1997. What happened in May 1997? How does one get large observed (and very large modeled) flows at the end of the dry season and before hurricane season? Something is very odd here and the reason should leap out of the records. What happened? And did large-volume impoundment or diversion to tidewater in fact take place upstream in the larger system, to cause the divergence of the observed and modeled flows?

Fig. 11. Lake Okeechobee stage hydrograph: The modeled results fit very well with interpretations and observations from the natural system. They are not low though, even though it was a drier (lower rainfall) period that was modeled and even though the Kissimmee River inflow seemed to model low in the reference range. The lake level is buffered by a high sill elevation and two overflow outlets: it cannot lose water by outflow below the sill level and it readily loses overflow water above these levels. Good conformance is thus weakened as validation evidence.

Fig. 12. Long term average overland flow: Very informative results, in terms of relative magnitudes both geographically and between marsh types (e.g., the marl and marly sand areas of Taylor Slough and the southeastern near-coast marshes and Big Cypress Swamp seem to show more wet season vs. dry season difference than the peat areas; it would be worth knowing why). The working definition for "wet" and "dry" seasons should be repeated here. It seemingly refers to rainfall seasonality rather than water-level seasonality (which lags rainfall) because there is not a large difference in runoff between these seasons on many transects. This, if so, is unfortunate because runoff characteristics are a function of water levels, not rainfall per se. Shifting to high-water and low-water seasons probably would be much more revealing.

Table 13, Overland transect flow: There should be a few words explaining the negative values for T-11. Does it mean opposite direction of flow?

Table 14. Sawgrass plains water budget: Flow sections #2 and #4 show a significant amount of water flowing in from the higher sandy lands to the west and east (about one-quarter the amount from Lake Okeechobee, a recognized principal input). This is seemingly counter to an interpretation in the main report's text on the conceptual model.

Rainfall and ET are the clear dominant factors, which again shows the importance of more confidently estimating ET for specific vegetation and flooding conditions.

The percentages of "total" flow seem meaningless when total flow has both inputs and outputs. The surface inflow from Lake Okeechobee as a percentage of total inputs would be very illustrative though.

This mingling of groundwater flow estimates with overland flow in many cases obscures or adds little as well. How much water crosses the groundwater to surface-water boundary (either direction, but especially up) would have much more importance as ecological or management information. Does groundwater rise in places to contribute significantly to surface water flow in wetlands (i.e., beyond its importance in contribution to rivers)?

Table 15. Ridge and slough water budget: Flow section 1. "Groundwater (out)": If "out" here means from the Everglades into Big Cypress Swamp, this is not very likely. Even though here it is just a tiny flow, it is these sorts of matters that should become very well understood conceptually in the use of the model. Which direction is "out" here? Presumably westward, but for T-11 (elsewhere in the report) the negative sign on flows makes this a little harder to figure. (Also, Flow Section 7's "out" flow actually seems to be into the Everglades [if #7 is Loxahatchee Slough]).

"In" and "out" and arrow directions need to be made consistent and obvious in meaning.

Also, for section #1, how can there be more "out" flux of groundwater but more "in" flux of surface water, especially when the model always assumes that the two flow in the same direction at any given time. Again, conceptual understandings of details are going to be very important.

Flow section #6: Flow laterally out of Loxahatchee Slough (over its eastern flanks) as arrow #6 implies seems a bit unlikely as well. Certainly it is possible to envision (contrive) ways that this could happen at high water if the western side is significantly higher, but is this actually so?

Flow sections #1 and #8: Again we see significant contributions from runoff from sandy adjacent terrain.

Table 16. Ochopee marl marsh: Please define "River seepage." We assume it is groundwater into rivers.

Table 17. Rockland marl marsh: Flow section #1: The low flows across this boundary suggest that the interpretation (made in the conceptual model) that this area floods mainly by rising and encroachment of water from Shark Slough to the northwest is not valid and this marl marsh instead flooded (and floods) mainly from direct rainfall. (Incidentally, the Ochopee marl marsh on the other side of Shark Slough shows closer to the expected condition, with dominance by surface inflow from the adjacent peatland Everglades, but here it is mainly flow from the north, not in from Shark Slough to the east. Very useful information here for understanding the original system.) This is an excellent example of the type of very important new evidence that might be obtained from the model regarding the natural system that is no longer directly observable (even though in this specific area and instance, extensive and expensive field work could probably still ascertain it). The modeled dominance of groundwater here is difficult to understand (and thus accept) even despite the small absolute magnitude of the flows.

Flow section #2: Flow toward the adjacent rock ridge (into it as groundwater and over it via transverse glades at high water) is dominated by overland flow but has a substantial groundwater proportion. Apparently the flow through the transverse glades dominated, despite this taking place only at higher water levels and despite the very high transmissivity there of the Miami Limestone. Hydrogeologic data suggests that during periods of high rainfall there should have been some flow in the opposite direction (Parker's studies from the 1940s). Probably either the 34-year modeled period had no such events or their effects are simply unseen in the overall summation.

Flow section #3: Half of this flow passes through or beneath the porous Everglades Keys as groundwater. In both these areas, how much of this groundwater flows laterally through the rock ridges rather than deeper beneath it would be very informative to know; that is, how much surface water passed into the upstream flanks of the ridges to seep out again as surface water on the other side?

Flow sections #2 and #3 would be good places to test the sensitivity of the modeled flow direction to characteristics of the ridges (e.g., transmissivity and ET), that is, how much lower would each factor have to be to allow a groundwater mound to build and create a hydraulic dam, as was shown for near Miami in some of Parker's studies from the 1940s.

Table 18. Perrine marl marsh west: Flow section #2: Groundwater flow into this area from beneath the Everglades Keys (rock ridge extension) is shown to be substantial. Again, it is difficult to determine readily how much of this contributes to surface flow in the area itself, but this would be the main aspect of ecological and thus management importance.

Table 19. Perrine marl marsh east: Flow section #2: Groundwater flow from beneath the adjacent rock ridge dominates here, but again, how much surfaced again i.e., how much essentially flowed through the ridge proper? Comparison with flow into the ridge upstream (rockland marl marsh, basically section #2) should tell how much water the ridge itself contributes but something seems amiss for the direct comparison: perhaps the two sections don't align exactly (nor was there any reason they needed to) but they are close enough to aligning that comparisons would be interesting. If they do align, more ground water flow is produced beneath the ridge, as might be expected, but surface flow disappears, which can't really happen here. Eventually, particle tracking could be used to align such boundaries for such critical comparisons. This directly relates to the hydrology of springs and ultimately to salinity controls in Biscayne Bay and the coastal waters to the south of it.

There is at least one error in the last three lines.

Table 20. Taylor Slough: Flow section #3: Discharging of one-fifth of the slough's surface flow over its southeastern flanks (rather than all down its axis) is at first a surprising result. This might merely mean the high-water dominance of the sums (whereas surface water may flow in the opposite direction at some lower stages) or instead it may mean the dominance of the water inputs to the slough over the control by the topography of the slough itself. This is very intriguing and useful information.

Appendix B: Questions Provided by the Panel Prior to Workshop March 1 and 2, 2007

Following are the largely unedited comments that the panelists submitted to the District in preparation for the workshop of March 1 and 2 of 2007. These "raw data" is included because it expresses details and questions beyond what can be summarized in the main body of this report. Furthermore it covers nuances of interpretation that may be different among panelists. The District should definitely consider these comments seriously and diligently address them as necessary before finalizing the implementation report.

From Rafael L.Bras

General Comments

The documents reviewed by this panel represent an impressive and honest effort to model the hydrology of south Florida prior to significant development. The model presented is best considered in two parts: the conceptualization of the state of the system circa 1850 and the mathematical representation of the processes important to the system. I was impressed by the formulation of the conceptual model. Clearly a lot of literature has been reviewed and a coherent image of the region prior to development has been created and quantified. This is a defensible exercise, even if interminable. Methodologically this model has proven legacy and hence there is no reason to believe that the mathematical formulation is seriously lacking.

Having said the above, I do have questions of various degree of importance. I also feel strongly that the report as provided to the panel needs a lot of work. It is simply not clear, uneven and fails to do justice to the effort and the model. It is understood that this is a draft produced under less than ideal circumstances; nevertheless that is what we had to review.

At the time of this writing the final results of he model are not available. Hence comments on results are necessarily limited and subject to change.

Although not part of the NSRSM documentation, the panel had the opportunity to review the uncertainty analysis efforts on INTERA. This reviewer was very pleased with that report. It is very well written; it chooses appropriate methodology for the problem at hand and demonstrates the value of the results. This leads to my first general recommendation:

General recommendation 1: By definition this is an exercise that involves difficult educated guesses based on incomplete information. Furthermore, it is impossible to ultimately prove or disprove the results, we can only argue reasonableness based on the best information and scientific deduction. Hence, uncertainty analysis should be the tool to identify the elements of the model and parameters that should receive attention in order to reduce uncertainties. In essence uncertainty analysis is the only tool we have, in this situation, to prioritize where to engage in refinements and how to spend resources. It is also crucial that decision makers realize that uncertainty is unavoidable and can only be incrementally reduced. This idea is certainly consistent with adaptive management as adopted by the SFWMD. Uncertainty analysis must be extended to prioritize further improvements on the model.

Although verification of the model, as a whole, is limited by the reliance on reconstructed historical conditions and lack of data, it should be possible to test elements or sections of the model. If the elements/sections are well behaved then the overall behavior becomes more acceptable. For example, the model could be used to model the "natural" conditions of the Everglades National Park, even assuming the known controlled inflows. Other analogous exercises can be found.

General Recommendation 2: Carry out independent testing of elements of the models. These elements can be partitioned geographically or by hydrologic processes.

It is unrealistic to expect that a distributed model of this nature, particularly one based on historical reconstruction, be able to reproduce behavior at particular points in space and time.

General Recommendation 3: All comparisons and verification should be of statistical nature (which they generally are) and of regional, not punctual, nature.

Given the nature of this model, it is best to make it self-contained, meaning that it should be hydrologically closed in order to minimize the need to use flux boundary conditions since we would need to specify those fluxes.

General recommendation 4: Consider including the upper Kissimmee basin within the model boundaries, as well as extending the western boundary of the model in the Big Cypress area.

The most important part of the model is the reconstruction of conditions prior to development based on inferences from historical documents. Hence, all information utilized in that manner should be absolutely clear.

General recommendation 5: Historical figures and diagrams used in the document are generally unreadable. If they are important, they should be made crystal clear and redrawn if necessary. Some of the newer figures, particularly the legends, are also unreadable, unclear or not properly labeled.

The quality of the body of the report deteriorates progressively, with the most important chapters, Chapters 3 and 4, being the worst. A couple of broad recommendations:

General recommendation 6: Chapter 4 should not mix the representation of natural hydrologic processes with numerical solution considerations.

General recommendation 7: This reviewer finds the use of XML code as confusing and unproductive.

Natural System Hydrology

This should be the core of the report. It is the most important element since it presents the conceptual model. As it stands, it does not do the job correctly. Chapter 3 in the body of the report is too short and telegraphic. It needs to conclude with a clear description of the elements of the area, the flows and a succinct justification for their choice. The main report relies on Appendix A to provide the real information. As it stands this is not acceptable.

Appendix A comes from another report. I found it disturbing and confusing to read a verbatim excerpt that refers to sections and items that are found elsewhere. This needs to be adapted and integrated into this report. Honestly this is so sloppily written that I had a hard time following it. The main message here is that although you have the information, Chapter 3 and Appendix A are not acceptable as they are. A-3 makes no sense relative to the rest of the material. This absolutely needs to be improved.

I like the idea of vegetation "tuned" to the hydrology and believe it is true, but given the importance of this statement it is not thoroughly justified.

I really do not follow all the logic that leads to the key assumption that the range between high annual stage and annual low stage is 2 feet. Please go over the observations that justify this assumption.

After apparently finding significant evidence supporting groundwater flows to surface water, why was it discounted? Am I missing something? If this was true it is very important.

The use of hydrologic equilibrium in A-47 appears incorrect. Why can't you have equilibrium even if continuously draining?

Hydrologic Processes Representation

Chapter 4 suffers from a fragmented and uneven presentation. It really fails to inform the reader about what are the hydrologic elements of the model. I have no doubt, because of the legacy of the material that it is correct but it is simply not there.

<u>Domain and Mesh</u>

Are there any issues associated with using different vertical and horizontal datum(s)? Is geo-referencing a concern?

The statement is made that watersheds boundaries are compatible to those of the managed system RSM for "meaningful comparisons" Comparisons of what? Why should these two very different conceptualizations be comparable?

I have no idea what analyses are being referenced at the top of page 12.

Do you really mean that numerical errors are only found in conditions of stress? Errors do grow.

I have never heard not know what computational health is.

I cannot believe that disturbance of groundwater is the most restrictive and controlling scenario. More than the surface waters? If this is the case this is indeed a very forgiving system. Can you please explain that statement?

I find the use of “transmissivity” for surface waters really strange. This is misleading and confusing. Transmissivity is only defined for a vertically integrated groundwater system, as far as I know.

Where does the lambda test come from?

Badness??? What is it? Again, calling conveyance a transmissivity is at the very least unconventional.

Explain the minimum slope statement in page 14; I assume you intend to say "which had the smallest” in page 14.

Topography

The reference to Figure 9 in p.18 makes no sense to me. There are no contours in the figure. The figure is impossible to read.

Figure 10 shows minimal changes. Do not use black on blue.

What is pool A, B, C and D as referenced at the top of page 21?

Please label locations in Fig 12.

Figure 15 is unreadable.

Appendix B, discussing topography also needs a lot of work. I really dislike the use of internal memos to pass for narrative. It just does not hang together, it is full of errors and statements like: “because we didn’t have anything better to use, we use it. “ Those are not words for a public document. If sources are not used, do not mention them.

Rainfall

Please discuss justification for screening of precipitation data.

Figures 17 and C.1-3 giving gridded annual average precipitation values do not look the same. Why? The discussion in section 2.2.2 “Transformation to Grid-based Data Set “ is confusing.

I have seen better discussions of how the precipitation data set is created. I am nevertheless more interested in understanding the patterns of rainfall that are shown. Why the corridor of high rainfall extending east to west just north of the Everglades National Park? Is it real?

The discussion of the PRISM data set seems disproportional and superfluous given that it is apparently not used except to define wet and dry years. I also do not agree with the statistical fitting of different distributions in adjacent basins. It is just not meaningful. You are being controlled by statistic, not physics. Finally, please explain the significant differences between Fig. 17 (1914-2000) annual average rain and Figure C.2.2, the same for PRISM.

Reference Evapotranspiration

The discussion of the computation of potential evapotranspiration is interesting and thorough. Although well written, it still has random references to other documents and use of jargon that is probably not appropriate. Some of the figures (i.e. D.10) are not readable. The whole discussion on correlation of data sources may have been a useful exercise in the original study but really just clutters this report.

Fig. D14 should refer to daily minimum relative humidity, correct?

The sensitivity analysis is good but needs to be integrated to the results and discussion.

I would have liked to see more discussion of the results, like the attempt to compare rainfall and potential ET results (Figures D.36 and D37 or equivalent ones in the main chapter).

I am not terribly worried about lack of correlation between the two when the computation is made pixel to pixel at the average level. Nevertheless a discussion of patterns is indeed worthwhile. Can you use the sensitivity analysis to explain the patterns? I suspect that relative humidity may be playing a large role, rightly or wrongly, since ET is higher upwind of the lake and in the interior. An important question, not discussed, is how this compares to old results used by the District. If I recall, it is very different. Previous results had bands of ET oriented East-West.

Landcover

The vegetation and landcover classification is not given enough attention in the main report. The Appendix (E) appears very thorough, informative and well written.

Watermovers

The discussion of watermovers is not well done. It is uneven; the use of xml is unnecessary and confusing and could be done far more cogently.

The units of Eq. 7, page 35 cannot be right.

Why ET from a lake depends on depth?

How can you have target elevations for discharge in estuaries and lagoons that are below sea level? What am I missing?

Figure 22 is unreadable.

The whole section on area-stage -volume relationships for water bodies could be done more concisely and with less repetition.

The overland flow discussion is simply not well done. The so-called Kadlec and Manning are the same equations with different coefficients. Save yourselves some words. Notation is all confused, for example see xml in Table 24. What is the K there?

Is Table 26 parameters or conveyance?

What is the connection between conveyance and hydraulic conductivity (see P 57)?

Can you justify the large Kadlec coefficients physically?

How does storage volume change with ET rate (P 58)? Please explain.

I find no reference to Table 33.

Hydrogeology

I am confused, is the hydrogeology a single layer?

Equation 14 follows a definition of transmissivity. It makes no sense.

I find the whole description in the middle of P. 63 about the computation of transmissivity and aquifer thickness completely circular. Please explain.

What is the K in the units of transmissivity at the top of P. 63? I have never seen this notation. Are you implying hydraulic conductivity? K is used for many variables (see surface waters discussion).

Page 64, middle is very vague. There are no manmade features in this exercise.

The variables in Table 35 are never defined.

Overall I find the discussion of the hydrogeology, including Appendix F, unsatisfactory.

<u>River Network</u>

Page 67, third paragraph, is not a sentence. Same page, do you mean Figure 36?

Page 68, you use Greek delta and d for the same variable.

I cannot believe hydraulic conductivities as high as given at the top of P. 69. Do you mean Figure 37? What are you trying to say in the h definition in that page? It is garbled.

Model Results

My understanding is that the model results we have at hand are being revised. Hence, comments must be interpreted with the caveat that ultimately they may not apply to the final results.

Overall the Results and Evaluation document provided reads reasonably well, although it could use more discussion and analysis.

Page 1 of the report says that the base simulation is "representative of a drier than average decadal climate oscillation" and then tries to attribute lower than reference values model results to this drier period. This argument does not follow. The reference ranges were inferred from historical records that, based on Figure 1, were in fact comparable to the driest of the simulation period. If anything, the simulation should have been on the high end.

I am not sure that accounting for a one-year start-up is sufficient. Have you tested this?

The use indicator sites (pixels or grids) is the wrong approach. Such punctual comparisons are fraught with error. It is really wishful thinking that the model can reproduce a particular point. It would be more reasonable to make comparisons over regions, averages over large spaces.

The use of the box in the stage duration diagrams is difficult to interpret. You have to explain it better. For example, remind the reader that you are translating months of time to percentages of the year. Also help the reader interpret that over this period he depth must be above zero.

It does seem that the results are biased low. Any explanation?

Although overall the results seem reasonable, some need explanation. Some of the Max, Min simulated average water levels are way out of range. Do values of –12 or 7 make sense? Do these results sow continuity with adjacent grid cells?

Please use consistent units throughout. You mix acre-ft, cfs, etc. all in Table 8!

Please explain large discrepancies in Fig 19.

The water budgets shown at the end of the chapter are very valuable and interesting. They need more discussion and explanation. Why are there so large differences between these results and those of NSM? What are the implications to decision making?

Uncertainty Analysis

This is not an official part of the reviewed document, but it should be. As stated in the opening section this is very important and the report is very well done. These ideas should be pursued and integrated into the strategy to reduce uncertainties.

Editorial comments

Following are limited editorial comments. I will provide my marked-up copy for more extensive comments.

A-5 reference missing, paragraph after Water Depth

A-6 Caption of A1-1 separated from figure

A-8 mid-October not min – October

A-8 What is "levelly"?

I found the referencing of Plates out of order very distracting.

I am confused, why two figures A1-7?

Why A1-5 comes after A1-7?

Two pages A-40 and too late in appearing (in A-40) Figures A1-1

Line on top of Page A-43 makes no sense.

A-70, I do not find a figure A2-4, missing page

References to Figures F-3 and F-4 in A-74; there are no such figures. Again the result of use of previous write-ups carelessly.

Chapter 4

P 10 Appendix A is mislabeled, is it really necessary?

P 12, second paragraph, the word is "a priori"

Results document

P2, second bullet, “discretized”

P4, top paragraph, line continuity

P5, the caption of Figure 3 is out of place.

From Wendy Graham

1. General comments

Overall impressions:

It is clear that a lot of knowledge and experience has gone into the development of NSRSM v2.0. I have no doubt that the model contains all the important hydrologic processes necessary to perform regional scale natural system modeling in south Florida, and I would venture to say that proper and sound scientific approaches were used in the implementation of NSRSM. However the current structure and content of the report, particularly Chapters 4 (Implementation) and 5 (Results and Evaluation) make it difficult for an outsider to independently come that conclusion. Significantly more information regarding the conceptual underpinnings of the model, and its numerical implementation is necessary before strengths and weaknesses of these can be identified.

2. Specific Comments

Chapter 1: Executive Summary

The purpose and intended audience for this report should be specified in the Executive Summary. If this report is not intended to present the conceptual model and numerical implementation behind NSRSM it should be made clear here, and references that contain this information should be provided.

Lines 24 -29. Define landscape level versus regional system performance.

Line 30. The models performance is described as “good” and simulated flows are defined as “comparable” to observed natural system. These qualitative judgments should be quantified as much as possible here and in Chapter 1.

Chapter 2: Introduction

A more detailed roadmap of this report and its appendices is needed to help the reader “understand the big picture” message of the report and to guide the reader through the myriad of details. In particular the original publication and purpose of the documents that are excerpted in the Appendices should be presented at the beginning of each appendix at a minimum, and possibly also summarized in the introduction.

Chapter 3: Natural System Hydrology

p. 9. Figure 4 is illegible

p. 9 line 18 “hydraulic conductivity is relatively high… and correspondingly lower….” This should be quantified.

Chapter 4: RSM Implementation of Natural System Hydrology.

General Comments:

In my opinion this chapter needs major restructuring to help independent reviewers fully understand 1) the conceptual model behind the NSRSM; 2) the numerical implementation of the conceptual model; 3) the parameterization of the model to simulate pre-drainage conditions; and finally 4) an analysis of numerical accuracy of the model as parameterized. All chapter headings and sub-headings should be given a numbered hierarchy so that it is easy for the reader to determine where he/she is in the chapter, and what level of importance the current details possess. As it is the subsections seems to present a hodge-podge of information and there does not seem to be any parallel structure between them.

In the current structure the chapter launches immediately into an evaluation of model mesh size and computational accuracy. An overview of the conceptual model and model differential equations are never presented. Therefore the reader has no context in which to evaluate the information given and it is difficult to evaluate whether the analyses presented are the most appropriate and important to conduct to evaluate numerical accuracy.

In general I don't find that the tables containing the XML code in this chapter contribute much to the understanding of the methodology.

Detailed comments:

p. 10 Appendix I (complete NSRSM v2.0 XML) is missing, and when supplied should be titled Appendix J.

p. 11 Mesh Evaluation section is generally unclear and poorly explained. Insufficient references are given for the methods used and equations specified.

p. 12 line 14. I question the statement " Numerical errors are found only under conditions of stress". There may also be spatial discretization errors associated with spatially variable but temporally constant patterns that do not dissipate.

Line 31 …"…. was determined through the use of ArcGIS" . How was this done? Using what utility? What results were obtained?

Line 32 … what is the definition of " good computational health".

Line 38 "Badness Test"… is this standard terminology? Sounds strange.

Line 42… why is the "worst case" groundwater scenario the direst day on the rainfall time series??? More explanation is needed here.

p. 13 What is the basis for equation (1)? A reference should be provided for this methodology.

Line 8 Why does period of 5.7 days give a 5% error limit? And what is the error limit?? Error in what?

Line 17. " with sides a fraction of the calculated length…" what fraction? Why may lambda be divided by 5 or 6?

Line 19. It is stated here, and previously, that transmissivity is defined differently for surface and groundwater but no details are given. How are they defined?

Lines 24-26… Similar to above how is it determined how many cells to divide lambda by? Why 5 or 6 for adequate discretization? What is adequate?

Lines 27-29. If the eastern part of the domain has higher conductivity why does it have lower lambda and require smaller cell sizes... this does not seem consistent with equation 1. The statement that sawgrass and marsh landscapes can be modeled with larger sized cells indicates that vegetation is controlling lambda?? Is this for the groundwater computation or the surface water computation? If this is for the surface computation the equation for surface transmissivity should be given. If it is for groundwater, why does the vegetation control it?

p. 14. Define "Mesh Computational Health, and Badness". What is the impact of "badness" in the solution of the governing equations? What is the difference in the impacts measured by badness and lambda.

Line 23. Why is 1x10-8 the smallest value of the slope of the water surface? Why was this value chosen and what is the implication of choosing other values?

Lines 25-28. Why is the wettest day of the rainfall time series the most demanding case for surface water simulation?

p. 15. Line 6 86400 is not a multiplier it is a unit conversion factor

Lines 8-16. "represents and captures" What is the implication of a badness test less than 500 or greater than 6000? What is too small? What is the basis for the upper badness test limit of 10,000? How was this established? References should be provided.

p. 18. Figure 9 legend is illegible and cryptic. Line 18, what is connsm?

p. 19. Figure 10&11 what is the significance of the differences observed in these contours? Some discussion is in order.

p. 21 line 6. “and determined to reasonably represent”. How determined? What is reasonable? Can this be quantified? If not perhaps say “determined to qualitatively represent”.

Line 12. What type of processing artifacts?

Line 14. How were pre-drainage natural system landcover features determined?

Line 20. What is the “mesh environment”?

Lines 24-26 are unclear. Is this arcgis speak? 100’ should be 100 ft? What “does elevation was calculated to the mean”?

It would help if the locations of landscape features described in sub-headings on p. 22 to 25 were shown on a map.

p. 22 lines 7-11. How were elevation ranges 4.5 to 6ft and 8.9 to 14.6 feet decided upon?

p.23 line 6 cite affiliation of Mike Duever

Line 1. Sentence containing “reselected from the Southwest Florida Feasibility Study” is unclear.

p. 24. What are the core locations in figure 14?

p. 24. Line 5, which area?

Figure 15 is illegible.

p. 26. ”The general procedure…….can be described as follows” might more appropriately read
“The general procedure…. Included:”

p. 26. I do not think Table 3 that includes example XML code (or others later in document) are particularly useful. In general they do not add to generally understanding of the methodology. A verbal description is better.

p. 25-27. There is no discussion of the PRISM estimated rainfall here. When was this used?

p. 28 lines 12-17. This definition of terms should be supplemented by a description of the conceptual model underlying ET calculates and an equation to increase reader understanding.

Line 14. “in relative to” should be “in relation to”.

Lines 18-20. Why does evaporation ***rate*** depend on surface depth of lake Okeechobee.

p. 32. It would be useful to have landcover names in the legend.

p. 32-54. Entire WATERMOVERS section needs to lead off with a discussion of the conceptual model of water movement implemented in NSRSM. It would help to have mass balance equations for the system presented before particular flux equations for specific parts of the system are described. Units should be given for all terms in all equations.

p. 34 line 8. Define shore line.

Line 9. Define transmissivity for the surface system and provide units.

Line 10. This statement is confusing… what are higher and lower heads in the lake and the cell?

Line 19. Hydraulic conductivity was used for the conveyance term…what conveyance term?

p. 35 eq 7. Why is Qin not multiplies by As like the other terms. Define units for all terms.

p. 37 line 8-9. Why are there different ***rates*** of ET over shallow and deep water? How is the dividing line between shallow and deep water determined.

p. 40 lines 20-23 can comparable be quantified?

The section describing NSRSM Lakes is very repetitive. It could be made more concise by only describing the general procedure once, and putting necessary details in a table, and perhaps putting the plots of the stage area-volume relationships in an appendix. A map showing the lake locations would be helpful. The XML code tables are not necessary.

p. 53 Figure 31 needs a legend.

p. 56 line 6, define detent.

Line 7. "dependent of depth when" should be "dependent on depth and"

p. 57 line 10 is Harvey mis-spelled?

<u>Stage Volume Converter section</u>

In general…. How do you know that this method of converting stage to volume is better than using an equivalent flat group model? How sensitive are results to elevations and area percentages assumed. Are mannings and kadlec coefficients calibrated for this type of depth-volume relationship?

p. 59 explain that "percentages" are percentages of cell area?

Hydrogeology Section

p. 61 line 13. What is the bottom of the single layer NSRSM? Readers will not be familiar with this.

p. 62 line 10. Equation 14 is not an equation for transmissivity as stated in line 8. You need units for all terms.

p. 63. Should soil storage coefficient be aquifer storage coefficient?

Line 5. Interpolated from what??

Line 8. On what basis was storage coefficient set to 0.2?

Lines 11-12. Why does the greater global aquifer thickness result in no mass violation errors? This is the first discussion of mass balance errors at all.

Lines 13-26. The logic in this sentence is circular. I don't understand the point of this exercise and the calculations in table 34.

Hydrologic Process Modules

p. 64 lines 4-34. This section is repetitive.

p. 65 lines 1-7. These parameters need to defined and their use explained in the narrative section.

p. 65 Table 36. I don't understand the date sequence in this table. Values appear duplicative?

p. 66 Table 37. Parameters in table need to be defined and their use described

River Network Section

p. 67 line 16. Define GMS

p. 68 lines 22-29. Provide units for all parameters. Be consistent…is depth of sediment layer d or d?

p. 68 line 32. Description of k/d is confusing.

Line 35. " …can be 1/10…" For what conditions does this hold? Depends on sediments. Probably not for limestone.

p. 69 lines 6-8. Sentence is confusing. What are the bank_height token and the bank_coeff token?

Line 14. What is 4.#1 ?

Line 17. equation 17 needs a reference.

p. 70 line 19. This is the first mention of a solution matrix. Overall conceptual model and numerical scheme needs to be described in the introduction of this section.

Line 23. Strange description… if source vector is set equal to difference between specified and existing head then volume of water segment must be changing to increase head?

Line 27. Why was it decided to use third-type condition?

Boundary Conditions Section

p. 74 line7. What is a wall-type general head boundary condition?

Chapter 5: Results and Evaluation

In general the Results and Evaluation section needs more discussion and analysis of the results presented. Many tables and figures are presented that are not referenced at all in the text. A quantitative discussion of the level of confidence District Staff have in the model predictions and implications of errors in the model predictions should be included

<u>Specific Comments:</u>

p. 1 2nd paragraph. " the landscape level" needs to be defined.

p. 5. How do the areas and percentages in Table 2, and the weighted mean calculated from them compare to the stage-volume methodology for ridge and slough systems presented in Chapter 4?

Fig 4. Terminology is confusing. Wet, dry, maximum, minimum, and 90th percentile seem to be used interchangeably. Are these average annual max/min over the simulation period? Or are they 90% water levels over the simulation period?

Figs 5-13. I do not understand the significance of the green rectangle in the stage-duration curve (even after phone conference!). This needs to be explained clearly in the text.

In general hydrographs and stage duration curves for ridge and slough, sawgrass plains and marl marsh appear to simulate the system within the reference conditions quite well; however the Sawgrass plains inundation durations seem a little low in comparison the reference range, especially when compared to the other landscape types.

In Tables 3 through 7 which show results for evaluation areas 31 though 5 there seem to be some significant departures between reference conditions and simulation results which need to be discussed thoroughly in the text.

p. 18 first paragraph…. “Lower west coast rivers have not experienced significant improvement therefore flows are compared to current monitoring data”. The choice of the word “improvement” is interesting given the massive investment in re-plumbing the Everglades to reverse previous “improvements” along the northern and eastern boundaries!

p. 20. Shark River Flows are significantly overpredicted after May 97. This needs some explanation/discussion.

p. 21 figure 20. Computed Average Annual ET looks a little low.

p. 23 Figure 23 is presented but not referenced or discussed in the text. What is the significance of this figure? I assume they are simulated results… are there reference conditions or previous NSM simulations to compare them to?

p. 24 it appears from Fig 25 and Table 9 that overland flow values for NSRSM are systematically larger than those from NSM. Is this expected? Results should be discussed and explained.

p. 26. Similarly, the Water Budget for NSRSM is significantly different than NSM. This should be explained and justified.

The Figures on p. 25 and 26 are presented but not referenced or discussed in the text.

p. 27 line 1. “good correspondence” should be defined and quantified to the extent possible. If not possible should be described as qualitatively similar.

Appendices

Each Appendix that is an excerpt from another report should begin with an explanation of what report the section is taken from, where the full report can be found, and the purpose of including excerpted section in this report. Since most of these appear to be from already published reports I will not include any editorial comments.

Appendix C contains descriptions of the SFWMM v5.5 Rainfall as well as analyses of PRISM rainfall time series. It is not clear to me whether results in this report use the PRISM rainfall time series at all. This should be clarified.

Appendix D: Methodologies for computing long-term reference ET appear to be much improved over those previously used in SFWMM.

Appendix E: Vegetation mapping is not my area of expertise but methodology appears appropriate and comprehensive

Appendix H: This appendix in particular needs an introduction and more narrative. It is hard to follow. A reference should be provided for the Sealink Model.

Appendix I: It is not clear to me how and where many of these vegetation parameters are incorporated in model simulations.

From Lance Gunderson

General Comments

1) Model Application. One of my major questions about the report is what is the context or use of this entire project? That is, it seems to me that one of the considerations in building such a model would be the reason for the model. It would be very nice to have a better understanding of why this model was built, and, how will it be used. In the introduction, on line 17, it states that this is used in combination with other adaptive management tools in restoration plan formulation and target setting. How will these be used? This is very general statement, and I didn't find much more throughout the report to help refine or clarify this statement. If indeed, this is used in a context of adaptive management, models are used to 1) integrate knowledge and understanding about key resource issues, 2) help clarify uncertainties, and 3) help formulate better questions or hypotheses about system dynamics which would guide management actions (Walters 1986). I think that this model does indeed do the first objective, to integrate understanding. I think the integration of the work by McVoy and others with the model development has been a very productive exercise and reflective of this application. Certainly the report reflects great deal of work going in to trying to highlight and resolve key uncertainties about the model (more categories than in others). These are discussed much more in the next general comment. The third part is what I see missing. These are scattered around the report, but not ever explicitly posed as questions.

2) Identifying different types of model uncertainties.
I am not a modeler, but it seems to me that the report deals with a bunch of different kinds of uncertainties, associated with the model. For lack of a better format, I thought it was useful to think about them in the following categories. I think that Rafael has highlighted these in his proposed format for comments (which I like very much). Just another way of thinking about it that made sense to me.

a) Input data uncertainty- Clearly, a great deal of effort and thought went into developing the topographic, rainfall, potential ET, inflow, and boundary condition data sets. Overall, I think this is an excellent effort, and one about which the District staff should be very proud.

Rainfall: A few minor issues that I have concern about involve the 'filtering' of the rainfall data set, which removed large and small values. I would like to know more about what that meant to the model output. We know that local rainfall amounts can be very intense, yet how many were removed from the data? I also wonder about the period of data used, knowing that it may not be totally reflective of longer term cycles- e.g. they do not capture the floods of the late 40's, nor the droughts of the early 40's and 60's. I also couldn't understand the rationale of the separate rainfall analyses. Why were these done, and what do they say about uncertainties of the precipitation data.

Topography. My main uncertainty with this involved understanding what was done. The text is full of jargon, and is not clear how topographic data were assembled. Also, I would like to see a little more of a scaling issue addressed- that is what is the relationship between the topographic assignment for a given cell and the spatial variability of topography within that cell. . I don't see this as much of a problem in the sawgrass plains, but perhaps more of an issue in the ridge/slough and big cypress landscapes.

ET. Since this is a critical process, I wonder about the reliability of using one technique. Especially in developing the potential estimates. Was there some work where other approaches were used? I am suggesting that an approach of triangulating among different methods might be in order.

Land Cover (Vegetation) input. While I think the categories are fine, there is more literature to reference than what is in the report. I also have a similar concern (question) regarding land cover as topography—What about scaling errors? - that is, how do vegetation cover types scale to the model cell? What type of variability exists in the amount of cover within composite land covers (such as ridge and slough)? How was this addressed?

Boundary conditions- sea level. I couldn't figure out what was done- how were four daily values converted to one daily value? How was the spatial distribution of values assigned? .

b) parameter uncertainty.

I am still not sure how the land cover and perhaps other variables- such as water level influence the ET function.

I'd like to see some assessment of the sensitivity of the uncertainties in roughness or flow coefficients as they relate to land cover. They appear to me to be assigned by an expert opinion.

c) structural uncertainty- While most of this would appear to be straightforward, I have concerns in two areas; one is the ET function and the other is in what I see is in the lack of feedbacks between vegetation/soils/ and hydrologic processes. I think I know how the ET function works, but would like to see a little more clarification, in terms of relationships to water level and vegetation type. A figure would help a lot. The other issue is not just how vegetation influences hydrologic processes (ET and flow), but how the hydrologic processes alter vegetation types—which would in turn alter the hydrology. There are portions of the report, where the authors acknowledge these linkages exist, but it doesn't seem to be part of the model. I think this is an important consideration if indeed the model simulation period is expanded up to 100 years.

d) performance uncertainty- (or the opposite, how credible is the model). How well does the model 'track' key indicators? How does the credibility of the model affect implications for use in planning or other ways in which the model will be used?

3) What are the key questions about system complexities and dynamics (useful for restoration) that the model can help refine or clarify? This relates to the issue above- of providing more of an iterative context for the model construction and application.

*How has the hydrology changed in critical portions of the Everglades?- Not just in water levels, depths, hydroperiod, but also flows.

*Contribution of Lake to Everglades water budget?

*Contribution of 'upstream' area to key downstream portions- WCA's? ENP/Tamiami flow section? Florida Bay?

*Nature of linkages with Big Cypress region

*How did the hydrology operate at different scales? Spatially and temporally?

*What can the model say about trying to restore the hydrologic regimes in what is left of the pre-drainage system?

Specific comments:

Introduction

This is a large, complicated report, with information spread out among chapters and appendices. It would be very nice to have an over view of the report in the introduction. This would help the reader immensely in terms of navigating through the report. This would likely include what is in the report, what is the order of the chapters, why they appear as they do.

Chapter 2

Is this a two dimensional model? I thought it simulates in three dimensions (xyz). Why would you call it two dimensions?

Chapter 3

Isn't there a lot of uncertainty about the linkages between the lake and 'outflows'? Certainly during very wet periods, linkages occurred between the lake and Caloosahatchee basin.

Would water ever move out of the Everglades into Big Cypress during high water? Would water flow out of Big Cypress into the Everglades along the entire reach, and not just the northeastern or Mullet slough portions?

Chapter 4

What is the relationship between cell length and topographic variation? It seems to me that this should be evaluated, as it could be as important as errors associated with rainfall input or groundwater 'disturbances'

Not sure what 'computational health' means. No viruses?

Would it be helpful to do a sample calculation of lamda and badness, to give an idea of range of values?

Topgraphic Data

Discussion of filling in gaps in topographic data is full of jargon (processing artifacts, base grid, topo patch, to name a few). Suggest either clarifying or deleting.

Clarify how elevation was determined for each cell in the model. This is pretty obtuse.

P 23, line 8-9
The text reads the elevations on the Everglades side were a foot higher? This is not shown in the elevations on figs 10/11.

*Couldn't some of the uncertainties in elevation, mullet slough, buttonwood embankment, lake Okeechobee, be evaluated by adjusting elevations in the model?

Rainfall,
Page 26- not sure why the xml script is included. What is the purpose?

Page 28.
ET is adjusted according to crop type. No crops except for some fruit trees and corn was probably planted on tree islands in pre drainage glades. Why use this term?

Useful to draw model of how ET is modified. For example, how does correction Kc vary with depth. What is extinction depth?

Same comment about xml script –

The ET values seem high in figure18 averages of 50-53"?

Vegetation-
References? The landcover classes were probably based on prior work.
Difference between wet praire over pear middle/north everglades and southern marl prairie?

Page 32 why have watermovers in natural system? No pre Columbian canals?

Do I understand that water is shunted from the lake at stages greater than 20.5? What about lake contributions during high water years?

P 64-
I am not following what goes on with the HPM parameters-
P65 what does table 35 mean? Where did it come from? Experiments? Guesses? Literature?

Same for table 36.

P 74. What time scales used in the model/ average daily rainfall? Daily ET? What are the time steps for boundary (sea level) fluctuations? hourly?

Appendix A

The model is set up so that the landcover/vegetation affects water movement- through ET and flow, but what about the opposite? That is how can you discount feedbacks between hydrology and vegetation?

A7 what is a tuned landscape- equilibrium? The system was always in a disequilibrium- especially between the sawgrass/slough landscape types.

A-7 Average high and low- not sure what these means, or why is it important. Not to key aspects of vegetation, hydric communities. What matters more is patterns of variation.

Not sure what a subpopulation. Isn't this variation you are discussing, a cross scale problem? That is, there are more spatial differences in water levels/ depths.

The long term averages cannot and should not be forced into an annual model That is, we know that the system operates on different scales, not just an annual cycle.

A-8

I think there is a difference between an assumption (something that you take to be true) and a hypothesis (something that you think might be true). One you can 'test', the other you can't.

A-9

The logic of justification of calculation of long term averages is important to biology and practically. Neither seems to make sense to me.

There is a lot of literature to suggest that it is not the long term average water depth that is biological important. Instead it is much more of a system that flips between multiple configurations, depending on variation in climate. It is not a mean/deviation model that makes most sense to me.

Figure 1-13 the data in fig a13 don't agree with the model. Indeed, they show something else, very different.

A-13. Yes, I agree that hydrologic indicators (average water depth) cannot be used to correlate with vegetation changes. Yet, isn't this what you are doing in this section? The reason is that other factors- primarily fire, and other aspects of the hydrology (which are not captured by average annual depth or range in depths) are as important.

A-14. Table 1-3. It is not clear how these values were derived, either from the table heading or from the text.

Inflows to the Everglades- Aren't these big uncertainties? Even after the careful analyses presented here. For example, there is little mention of the earliest ca 1880 dredging, which connected the Caloosahatchee.

A-18. You do a good job of estimating the flow cross sectional area. Why not use the model to come up with ball park estimates of flow volumes?

In this section (and others) qualitative statements are made such as 'surface flow east and west ….does not appear to have been large'. References for this statement?

A-31, I don't understand how plate 15 helps with this explanation of pre and post drainage water depths. Please clarify.

A-38. Why would this move through convection? Convection is transference of heat through moving fluids.

A-43. Mention of feedbacks among flow, vegetation. Yet this is not evaluated in the model (at least to my understanding)

C-3 – end of paragraph 3. I don't know what is meant by 'support identification of monthly and annual data trends'. How does this affect the model?

C-3- paragraph 4. What is "a data availability issue'?

C-4- what is the consequence of filtering out extreme daily values? Daily or monthly? I wonder what the impact of this sorting is on the model runs?

Is one of the reasons for 'smoothing ' rainfall data to deal with numerical problems?
C-7/8. How robust is this method (TIN) to other spatial methods? It seems as though it would 'smooth or average' the values for the centroids and triangles. Can you provide some comparison with isohyetal lines? Thiessen? Other approaches.

C-9. This is a very different picture than the one portrayed in figure C-18. Is the difference due to the categories (inches of rain) mapped? Or is something else at play?

C-19 indicates a larger average rain along the coast, where c-9 shows much lower. Is this an artifact of the method?

Appendix C-13. I am not sure of the point of this analysis. One is I don't see the relevance to the model. I would much rather see what the range of estimates (uncertainties) suggest for the model output, rather than concentrate of what is the 'correct' or most defensible inputs.

C-15. 4 kilometer resolution- Not sure what that means? The paragraph suggests a grid. Is the grid 4 km x 4 km or 4 km2?

C-16. Why 15 rainfall basins? This is the first time this is introduced.

C-16-17. The figures generated from this formula strike me as very odd and not comparable. Each one has a different 'bin' width. Without similar bin widths, I have no idea what these graphs mean. Also, what is the null distribution against which these values (mean, std, etc) are calculated? Is rainfall normal? It is not my field, but I don't think that it is.

C-27-28- what is the point of these graphs? Is there a hypothesis being tested? Or is this an exercise in data crunching?

D-5. Were crops grown in pre-drainage Everglades? Why use the word?

D-14. I am not sure what any of this analysis means. First, I am assuming that the analyses throughout this section were done using some type of regression approach. This is not appropriate tool!. A regression assumes that all of the error is associated with the dependent variable and not with the independent variable. Yet, errors are associated with both of these data sets. The appropriate comparison is a correlation analysis. Second, with from 4 to 10 years of hourly data, I estimated that each regression involved tens of thousands of points. With so many data points, large correlation coefficient values are expected. In fact, what surprised me is that they are so low!!

D-65 So, I am still a bit puzzled of the 'effects on the model' of these transformations on the data sets. It seems there is a bit of rescaling, and am not sure why this was done; because estimates were too high?

E-10. Not sure how one 'cross-walks' data.

What about other data sources? Ives vegetation map, John Henry Davis? Alexander and Crook?

E-17m Why break the project into these subregions? They don't make a lot of sense to me, at least in terms of vegetation.

E-71. All of the classifications of land cover (vegetation) types appear to me to be very reasonable. I would have liked to have seen more references to past work, rather than these meager few

I have no comments on appendices F, and G- ; outside of my expertise. .

Appendix H. The tidal data are important, but I am not sure I follow what was done. Somehow a daily value was calculated, adjusted for changes in elevation to NGVD or something. It is pretty cryptic, and not easy to follow how a daily value was determined, and how the information was translated to boundary cells around the model.

Appendix I-

As mentioned earlier, it would be nice to have a clearer picture of how the ET 'function' and the flow coefficients work with respect to the vegetation. Without that (forthcoming I see), it is hard to comment. But I did have two comments;

I-1. Growth table. While some seasonality does occur in south Florida, I am not sure about these tables. It seems as though some analog from productivity studies could be used to develop these relationships, rather than Mike Duever guessing (which is what this appears to be). While Mike is extremely knowledgeable, has great judgment, it would be better to see some references. Besides, the only plant that I know is deciduous is cypress, which is probably important in areas of cypress dominance--the big cypress regions and the eastern margin of the Everglades--but none of the rest.

I-11. Also, in terms of the mannings (flow coefficients) I found some work that I had done for my thesis, using the empirical model of Petryk and Bosmajian, 1975, (n= water depth^.67*stem densities^.5). I had pulled out stem densities from a variety of references from – Herndon, 19991, Goodrick 1984, Olmsted 1980, Gunderson 1982. It might be worth a look. This may be meaningless, because I don't know the Copp reference. Let me know if you want to look at my thesis.

Results

Figures 5-13.
I think I understand what the green boxes mean. It is clearer in the hydrograph, but don't understand it for the stage (depth) duration curve.

Page 13- table 3. Am I missing something, or it seems as though cell 1924 mesic pine flatwoods has a 12 month hydroperiod, and a max depth of 4 feet? I doubt there would be many pines.

Page 20. What happened in the 1997 simulation year? Flows went very high in Harney and Shark river?

Page 21. Any idea why the variation? These are all the same landcover types. Is it due to spatial variability in drivers?

Figures22-26 (and figures on page 26) are very nice summaries. What type of variability occurs around these annual summaries?

From Peter Stone

General Comments

Editorial comment on my own following comments

I have very carefully read everything that was sent plus some additional materials from the background items and have previously closely examined the entire draft report on predrainage vegetational and hydrologic conditions. (The lone exception to reading the materials sent to me is the less-related final third of the 2006 NRC report, which I will soon finish.) I have tried to follow and understand every important number and every rationale, with the rare exception of a few statistical formulae that I will leave to others.

I will begin with general or overall comments that summarize my opinions after reading everything, after marking areas of question or concern, and after mulling it all over for a while. I will then re-visit a few of those main concerns and finally go over specific items in about the order I encountered them. I am sorry for the redundancy, but systematic compilation and editing come later *Posted: 05 Feb 2007 02:26 PM*

General comment

There is an enormous amount of material in the main report and appended and supplemental materials that I agree with and need not comment on specifically, but I do want to note that general agreement here. A vast effort has gone into this model. It is quite obviously an honest and well-intended effort. Virtually heroic efforts have gone into the gathering and compiling and rendering fit of some of the baseline data for the model. Finally, the District staff has been admirably open in desiring all manner of comment, reservation, and criticism, not just agreement. *Posted: 05 Feb 2007 02:32 PM*

Main concern

My main reservation or concern—my principal worry about the model—recurred in many parts of the central document as I read it. It relates to uncertainties, especially to uncertainties or approximations in highly important functions in the hydrologic regime, the ones that have such great influence on modeled results (i.e., actual ET, conveyance in various vegetation), but including as well the considerable uncertainty that remains (much unavoidably) in historic predrainage conditions, including merely water levels (despite old depth measurements being among the most-certain data from predrainage times). *Posted: 05 Feb 2007 02:39 PM*

Dealing with the main uncertainties

Some of these uncertainties can be much reduced by further measurement (e.g., applied research on the hydrologic factors) and some must be dealt with simply by a clear recognition of the inherent uncertainty, especially by users of the output. Most especially it should by recognized by decision makers who may be organizationally distant from the deeper understanding and workings of the model. I did not see nearly enough mention in the main document as to how this basic underlying problem of approximation and uncertainty will be dealt with, perhaps most especially dealt with in a way that it cannot be ignored by eventual users. The impression left is that some of the most important values are now "good enough." I know that cannot be true. How the model will be improved in the future needs some specific discussion. This will have two benefits: (1) the interim readers (and users) are better warned of the present approximations, and (2) the agency becomes committed to pursuing specific improvements. *Posted: 05 Feb 2007 02:42 PM*

Dealing with the main uncertainties

My concerns about the uncertainties (e.g., approximations in input values) were considerably allayed when I read the contract study on uncertainty/sensitivity analysis plus the emphasis placed on adaptive management in the 2006 NRC review document. These do not solve the present degree of uncertainty in the model but they do show ways to recognize, address, reduce, or deal with it. This can work only if these paths are energetically and willingly followed. Too often, "It's good enough now to start with" becomes in management's mind "It's finished" and thus to an implied "It's perfect" (this also is a problem when more funds are wanted at lower levels for improvements). Recognition from the start that more is needed is important. I think considerably more work should be undertaken to (1) reduce uncertainty in the most important factors (especially ET and conveyance in vegetation: some specific comments will come later), (2) evaluate the effects of uncertainty (many more scenarios such as were shown in the uncertainty/sensitivity demonstration study), and (3) assure that uncertainty and sensitivity are well understood by users.

It is assumed that the model will evolve over time as inputs and calibrations get better and these concerns, except #3, need not hinder its early appropriate use. #3 requires, though, that end users know that results are central tendencies, or at best they are most-probable conditions, and that an uncertainty value must be well recognized and preferably known numerically (even if itself only approximately). More generally: how models have been and are misapplied (usually but not always unintentionally) should be thoroughly and explicitly understood by this model's users (and users of all important models) to avoid the same. Many of the aspects in this comment by no means preclude early effective use of the model, long before some of the farther refinements, so long as the uncertainties are recognized and understood. *Posted: 05 Feb 2007 02:58 PM*

Archiving changes

If this model is to evolve or improve over time, as it must, then there should be built into the system some way of archiving the calibration values of important old versions that have been changed (I may not be using "calibration" precisely, but I mean the constants and look-up tables or even equations and such that are subject to change). *Posted: 05 Feb 2007 03:00 PM*

Building expertise on and extracting more information from the model

Expertise beyond numerical-modeling expertise needs to be formally and continually applied to the model. This will likely come mainly from district scientists and engineers by their experience in actual use of the model in real problems. It should begin much earlier, however, that is, now. Much of this can be by regular hydrologists well trained in the fundamentals and need not be modelers. An analogy: a great new design for a sailboat and its construction are steps one and two; learning to recognize and make use of its advantages and recognize and limit the effects of its quirks is the next; this does not require a naval architect or shipwright and is probably best not done not by one. This is needed in all aspects here, with the model, but was made most clear in reading the uncertainty/sensitivity study's brief discussion of results. Why (physically) certain results were obtained was obvious to the writers (and to me) in many cases and we agreed for most of them, but there was some that could be disputed and, more importantly, more information that could be extracted even from those few examples. This effort—made alongside more sensitivity evaluations and actual applied use of the model—would greatly inform users and those charged to help users as to how the modeled system behaves and why. It has considerable potential to tell how the actual physical system behaves and behaved as well, especially in its details. Much of this will be merely conceptual analysis. It will greatly increase the understanding of the modeled and actual system, one of the ultimate objectives one hopes (i.e., beyond getting simple predictions).
Posted: 05 Feb 2007 03:07 PM

Another editorial comment, which should have preceded

Aspects of these (my) general concerns come from experience in seeing models used for high economic stakes elsewhere and mostly relate to future use and not to specific statements in the reviewed documents.

Most or all of these general concerns or suggestions are probably already well recognized by the district staff and likely already have been considered in their planning. I mean no slight by stating the obvious. Let my noting them now simply be a call to mention or treat them explicitly in the main document (or perhaps more emphatically and in more detail if I have missed them). Posted: 05 Feb 2007 03:18 PM

Editing and possible source of editing suggestions

The main document is hard to follow in several places due to names and concepts/terms and abbreviations and such that have not yet been identified or described sufficiently. Some hopping around was needed (forward and then back) to find some of these items and not every one was found. This is to be expected when those thoroughly familiar, i.e., experts, do the writing, as they must. It also appears to result from using extracted portions of related documents compiled into this new one. Now the main document should be carefully read by well-trained hydrologists that have had nothing to do with its writing and development. Have them mark everything they do not recognize or, more importantly, do not immediately understand, when they encounter it (as well as make any other useful suggestions to help the first-time user). This, I believe, is not exactly the same as the technical editing that is to be contracted, which is also necessary. Maybe have some new employees who you want to be familiar with the model anyway do some of this type of reviewing. *Posted: 05 Feb 2007 05:17 PM*

A disclaimer on uncertainties

I want strongly to note that none my comments or my overall concern with remnant uncertainties (those readily reduced and those not) is meant to imply that these are viewed as a failing in the model or in any way a crisis. Unfortunately, recognition of limits (or sometimes worse, weaknesses) in a complex system can easily be misinterpreted by the lay public as a terminal condition, as reason for rejection. One need look no farther than the present consternation over global warming to see that lack of incontrovertible proof is not rarely taken (sometimes dishonestly) for a lack of strong evidence, especially when someone doesn't like the inferences. That the last will occur at some point with this model is almost a given. A strong overt recognition by model users of its inherent uncertainties is, to me, absolutely necessary for effective use. If this clear recognition will raise problems elsewhere, then alongside it, and because of it, perhaps some groundwork should be laid for explaining modeling with uncertainty to the lay person (technically), which will include many officials.

The saying, "Don't let the perfect be the enemy of the good" has much to convey here. The model can't be perfect and it doesn't need to be. The example I like to use is the classical aquifer pumping test. Look at the list of assumed conditions for the equations, then contemplate how few are truly achieved in any real-world situation, and finally try to guess at how vast is the number of successful water supplies that have been designed with the results. It is plenty good enough, in most cases, and where it is not is well recognized by the skilled user. *05 Feb 2007 06:02 PM*

Comment on the review and on my comments

The instructions received have been both broad and specific, from verbal explanation that basically (almost literally) said "Read everything and comment on anything," to the written and then verbally repeated request to provide specific ("bullet point") questions for a district staff response. I will attempt to do both. Many points are comments and not questions. Most questions are rhetorical, standing for "Have you considered?" I will add and assemble "bullet point" questions for the most important direct ones.

I find the teleconferences necessary but unavoidably clumsy, difficult even for getting out general comments or concerns (the verbally experienced professors perhaps find it less so). It is hard to cover much or to consider anything finally resolved there and thus some points made by me or others are repeated in my written comments.

The verbal requests to focus mainly on the principal report document and to make suggestions for improvements implies that the perspective of several classes of readers should be considered, especially outside readers or users, including critical and perhaps even skeptical or suspicious ones. Allaying confusion (up to suspicions) of lay interests groups is, to me, an important goal, along with communicating completely and coherently to the technical reader or user. I work at an agency that uses models and one that receives modeled evidence from outside, both evidence honestly prepared and that not. The interested public has a right to fear misuse of models (unfortunately) and to have those fears allayed.

In summary, my comments will be broad ranging and often general.

Finally, but importantly, I mean no offense in the many restatements of the obvious. I expect most of the general comments have been thought about already. Let me then merely re-emphasize them. *Posted: 13 Feb 2007 04:51 PM*

2. Natural System Hydrology / General Comment / Springs

2. Natural System Hydrology
General Comment
Spring inputs to the Everglades

There is no physical evidence whatsoever for a substantial spring in the Everglades interior (i.e. away from its immediate eastern edge along the coastal ridge) and no credible report of one. It would have to discharge from the Floridan aquifer to be substantial and we know what a profound effect that mineralized water would have on downstream vegetation and sedimentation by the effects of the long free-flowing of the old oil-exploration well at Grossman's Hammock (NE Everglades National Park). People imagine or invent wonders for the interiors of remote areas: witness the supposed Wakulla volcano of a century ago in northwest Florida. What Willoughby saw is harder to discount, but must have been from very local sources given the low elevation and large distances to any "uplands" in South Florida that could have acted as a major recharge area. Water that went into the limestone within a few hundred meters to a kilometer or so upstream is far the more likely source. The situation in present upper CA2A is a limited analog, where higher water in nearby CA1 (the cut of Hillsboro Canal) raises the head in the uppermost limestone (recent work by the Duke University team showed an upward gradient in nearby northern CA2A). Peat and marl must act as a semi-confining layer. This also must help explain the reported dry-season drainage across marl and into a tiny sinkhole in the SW Everglades. This hydraulic impedance to vertical flow was noted for the Everglades peatland by soils investigators of the early 1940s, especially for the subpeat marl (the "tomato can" bailing incident that Chris McVoy and others will know the source of [Florida Geological Survey or Soil Science Society of Florida article with "Jake" Stephens as one of the authors]). No true perching was evidenced though. Whether this source and degree of impedance warrants inclusion in the model's ground-water calculations deserves some brief consideration, though it may well be inconsequential. *Posted: 13 Feb 2007 04:55 PM*

2. Natural System Hydrology / General Comment / Ground Water

2. Natural System Hydrology
General Comment
Ground-water input to the Everglades

Ground-water input possibly did have some limited importance to Everglades hydrology, perhaps especially to the eastern marl prairies flanking Shark Slough in Everglades National Park. The model may be a good tool for revealing this. A recharge mound builds within the coastal rock ridge there (Miami and to the S and SW: Parker et al., 1955, noted this decades ago but it would be predicted in any case). If surface waters in Shark Slough receded faster than the recharge mound drained eastward into Biscayne Bay (or Taylor Slough, etc.) then perhaps the rocky marl marshes stayed wetter than water levels in central Shark slough (the peatland axis) might otherwise imply. That is, wetness in the marl flanks may not have been totally controlled by Shark Slough water levels and direct rainfall. A critical ecological effect may have occurred in the dry season. To a lesser extent this in-seepage condition might apply to all the edges of the Everglades where there was not active flow out of the Everglades. The steeper the upward topographic gradient outward, the greater the probable influence (except at Lake Okeechobee where obviously lake level and not the lake bottom is the surface of concern).

More overt influence of ground-water seepage/surfacing might exist for Hungryland and Loxahatchee Sloughs and the Allapattah Flats (and, as mentioned elsewhere, from the southern extension of the central Florida ridge east and west into the Kissimmee River and Fisheating Creek drainages). *Posted: 13 Feb 2007 04:58 PM*

2. Natural System Hydrology / General Comment / Water Depths

2. Natural System Hydrology
General Comment
Prevailing water depths and ranges in the pre-drainage Everglades.

These are known only approximately, not with any great precision or accuracy (as pointed out in the document text) even though they are critical information for restoration. There may still be ways to reduce these uncertainties. Presented are several sources or types of evidence: (1) recorded and reported depth measurements, (2) observed or estimated ranges by early observers, and (3) general constraints inferred by the ecology of reported vegetation. No. 1 gives the most precise data, but these are highly scattered in place and time and also have the strong possibility to bias toward deeper water (of sloughs, where boat-borne travel is focused, and possibly even by lack of travel in dry years, or travel only to wettest places in dry years). Some of the data are hard to envision without such biases (some quite deep water in normally low-water months). No. 2 may be the strongest evidence for the direct purposes here, but is the most general (the estimates of ranges by interested early observers); this range is then positioned vertically by more tenuous assumptions (low-water and high-water depths at slough bottoms and ridge tops. Furthermore, assumption is made of greater local relief in the past (slough bottoms to ridge tops). Is this strongly evidenced somehow? Does it significantly influence the model performance (or does it average out in flow characteristics)? No. 3 has the advantage of much wider direct application by the greater information on areal distribution of vegetation. With vegetation, though, boundaries may be at one hydrologic threshold or the other (wet or dry) and conditions varied across the areal extent. Nos. 1 and 3 seem to have potential for useful future work for refinements reducing uncertainty, i.e., better approximation.

Regarding No. 1, can the time-points of the depth data eventually be assigned to wetter or drier years (as well as by season)? Can data from long-term South Florida rainfall stations be correlated well enough with still older stations not too far away (Tampa? Nassau? Havana?) to push the time range of assessment of wet vs. average vs. dry years back farther to better "weigh" the old water depth observations?

Regarding No. 2, does ecological data (especially for sawgrass and white water lily) exist that will more precisely constrain the previous hydrology of sawgrass plains and ridges and water lily sloughs? Or stated differently, are the approximate ranges and depths presently used (that were derived ultimately from some ecological studies in the altered system) good enough for the intended purposes? And are they good enough merely to start or good enough overall? Restoration efforts likely will be producing better information of plant community tolerances/preferences. The main plants at the edges of their dominated communities were presumably at the edges of their tolerances (at least under conditions of their competition). The District possesses data that relate here, for instance the recorded hydrology of central CA2A (2-17 gage) for the years that sawgrass was edged back, presumably by greater flooding. What does that tell one of the likely hydrologic regime of the edge of the original sawgrass marshes at the western and northwestern shore of lake Okeechobee. What does that tell one of former lake level? The newer sawgrass marshes of northwestern Lake Okeechobee come with similar hydrologic records, at least via lake level. Ultimately the question is: is there any reasonable way to further constrain the typical or average flooding regimes? *Posted: 13 Feb 2007 05:02 PM*

2. Natural System Hydrology / Overall General Comment

2. Natural System Hydrology
Overall General Comment

Great work has been done in the historical reconstruction or the Everglades system. Very good deductive reasoning has been applied to the Saint Lucie area where so much less recorded evidence is yet available.

However, much detail should remain open to reasonable challenge. *Posted: 13 Feb 2007 05:05 PM*

2. Natural System Hydrology / Specific Comments

2. Natural System Hydrology
Specific Comments

A-3 bullet #5 editorial

"Seasonal" rather than "bimodal?" The latter is usually used for June and August having higher average rainfall than July in some parts of the region.

A-5 P 1 minor issue

The long-term existence of the Everglades with very generally the same plant communities but under greatly altered slope through time (by peat deposition) challenges the “tuning” interpretation. How buffered (sensitive) is the hydraulic system to alteration in slope? Rainfall? Lake Okeechobee’s contribution? The model may very well help to tell.

A-6 P 1 major/minor issue

A lot of inference goes into adding support to the direct measurements and estimates (made historically) of how deep the sloughs got and thus, overall, the hydrology of the marshes. The inferences have varying strengths. Their importance varies as to how importantly the estimates are used. The wetness of tree-islands in some part of the Everglades is weakened by the many types of tree-islands (it would help to know where), though admittedly the two-tiered type with a distinctly elevated hammock “head” is widespread. This is a major issue now only if it is inconsistent with estimates of average conveyance (later, obviously, it relates to degree of restoration).

A-6 F A.1-1 minor issue / editorial

Baldwin and Hawker’s data are not “predrainage.”

A-7 P 4 minor issue

Where is any strong or direct evidence for “flattened topography” except for reports of tree-islands burning? Can we actually safely assume that sawgrass ridges are now commonly lower in most areas or sloughs partially infilled? Has anyone compiled the long-term records of Everglades muck fires since drainage?

A-7 “1 minor issue

These are correctly labeled as “hypotheses.”

“Uniform throughout” may not be accurate even approximately. The northern edge of the sawgrass plains conceivably could have been near their dry threshold and the southern edge near the wet threshold.

A-8 “2.” editorial

“miD-October”

A-8 “3.” major issue

The 2-foot range has some obvious challenges and possibly is on the low side (e.g., several of the historical estimates of ranges go to 3+ feet: Table A.1-1). This is discussed and the 2-foot range supported elsewhere in the report with inferential evidence, but no strong evidence seems to preclude 2-3 feet. Can, say, a 2.5-3-foot range safely be discounted? Maybe the high end is considered more of an extreme (more like log-normal distribution).

A-8 P 4 "(1)" major issue

The assumption of leveling of peat in the direction transverse to flow is, I think, tenuous. Major sub-regional swales (roughly N-S) were shown in the NE and SW Everglades in ca. 1945 reports and some are shown along the sides the northern Everglades in the topographic reconstruction map in the model report. Part of the indications in the 1940s conceivably may have been from errors in computing subsidence in the intervening terrain, but the slough-filled swales that remained were themselves somewhat less-impacted by early drainage and are relatively "natural." There is no obvious strong reason to discount those large shallow swales as natural features, certainly no simple reason such as water will tend to level out. They could have received more input water (lineations marking flow directions suggest this: Fig. A.1-4) with more or less level water across the Everglades (W-E) or they may have simply directed more water by being deeper, with water remaining more elevated on a slightly elevated axial interior by direct rainfall, the considerable resistance to overland flow over long distances (especially with shallow water and especially E-W against ridge-and-slough "grain"), and with Lake Okeechobee delivering considerable discharge near the center axis of the Everglades. Flows directions in Figure A.1-7 give support the reconstruction of the northern swales, as does the southward central protrusion of the reconstructed sawgrass plains. But what of the swale formerly interpreted for the southwestern Everglades and into the axis of Shark Slough?

Returning to peat-surface leveling transverse to flow, even in just considering the elevation of sawgrass ridges in the ridge-and-slough terrain aside the southward protrusion of sawgrass plain, it may not be safe to conclude that sawgrass land was level E to W. More generally, why would such a wide (~40-mile) peatland be interpreted to be level transverse to flow?

Part of what makes this important is that a swale on the southwest side of the Everglades has strong implications to conditions in Shark Slough in Everglades National Park.

A-8-to-A-9 sentence minor issue

Does not the "squeezing" of Everglades flow into the narrower (at least for the width that receives peatland depths of flooding and hydroperiods) flow channel of Shark Slough challenge some assumptions regarding uniformity of hydrologic conditions across the main vegetational landscapes? Or else the larger losses to the transverse glades upflow, minus the smaller inflow from Mullet Slough (and elsewhere), would have to have been accommodated (negated) very closely and coincidentally by the narrowing of the flow.

A-9 P 2 minor issue

Why is it important at all to use the tenuous simplification of water stages as linear plots between seasonal highs and lows?

A-9 P 3 Line 7 minor issue

Considering 25-50 year averages as ecologically controlling runs a bit counter to mention (elsewhere) of fairly rapid vegetational changes under regional drainage. Degree of shift multiplied by time must play some role in degree of final effect, but even there it is probably not a linear relationship.

A-10 Table A.1-1 editorial

Smith's 1848 estimate of 6 feet sounds like he meant to depth rock when read in the original.

Rogers' and Mallory's data are missing

A-11 Fig. A.1-3 major issue

How does one interpret the deep to very deep water values (>/= 2 feet) in the dry season? There is something we do not understand here, in some of the most direct and applicable data.

A-11 P 1 minor issue

The assumption of 1-foot maximum depth for the western edge of the marl transverse glades seems open to ready challenge. Marl formation is probably fairly insensitive to maximum water depth, in part because sawgrass is not prone to forming dense (shading) communities on rock or marl soils.

A-11 P 2 last sentence major/minor issue

The 2-foot depth range is, as is stated, consistent with much other evidence, but still does not seem to be tightly constrained.

A-12 Table A.1-2 minor issue/editorial

"*** = 'Not corrected for porosity'"

I can guess at the meaning (and I think it is mentioned somewhere else in the report), but should it be stated here? More importantly, does this imply that all the other ranges have been so corrected?

A-12 P 2 Line 4 editorial

"maximum slough water <u>elevation</u>"

A-12 P 2 last sentence minor issue

We have no good surviving analog for the custard apple swamp. It was unique. Can we safely assume from other information that it probably represents a deeper environment than for sawgrass?

A-13 P 4 Line 9 (2nd from last sentence) editorial

The "generally" (used twice) probably needs a little more explanation as to where ("All but southern ends of the WCAs?).

A-14 Table A.1-3 minor issue

Marl marsh is shown as mostly "wetter" than sawgrass marsh by high-water depth but "less-wet" by hydroperiod. This is not impossible but is it likely, or what does it show?

Also, the closeness of conditions of the several communities challenges these simple measures of hydrology as reflecting the ecological controlling factors. (The risk of course being: If you 'get the water right' [by these criteria] it may not come.")

A-14 P 1 "Inflow" major/minor issue

The lack of linear patterns pointing from adjacent uplands into the Everglades is not necessarily evidence (and thus is not strong evidence) against significant inflow off the mineral-soil "uplands east and west of the Everlades. Slow surface flow may well have been distributed (diffuse) or short enough seasonally not to leave a mark on sandy terrain. (Plus there are lineations suggesting inflow in places, noted below.)

Similarly, while ground-water input may have been significant along some edges of the Everglades, it is very unlikely to have been substantial. It's greatest likely effect was where the highly permeably Miami Limestone ridge and Everglades Keys abut with relatively steep slopes directly against the southeastern Everglades.

A-14 P 2 Line 8 (3rd from bottom) minor issue

Regarding "formation of the soil," the custard apple swamp probably had nothing at all to do with the deposition of the muck and very possibly was not there when the muck deposited.

A-16 P 2 Last sentence minor issue

The muck more likely was leveled by deposition, not biochemical degradation.

A-17 P 5 minor issue

Are the "dead rivers" safely assumed to have been inconsequential? The longest might easily have been (depending on surrounding topography) more important than various ground-water flows mentioned elsewhere. That these channels existed or persisted suggests some significant flows (which could have been into the lake at times).

A-18 P 4 Line 1 major issue

The idea that Lake Okeechobee flowed into the Everglades most of the year (mainly through the poorly described/recorded and thus poorly understood custard apple swamp) is derived mainly from topographic and ecological data from the western, not southern, shoreline. It is strong evidence, but not direct. Inferences from this evidence involve knowing very precisely the elevation of the southern shoreline, which lay on organic sediments and was a swamp, out of sight and perhaps hard to survey in a manner to reveal microtopography (as mentioned). I would consider conditions in this area still insufficiently known considering that it is interpreted here to be a major inflow area to the Everglades and that mid-20th Century investigators have left records instead downplaying the lake's influence. The model might be used to test the sensitivity of downflow conditions to conditions at this long-gone shoreline. (It may be that even if this shoreline band were in hydrologic effect [say by microtopography] a foot higher, or had high flow resistance in the intervening foot, that Lake Okeechbee simply overcame it by rising farther because outflow to the west was the real limit. This is simply conjecture but shows how the model itself might reduce some uncertainties.)

A-18 P 4 minor issue

Brown and Godden might well have been responding to the highly publicized "drain the Everglades" campaign and to knowledge (perhaps even signs) of its actual physical start rather than to any actual observation of environmental changes from pre-1905 drainage near Lake Okeechobee.

The wider downstream effects of the post-1880 pre-1905 drainage still seems conjectural. There must have been some effect, but do we have actual record of it. Small's experiences on Lake Okeechobee ca. 1913 (1914? I forget) vs. ca. 1917 (1919?) gives real challenge to there being large widespread effects pre-1905.

A-18 P 6 major issue

High-water flow off the adjacent sandy "uplands" into the northern Everglades does not seem to be safely discounted simply by lack of oriented vegetation. On general principles such runoff seems likely (slope, shallow water table even in the dry season [the cypess domes], and abundant rainfall). Ives had no way of knowing where the water came from.

At the extreme northeastern side and on part of the northwestern side of the Everglades, lineated vegetation definitely does suggest seasonal surface-water inflow (Fig. A.1-4).

A-19 P 3 major issue

It is hard to resolve "credible" with "large groundwater discharges" into the Everglades. There is simply no physical evidence at all. A 105-foot deep sinkhole in the middle of the Everglades now evidences no visible flow. If it doesn't flow, what would? How could it have stopped flowing under imposition of drainage? A reason could be contrived, but how likely would it be?

Discharge in places of extremely shallow ground water of the water-table aquifer (perhaps merely by lateral oozing) may be a different matter. King apparently saw intercalated marl layers or areal patches of marl in the peat (both still exist) west of Miami. These very much need explaining sometime (not here) and are hard to interpret as to geochemical source given the reconstructed flowlines. Very shallow ground water seeping westward off the limestone coastal ridge is a plausible source.

A-23 P (2^{nd} from bottom) minor issue

Physical flushing of organic matter from cypress strands doesn't seem very likely. Velocities are low generally (though maybe there were occasional extremes), the material doesn't float, and where would it go? We simply do not know much yet about what is required to form peat. Perhaps these strands were often dry where the organics could oxidize, or perhaps litter burned regularly. Conceivably even, these were fairly newly flooded by the slowly rising peatland and water surface of the Everglades.

A-24 Figure A.1-7 major issue

Why is ridge-and-slough not mapped farther north on the west side where there is strong indication of directionality?

A-28 P 3 minor issue

Were rapids not at sills of some sort, or were they simply at breaks in slope?

A-29 P 6 (bottom paragraph) minor issue

Sills on the marl transverse glades probably do indicate that Shark Slough waters commonly reached that high. Water at the sill might not be proof positive of Everglades water being that high though. It is not beyond possibility that water flowed westward from the transverse glades, to a slightly lower Everglades water level. Flow could have been fed by the recharge mound of shallow ground water beneath the adjacent coastal ridge. This is not likely at all to be the general condition, but here is just used as another example of plausible alternative interpretation giving rise to uncertainty. Thus direct recorded observation of eastward or southeastward flow is the real evidence, not the presence of marl at the sill: that latter instead proves it was regularly long wet.

A-32 P 2 editorial

Gleason (1972; Gleason et al., 1974) first described in detail these peat-marl interlayers in Taylor Slough. He may have been the first to describe them at all (I forget whether Craighead or the soil surveys encountered them).

A-35 P 2 major issue

The outflow of ground water from the Everglades is still a subject filled with uncertainty and is by no means "well established" in the sense of being beyond reasonable challenge. The people who originally interpreted it were without direct information or conceptual knowledge to be able to know. It may well have taken place and the model will be a good tool for evaluating its former likelihood. It should not be assumed. Recharge mounds on the coastal ridge may have acted as a hydraulic (not physical dam) with all the springflow to the east coming from recharge on the ridge, not from Everglades water flowing through or more deeply beneath the ridge. It is at least possible that not even the Biscayne Aquifer flowed from an Everglades recharge area to offshore discharge area. If this shallow aquifer is very poorly isolated (confined) beneath the ridge it might have flowed instead from the ridge westward into the Everglades (this being the main plausible mode of any ground-water or spring discharge occurring at all within the Everglades interior).

A-36 P 1+4 major issue

Regarding an "active groundwater connection between the pre-drainage Everglades and the Atlantic Coast," see above.

The connection is not only not quantified, it is not yet proved or even suggested with high confidence. The termination of spring flows after regional drainage suggests an underground hydraulic connection with the Everglades but not previous flow direction or regime.

The assumption of eastward flow beneath the ridge shares many of the deficiencies in evidence as for springs in the Everglades interior: vague suggestion at best, hearsay at worst. Let the model eventually shed some light.

A-37 Table A.1-5 minor issue

Regarding, "no directional patterning": I believe that tree-island tails show strong downflow directionality in some marl marshes (Spackman's early work or maybe it was White's "geomorphology" volume shows photos. I have not looked widely.)

A-38 P4 major issue

How is conveyance anisotropy in the ridge-and-slough area to be assessed? It would be expected to vary with water depth (with least difference at high water) and the difference (for high water vs. levels closer to the runoff stagnation threshold) would be influenced strongly by local relief. This then returns to the question whether the current least impacted ridge-and-slough areas (best analogs to the original system) have been "flattened" and what was the original local relief? The answers used in the model are (apparently of necessity) still pretty inferred. Note that a strong degree of anisotropy can allow a greater variation from level of the water and peat surface in the direction transverse to flow, and specifically could help maintain long-hydroperiod marsh (water lily and sawgrass) in the axial part of the Everglades basin even with shallow troughs toward the edges (as interpreted by others from ca. 1940 conditions).

A-39 P 5 editorial

"Consequent drainage" probably has no valid meaning for an accreted peatland surface.

A-40 Figure A.1-1 major issue

Why is ridge-and-slough not interpreted for the strongly lineated area of the northwestern Everglades?

A-41 Figure A.1-8 editorial

Is this not in fact "based on" rather than "consistent with" narrative accounts?

A-42 P 2 (2nd to last line) minor issue

The accreting peat surface does as much to level the water surface as vice versa.

A-44 P 2 editorial

In discussion of a hydrologic model, "flow paths" might well imply flow lines, which cannot divide. Maybe "broad flow paths" at first mention?

A-46 P 1 minor issue

Ground-water flow southward from the sawgrass plain at low-water times is really not feasible as a significant factor because of the large distance involved. What could be significant in this regard is some lateral flow of the shallowest ground (really soil) water into nearby rills or minor sloughs, with then surface transport southward out of the sawgrass area. Harshberger (ca. 1914) I believe noted occasional and then increasing openings in the sawgrass (before reaching ridge-and slough?) on his trip southward. These openings may as well explain some of the lineations shown for just north of the mapped border of the ridge-and-slough area (Figure A.1-7).

A-47 P 1 minor issue

The year-round discharge of rivers heading up in the peat transverse glades may not be strong evidence for continued flow eastward from the Everglades. The strength of evidence would vary by river and depend on its surrounding geomorphology (and could be assessed this way). For example, the branches of the Middle River near Fort Lauderdale would probably have flowed year round with no Everglades contribution to its headwaters. Normal stream baseflow from the closely adjacent sandy coastal ridge would likely have kept it flowing.

A-47 P 4 Line 13 minor issue

The nutrient pulse to local roots likely took place during the dry season, not the beginning of the wet season. The onset of flow could move it much farther though.

A-61 Plate 36 minor issue/editorial

As mentioned in the report elsewhere, "scrub" is probably incorrect (it is good that you are correcting this interpretation, which has stood for too long). Do you not want that noted here on the figure caption?

I grew up (50s-60s) in a part here clearly identified as “scrub” but the remnant very large old pine trees scattered in the neighborhood were all slash pine, not sand pine.

A-78 P 1-3 minor issue

A shallow low-permeability layer may or may not promote ground-water contribution to streams. It might instead promote runoff and reduce ground-water input. It depends largely on the geometry (topography/bathymetry).

A-78 P 1-3 major issue

How will this common occurrence of a shallow low-permeability layer be handled in the model? *Posted: 13 Feb 2007 05:09 PM*

7. Model Results / General comments

7. Model Results

General comments

The results shown are very encouraging. They are almost surprisingly good, for the Everglades region at least, especially considering the complexity of the modeled region and the possible multiplication of errors downstream. That said, there are some overall aspects that need attention and areas where readers of other types (especially skeptical or distrustful ones) might find fault or reservation and thus should be accommodated ahead of time.

Excellent results garner suspicion. Better description is needed of:

(1) how the indicator cells were selected

(2) why 90^{th} percentile was chosen for average maximum and minimum water level in some illustrations (all?, I myself am confused in places) (“why” is intuitively obvious, but just say why to avoid any remaining wondering)

(3) and especially, how the demonstrated data fit into the whole set of data, say by major landscape feature.

(4) whether the model was “tweaked” (and if so, how) or are the demonstrated results the straight-forward output from the theoretical and empirical methods and values described in the report?

For #2, perhaps add illustrations from a few <u>randomly selected</u> cells (and declare that) showing all (or maybe a much finer-spaced assemblage) of the original data and where the 90^{th} percentiles lie.

For #3, there is really needed a much larger assemblage of cells that more assuredly cover the range of results in this validation or demonstration data for each major landscape (here, ridge-and-slough). Show all of them. If computational time is a problem with that, show every fourth, tenth, or whatever can be handled readily. Or maybe 100 randomly selected cells.

For #4, one should very briefly include such things as why different parts of the measured range of conveyance was used in different areas of the ridge-and-slough landscape (verbal information in teleconference)

In summary, build stakeholder confidence.

It is obvious in presented results (mainly from outside the Everglades) that individual cells can give extreme and implausible results (perhaps impossible). Obviously the specific reasons for such results need to be tracked down and that knowledge reapplied to the model as a whole. If these are not spurious and simply errors that can be corrected, then--very importantly--they begin to tell at what areal scales the model can be given confidence or little confidence. How will it be eventually determined whether 4, 16, or many more cells should be averaged (by some criterion) to give predictions for specific areas of high interest. Ultimately, what is the minimum area one should give confidence to in model output?

The extreme and implausible results shown come from the area outside the Everglades and notably include some levels far below ground. This implies that it is likely a problem in computing ground-water flow (drainage). Is this problem wider spread and simply not as noticeable elsewhere (say wrong transmissivity, but noticed in extreme just near where an incised body of surface water provides a drain)? Some cells with implausibly low-water levels are near surface water (i.e., 2413 and 6498, pg. 15).

The above point returns to a general comment made elsewhere: sensitivity behavior and unusual/implausible results should be closely examined qualitatively by staff hydrologists, to build expertise in the modeled system's behavior and performance.

As mentioned in the report, some of the results track low in the predicted range of depths. Could better additional validation be obtained for the natural system by using an artificial "average 35-year record" constructed by some simple additions to the actual 1965-2000 record, to retain natural rainfall variability but obtain average decadal rainfall?

A final main but general question remains regarding how well the model is validated by the encouraging output, from the Everglades region especially. How readily could it fail badly? If a region is nearly flat but sloped enough to drain and has some vegetational resistance to flow and abundant rainfall input, can the degree-of-flooding results be far off even with marginally accurate modeling? Sensitivity analysis again seems key.

Can the needs of other future users be readily accommodated, say for example (1) someone who would like to have an estimate for total natural inflow of freshwater inflow into Biscayne Bay: with river, marl transverse glades, and ground water, or (2) into Florida Bay by surface water?

How will challenges to the highly derived modeling results be incorporated into modeling expertise or into the model itself? For example, if reliable paleosalinity data from Biscayne Bay or Florida Bay challenges modeled reconstructions?

Along these same lines: one of the most important parts of the remaining Everglades (to the national public probably the most important part) is Everglades National Park, at the end of the long flow lines, recipient of the cumulative error of everything assumed or computed for upstream. Can the model readily be adapted to meet their needs if, say, they would simply like to assume some different inflow from upstream (say, specify distributed flow across the latitude of Tamiami Trail or Alligator Alley)? Other modelers might rather specify output from Lake Okeechobee rather than compute it from calculations and assumptions from the Kissimmee River valley, etc. *Posted: 14 Feb 2007 05:05 PM*

7. Model Results / Specific Comments

7. Model Results
Specific comments

Pg. 1 P 2 and Fig. 1 major issue

1965-2000 does not look "drier than average" in Fig. 1. For one, nearly all of it is plotted as positive in "annual deviation from measures of central tendency" (which needs explanation). Secondly, "eyeballing" of the entire data set suggests a value of ~20-25, not zero for a mean for these data. I am missing something, thus so might other readers.

Is 1965-2000 in-fact of lower rainfall that the average of the entire record?

Pg. 2 First bullet major issue

How were the monitored cells selected and assured to be representative?

Pg. 5 P 1 editorial

Varying between 1 to 2 foot depth gives a 1-foot range.

The relation between average and actual depths in the mesorelief of the ridge and slough is hard to keep track of in a number of places. A suggestion will be made below regarding a figure.

Pg. 5 P 1 and Figure 4. minor issue / editorial

"Long term average seasonal water levels" in text and "annual average" in figure title should be more specific. For example, "average high and low water levels" for the text? And are these 90th percentiles? The figure is even less clear. "Dry Season" and "Wet Season" should be "high water" and " low water" or something similar instead. They are not averages for the whole seasons, as the figure now implies.

Pg. 5 Figure 4. comment

Excellent agreement and for many and widely scattered cells.

This should eventually be put in context though for the entire ridge-and-slough landscape (see general comment).

Pg. 6 P 1 minor issue

"Subset" triggers the skeptic (and properly so). One should tell somewhere how these are selected and how they are representative.

Pg. 5 Fig. 5 and ALL figures for the ridge and slough. minor issue / editorial

Marks on the Y-axis where the slough bottom and ridge top lies (on average) would really help. A separate figure showing this relationship schematically would help. At this point I'm confused as to whether the bottom green line on the broad rectangles are slough bottom or average elevation for a ridge-and-slough cell. Readers should not have to search hard for this clarification.

Similarly, the reference values in text and tables are given in months, not percentages of a year, so a months/year scale should be added to the X-axis.

Pg. 6-10 Figures 7-13 comment, but important issue

These results are very encouraging and appealing and are strong evidence.

It would be stronger evidence given some better idea of how representative these results are among stations (cells)(their reasonable spatial distribution and representation are shown).

Pg. 13 Table 3 (and later tables on following pages) major issue

Obviously some cells (e.g., #1924 on this page) can give erratic, implausible, or impossible results. How will this be handled?

Pg. 18 Table 8 major issue

The Kissimmee River is shown to decline in flow in the last three stations downstream. Probably no appreciable stream in South Florida is a losing stream. What explains this result? (Nonchanneled floodplain flow?) Is the K3-K1 difference significant for modeling needs?

Pg. 18 Table 8 comment

Lake Okeechobee outflow to the Caloosahatchee River is shown as ca. 80% of the inflow from the Kissimmee River, the principal influent stream. This probably shows how sensitive the computed flow into the northern Everglades is to properly computing the outflow to the Caloosahatchee River, and that latter calculation is probably only approximate given the more complicated geometry and vegetation of the upper Caloosahatchee River valley (rapids, channels in marsh, Lake Flirt as a "mud hole," and Lake Hichopee possibly half-marsh).

Pg. 19 bottom + pg. 20 Figs. 17-19 minor issue

The measured flow is with a huge altered and managed system lying upflow of it while the simulations are (I assume) for natural condition. Are the time periods of large disparity in the plots (notably for Shark River, Fig. 19) related to management?

Pg. 21 p 2 + Fig. 20 major issue / editorial

Does the range of "observed values" of ET and in the green lines have some systematic variation (say, by variability in the vegetational mosaics tested, area of the Everglades, or by year)?

There is a ca. 20% (of low value) difference in measured (?) ET rates. The modeled results show far less variability. Does this mean that the calculation of ET is not sufficiently accurate? ET is such an important driver in the hydrologic system that this gives pause. It would be far less important if the tighter modeled range centers on or near the average reported ET rate (not shown). It clearly does not lie very near the midpoint.

editorial

Some mark at an average value (mean or median) for the nine remnant ridge-and-slough stations would be useful.

Pg. 22 Figure 21 minor issue

The "dead rivers" on the south side of Lake Okeechbee look long enough here that they might reach ground lower than the lake shoreline and have some measurable effect in outflow. Are their lengths exaggerated on this old map

Pg. 23 Figure 22 comment

Good agreement and well suited for the modeling.

But note, with a closed depression lying below regularly attained spill levels and effective outlets above those spill levels it is not as strong of evidence for the validity of the input flows to Lake Okeechobee as first impression of the conformance might suggest.

Note also, very little vertical shifting (say, under control of the computed discharge westward to the Caloosahatchee River) would greatly influence southward discharging into the Everglades. (I realize that this problem is buffered, as there is only so much water to flow.)

Pg. 23 Figure 23 major issue

A useful and revealing illustration.

But note, it does not agree in very important particulars with Table 8 (if outflow to the Caloosahatchee River can be taken to be “southwest rim.”)

Pg. 24 Table 9 editorial

Are the negative numbers for T11 merely directional?

Pg. 25 Figure 26 editorial / minor issue / comment

Units? Feet presumably.

Wet season: A strong vector goes into Lake Okeechobee on the northeastern side. Is this a stream? Does not the lake have a sand shoreline ridge there?

Flow off the sand terrain into the northwestern Everglades is again shown (as well as in figures on pg. 26) and also into the extreme northeastern boundary. Is not flow <u>off</u> the sand terrain into the Everglades discounted in the text somewhere?

Pg. 27

“Evaluation.” Mention is made that cells will continue to be “improved.” How so?
Posted: 14 Feb 2007 05:08 PM

Additional input 15 February, 2007

Questions
(If some points are covered in the text, my apologies; it is hard to remember every previous point in a thick tome when questions later arise.)

>How will the model be further validated and calibrated?

The encouraging results from the Everglades (especially) probably don’t test the ground-water components critically (which will increase in importance in the managed system, to be evaluated later).

In the sandy areas elsewhere, where ground water plays more of a role, the tighter vegetational mosaics and more complicated surface geometry probably do not widely allow the same confidence in interpreting natural conditions for any given area, by which to test.

The present demonstration modeling shows some surface water levels predicted in the lower ends of the interpreted former ranges, which may result from lower-than-average rainfall (as mentioned in the text) but may as well result from actual ET or surface-water transmissivity values being higher than estimated.

Very important areas under protection have little direct data of several important types (several mapped figures show few data points in Big Cypress National Preserve, especially, but this also affects results for downstream Everglades National Park).

>How will the accumulated understanding of uncertainty be communicated to users or recipients of model results?

For example, how will uncertainty be attached with plots of water-level predictions?

>How will the minimum area of reliable or confident prediction be determined for future users?

Presently, single cell results are risky.

>How will later challenges to validity be accommodated, or at least approached (depending on the strength of evidence of the challenge)?

For example, suppose reliable paleosalinity data from Florida Bay are incompatible with predicted discharge.

>Will subareas of the model readily be able to be considered separately, with specified flow boundaries?

Will users (especially for the southern Everglades) be able to remove all the calculations (and assumptions and errors) from upstream in order to test responses in their area of interest under other conditions of assumed inflow?

>Will modeling of extreme flow events of at least several-day duration suffer from the limitations discussed for transient events and are these limitations severe?

Are large-area transient events in flow hard to model, specifically after a wide area tropical-storm deluge?

For example, extreme flow is possibly important in understanding ridge-and-slough ecology and maintenance.

>Is ground-water flow evaluated or included coming from the southernmost extension of the central Florida ridge, east and west into the Kissimmee and Fisheating Creek drainages, respectively?

>Is anisotropy of surface-water transmissivity safely considered not important enough to include for the ridge-and-slough terrain, especially under flooding levels below the ridge tops?

>Is the 1:10 anisotropy estimate for vertical vs. horizontal hydraulic conductivity (chap. 4, pg. 68-9) truly justified for the sediment and directions of flow it is applied to here?

The rule-of-thumb would have come from much different materials and possibly much different distance scales?

An additional question

(I would answer the last part of this myself, but I don't have ready access to the documents. I also have to rethink the hydrology myself.)

>Can a broad shallow swale on the western side of the natural Everglades be rejected (or at least be considered not evidenced)?

Some older literature suggests that one was there.

The linkage (used in the topographic reconstruction) for slope being perpendicular to flow direction really applies to the surface of the water. It is applied to the underlying land surface by an assumption of even depth.

We do not yet know what directly controlled/imposed the vegetational lineation. If it is was a high-water high-flow factor it may not preclude a shallow swale occurring on the western side of the Everglades (angled inflow off the higher axis of the Everglades during declining water levels leaving no sign).

The apparent lack of lineation in the westernmost Everglades pointing toward Mullet Slough, which is known to discharge appreciable water, might weakly suggest that the Everglades there easily was able to absorb that water into southern flow without the inflow spreading out too much. This too suggests appreciable depth there.

What was the original evidence for a swale along that side? Did not some cross-section figure from an early survey show it? Did not an early west-to-east exploration party describe entering the Everglades, passing through deeper water, then back into shallower? I realize that part of the old vegetational mapping that suggests it apparently resulted in large part from alteration of the area to the east by drainage

From Jared Bales

NATURAL SYSTEM REGIONAL SIMULATION MODEL (NSRSM) V2.0 IMPLEMENTATION REPORT

The report contains a reasonably thorough documentation of the NSRSM model. NSRSM is conceptually different from most hydrologic models, but the approach used to develop the model is sound. Calibration and verification of the model is difficult because of the absence of pre-development data and model inputs, but the approach used for calibration is somewhat clever.

There are, I think, at five broad areas that could be addressed more comprehensively:

(a) A description of appropriate applications of the model and suggestions for specific types of applications is needed; this is addressed in the first comment below.

(b) The report should explicitely give some guidance on the expected accuracy of the results . . . "hydroperiods are likely reasonable to within 14 days; water levels are likely reasonable to with 0.5 m, etc." Recognizing that any such guidance will be largely speculative, it will, at least, give those less familiar with the model than SFWMD staff some benchmarks for interpreting and using model results.

(c) A more complete analysis of the sensitivity of model results to changes in model input and assumptions would be helpful; this is addressed in several places in the comments below.

(d) It could be useful if hydrologic components of the NSRSM were tested individually, if possible. Is it possible to verify the the 'watermover' algorithms work independently from other aspects of the model? What about the overland flow component, etc? What do we know about the reasonableness of individual algorithms? Where is the most uncertainty in the hydrologic formulations (not data or parameters)? One undesirable scenario is that the combination of many errors in the algorithms and date yields reasonable results for the wrong reasons.

(e) There needs to be a broad overview of the modeling approach. We have descriptions of individual pieces of the model, but it is not at all clear how the pieces fit together. A flow chart or similar diagram that show how water moves through the system, and which model components are used to simulate this flow, would be useful. What is the general conceptual approach to the model construction? This is never clearly stated.

Major comments: (Note: p. 9-15 means page 9, line 15.)

1. The function of NSRSM has been stated . . . to simulate pre-development hydrology in the Everglades. The question remains: Why? How should these results be used? Given all of the uncertainties in the model input (topography, climate, vegetation, etc.) how can pre-development simulations be used for

CERP? Unless the SFWMD gives strong guidance on this issue, the model likely will be misused.

2. Comparisons were made between NSM and NSRSM (Results and Evaluation document), but what do we learn from these comparisons? What can be learned from RSM (the managed system model) calibrations and tests that could be useful to users of NSRSM?

3. The conceptual model of the S. FL developed in Ch. 3 is the foundation on which much of the rest of the work rests, yet there are few citations for the statements made about pre-development conditions. This, I think, is very important to the credibility of the model. Much is made of Parker's results: on what were Parker's estimates of flow direction based? Has that work been verified more recently?

4. p. 9-15: 'Historically, this provided a source of ground water to the Everglades.' . . Were aquifers historically artesian? Later in the report, I think the point is made that there is not much exchange between the aquifers and the surface system.

5. p. 10-18: Why NGVD 29?

6. p. 10-18: 'NSRSM contains watershed boundaries similar to managed system.' This reads as if the topography of the pre-development and managed system are the same, which you have shown that they are not. I presume you are talking about sub-areas of the model, but this should be clarified.

7. p. 12-4: 'Stresses and errors due to common conditions in South Florida . . . ' I don't know what this means.

8. p. 12-12: I found this para. somewhat confusing. It seems like several different types of error are being confused. There is error associated with the mesh configuration (size and geometric configuration), and with the temporal discretization. There is error associated with the numerical scheme used to solve the governing equations, and with the way boundary conditions are applied. There is error associated with the exact form of the governing equations. The discussion in this section is supposedly about 'mesh evaluation,' and I don't think issues like spin-up, initial conditions, etc. should be confused with errors associated with the mesh. The mesh was never really evaluated, in that different meshes were not tested.

9. p. 12-41: Why is a disturbance in ground water the 'worst case?'

10. p. 12 – 17: Was the model applied using grids different from the 'final' grid in order to determine if the simulations were a function of the grid?

11. Fig. 10—How was the discontinuity in contours at western border of the basin treated?

12. p. 21—Were any tests conducted to determine the sensitivity of the model results to topography, particularly in areas of greatest topographic uncertainty?

13. p. 23-24—Why was the Buttonwood Embankment considered to be ‘more continuous’ for pre-development conditions? How does this assumption (fewer breaks in the embankment) affect simulated results?

14. p. 33-13: Why would there be physical watermovers in the NSRSM?

15. p. 33-18 and following: Were shunts in each of the computational cells adjacent to the two lakes, or only in selected cells? Why was “the conveyance for the model was computed by multiplying 10.0 by the length of cell wall adjacent to the lake?”

16. p. 35-36: eq. 7 is not dimensionally homogeneous. Is A_s supposed to be on the left side of the equation?

17. p. 36-5: “Once the storage is calculated . . . “ Where is the storage calculation described? Also, this sentence appears to be generic and taken directly from some other document. Presumably, all of the lakes in the NSRSM have been assigned a shape. Likewise, in line 12. Presumably everything has been designated as a lake or a pond in the NSRSM. In fact, it might be useful to identify the lakes (including shape—circle or parabola) and ponds (if not too many) in a table.

18. 36-11: “ . . . although they do decrease the area of the cell . . .” This would be clearer if something like “ . . . effective flow area of cell . . . “ was used. The actual area of the cell is unchanged by the pond.

19. Tables 11-12, and elsewhere: While I can read the code, I’d rather see the procedures written as mathematical equations or as text. It is not clear to me the criteria ‘shallow’ and ‘deep.’

20. p. 39-2: Was there any difficulty in identifying the seaward boundary of the lagoons?

21. p. 39-6: How are the effects of varying tide levels accounted for in the stage-discharge lookup table? To what location is the target elevation referenced . . . i.e. exactly where on the St. Lucie River estuary is the target elevation ‘measured?’

22. Figs. 23-24: How does the volume increase while the area does not . . .Are the walls vertical?

23. p. 46-2: What does ‘implicitly modeled’ mean?

24. A map showing the locations of all of the reservoirs/lakes/ponds would be useful.

25. Eq. 10: Units on this equation are unusual, to say the least. Units for *a* would have to be length raised to some power divided by time. For example if beta is 2, then units of *a* would be length raised to zero/time, but if beta is some other value, units of *a* would have to be something such that the product of L * a * d^{beta} had units of length cubed.

26. p. 56-7: What is Manning’s b?

27. p. 57-4: What is the Kadlec coefficient? The product of all of the terms on the right side of eq. 10, except for the depth term?

28. Tables 27-29: What are the units? Is the maximum depth only 1 (foot?)?

29. What is the sensitivity of model results to these estimates of hydraulic conductivity in the peat (p. 57-10) and Marl Marsh (p. 58-7)?

30. Table 30: It is unclear whether this table is for a particular cell, or for all cells in the Ridge and Slough—the first part of the sentence says ‘a typical Ridge and Slough cell.’

31. p. 60-9: How were peat layer thicknesses estimated, particularly for pre-drainage conditions (this may be described elsewhere, but I missed it)? What is the sensitivity of model results to errors in estimates of the peat layer?

32. p. 61-8: Ground water needs to be defined. When is water ‘ground water?’ When it is in the peat layer? Below the peat layer? In a confined system? There is some discussion of this beginning p. 61-13, but it is not at all clear.

33. How is the ground-water system connected to the surface water system (see comment (e) at the beginning of this discussion.

34. p. 63-27: I did not clearly understand how the HPM fit in with the overall NSRSM. The information in tables 35 and 37 was presented without much context.

35. p. 67: There are quite a few incomplete sentences in the ‘River Network’ section, making it quite difficult to follow.

36. p. 67-13: Terms here need to be defined or describe: nodal connectivity, etc. Line 16—what is the GMS?

37. p. 68-20: Are rivers in contact with the aquifer? This is another case where some conceptual description of the water and depiction of water movement from compartment to compartment would be extremely useful.

Minor comments

1. p.3-2: I’d be a little less definite: ‘seems to reasonably simulate . . . as best as we can reconstruct pre-drainage conditions.’
2. p. 11-6: Finite difference or finite volume? Previously stated that NSRSM uses a finite volume scheme.
3. p. 13-7 and 13-19: Repetitious.
4. p. 28-15 and elsewhere—‘specific landuse type’. . . as this is a predevelopment model, I think it would be more appropriate to use either ‘vegetation’ or ‘landcover,’ rather than ‘landuse’ for the sake of clarity and consistency.
5. p. 32 and following—water ‘conveyance’ instead of ‘mover?’ Mover implies (to me) something that picks up and carries the water, whereas I think you are talking about a flow path.
6. p. 38-28: Did you mean NSRSM or RSM?
7. Table 13: What are the units in the table?

8. Fig. 22 is illegible.
9. Fig. 26 and 31: Explanation for the various depth shadings is needed.
10. p. 63-30: Did you mean NSRSM or RSM?

NATURAL SYSTEM REGIONAL SIMULATION MODEL V2.0 RESULTS AND EVALUATION

The primary observation about this document is that results are presented with no interpretation. What do the results indicate about model performance in general, about specific hydrologic routines, about spatial patterns, about performance for the various landcovers, about interpretation of the NSRSM results, etc.? This could be a very useful document, but the Evaluation on p. 27 is very weak.

1. p. 2: 'Model output monitoring sites . . . are displayed in Fig. 1.' Fig. 1 shoes in the 'decline in rainfall in South Florida since 1960.'
2. p. 4: It would be useful to define 'reference range' here.
3. p. 5: First paragraph notes that the dry season minimum seasonal water level is 1 foot for ridge and slough, but Table 1 says 0 ft. Note also that Fig. 4 shows the minimum as 0 ft.
4. Table 2: it is unclear what the 'weighted mean' is . . . is it the weighted mean average elevation?
5. Fig 4, and throughout this particular document: There is little to no interpretation of the results. What does it mean that the dry season water levels are all higher than the reference level? What are the implications for application of NSRSM? Where might the error be . . . in the topography, or the ET, or ground-water recharge, etc.?
6. Fig. 5 and following: What is the green box that is bracketing the 80 – 90 percent exceedance?
7. Fig. 5 and following: It appears that, in general, peaks and minimums are undersimulated relative to the reference condition. What is the implication (see comment #5 above).
8. p. 10: What is POR, and how does %POR x 12 months = inundation duration?
9. Fig. 14 and others: Cell ID's are illegible. It would be useful if this presentation and similar ones could be placed in some kind of geographic context. There appear to be patterns in the series in fig. 14 (higher simulated durations to the right of the figure, somewhat lower durations in the middle), but there is no way to relate this to the model or the landscape. One might be able to make some conclusions about geographic patterns if this were presented differently.

10. p. 18: Do you really want to say ‘have not experienced significant <u>improvement</u>?’ Wouldn’t ‘modifications’ be better in this context?

11. Table 8: Why not use consistent units? Both cfs and m Ac-ft/yr are used (note column heading says ‘cfs’).

12. Fig 17 – 19: What might we conclude from the fact that all of the simulated flows are higher than the measured?

13. Fig. 20: ET is on the low end of the reference range, and simulated flows (figs. 17 – 19) are high? Any relation?

14. p. 22: Why would the model be unable to more accurately simulate Lake Okeechobee ET? Has anything change from pre-development?

15. Fig. 23 is not particularly useful in it’s present form. ‘Sough Rim’ etc. must mean outflows along South Rim? This could be clearer. In 1969, the avg. annual stage rose, and the mass balance indicates an increase in volume. 1970 and 1971 indicate a decrease in stage, but the volume (difference in gains and losses) seems to change very little. To what is the fall in stage attributed. A better depiction of the volume change relative to stage would be helpful. A plot of cumulative volume change might be good.

16. p. 24: What do we learn from the comparison of NSM and NSRSM results?

1