SCIENTIFIC REVIEW OF THE RECOMMENDED MINIMUM FLOWS FOR THE HOMOSASSA RIVER SYSTEM

Scientific Peer Review Report

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Scientific Peer Review of Proposed Minimum Flows and Levels for the Homosassa River System

EXECUTIVE SUMMARY

The Review Panel visited the Homosassa River system via boat and portions of the Hidden River by land. We accepted the District's charge to the panel and formulated eight questions that we felt must be answered before accepting the minimum flows proposed for this river system. The Panel agrees that the Homosassa River System's flow is dominated by spring discharge and minimum flow criteria do not need to be evaluated seasonally. The District's approach of using a threshold of acceptable change, 15%, is reasonable and defensible. The District has amassed an adequate database for purposes of the MFL (Minimum Flows and Levels) evaluation, although there was a lack of historical data for some biological components and some additional analyses of some biological data might be useful. The District has followed a credible and defensible approach in determining that current groundwater pumping in the Northern District has not affected the quantities of base flows in the Homosassa River system. However, the impact that groundwater pumping in the Northern District has had and will have on salinity and other water-quality parameters in spring discharges and base flows in the river is not well understood. The current assumption that salinities in the Homosassa River system today represent base flow conditions needs further evaluation. Changes in the quality of water exiting springs are as critical to future biological resources as changes in overall flow. Traditionally, reductions in downstream flow result in the upstream migration of the freshwater-saltwater boundary. In the Homosassa System, however, there is the additional impact of saline water flowing from springs. Evidence presented by Leeper et al (2010) is adequate to conclude that the proposed maximum 5% reduction in Minimum Flow satisfies the language and intent of the Statute and will result in "no significant harm" to the flora and fauna of the Homosassa River System. The use of the Homosassa River by Manatees as a thermal refuge in winter will not be impacted by this reduction. Suggestions for additional data collection and analyses are made in this review.

INTRODUCTION

The Florida Legislature requires that Water Management Districts establish minimum flows and levels (MFLs) for state surface waters and aquifers within its boundaries. The purpose of the statute is to protect Florida's water resources for the future. This protection extends to the fauna and flora within the water body through the requirement that the ecology of the area be protected from "significant harm" (Florida Statutes, 1972 as amended, Chapter 373, Section 373.0421). Once Water Management Districts have determined an MFL for a watershed, maintenance of the MFL becomes part of the planning process for future withdrawals. The same Florida statute requires that Districts develop strategies that will achieve recovery to the MFL within 20 years or to prevent withdrawals from decreasing flows below the determined MFL.

Water management districts are required to use the best information available in establishing the MFL for a watershed and to plan for low water flow conditions associated with season. A minimum flow is the point below which further water withdrawals will cause significant harm to the water resources or ecology of the area or significant harm to the water resources of the watershed. Thus, Water Management Districts must consider a wide array of impacts in the development of their MFL levels based on a variety of different information, which may be more robust for some resources than others.

The Southwest Florida Water Management District (SWFWMD) has begun the process of developing MFLs for watersheds within their district. Using guidance provided through Florida Statutes, SWFWMD has used a data collection/data review process to develop a recommended MFL for 15 of its watershed segments. Each of these recommended MFL levels was evaluated by a panel of independent reviewers. The Panel examines documents and data provided by SWFWMD staff and makes a recommendation with respect to the proposed MFL. Once the Panel recommendations are reviewed by SWFWMD, minimum flows are codified by rule and used in future decision making within the specified watershed segment.

Because many of the watersheds have been structurally altered by canals, dams, etc, identifying a baseline condition that incorporates structural and hydrological alterations within the hydrologic system is not straightforward. Determining MFLs for a watershed must incorporate current conditions and often uses data which may or may not have been affected by these structural alterations.

A number of the SWFWMD watersheds, including the Homosassa River, are dominated by artesian spring flows from the Floridan aquifer. How water moves through the Floridan aquifer is not as easy to understand as surface-water flows. While this adds a level of complexity not found with watersheds dominated by surface-water flow, it does simplify the development of an MFL since most of the annual variation resulting from seasonal variations in rainfall is eliminated.

The development of MFL's must consider protection of not just water resources, i.e., freshwater flow, storage, etc, but attributes of the natural world associated with flows or water levels that are valuable to people (State Water Resources Implementation Rule, Chapter 62-40.473, Florida Administrative Code). Recreational values inherent in fishing and hunting are important considerations in setting MFL and dependent on the aerial extent of freshwater, marine, and estuarine habitats associated with a river. Navigation and aesthetic values should be considered as well as the function of a river system in absorbing and transporting nutrients and sediment. The development of an MFL for any system is a complex undertaking.

The Panel for the review of the MFL for the Homosassa River system was provided a draft copy of the report prior to an on-site visit on August 10, 2010. During that visit, we observed by boat almost the entire system, with special emphasis on springs, which are the primary sources of river flow. We also visited, via vehicle, the Hidden River and its watershed. The Panel met the evening of 10 August 2010 and discussed our initial impressions of the Homosassa River system and what we felt were key questions which needed to be answered in the MFL recommendation and supporting documents. These questions became the focus of our review process. Central questions were:

1. Is the District's threshold of a maximum 15% change of resource within the system a reasonable approach?
2. Was there an adequate data base for development of the regression model?

3. Was there an adequate data base for development of the hydrodynamic model?
4. Were the models used by the SWFWMD the best models for determining the MFL for the Homosassa River system?
5. Was the data collection approach adequate to determine the past and present natural resources on the river system?
6. Were appropriate assumptions and analyses made in the use and extrapolation of these data?
7. Was the weight of evidence enough to convince the panel that the recommended MFL satisfied the Florida Statute establishing the MFL requirement?
8. Are there additional data that should be collected in the future that would add confidence to the MFL SWFWMD recommendations?

The following sections are arranged as follows: Critical Questions, General Comments and Recommendations related to the eight questions above. Specific Comments follow and are aspects of the document or appendices we found confusing or that appear inaccurate. These should be corrected or explained to eliminate the confusion. Finally, there is an Errata Section.

Critical Questions

Question #1 - Is the District's threshold of a maximum 15% change of resource within the system a reasonable approach? **Yes**, while it may be somewhat arbitrary, setting a quantifiable threshold provides a means to evaluate the impact that reductions in discharge would have on fish and invertebrates, salinity-based habitats, and the extent of thermal refuge for the Florida manatee. While reasonable, many of the r^2 values were low (but significant) and only positive relationships were examined. Both positive and negatives ones should be examined if the goal is to not dramatically change the community structure of the entire system.

Question 2 - Was there an adequate data base for development of the regression model? **Yes**, the salinity, tide stage, and discharge records for gage sites in the river and the salinity measurements made by SWFWMD and other agencies provided an adequate data base for the empirical regression models developed to describe salinity in the main channel of the Homosassa River. **Yes**, for most of the biological response measures (plankton, fishes, and manatees). The benthic analysis was incomplete, however. There were also considerable data sets for SAV and EAV that seemed to contradict each other.

Question 3 - Was there an adequate data base for development of the hydrodynamic model? **Yes**, the stage, salinity, and temperature data at the USGS Shell Island gage, the salinity and temperature data at the USGS Homosassa Springs and SE Fork gage sites, the discharge data at the USGS Homosassa Springs, SE Fork, and Homosassa Springs gages used to model the discharge at Halls River, the salinity data in Halls River and at the Homosassa Springs gage, and meteorological data measured at the FAWN-IFAS station at Brooksville in general provided an adequate data base for development of the hydrodynamic model.

Question 4 - Were the models used by the SWFWMD the best models for determining the MFL for the Homosassa River system? **Yes,** the EFDC hydrodynamic model is well documented in the literature, and it has been widely used to simulate flows and water-quality parameters in estuarine and coastal applications. Also, the use of regression models to empirically relate river discharge and salinities is acceptable. The assessment of the impacts of pumping on spring discharges (Basso 2010) is based on a proprietary version of MODFLOW, which also is well documented and widely used to simulate groundwater flow systems. Additional study of the relationship between withdrawals and spring flow at different springs should be done with the goal of understanding any potential increases in salinity at saline springs or decreases in flow at freshwater springs that might be caused by withdrawals.

Question 5 - Was the data collection approach adequate to determine the past and present natural resources on the river system? **Yes**, with respect to flow, this approach is quite adequate to conclude that present-day spring and river discharges can be considered baseline or natural flows [also, please see response to the next question concerning water quality]. The approach assumed that present-day flow records were representative of past, or baseline, conditions based largely on the determination using a numerical groundwater flow (Basso 2010) that groundwater pumping in the Northern District of SWFWMD has reduced historical spring flows in the Homosassa River system by an insignificant amount (approximately 1 percent). With respect to many natural components, the answer was **no**. There were some data for SAV/EAV and water quality from earlier reports, but not much else besides those. Obtaining data on past resources that are not considered of economic value is often difficult. Data collected as part of the current MFL document will serve as a baseline for future modification of MFL evaluations.

Question 6 - Were appropriate assumptions and analyses made in the use and extrapolation of these data? In response 5 above, **yes** it is reasonable to assume that present-day spring and river discharges represent baseline or natural flows. However, it can only be inferred that present-day salinities discharging from the springs into the river system are still at natural levels. Based on the lack of a calibrated numerical groundwater transport model for the Northern District or other means to address this issue currently, this is the best that can be done at this time. Addressing the need for data that can be used to calibrate such a model should be a priority for future research and monitoring.

There were also some questions of providing additional information with respect to assumptions used in the detailed analyses provided. For example, low r or R values in many analyses were not compared to the 'norms' of statistical procedures. These should be provided.

Question 7 - Was the weight of evidence enough to convince the panel that the recommended MFL satisfied the Florida Statute establishing the MFL requirement? Generally, **yes**, it would satisfy the statute, but because of the variability and low predictability of input data, there could be problems with the accuracy of the predictions.

Question 8 - Are there additional data that should be collected in the future that would add confidence to the MFL SWFWMD recommendations? Yes, as noted in previous questions, priority should be given to collecting additional data as part of an investigation intended to resolve some of the salinity and temperature results obtained using the hydrodynamic model. Also, additional groundwater quality data should be collected as part of an investigation to better understand the flow and water-quality aspects of the springs in the Homosassa springshed and to determine whether spring salinities will increase in response to increased groundwater pumping in the Northern District of SWFWMD.

We feel the District should take a multivariate approach as illustrated in their analyses in the appendices using Primer statistics. The goal of the MFL process is to do no ‘significant harm’, which in many cases is a professional judgment call. The suggested multivariate approach outlined at the end of this document (The sections on Chapters 4 & 5) would improve the ability to make predictions of potential outcomes based on flow reductions. These outcomes would be more holistic and at the heart of the MFL process.

General Comments and Recommendations

Water Quality in the Springs

The water quality in the springs that discharge into the Homosassa River system varies from fresh to brackish. The Homosassa Main Springs and Halls River springs discharge brackish water, and the springs of the Southeast Fork discharge relatively freshwater, based on Yobbi and Knochenmus (1989). Halls River Head Spring, Homosassa Springs, and Hidden River Head Spring discharge sodium-chloride water, which indicates a seawater origin, and Trotter Spring in the Southeast Fork discharges mixed-ion water, which is the result of freshwater and saltwater mixing (Knochenmus and Yobbi 2001). The variability of the quality of the water discharging from the springs of the Homosassa River system is explained in terms of the existence of a coastal transition zone between freshwater and saltwater in the groundwater system (Leeper et al. 2010). Differences in water quality among springs are attributed to the depth of individual spring vents, the proximity of a spring to the Gulf of Mexico, and the transient location of the saltwater-freshwater interface, which creates a zone of mixing that changes seasonally and diurnally (Knochenmus and Yobbi 2001). The transition zone moves horizontally and vertically in the Floridan aquifer in response to tidal fluctuations in the Gulf of Mexico and changes in water levels in the aquifer (Champion and Starks 2001). The age and residence time of groundwater discharging to springs in the Homosassa River system apparently have not been determined. However, in a somewhat similar hydrogeologic setting in the Suwannee River basin, relatively young ages and residence times of spring discharges ranging from 5 to 50 years were estimated by Katz et al. (1999). In general, these description and explanations of water-quality variations among the springs can be summarized in terms of the hypothesis that present-day seawater intrusion and recirculation in an active groundwater flow system result in a saltwater-freshwater interface that moves horizontally and vertically in response to tides and changes in regional groundwater levels, causing spatial and temporal variations in salinities in the springs. In this context, it can be expected that future withdrawals of freshwater from the groundwater system in the Northern District that affect groundwater levels also may affect spring flows and water quality in the Homosassa River system. Potentially, withdrawals of fresh groundwater in inland areas will reduce freshwater spring discharges and also cause the saltwater-freshwater interface to move farther inland, thus resulting in a disproportionate increase in salinity in the spring discharges into the river system. Accordingly, the Panel recommends that SWFWMD conduct future investigations to better quantify the relation between the salinities of the springs discharging into the Homosassa River and saltwater intrusion in the Floridan aquifer. Also, the Panel recommends that SWFWMD investigate the impacts that groundwater pumping in the Northern District potentially has had and will have on salinities and other water-quality parameters in the springs and base flows in the Homosassa River system.

Groundwater Modeling

For the purpose of developing minimum flow recommendations, the Homosassa River system is considered by SWFWMD to consist of the Homosassa River, Southeast Fork of the Homosassa River, Halls River, Hidden River, and springs associated with these rivers (Leeper et al., 2010). As described by Leeper et al. (2010) and in more detail by Basso (2010), it was determined that current groundwater use in Citrus, Hernando, Pasco, and Sumter counties has not had any significant impact on spring discharge in the Homosassa River system. This was accomplished by running the Northern District groundwater model (HydroGeoLogic, Inc., 2008) for two scenarios, i.e., one scenario representing 2005 conditions and the other with no pumping representing pre-development conditions. It was concluded that the resulting decrease in spring discharge in the Homosassa River system represents an insignificant decrease of 1.1 percent. Based on this result, the measured and modeled flows used in the minimum flow analyses were considered baseline or natural flows. The Northern District groundwater model is a fully three-dimensional groundwater flow and saltwater intrusion model developed by HydroGeoLogic, Inc. (2008) for the northern part of SWFWMD consisting of Hernando, Sumter, and Citrus counties and parts of Pasco, Polk, Lake, Marion, and Levy counties. The groundwater flow and solute transport code MODFLOW-SURFACT was used to develop a numerical groundwater flow model of the Northern District and to develop a saltwater-intrusion model for the coastal areas of the Northern District. The groundwater flow model was calibrated to steady-state conditions representing 1995 and to transient conditions representing 1996 to 2002. However, as pointed out by HydroGeoLogic, Inc. (2008), the saltwater intrusion model was not calibrated; instead, a qualitative evaluation was conducted to assess whether the saltwater intrusion model produced the general distribution of chlorides observed from monitoring wells.

SWFWMD has followed a credible and defensible approach in determining that current groundwater pumping in the Northern District has not affected the quantities of base flows in the Homosassa River system and thus that recently measured flows in the Homosassa River system can be treated as base flows without adjustment in this study. The impacts that future increased groundwater pumping will have on the quantities of spring discharges and base flows in the Homosassa River system were not addressed in Appendix B (Basso 2010), but it is certainly reasonable to conclude that the Northern District groundwater model also could be used to assess such impacts. Thus, the impact that groundwater pumping has had and will have on the quantities of the spring discharges and base flows in the Homosassa River system appears to be well defined. By contrast, the impact that groundwater pumping in the Northern District has had and will have on salinity and other water-quality parameters in spring discharges and base flows in the river is not well defined. It can only be inferred that recently measured salinities in the Homosassa River system represent base flow conditions, because the lack of a calibrated saltwater intrusion component in the Northern District groundwater model precludes a quantitative assessment of salinity changes in the spring discharges using this model. The assessment of groundwater conditions and impacts described by Basso (2010) and summarized by Leeper et al. (2010) is quite adequate based on the criterion of using the “best available information” concerning the quantities of the spring discharges and base flows in the Homosassa River system. However, determining how salinity and other water-quality parameters in the springs that discharge into the Homosassa River system will change in response to changes in groundwater pumping in the Northern District cannot be accomplished currently using the existing Northern District groundwater model. Accordingly, the Panel recommends that SWFWMD add a calibrated saltwater intrusion component to the Northern District groundwater model in a future investigation (or otherwise quantify the relation between changes in groundwater pumping and the water quality of spring discharges) to address this issue.

Detailed Comments

Chapter 1

The explanation regarding the adoption of the 15% loss standard was useful in reviewing the remaining chapters and sections. There is the potential, however, that this standard might over-emphasize what are essentially very small changes when the initial habitat or resource is small. Caution should also be exercised in assuming that high volumes may be withdrawn during high flow events (page 24). High flow events can be extremely important in resetting systems, e.g. removing accumulated fine organics from sandy bottoms. This may not be an issue for the Homosassa given that the primary discharge is from springs, but should not be universally applied when developing regulations regarding water removal.

Chapter 2

On pages 38-39, land use in the Homosassa River drainage basin was mapped and delineated for 1990, 1995, 1999, 2004, 2005, 2006, 2007, and 2008 (Table 2-1). The point is made that generally little change occurred in land use/cover in the watershed in the years between 1990 and 2008 (Table 2-1). This observation is somewhat limited in value, however, because the Homosassa River *surface-water* drainage basin, which consists of approximately 55.6 square miles, overlies only part of the Homosassa Springs *groundwater* basin, which consists of approximately 270 square miles (Knochenmus and Yobbi 2001). This is clearly indicated in Figure 2-6, on page 37. The observation that land use has not changed significantly would be better made if land use from 1990 to 2008 in the groundwater basin, or springshed, could be compared. Apparently this section was written to point out that land-use has not changed from 1990 to 2008 and, thus, that the springs have not been affected during this period. If so, this point should be made explicitly.

Box plots are used in figure 2-12 (page 48) and in many others throughout the report to indicate the range of data for tides and other parameters. Are the box plots standardized; do all of the box plots show the same range of information? It is suggested that the information shown in the box plots (minimum, maximum, median, and lower and upper quartile) be specified the first time this type of plot is used.

The variability of the quality of the water discharging from the springs of the Homosassa River system is described (Page 68, 1st-3rd paragraph) and explained in terms of the coastal transition zone between freshwater and saltwater in the groundwater system. It is noted that the Homosassa Main Springs and Halls River springs have been described as brackish systems and that the springs of the Southeast Fork have been described as freshwater systems (Knochenmus and Yobbi 2001). Differences in water quality of the springs are explained in terms of the differences in the vertical and horizontal location of the transition zone and its spatial and temporal variability. Is it possible to illustrate the relation of the springs to the saltwater-freshwater transition zone by constructing a vertical hydrogeologic cross-section aligned with the direction of groundwater flow based on existing water-quality data and/or the numerical modeling results (Hydrogeologic, Inc. 2008) described by Basso (2010) in Appendix B?

Ratios between top and bottom salinities in the Homosassa River during 1984 and 1985 (page 78) were on the order of 0.85 to 1.0 (Yobbi and Knochenmus 1989), i.e., top salinities generally were equal to or less than bottom salinities. In Figure 2-31 (page 80), synoptic salinity profiles for the river surface in 2007 and 2008 are shown in the top panel, and salinity profiles for the river bottom are shown in the bottom panel. The surface salinities generally appear to be less than corresponding bottom salinities for these measured data. However, the median values for the EFDC model in the top panel of Figure 2-31 for surface salinity appear to be greater than the corresponding bottom salinities for the EFDC model in the bottom panel of Figure 2-31. Is there a contradiction between the observed salinity data and the EFDC model results shown in Figure 2-31? If so, an explanation needs to be provided. [Please see further comments below pertaining to Appendix A (HSW Engineering, Inc. 2010) in which salinity profiles shown in Figure F-3 along with the salinity profiles shown in Figures F-1 and F-2, which correspond to Figure 2-31 in Leeper et al. (2010), also indicate that top salinities generally are less than bottom salinities.]

The legend for Figure 2-31 (page 80) indicates that the solid green line shows the median EFDC model salinity for the river surface. Figure F-2 in Appendix F (HSW

Engineering, Inc. 2010), which is included in Appendix A of Leeper et al. (2010), indicates that this line is the median EFDC model salinity for the river *bottom*.

Salinity, tide stage, and discharge records were used to develop empirical regressions for modeling or predicting salinity in the main channel of the Homosassa River (Pages 82-83). Summary descriptions of the regression equations are presented by Leeper et al. (2010), and details regarding development of the regression models are provided in Appendix A (HSW Engineering, Inc. 2010). The regression models consist of sets of equations for predicting the locations of surface and bottom isohalines for salinities of 3, 5, and 12 in the Homosassa River based on the combined flow at the USGS Homosassa Springs and Southeast Fork Homosassa Spring gage sites and the tide stage at the USGS Homosassa River stage gage. The equations account for 53 to 59% of the variability in the salinity measurements, based on r^2 values presented in Table 2.10. Are these results acceptable for empirical models, i.e., are there any generally accepted standards or guidelines to which these regression results could be compared?

One main concern is the weakness of the hydrodynamic model results. The authors state and illustrate (Figure 2-37) in Leeper et al. (2010) that the model overestimates and underestimates the empirical regressions at a number of flow rates and locations. In particular, it appears from Figure 2-37 in Leeper et al. (2010) that modeled 3 psu (practical salinity unit) isohaline locations versus flows for all 3 locations (surface, bottom, depth-averaged) between 160-170 cfs are always high (upriver) compared to the empirical model results and those from 120-150 cfs are mainly low in bottom isohaline locations (mid river), but high in surface and depth-averaged locations (mid-river). This is disconcerting as these relate to where the 3 psu isohaline should be for 2007 baseline period, but the hydrodynamic model does not do a good job and thus predictions may also not be accurate. In contrast, the empirical regression r^2 values ranged from 0.63-0.73 and suggest these may do a better job in predicting impact with future water withdrawals.

The predicted locations of the surface, bottom, and depth-averaged 3 psu isohalines as a function of total spring flow for the Homosassa River in 2007 are shown in Figure 2-37 (pages 88-89). Leeper et al. 2010 notes [and it is quite apparent in the top panel in Figure 2-37] that there are significant differences in the model-predicted isohaline locations for surface salinities, i.e., the surface salinities predicted by the EFDC hydrodynamic model occur farther upstream than locations predicted using the empirical regression models. In the empirical model results, bottom salinities extend farther upriver than the surface salinities, which is consistent with the results of Yobbi and Knochenmus (1989), in which top salinities in the Homosassa River during 1984 and 1985 generally were equal to or less than bottom salinities (see comment above relative to Page 78). However, in the EFDC hydrodynamic model results in Figure 2-37, there is no distinct difference between the surface and bottom isohaline locations. What is the significance of this result? Should it be concluded that the EFDC model over-predicts surface salinities? If so, how does this affect the determination of salinity and temperature changes used to predict the impact of reduced flows in setting minimum flows for the Homosassa River? [Please see further comments below pertaining to Appendix A (HSW Engineering, Inc. 2010) in which the predicted locations for the 5 and 12 psu isohalines in Appendix J are discussed.]

In Appendix F in HSW Engineering, Inc. (2010), synoptic salinity profiles for the river surface between December 2006 and July 2008 are shown in Figure F-1, and salinity profiles for the river bottom between December 2006 and July 2008 are shown in Figure F-2. The surface salinities generally appear to be less than corresponding bottom salinities for these measured data. However, the median values for surface salinity for the EFDC model for 2007 in Figure F-1 appear to be greater than the corresponding bottom salinities for the EFDC model for 2007 in Figure F-2. Longitudinal profiles of surface and bottom salinity measured on individual dates illustrate water that is generally well mixed or *weakly stratified with bottom salinity several psu higher than the surface salinity* (Figure F-3) [page 2-21, 1st paragraph, italics added]. The measured surface and bottom salinity profiles in Figures F-1 through F-3 apparently contradict the results that were calculated using the EFDC model shown in Figures F-1 and F-2. Is there a contradiction between the observed salinity data and the EFDC model results? If so, an explanation needs to be provided. A similar comment was noted for page 78, line17 (Leeper et al. 2010).

Three isohaline models (3, 5, and 12 psu) were developed for predicting the location of surface and bottom water-column salinity isohalines using synoptic data for 2005 through 2009 (p. 2-29, last paragraph and p. 2-30, Table 2-4). The isohaline models explain about 50% to 60% of the variation in the measurements used to develop the models (Table 2-4 and Appendix I-3). R^2 in Table 2-4 needs to be defined. This parameter is often used to indicate a correlation coefficient, but is that the case here? It is defined in Appendix I-3 in HSW Engineering, Inc. (2010) as R squared = 1 – (Residual Sum of Squares)/ (Corrected Sum of Squares); this definition should be added to Table 2-4. Six values of the standard deviation of the residuals between observed and calculated surface and bottom salinities for 3, 5, and 12 psu can be extracted from the histograms in Appendix I-3 in HSW Engineering, Inc. (2010). Including these values, which range from 0.719 to 1.85, in Table 2-4 would provide an additional means to assess how well the regression models predict salinities.

The maximum observed surface and bottom salinities at the Homosassa River gage and the maximum observed bottom salinity at the Halls River gage (p. 3-11, Table 3-4**)** are significantly greater than the respective simulated salinities at these gages (i.e., 19.13 > 9.60, 18.79 > 9.70, and 16.07 > 4.12 psu). Also, the root mean square errors at these gauges (2.08, 2.02, and 1.15 psu) appear to be relatively large. Are there recommended calibration guidelines for estuarine models to which these results could be compared? For example, the Pearson Coefficient R values in Table 3-4 for the Shell Island gage are relatively large (0.91, 0.90, and 0.90), but the values for the Homosassa River and Halls River gages are relatively small (0.50, 0.55, and 0.35). The values for the Homosassa and Halls River gages, particularly the Halls River value of 0.35, are less than the minimum correlation coefficient of 0.60 preliminarily recommended by EPA (1990) for estuarine water quality models. Does this indicate that the Homosassa River model is not well calibrated?

Appendix B in Leeper et al. (2010), in the second paragraph of the Introduction**:** Hidden River should be included in this paragraph to be consistent with Leeper et al. (2010) and Table 2 (p. 12). On page 4 of the first line, it states that the “ground-water basin …is approximately 292 square miles….” This is different from the value of 270 square miles in Leeper et al. (2010) (p. 36) that was determined by Knochenmus and Yobbi (2001). However, these values are considered “similar” (Leeper et al. 2010, p. 36), which seems to be a reasonable way of reconciling the difference.

On page 10, 3.2 2005 Scenario: To determine drawdown in the UFA and potential impacts to spring flow in the Homosassa River system, average annual groundwater withdrawals in 2005 (438.1 mgd) were simulated in the NDM…and compared to non-pumping conditions (zero withdrawals). Please clarify who did this analysis, i.e., did HydroGeoLogic, Inc., or SWFWMD do this analysis? Is the 438.1 mgd scenario the same as scenario 1 in the HydroGeoLogic, Inc. (2008) report? It appears to be, but this pumping rate does not seem to be listed explicitly in HydroGeoLogic's report. Please indicate if the 2005 condition in Basso (2010) is the same as scenario 1 in HydroGeoLogic, Inc. (2008). Also, please indicate the source of the discharge values for the 2005 pumping scenario in Table 2 (p. 12) (apparently they are from Table 5.2 in the HydroGeoLogic, Inc. report).

On page 11, Table 2 states that the discharge at Hidden River Spring Head is reduced 4.0 percent, while all of the other spring discharges are reduced by approximately 1.0 percent, except for Belcher Spring, which is reduced by 2.0 percent. Is the result for Hidden River Spring Head correct? If so, is there a reason why it is so much larger than the other results?

Table 2.8 in Leeper et al. (2010) indicated that the estimated salinity of water coming from different springs varies from 0.1-3.9 ppt, even though they are spatially close. This is perplexing. How can this happen if they are using the same groundwater sources, and we could not find sufficient evidence suggesting why this is occurring nor how this may be influenced differentially by water withdrawals. Is it possible that water withdrawal in one location could only influence the very low salinity springs and thus, elevate the contribution of the high salinity spring water into the system? Ratios of ions in the saline springs (Table 2.6) argues that this is dilute seawater and not just water with high solids derived from minerals in the rock strata through which the springs flow. The oceanic ratio of Na to Mg is 8.213 (Sverdrup et al. 1942), while the ratio in Hall's River Spring #1 was 7.8, 7.9 for Hall's River Main Spring and 8.08 for Homosassa Main Spring #2. Analyses of any inert sea water derived ions from Table 2.6 found similar sea water-like ratios, arguing that the spring discharged dilute seawater. Is this fossil seawater as has been proposed for other similar Florida springs (Scott et al. 2004)? It appears more data are needed to substantiate and verify why this is occurring as it may have some indirect impacts on the contribution of saline waters to the Homosassa River from springs with high salinity compared to other springs. Additional pumping from the spring shed could have very different impacts if flow was reduced from one of the saline or non-saline springs.

It is not clear that there is an adequate understanding of the aquifer itself, residence time for water in the aquifer, or the ultimate source of salt (fossil or modern source) in the saline springs.

In Leeper et al. (2010, page 84) – the hydrodynamic model is "… somewhat problematic" and suggested model accuracy could be improved by adding data from downstream side channels. They also note water temperatures are slightly under-predicted in warm month and over-predicted in cold months (page 84) suggesting the thermal effect of spring discharge may be underestimated. Also, maximum salinity at the Halls and Homosassa Rivers gage sites were underestimated by the calibration and validation periods.

Finally, in Appendix A, page 2-20, paragraph 2, lines 13-15, the authors state '... river stage as measured at the springs is a variable used in calculating spring flow and therefore the independent variables spring flow and tide are related." This is of concern as this interdependence may influence (increase) the models predictability and thus this autocorrelation is problematic from a statistical point of view. How this influences the model outcome and thus prediction is not explained or considered.

Chapter 3 - Vegetation

The narrative in the vegetation section of Chapter 3 (Leeper et al. 2010) is based on a variety of historical and more recent reports (Hoyer et al. 1984, Fraser et al. 2001a,b, Fraser et al. 2006, PBS&J 2009), which indicate some contrasting findings in terms of submerged aquatic vegetation (SAV) and emergent aquatic vegetation (EAV) relative to environmental factors in the Homosassa River. Hoyer et al. (1984) noted significant relationships between SAV distribution and abundance and flow, salinity and light levels in the Homosassa River.

However, more recent research (Frazer et al. 2001a,b; Fraser et al. 2006) indicates significant changes in SAV in the river in terms of number of sites without SAV (104%

decrease) between 1998-00 and 2003-05. There was also a mean reduction in biomass of filamentous algae and most macrophytes (~ 67%) and macroalga biomass (62%), but an increase (85%) in periphyton biomass on SAV between time periods. Because the more recent survey period had lower salinity, they suggested salinity was not as influential as elevated nutrient loadings and possible eutrophication in the Homosassa River. In contrast, the most recent survey (PBS&J 2009) suggested distribution and abundance of SAV and EAV was clearly delineated across salinity zones based on known species tolerances, but that SAV, because of the marked decline, was not a good indicator of increasing salinity and thus, changes in flow. In fact, they believe EAV is a much more predictable indicator of mean salinity along the river and that freshwater species respond quickly to reduced salinities.

Finally, Appendix E (page 3-11) PBS&J (2009) indicates that the relationships between nutrient loads and SAV have not been clearly defined or quantified and thus, predicting impacts due to epiphyte growth and SAV loss are not possible presently. They also note until these relationships are quantified, restoration is not possible. Somehow, the District needs to decouple nutrient load issues from salinity changes in the system before they can accurately decide on which is driving these relationships.

It is clear more research is required to clarify the relationship between SAV and EAV distribution and abundance relative to nutrient loads, salinity changes, and light level modifications along the Homosassa River relative to proposed flow reductions. This must include examining groundwater sources of nutrients into the system and these sources may be influenced by water withdrawals based on the proposed MFL scenarios.

Forested tidal wetlands were noted in the report, but little information reported on the extent of the freshwater tidal swamp within the Homosassa River system. Impacts to this important part of the ecosystem will be hard to calculate because the Homosassa is on the Tropical-Temperate boundary where saline-tolerant mangroves can easily displace salinity intolerant species such as Ash (*Fraxinus* spp.) and red maple (*Acer rubrum*). In a typical transition from freshwater to saltwater within an estuary, a potential reduction of flow would result in an upstream migration of the freshwater to saltwater boundary that could be easily modeled. With the source of flow in the Homosassa system consisting of multiple springs, some of which release saline water, impacts of the freshwater-saltwater boundary are difficult to predict without a better understanding of the aquifer system from which the springs emerge. It would be prudent to develop a map of the tidal, forested wetlands for future comparisons. There is some suggestion that changes have already occurred (See pages 3-14, Appendix E). Freshwater tidal swamp species extended further downstream than their aquatic counterparts (See Appendix E). Woody species can often persist even after salinity has increased. Alternatively, these tidal swamp species may be holding on because they are at an elevation slightly above the tides.

Sea level rise on this flat landscape also has the potential to greatly increase the extent of tidal marsh and swamp and should be modeled to understand the long-term changes that may impact the Homosassa even without flow modification. This may be critical if some of the forested wetlands are just above the current high tide level as noted above.

If saline water is currently intruding into the aquifer and is the source of the salt in some of the springs feeding the Homosassa River, even a slight change in sea level could increase the salinity of these springs. Even a small change in salinity and/or sea level could greatly alter the extent of freshwater wetlands in the upper reaches of the Homosassa River system. Tidal forested wetlands are an obvious component of the landscape and a sudden loss of this vegetation could appear to be the result of some change in management, when it may just be the result of crossing a critical threshold caused by sea level rise.

Benthic Macroinvertebrates

Results from Grabe and Janicki (2009; Appendix D) did not note any eastern oysters collected in the top 50 species they reported on in Chapter 3, nor are they listed in Tables 3-3 and 3-4 in Appendix D. In contrast, Water and Air Research (2010; Appendix F) found live eastern oysters in their study and Chapter 3 noted "The distribution for live oysters differs from that reported by Grabe and Janicki (2009) who found oysters in dredge samples collected between river kilometers 4 and 9 during their May 2008 sampling events" but no explanation for this difference is provided. Oyster data can be found in Table 3-7 in Appendix D, but this species was not mentioned directly in the text.

In terms of the barnacle study (Culter 1900; Appendix G) noted in Chapter 3, it would be interesting to note if there were any patterns in the distribution of the presumed exotic species, *Balanus amphitrite*, in relation to salinity along the Homosassa River, which might suggest that water withdrawals might enhance their distribution and abundance in areas along the river compared to baseline.

One of the potential problems in the analysis of benthic data is using both RKM and salinity in their forward stepwise multiple regression (Appendix D; Table 3-5). If I am correct, aren't RKM (position in river) and salinity potentially correlated and thus if both are included in model (as in the Shannon Diversity regression in Table 3-5) it should inflate the adjusted R^2 values? This can easily be examined in regression in a number of ways and should be examined in all models. Also, the adjusted R^2 values for density and Shannon diversity are low (< 0.40) and thus, do not explain much of the variation in the models. They may be lower if you exclude either RKM or salinity if they are highly correlated.

Another potential problem is the interpretation of the results from the ANOSIM procedure listed in Table 3-6 (Appendix D). One caution with Primer statistics illustrated in Appendix D is that the MDS plot stress levels are not reported in Figure 3-10; these should be reported in all such plots. Also, with ANOSIM, Clark and Warwick (2001, page 6-4) note R-values are as important as p-values in determining significance and 5 of the 7 significant pairwise comparisons have R values < 0.5 While significant, having high p-values with low R-values suggest a re-evaluation of how these plots are interpreted.

In the executive summary section of Leeper et al. (2010) and in Chapter 4 (paragraph 1, lines 5-8), point out only the fish and invertebrate plankton, nekton, salinity-based habitats, and manatee data are used to set MFL levels, as those appear to be the most-sensitive to water withdrawals. Thus, the issues with the benthic data noted above may be less problematic in reference to setting the MFL, but the issues need to be examined and re-evaluated (if necessary) for the final document such that no spurious interpretations are made.

Plankton and Seine & Trawl Data Sets

The authors indicate in Appendix H (page 72) that "Some characteristics of the plankton community in the Homosassa River estuary suggest that the area has become more eutrophic." The authors suggest that reduced abundance patterns of presumed indicator species (a copepod, mysid and the bay anchovy) compared to other non-spring-fed systems and regular occurrences of large shifts in dissolved oxygen (DO) concentration (Appendix H, page 72, parag. 2, line 5) is evidence of increased eutrophication. The data presented in Table 3.2.1 and Figure 3.2.1 in Appendix H (pages 26-27) illustrate high and low DO values based on depth and location strata, but the text states that dissolved oxygen "occasionally reached strong supersaturation levels during winter and spring months, …". This seems to contradict the statements above about regular occurrences of supersaturation. Also, both of the presumed indictor species are very common across their range and are found in non-eutrophic and eutrophic systems as well, so it may be useful for authors to cite some literature on them being an indicator species relative to the potential eutrophication issues they note. There is also no mention of these concerns in water quality section of Chapter 2 in Leeper et al. (2010, page 90), although they do note some low DO (< 5.0 mg/L) were observed in all sections. However, data presented in Chapter 3 (pages 97-98), based on Fraser et al. (2001a,b; 2006) suggested increases in nutrient loads in the system over time and noted for SAV and EAV that nutrients may be more influential on distribution and abundance compared to salinity changes. It is sometimes hard to glean important data from Leeper et al. (2010) because it may not be in the section you expect and in this case, we expected it to be in water quality, not in the SAV/EAV section. There is clearly some inconsistency in how different authors view presumably the same data sets or how data are logically provided in Leeper et al. (2010).

The authors in Appendix H (page 73) indicate "…has a relatively deep channel throughout much of it length (Fog. 2.7.4.1), and this channel may facilitate two-layered estuarine circulation …" but really provide no data illustrating two-layered flow patterns. In contract, in Chapter 2 of Leeper et al. (2010) these authors indicate vertical water temperature data (page 72, paragraph 2, line 2) and vertical salinity data (page 78, paragraph 2, line 5) suggests a relatively well-mixed system. There are clearly some inconsistencies in how different authors view this system and there should not be these inconsistencies in a single document.

One of our main concerns in Chapter 3 of Leeper et al. (2010) is the quality of the regressions (linear and quadratic) in terms of their explanatory power relative to flow issues. For example, in the plankton section (pages 116-117; Table 3-4) the authors note that only 28 of 64 plankton-net taxa showed some significant response to the range of flow encountered. Of the 28 noted, only 5 had significant positive relationships (abundances increased with increased flow) and the remaining 23 had negative relationships (decrease in abundance with increased flow). The authors then focused on those five taxa with positive relationships. The authors also note that the coefficients of variation (adjusted r^2) ranged from 0.29-0.62 for time lags of 36-120 days; however, careful examination of Table 3-4 shows these values ranged from 0.25-0.72 and 50% (n = 14) had r^2 values < 0.50. Also, eight taxa (29%) had issues of possible serial correlations (significant DW values). The authors justify these r^2 values for both plankton-net and seine and trawl collections by stating “Some of these relationships had very good fit, suggesting that these relationships are not spurious” (Appendix H; pages 40 and 68). We are not sure if those fourteen taxa are really relevant to the discussion as only up to 50% of the response appears to be explained by flow. In most biological responses, 50% may be statistically significant, but not be biologically meaningful. The summary (page 160) in Leeper et al. (2010) makes note of this low accounting of variation of individual regressions.

Similar patterns in these regression coefficients can be noted for the seine and trawl data sets. For example, the authors noted that 40 (41?) of the 53 pseudo-species had significant relationships to flow while 13 had quadratic and 27 (28?) had linear relationships (page 116). Of the linear relationships, 12 had negative responses and 15 (16?) had positive ones with time lags from 1-203 d. The reported r^2 values ranged from 0.20-0.78 for those positive responses; however, 37% (n = 10) of these had r^2 values < 0.50. Also, seventeen pseudo-species (32%) also had issues of possible serial correlations (significant DW values). The authors justify these r^2 values for both plankton-net and seine and trawl collections by stating “Some of these relationships had very good fit, suggesting that these relationships are not spurious” (Appendix H; pages 40 and 68). As noted above, we are not sure these 10 pseudo-species add much to the discussion of altered flow, since as explained < 50% of the variance is probably not very biologically meaningful. The summary (page 160) in Leeper et al. (2010) makes note of this low accounting of variation of individual regressions.

We also question not discussing the negative relationships (most of the taxa and pseudo-species collected) as the regressions suggest that as flow is reduced, abundances of many of these taxa or pseudo-species would increase and presumable expand into upriver locations as salinity changes with flow. This should have consequences relative to community structure patterns over some time frame, which may ultimately modify community structure in the system overall. This may be more relevant if some exotic species are present (i.e., striped barnacle; Culter 2009).

One caution with Primer statistics illustrated in Appendix H is that the MDS plot stress levels are approaching values that are of concern in interpretation (stress = 0.20 and above; See Clarke and Warwick 2001); thus, the 2-D fit of a 3-D plot may not be very good. Also, with ANOSIM and tests that generate R-values and p-values, Clark and Warwick (2001, page 6-4) note R-values are as important as p-values in determining significance and some in Appendix H have high p-values with low R-values.   These need to be re-evaluated and they may not be as strong a relationship as suggested.

Chapters 4 and 5

The approaches used for individual responses to flow changes are reasonable, but a more holistic approach is really required. Below is a suggestion of a way (there may be others) to examine plankton, nekton and benthic responses with Primer statistics and couple those results with salinity-based habitats and manatee thermal habitats data currently in place. In Appendix H (pages 54-72), the authors conducted some very interesting multivariate community analyses, but these were not discussed in Leeper et al. (2010), and, in part, they support our concern about community structure change given the individual empirical relationship for plankton, seine and trawl data sets outlined above. It might be very useful to examine carefully the community structure changes using Primer statistics (MDS, ANOSIM, SIMPER, etc.) of the taxa and pseudo-species relative to flow reduction scenarios. We believe one could use the individual empirical relationships (both positive and negative ones) to estimate abundances at particular flows coupled with predicted changes in salinity, etc. These calculated abundance values could be used to create a new data matrix and run some of the appropriate Primer statistics to see if overall assemblage structure would change under different flow scenarios. One could do this at some estimate above and below the linear (mean) values (i.e., ± 10, 15, 20 %) based on the empirical relationships. This may provide some indication of how much the assemblage as a whole could change given the scenarios of interest (change in water flow) and would be a more holistic approach than the standard individual responses documented in Leeper et al. (2010). Given Leeper et al. (2010) currently lists 20 total individual responses used for 2007 and 1996-2009 baseline estimates of non-lagged data, these could be used for the suggested analysis and when the final report is completed on the benthic surveys, they may be able to be incorporated as well. In Primer, we could see rows of species, taxa and pseudo-species with columns being baseline abundances for 2007 and 1995-2009 and then have other columns based on generated abundances given reduced flows (as done individually already). These could be ordinated in MDS, compared with ANOSIM among flow scenarios, and, if you used SIMPER, you could show which flow rates produced significantly lower abundances estimates by species and how many species responded holistically instead of individually. It could be that some taxa do worse or better in different flow scenarios, thus impacting the overall assemblage composition.

Can figures be generated using tables 5-20 through 5-22 that would show salinity-based shoreline changes using data from both the hydrodynamic model and the empirical regression models? It might help visualize how the potential change would look in the Homosassa system.

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Page	¶ #	Line #	Figure (F) **Table (T)**	Comment
5	6	3		Change “model” to “models”
19	Title			Change “Acknowledgements” to “ Acknowledgments” (no “e”)
20	2	5		“…at least 19 named or identified springs or vents.” There appear to be 20 named springs in Figure 2-3, page 33. Should these numbers be the same?
30	3	2		Delete the “a” before Bluebird Springs Park.
49	2	5		Add “k” to Southeast For (k) gage site.
52			F. 2-16	The discharge data in this figure appear to match the discharge data for gage site USGS 02310700 Homosassa R at Homosassa FL; apparently, this gage site number should be in the figure caption, instead of the gage site number that is listed in the caption (02310690), which is the number for Halls River.
53			F. 2-18	The gage site number for Hidden River should be number 02310675, instead of 02310690, which is the number for the Halls River gage.
54	3	8		“poteniomitric” should be “*potentiometric*”.
54	3	10		It is suggested that **nodes** be replaced with *“drain cells”*.
55			T. 2-4	It is suggested that Abdoney, Belcher, McCain, Pumphouse, and Trotter No. 1 springs be identified as comprising the Southeast Fork springs complex.
61	1	9		“sand, silt, muck and silt”
65	3	5-8		The text says 14 stations (10 in Homosassa River and 3 in Halls River and 1 in SE Fork). However Figure 2-24 has 19 stations (13 in Homosassa River and 6 in Halls River). Hard to rectify stations plotted on 2-24 and data on 2-25??
66	1&2			The text cited Figure 2-24 in both paragraphs and it appears it should be Figure 2-25?
66	3	4		What is “B121”?
66	3	7		“figurer” should be “figure”
66	3	7		Is the part of this sentence that states “locations of these sites are not shown in Figure [sic.] 2-25” written correctly? If so, is it possible to include a reference that does show the locations of the sites?
75			F.2-28	This figure is real hard to interpret because of small size and overlap of the symbols.
75			F. 2-28	It is suggested that the gage number be included in the caption.
77	1	3		Can the formulas of Cox et al. (1967) be included in the text?
80			F. 2-31	It is indicated that the solid green line shows the median EFDC model salinity for the river **surface**; Figure F-2 in Appendix F (HSW Engineering, Inc. 2010), which is included in Appendix A of Leeper et al. (2010), indicates that this line is the median EFDC model salinity for the river *bottom*.
81			F. 2-33	It is very difficult to read the legend and understand what data are presented in this figure.
83	2	1		Change “prediction” to “predicting”

Page	¶ #	Line #	Figure (F) Table (T)	Comment
84	1	11-12		How was a **temperature constant of 23.2ºC** used? Should this be "*constant temperature* of 23.2ºC"?
84	1	14		It is suggested that **concordance** be replaced with *"agreement"*.
87			F.2-36	The upper 2 panels are almost impossible to separate observed from simulated signals. I would attempt to make larger as these tell a great deal about model simulation patterns compared to the observed. I suggest you change the two colors so that they do not produce black when overlapped.
88	1			Paragraph describing predicted salinities: Coefficients of determination …ranged from 0.63 to 0.73 (HSW Engineering, Inc. 2010). It is suggested that the specific location for these results, i.e., the table number in Appendix A, be included in the text on page 88.
94	1	6		Looks like this should be Figure 2-20, not 2-23??
97	2	7		Earlier "discharge" was measured as cfs, here it is m/s
99-100			F.3-1 to 3-3	These are very small and hard to read. Color patterns are reasonable but dots are almost impossible to see clearly.
101	1	4		Delete "relatively"
101	1	7		Should be "physiochemical"
107	1	5		Delete "a" before size transects in the …
107	2	3		*Guekensis* is spelled *Geukensis*
110			F.3-2	*Guekensis* is spelled *Geukensis*
110	1	3		Should read "meta-analysis"
110	1	7		The word "tidal" appears redundantly
110	2	2		"sampes" should read "samples"
111	2	7		Should read "suggests" plural
114	3	8		"paludosus" should be in italics
116	3	1-4		I count 41 of the 53 pseudo-species having significant relationships, 13 with quadratic but 28 (not 27) with linear. Also, the authors list 12 negative and 15 positive linear responses but Table 3-5 has 16 positive linear responses. This may explain the 1 difference noted. Needs correction in text.
116	4	7		Seminole killifish (*Fundulus grandis*) is actually Gulf killifish.
123	1	1		Should read "red tides." The period inside the quotes.
124	1	11-14		Redundant "probabilities"
126	2	8		Again, Seminole should be Gulf killifish.
126	2	9		"mollies" should be "molly."
134-135			T. 5-1	Callinectus sapidus in this Table is mis-spelled and should be in italics. It is spelled *Callinectes sapidus* in the seine-net, taxon or pseudo-species and trawl-net sections. All should be in italics. Also, *Lepomis punctatusi* and *Micropterus salmoidesi* are mis-spelled and the "i" on the end of both species name should be deleted.
134	1	6		Looks like Table 5-2 should be Table 5-1.
154	1	10		Delete "of"

Page	¶ #	Line #	Figure (F) Table (T)	Comment
154				No page number
Appendix A Edits and Typos				
xiii				**Table of contents, p. xiii:** Consistent with the information presented in the table of contents for Appendices A-I in HSW Engineering, Inc. (2010), the figures contained in Appendix J in HSW Engineering, Inc. (2010) should be listed in the table of contents.
2-19	2	4-7		Something is missing in this sentence. It makes no sense to me.
2-19	2			Figures 2-25 through 2-33 should immediately follow p. 2-19 if possible.
3-10	2	3		Table 3-3 cited should be Table 3-4.
3-11		3	T. 3-4	Headings in line 3, columns 3 and 4 for the Shell Island gauge are both labeled “Middle”. Should the heading in column 4 be labeled “Bottom” instead? Are the data correctly entered in these columns? Also, it is noted at the bottom of Table 3-4 that “**R is the Pearson Coefficient....”** Is this coefficient defined or referenced somewhere in the report?
4-4	1		F. 4-3 & 4-4	Printed off the page and you can not see legends or captions.
Appendix B Edits and Typos				
				Second paragraph in Introduction: To be consistent with Leeper et al. (2010) and Table 2 (p. 12), Hidden River should be included in this paragraph.
Appendix C Edits and Typos				
Appendix D Edits and Typos				
3-16	1	2		Delete “s” from compares.
3-18	4	5		Peebles 2005 not cited in literature cited section (note to add it if found in red).
Appendix E Edits and Typos				
3-7	6	4-6		Figs 5 & 6 are printed off the page (can not read scales, etc.) and have no figures legends. Same for appendices A-C.
3-8	1	1		Ruppia must be in italics.
3-13			F. 7	Figures 7 & 8 are not cited in the text.
3-14			F.8	Figures 7 & 8 are not cited in the text.
3-16			T.2	Plant names must be in italics like all other tables.
Appendix F Edits and Typos				
			T. 6	Table 6 – *Geukensia* also misspelled as noted above.
Appendix G Edits and Typos				
Appendix H Edits and Typos				
17			F. 2.7.4.2	TIN is upper case is in upper panel but lower case in lower panel.
29	3	2-3		Names need to be in italics.
30	1	3		“fro” should be “from”.
30	1	13		Peebles & Flannery 1992 is not cited in literature cited section.

Page	¶ #	Line #	Figure (F) Table (T)	Comment
31	3	4		Merriner et al. 1976 also not cited in literature cited section.
31	4	6		Peebles 2002 also not cited in literature cited section.
73	5	7		“appeanace” misspelled.
74	2	5		“esutuary” misspelled.
Appendix I Edits and Typos				
Appendix J Edits and Typos				
Appendix K Edits and Typos				
Appendix L Edits and Typos				
Appendix M Edits and Typos				
Appendix N Edits and Typos				
Appendix O Edits and Typos				