NUCLEAR REGULATORY COMMISSION
ATOMIC SAFETY AND LICENSING BOARD
Before Administrative Judges:
Alex S. Karlin, Chairman
Dr. Anthony J. Baratta
Dr. Randall J. Charbeneau

	)	
In the Matter of:	)	
PROGRESS ENERGY FLORIDA, INC.	)	
(Levy County Nuclear Power Plant, Units 1 and 2)	)	Docket Nos.
	)	52-029-COL, 52-030-COL
	)	
	)	July 31, 2012

PRE-FILED REBUTTAL TESTIMONY OF DR. TIMOTHY HAZLETT IN SUPPORT OF CONTENTION C-4 REGARDING ENVIRONMENTAL IMPACTS OF LEVY UNITS 1 AND 2 ON WATER RESOURCES AND ECOLOGY

Q.1. Please state your name.

A.1. I am Dr. Timothy Hazlett.

Q.2. What is your educational background and experience?

A.2. My educational background and experience is described in response to Question 2 of my Prefiled Direct Testimony (**Exhibit INT101**), which was served on the Atomic Safety and Licensing Board ("ASLB") and the parties on June 26, 2012. In addition, a copy of my curriculum vitae is attached to my Prefiled Direct Testimony as **Exhibit INT102**.

Q.3. What is the purpose of your Rebuttal Testimony?

A.3. The purpose of my Rebuttal Testimony is to respond to the Prefiled Direct Testimony submitted by witnesses for Progress Energy Florida ("PEF") and the NRC Staff on June 26, 2012.

Q.4. Could you please provide a summary of your overall conclusions?

A.4. Yes. Overall, there is agreement between the NRC Staff and me that the model relied on by PEF and the NRC Staff is not sufficiently realistic to provide reliable predictions about the effects of the proposed groundwater withdrawal upon wetlands. Furthermore, all the experts, including PEF's experts, agree that there has been no attempt here to realistically model the karst geology and the interactions between the surface water and the aquifer. There is also no dispute among the experts that significantly more site characterization would be needed to enable the creation of a more realistic and reliable model. Although NRC's and PEF's experts have not discussed in detail what additional data would be required, this should not be controversial. In addition to more boreholes, this characterization would include flow tracing and mapping of the existing wetlands and karst features. However, the Staff and PEF stop short of acknowledging that the EIS for LNP should be revised to incorporate such additional characterization and the use of a reasonably accurate model in order to predict impacts of the proposed changes to the existing groundwater regime and derive appropriate mitigation. Instead, they assert that any significant adverse impacts can be detected and mitigated in the future.

I believe that it is feasible for PEF and the NRC Staff to gather relevant data and use an appropriate model to make a reasonably accurate prediction of the environmental impacts of LNP on wetlands. To date, this has not been done. The peer review for the model used here noted that the model has serious limitations, such as the inability to predict salinity or water quality changes and very limited ability to predict how groundwater withdrawal could affect surface water flows and springs. (**Exhibit INT105 at 7**). This peer review also suggested how the model could be modified to be an "integrated surface water and ground water tool that includes all of the major hydrologic processes except for hydraulic routing of water on the overland flow plane." *Id.* at 11. Without these improvements, the current model cannot simulate how the proposed withdrawal would affect water quality in the area, such as by causing increased salinity.

Q.5. Does the NRC Staff's or PEF's testimony contain any new information regarding the environmental impacts of LNP that is relevant to your initial testimony, but was not included in the FEIS? If so, how does that information affect the opinions expressed in your initial testimony?

A.5. Although there is some new information in the NRC and PEF testimony about how the groundwater modeling was done, it only serves to reinforce my original opinions, which remain entirely valid. For example, the testimony presented shows that the NRC Staff and PEF disagree about which calibration of the groundwater model is the best one to use. I agree with Staff that mathematically speaking, the recalibrated model provides the better fit. In addition, using the recalibrated model is more conservative because it predicts higher impacts. Nevertheless, as discussed in my Initial Prefiled Testimony A.3, the model is fundamentally inadequate to predict groundwater behavior, regardless of whether it has been recalibrated, for a number of reasons. The primary reason for this inadequacy is that the model fails to take into account that the aquifers are karstic and therefore not uniform. The model only provides an average prediction of effects over space and time. While this may not be critical when examining the ability of the Upper Floridian Aquifer to supply groundwater to widely dispersed withdrawals, it is not adequate here where a large withdrawal could have significant local effects on surface water and springs. These effects would be particularly pronounced in times of drought, which the current model cannot simulate at all. In general, the testimony by PEF and the NRC Staff confirms that the authors of the FEIS have not taken adequate account of the karstic nature of the geology in the vicinity of Levy Nuclear Power Plant or the potential for extended drought conditions. Therefore the testimony tends to support my conclusion that the model relied upon in the FEIS cannot make reliable predictions about localized impacts.

Q.6. The original model predicts drawdown of less than 0.5 feet in the surficial over a 60 year period and the recalibrated model predicts a drawdown of 0.5 feet at a 1 mile radius during the first year, increasing to a 30 mile radius after 60 years. PEF SoP at 28. PEF asserts that the both models can be used to predict impacts on wetlands and conclude they are insignificant. *Id.* at 29. Do you agree that it is reasonable to use the both models for that purpose?

A.6. No. The difference in onsite simulated drawdown in the Unit 1 & 2 Area in the UFA and SAS between the original model and the recalibrated model is approximately 1.6 ft, or a factor of four. (Exhibit NRC001 p.5-27). In the original model, maximum predicted drawdown was about 0.4 ft and in the revised model the drawdown is about 1.6 ft. If the results of a model change that much due to mere recalibration, it is an indication that the model itself is not conceptually accurate or that the amount and types of data that are put into the model need to be re-examined. In other words, the difference indicates a problem rather than providing a basis for concluding that impacts are insignificant.

Q.7. Do you agree with the Staff that the original groundwater model was a poor fit to the measured data? (Staff SoP at 24)

A.7. Yes.

Q.8. Do you agree with PEF that the recalibrated model also had major problems with fit to the general understanding of the area? (PEF SoP at 24)

A.8. Yes. In addition to the deficiencies I noted in my initial testimony, I think it was most limited by lack of Surficial Aquifer data outside the site area and lack of inclusion of wetlands as hydrologic features in the model.

Q.9. Do you agree with the Staff that neither calibration of the existing groundwater model can predict wetland impacts due to high variability in input parameters and weather patterns? (Staff SoP at 25; PEF SoP at 30)

A.9. Yes.

Q.10. Does the staff take sufficient account of the presence of preferential flow paths in the local geology? (Staff SoP at 40; PEF SoP at 17)

A.10. No. In the groundwater modeling there was no representation of preferential flowpaths in the subsurface.

Q.11. Is it reasonable for the staff to consider the effect of drought conditions for salt drift but not for groundwater dewatering? (Staff SoP at 50)

A.11. This is inconsistent, as saltwater intrusion could be caused by both salt drift and groundwater withdrawal, but the FEIS only attempts to assess the effects of salt drift. Moreover, as I discussed in my initial testimony, the FEIS makes no attempt at all to model the salinity changes that could be induced by the pumping and other proposed changes to the groundwater flow.

Q.12. At page 8, Dr. Rumbaugh states: "The primary cause of uncertainty in groundwater modeling is the lack of complete subsurface data. Drilling wells is expensive; thus, few wells are drilled in any given area. Another source of uncertainty is the fact that some important parameters of the model cannot be measured directly. A good example is recharge (the amount of precipitation that infiltrates the aquifer). Although hydrogeologists can obtain a good approximation of rainfall quantities from gauging and from weather radar data, they cannot measure precisely how much of that rainfall actually makes it to the water table and recharges the aquifer." Did the NRC Staff or PEF evaluate the uncertainties listed above?

A.12. The model(s) used an approach that took advantage of available data, but made no attempt to evaluate the uncertainties induced by the lack of data. The factor of four difference between the drawdown estimates from the two models shows that these uncertainties are very high.

Q.13. Would tracer testing and more well data have helped settle these same uncertainties?

A.13. Successful tracer test results would have added data that may have been useful in enhancing the model(s), if they had the capability of modeling preferential flow paths.

In addition, wells ideally should be spread throughout the model domain and screened in every aquifer represented in the model, in order to eliminate any spatial bias. Clustering of wells would be anticipated in areas of interest.

Q.14. Is it reasonable to use only average precipitation and other weather data in modeling the drawdown impacts at LNP?

A.14. For model predictions made decades into the future, this may not be reasonable, as climatic effects are unaccounted for, such as change in sea level or extremely wet or dry periods.

Q.15. At page 17, Mr. Rumbaugh states: "[The T& J Well] I had considered and rejected as a calibration target in the original calibration of the DWRM2 because the water-level measured at this well was so high that it may not be representative of the regional UFA flow system." Should

this well have been rejected? Could the high water level actually been representative of the UFA system?

A.15. Models should always take advantage of using available data. Excluding a data point may have legitimate provable reason, such as faulty surveying of an elevation or well construction. However, one should not reject data simply because it does not conform with a preconceived notion of reality (conceptual model).

Q.16. Do you agree with the following statement from the Staff's testimony, A.41: "One of the largest sources of uncertainty with respect to estimating groundwater usage impacts is hydraulic properties in the vicinity of the proposed wellfield. No characterization wells have been drilled in this area and thus hydraulic property estimates are based on regional interpretations. This uncertainty will be greatly reduced once the planned wellfield hydraulic characterization activities have been conducted."

A.16. I do believe the *model* uncertainty will be reduced by the additional characterization. However, it is impossible to quantify *a priori* the real reduction in uncertainty that could be obtained by further data acquisition, because the model has fundamental conceptual weaknesses. Furthermore, as discussed in the peer review, a full and integrated (groundwater and surface water) characterization over a range of spatial and time scales and using a variety of field techniques would provide the best results (Exhibit INT105 at p. 11). In addition, the model should take account of local preferential flow pathways.

Q.17. Does the groundwater modeling accurately simulate the impacts upon wetlands?

A.17. No. The model cannot simulate the impacts on wetlands, because it is not an integrated surface water and groundwater model. As discussed above and in my initial testimony, the peer reviewers **(Exhibit INT104)** believe that such an integrated model would be a major improvement and I believe it is necessary here to simulate the impacts on wetlands from the groundwater withdrawals and other changes. For example, reducing the water table could induce additional recharge from wetlands into the lower aquifer. Such induced recharge in the zone of capture for the simulated pumping necessarily should include simulated interaction with surface water features. As also discussed above and in my initial testimony, the model should also include preferential flow paths, such as fracture traces, bedding plane parallel fracture or dissolution features, or other karst conduits. The end result of including these types of features in the model would be a simulated cone of depression that would generally not appear radial in map view, but rather would extend outward in a "star" pattern, seeking water both horizontally and vertically along the paths of least resistance.

Q.18. Do you swear in accordance with 28 U.S.C. § 1746, under penalty of perjury, that this testimony is true and correct?

A.18. Yes I do.

Executed in accord with 10 C.F.R. § 2.304(d) by:

Electronically signed by
Timothy J. Hazlett, Ph.D.

July 31, 2012