DRAFT

COMMENTS SUMMARY REPORT

EXTERNAL PEER REVIEW OF "EVALUATION OF ALTERNATIVE STORMWATER REGULATIONS FOR SOUTHWEST FLORIDA" (THE 'HARPER METHOD')

Prepared for:

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I. INTRODUCTION

Southwest Florida is one of the fastest growing coastal areas in the United States. Extensive areas including wetlands are rapidly being developed into residential and commercial sites. Stormwater management and potential water quality degradation are inherent issues. Florida rules require permits for new stormwater discharges. Under these permits, performance standards and water quality standards are assumed to be met when best management practice design criteria are implemented. In addition, some existing systems were constructed prior to the adoption of current regulatory requirements. The required performance is 80 % reduction of the average annual load of pollutants that would cause or contribute to violations of state water quality standards, with 95% removal of the average annual pollutant load for Outstanding Florida Waters. Unfortunately, additional data are needed to confirm that the commonly used stormwater system designs meet these Florida performance standards.

In August of 2003, in order to address concerns about whether incremental permit reviews under Section 404 of the Clean Water Act were adequately addressing cumulative and secondary effects of wetland fills in the rapidly growing southwest Florida area, the U.S. Army Corps of Engineers (Corps) released the Final Environmental Impact Statement on Improving the Regulatory Process in Southwest Florida, Lee and Collier Counties, Florida (EIS). The EIS predicted continued water quality degradation with the "no action" alternative and recommended the use of specific permit review criteria to reduce habitat fragmentation and provide greater predictability for the applicant. For water quality protection, the EIS recommended that applicants show that post-project pollutant loadings will not exceed pre-project loadings. In order to make this determination, an interim method was agreed to by the federal and state wetland regulatory agencies. The selected method is described in a 2003 report by Environmental Research and Design, Inc. (Harvey H. Harper, Ph.D., P.E.) entitled "Evaluation of Alternative Stormwater Regulations for Southwest Florida" (Harper Method).

Ongoing development in southwest Florida and addressing cumulative stormwater pollutant loads have several interrelated aspects. These include defining appropriate stormwater treatment, defining water quality permit conditions and monitoring requirements, obtaining water quality certification, and meeting water quality standards and assuring that the designated use of Florida waters is met. In addition, cumulative water quality degradation resulting from past, present and future stormwater pollutant loads could result in water body impairments, mandating development of Total Maximum Daily Loads (TMDLs). It is imperative that post-project pollutant loads are not greater than pre-project pollutant loads so that impairments and TMDLs can be avoided.

The U. S. Army Corps of Engineers is currently requesting the water quality determinations described in the EIS, and the South Florida Water Management District is certifying them as part of their water quality certification under Section 401 of the Clean Water Act. The Harper Method is currently being used by Florida and federal regulatory agencies as a tool to assure that post-project pollutant loadings do not exceed pre-project loadings for projects that involve filling over 5 acres of wetlands. Various parties have raised questions or criticisms regarding the method, its assumptions, or its use. Therefore, it is important that the method undergo external scientific peer review.

Peer review is an important component of the scientific process. It provides a focused, objective evaluation of the document or material submitted for review. The criticism, suggestions and new ideas provided by the peer reviewers stimulate creative thought, strengthen the reviewed document and confer credibility on the product. Comprehensive, objective peer review leads to good science and product acceptance within the scientific community. This document was peer reviewed by Dr. James P. Heaney, Mr. Jonathan E. Jones, Dr. Larry A. Roesner, Mr. Ben R. Urbonas, and Dr. William W. Walker.

II. CHARGE TO THE PEER REVIEWERS

The peer reviewers were charged with reviewing the Harper Method to determine its appropriateness for determining stormwater treatment system designs. Post-development nutrient loading must not exceed pre-development loading. Identify deficiencies, recommend modifications, and/or recommend an alternative approach that is relatively simple to employ and straightforward. The Method should accurately estimate pre-project and post-project stormwater pollutant loadings in southwest Florida, and it should be sound from an engineering and scientific standpoint. The Method should minimize the amount of user interpretation required.

1. Does the Harper Method accurately estimate pre-development and post-development pollutant loads for southwest Florida? Why or why not?

2. Is the Method an appropriate method to use for stormwater treatment design system recommendations on a site-specific level? Why or why not?

3. Overall, does the Method reflect the current state of knowledge for stormwater treatment systems? Do you know of a better or more appropriate approach?

4. Does the Method utilize commonly accepted scientific and engineering assumptions and approaches?
5. The Method estimates annual pre-development and post-development runoff from subject property by assigning pollutant concentrations based on land use, estimating water runoff volumes, and calculating pollutant loads. What would you expect to be the greatest sources of error in this approach? How would you reduce the error?

6. What effect would variability in annual rainfall and hydrology have on the overall accuracy of the Method?

7. Land use runoff characteristics were based on a 1994 study by Harvey Harper, Stormwater Loading Rate Parameters for Central and South Florida, and a supplemental literature search. Are you aware of additional data that would be appropriate for southwest Florida?

8. Criticisms have been raised regarding runoff volumes and constituent concentrations for isolated wetlands in southwest Florida. The example on p. 3-23 uses a runoff coefficient of 0.225. Some have suggested that isolated wetlands do not discharge, or if they do they have a nutrient concentration of zero. What is a scientifically sound runoff coefficient for isolated wetlands in southwest Florida? If they do discharge, what is an accurate nutrient concentration for the discharge? Why?

9. Criticisms have been raised regarding runoff volumes and constituent concentrations for flow-through wetlands in southwest Florida. For flow-through wetlands, the Method example assumes that 50% of nitrogen, phosphorus and water volume are retained, and 50% are discharged. Are these assumptions scientifically sound? Why or why not? What is an appropriate runoff coefficient for flow-through wetlands in southwest Florida? What is an accurate nutrient concentration for the discharge?

10. Does the Method appropriately account for pollutant loads from existing and new stormwater ponds? Why or why not? If not, what is a better approach?

11. At what depths do stormwater treatment ponds in southwest Florida offer the greatest treatment benefits for nutrient removal? Are stormwater treatment ponds with depths greater than 6-8 feet (ie. 20 or 30 feet deep) expected to offer treatment benefits or have problems that would not be expected to occur in a pond that is only 6-8 feet deep? Do the benefits provided by a deeper pond exceed potential problems associated with the increased depth?

12. The Method uses some data from residential wet detention lakes to determine natural open water quality conditions. Is this scientifically sound? Are you aware of other appropriate sources of data or approaches?

13. Are the nitrogen, phosphorus, TSS and BOD percent removal vs. residence time curves used to determine the required residence time for wet detention ponds defensible from a scientific and engineering perspective? Do they overestimate percent removal? Are you aware of other sources of information for assessing percent removal vs. residence time?

14. Is the equation on page 3-22 of Harper that relates anoxic depth to secchi depth, chlorophyll a and phosphorus scientifically sound? If not, why not and can you suggest an alternative that improves the scientific accuracy?

15. Is the approach used to select and size the required stormwater treatment technically sound? Why or why not?

16. Are there oversimplifications in the Method that result in large inaccuracies or errors? If so, what are they and how would you address these inaccuracies?

17. Are you aware of other data, studies or models that are more appropriate for application in southwest Florida?

18. Could parts of the Method be clarified to increase consistency among users?

19. In your opinion, what is the weakest aspect of the Harper Method, and how would you correct this?

20. Identify any changes that you would make to the Method so that it more accurately assesses pre-project and post-project loads for southwest Florida.

21. Does the Harper Method lend itself to refinement as more data become available? Which aspects are in need of refinement the most?

22. Are you aware of an alternative approach to the Harper Method that utilizes sound engineering and science that could be used in southwest Florida to determine stormwater treatment design requirements so that post-development loadings do not exceed pre-development loadings?

III. GENERAL COMMENTS

James P. Heaney

Prelude

The questions being posed about the Harper Method (HM) and its applicability to Southwest Florida are being asked in many areas of the United States. My groups at the University of Colorado and the University of Florida (since September 2003) have been conducting national studies to answer these questions under sponsorship of the US Environmental Protection Agency, the Water Environment Research Foundation, the National Cooperative Highway Research Program, and the Jacksonville District of the U.S. Army Corps of Engineers. The EPA report is complete and is being processed for final publication. It was done in collaboration with Professor Wayne Huber and his group at Oregon State U. The WERF and NCHRP reports will be completed in May 2005 and published later this year. The Corps effort is underway and a report should be available in late 2005. These reports, several published papers, three dissertations and three theses that also have relevant background information are referenced at the end of this review. Other citations are also included.

Many of the questions ask for general conclusions about the HM. The overall evaluation of HM needs to be done by evaluating its components, e.g. rainfall, runoff, concentration estimates, load estimates, and BMP performance evaluation. The bulk of my evaluation is presented using this format. Then, responses to the questions are framed within the evaluation of the components of HM. The questions are answered in the order that they were posed.

In order to answer your questions in a more systematic manner, my evaluation of the individual components of the HM are presented below (*Section V and Appendix A of this Comments Summary Report*). The responses to the specific questions are presented in the following section (*Section IV of this Comments Summary Report*). They follow the format of the HM report.

Jonathan E. Jones

The Report is well organized, logical, and easy to read and implement. The authors' rationale is clear and straightforward, and the guidance and methods that they provide can be utilized by design engineers with little difficulty. The authors are obviously highly qualified and are well versed on stormwater quality management, not only in southwest Florida, but nationally, as well. Significant time and effort were invested to prepare the Report, and there are many praiseworthy aspects of the document. The recommendations of the Report are intended to achieve the goal of "no net increase in pollution for selected stormwater constituents under post-development conditions." In our experience, this is a very aggressive goal to meet, which goes well beyond the typical requirement of utilizing best management practices (BMPs) designed in accordance with standard engineering practice to manage stormwater runoff quality. This goal is achievable for only certain pollutants and under certain site-specific circumstances.

The Harper Method could be utilized as a regulatory tool; however, there are a number of aspects where the current scientific and/or engineering basis is not sound and where certain assumptions should be revisited. We also suggest that some additional "reasonableness checking" would be desirable. These questions and concerns are defined in the remainder of our letter. These are not "fatal flaws," but until these questions and concerns are addressed, along with those from other peer reviewers, it would be best to utilize the Report for background information and general guidance rather than as a regulatory tool.

Larry A. Roesner

Summary

It is curious to me that anyone would want to take a problem as difficult as estimating pre- and post-development nutrient loading, and removal efficiency of BMPs, and reduce it to an approach that: 1) "...is relatively simple to employ and straightforward.", but at the same time 2) "...accurately estimate(s) pre-project and post-project loadings in southwest Florida and it should be sound from an engineering and scientific standpoint." and 3) "...minimize(s) the amount of user interpretation required."

That being said, my review of the Harper Method (called The Method hereafter) can be summarized as follows:

1. The Method is "simple to employ and straightforward."

   The Method provides estimates of pre-project and post-project loadings in southwest Florida. It is based on *state of practice* scientific and engineering technology and, in my opinion is more sophisticated than the technology used by 75 - 80 percent of the US municipalities operating under EPA Phase I stormwater regulations. But The Method is lacking in two areas with respect to the *state of knowledge* that exists in the stormwater management field: 1) The Method assumes that the Water Quality Capture Volume (WQCV) is fully recovered between storms, which is not always true; and 2) the use of percent removal versus time to compute the efficiency of constituent removal in the BMPs. The result of the assumption regarding availability of detention storage is that overflow frequency is underestimated, which means that pollutant removal is overestimated. The method of estimating constituent removal also overestimates the removal efficiency of the BMPs, but again, the degree to which the "accuracy" of The Method is compromised cannot be determined without additional analysis.

   There are ways to overcome these deficiencies; improving the accuracy of the constituent removal efficiency is straightforward and easy, but accounting for water left from the previous storm in the BMP at the beginning of the following storm would increase the complexity of The Method over its current simplicity. One must chose between simplicity with reduced accuracy, and more complexity with greater accuracy. Since the authors of The Method acknowledge that the current Method ignores carryover storage in the BMPs, I wouldn't be surprised if they made a conscious decision to give up this accuracy for the sake of simplicity

2. The Method "minimize(s) the amount of user interpretation required."

   So, in my opinion, The Method achieves, without question, at least two of the three major objectives specified for it. Whether is meets the accuracy objective is an open question, because no guidance has been given regarding the desired degree of accuracy expected of The Method, i.e. how accurately should The Method estimate: a) pre-development stormwater pollutant loadings; b) post-development loadings; and c) pollutant removal efficiency?

Addressing Accuracy

While there are many assumptions and approximations in the model that affect its accuracy, three attracted my attention. They are: 1) The assumption that the WQCV is empty at the start of each event; 2) The method that is used to compute constituent removal in the BMPs, and 3) The accuracy of The Method given the variability in the data base used to determine concentrations in the runoff, and constituent removal in the BMPs given the scatter in the curves of percent removal vs. time. These issues are discussed below.

<u>*Assumption that the WQCV is empty at the start of the storm.*</u> The Method assumes that the Water Quality Capture Volume (WQCV) is empty at the beginning of every rainfall event. This assumption is undoubtedly violated numerous times over the year, especially in the summer when the average time between storms (1.66 days) is less than the recovery time of the WQCV. This means that pollutant removal during the summer season is probably overestimated. To what degree this affects the "accuracy" of The Method cannot be estimated without further analysis such as that done by Camp Dresser & McKee (2004) in their peer review of The Method. In my personal experience with stormwater management, accounting for residual runoff in storage at the beginning of the next storm is important, and I would recommend that the author's of The Method consider modifying the protocol to use a continuous simulation model like HEC STORM or equivalent that tracks the volume of runoff in storage over time, and can take into account the timing between storms to determine the actual amount of runoff that is captured in the BMP, and the amount that overflows untreated to the receiving water(see Question 5 below). It is also my opinion, based on experience with continuous simulation at numerous geographical locations, that a 10 year rainfall record is sufficient to generate the water quality statistics required to determine pre-and post-development runoff quality and water quality constituent removal efficiency of BMPs.

<u>*Methodology for computing constituent removal in the BMPs.*</u> The method for computing percent removal of pollutants in The Method has been shown by several investigators (see ASCE/EPA, 2002 and Wong (2002) for example) to be problematical. A much better way to express constituent removal is as a function of the difference between the influent concentration and a "background" concentration such as the pre-development runoff concentration, or the ambient concentration of the receiving waters. Some thought might be given to modifying the ordinate of the charts in Figures 3-6 to be:

$$(C_{in} - C_{out})/ C_{in} - C_o) \qquad (1)$$

where C_o is the background or equilibrium concentration.

Alternatively (and better), it is recommended that the effluent concentration be expressed as:

$$C_{out} = C_o + (C_{in} - C_o) \bullet e^{-kt} \qquad (2)$$

where *t* is the residence time in days and *k* is the removal rate coefficient.

<u>*Accuracy of computed stormwater loads and removal efficiency calculations*</u>. The Method estimates pre- and post-development stormwater pollutant loads using the average concentration for various land uses from Tables 5, 6, and 7. But the expected load (or the load that one would measure in the field) would vary around that number being dependent upon the variability of the concentrations data set that comprises the average concentration value used in the equation on page 2-14. To examine the accuracy with which the model can predict pre- and post-development loads, assuming that there is no error in any other part of the Method, one must set up a set of equations that includes error estimates of the following form:

(3)

(4)

(5)

where

$Q_{i,pre}$ and $Q_{i,post}$ = pre-and pos-development annual runoff as determined by the Method,

$\bar{L}_{i,pre}$, $\bar{L}_{i,post}$, and $\bar{L}_{i,treated}$ = estimated pre-development, post-development and treated post-development watershed load of constituent *i* as determined by The Method,

$\bar{C}_{i,pre}$ and $\bar{C}_{i,post}$ = the pre- and post-development concentrations of constituent *i* in pre-and untreated post-development runoff respectively, and

$\bar{R}_{i,u}$ = removal efficiency (fractional) of treatment type *u* for constituent *i* as determined by The Method.

The terms *eL, eC, and eR* are the magnitude of the error associated with the pollutant load estimations, constituent concentration estimations, and BMP removal efficiency, respectively.

The error terms can be calculated from Tables 5, 6 (and the data used to develop average runoff concentrations for other land uses in Table 7), and the graphs of percent removal vs. residence time for various constituents and treatment methods in Section 3 of the report. The results can be used to assess the accuracy of the estimates of pre-development pollutant loads, post-development loads, and treated post-development runoff based on the accuracy of the water quality data used in The Method.

Ben R. Urbonas

General Comments

The report offers a simplified method, called *Harper Method*, in how to calculate the changes in annual loadings for a number of constituents in response to land-use changes. Nitrogen and phosphorous are the constituents of greatest concern that are being addressed. It also suggests means of sizing "dry retention ponds" and "wet detention ponds" to reduce the average annual wet-weather loads of these constituents to levels that existed before land-use changes occurred. It is based on a number of assumptions and some data are used to support these calculations. However, there appears to have been no attempt to calibrate this method in total using local field data. In addition, it is based on hydrologic methods that are of questionable validity when used in urban areas and on the use of pollutant removal "*efficiencies*".

Providing the stormwater management community, including the regulators, with simplified methods that are relatively easy to understand and follow is a laudable goal and there are a number of successful examples where this has taken place in United States. The Harper Method is an attempt to do so. However, despite other issues that will be discussed later, it provides questionable guidance in the design of both "wet" and "dry" facilities, relies too heavily on individual assumptions about the pre- and post-development site conditions, gives no guidance in how to select accurate parameters such as CN numbers and the degree of DCIA and nDCIA the new development sites will have. All of this can lead to annual runoff calculations that may or may not be representative for a specific development site.

Summary

The reviewing this document reveals the following positive and questionable aspect of the Harper Method:

Positive Aspects of the Harper Method

1) The Harper Method provides a concise protocol for calculating pre-and post-development annual loads for constituent, mostly nutrients, found in stormwater surface runoff.

2) The Method provides consistent protocols for comparing annual loads of constituents delivered by stormwater surface runoff.

3) The method is based in part on field data for constituent concentrations and annual runoff coefficients. Whether that latter correlates well with the calculated annual runoff coefficients based on the NRCS hydrologic methodology is not clear.

Questionable Aspects of the Harper Method

1) The methodology assumes that anything entering the groundwater regime is out of the system and does not affect the receiving waters further. Whether that component of hydrology and water quality is of importance is not clearly answered in this report.

2) The annual surface runoff calculations are based, to a significant degree, on the NRCS hydrologic methodology and require a number of assumptions to implement. My experience has been that the assumptions made can make profound differences in the final answers. Each assumption, unless it is field-data verified, can contribute to calculations that can be different from the site conditions and how they are changed during development.

3) The constituent removals are based on percent removals published (i.e., efficiencies) in literature. This approach has many fatal flaws as the published "efficiencies" were driven by the constituent concentrations entering the treatment facility and are very broadly scattered. Much more effective and reliable way of calculating what leaves the treatment facility is to use seasonal or annual, average effluent concentrations from the tested facilities.

4) In my opinion, and unless groundwater flow calculations can show otherwise, the details for a dry retention basin show inadequate separation between seasonal high groundwater and the pond's bottom. The one-foot separating will be hard pressed to permit full emptying of the pond's volume within the specified 40 hours average separation between storms during the wet season.

Final Conclusion

The Harper Method is based on technologies that I would question for estimating pre- and post-development average annual loads of constituents in stormwater surface runoff. It does provide, however, a methodology that can provide SW Florida a consistent approach for making such calculations. Can we say without reservation that if gives erroneous results? No. However, its methodologies and recommendations leave me to suspect its accuracy, especially if it is to be relied upon to protect sensitive eco-systems.

William W. Walker

Without BMP's, urban development can increase phosphorus export per unit area by a factor of 10 or more. The loading functions developed by ERD indicate that this is also the case in Florida. The stated goal of development with no net increase in phosphorus (or nitrogen, TSS, BOD) loading is ambitious but potentially attainable with the appropriate mix of BMP's, including source controls, swales, infiltration devices (swales, dry ponds) and wet ponds. The document lays out a "cookbook" to assist engineers, planners, and regulatory agencies in designing and evaluating developments. Similar approaches have been applied in other regions (e.g., Scheuler's "Simple Method"). The acknowledged simplifications and uncertainties associated with these desktop procedures are necessary for practical purposes (large numbers of developments with minimal site-specific data, cost, regulatory burden). In larger developments, consideration should be given to performing more detailed assessments that include collection of site-specific runoff data and more sophisticated stormwater modeling, especially when the cumulative impacts of regional development are of concern.

I have found dynamic rainfall/runoff models to be useful for generating runoff coefficient functions or lookup tables similar to Table 4 and for evaluating performance of infiltration devices, swales, and detention ponds (P8, Walker, 1990). These are relatively simple and driven by the same information used by ERD's methodology (hourly rainfall, impervious area, SCS runoff curve number) but do not require the assumption that rainfall events are independent or that devices drain completely between events. P8 routs suspended solids with a distribution of settling velocities or other constituents with simple first or second-order kinetics though networks BMP's. It does not require the assumption that the cumulative performance of BMP's in series can be predicted in a simple linear fashion (Section 3.3, p 3-17). Because of the distribution of particle sizes and settling velocities in runoff, the first detention pond in a series will generally have a higher removal efficiency at a given water residence time, as compared with downstream ponds. This concept also applies to nutrients. P8 was developed for designing site BMP's to achieve regional TSS removal criteria or to compare pre-development vs. post-development scenarios with minimal site-specific calibration and is currently used extensively in Minnesota & Wisconsin. This type of model might be considered in situations calling for more detailed assessments.

One way to simplify this type of assessment is to focus on one or two key stormwater pollutants rather than the entire suite. TSS and/or phosphorus have been used elsewhere. Because of the relative time scales of removal mechanisms and relative magnitudes runoff concentrations, BMP's designed to remove phosphorus and fine suspended solids will typically remove most other constituents of concern (trace metals, hydrocarbons, BOD, nitrogen), or at least bring them down to stable or "irreducible" levels.

IV. RESPONSE TO CHARGE

> **1. Does the Harper Method accurately estimate pre-development and post-development pollutant loads for southwest Florida? Why or why not?**

James P. Heaney

This overall question can only be answered by benchmarking HM against more refined information. The hierarchy of definitive knowledge in descending order is as follows:

1. Monitoring for the study area that measure rainfall, runoff, water quality, imperviousness, etc. This information is used to calibrate a process simulation model such as SWMM. The simulation model is run for several years of rainfall data and predicts annual pollutant loads for that site. A Florida example of such a database is Dr. Betty Rushton's Florida Aquarium parking lot monitoring data in Tampa. However, modeling has not been done for this site.

2. Same as 1 with monitoring data from a comparable study area.

3. Continuous simulation of pollutant loads using a process model like SWMM that is calibrated using "typical" values for the parameters of the model and site-specific climatological, land use, BMP and other data.

4. Frequency analysis of the precipitation data and estimate annual runoff based on this frequency distribution. Assume "typical" values for pollutant concentrations. The HM falls into this category. The frequency analysis can be based on *flow rates or volumes.* The HM is based on volume per event. BMP effectiveness is based on a constant % removal or an assumed residence time removal equation.

5. Design event methods wherein a single rainfall event is used to evaluate performance. Pollutant concentrations are based on "typical" values and BMP efficiencies are based on point estimates, e.g., 80% removal.

6. Design event methods based on assumed annual pollutant loads that are based on "typical" conditions. BMP efficiencies are based on point estimates.

A major effort has been made in our studies during the past several years to find high quality databases that could be used to do evaluations in the first category. However, they are rare. Some of the best available data is based on USGS studies in the 1970's in southeast Florida wherein rainfall, runoff, and water quality were measured for four catchments: low density urban, high density urban, commercial, and highway (Miller 1979). Some of my evaluations for this review are based on our recent analysis of this data.

The HM report does not benchmark their method against measured data for a catchment. Thus, it is impossible to judge its accuracy. My experience would suggest that it should be more accurate than single event, annual load methods since it separates the effects of flow and concentration. It is also safe to say that it would be expected to be significantly less accurate than options 1 to 3 listed above.

Jonathan E. Jones

Questions 1 and 2 are addressed in tandem due to their interrelated nature. The distinction between whether the Harper Method is able to "accurately estimate pre- and post-development pollutant loads" versus whether it is "appropriate for stormwater treatment system design on a site-specific level" is fundamental to the comments we are providing in this review. We view "accuracy" as a scientific criterion, as opposed to "appropriate" as an engineering criterion. Of course, engineering methods that are "appropriate" should be as "accurate" as practical, given constraints on data availability, state-of-the-practice methods that are available, ability of the design engineer to apply the method to meet criteria, etc. However, engineering is an application of science and other disciplines, and assumptions and simplifications that influence "accuracy" must be employed to create an "appropriate" regulatory tool.

Conceptually, the Harper Method appears to be a reasonable method for stormwater regulation in southwest Florida, given that the comments and suggestions stated in this letter are addressed, along with comments of other peer reviewers. The accuracy of the Harper Method is far more difficult to assess. A method with assumptions and simplifications that are necessary for realistic applicability as a regulatory tool, by definition, cannot be expected to be accurate for every given site. From an engineering perspective, the Harper Method, with appropriate revisions resulting from peer review, may be a useful and appropriate regulatory tool. When site-specific data are available that provide more detailed information regarding the underlying assumptions and simplifications of the Harper Method, there should be regulatory flexibility to allow this information to be taken into account for site-level design. Comments in the following section further illuminate this fundamental point.

Larry A. Roesner

As discussed above, this question is difficult to answer without knowing the degree of accuracy desired. Things that affect the accuracy of The Method include:

- The accuracy of the data used to develop pre- and post-development runoff concentrations. Average concentrations are used in The Method, which means that the accuracy of the resulting answers must be interpreted within a statistical measure of the variation within the data set used to generate the averages. Equations 3 and 4 can be used to develop such an error analysis.

- There is the question of how much the assumption that the WQCV is empty at the start of each storm affects the answer.To answer this would require that a continuous simulation model like HEC STORM be used but substituting the Harper Method for runoff calculation, and comparing the results of this simulation with the current Method.

- The regression equations for computing percent removal of pollutants by BMPs have low R2 values. The error inherent in the present method can be estimated using equation 5 above. But it appears that a scientifically better way of expressing removal would be to express removal as a function of the difference between the influent concentration and a "background" concentration as expressed in equation 2 above.

Ben R. Urbonas
See discussion on Section 2 in my "Peer Review" dated March 6, 2005.

In my judgment the answer is no. The runoff volumes are not being calculated accurately for reasons stated in the review.

William W. Walker
See comments in Section 2.1 & 3.5.1

2. Is the Method an appropriate method to use for stormwater treatment design system recommendations on a site-specific level? Why or why not?

James P. Heaney
It's an improvement over single design storm methods or annual loadings. Design storm approaches make very strong assumptions about the expected loads and BMP effectiveness. Annual load methods combine the flow and water quality calculations into a single number. HM separates the runoff effects from the concentration effects using a storm frequency approach.

Jonathan E. Jones
See response to Question 1. Questions 1 and 2 are addressed in tandem due to their interrelated nature.

Larry A. Roesner
The Method is appropriate for use; many Phase I communities use similar protocols. But as stated above, there may be limitations in its accuracy due to the way that carryover BMP storage is handled and the way that pollutant removal in the BMPs is calculated.

Ben R. Urbonas
In my judgment the answer is no. This is because the runoff volumes are not being calculated accurately and "percent removals" are used instead of average annual effluent concentrations. In addition, there appear to be questionable recommendations as to the vertical separation between bottom of retention pond and high groundwater table. The approach also appears to underestimate the "retention basin" volumes and, as a result, overestimate the pollutant loads removed.

See discussion on Section 3 in my "Peer Review" dated March 6, 2005.

William W. Walker
I have concerns about some of the assumptions. See comments on all sections.

3. Overall, does the Method reflect the current state of knowledge for stormwater treatment systems? Do you know of a better or more appropriate approach?

James P. Heaney

Its use of the frequency distribution of runoff is a definite improvement over single event evaluations. However, some of its assumptions appear to be unrealistic. Frequency approaches have been promoted since the 1970s with initial work by Howard (1976) and DiToro and Small (1979). Adams and several co-workers have refined these approaches. A summary of frequency approaches can be found in Adams and Papa (2000). It would be helpful if the HM cited this earlier work and built on its findings.

Frequency approaches have been touted as a simpler alternative to continuous simulation methods such as STORM that predate the use of frequency approaches. However, as Goforth et al. (1983) have shown, the accuracy of frequency approaches needs to be verified using process simulators like STORM or SWMM. The early (Pre PC) rationale for using frequency approaches instead of process simulators was that they were advantageous from a computational point of view. However, with the widespread use of microcomputers, it is quite easy to set up the process simulators and get more accurate results. Research by Heaney and colleagues during the past several years has led to the development of process simulators on spreadsheets that also allow for direct optimization of stormwater designs (Heaney and Lee 2005). Thus, it is very easy to do process simulation directly. For infiltration systems where storage is not significant, the frequency approach can be used (Pack et al. 2004).

Jonathan E. Jones

Questions 3 and 4 are addressed in tandem, as there is considerable overlap between the questions. In many respects the method does utilize commonly accepted scientific and engineering assumptions and approaches and does reflect the current state of knowledge for stormwater treatment systems. However, as discussed in the next section of our letter, in certain areas, this is not the case. To provide a few examples:

- There is undue emphasis on "percent removals" for BMPs and no discussion of observed effluent concentrations. Influent and effluent concentrations can significantly influence the percent removal of a stormwater BMP and effects on receiving waters. We do not recommend reliance on percent removal as a measure of BMP performance or receiving water impacts unless, at a bare minimum, influent concentrations are taken into consideration and reported along with reported or expected percent removals.

- Onsite methods for stormwater management, often referred to as "low impact development," "conservation site design," "better site design," or "minimizing directly connected impervious area," are not mentioned, yet they are highly desirable for water quality management.

- Much more emphasis should be placed on a multi-layered, "treatment-train" approach, per recommendations of organizations such as the American Society of Civil Engineers, Water Environment Federation, American Public Works Association, U.S. Environmental Protection

Agency (USEPA), and others. In addition, to obtain the ambitious goal of assuring that post-development water quality is not worse that pre-development water quality for selected constituents, it is incumbent upon designers to consistently use conservative design assumptions, and this design philosophy is not mentioned.

- There is no discussion of either nutrient or metals speciation.

- The assumption of 100% pollutant removal for dry retention basins is unduly optimistic, particularly for nitrogen, but for other pollutants, as well.

Larry A. Roesner
The Method reflects current state of *practice* for most areas of the United States, but I do not believe it represents the current state of *knowledge*, which has been addressed above.

Ben R. Urbonas
In my judgment the answer is no.

See discussion in my "Peer Review" dated March 6, 2005.

William W. Walker
See General Comments. Other sections also contain suggestions.

4. Does the Method utilize commonly accepted scientific and engineering assumptions and approaches?

James P. Heaney
The report is weak on comparing the HM to methods developed elsewhere particularly outside of Florida. Its use of removal equations with residence times approaching one year is particularly suspect as discussed earlier.

The use of a performance criterion based on no increase of pollutant loads is very unusual. It may not be a realistic control goal as described earlier.

Including wetlands and lakes/open water as "land uses" is very unusual. They are usually viewed as wet-weather controls.

It is difficult to assess many aspects of the HM report because of lack of access to the source material, much of which is unavailable.

Jonathan E. Jones
See response to Question 3. Questions 3 and 4 are addressed in tandem, as there is considerable overlap between the questions.

Larry A. Roesner
Yes.

Ben R. Urbonas
In some cases a limited yes, but for the most part no.

William W. Walker
I don't agree with all of the assumptions, as noted in the attached. Otherwise it is a reasonable approach.

> **5. The Method estimates annual pre-development and post-development runoff from subject property by assigning pollutant concentrations based on land use, estimating water runoff volumes, and calculating pollutant loads. What would you expect to be the greatest sources of error in this approach? How would you reduce the error?**

James P. Heaney

Runoff coefficients can vary widely within a given land use. It is essential to separate directly connected impervious area (DCIA) from other impervious area (OIA) and pervious area (PA) (Lee and Heaney 2003). The sample sizes used to estimate runoff coefficients are very small, e.g.,

- Runoff coefficients for single family residential-2 (other 5 values are calculated in an unspecified manner).

- For DCIA, all of the precipitation can be assumed to runoff after deducting for an initial abstraction [see Lee and Heaney (2003) for an analysis of four land uses in southeast Florida]. However, runoff from pervious areas is a much more complex combination of infiltration rates, available storage, and the effect of antecedent conditions including the effects of irrigation on soil moisture storage. HM provides a way to include the impact of non-DCIA using the probability distribution of the runoff. The key assumption is the available soil moisture storage or infiltration rate.

- A critical and unsupported assumption in HM is to define a storm event as ending if it hasn't rained for three consecutive hours. The proper event definition depends on the residence time of the stormwater (Heaney and Lee EPA 2005 report). Residence time is the sum of overland flow travel time that can be estimated using time of concentration formulas and residence times in the BMPs. Early work focused on centralized controls where event definitions of several hours were appropriate. However, for decentralized controls (AKA low impact development), event definitions are more appropriate.

Jonathan E. Jones

While there are potential errors related to hydrologic information, including rainfall data and gauge used, site-specific characteristics, and other variables discussed in greater detail in the following section, one of the most significant sources of potential error in the Harper Method is reliance on event mean concentration (EMC) pollutant concentration data from a limited number of sites for calculation of pollutant loads. EMCs are useful and are applied by stormwater engineers routinely; however, there is little doubt from published values in the literature and the International BMP Database that there is tremendous variability in pollutant concentrations within a single storm event and from one storm event to another. For example, consider runoff from a parking lot adjacent to a recreational area that is heavily used on weekends. Pollutant EMCs are likely to higher for a storm event occurring on a Sunday evening after a weekend of heavy use in the summer than from runoff later in the following week after a period of reduced use and antecedent events that have already washed off pollutants. Engineers and scientists will always want "more data," and the accuracy and appropriateness of an engineering method will improve as more data become available for analysis. Water quality data can be expensive to collect (more so than hydrologic data), so it is not surprising that water quality data and assumptions would be a large source of error in the estimation of pollutant loadings.

Larry A. Roesner

As stated above, the greatest sources of error are:

1. Assumption that the WQCV is empty at the start of each rainfall event, and
2. The method for calculating percent removal of pollutants in the BMPs

I recommend: 1) using a continuous model such as HEC STORM to process the long term record of rainfall data and simulate runoff, water capture and release in the BMPs, and removal of pollutants, substituting Harper's algorithm for computing runoff from any given storm, but with soil moisture capacity computed continuously using evapotranspiration rates to recover soil moisture capacity between storms; and 2) change the method of computing pollutant removal to a form like equation 2 above.

Ben R. Urbonas

Greatest source of errors are runoff volume using the NRCS-based method and then the resultant load calculations, followed by the use of "percent removal" by treatment facilities instead of effluent concentrations. This could be improved using continuous simulation with a calibrated model to generate seasonal and annual runoff volumes form different land uses and types of soils and the volumes captured by "retention ponds."

See discussion in my "Peer Review" dated March 6, 2005.

William W. Walker

The Method estimates annual pre-development and post-development runoff from subject property by assigning pollutant concentrations based on land use, estimating water runoff volumes, and calculating pollutant loads. What would you expect to be the greatest sources of error in this approach? How would you reduce the error?

6. What effect would variability in annual rainfall and hydrology have on the overall accuracy of the Method?

James P. Heaney

During dry years, virtually all of the runoff would emanate from DCIA. Accordingly, the pollutant concentrations should be based on functional categories such as streets and roofs and not on aggregate land use such as single family residential.

During very wet years with major storms, loadings from non-DCIA areas could predominate with a disproportionate amount of the annual load coming from the larger storms as non-DCIA areas are flushed out by significant overland flow.

What situation is "critical" depends on the nature of the receiving water and the mix of DCIA and other areas. The combined impact of the wet-weather flows as flow increases can range from almost a pure dilution that occurs in some CSO and SSO situations (Wright 2003) to an increasing concentration as flow increases that is more typical of non-DCIA land uses like agriculture and natural areas

Jonathan E. Jones

Variability in average annual precipitation and hydrology can have significant effects on the accuracy of the Harper Method, and more importantly, on BMP effectiveness and receiving water impacts. The idea of an "average year" is useful for planning and regulatory purposes, but, in terms of actual precipitation for a given year, is an anomaly. Most, if not all, years will be above or below average in terms of precipitation and many other environmental variables. Above-average years pose greater challenges to water quality protection that below-average years. Consider the antecedent dry period reported in Table 3 of the Report of 1.66 days versus the assumption of full recovery of treatment volume for dry retention ponds for the wet season stated in Table 8 with a recovery criteria of < 40 hours. For antecedent dry periods less than the mean, a pond with a 40-hour recovery time would not be fully recovered in time for the next event.

To account for variability in hydrologic variables, including precipitation from year-to-year, we would recommend continuous simulation. We realize that is it not practical for a regulation to require continuous simulation for a site-level design; however, we believe that continuous simulation using available precipitation data for a variety of land uses and levels of imperviousness could be useful for developing sizing guidelines for water quality facilities. Recent research by Dr. Larry A. Roesner, P.E. of Colorado State University, who has extensive experience in Florida through his work with CDM, could be helpful to review in regard to continuous simulation. At a minimum, continuous simulation should be used as a "reasonableness check" on guidance provided for sizing using the Harper Method.

Larry A. Roesner

The Method describes performance in an average rainfall year. Due to the way that it handles WQCV available at the beginning of a rainfall event, I would expect the model to be more accurate in low rainfall years when the time between storms ins larger, and the assumption that the entire WQCV is available at the start of the rainfall event is more often met. I expect The

Method to be less accurate in wet years when the timing between successive rainfall events is shorter than normal and the WQCV will often not be wholly available.

Ben R. Urbonas
See comments 1, 2 and 3 on page 2 of 7 in my "Peer Review" dated March 6, 2005. The method used to reduce continuous rainfall record to annual runoff coefficients assumes statistical independence between many variables.

See discussion in my "Peer Review" dated March 6, 2005.

William W. Walker
No.

7. Land use runoff characteristics were based on a 1994 study by Harvey Harper, Stormwater Loading Rate Parameters for Central and South Florida, and a supplemental literature search. Are you aware of additional data that would be appropriate for southwest Florida?

James P. Heaney

This 1994 study is not in the open literature so it is hard to evaluate it without being able to review it. The runoff characteristics in the HM database are based on very few studies. Most of the estimated runoff coefficients in the HM database are based on calculated values. However, the report does not tell how the calculations were done. Lee and Heaney (2003) have evaluated runoff coefficients for four land uses in southeast Florida based on the USGS studies done during the 1970s. This database remains one of the best ones for estimating runoff and loads. HM (Table 8.1) lists a calculated runoff coefficient for the Pompano Beach USGS study by Mattraw et al. (1978). However, Mattraw et al. (1978) provide rainfall-runoff estimates (Lee and Heaney 2003). Similarly, HM shows a calculated runoff coefficient for the Miami Kings Creek Apartments studied by Miller (1979) as presented in Lee and Heaney (2003).

Jonathan E. Jones

We are not aware of additional data that would be appropriate for southwest Florida, although we presume that the Report authors have contacted the following stormwater experts for land use runoff characteristics and loading rate parameters: Eric Livingston, State of Florida; Dr. James P. Heaney, P.E., University of Florida; Dr. Martin Wanielsta, University of Central Florida; Dr. Charles Rowney, P.E., Consultant; Dr. Larry A. Roesner, P.E., Colorado State University; and Dr. Betty Rushton, P.E., South Florida Water Management District. In addition, Dr. Robert Pitt of the University of Alabama has developed a comprehensive land use runoff characteristics database, which could provide a valuable check.

Larry A. Roesner

No, I assume the authors are familiar with the national databases and have reasons for using the numbers that they did, based on their many years experience in Florida.

Ben R. Urbonas

The Nationwide Urban Runoff Program report published by EPA in early 1980s and the follow-up data collected by Phase 1 municipalities for EPA and the States when they applied for their stormwater discharge permits and later compiled and published by Pitt {**http://unix.eng.ua.edu/~rpitt/Research/ms4/Paper/Mainms4paper.html**}.

William W. Walker

No.

8. Criticisms have been raised regarding runoff volumes and constituent concentrations for isolated wetlands in southwest Florida. The example on p. 3-23 uses a runoff coefficient of 0.225. Some have suggested that isolated wetlands do not discharge, or if they do they have a nutrient concentration of zero. What is a scientifically sound runoff coefficient for isolated wetlands in southwest Florida? If they do discharge, what is an accurate nutrient concentration for the discharge? Why?

James P. Heaney

Runoff from any control including wetlands can only be calculated using a water budget. There is no basis for picking a single runoff coefficient. Wetlands are just a form of detention system.
Table A.20 shows the database upon which wetlands runoff coefficients are estimated. Only one data point with a value of 0.303 is presented (Harper et al. 1985). The other three are calculated by an unspecified method.

Wetlands can be evaluated like any other storage-release BMP. Continuous simulation can be used to estimate their expected performance. This approach is much better than selecting a single performance measure such as a runoff coefficient of 0.225 for all wetlands. Kadlec and Knight (1996) describe methods to evaluate wetlands and include a database on wetlands performance.

Jonathan E. Jones

We have considerable experience with wetlands hydrology and water quality, although not in Florida, specifically. Our experience does indicate, however, that it is very difficult to generalize runoff coefficients and nutrient concentrations for wetlands – this is one area where site-specific data are highly desirable. With regard to the specific questions posed in No. 8, some isolated wetlands do not discharge (except during extreme events), while other isolated wetlands do discharge – again, this is highly site specific and may be seasonal in nature. It is important to bear in mind that in most wetlands, for much of the time in a typical year, soils are saturated to the ground surface. Consequently, if a rainfall event occurs, there will be a discharge from the wetland. We strongly disagree with suggestions that discharges from isolated wetlands would have nutrient concentrations of zero – this is not supported by any scientific literature regarding wetlands that we are familiar with. We cannot address your question for a scientifically sound runoff coefficient and an accurate nutrient concentration; these parameters should be developed using the best available data, supplemented by additional field data for isolated wetlands. When all of these data are gathered and analyzed, there is the distinct possibility that there will be so much scatter in the runoff coefficient data and nutrient yield

Larry A. Roesner

I doubt that there is a *scientifically sound* runoff coefficient for isolated wetlands. All runoff will be captured in the wetland until its retention capacity is exceeded, at which point it will overflow (discharge). This phenomenon could be represented in STORM, where the wetland retention capacity is set as the storage volume, and the "treatment rate" set at the combined rate at which water is removed from the wetland by ET and infiltration to the groundwater. The computed "overflows" will represent the periods of discharge from the wetland.

Regarding the nutrient concentration of the discharge, there is no such thing as *zero* concentration of nutrients. I do not know an "accurate" concentration for the nutrient concentration during discharge. I expect that it would be low and close to the "background" concentration as discussed above, but it is commonly reported in the literature that during certain periods of the year, wetlands become net *dischargers* of nutrients, so I would not think that there is a single value for the nutrient concentration in the discharge from an isolated wetland. I am not familiar with the literature regarding background concentrations of nutrients or nutrient export from wetlands in southwest Florida.

Ben R. Urbonas

I have no opinion on this except these coefficients appear to be based on data in Florida. One has to investigate how these data were collected and processed to answer this question. I just assumed it is accurate.

William W. Walker

See comments on Section 3.5.1.

9. Criticisms have been raised regarding runoff volumes and constituent concentrations for flow-through wetlands in southwest Florida.
- **(a) For flow-through wetlands, the Method example assumes that 50% of nitrogen, phosphorus and water volume are retained, and 50% are discharged. Are these assumptions scientifically sound? Why or why not?**
- **(b) What is an appropriate runoff coefficient for flow-through wetlands in southwest Florida?**
- **(c) What is an accurate nutrient concentration for the discharge?**

James P. Heaney

a) These assumptions are not sound. Flow-through wetlands vary widely in how they function. A process simulation can be used to make an informed estimate of the fate of the water and N and P

b) A water and nutrient budget is needed to answer this question. It is easy to do this calculation on a spreadsheet.

c) Answer depends primarily on the initial concentration of water entering the wetlands, the wetland kinetics and the associated residence time in the wetland. Process simulators such as DMSTA (Walker and Kadlec 2005) can be used to make these estimates. Extensive data are available from the studies of Stormwater Treatment Areas (STAs) as part of the Everglades Restoration. These studies have been done at a wide range of spatial scales under a variety of loadings. We are currently evaluating these studies as part of a project to estimate the nutrient effluent quality from the STAs downstream of a proposed reservoir.

Jonathan E. Jones

These assumptions, based on literature that we are familiar with and data provided in the Report, are not scientifically sound. A 50%-50% assumption for phosphorus and water volume retention by flow through wetlands may be a useful approximation for regulatory purposes, but does not account for the complexities of wetland hydrology and water quality, including seasonal and annual variability in groundwater levels, biological and chemical processes, and other factors. On any given site, it is unlikely that the 50%-50% assumptions would be accurate on an annual or even long-term basis. Please refer to comments on Question 8 above regarding site-specific data, variability, and scatter.

Larry A. Roesner

These assumptions may be alright on the average, but for small storms, or during low rainfall years, a much higher fraction of the runoff volume and its constituents will be retained in the wetland since the residence time is higher. I expect that they would behave similarly to the curves of percent removal versus residence time shown in Figures 3-6 for wet detention ponds. During wet years, I would expect smaller volumes to be retained and higher concentrations of nutrients to be discharged due to the shorter detention times, and shorter periods between storms for the wetland to recoup detention storage capacity.

Ben R. Urbonas
I have no opinion on this except these coefficients appear to be based on data in Florida. One has to investigate how these data were collected and processed to answer this question. I just assumed it is accurate.

William W. Walker
Models are available for predicting water & nutrient budgets of flow through wetlands (e.g., http://www.wwwalker.net/dmsta) It is not possible to generalize about percent removals of flow or nutrients as these would be highly site-specific. A conservative assumption of no removal would be appropriate without site-specific modeling.

10. Does the Method appropriately account for pollutant loads from existing and new stormwater ponds? Why or why not? If not, what is a better approach?

James P. Heaney

Here again, the direct way to answer this question is to set up and run a process simulator, ideally with some local data. The simulations can be done on a case by case basis or a range of simulations can be done to generalize these results. We are using this approach in a soon to be completed study for the National Cooperative Highway Research Program (NCHRP). For example, the HM type of frequency distribution for Ft. Myers is being run for cities across the United States.

Jonathan E. Jones

Although we agree with much of the discussion regarding stormwater ponds (wet detention systems) in the Harper Method, we do have some specific questions and concerns, as presented in the following section. For example, the method for calculating detention time provided on Page 3-10 provides an average annual detention time, for an average runoff year. This calculated value is not representative of a wet period and/or a wet year. We pose questions, below, regarding the regression graphs (Figures 3-6). For example, it is not clear from the text what the nature of the data points is. There are fewer data points than we would have expected given the magnitude of wet pond data available for southwest Florida. There are very few data points for residence times beyond 100 days. Interestingly, it appears that if the data points for residence times of greater than 100 days were excluded, the "R2" values would be significantly lower than they currently are (and they are already low, in the case of nitrogen and phosphorus). The regressions graphs show percent removal on the vertical axis; it would desirable to have effluent concentrations on the vertical axis, instead, or alternatively, to use a method that normalizes percent removal to influent concentrations along with reporting of influent and/or effluent concentrations in conjunction with the graphs.

We do agree with the basic assumption made in the Harper Method that pollutant removal can be correlated with residence time, and if the authors can address the concerns and questions that we have raised in this regard, we would be satisfied that the approach is reasonable.

Larry A. Roesner

The Method is OK, as discussed above, but I believe its level of accuracy would be improved by: 1) changing to continuous simulation so residual runoff in the BMP from previous the previous storm is adequately accounted for; and 2) computing pollutant removal in the pond with equation 2 which takes into account the "equilibrium concentration" of constituents in the water.

Ben R. Urbonas

This was addressed above under item 2. Also, see discussion on Section 3 in my "Peer Review" dated March 6, 2005. The fundamental issue is the use of "percent removals" and the hydrologic method use as the basis for estimating annual load calculations.

William W. Walker
No opinion.

11. a) At what depths do stormwater treatment ponds in southwest Florida offer the greatest treatment benefits for nutrient removal?
b) Are stormwater treatment ponds with depths greater than 6-8 feet (ie. 20 or 30 feet deep) expected to offer treatment benefits or have problems that would not be expected to occur in a pond that is only 6-8 feet deep?
c) Do the benefits provided by a deeper pond exceed potential problems associated with the increased depth?

James P. Heaney

a) Depth is less important than residence time. Pollutant uptake is a nonlinear function of residence time with strong diminishing marginal returns as detention time increases.

b) The HM includes regression equations to estimate pond stratification as depth increases. Probably a more direct concern is that the pond would be below the ground water table. A variety of impacts could occur in this case.

c) Do the benefits provided by a deeper pond exceed potential problems associated with the increased depth?

Jonathan E. Jones

WWE has not had occasion to determine which depths for stormwater treatment ponds in southwest Florida offer the greatest treatment benefits for nutrient removal. Our experience elsewhere in the United States indicates that it is preferable to construct wet ponds that are deeper than 6 – 8 feet, primarily because this reduces algae problems and provides room for sediment accumulation. As duly noted by the Harper Method, the problem with very deep wet ponds (20 – 30 feet) is that they can stratify, such that incoming storm flows do not mix with water in the hypolimnion. However, as the authors note, if stratification is likely to be an issue, this can be addressed with water column mixing.

Larry A. Roesner

There is no single answer, but based on my 15 years experience with stormwater management in Florida, and experience with ponds in other areas of the United States, I have formed some hypotheses that, while untested, I believe to be pertinent.

In most areas of the US, a pond over 6-9 feet deep will stratify and become anaerobic because there is no mechanism for aerating the water that deep. But I have seen ponds much deeper than that in Florida that are very clean and not stratified. My hypothesis is that these ponds are dug into the groundwater aquifer and *that the groundwater is moving through the pond at a sufficient rate to keep the pond from stratifying*. In this case, I believe the deeper the pond, the better it is at removing pollutants from runoff. But if the pond is dug into the groundwater that is stagnant or moving very slowly, then the pond will stratify as ponds do in other locations in the US become anaerobic and not perform as well as the shallow ponds (6-8 feet deep).

In Southwest Florida, I expect that most, if not all, deep ponds penetrate the groundwater table so the question of depth vs. effectiveness is dependent upon the rate of movement of the groundwater. But for those near the coast, I would expect the groundwater to be saline so that the pond basically floats on the saltwater groundwater. In this case, I would not be surprised to see the saline portion of deep ponds go anaerobic because the fresh water bubble will not be that deep and rate of transfer of oxygen across the saltwater-freshwater interface will be low.

Ben R. Urbonas

See discussion on Section 3 in my "Peer Review" dated March 6, 2005

William W. Walker

See Comments on Section 3.3.2. I have not seen specific data that justify these deeper non-conventional designs.

12. The Method uses some data from residential wet detention lakes to determine natural open water quality conditions. Is this scientifically sound? Are you aware of other appropriate sources of data or approaches?

James P. Heaney
Analogies between wet-weather detention systems and natural lakes, sedimentation units in wastewater treatment plants, and wastewater oxidation ponds have been used for many years to approximate the behavior of wet weather detention ponds. Natural lakes tend to have detention times in the order of months as opposed to a few days for stormwater ponds. The kinetics shown in the HM with residence times approaching one year are very atypical of stormwater detention systems. A more representative range would be residence times of 10 days or less.

Jonathan E. Jones
We do not agree that it is appropriate to utilize data from residential wet detention lakes to determine natural open water quality conditions. We are not aware of other appropriate sources of data for natural open water bodies in southwest Florida.

Larry A. Roesner
Before rendering an opinion on this question, I would want to know which data came from residential ponds and which from natural lakes. I could not determine from the report, which data were taken from residential wet detention lakes. The report does say that that water quality in the lakes reported ranged from mesotrophic to hypereutrophic. If the data taken from the residential lakes is primarily in one of those categories, while the remaining data are in the other category, then there is obviously a bias in using residential ponds. I note that the report *does* recommend that site specific data be used when available.

Another consideration is the wet season residence time of the lakes. The design criteria specified in The Method for residential ponds, are intended to result in a man-made lake that behaves similarly to natural systems if the residence time is more than 30 days during the wet season. If the residential lakes from which data were taken for this study were designed according to the guidelines of The Method, and have a residence time of more than 30 days during the wet season, my opinion is that the water quality in those lakes is close to natural.

Ben R. Urbonas
See my answer to question 1 and my discussion on Section 3 in my "Peer Review" dated March 6, 2005.

William W. Walker
Monitoring.

13. (a) Are the nitrogen, phosphorus, TSS and BOD percent removal vs. residence time curves used to determine the required residence time for wet detention ponds defensible from a scientific and engineering perspective?
(b) Do they overestimate percent removal?
(c) Are you aware of other sources of information for assessing percent removal vs. residence time?

James P. Heaney

(a) See the discussion in the previous section regarding these performance curves.

(b) and (c) Fitting a single simple equation over such a wide range of detention times is quite risky since the kinetics would be expected to vary from an initial high removal rate in the first few hours to much lower rates as time increases. Also, k depends heavily on initial concentration which is not considered in this method. Expressing removal as a function of residence time, initial concentrations, and reaction rates is a standard practice in wastewater and stormwater process engineering (e.g., Metcalf and Eddy 2003). Only a few reliable data sources are available for stormwater detention systems due to lack of complete monitoring data to do a mass balance on water and the constituents of interest including change in storage in the control unit. Recent research stresses the importance of initial concentration on the expected performance of the control unit. Also, it is not desirable to use only normalized plots of % control vs. detention time since this data masks the actual conditions.

Jonathan E. Jones

As noted in the response to Question 10, there are concerns related to the nitrogen, phosphorus, total suspended solids (TSS), and BOD percent removal versus residence time curves presented and the Report. These concerns, in many ways, are related to the amount of data used for statistical analysis, but also include pre-processing of data (averaging and other methods used to derive data points), statistical significance of results, and statistical methods (e.g., using a fourth order polynomial equation for BOD to closely fit what is widely recognized as a first order process to achieve an R2of 1.0). Judging whether the curves presented over- or under-estimate percent removal is not possible given the scope and budget of this review. The important thing to understand is the variability in the data (characterized by the R2 value shown on the figures). The scatter of the data shows that the curves can both under- and over-estimate performance in terms of percent removal. Greater confidence and less under- and over-estimation can only come through collection of more data, but can never be completely overcome due to site-specific characteristics.

Other methods are available for estimating removal for some pollutants. For example, the USEPA Quiescent Dynamic Settling Model for detention pond design (USEPA 1986), which is based on particle size distribution could be applied for TSS and particulate.

Larry A. Roesner

The use of percent removal curves used in The Methodology is a typical approach used by many municipalities for a long time and is defensible. But in the last several years, investigators have had problems with this approach when constituent concentrations are low. A more accurate way to express removal is given by equation 2 in the Summary. See ASCE/EPA (2002) and Wong (2002) for more information on this approach.

Ben R. Urbonas

I do not believe they are defensible. See discussion on Section 3 in my "Peer Review" dated March 6, 2005.

William W. Walker

Probably over-estimate removal for N,P, & BOD. See comments on Section 3.2.2.

14. Is the equation on page 3-22 of Harper that relates anoxic depth to secchi depth, chlorophyll a and phosphorus scientifically sound? If not, why not and can you suggest an alternative that improves the scientific accuracy?

James P. Heaney

The equation on p. 3-22 is empirical; it's simply a linear regression. It is derived from 1974 empirical equations of Dillon and Rigler. The Dillon and Rigler reference is missing in the report. I doubt that any simple regression equation is going to provide an accurate estimate of the anoxic depth unless the database reflects similar systems in Florida.

As a minimum, a more thorough literature review of stratification in stormwater ponds should be included. If these ponds have more typical residence times of a few days or weeks and highly variable stages, then one would not expect stratification to be an important factor. Here again, a simple process simulation model would provide a much more meaningful approach than using a regression equation.

Jonathan E. Jones

The scope of our review does not enable us to spend the time necessary to evaluate the relationship (if any) among anoxic depth versus secchi depth, chlorophyll a, and phosphorus. We agree, however, that this is an important question to evaluate. If feasible, one or more lake water quality scientists (limnologists) with expertise in southwest Florida should address this question.

Larry A. Roesner

The fact that Harper used a data set of more than 150 sets of measurements and got an R2 of 0.98 for the fit of his equation to that data indicates that the empirical relationship is a scientifically valid description of the relationship between the variables. Whether it can be defended on the basis of first principles is another story; but we have used empirical functions to describe cause-effect relationships since the beginnings of scientific investigation, and I dare say there are more empirical descriptions of cause-effect relationships in the ecologic literature than there are equations derived from first principles of physics, biology and chemistry.

I can see some reason behind the selection of variables, i.e.

- Secchi disk depth is a measure of light penetration and thus the depth of solar heating, which is a principal factor in determining the depth of the thermocline,
- Chyl-*a* is a direct indicator of night-time respiration a principal drain on the oxygen in the water, and
- Total P is a good indicator of the rate of the algae growth rate (being the limiting nutrient) and indicates the potential for sustaining the O_2 demand.

Ben R. Urbonas
See discussion in my "Peer Review" dated March 6, 2005.

William W. Walker
See comments on Sections 3.2.1 and 3.2.2.

15. Is the approach used to select and size the required stormwater treatment technically sound? Why or why not?

James P. Heaney
See discussion in the previous section.

Jonathan E. Jones
We have three primary concerns regarding whether the Harper Method is "technically sound" with respect to selecting and sizing of stormwater treatment facilities:

- Please refer to the comments in responses to Questions 3 and 4 regarding "treatment train" and "multi-layer" stormwater management approaches. The Harper Method addresses only two types of BMPs. Stormwater management literature clearly supports a multi-layer approach rather than a "end of pipe" treatment approach. Onsite BMPs, conveyance BMPs, and/or others, in conjunction with the BMPs addressed in the Report, are the most effective approach for stormwater management for development.

- A second underlying concern in the heavy reliance on percent removals in the Harper Method. This has been discussed in responses to questions above and is further discussed in comments in the following section.

- Driscoll (Driscoll et al 1989) performed analysis of precipitation across the United States and in Florida for USEPA and used a gauge in Miami with a longer period of record than the Ft. Meyers gauge used by the Harper Method. Accounting for the exclusion of data from 1980 - 1984, and 1986 – 1992 for the Ft. Meyers gauge per footnotes on Tables 1 and 2, the Harper Method is based on a 22-year period of record. We have not analyzed the Driscoll data in detail to determine which years may have been excluded in his analysis; however, we believe that the Report should explain why this gauge is not considered appropriate. In addition, Driscoll used a 6-hour inter-event criterion for storm separation to define storm events versus the 3-hour criterion used in the Harper Method. A 3-hour criterion may be appropriate; however, no discussion or justification is provided in the Report, and significant differences in the number of annual events, mean event precipitation, antecedent dry times, and other hydrologic factors arise from assuming a 3-hour versus a 6-hour storm separation time to define a "storm event". Additional discussion is provided in the following section.

Larry A. Roesner
I believe so; the approach follows closely the state-of-practice as defined by ASCE and WEF for the design of urban runoff BMPs. I was one of the original developers of the approach for retention pond sizing; it is based strongly on Vollenwieder principles of lake behavior (see Hartigan, 1989)

Ben R. Urbonas

I do not believe it to be sound. See discussion on Section 3 in my "Peer Review" dated March 6, 2005.

William W. Walker

See comments on Sections 3.2.1 and 3.2.2.

16. Are there oversimplifications in the Method that result in large inaccuracies or errors? If so, what are they and how would you address these inaccuracies?

James P. Heaney

Key sources of error in HM are:

(a) Using only one climatic station and only part of the available record.

(b) Using only 19 bins instead of using bins with increments of 0.01 inches.

(c) Basing soil moisture storage on CN alone and not calibrating it against local infiltration measurements.

(d) Defining DCIA to include: "It is also considered directly connected if runoff from it occurs as concentrated shallow flow that runs over a pervious area, such as a roadside swale, and then into a drainage ditch." (p. 2-5, paragraph 2). Recent studies in California (Barrett 2004, Pack 2004) show that roadside swales have a major beneficial effect in reducing the quantity and improving the quality of highway runoff. DCIA should not include this type of other impervious area.

Jonathan E. Jones

Please see responses to Nos. 2 and 3.

Larry A. Roesner

There are oversimplifications as noted above in the Summary above. But as I have noted previously, the effect on accuracy – or lack of accuracy – has not been determined. Based on my experience, I recommend the changes to the model suggested under question 5 be implemented.

Ben R. Urbonas

There are potential errors and inaccuracies. They may be large, but without a comparative study using methods I suggested in my "Peer Review" dated March 6, 2005 I cannot make a definitive judgment.

William W. Walker

Performance of wet detention facilities dependent on depth & concentration, not just water residence time. The method may under-estimate runoff from pervious areas (see comments on Section 2.1)

17. Are you aware of other data, studies or models that are more appropriate for application in southwest Florida?

James P. Heaney

My group at the University of Colorado (until Sept, 2003) and the University of Florida (Sept. 2003-present) have been conducting research for the US EPA, WERF, NCHRP, and U.S. Army Corps of Engineers, Jacksonville, to develop improved methods for simulating and optimizing wet-weather water quality. Much of this work has been done in collaboration with Professor Wayne Huber of Oregon State U., a long-term colleague at the U. of Florida from 1968-1991. The focus of the research during the past several years has been on smaller BMPs associated with source or upstream control. Interest in decentralized controls has come from a desire to manage the problem at its source as a more sustainable solution than primary reliance on larger, centralized downstream controls. In order to evaluate decentralized controls, it is necessary to model stormwater behavior at the individual parcel level where response times are very short and of the order of a few minutes for overland flow. A conceptual advantage of micro-scale evaluations is that it is possible to collect much more complete data sets than is feasible at larger spatial scales. The on-going studies for the US Army Corps of Engineers are an example of an evaluation of a large stormwater treatment area in the Everglades Agricultural Area. Excellent data are available for these studies. The best data sets that we have found for Florida are:

(a) USGS studies of four catchments in southeast Florida (Lee and Heaney 2002).
(b) A variety of studies by Dr. Betty Rushton of SWFWMD.
(c) Studies in support of the Everglades Restoration.
(d) Early highway studies by UCF (Wanielista, Yousef, et al.) appear to be good but we don't have these data sets.

A significant effort was expended in evaluating all available data on the performance of stormwater BMPs. The approach was to select the best available data and then run SWMM and our spreadsheet models to evaluate the integrated performance of these BMPs. The results were very sobering. For a variety of reasons, there is virtually no reliable data on the performance of wet-weather BMPs that is sufficiently detailed to do process modeling. The entire ASCE/EPA BMP database is populated with storm event measurements only. No intra-storm data are available nor is information available on how storage changes in the control units over time. Another critical piece of information is treatability data for stormwater constituents, e.g., settling column data. Here again, reliable data are meager. Rapp (2004) shows how these data can be analyzed to estimate removal as a function of residence time and initial concentration. Finally, the assumed mixing regime is an important determinant of water quality control effectiveness. Tracer studies allow us to estimate the degree of short-circuiting that can occur in control units but this data is rare for stormwater controls. Kadlec and Knight (1996) show how tracer data can be used to estimate the actual mixing regime. Thus, we have had to conclude that insufficient data are available on comparable controls to argue that their performance can be used to provide a reliable guide to the expected behavior of new BMPs in SW Florida or elsewhere.

On the positive side, this research has demonstrated some key elements and newer methods for evaluating wet-weather BMPs including:

(a) Spreadsheets (SSs) can be used to simulate BMP dynamics except for complex cases of backwater effects in sewers. SSs are used by virtually all engineers and stormwater professionals. SWMM 5.0 is now available. It is much easier to use than the old Fortran SWMM. Thus, it is easier than ever to set up process level simulations and run them for one or more years using an hourly time step. Time dependent process simulation is definitely the preferred way to evaluate BMP performance.

(b) We recommend the simpler frequency approach that is used in the HM for systems where the storage effect is relatively unimportant, e.g., infiltration systems. Pack (2004) shows how this method can be used to estimate the effect of highway right of ways that infiltrate runoff from non curb and gutter highway runoff. Unlike the HM method that assumes that a fixed **storage volume** is available for each storm, a **constant infiltration rate** is assumed. Infiltration rates can be measured locally using infiltrometer tests. The frequency database is the actual hourly data and no separate storm event definition is needed. It is quite simple to determine the pdf for the original data set. Then, all runoff hours that are less than or equal to this rate are infiltrated. Also, a prorated portion of the higher inflows will be infiltrated. The balance that is overland flow will receive some treatment as it moves across the right of way as overland flow. We have excellent calibration data for these systems for highways based on the extensive CALTRANS research program (Barrett 2004). Volume and concentration control are evaluated separately so that their individual effects can be evaluated.

(c) For systems with significant storage effects, it is essential to route these flows through the storage-release system and to depict the mixing regime and the kinetics. The HM assumes complete mixing which provides poorer treatment performance than plug flow that is more appropriate for control systems with shorter residence times. For storage systems, a release rate must be assumed. Indeed, various combinations of storage volumes and release rates should be evaluated to find the least cost combination of storage and release rate. The measure of performance should be % control of annual pollutant load, not annual runoff volume (Lee, Heaney, and Lai 2005). Typical ranges in residence times for efficient controls are of the order of 1 to 5 days. By sharp contrast HM describes detention times of up to one year. Typical ranges of detention times for wastewater and stormwater controls are shown in Table 17.1. None of these residence times approach the HM values. The HM residence times are more representative of the behavior of natural lakes.

Table 17.1. Typical ranges of residence times for wet-weather controls

Control	Times, hours
High-rate wet-weather treatment	.25 to 1
Infiltration systems	0.1 to 48
Detention Basins	2 to 120
Wastewater Treatment Plants	10 to 12
Wastewater wetlands	50 to 500

Jonathan E. Jones
Please see response to No. 7.

Larry A. Roesner
No, not that are simple to apply and interpret as required in the Objectives

Ben R. Urbonas

See my "Peer Review" dated March 6, 2005.

William W. Walker
No.

18. Could parts of the Method be clarified to increase consistency among users?

James P. Heaney
Yes. The frequency calculations and extensive lookup tables that link DCIA and non-DCIA areas can be greatly simplified as follows:

(a) Calculate the runoff from the DCIA and non-DCIA areas separately and add them at the end. The current method of using lookup tables is clumsy and the combined use of DCIA and CNs can be expressed more directly as two separate calculations.

Jonathan E. Jones
Yes, there are parts of the method that could be clarified to increase consistency among users, and these are discussed below. However, in general, the Report does a good job of clearly explaining the Harper Method, and we believe that most stormwater BMP designers would be able to utilize it with consistency.

Larry A. Roesner
I think The Method is pretty straight forward. I assume that The Method has be coded into a user friendly spreadsheet with user instructions. If so, what can be simpler or more consistent for users?

Ben R. Urbonas
I do not believe so, but could be proved wrong.

William W. Walker
You might consider converting it all to a spreadsheet to facilitate use.

19. In your opinion, what is the weakest aspect of the Harper Method, and how would you correct this?

James P. Heaney

The performance equations appear to be unrealistic and use % removal as the ordinate instead of actual concentrations. Their origin is obscure. Fitting a single equation to residence times approaching one year doesn't appear to be realistic.

I would first find out the nature of the data that are the basis of these equations. A significant part of the unusually long detention times is the use of very deep detention basins that would be expected to be below the ground water table. These deep excavations do provide a source of fill that may be a significant economic benefit in South Florida.

Jonathan E. Jones

We have not specifically defined the single weakest aspect of the Harper Method. Areas of significant concern include: exclusive reliance on percent removal; questions regarding precipitation analysis (described below); handling of wetlands and open waters/lakes; and the endorsement of only dry retention and wet detention as acceptable BMPs. We also are concerned about the assumption of 100% pollutant removal and the regression methods shown in Figures 3-7.

Larry A. Roesner

See question 16 above.

Ben R. Urbonas

Runoff volume estimating procedures based on the NRCS methodology, the method chosen to analyze and systematize the continuous rainfall records, and the use of "percent removals" instead of average effluent concentrations. See my "Peer Review" dated March 6, 2005.

William W. Walker

Lack of support for wet detention pond design criteria, which are probably optimistic.

20. Identify any changes that you would make to the Method so that it more accurately assesses pre-project and post-project loads for southwest Florida.

James P. Heaney

Pre and post-project loads

(a) DCIA-Published concentrations for DCIA's should be relatively accurate since they represent the result of generic urban activities. DCIA's can be expected to follow the buildup-washoff process with a finite amount of available material on the impervious surface. Accordingly, a plot of concentration as a function of storm flow would tend to show decreasing concentrations. Thus, the existing concentration database should be compared with estimates from other areas to estimate concentrations. The associated runoff volumes for DCIA can also be estimated fairly well because it is a simple rainfall-runoff relationship (Lee and Heaney 2002). The only real debate is over the initial abstraction at the beginning of a storm.

(b) Non-DCIA-Concentrations from non-DCIA areas are much more difficult to estimate since they depend heavily on the mix of other impervious area and pervious area. Also, the availability of pollutants from pervious areas is more complex since the sources are not well defined. Pervious areas would tend to behave like natural watersheds with concentrations increasing as flow increases. These two areas could be weighted to estimate a composite concentration. Spatial scale is very important for non-DCIA areas since overland flow is complex in this situation.

Jonathan E. Jones

Suggested changes to the Harper Method are provided in the following section. We do think it very important to emphasize that the Harper Method should be subject to periodic updates in response to new data, new methods, user experience, etc. The authors are clearly interested in comprehensive literature reviews and data acquisition, and we suspect that they would agree with viewing this Report as a "living document".

Larry A. Roesner

See question 16 above.

Ben R. Urbonas

This is beyond the scope of this peer review. You are asking a protocols design question.

William W. Walker

See recommendations regarding model for P removal in wet detention ponds (Figures 2-3).

21. Does the Harper Method lend itself to refinement as more data become available? Which aspects are in need of refinement the most?

James P. Heaney

HM would benefit by having a much firmer conceptual foundation upon which judgments could be made about the benefits of more data and improved formulations. Ideally, a few long-term monitoring sites would be used to provide a benchmark for evaluating how these systems actually behave. Dr. Betty Rushton of SWFWMD already has considerable data. Process simulators such as SWMM or our SS SWMM or SS STORM could then be set up to model the behavior of these sites using the local data for calibration. Given the calibrated simulation model, the BMP design could be optimized and provide valuable information about how these systems actually behave. This detailed process level information can then provide a sound basis for judging the merits of any simplified method like HM or single design storm approaches. The simpler methods could also be calibrated using the more detailed data sets.

Jonathan E. Jones

Please see response to No. 20.

Larry A. Roesner

Nearly any algorithm lends itself to refinement as more data become available, and the Harper Method is no exception. For those aspects in most need of refinement, see question 16 above.

Ben R. Urbonas

Yes, if the suggestions in my "Peer Review" dated March 6, 2005 are followed.

William W. Walker

Yes. Justification for wet detention performance curves. Additional runoff concentration data for various land uses.

22. Are you aware of an alternative approach to the Harper Method that utilizes sound engineering and science that could be used in southwest Florida to determine stormwater treatment design requirements so that post-development loadings do not exceed pre-development loadings?

James P. Heaney
A variety of approaches that we have developed during the past several years have been described in this review. These methods could significantly improve the evaluation methodology.

Jonathan E. Jones
There are many alternative approaches to the Harper Method that could be used in southwest Florida to determine design requirements. However, it is far beyond the scope of our review to describe these alternative methods. Furthermore, our "bottom line" view on the Harper Method is that it would be an acceptable regulatory tool, provided that the questions and concerns that we have raised herein (along with those from other peer reviewers) are addressed by the authors.

Larry A. Roesner
Obviously there "are alternative approaches to the Harper Method…." These include:

- The Florida Watershed Management Model, which is similar, but not tailored specifically to Southwest Florida.
- The model MUSIC developed by Wong (2002) which is more detailed than The Method, and more difficult to apply.
- HSPF or SWMM which are very sophisticated models, and not easy to apply.

So at this time, and subject to refinements recommended under question 16, I do not think there is an alternative approach that: 1) "…is relatively simple to employ and straightforward.", but at the same time 2) "…accurately estimate(s) pre-project and post-project loadings in southwest Florida and it should be sound from an engineering and scientific standpoint." and 3) "…minimize(s) the amount of user interpretation required."

Ben R. Urbonas
Yes, see the suggestion in "Peer Review" dated March 6, 2005 are followed. Beyond that the question exceeds the scope of the peer review assignment and requires specific design of methodology.

William W. Walker
Yes. Schueler's "Simple Method." P8 Model. See General Comments.

V. SPECIFIC COMMENTS

INTRODUCTION

William W. Walker
Page 1-1. Bottom Paragraph. Mass Units
While the Kg units are fine because you have to pick something, but this is the first I've heard that pounds are not "scientifically defensible."

SECTION 2

Section 2.1

James P. Heaney

<u>Precipitation</u>

HM uses hourly rainfall data that are reported in intervals of 0.01 inches. This information is taken from the NCDC database. HM uses only part of the Ft. Myers hourly data for 1960 to 1993. They exclude 12 years from these 24 years but don't explain why. Hourly data are now available through 7/2004. Ft. Myers, FL is a good data set since it presents rain depths measured in 0.01 inch increments. Many of the hourly data sets only report data in 0.10 inch increments leading to significant errors in the analysis. It is very easy to get this data for any NCDC station (http://ols.nndc.noaa.gov/plolstore/plsql/olstore.prodspecific?prodnum=C00313-TAP-A0001) by downloading it from the NCDC web site onto a spreadsheet. If the one hour data are used directly, then it is simple to sort them and develop the cumulative distribution function (CDF). The initial abstraction can be deducted before doing the frequency plot. This CDF can be used as a rate of runoff or volume of runoff if a storm event is defined to last one hour. I suggest doing a careful analysis of NCDC stations in the region of interest, and generating the precipitation statistics for several stations to better define the spatial variability of the precipitation. Also, using a longer period of record would be helpful.

The HM follows the more typical approach of defining a storm event. The HM is based on a single storm event definition that states that a storm ends if it hasn't rained for three consecutive hours. This assumed value is referred to as the minimum interevent time (MIT). The storm event statistics are very sensitive to the selected MIT. Bases for selecting MIT include:

- Statistical convenience (Adams and Papa 2000)

- Knee of the curve wherein further increases in the MIT reduce the number of events per year at a much lower rate (Heaney et al. 1977)

- The expected travel time through the catchment and the control(s) (Heaney and Lee 2005). The travel time through the catchment can be approximated by a variety of time of concentration formulas. The residence time in a BMP can be estimated based on its capacity and expected flow through rates. This last definition is the only one that recognizes the dependency of the event definition on spatial scale. Accordingly, the MIT should depend on the size of the catchment. At the shorter end of the MIT is the fact that the parent precipitation data are hourly.

- The range of MITs that have been used by investigators typically range from 3 to 12 hours. The results of the analysis are fairly sensitive to the selected MIT especially for smaller MITs such as 3 hours.

- The HM partitions the precipitation data into only 19 bins necessitating significant interpolation. For example, according to Table 1 in the HM report, 45.2% of the annual events are 0.10 inches or less. This category is represented as a single bin. The data are reported in 0.01 inch increments. The most accurate way to represent them is to generate the probability density function (pdf) using bins for each 0.01 inch increment. This calculation is simple to do using a spreadsheet, e.g., Excel's Tools/Data Analysis/Histograms, once the hourly data have been divided into events. Heaney and

Lee (2005) present software that automatically partitions the hourly data into event data for a given MIT. If an MIT of 0 hours is used, then the data can simply be sorted without needing any special software.

Overall, suggested improvements in characterizing rainfall are as follows:

- Use data from the nearest NCDC gage and generate the CDF for each gage. The HM might be extended to generate these CDFs using the above methods.

- Include a procedure for calculating the appropriate MIT based on the nature of the study area with shorter MITs for smaller catchments.

- Use smaller bin increments to more accurately define the CDF.

- Relate the HM to available literature on this subject.

- Generate the CDF using the one hour data so that flow rates as well as volumes can be evaluated.

<u>Runoff</u>
DCIA Runoff

HM calculates annual runoff as the sum of the DCIA and non-DCIA runoff. DCIA runoff is simply rainfall – 0.10 inches for each storm event. The HM report, Table 1, cites a long term rainfall average of 53.15 inches per year. The selected MIT affects the number of events per year. For example, if there are 200 events per year using a given MIT, then the initial abstraction accounts for 200 events/year*0.10 in./event or 20 inches per year of initial abstraction, nearly 40% of the total precipitation. This calculation assumes that the entire 0.10 inch of depression storage is available at the beginning of each event. For shorter MIT's like 3 hours, this may not be the case. Conclusions on initial abstraction are as follows:

- The use of 0.10 inches per event for initial abstraction is a common value.

- The use of a 3 hour MIT is not a typical assumption. The MIT definition strongly affects the number of events per year. The appropriate definition of MIT should depend on the travel time through the catchment and expected residence time in the BMP.

The HM is to be commended for evaluating the effect of DCIA separately. Studies by Lee and Heaney (2002, 2003) clearly demonstrate the importance of evaluating the effects of DCIA separately since it has a disproportionally large impact on total annual runoff. I strongly disagree with the second part of their definition of DCIA (P. 2-5).

"It is also considered directly connected if runoff from it occurs as concentrated shallow flow that runs over a pervious area, such as a roadside swale, and then into a drainage system."
Our highway runoff studies show the high rate of infiltration and pollutant removal occurs in swales (Pack 2004, Pack, Heaney, and Lee 2005). They do not behave as DCIA. DCIA should be defined as the runoff that flows directly to the storm drainage system over directly connected impervious areas.

Non DCIA Runoff

The non-DCIA portion of runoff is calculated by finding a weighted average curve number (CN) where CN is a weighted average of the CNs of the pervious area (PA) and other impervious area (OIA). This method has been used by other investigators. The end result of this calculation is an estimate of the average available storage per event, S. This method assumes that all of this storage is available at the beginning of each event. This may not be the case for several reasons including:

- The soil moisture storage is still draining from the previous event. Using a 3 hour MIT, it is highly likely that this condition may not be satisfied for many events.
- Soil moisture storage is affected by antecedent irrigation as well as antecedent precipitation. Irrigation is a significant component of the annual water budget in warmer climates like southeastern Florida. A general rule of thumb in irrigation scheduling is to fill the soil moisture storage if it becomes half empty (0.5*S). Irrigation occurs every 1 to 3 days.

Urban soil compaction can cause a significant reduction in soil moisture storage. Thus, it is important to conduct local infiltrometer tests to better estimate the expected actual rate of infiltration (Pack 2004, Sample 2003).

To overcome difficulties in measuring soil moisture *storage volumes*, we have opted to use *infiltration rates* and hourly data to evaluate infiltration based systems (Pack 2004, Pack et al. 2005). If a significant storage effect exists, then continuous simulation of the dynamics of the storage effect is needed to avoid having to arbitrarily assume an MIT and a single value of storage that is always assumed to be available at the beginning of each event (Rapp 2004, Rapp et al. 2005).

The HM method for calculating DCIA and non DCIA runoff requires the determination of a single runoff coefficient as presented in Table 4 for various combinations of DCIA % and non DCIA CNs. I was able to verify these calculations for a few assumed combinations of DCIA and Non-DCIA by separating the calculations into DCIA runoff and non-DCIA runoff. If DCIA = 100%, then according to the last row of Table 4, the annual runoff coefficient is 0.782. Our evaluation of the USGS data for a shopping center in southeast Florida (Lee and Heaney 2002) with a DCIA of 98% indicates a runoff coefficient of about 0.95. The much lower coefficient of 0.782 is probably due to the assumed impact of initial abstractions for the relatively large number of events per year associated with an MIT of 3 hours. This evidence suggests that either the assumed initial abstraction and/or the number of events per year are incorrect.

Given the cdf histogram of annual runoff, it is simple to calculate the non-DCIA runoff using a simple spreadsheet calculation. A given storage, S, inches/event, will capture all of the runoff up to a volume of S, and a prorated portion, of the volume for storms larger than S. For example, if S = 2.0 inches, then the annual runoff from all storms <= 2.0 inches is captured, and a proportionate (S/2) amount of the larger runoff events are captured. The calculated % control can be determined by a simple SS calculation for S = 2 inches. This result can be determined for any specified value of S by repeating this calculation. This can be done easily using the one-way data table feature in Excel.

Suggestions for determining annual runoff are as follows:

- Separate the DCIA and non-DCIA calculations.

- Express the non-DCIA calculations as a function of assumed storage, inches/event, and develop a performance function that expresses runoff coefficient as a function of assumed storage.

- Calibrate the basic assumptions against measured values in the literature that provide more accurate estimates, e.g., the USGS shopping center data in southeast Florida analyzed by Lee and Heaney (2002, 2003).

- Put the calculations onto a SS template for users that replaces the Table 4 calculation.

Section 2.1
Ben R. Urbonas

1. The use of a 33-year rainfall record of hourly precipitation depths to develop total storm depth characteristic and statistics is to be applauded. The use of continuous rainfall records for the development of the types of methodologies contained in the report is essential to paint a realistic annual runoff and loading picture. The analysis of the data identified the differences in mean storm durations, depths, and antecedent dry periods between the dry and wet seasons.

2. The selection of 3-hours of no rain as the basis for defining the beginning of a new storm event has to be questioned. Driscoll, et. al., in their report to EPA in the 1980s recommended 6-hour separation to define new storms on the basis of statistical analysis of rainfall record throughout the United States. His suggestion may not be valid here where the Harper Method bases load calculations on dry retention basins that empty in 40 hours. The use of longer time-of-separation to define a new storm would reveal different mean storm depths, storm durations and antecedent dry periods. The 3-hour time of separation is also inconsistent with the suggested 40-hour emptying time for dry retention ponds, for which a 24-hour emptying time would be more representative of filling and emptying statistics for these retention ponds. At least a 24-hour separation time would assure that for the majority of storms the full retention volume is available and the volume occupied by a prior storm has been vacated. Three hours hardly gives the wet ground time to recover from a previous rainfall event and, in addition, can bias the statistics by showing the full capture of may small storms.

3. One other concern is the use of very small events in calculating seasonal average/mean depths (e.g., Dricoll, et. al. filtered out storm events that had less than 0.1 inches in depth from their statistical analysis). They have large numbers of these runoff events that do not produce surface runoff and tent to only wet the surface and evapotranspire, thus depressing mean storm depth that can result in smaller treatment facilities.

4. The use of the National Resource Conservation Service (NRCS) method in calculation runoff form urban areas can be problematic for urbanized areas. This becomes event more of a concern when the DCIA and non-DCIA are part of the calculation. The use of composite CNs for a catchments that have pervious and impervious surfaces typically depresses the numbers of runoff events and the amount of runoff volumes that occurs. By using composite CN numbers, the incipient runoff that occurs from DCIA surfaces does not appear until significant amount of precipitation has occurred, while in the real world, these surfaces begin to show some runoff after 0.06 to 0.10 inches.

5. The use of other distributed rainfall runoff models, especially if one has calibration data, can produce results that may be more defensible. For example, if HSPF or the EPA SWMM version 4.4 would have been employed in a continuous simulation mode, more defensible <u>seasonal</u> average runoff coefficients would have resulted. Those coefficients could then be used for estimating average seasonal and then average annual runoff volumes and pollutant loads. The use of only annual average runoff coefficient for the post-development period can produce a lesser total annual runoff volumes and loads than using seasonal values.

6. I had a hard time wrapping my mind around how to use Table 4 in a way that I felt comfortable. First I need to know or assume pre-development CNs in the catchments. Then I need to calculate, or assume, the composite post-development CNs. Am I to assume that the same soil and vegetation characteristic apply for pre- and post-development conditions, event though the soils have been reworked extensively? Or am I supposed to account for changes in soil composition in making my new CN assumptions? Then, given those assumptions, I am supposed to calculate the non-DCIA CN.

Nevertheless, the 100% DCIA with 98% non-DCIA runoff coefficient seems to be reasonable. One question that comes to mind is whether the local experience indicates that the average annual runoff volume in SW Florida for 0% DCIA is only 0.1% of the annual precipitation as the table implies? Some of the runoff data in this report seems to argue against that.

Section 2.1
Page 2-1, Calculation of runoff Volumes
William W. Walker
The definition of rain events is critical to predicting runoff from pervious areas using the SCS method and for evaluating the performance of infiltration devices. The analysis assumes that each rain event is independent and that infiltration devices drain completely between events. It seems as though a 3-hour inter-event period is very short relative to the recommended 40 hour drawdown period. The independent-event assumption may not be valid for evaluating dry detention ponds or other infiltration devices. The effect of this will be to separate some very large storms into smaller ones. As it is used here, the SCS method will also under-predict runoff from pervious areas if the inter-event time is too short. This, in turn, may lead to under-estimation of runoff coefficients and under-design of BMP's. Sensitivity of the computed runoff coefficients (Table 4) and infiltration device designs to the 3-hour inter-event assumption should be evaluated.

Section 2-1, General Comment
Jonathan E. Jones
It is not clear from the text whether a normal or log-normal statistical analysis was performed.

Section 2.1
Page 2-1, Paragraph 2
Jonathan E. Jones

This paragraph states that based on a review of the "available meteorological records, Ft. Meyers is the only major city in southwest Florida which has sufficient long-term meteorological data for estimate of rainfall trends." We believe that this statement requires further justification. As a part of our research and creating the International BMP Database with other co- principal investigators including Eric Strecker, P.E. and Ben R. Urbonas, P.E., we obtained information on the data sets used by Driscoll et al (1989). The Driscoll data included a rain gauge in Miami (WSCMO AP, Station ID 5663) with a period of record running from 1949 to 1987. Why was this data suitable for use by Driscoll et al, but not by Harper? Should this data be examined and integrated into the Report?

Section 2.1
Page 2-1, Paragraph 3
Jonathan E. Jones

The Harper Method specifies a storm separation criterion of 3 hours for defining an individual "storm event". What is the basis of the 3-hour event separation criterion? Driscoll et al (1989) used a 6-hour storm separation time. We recommend that the authors provide better justification for the use of the 3-hour storm separation time in the Report. The effects of using a 3-hour versus a 6-hour storm separation criteria can be significant (refer to Page 2-2, Paragraph 2):

- <u>Average Annual Number of Events</u>: The analysis by Driscoll et al of the Miami gauge using a 6-hour storm separation criterion resulted in an average of 78 events per year. The 3-hour storm separation criterion used in the Harper Method results in 115 events per year on average.
- <u>Storm Duration</u>: The Driscoll analysis with the 6-hour storm separation criterion resulted in an average storm duration of 6 hours. The analysis in the Harper Method using the 3-hour storm separation criterion provides a mean value of 2.32 hours.

Page 2-2, Paragraph 2, Sentence 2
Jonathan E. Jones

Describes the number of rainfall events falling into different depth categories, is also significantly influenced by the storm separation criterion that is used. In general, a shorter storm separation criterion will result in a greater number of smaller events.

We note that the average annual precipitation from the Miami gauge used by Driscoll and the Ft. Meyers gauge used in the Harper Method are quite similar. We raise this issue not because we necessarily believe a 6-hour storm separation criterion is more appropriate than a 3-hour storm separation criterion. We raise this issue to highlight the importance of the storm separation criterion on characterization of storm events. Whether a 3-hour, 6-hour, or other storm separation criterion is used in the analysis, we believe that it is important to justify the choice and explain this in the Report.

Section 2.1
Pages 2-3 and 2-4, Tables 1, 2, and 3
Jonathan E. Jones

Why are the years of 1980 – 1984 and 1986 – 1992 excluded from the analysis? This exclusion results in a precipitation record of 22 years for analysis. We are somewhat surprised that other long-term precipitation data are not available for southwest Florida. Would it be possible to use data from another site to try to fill in some of the gaps in the Ft. Meyers gauge record?

Section 2.1
Page 2-4, Table 2
Jonathan E. Jones
We note in this table that the standard deviation reported for annual rainfall is 8.68 inches. This highlights the importance of previous comments on reliance on averages for sizing of stormwater facilities. For year with total annual rainfall, one standard deviation above the mean value (in general, not considered an extreme or rare occurrence, especially for precipitation) would have total precipitation roughly 15% greater than that of an average year. For water quality facilities sized based on annual averages, this would be expected to result in significant untreated flows. The untreated flows in wetter than average years would not necessarily be "offset" by decreased demands on facilities or better performance of facilities in drier than average years.

Section 2.1
Page 2-5, Equation for nDCIA Curve Number (CN)
Jonathan E. Jones
The equation on Page 2-5 for nDCIA CN is a linear interpolation between (100 – Imp) and (Imp – DCIA). In fact, CNs are non-linear functions. Errors can occur when applying linear combination techniques to these non-linear functions. The greatest potential for errors would occur when the values for (100 – Imp) and (Imp – DCIA) are similar. A better approach might be to apply the equations that follow this first equation to the (100 – Imp) and (Imp – DCIA) terms separately to determine excess precipitation and then to combine.

Section 2.2
Pollutant Concentrations
James P. Heaney
Table 7 in the HM report presents the results of the estimated concentrations for total N and P, BOD, TSS, copper, lead and zinc based on review of the literature, primarily in Florida. This information is presented for 13 land uses, wetlands, and open water/lake categories. The wetlands concentrations in Table 7 are based on the wetlands summary data presented in Table 5 of the HM report. It is surprising to see wetlands and open water/lakes listed as "land uses". They are typically viewed as control areas, not source areas.

Pollutant concentrations for a given land use depend heavily on DCIA and the size of the catchment. Measurements taken at the bottom of a catchment are fairly accurate for DCIA since no significant pollutant removal occurs during overland flow on the impervious surfaces. However, runoff across pervious areas is a much more complex process that includes the influence of infiltration and uptake by vegetation. For example, recent results for runoff from highways in California indicate that most of the pavement runoff onto the adjacent right of way infiltrates within a few feet (Barrett 2004). Thus, measurements taken at a downstream outlet reflect a much lower flow and concentration due to the effect of treatment along the way. Our ongoing studies of low impact development illustrate the important influence of upstream control on the flow and concentration that reaches the outlet. This is especially true for larger catchments. Thus, spatial scale needs to be incorporated explicitly. The HM database for single family residential ranges from 30 to 897 acres. Thus, a significant in-system effect occurs at these spatial scales. Ideally, the monitoring is done at a small spatial scale that represents the pollutant buildup and washoff process only. How much of it actually reaches a downstream outlet depends on a delivery ratio.

The effect of spatial scale has traditionally been estimated using a delivery ratio coefficient, e.g., the Universal Soil Loss Equation. The delivery ratio is hard to estimate and is often used as calibration parameter in a process simulator. Alternatively, one can adjust the assumed concentration at the source. Recent research by my group on source control for wet-weather systems indicates the vital importance of accurately estimating the local benefits of pervious areas that store and/or infiltrate much of the annual pollutant load from these areas.

The HM database for single family residential areas ranges in size from 25.1 to 897 acres (Table A.1). At 900 acres and a length:width ratio of 3:1, the length of flow in the catchment is on the order of almost 2 miles. Thus, it is unrealistic to assume that pollutant concentration data collected for small spatial scales can be upscaled to be accurate for much larger areas, especially for non-DCIA.

Pitt et al. (2004) have summarized stormwater concentration data based on a national review of NPDES permit data. A comparison to the values listed in HM is shown below. In general, the assumed values are similar.

Comparision of mean (HM) and median (Pitt et al. 2004) pollutant concentrations

Land Use	**Source**	**TSS (mg/l)**	**BOD5 (mg/l)**	**Phosphorous Total (mg/l)**	**Copper Total (ug/l)**	**Lead Total (ug/l)**	**Zinc Total (ug/l)**
Residential	Pitt	48.0	9.0	0.300	12.0	12.0	72.0
Residential	HM	26.0	7.4	0.335	31.0	39.0	73.0
Commercial	Pitt	43.0	11.9	0.220	17.0	18.0	150.0
Commercial	HM	84.0	12.3	0.300	23.0	17.5	140.0
Industrial	Pitt	76.4	9.0	0.260	22.0	25.0	210.0
Industrial	HM	93.9	9.6	0.310	No Data	202.0	122.0
Freeways	Pitt	99.0	8.0	0.250	34.7	25.0	200.0
Highways	HM	49.1	6.7	0.270	40.0	211.0	167.0

I am perplexed as to how the data in Table 5 (wetlands) and Table 6 (lakes/open water) can be used to represent "land use" data. I presume that these data represent samples taken within these systems and are not outflow data. Wetlands and lakes/open water are normally considered to be part of the subset of wet-weather *controls* that can have a significant positive impact on water quality. Residence times in these systems range from a few weeks to many months. For wetlands, Kadlec and Knight (1996) present a thorough overview of the expected performance of wetlands for water quality control. Extensive data on wetlands are available from numerous studies associated with the Everglades Restoration. Similar evaluations have been done for lakes and reservoirs for many years. From a process engineering point of view, they may be viewed as relatively large reactors that have a significant influence on flow patterns through a storage effect and pollutant concentrations through physical, chemical, and biological processes. They can be analyzed as storage-release BMPs using a variety of methods.

Section 2.2
Ben R. Urbonas

1. This section provides a good summary of the constituent concentrations data (averages) used as the basis for annual load calculation in this report.

2. The use of average concentrations arrived at using field data lends credibility to the Harper Method. All surface runoff water quality data are very noisy and do not correlate well with the size of the runoff event and the use of averages is as accurate as it gets at the current state-of-practice.

Section 2.2
Page 2-8, Table 4
Jonathan E. Jones
Are the values in Table 4 consistent with field data (data from gaged watersheds with known DCIA characteristics)? We note that for a CN of 98 and 100% DCIA, which would equate to something like a paved parking lot or rooftop that is entirely impervious, the annual runoff factor is 0.782. In other words, on an annual basis, 78% of the precipitation becomes runoff and 22% is lost, presumably through evaporation. Is this reasonable and consistent with local experience or should annual runoff factors for largely impervious surfaces be higher?

Section 2.2
Page 2-9, Paragraph 3, and Page 2-10, Paragraph 1
Jonathan E. Jones
The text states that wetland monitoring data are available from 19 separate stations, representing a variety of wetlands with various degrees of impact. However, the text goes on to indicate that these 19 wetlands will be used as "estimates of pre-development wetland characteristics for loading evaluation purposes." Based on our current (limited) understanding, it would not appear appropriate to use impacted wetlands as the basis for pre-development wetland characteristics. In addition, we do not believe that the data in Table 5 are consistent with the authors' statement that, "In general, measured concentrations for the evaluated parameters appear to be relatively consistent between the measured wetland systems." To the contrary, we observe a wide range in the data for Table 5; for example, total phosphorus concentrations in the wetlands range from 0.01 milligrams per liter (mg/L) to 0.025 mg/L, and total zinc values range from 0.00 micrograms per liter (µg/L) to 29.37 µg/L, to provide only two examples.

Section 2.2
Page 2-10, Paragraph 2
Jonathan E. Jones
It is not clear to us that Table 6 provides sufficient data to enable a correlation between land use and lake water quality. We strongly agree with the authors' statement, "If available, site-specific water quality data for water bodies in a particular project area should be used." The following statement in the Report is not clear to us:

> *In the absence of site-specific data, the mean values summarized at the bottom of Table 6 can be used as estimates of ambient characteristics of open water/lakes to be used in generation of pollutant loadings for this particular land use category.*

What "particular land use category" are the authors referring to? Are they suggesting that the mean values are appropriate for open water/lakes in areas that have been unimpacted? If so, this would not appear to be an appropriate assumption because some of the open water/lakes in Lee and Collier Counties have apparently been impacted. We also have concern about utilizing the mean values for total nitrogen and total phosphorus (particularly total phosphorus) given the spread in the data and the fact that selecting the mean value would result in half of the samples having higher concentrations. There are too few biochemical oxygen demand (BOD) and total suspended solids (TSS) data in Table 6 to calculate meaningful mean values.

Section 2.2
Page 2-11, Table 5
Jonathan E. Jones
In addition to the questions raised earlier regarding Table 5, it would helpful if the authors could provide additional information regarding the form (speciation) of the metals, as well as the speciation of the nutrients.

Section 2.2
Page 2-13, Table 7
Jonathan E. Jones
Are the data in Table 7 generally consistent with data from other studies such as the National Urban Runoff Program, monitoring data from the stormwater National Pollutant Discharge Elimination System (NPDES) Permitting Program (Phase I and II cities), the University of Alabama Runoff Quality Database, etc.? We presume that the authors are citing various sources for the values in Table 7. It would be helpful if these citations could be indicated. We question the zero percent impervious values for agricultural land. The percent impervious value for wetland is shown to be zero, yet the authors use an annual runoff coefficient for isolated wetlands of 0.225. Are these parameters consistent?

Section 2.3
Pre- and Post-Development Loadings
James P. Heaney
Given an estimated annual runoff volume and average concentration, it is simple to calculate the annual load as shown in the equation in Section 2.3 of the HM report. Other than a typo. in how the summation is identified in this equation, this calculation is straightforward.

What is not straightforward and very atypical is the assumed performance requirement that no net increase in pollutant load is allowed following development as expressed in the equation:

Required removal efficiency (%) = {[(post development load – predevelopment load)]/(post development load)}*100

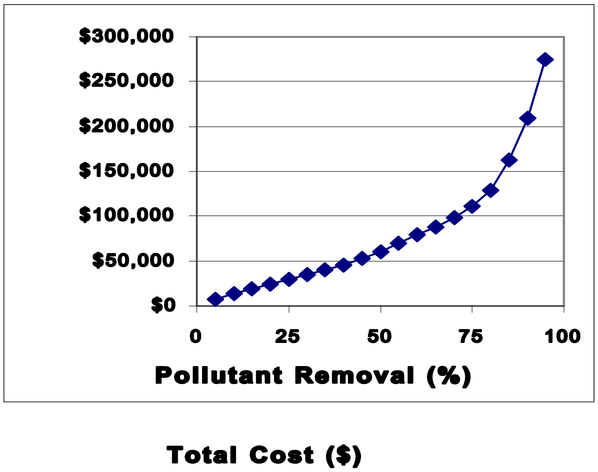
This performance measure may be very difficult, if not impossible, to meet for a variety of reasons:

- If the predevelopment condition is a pristine natural condition, then the concentrations are already at their natural background level. Thus, any new development will increase these loads.

- This equation is assumed to be applied across the seven pollutants listed in Table 7. hus, the condition must be satisfied for all of them.

- We don't have BMPs that can remove pollutants down to a background level except at an enormous cost. Numerous studies dating to the late 1970's, e.g., Heaney et al. (1979) have shown that the incremental cost of wet-weather control increase very rapidly beyond control of 80-85% of the pollution. In order to control the remainder of the pollution it is necessary to include very large storage facilities that can capture and treat large storm events that only occur rarely. A recent example of a cost-effectiveness curve is taken from Rapp et al. (2004) based on an optimized detention system design is shown in Figure 1. Marginal costs increase rapidly beyond a control level of about 75%.

Figure 1. Cost-effectiveness curve for a highway detention system (Rapp et al. 2004).



Given these conditions, the typical way to express wet-weather performance measures is in terms of a specified % reduction in either total pollutant load or, better, total removable pollutant load that recognizes that a background level exists for each constituent.

Section 2.3
Ben R. Urbonas

1. This section provides a method of calculating the average annual loadings of constituents in stormwater runoff. Its accuracy depends on the accuracy of the runoff volume calculations (see discussion above). If the calculated average annual surface runoff volumes are questionable, so will be the calculated constituent loads.
2. Why is efficiency in terms of percent removal being addressed and is an issue? Is it not the goal to not exceed the pre-development loadings? If that is the case the only question that needs to be answered is whether the post-development load is more, less or the same as the pre-development annual load.

SECTION 3

Section 3
Stormwater Treatment Options
James P. Heaney
The HM report concludes that dry retention and wet detention systems are the only viable options. Our recent research indicates that BMPs can be classified in terms of performance as infiltration rate dominated systems, e.g., swales, and storage-release systems such as detention systems. This could be viewed as being analogous to the HM approach if dry retention is viewed as an infiltration system.

The dry retention system is assumed to empty in no more than 40 hours as attested by a registered geotechnical engineer.

For the wet detention system, the HM method bases performance on residence time in the detention system. They assume complete mixing for these systems as opposed to plug flow that may be more appropriate.

The curves for N and P show residence times of up to 300 days and 15-20 data points. Unfortunately, they never describe the source of the data. Several years of data would be needed in order to get meaningful estimates for residence times greater than 100 days. Perhaps the authors used lake and wetland data for these estimates. We have found very few defensible data sets that allow us to simulate the performance of wet ponds with detention times in the more normal range of 1 to 10 days. The HM curves ignore the important effect of initial concentration on % removal. If the influent is relatively high in initial concentration, then it is easier to remove and the % control would tend to be higher. These curves also ignore the important fact that there is an irreducible concentration for each of these constituents. Thus, it is more meaningful to evaluate control of the removable, not the total, fraction.

The basis for these performance data sets is of vital importance in judging their validity. This removal is a function of both concentration changes and volume changes, not just concentration reductions. For short (less than 5 days) residence times, concentration changes would tend to dominate while for the very long residence times shown in these figures, volume changes could be important.

For TN, TP, and TSS, the HM equations are:

% TN removed = 27.25+8.4216*ln(Res. Time)
%TP removed = 44.583+8.0847*ln(Res. Time)
% TSS removed = 49.362+10.062*ln(Res. Time)

This functional form is not typically used in estimating the kinetics of a treatment reactor. Using this functional form, the % removal goes negative as residence time approaches 0, e.g., TN % removal is negative if detention time is less than 0.039 days. The % removal exceeds 100% for larger values of detention time, e.g., % removal of TSS exceeds 100 if detention time is greater than about 153.3 days. A better functional form would be to use first order kinetics with a minimum achievable concentration, C_{min}. This equation is used for evaluating the performance of wetlands (Kadlec and Knight 1996). Rapp, Heaney, and Lee (2004) have also found that this equation fits stormwater detention performance well with only two parameters, k and C_{min}. The equation is:

$C = C_{min} + (C - C_{min}) * exp(-kt)$

Where C = effluent concentration, mg/l
C_{min} = minimum attainable concentration, mg/l
k = rate constant (1/hr), and
t = residence time, hr.

The HM equations are presented in normalized form of % removal. However, it is not possible to evaluate them without knowing how % removal is determined, i.e., is it:

- % concentration reduction averaged over many storms?
- % load reduction averaged over many storms?
- Other?

The BOD curve is a polynomial with a perfect R^2. However, all that the HM method does is to use the standard first order kinetics with no minimum concentration, or
$y = 1 - C_{in}/C_{out} = 1 - exp(-kt)$
where they assume k = 0.1/day.

This equation is identical to their polynomial. It is the special case where the minimum achievable BOD concentration is zero, an untenable assumption.

The effect of initial concentration on the settleability of TSS is shown dramatically in Table 1 that is taken from Rapp et al. (2004). In test 7, with an initial concentration of 721 mg/l, the concentration after 2 hours is 103 mg/l, an 86% reduction. In sharp contrast, in test 1, the initial concentration of 15 mg/l is reduced only to 14 mg/l in two hours, a 7% reduction. Thus, it is essential to know how the % removal is calculated and how it accounts for the effect of initial concentration and changes in volume due to evaporation and other losses.

Table 1. Setteability of Suspended Solids in Urban Runoff (Randall et al. 1982)

	Total Suspended Solids Concentration, mg/L						
		Sedimentatiom Time, hours					Maximum % Removal
Test No.	**Initial**	**2**	**6**	**12**	**24**	**48**	
1	15	14	14	13	11	2	87
2	35	20	18.5	18	14.5	7	80
3	38	24	16		6		84
4	100	45	34	30	19	7	93
5	155	21	17	12	9	7	95
6	215	67	40	26	17	9	96
7	721	103	34	30	18	18	98

In summary, the basis underlying the pollutant removal relationships used in the HM needs to be carefully reviewed for the following reasons:

- The source(s) of the data are unknown.

- The assumed residence times of up to 300 days are well beyond the normal residence times that are of the order of a few days or weeks. These residence times are contradictory to the design guidelines shown in Table 9 that state that the bleeddown rate should be about 0.5 inch in the first 24 hours.

- How % pollutant control is defined needs to be clearly specified. Is it concentration changes only, or is it a composite of concentration and flow changes?

- The effect of background concentration needs to be included. It is impossible to achieve 100% control due to background concentrations. Attention should be focused on removal of the controllable portion of the load.

Section 3.1
Ben R. Urbonas

1. This section discuses the need to achieve 60 to 95% pollutant "removal efficiencies" and concludes that only two treatment systems can achieve this goal, dry retention and wet detention.

2. The use of pollutant "removal efficiencies" instead of average effluent concentrations from the treatment facilities is suspect when calculating average annual loads. The EPA/ASCE International Database Project published more than one interpretative report recommending the use of "average effluent concentrations" instead of "removal efficiencies" (i.e., percent removals) [ref.: www.BMPDatabase.org, (2000), "Determining Urban Stormwater Best Management Practice (BMP) Removal Efficiencies"; Stecker et. al., (1999 & 2004), two different ASCE Proceedings, other papers and reports]. One of the issues is that "removal efficiencies" are proportional to influent concentrations, which have very wide bands of standard error of estimate. Effluent concentrations data exhibit much tighter bands of standard error of estimate and are more appropriate to use when estimating average annual load calculations. There are other considerations that are discussed in the above-mentioned references that argue for the use of mean effluent concentrations when making constituent load estimates.

Section 3.2
Ben R. Urbonas

1. This section provides a method of calculating the average annual loadings of constituents in stormwater runoff. Its accuracy depends on the accuracy of the runoff volume calculations (see discussion above) and on the validity of the assumptions made.

2. The assumption that the available treatment volume (Assumption 2) is fully recovered is not justified by simple independent variable statistical analysis that was used to develop the Harper Method and the 3-hour storm separation period in analyzing the rainfall data to develop the Harper Method. Long-term rainfall, retention-containment and runoff simulations would have been a more robust method supporting this assumption or suggesting something else.

Section 3.2
Page 3-2, Numbered List of Assumptions, Item Nos. 2, 3, and 4
Jonathan E. Jones
We offer the following comments with regard to these assumptions:

- Assumption No. 2: We indicated our concern with this assumption in the previous section. Based on the criteria in Table 8, it would possible to have a facility designed for a recovery time of nearly 40 hours. Based on the average inter-event time of 1.66 days for the wet season stated in Table 3, this facility would not fully recover for many events (any events with less than mean inter-event dry time). In addition to this observation, we are aware that often there is a trend in southwest Florida for daily convective thunderstorms in the summer months. Averaging techniques, even with differentiation between wet and dry seasons, may not capture this characteristic. We believe this underscores the importance of continuous simulation as a verification method or underlying principle.

- Assumption No. 3: Pollutant loads may be greatly influenced by antecedent conditions, particularly the time for pollutant buildup. A storm with 0.5 inches of runoff that occurred after an extended dry period with considerable time for pollutant buildup would likely have a higher pollutant load than the same amount of runoff when antecedent conditions were wetter. We realize that assumptions such as this one are necessary to create a practical method that can be used by developers, engineers, as well as regulators; however, from a scientific standpoint, this is not technically sound.

- Assumption No. 4: As with Assumption No. 3, this assumption may be necessary to create a method that is practical and useable; however, it is to technically sound. Removal efficiencies for runoff constituents will vary from event to event and, for many BMPs, such as wetlands, there may be seasonal or other trends in addition to the natural variability in performance that make the use of a constant year-round percent removal questionable from a technical and scientific standpoint. This is well established in the literature.

Section 3.2
Page 3-2, Paragraph 2
Jonathan E. Jones
Some general comments regarding the potential treatment options defined in the Harper Method are as follows:

- There is undue emphasis on pollutant removal efficiency percentages. It would be helpful if the authors could either replace their percent removal approach or supplement it with probability-based effluent concentration data.

- There are more BMPs than only wet detention and dry retention facilities that could be utilized.

- Discussion regarding onsite treatment techniques, such as low impact development, minimizing directly connected impervious area, and others would be desirable. It appears that the authors are exclusive advocating an "end of pipe" approach. However, there is a general consensus that stormwater quality management should begin with source controls and progress upwards (in size) from individual lots to larger, regional facilities.

- It would be helpful for the authors to mention that source controls are potentially quite valuable at limiting discharges of nitrogen and phosphorus. This is particularly true for larger areas such as parks and golf courses.

- It would helpful if the authors could emphasize that a multi-layered approach to BMP utilization, featuring a broad array of both non-structural and structural BMPs, is highly desirable. Such an approach emphasizes redundancy and minimizes undue reliance on any one technology. In addition, we suggest that the authors recommend that conservative design assumptions are necessary to overcome the limitation of variable BMP performance.

- The authors may want to note that the Harper Method provides only selected design criteria for dry retention and wet detention facilities, and other references should be consulted for additional criteria. For example, the whole subject of dam/embankment design from the standpoint of stability, safety, etc. is not covered.

- Given that many facilities accessible to the public will be designed in accordance with the Harper Method, we suggest that the authors emphasize that, first and foremost with any design, is the need to protect public health, safety, and welfare, and there are many techniques to accomplish this, such as flattening side slopes, including debris/safety racks on outlet structures, utilizing railings where necessary, etc.

- The authors may wish to provide some design guidance regarding water quality outlets for these facilities, as there has been considerable progress with design details for such structures.

- One of the significant findings of the International BMP Database is that there appear to be tangible water losses associated with BMPs. That is, in general, more stormwater flows into BMPs over time than exits the BMPs, due to various losses. Given that the Harper Method is based on loads rather than concentrations, it would be valuable for the authors to account for these losses.

- We suggest that the authors devote more discussion to the critically important subject of maintenance. For instance, it is essential to maintain dry retention facilities so that they are capable of continually infiltrating stormwater at the design rate. The nature of this maintenance is not discussed. The long-term tendency of infiltration-based measures to plug is not mentioned. The authors might consider advocating forebays with regular (periodic) and unscheduled (i.e., after a tropical storm or hurricane) maintenance for both the wet and dry storage facilities to reduce solids loading.

Section 3.2.1
Ben R. Urbonas
The suggested detail in Figure 1 for “Dry Retention” does not follow recommendations contained in most design guide references, such as the Water Environment Federation’s Manual of Practice for Stormwater Quality Management (WEF MOP #23). Most design guidance recommends a minimum separation of 4- to 5-feet between the bottom of an infiltration basin (which the dry retention basin is) and the seasonal high groundwater table.

The issue here is that in order for the basin to evacuate via infiltration through the basin’s bottom and sides within the specified period of 40 hours, the underlying soils have to have high hydraulic conductivity and a large cross-sectional area and the hydraulic gradient has to be sufficient to drive the groundwater flow at velocities high enough for that to happen. A one-foot separation as shown in Figure 1 raises a question if those conditions can all be met to have these basins fully evacuate in 40 hours during high groundwater seasons, especially during the wet seasons. Again, continuous simulation of this phenomenon at different sites using their specific groundwater and geologic conditions could answer this question. For this reasons alone one can question whether the Harper Method accurately estimates average annual stormwater loading rates leaving the retention treatment systems.

Section 3.2.1
Page 3-3, Dry Detention Systems
William W. Walker

See above comments on potential sensitivity to the assumed storm inter-event time. The first paragraph recommends a 72-hour drawdown time. Table 8 indicates 40 hours.

The 40-hour drawdown requirement is left up to an engineer certification. I guess this is in lieu of direct modeling. The infiltration rate is a critical parameter in this computation. It might be helpful to provide some guidance/ regional information on typical infiltration rates. Conservative assumptions should be required to allow for decline infiltration rates over time associated with filtration of runoff particles. I did not see any mention of maintenance requirements to ensure longevity of dry or wet detention systems.

A removal efficiency of 100% is assumed for retained water. This is equivalent to assuming concentrations of 0 mg/l in all water that infiltrates. A significant fraction of the infiltrating flow may eventually reach drainage canals connected to downstream water bodies. This assumption may be reasonable for suspended solids, but not for nutrients. For example, seepage collected in perimeter canals of Everglades Stormwater Treatment Areas (STA-1W), averages about 0.025 mg/l Total P and 3.5 mg/l Total N (SFWMD data). SW Florida regional values could be estimated from groundwater samples. The seepage phosphorus concentration is much lower than the typical runoff concentrations shown in Table 7 (0.2-0.5 mg/l) , but the seepage nitrogen concentration is greater than all of the runoff TN values (1.1-2.8 mg/l). The assumption of 100% removal is more likely to have an effect on BMP designs controlled by nitrogen.

Section 3.2.1
Page 3-4, Figure 1
Jonathan E. Jones

We believe that it is necessary to have more than one foot of difference in elevation between the dry retention facility bottom and the seasonal high groundwater table elevation. In addition, some projection of the maximum groundwater table elevation would be preferable to the seasonal high elevation, to provide for a more conservative design.

Section 3.2.1
Page 3-4, Paragraph 1
Jonathan E. Jones

The Report notes that stormwater pollutants are trapped in relatively stable associations in the upper 4 inches of soil within retention basins, yet states at the top of Page 3-5 that lateral distances between retention ponds and surface water bodies should be maintained as large as possible, at least 100 feet or more, depending on site conditions. It is not clear to us that these parameters (4 inches versus 100 feet) are consistent.

Section 3.2.1
Page 3-5, Paragraph 3
Jonathan E. Jones
We disagree that 100% removal rates can be assumed for nitrogen, phosphorus, BOD, and TSS. First, soluble fractions of these pollutants will be mobile and will not be entrapped in soils. For example, nitrate, the predominant form of nitrogen in urban stormwater runoff, is highly mobile. Secondly, even though the stormwater in dry retention facilities is infiltrated, it is not as if the water disappears. Instead, this water will eventually resurface downgradient. This is a particularly important consideration since the Harper Method is focused on average annual conditions rather than individual runoff events. Regarding pollutant removal efficiencies, have the authors considered recommending monitoring programs for representative dry retention and wet detention facilities? The method could be periodically updated in response to the collected data.

Section 3.2.1
Page 3-6, Paragraph 3
Jonathan E. Jones
Our concerns with the 1.66-day mean antecedent dry period between rain events during the wet season and the <40 hour recovery time criteria for dry retention ponds was stated on our response to Question No. 6. We believe that continuous simulation would be helpful for better understanding the implications of using a method that relies heavily on mean values for sizing. We do not believe that it would necessary to perform continuous simulation for individual sites (this would not be practical); however, we believe that continuous simulation using a range of land uses and/or imperviousness levels to see how facilities sized using the Harper Method would perform hydrologically over the period of the precipitation record would be useful.

Section 3.2.2
Ben R. Urbonas

1. One concern is the recommendation of ponds that exceed 12 to 15 foot maximum depth and are more likely to stratify crating anoxic conditions that can remobilize the constituents deposited on their bottom. This issue is very complex and one cannot draw generalized conclusions however. Staying with the current state-of-practice recommended in the WEF MOP #23 would be more conservative.

2. The greater concern is the use of "removal efficiencies" to calculate annual loads instead of dry or wet pond effluent concentrations based on data in the region for such facilities (see the discussion above on this issue). Suggest looking at the data scatter on Figures 3 and 4 for the two constituents of greatest concern. Although the $R^2 = 0.68$ shows some level of correlation, this probably could be improved if effluent concentrations were used instead. One has to recognize that high percent removals are associated with high influent concentrations and low ones with cleaner influent.

Section 3.2.2
Page 3-7, Wet Detention Systems
William W. Walker
The methodology generally leads to wet pond designs that are deeper (6-30 ft) and have longer residence times (100-300 days) than conventional design criteria (3-5 ft, 14-30 days). The designs are apparently driven by the residence time performance curves, high P removal requirements, and economic considerations (favoring lower surface area). Deep ponds may be desirable to reduce wind-induced re-suspension, provided they are also aerated to avoid

stratification. Unlike the residence-time based performance curves (Figures 3-5), a conventional particle settling model based upon areal water load would favor shallower ponds to provide a given water residence time. Supporting performance data are needed to justify these unconventional deeper designs.

Section 3.2.2
Pages 3-11 and 3-2, Figures 3 and 4
Jonathan E. Jones

As noted above, we were surprised by the relatively few number of data points for percent removal versus residence time used in these plots. We are surprised that more data are not available for wet ponds in southwest Florida. There is considerable scatter in the data, and the R2 values (0.68 for nitrogen and 0.72 for phosphorus) are modest. Data for residence times of less than 100 days are particularly scattered. We would be interested to see the R2 values with points for residence times in excess of 100 days were removed, and we believe that they would be significantly lower than those shown on the graphs. Information is not provided on these graphs that allows the use to determine the potential influence of influent concentrations on percent removals, nor is information provided that would allow a user to determine the effluent quality that could be expected to be achieved. As stated earlier, we believe that the heavy reliance on percent removal without acknowledgment of the importance of influent and effluent concentrations and loadings is one of the greatest weaknesses of the Harper Method.

Section 3.2.2
Page 3-11, Figure 3, Nitrogen Removal
William W. Walker

The USEPA (1978) collected nitrogen budget data from Florida lakes that could be used in testing the ERD's nitrogen removal equation (Figure 3). A preliminary review of that data suggests that the equation over-predicts N removal in all of the lakes. I agree with CDM's suggested alternatives, including the second-order nitrogen model (Walker, 1985;2004), but have not tested it specifically on Florida lake data. A simple relationship between removal rate and residence time is not expected for nitrogen. Processing in a pond or wetland can reduce concentrations to stable "background" levels on the order of 0.5-1.0 mg/l, generally in the form of dissolved or colloidal organic nitrogen. The second-order or first-order K/C* models (Kadlec & Knight, 1996) would be more appropriate.

Section 3.2.2
Page 3-12, Figure 4,. Phosphorus Removal
William W. Walker

I agree with CDM's Peer Review Panel that the procedure used to estimate performance of wet detention ponds (ERD Fig 4) predicts substantially higher phosphorus removal efficiencies relative to other methods and typical performance data. Figure 1 compares model predictions with nationwide data from detention ponds (Walker, 1987) and Florida lakes data recently compiled to support modeling of SFWMD/CERP regional storage reservoirs (Walker, 2005). The model prediction skirts the upper boundary of the datasets. One limitation of this comparison is that the Florida lakes had mean depths ranging from 3 to 11 feet, considerably less than ERD's 15 to 30 ft design concepts. With the exception of rock pits, lakes in that depth range are probably rare in Florida.

ERD's equation was reportedly based upon data from wet detention ponds in Florida, but there are no details provided. Reasons for the apparent differences between ERD's detention ponds and other Florida lakes are unknown. Differences in depth, size, or mixing regime (plug flow vs.

completely mixed) may be factors. Given no information on ERD's datasets and the substantial differences between model predictions and data from Florida lakes and other systems, ERD's model is not recommended for use in design. Analysis of the ERD dataset would be needed to justify any application. I agree with CDM's recommendation that efficiencies should be computed from cumulative budgets, rather than discrete event data. Facilities with short periods of record, alum treatment, other chemical enhancement should be excluded.

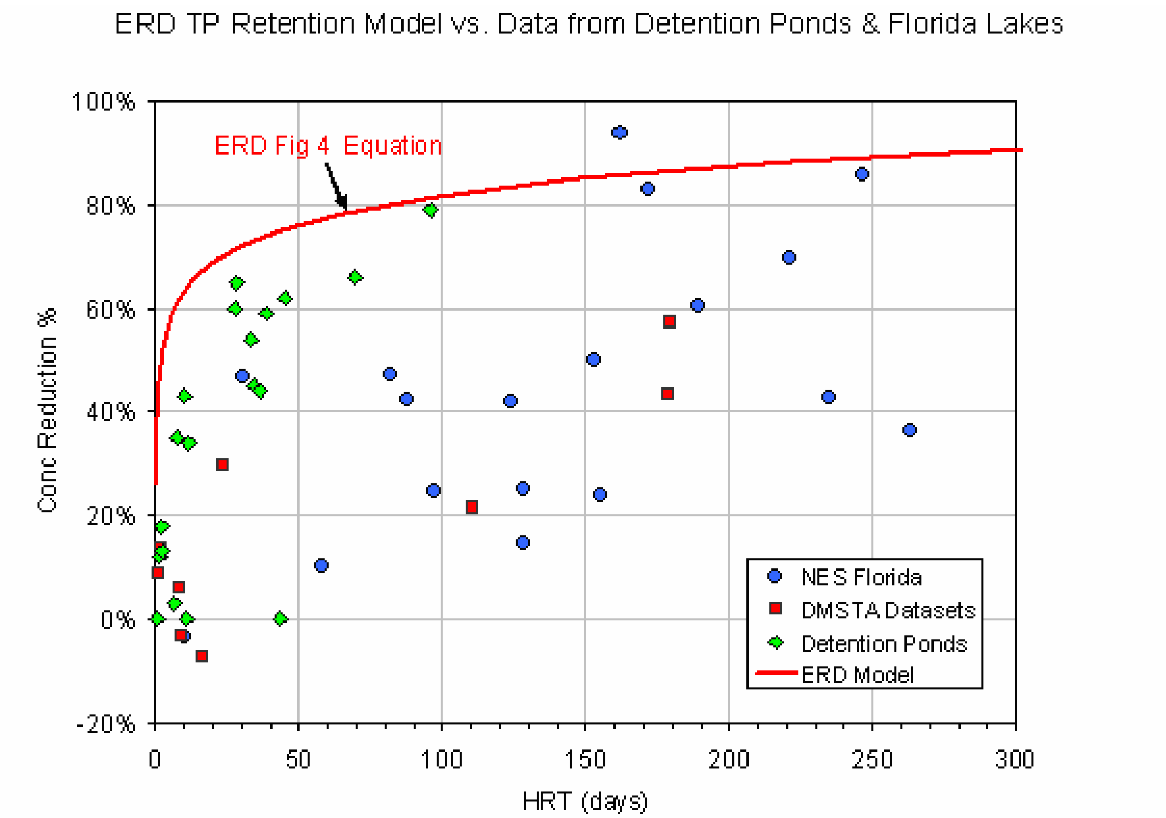
CDM (Appendix A) describes a second-order empirical model developed by the author (Walker, 1987) as an alternative method for predicting P removal. That model was originally developed based upon nationwide data from Corps of Engineer reservoirs (Walker, 1985) and used in software for assessment and prediction of reservoir eutrophication (BATHTUB, Walker, 2004). The model was later applied to nationwide data from 24 detention ponds (3 in Florida) (Walker, 1987) and incorporated into software for routing phosphorus through networks of detention ponds (PONDNET, Walker, 1989). Removal efficiency is computed as a function of four factors: mean depth, hydraulic load (=depth/residence time), inflow TP concentration, and inflow SRP/Total P ratio. Calibration to Florida lake data is shown in Figure 2. Since inflow SRP/TP values were not available for each lake, an average of 0.5 is used. This is typical of SFWMD long-term monitoring data from structures and pump stations discharging into the Everglades Water Conservation Areas and Lake Okeechobee. Results demonstrate the applicability of the second-order model to Florida lakes. A simple first-order settling velocity model [$R = K / (K + Q)$, where Q = hydraulic load m/yr, K = settling velocity 1.4-5.6 m/yr] fits the Florida lake data about as well as the second order model (Walker, 2005).

It is difficult to obtain P removal efficiencies above 60-70% by increasing basin area or residence time in a single well-mixed pond. Walker (1987) used the second-order model to evaluate the sensitivity of performance to other design features, including depth, infiltration rate, mixing regime, and the SRP/TP ratio. For a given residence time and inflow concentration, the model predicts that performance could be improved by increasing depth (subject to stratification concerns, discussed below), by increasing infiltration rate, by promoting plug flow vs. well-mixed hydraulics, and/or by chemical treatment to reduce the inflow SRP/TP ratio. Theoretically, these concepts can be applied to enhance performance at a given residence time or pond volume.

In CDM's example (Depth = 15 ft, Inflow P = 335 ppb , HRT = 270 days, Appendix A & Table 2-1, p6), the predicted phosphorus removal efficiency would decrease from 76% to 72% as a consequence of the higher SRP/TP ratio calibrated in Figure 2 (.50 vs. .33). The equations cited by CDM and in Figure 2 apply to a single, well-mixed pond. Based upon size and shape, this is a reasonable assumption for most of the Florida lakes in the calibration dataset. With ideal plug flow [($R = N / (1 + N)$, Walker, 1987] , the removal increases to 90%, similar to that predicted by ERD's model. This is an extrapolation of the model beyond the depth range of the Florida lake dataset (15 ft vs. 3-11 ft), but is within the range of the nationwide datasets (detention ponds, Corps reservoirs). With an inflow concentration of 300 ppb and depth of 15 ft, the plug flow version of the second-order model gives similar predictions to ERD's model at residence times > 50 days (Figure 3). This is a theoretical comparison, since I am unaware of any performance data from detention facilities with this depth and ideal plug flow. The predictions diverge for mixed systems, shallower depths, or lower inflow concentrations. Analysis of the supporting data for ERD's model would be required to determine whether the divergence in Figure 1 can be explained by greater depths and ideal plug flow conditions in the detention ponds used to develop the model.

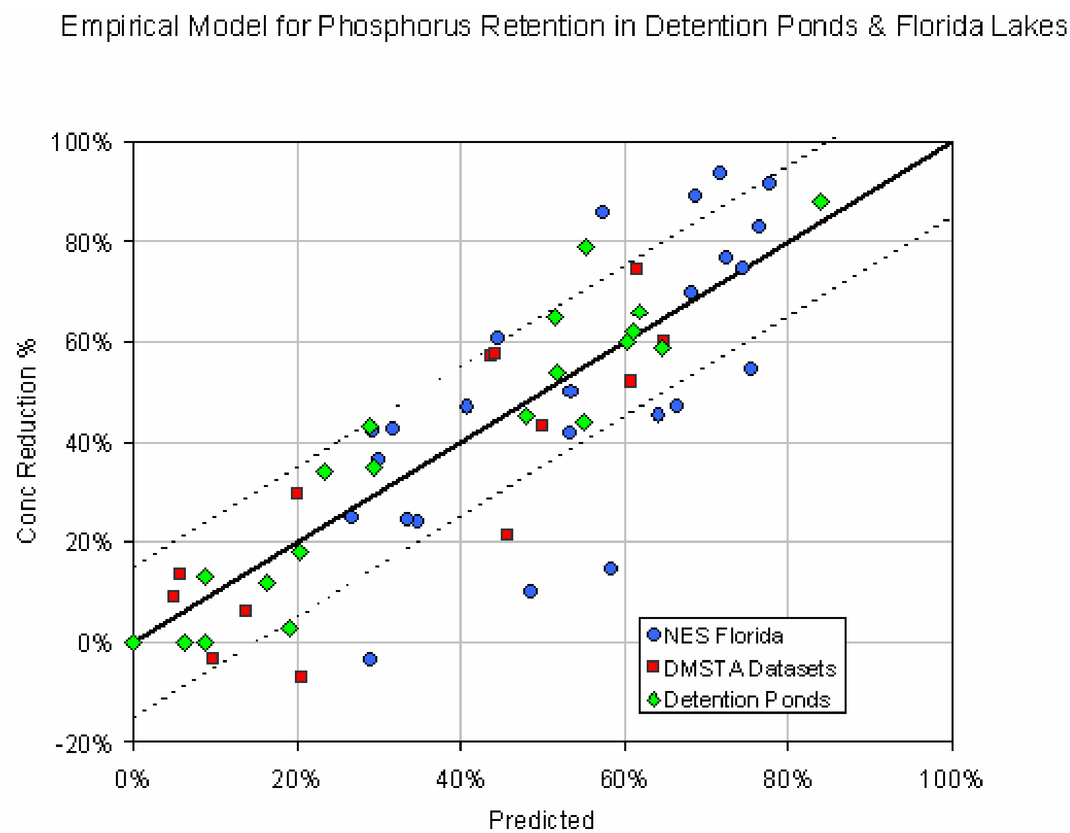
It is unlikely that ideal plug flow can be obtained in a single pond without a highly elongated shape and baffling. A design concept involving ponds in series followed by a shallow marsh was recommended for application in Minnesota (Walker, 1987;1987b). The associated gradients in depth and biological communities potentially promote a wider range mechanisms for assimilation of stormwater pollutants, as compared with single deep pond. Based upon the performance of Everglades Stormwater Treatment Areas (Walker, 2005), phosphorus removal rates in shallow (1-2.5 ft) marsh communities are higher than those typical of lakes (3 – 11 ft), as measured by effective settling velocities (10-30 m/yr vs. 2-6 m/yr). Wetland cells could be used for polishing provided that shallow water levels can be maintained by detention of storm flows in the upstream ponds.

Figure 1



NES Florida	USEPA National Eutrophication Survey, Florida (EPA, 1975)
DMSTA	DMSTA Reservoir Calibration Datasets, Florida (Walker, 2005)
Detention P	Runoff Detention Ponds, Nationwide (Walker, 1987)
ERD Equation	R = 8.08 ln(HRT) + 44.6

Figure 2



Second Order Model (Walker, 1985;1987)

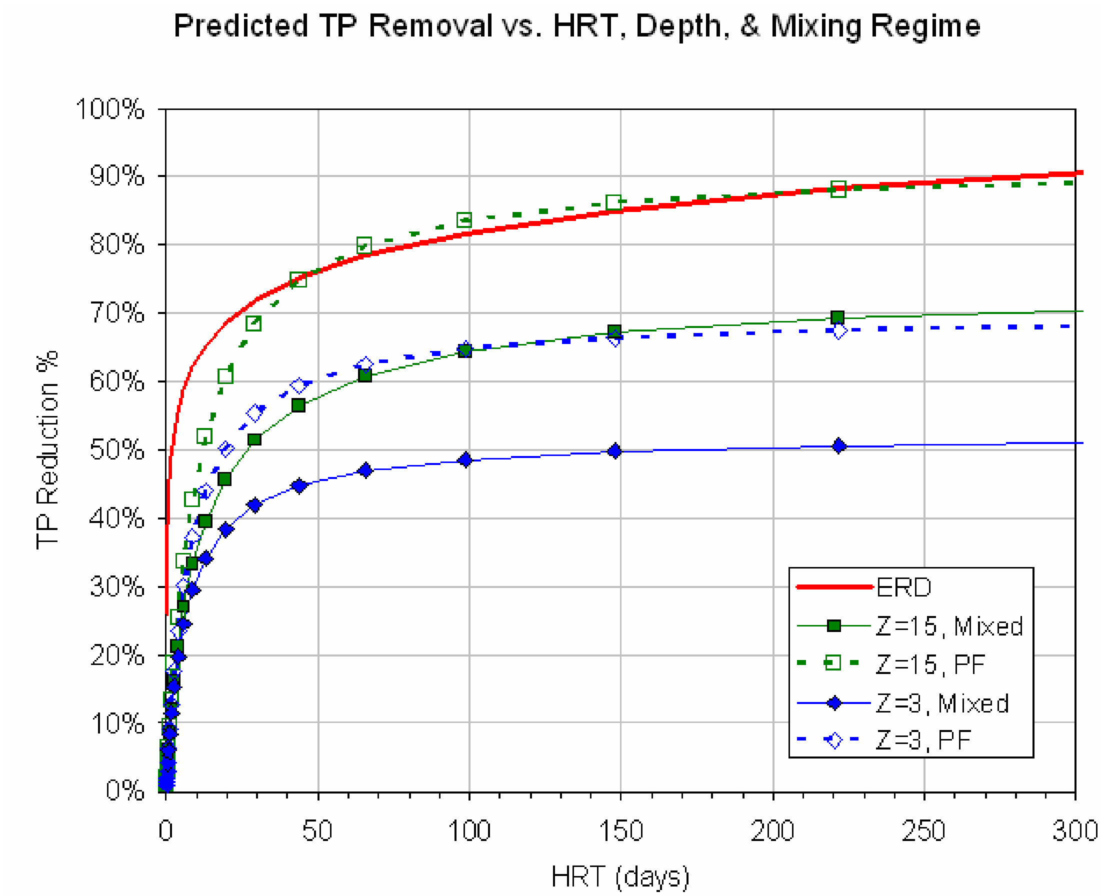
$$R = 1 - (-1 + [1 + 4 N]^{0.5}) / 2N)$$

$$N = .056 \, Pi \, Z \, / \, (Q + 13.3) / F$$

Symbol	Description	
Z	mean depth (m)	
Q	hydraulic load (m/yr)	
Pi	inflow TP conc (ppb)	< 1000 ppb
F	inflow SRP / Total P	
R	concentration reduction (%)	
N	second order reaction rate	

		F	
NES Florida	USEPA National Eutrophication Survey, Florida (EPA, 1975)	0.50	Regional Mean, SFWMD
DMSTA	DMSTA Reservoir Calibration Datasets, Florida (Walker, 2005)	0.50	Regional Mean, SFWMD
Detention P	Runoff Detention Ponds, Nationwide (Walker, 1987)	0.30	Walker, 1987

Figure 3



ERD	ERD Figure 4	
Z	Mean Depth, Feet	Walker (1987) Model (Figure 2)
Mixed	Completely Mixed Basin	See Equation in Figure 2
PF	Ideal Plug Flow	R = N / (1 + N), N defined in Figure 2
Inflow SRP/TP	0.5	
Inflow TP	300 ppb	

Section 3.2.2
Page 3-13, Figure 5, TSS Removal
William W. Walker
The TSS performance curves (Figure 5) appear to be reasonably consistent with other design criteria. As for nitrogen and phosphorus, it seems appropriate to provide more details on the supporting database.

Section 3.2.2
Page 3-13, Figure 5
Jonathan E. Jones
We have many of the same concerns with this figure related to percent removal that we expressed in the comments on Figures 3 and 4. The R2 value of 0.95 is significantly better than the R2 values for Figures 3 and 4. This is not surprising given the fact that TSS removal is much less susceptible to seasonal variability than nutrient removal by BMPs. We are surprised that only 12 data points are available for TSS removal as a function of residence time.

Section 3.2.2
Page 3-15, Figure 6, BOD Removal
William W. Walker
The BOD removal efficiency curve (Figure 6) is not based directly on performance data but generated by a model that assumes first-order decay, steady flows, and ideal plug flow conditions. While the assumed decay rate (0.1 / day) seems reasonable, the model would over-predict removal efficiency in storm-driven ponds with dynamic flows and concentrations or in ponds with less than ideal plug flow. ERD notes a background concentration of 1-2 mg/l attributed to algae and detritus. The K/C* model (Kadlec & Knight, 1996) with a completely-mixed assumption seems more appropriate. It is unlikely that BOD removal requirements will control BMP design, given the requirements for phosphorus and TSS removal. If application experience indicates this to be the case, consideration should be given to dropping the BOD performance requirement altogether.

Section 3.2.2
Page 3-15, Figure 6
Jonathan E. Jones
The same concerns that we have expressed for Figures 3, 4, and 5 with regard to percent removal also apply to Figure 6. We are encouraged to see that the authors address the idea of an irreducible concentration for BOD on Pages 3-14 and 3-16; however, we wish that Figures 3, 4, 5, and 6 better took this concept into account. The polynomial curved fitting (using a fourth order polynomial to represent what is widely recognized as a first order process) to achieve an R2 value of 1 does not appear to have a scientific basis.

Section 3.3
Ben R. Urbonas
The equation for "Overall Treatment Train Efficiencies" suffers from the assumption that a successive treatment facility will have the same "efficiency" (i.e., percent removal) as does the first one. That is not the case. Subsequent facilities often have to treat cleaner and cleaner inffluent. As was reported by Schueler and others, there is a lower limit to the effluent concentrations that can be achieved. The use of average effluent concentrations for each treatment facility in the "train", as suggestion WEF MOP #23 can produce more believable results.

Section 3.3
Page 3-17, Paragraph 2
Jonathan E. Jones
The equation for overall treatment train efficiency overstates the efficiency of the second BMP in series. This efficiency will be reduced because the first BMP in series will initially remove a significant portion of the pollutant load (pollutant removal efficiency typically declines in relation to influent concentration).

Section 3.4, Evaluation of Pond Stratification Potential
James P. Heaney
Pond stratification is rarely an issue in stormwater BMP evaluations since the normal range of residence times are in range of one to several days. Stratification could become an issue if storage lasts for several months as suggested by the performance curves in the previous section. The method proposed in the HM report is based on regression equation.

Section 3.4
Ben R. Urbonas
1. Good discussion on evaluation of pond stratification in this report.

2. The regression equation on page 3-22, as reported on page 3-23 has a very strong regression coefficient for the data supporting it. This implies confidence in its predictability.

3. Stratified ponds have been known to have seasonal turnovers, which release the constituent remobilized from the bottom deposits into the water column. Mechanical aerating of the deeper ponds is recommended in the report and is one solution to this potential problem.

Section 3.4
Page 3-18, Paragraph 2
Jonathan E. Jones
We suggest that one or more limnologists with experience in southwest Florida review Section 3.4 and offer recommendations, as appropriate.

Section 3.4
Page 3-18, Evaluation of Pond Stratification Potential
William W. Walker
Concerns about avoiding destratification are justified. Without aeration, intermittent periods of stratification and destratification driven by cyclical weather patterns could recycle nutrients, stimulate algal blooms, and cause fishkills. The method for predicting stratification potential is based upon a multiple regression relating anoxic depth to phosphorus, chlorophyll-a, and Secchi depth. Causal inferences from a multiple regression based upon three correlated independent variables are risky. Empirical equations based upon data from Canadian lakes are used to estimate chlorophyll-a and Secchi from phosphorus. The applicability of these regressions to Florida lakes is not supported. The Secchi term of the regression model appears to be dominant but ignores the effect of non-algal turbidity or color.
The combined equations predict anoxic depths ranging from 12 to 3 feet for total phosphorus concentrations ranging from 40 to 200 ppb. The anoxic depth should be compared with the maximum depth of the pond (vs. mean depth) to evaluate the need for aeration. This suggests that aeration would be required for most wet ponds sized according to this methodology.
It seems likely that the stratification depth would also depend upon mean depth, maximum depth, flushing rate, and/or surface area (as related to wind fetch). Walker (1985; 2004)

developed an empirical model that relates the mean depth of the upper mixed layer to mean depth in Corps of Engineer reservoirs. While it has not been tested in Florida, the model basically predicts that reservoirs with mean depths less than 12 feet are unstratified. Reckhow & Chapra (1983) describe a function that discriminant function that predicts oxygen status (oxic vs. anoxic) as a function of depth, areal water load, and areal phosphorus load. Again, this model has not been tested in Florida, but indicates the importance of morphometric and hydrologic factors.

Section 3.5, Estimation of Loadings From Wetland Systems
James P. Heaney
As mentioned earlier, wetlands and open water/lakes are usually viewed as controls, not sources. Using a runoff coefficient of 0.225 is arbitrary. Equally arbitrary is the assumption that 50% of the annual runoff into a wetland will be retained and 50% will be discharged. Wetlands vary widely in their design and operating regime (Kadlec and Knight 1996).

Section 3.5
Ben R. Urbonas

1. This part of the report is mostly based on data collected in Lee and Collier Counties. Analysis protocols based on local data sets, provided they are accurate and representative of SW Florida, have sufficient populations and are properly analyzed, should be much more reliable than ones based on assumptions and modeling.

2. Thus, unless that data are wrong or not representative of SW Florida, the approach suggested in this section should yield believable annual loads from wetland systems in SW Florida. However this observation may not apply if the natural wetlands receive urban runoff and the increased constituents it carries.

Section 3.5.1
Page 3-23, Isolated Wetlands
William W. Walker
The runoff coefficient for isolated wetlands (0.225) seems high. I assume that "isolated wetlands" refers to ones driven only by rainfall. Marsh evapotranspiration rates measured by SFWMD (Everglades Nutrient Removal Project) average 52.2 inches/yr (1996-2004). This is only slightly below the 53.3 inches/yr Ft Meyers rainfall and indicates a 1.1 inch/yr runoff rate or a runoff coefficient of 0.02 for wetlands that are continuously flooded. While I could not find this in the report, estimation of the "runoff coefficient" for lakes should also be justified relative to regional lake evaporation and rainfall rates.

I searched the report for the basis of the 0.225 assumption for wetlands. Page 2-23 refers to Table 5, but there are no runoff coefficient data in that table. Page 4-13 refers to Table 7, but it likewise does not contain runoff coefficients. It seems to have come from Table A.20, which lists runoff coefficients for 4 wetlands that average 0.225. However, the footnote indicates that three of these values were "calculated estimates" and only one was directly measured. This is an insufficient basis for recommending a default value.

The reported phosphorus concentration for wetlands (Table 5, Table 7, 0.09 mg/l) seems high. It is not clear whether the supporting data (Table 5) are from wetlands that are truly isolated and contain no external inputs. Note that the median concentration in Table 5 is 0.05 m/l (vs. .09 mg/l mean) and the frequency distribution is highly skewed. Background marsh concentrations in the Everglades are <0.01 mg/l (SFWMD data)   Pollman et al (2002) reported volume-weighted mean concentration of .005 mg/l in rainfall at 10 Florida sites, 5 of which were in SW Florida. The wet deposition rate averaged 7.5 mg/m^2-yr. Estimating dry deposition at twice that, the total deposition would be ~22 mg/m^2-yr. With ERD's values for wetlands (Rainfall = 53.3 in/yr, Runoff Coef = 0.225, TP Conc = .09 mg/l), the runoff rate would be 11 inches/yr and unit area export would be 27 mg/m^2-yr. This is similar in scale to regional estimates of bulk atmospheric deposition. Because of plant uptake, physical processes, and peat accretion, an isolated wetland would be expected to export less phosphorus than it receives from the atmosphere.

Assumptions for wetlands would have a large effect on BMP requirements in some cases. A more extensive search of regional data is recommended to identify appropriate runoff coefficients and concentrations for wetlands. Meanwhile, I recommend that site-specific data be required to estimate baseline loads for projects involving wetlands.

Section 3.5.2
Page 3-24, Paragraph 3
Jonathan E. Jones

This paragraph was the subject of Question 9 in the charge. As noted above, the 50%-50% assumption for annual runoff volume is arbitrary. This may be a useful assumption in absence of other data; however, we strongly recommend the use of site-specific data when available. Wetland hydrology varies seasonally and from year to year. It would be useful for the authors to provide their rationale behind the 50%-50% assumption. We believe that it is important for the Report to acknowledge the limitations of this assumption and to allow for the use of site-specific data when available in lieu of the assumption.

SECTION 4

Section 4
Ben R. Urbonas

1. The design examples are easy to follow.

2. As was discussed above, each example requires a number of assumptions on behalf of the person doing the analysis. It is the accuracy and validity of the assumptions made that will determine the accuracy of the comparisons between the pre- and post-development loads of constituents of concern. Without strong guidance from professionals overseeing the planning and design of new developments, this methodology can become a numbers game.

3. One minor example of the assumptions that was made can be founding example 2 where the runoff coefficient from the wetland remains unchanged, yet it may be receiving additional runoff from the newly developed lands. Typically, when larger amounts of hydrologic loading occur, a larger percentage of runoff takes place (i.e., has higher runoff coefficients).

4. In example 2 a natural flow-through wetland is filled, which is a questionable development practice in light of "no net loss of wetlands" federal policy. Mitigation of filled wetlands is not discussed in the report or in this example. Without knowing the regulatory wetland policy in Florida one cannot answer if this example provides a suggested practice that is acceptable. In addition, it assumes that the soils used to fill the wetland will retain their pre-development hydrologic characteristics. This assumption for Type C and D soils may not result in significant runoff differences, as they are very thigh to begin with, but can be way off for Type A and B soil groups.

5. Another questionable assumption occurs in example 2. It is assumed that the rate of removals of pre-treated runoff (i.e., by swales in this case) is the same by the pond as though the water entering it was untreated runoff. Does that mean that if the runoff entering the pond is very clean one can still expect the rates of removal (efficiencies) assumed in that example? There are diminishing returns in removal of constituents by ponds or any other treatment facility and eventually a limit in constituent concentration is reached after which one cannot expect cleaner effluent.

Section 4.1
Page 4-5, Paragraph 3
Jonathan E. Jones
It is not clear to us why the authors are suggesting that dry retention is an option on a site that has entirely Class D hydrologic soil group soils. With soils this tight, an infiltration-based method should not work.

APPENDIX A

Jonathan E. Jones

Appendix A

We offer the following comments on the tables included in Appendix A summarizing hydrologic characteristics and stormwater pollutant characteristics for different land uses and different sites. Many of the comments that we provide below are relevant to multiple tables in this appendix. Please note that we have conducted our review of the data and information in Appendix A at a cursory level, and that additional and more detailed comments could be made with a more in depth and time consuming review. Our observations are as follows:

- **Table A.1, Column 2**:The runoff coefficient of 0.169 for a 43.9% impervious site in Pompano Beach is significantly lower than we would expect.

- **Table A.2**:We believe that it would be useful to include the number of events for each study and the coefficient of variation for mean values in this table. This would provide the reader with some characterization of the limitations and variability of the data used. With regard to the overall mean value reported in the last column of the table, we question whether this value is an average of mean values from each site or an average of individual values pooled from all sites. Based on a quick check of the first row for total nitrogen, it appears that mean values for each site have been averaged to calculate the overall mean value. This may not be an appropriate technique if some sites have significantly more data than other sites. EMC data from sites with larger data pools are under-represented using this technique, while data from sites with one, or only a few, data points are over-represented. We would recommend comparing the overall mean value obtained from averaging individual data points from sites pooled together with the values that are reported in the tables in Appendix A.

- **Tables A.5, A.7, and A.9**:We note in these tables that runoff coefficient values reported in many cases are higher than the maximum annual runoff coefficient of 0.782 in Table 4. In Tables A.5 and A.7, it is not clear if the runoff coefficients reported are for individual storm events or represent annual average runoff coefficients. In Table A.9, however, the fifth row is clearly labeled "annual runoff coefficient". Four values in this row are greater than the maximum value in Table 4, and, in fact, one of these values (C = 0.85) from the Orlando I-4 site is from a study conducted by Harper in 1988.

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APPENDIX A

REVIEWER COMMENTS