UNITED STATES ENVIRONMENTAL PROTECTION AGENCY
WASHINGTON, D.C. 20460

July 31, 1985

OFFICE OF
THE ADMINISTRATOR

Hon. Lee M. Thomas
Administrator
U. S. Environmental Protection
Agency
401 M Street, S. W.
Washington, D.C. 20460

Dear Mr. Thomas:

The Environmental Engineering Committee of the Science Advisory Board was asked by the Office of Water to review a report entitled "A Probabilistic Methodology for Analyzing Water Quality Effects of Urban Runoff on Rivers and Streams." The Committee has completed its review, and is pleased to forward its report.

The Committee believes strongly that statistically-based approaches to water quality management are an important tool for the decision-maker, and commends the Agency for supporting the effort under review. The method described is technically sound, but only for the specific applications for which it was developed. The Committee has serious concerns about apparent Agency interest in using the aproach in situations for which it is not technically suitable. The Committee does not believe that the technique, as it now exists, should be extrapolated beyond the purpose and application area for which it was developed without appropriate additional development and verification, nor should it be used by individuals who do not fully understand the approach and the assumptions inherent therein.

If you have any questions, or should you wish any further action on our part, please call on us.

Sincerely,

Raymond C. Loehr
Chairman, Environmental
Engineering Committee
Science Advisory Board

Norton Nelson
Chairman, Executive Committee
Science Advisory Board

cc: H. Longest
C. Myers
D. Athayde
S. Tuller
T. Barnwell
E. Southerland
T. Yosie

REPORT

on the review of

"A PROBABILISTIC METHODOLOGY FOR ANALYZING
WATER QUALITY EFFECTS OF URBAN RUNOFF ON RIVERS AND STREAMS"

by the

Environmental Engineering Committee
Science Advisory Board
U. S. Environmental Protection Agency

June, 1985

I. EXECUTIVE SUMMARY

The Environmental Engineering Committee believes strongly that the Agency should consider certain water quality phenomena in a probabilistic manner. The Agency is commended for supporting the effort under review and similar activities, and is encouraged to pursue other efforts to increase its capabilities to deal with random environmental phenomena.

In its review of a report entitled "A Probabilistic Methodology for Analyzing Water Quality Effects of Urban Runoff on Rivers and Streams", the Committee agreed to address, and report to the Agency on, two general questions:

A. Is the technique scientifically valid and adequate for the specific application for which it was developed?

B. Is it appropriate for further or broader application?

The technique presented in the report was developed for advective systems, i.e., flowing fresh-water streams. It was developed for nonpoint source pollutants which are conservative (such as total dissolved solids - TDS) or are reacting substances characterized by first-order kinetics (such as biochemical oxygen demand - BOD).

No serious flaws were found in the model, but some clarifications and improvements are suggested. The Committee believes that the technique is scientifically valid and acceptable <u>for the purpose and conditions for which it was developed and presented in the report</u>.

The Committee has serious reservations about the potential for indiscriminate use of the technique for purposes and applications beyond its current purpose. In order for the existing technique, as documented in the report, to be appropriate for use in water quality situations involving many commonly encountered real-world aspects, considerable additional development and validation would be required. Included would be capabilities to deal with non-conservative sequential reactants (such as dissolved oxygen or some toxic substances or nutrients). The technique as it exists is not applicable to tidal or estuarine systems, nor to lakes. Aspects such as these are frequently encountered with both point and nonpoint source pollutants.

The Committee does not believe the technique, as it now exists, should be extrapolated beyond the purpose and application area for which it was developed without appropriate additional development and verification. This could entail substantial additional effort. Furthermore, the Committee believes there is cause for great concern if a valid technique with deceptively attractive simplicity in its ease of application (such as the one described) is pushed beyond its capabilities. This is, of course, true for any mathematical modeling, simulation or predictive procedure. The potential exists here due to the technique's having been developed specifically for the Nationwide Urban Runoff Program (NURP), but being under active consideration for adoption and use in other problem areas such as waste load allocation and toxic/pesticide fate and effects.

II. INTRODUCTION

There appears to be increasing interest in the environmental community in the fact that many natural phenomena have strong random components, and that environmental regulations should explicity acknowledge this fact. The Water Planning Division, Office of Water Planning and Standards, as a part of the Nationwide Urban Runoff Program, has been developing statistical methods for the analysis of urban stormwater for some time. These methods have had limited use and have not, in general, addressed water quality impacts.

The Environmental Engineering Committee of the Science Advisory Board was asked, in a memorandum from the Office of Water, to review a report entitled "A Probabilistic Methodology for Analyzing Water Quality Effects of Urban Runoff on Rivers and Streams", dated February 15, 1984, and prepared by the Office of Water.

The principal issues for review were:

A. Is the technique described in the report scientifically valid and adequate for the specific application for which it was developed?

B. Is the technique appropriate for further or broader application?

The Committee agreed to accept the task, and organized a Subcommittee, including several consultants with special expertise in the area of stochastic modeling, chaired by Dr. Benjamin C. Dysart III (see Subcommittee roster, Appendix A), to conduct the review. The Subcommittee and its consultants met on November 26, 1984, and on February 25, 1985. In the course of its review, the Subcommittee examined the report (together with an earlier report prepared by the Agency's Office of Water Planning and Standards entitled "A Statistical Method for the Assessment of Urban Stormwater"), written comments by four individuals, and had an in-depth review of the report by Agency staff and their consultants (Dr. Dominic DiToro and Mr. Eugene Driscoll) who prepared the report under review. The SAB consultants, Dr. Mitchell Small and Dr. Barry Adams, were asked to prepare written comments, and these are attached as Appendices B and C.

III. CONCLUSIONS

All natural processes or phenomena such as hydrology and water quality have probabilistic or stochastic elements. However, the majority of the mathematical models used today are essentially deterministic in structure, i. e., they are based on the concept of conservation of mass and momentum, and attempt to simulate explicitly, using direct solutions of equations, the processes that transport and transform material and energy in the natural system. Statistical models, on the other hand, do not explicitly simulate natural system processes, but rather provide estimates of the value(s) of output (dependent) variables, given the values of input (independent) variables in the system and the statistical relationships between them. These models allow the direct consideration of random phenomena, such as streamflow or rainfall, as well as the performance of traditional treatment processes and other engineered control systems. They also reflect as yet undefined mechanisms.

They are limited, however, in that any statistical relationship derived from a given set of data for a natural system reflects the particular spatial arrangement and natural system processes existing when the data were collected. For any significantly different system, new data must be obtained and new statistical relationships developed.

The technique presented in the report was developed for advective systems (freshwater flowing streams), and for nonpoint source pollutants which are conservative or singular-reacting substances, e.g. total dissolved solids (TDS), suspended sediment, biochemical oxygen demand (BOD), and bacteria.

The Committee concludes that:

A. The technique is scientifically valid and acceptable for the purpose and conditions for which it was developed and presented in the report.

No critical flaws were found in the technique as presented in the report. No substantial statistical problems were found. Several conclusions were reached by the the Committee and its consultants (see Appendices B and C). These are summarized as follows:

1. The methodology presented in the report is acceptable for conservative and first-order reactive substances in flowing fresh water streams. In such systems, the assumption of instantaneous and perfect mixing is customarily made; the Committee believes that this is an acceptable practical approximation in the vast majority of cases.

2. While some reviewers contended that a simulation modeling approach was more appropriate than the probabilistic model, the Committee believes that these two approaches should be considered as alternatives, the selection of which would depend on the nature of the problem and the question addressed in a specific application.

3. In response to questions raised about the mathematical validity of the methodology, the Committee is satisfied with the approximations concerning log-normality, the mean recurrence interval, and the uncertainty analysis. This comment is not meant to preclude the examination of alternative procedures as described in the reviewers' and consultants' comments.

4. In response to the question concerning incomplete or inadequate field data, or data which are thought to be incomplete or inadequate, the Committee recognizes extrapolation from other locations of similar characteristics as a common and accepted practice.

B. <u>The technique, as it is described in the report, should not be extrapolated beyond the purpose and application area for which it was developed without substantial additional development and verification.</u>

The Committee believes that a probabilistic methodology has a very important role and contribution as a modern water quality management tool for regulation, planning and facility design. In order for the technique presented in the report to be appropriate for use in other water quality assessments, other capabilites would have to be included. Among these would be the capability to deal with non-conservative sequential reactants such as dissolved oxygen and some toxic substances and nutrients, with tidal or estuarine systems, and with multiple-source pollutant inputs within a stream reach.

Furthermore, the Committee believes that there is cause for great concern if a valid technique with deceptively attractive simplicity in its ease of application (such as the one described in the report) is pushed beyond its capabilities. In this case, the potential exists due to the technique's having been developed specifically for the Nationwide Urban Runoff Program but is apparently under active consideration in other EPA offices for adoption and use as is in problem areas such as waste load allocation and toxics/pesticides fate and effects.

The question of whether it would be worthwhile to modify, extend, and verify the technique to cover additional application areas is a resource question for the Agency more than a scientific one that can be addressed by this Committee. The Committee believes the potential surely exists to extend the technique to cover more applications. This remains to be done, however, and someone must decide to devote the necessary resources to the effort if the technique is to realize any potential beyond its existing limited capability. These resource requirements could be quite extensive.

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ENVIRONMENTAL ENGINEERING COMITTEE

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REVIEW OF PROBABILISTIC METHODOLOGY FOR ANALYSIS OF WATER QUALITY IMPACTS OF URBAN RUNOFF

Mitchell J. Small

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December 21, 1984

In general, I find the probabilistic model appropriate for the purpose and set of conditions for which it was designed. A discussion of pertinent technical details is provided in the final section of this review. I did find a clear-cut error in the initial equations given for the mean recurrence interval (MRI). However, the approximate equation is correct. That is, it approximates the corrected version of the MRI equation provided in my final section. As such, the results presented in the report are essentially correct. I also examine a number of the issues raised in the review of Kahn. But first, I'd like to address some of the broader issues relative to the model's applicability and possible extension.

The authors present the model as a tool for initial assessment. The conceptual nature of the model limits it to that purpose, and the limitations are highlighted in the report and in the reviews of Southerland, Barnwell and Roesner. There is always a temptation to push tools as easy to use as this into more detailed applications, waste-load allocation, etc. The reviewers appear to be wary of this. The first issue that should be raised, however, is not the tool, but the generality and importance of the question it addresses: the formulation of probablistic water quality criteria for nonpoint source impacts. Should we pursue more precise specification of water quality exceedence frequencies, as is done with air pollution? Is a 10 year MRI value appropriate to evaluate nonpoint source contributions? If so, then it is up to the working engineers and planners to devise appropriate analysis techniques to predict exceedence frequencies for their specific water system and problems. This could involve direct, deterministic simulation using inputs from a long-term period of record; stochastic simulation using synthetic flow and quality inputs generated consistent with observed or regional patterns; or direct analytical techniques such as developed in the NURP report. As indicated in the reviews, many analysts are more comfortable with simulation techniques, in part because random process properties (temporal and spatial persistence, correlation, etc.) are inherently included. There should not be an official push to require any one particular approach. As engineers receive more exposure to the probabilistic method and recognize its ease of use, some may wish to use it as a complement to their more detailed evaluations. This will provide working examples which will serve to either build confidence in the method, or pinpoint its weaknesses and limitations. This may be stimulated if analysts required to evaluate a 10 year concentration find the simulation approach too cumbersome. The point is, let the problem and need be better specified . . . this will drive the selection of tools.

As noted by Southerland and Barnwell, direct short-term concentration impacts from stormwater runoff are rare. Indeed it is interesting that the three test cases cited in the NURP report lead to a conclusion of no significant impairment. Problems which do occur, such as lake or estuary nutrient problems, long-term bottom sediment buildups, or coastal zone bacterial problems also have important probabilistic features. Research to develop analytical methods for these problems should, I think, be encouraged. The resulting models may again be limited to initial assessments, but could provide useful complements to more rigorous modeling approaches.

Another issue of general concern involves the role and characterization of uncertainty in this, and other similar models. The uncertainty computed directly in the current model is representative of inherent temporal variability. Some storms are small, some are large. Sometimes the upstream flow is high, sometimes low. Similarly for upstream and runoff concentrations. This variability is represented by fitted or estimated distribution functions. The parameters of these distributions are, however, also unknown, reflecting uncertainty of another sort. In a narrow sense this uncertainty may be represented using confidence intervals for means or variances estimated from observed data, however in reality it reflects our larger uncertainty in the overall structure and representation of the problem. This scientific uncertainty is somewhat subjective, but may be represented in the model and is (in principle) reduceable.

The NURP report addresses scientific-parameter uncertainty in its sensitivity analyses for stream-runoff correlation and upstream concentrations (Ch. 5). In recent years, however, more formal and complete methods for sensitivity and uncertainty analysis have been developed. These generally involve replication of the underlying model with random or stratified sampling of the uncertain input space, and examination of the resulting distribution of the output variable(s). The relative importance of the different input parameters is inferred using methods such as rank-order correlation analysis. A nice example of this technique is provided in the attached article by Jaffe and Ferrara (Water Research, 18: 1169-1174). This type of analysis should be performed on the NURP model to provide a more complete and unified picture of model sensitivity. An alternative approach is to allow users of the methodology the ability to perform rapid and exploratory sensitivity analysis with interactive graphic software.

Technical Review

The only major technical problem I was able to uncover is an error in the initial equations for the MRI, Eqs 3-34 and 3-35. The correct forms are as follows:

$$Pr\{C_o^{max} > C_o^*\} = 1 - Pr\{C_o < C_o^*\}^N \qquad (34a)$$

$$MRI = \frac{1}{1 - Pr\{C_o \leq C_o^*\}^N} \qquad (35a)$$

Eq. 36 is a proper first order approximation for Eq. 35a (though not for Eq. 35, which yields absurd results). The use of Eq. 36 is no longer necessary, however, because Eq. 35a will not blow-up, as did Eq. 35, when used with a calculator or in a BASIC program. The approximation is close enough, particularly for long return periods, so that none of the results presented to date need be redone. However, for future work, Eq. 35a is preferable.

I will address now some of the points raised by H. Kahn. Eq. 3-10 is correct because high values of D corresspond to low values of ϕ, so that the 95 percentile D corressponds to the 5 percentile ϕ. The legends on some of the probability plots are indeed reversed from usual convention, showing the probability of being greater, rather than less than, the indicated value. I don't expect this to provide too big a problem, however, for most users. The use of the term "arithmetic moments" is a matter of semantics designed to differentiate between the direct sample moments and geometric (log) moments which are also used. Additional clarification of the useage could perhaps be provided in the report.

The suggestion to use a beta distribution for ϕ is interesting. I believe that the beta result is only precisely true when Q_R and Qs are independent and gamma distributed with the same scale parameter (though not necessarily the same shape parameter). Neither of these limitations is appropriate for the problem at hand, but the beta distribution is sufficiently flexible over the range 0-1 so that it probably still provides a good approximation. It is not clear though that it will lead to either considerable simplification of the calculation procedure, or a more accurate estimate for the Co distribution finally computed (particularly in the numerical scheme, which seems to be the method of choice). I guess someone needs to push the calculation through to see if this is the case. Similarly for the logistics model. I can see where if -lnD is assumed to have a logistics distribution, then ϕ corressponds to the cdf measure. But I was unable to follow this through for computation of the moments of ϕ, or for other direct use in the model. In general, I am satisfied with the level of validation provided for the computational procedure in the NURP report and the recent paper of Di Toro (ASCE, June, 1984). The authors of the report may, however, wish to present some distribution plots for ϕ to see how large is the deviation from lognormality, and how *much* probability is predicted in the 'impossible' range, $\phi > 1$.

I believe Kahn is correct in noting the sensitivity of confidence intervals in the calculated MRI. I also believe that some of the "nonstatistical" factors which I raise above (when discussing scientific uncertainty) are most critical in determining the appropriate range of this sensitivity.

To summarize then, I find the direct model largely appropriate and scientifically credible for the purpose for which it was designed. A number of more general issues are raised in addressing the model's broader applicability.

REVIEW OF
A PROBABILISTIC METHODOLOGY FOR THE ANALYSIS OF WATER QUALITY EFFECTS OF URBAN RUNOFF

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Department of Civil Engineering
University of Toronto
January 1985

This review is provided at the request of the Probabilistic Methods Subcommittee/Environmental Engineering Committee/Science Advisory Board of the U.S. Environmental Protection Agency. The review addresses both the original document "A Probabilistic Methodology for Analyzing Water Quality Effects of Urban Runoff on Rivers and Streams", Office of Water, U.S. EPA, Washington, D.C., 15 February 1984, and comments on the methodology contained in memoranda from Henry D. Kahn (21 July 1984), Elizabeth Southerland (undated), Thomas O. Barnwell, Jr. (29 May 1984) and Larry A. Roesner (11 July 1984).

BACKGROUND

There are some issues concerning methodologies and models which require addressing before evaluating the probabilistic methodology.

Model Function

For the purposes of this discussion, a distinction is made between two pronounced differences in the function or role of models. A crude distinction is as follows:

(i) Physical Models - models which focus on relating cause and effect in terms of the physical variables of the phenomena involved in the processes being modeled.

(ii) Decision Models - models which focus on relating cause and effect in terms of the decision variables of the alternatives used to control the processes being modeled.

As the function of a model is a major determinant if its formulation, the formulation of a model directly defines its use: a decision model is less appropriate for explaining cause and effect at the level of the detailed physical phenomena being described in the problem; a physical model is less appropriate at the level of evaluating the cost-effectiveness of control alternatives for solving the problem.

Examples of commonly encountered physical models are SWMM III, QUAL II, RECEIV II, etc. while examples of decision models for evaluating water quality control alternatives are STORM, NPS and analytical probabilistic models.

Model Operation

Physical models and decision models are "operated" under different conditions. Physical models simulate details of the process, including particular water quality phenomena, under only very specific conditions (eg. 7Q10 conditions). Thus, the system behaviour is known in physical detail but only for very limited conditions. On the other hand, decision models simulate a more aggregate response of the process over the complete spectrum of conditions (eg. the entire probability distribution of flow conditions). Thus, the system behaviour is known in less physical detail, but it is more generally known over a wide range of conditions,

In a very coarse way, the difference between physical and decision models is the following:

Physical models describe the response of the system in terms of the details of the water quality transformations for surrogate conditions.

Decision models describe the response of the system in terms of surrogate water quality transformations for a complete range of conditions.

In this way, physical models are generally limited to the analysis of specific events while decision models analyze a continuum of events. A tradeoff in model selection is apparent: more detailed analysis of behaviour for less general conditions or less detailed analysis of behaviour for more general conditions.

(For a more detailed discussion of event simulation deficiencies, see the attached paper on "Design Storm Pathology" by Adams and Howard).

THE DECISION PROBLEM

The decision problem at hand contains a series of elements as follows: INPUT→DRAINAGE SYSTEM→OUTPUTS→RECEIVER→WATER QUALITY→USE. The inputs are meteorologic and are taken as given. The drainage system is comprised of a catchment and engineered components of the catchment (conveyance, storage, treatment, etc. components). The outputs are point and non point discharges categorized by variable time series of water and pollutant masses. The receiver is the stream, river, lake or estuary accepting the point and nonpoint discharges. The combined characteristics of the receiver and discharges determine the in-situ water quality which ultimately affects the level of beneficial use associated with the receiver.

The decision problem is to determine the appropriate level of beneficial use associated with the receiver and, simultaneously, the engineering and management measures to be deployed on the catchment in order to achieve that level of use.

It has been widely accepted that water quality criteria may be used as surrogate measures of the degree to which level of use is achieved. It is thus common practice that decisions regarding the engineering and management of the catchment are made on the basis of predicted in-stream water quality. Similarly, the magnitude and frequency of point and nonpoint discharges (the outputs of the catchment) may be used in turn as surrogate measures of the resulting in-stream water quality. In this way, the engineering and management measures may be evaluated on the basis of what happens at the "end-of-the-pipe".

The question is what level of information is most appropriate to the decision at hand? Is it preferable to have much information on what happens at the end-of-the-pipe, even though this is a surrogate for in-stream water quality which is in turn a surrogate for level of beneficial use; or little information on resulting water quality; or even less information on level of beneficial use? Clearly, as we move farther "downstream" in the process from input to use, we gain in detail but lose in scope. At some point, the quality of information from modeling efforts must be judged. For a given application there is an optimal tradeoff between detail and scope of information.

For example, if two mutually exclusive engineering alternatives for pollution control are available at similar cost and if one of the alternatives results in pollutant mass discharges with a smaller mean and variance than the other, it is clearly preferable. No "water quality modelling" is required to make the decision. A valid decision may be made on the basis of what happens at the end-of-the-pipe. This example is admittedly simplistic but it makes the point: the information provided by modeling efforts should be appropriate to the decision being made.

EVALUATION OF THE METHODOLOGY

It is in the above context that the probabilistic methodology must be evaluated. The models of the probabilistic methodology should not be viewed as simply hydrologic models, water quality models or even statistical models; rather, they should be viewed as decision models which contain elements of hydrology, water quality and statistics.

Thus, the adequacy of the probabilistic methodology must be evaluated with respect to the following:

Physical adequacy - How well do the models represent the physical phenomena (hydrology, hydraulics, water quality transformations, etc.) that they attempt to describe?

Mathematical/statistical adequacy - How good are the underlying simplifying assumptions made in the model development for reasons of tractability? Are the mathematical assumptions consistent with the physical processes and are the derivations correct?

Functional adequacy - Is the information provided by the methodology appropriate to the decisions made with the information? Is there a good balance between the scope and detail of the information? Do users have confidence in the technique?

An essential point to consider is that these different measures of adequacy cannot be applied in isolation. The model evaluation criteria must be applied together.

Physical Adequacy

The basis of the probabilisitic methodology is that probability distributions of upstream flow/concentration and urban runoff flow/concentration are transformed to a probability distribution of downstream concentration. The transformation is described by a simple mass balance. The major question regarding physical adequacy of this model are: how realistic are the data needs of the model and how realistic is the mass balance as a representation of the receiver?

The data needs are the statistics (mean, standard deviation) of the upstream flow and concentration and the urban runoff flow and concentration. These data requirements are minimal, and it is safe to say that alternative evaluation methodologies would require much more data. Hence, these data needs are judged to be quite realistic.

A potential problem exists in the estimation of the statistics of urban runoff flow/concentration. The methodology relies on deriving these statistics from field measurement. The evaluation of the methodology must ask whether these field measurements are generally available. If not, can such statistics be generalized from data at other measurement sites? Even if field data are available, they are applicable to the current condition of the catchment. If alternative engineering or management measures for urban runoff control are to be evaluated by the methodology, then such measures would change the statistics of urban runoff flow/concentration. To use the probabilistic methodology for the evaluation of alternative control measures, a satisfactory method of predicting the statistics of urban runoff flow/concentration resulting from these measures must be found. Here again, a reasonable conclusion is that methods for predicting these statistics may not be perfect but other methodologies with the same objective would encounter at least similar if not greater problems.

The second question regarding the physical adequacy of the methodology is the adequacy of the mass balance as a representation of the receiver. The two major problems arising from this simplification are the absence of reactions in the receiver and the assumption of instantaneous and perfect mixing. Both problems have been identified in the memoranda of Southerland and Barnwell.

Three approaches can be taken regarding the absence of reactions in the receiver: (i) build appropriate reactions into the model, (ii) restrict the use of the model to "conservative" substances and (iii) impute the quantitative effects that reactions would have on the model results based on no reactions. The first approach may indeed be possible; however, it may be undesirable in its detraction from the simplicity of the model. The second approach may be undesirable by virtue of nothing being truly conservative in this sense. Substances which may be conservative in the water column may be reactive with respect to sediment or biota. The third approach may have merit if the effects of the reaction are generally known and particularly if they are monotonic. For example, it may be sufficient to know if the model results are always conservative and if so, then to what extent are they conservative.

A similar approach may be taken regarding the assumption of instantaneous and perfect mixing. It may be adequate to live with the assumption with the understanding of which side the error lies and the extent of the error.

These remarks on the physical adequacy of the methodology must be taken in the context of the function of the methodology. It is reiterated that the physical adequacy cannot be judged in isolation from the role of the information produced by the models in decision making. It is my opinion that the proposed probabilistic methodology is appropriate, in terms of its physical adequacy, for its intended purposes in this sense.

Mathematical/Statistical Adequacy

The general questions concerning the mathematical/statistical adequacy of the probabilistic methodology are as follows:

i) are the derivations correct in a mathematical sense?

ii) are the distribution assumptions of the input (Q_s,C_s,Q_r.C_r) and ϕ appropriate?

iii) are there drawbacks associated with the numerical solution requirements of part of the methodology?

iv) are there other model outputs required (such as confidence limits on predicted MRI's as suggested in the Kahn memorandum)?

These questions are addressed in the same order below.

I have found the derivations to be correct except for the deduction of equations (34) and (35) from equation (33) on page 3-13 as noted in the review by Small. However, the estimation for MRI in equation (36) is correct.

The assumption of distribution function form is a constant problem. Since no real empirical distribution obeys any theoretical distribution, the selection of theoretical distribution form is always a compromise. Although the issues raised by Kahn are quite legitimate, there are two factors in favor of the log-normal distribution assumption: (i) many natural processes are well-approximated by log-normal distributions and (ii) the field data in the report strongly suggest log-normal distributions. Since I would find it difficult to justify alternative distributions, I would support the log-normality assumptions concerning Q_s, C_s, Q_r and C_r in lack of evidence to the contrary.

The assumption of the log-normality of ϕ is more obscure. The assumptions appear to have no significant disadvantage based on the results presented in the report. However, I would agree that some experimentation with other distribution function forms for ϕ would be useful.

The two computational techniques offered by the methodology consist of an approximate method of moments which yields closed-form analytical solutions and a numerical integration procedure which requires fewer simplifying assumptions. I am confident that a user of the methodology could rapidly gain an appreciation for the limits of applicability of the approximate method by running both procedures in tandem. I would think that the closed-form solutions would be appropriate for many applications. Even for those applications where this is not the case, the numerical procedures are not overly complex. I would judge them to be an order of magnitude less complex than operating simulation models as suggested in the Roesner memorandum.

Since there is uncertainty in parameter estimates used in the methodology, additional model output reflecting this uncertainty would be useful. The calculation of confidence intervals on MRI as suggested by Kahn would undoubtedly enhance the utilization of the methodology.

Functional Adequacy

The general questions concerning the functional adequacy of the probabilistic methodology are as follows:

(i) does the information provided by the probabilistic methodology strike a good balance between an adequate representation of the processes being modelled and an adequate scope for the problems being addressed, and as such does the methodology lend itself to a good decision support model?

(ii) will users adopt the methodology?

These questions are addressed in the same order below.

I believe that the methodology is an appropriate model for decision support. The results of the methodology are quite credible and the validations contained in the report are convincing. The information contained in Figures 4-6a & b is not only extremely useful but also easily attained. I would think that information of this kind would go a long way to improving decision making in the practice of urban runoff control.

The probabilistic methodology is not as familiar to most analysts as more conventional approaches such as simulation; however, I believe that this should not act as a deterrent to the acceptance of the methodology. This process of user acceptance could be greatly enhanced with "side-by-side" comparisons with simulation model output. As the profession adapted to simulation modelling from pencil and paper methods, so it should adapt to analytical models.

ALTERNATIVES TO THE PROBABILISTIC METHODOLOGY

The only serious alternative to analytical models such as those contained in the probabilistic methodology is continuous simulation, as indicated in the Roesner memorandum. Roesner quite properly points out that simulation models have the advantages of not requiring many of the simplifying assumptions of analytical models and being able to use historical meteorologic data directly. However, these features also represent disadvantages. Continuous simulation models require massive computer code and massive amounts of meteorologic data. This has meant mainframe computers and magnetic tapes of data which act as significant deterrents to continuous simulation. Models such as STORM have been available for about two decades, yet little widespread practical use has been made of them. Although such models will become more accessible when ported to microcomputer systems, analytical models will still have advantages.

In the future, analytical and simulation models should be used together. For the present, although continuous simulation is not widely-used, this form of analysis is essential for intelligent decision making in urban runoff control practice. Analytical models such as those proposed in the probabilistic methodology can fill this void.