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MEMORANDUM

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Subject : **Response to Comments of the NRC Report on ATSDR Water Modeling Study.**

The National Research Council (NRC) was requested to conduct a review by the Department of Navy (DON), under a mandate by the U.S. Congress (Public Law 109-364, Section 318). The U.S. Navy requested the NRC review to address whether adverse health outcomes are associated with past drinking-water contamination at U.S. Marine Corps (USMC) Base Camp Lejeune, North Carolina. The NRC review included an assessment of the Agency for Toxic Substances and Disease Registry's (ATSDR) current health study on birth defects and specific childhood cancers at Camp Lejeune and in particular, water-modeling analyses and findings to date. The NRC report released on Saturday July 13, 2009 (NRC 2009) covers a wide range of topics that include: (i) conceptual topics of exposure analysis and source characterization that are based on expert opinion of NRC committee members; (ii) water-modeling concepts that are based on the observations of the NRC committee and the critique of the science-based tools and analyses that are described and used in ATSDR's technical reports on Tarawa Terrace and vicinity (Maslia et al. 2007); and, (iii) the critique of findings and interpretation of water-modeling study results that were completed by ATSDR at Tarawa Terrace and vicinity at Camp Lejeune.

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To accurately respond to the comments made under each category that I have identified above, the review comments I am providing below are grouped under two specific headings. This is in an effort so as not to confuse the reader and mix-and-match the review comments reported by the NRC committee which range from "conceptual topics" to the "actual data reported" in the ATSDR water-modeling study. I hope this approach will provide ATSDR with a clear picture of a range of erroneous statements and mischaracterizations made in the NRC report which are very puzzling. Accordingly, the discussion included in my review comments will cover a range from "conceptual" perspectives on exposure analysis to "water-modeling analysis" and "application specific" topics that are addressed in the NRC report.

It is important to note that the review comments I am providing below are only associated with the water-modeling aspects of the current ATSDR health study and the NRC report, and do not cover the epidemiology topics. All references made to the "NRC" report refer to the recently released NRC report titled, "*Contaminated Water Supplies at Camp Lejeune—Assessing Potential Health Effects*" which is cited as NRC (2009) in the reference section of this memorandum. Furthermore, the reader should recognize that sentences in "*italic font"* are extracted verbatim from the NRC report and statements in "regular font" are my responses to those specific NRC report statements.

A. REVIEW COMMENTS ASSOCIATED WITH CONCEPTUAL TOPICS OF EXPOSURE ANALYSIS AND SITE CHARACTERIZATION:

1. **Comment on p. 29:** *Exposure assessment for epidemiologic studies of the effects of water-supply contamination includes two components. The first is estimation of the magnitude, duration, and variability of contaminant concentrations in water supplied to consumers. An important consideration is hydrogeologic plausibility: an association between a contaminant source and exposure of an individual or population cannot exist unless there is a plausible hydrogeologic route of transport for the contaminant between the source and the receptor (Nuckols et al., 2004). The second component is information on individual water use patterns and other water-related behaviors that affect the degree to which exposures occur, including drinking-water consumption (ingestion) and dermal contact and inhalation related to the duration and frequency of showering, bathing, and other water-use activities. Water use is an important determinant of variability of exposure to water-supply contaminants, particularly if it varies widely in the study population. Ideally, exposure-assessment strategies include both components, but in practice it may be difficult to obtain either adequately.*

   Response: In this comment, which also includes a reference to the work of one of the committee members (Nuckols et al. 2004), the NRC committee is providing the reader with their understanding of the components of an exposure study that is associated with pollutants that may exist in an aquatic pathway at a contaminated site. The aquatic

exposure analysis framework described in this statement is a conceptual statement and represents a very restrictive view of the exposure pathway analysis that needs to be considered at contaminated sites given the current understanding of the interaction between environmental pathways and the behavior of chemicals along those pathways.

Current knowledge in this scientific field recognizes that in an aquatic pathway exposure study the environment must be considered as a whole and scientific and regulatory approaches alike must take into account complex multimedia and intermedia interactions that exist in a multitude of potential environmental pathways at a site. In my opinion one should not emphasize only the concept of a "**hydrogeologic connection**" between the contaminant source and the exposure point as put forth by the NRC committee. This conceptual suggestion made by the NRC committee would be a very elementary and a restrictive exposure analysis framework.

As specialists in this field, we are well aware of the fact that pollutants released to an aquatic environment are distributed among environmental media such as air, water, soil, vegetation etc., as a result of complex physical, chemical and biological processes. Thus, environmental pollution is a **multi-pathway problem** and environmental exposure assessment methods require that we carefully consider the transport, fate and accumulation of pollutants in the environment as a whole, (Cohen 1986). Methods that are proposed to evaluate environmental migration or exposure characterization in this envirosphere must consider all potential pathways and also the interactions between these pathways. In the scientific literature, the multi-pathway approach to environmental exposure analysis is identified as Total Exposure Characterization (TEC).

Elements of this multi-pathway analysis for an aquatic contamination source are imbedded in the ATSDR water-modeling study that is being conducted for the Tarawa Terrace area of the Camp Lejeune site as much as possible given the data restrictions. The specific pathways and processes considered in the ATSDR water-modeling study are: (i) saturated groundwater; (ii) unsaturated groundwater; (iii) vapor emissions; (iv) multispecies analysis of contaminants in these three pathways; (v) mixing in the water treatment system; and, (vi) water-distribution system estimates.

In this analysis framework it is also important to recognize that one should not try to fit a physical problem to a model that may be readily available for use. Instead, appropriate models should be selected or developed that would fit the characterization of the physical problem at hand. Thus, selection of appropriate modeling tools to complete such an analysis is very important and is considered in sufficient detail in the ATSDR study. This is a very important point, which was either completely ignored in the NRC report or, steps taken by the ATSDR water-modeling team to address these issues in a sound scientific manner were considered scientifically not credible without providing any

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supporting evidence in the NRC report. I will revisit this issue in more detail in my comments below while providing case-specific public domain data and public domain information which is at odds with the path taken and the critique provided in the NRC report (see response B-2).

2. **Comment on p. 33:** *At a typical waste site, spent VOCs are present in the unsaturated zone (a partially saturated soil layer above the water table) in the form of dense nonaqueous-phase liquids (DNAPLs)*..... (after a lengthy discussion of what DNAPL is and how DNAPL-based contaminants behave in the subsurface and what the consequences of such a source are, the NRC report continues in this section with the following remarks linking DNAPL presence to the aquifers at Camp Lejeune.) ..... *The presence of low-permeability units (such as the Castle Hayne confining unit or any clay units) would limit vertical migration of both DNAPL and dissolved contaminants*.....

   Response: The NRC report does not provide any information for the **justification** of this conceptualization of the contamination source at the ABC One-Hour Cleaners site and Tarawa Terrace and vicinity other than providing a reference to a source concentration of 12,000 μg/L, reported in Chapter E of the ATSDR Tarawa Terrace report series (Faye and Green 2007, p. 38). This is followed by a reference to a number of 110,000 μg/L (p. 38 of the NRC report, second paragraph from bottom of page). As indicated in the NRC report, this is the highest possible concentration of tetrachloroethylene (PCE) in water. Because this reference number is given in the NRC report **without a reference citation, I question the credibility of this reference number. The NRC report also does not discuss the importance of this number in their conceptualization of the contaminant source as a DNAPL although they provide an extensive discussion of what DNAPL is**. Furthermore, the NRC report does not refer to a data source on the solubility levels of PCE in water like those data sources reported in Chapter D of the ATSDR Tarawa Terrace report series (Lawrence 2007) **that they are reviewing**. The NRC report does not refer to or cite a database that may exist in USMC files at Camp Lejeune, unknown to the ATSDR water-modeling team, that NRC committee members may have had access to that would indicate the presence of DNAPL-phase PCE at the site. The NRC report also does not refer to a systematic dry-cleaner disposal procedure that is reported in the documents they have reviewed for handling the disposal of the chemical PCE as a pure-phase PCE at the ABC One-Hour Cleaners site. The NRC report does not also refer to current remediation efforts at the site which may justify this characterization. This last item is a very important point which would refute this incorrect characterization based on the remediation technology adopted at the site by USEPA, North Carolina authorities who are overseeing these remediation efforts, and DON and USMC consultants who are implementing these remediation efforts (see response B-3).

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In the NRC report, the highest concentration of dissolved PCE, 110,000 μg/L, must imply the NRC committee understanding of the solubility level of PCE in water. Because a reference is not provided, I could not confirm this number and question its credibility. Our references indicate that the solubility of PCE in water is around 200,000 μg/L (= 200 mg/L) at 15°C or higher. In Chapter D of the ATSDR Tarawa Terrace report series (Lawrence 2007, p. D12, Table D9), solubility of PCE is reported to be 210, 000 μg/L (=210 mg/L) at 25°C, which is the solubility number I would like to work with for my analysis below. There are other references in the literature that report the solubility of PCE at much higher concentrations as well, which will not be referenced here. This is because I would like to focus on what is reported in the ATSDR Tarawa Terrace series of reports.

The 12,000 μg/L concentration reported in NRC report (and also in Chapter E of the ATSDR Tarawa Terrace report series [Faye and Green 2007]) as a justification for the presence of a DNAPL phase is about 5.7% to 6% of the solubility level of PCE (12,000/200,000 = 6% or 12,000/210,000 = 5.7%). The 12,000 μg/L concentration is the dissolved-phase PCE concentration in the groundwater at ABC One-Hour Cleaners as reported by ATSDR (Faye and Green 2007). Although this is a high concentration, this value is much less than PCE's solubility limit in water (200,000 μg/L at 15°C or 210,000 μg/L at 25°C). The location of the highest concentration sample within Tarawa Terrace and vicinity can be used to identify the source location at the site. High concentrations at a site may suggest the possibility of non-aqueous phase (NAPL) PCE (PCE in form of NAPL) presence but this does not guarantee a NAPL presence at the site, because in this case, 12,000 μg/L is 6% or less of the solubility limit of PCE.

Thus, the conceptual DNAPL contaminant source characterization that is provided in the NRC report without any justification and without any field data support is both extremely bothersome and irresponsible. This reference to the presence of a DNAPL-phase contaminant source at the site not only appears in the aforementioned comment on NRC report page 33, but it is repeatedly referred to in other pages and sections of the NRC report which is an attempt to discredit the ATSDR analysis and its findings from its source conceptualization origins. As a member of the ATSDR water-modeling team, I respectfully request that, through ATSDR, I should be provided with the recorded field data evidence that the NRC committee was privy to that would support the DNAPL conceptualization. Also reporting the solubility of PCE in water at about **half the value of the data reported in the ATSDR Chapter D report** (Lawrence 2007) **without providing a reference** (page 38 of the NRC report) is a scientifically unacceptable practice. Short of citing field data evidence and an appropriate reference for the solubility level of PCE as reported in the NRC report, I would question the scientific basis of the complete NRC report that relies on the accuracy of this erroneous conceptualization. Without field data evidence, the NRC review is based on hypothetical conditions and

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assumptions that are extracted from the scientific work of others (Figure 2-3 of the NRC report) which is based on studies that are conducted at other sites—and these sites have no relevance to the ABC One-Hour Cleaners site or Tarawa Terrace and vicinity. The purpose of this assertion (PCE DNAPL source conceptualization) and misrepresentation of data and site-specific conditions by the NRC committee is not clear to me.

During the NRC committee review process, the question of the characterization of the source was brought to the attention of ATSDR water-modeling team members in a request for information by an NRC committee member (Email communication from P. Clement to M.L. Maslia, ATSDR, May 5-11, 2008). During that time, ATSDR water-modeling team members provided the NRC with data ATSDR had on the subject matter clearly showing why we selected to simulate the PCE source as a dissolved-phase source. Furthermore, we clearly identified why the dissolved-phase injection procedure applied in the models used for the ATSDR water-modeling analyses. The information that was provided to the NRC was based on data from several remedial investigation reports, site reports, and other DON and USMC files (Shiver 1985, Roy F. Weston 1992, 1994). In these field study reports, there is no recorded data reported by DON and USMC consultants that would provide evidence of, or substantiate the existence of, the presence of a DNAPL source at ABC One-Hour Cleaners or Tarawa Terrace (see also my comment B-3). If the DNAPL source conceptualization that appears in the NRC report is based solely on the data source and information we provided to the NRC committee, then I do not agree with the NRC's source characterization conclusion. I, therefore, consider this to be a misrepresentation of the conditions at the site. If this conceptualization is based on any other information or data that we are not aware of, and if this information was provided to NRC by DON, the USMC, or their consultants, we need to be provided with that information and data. Because the reference to a DNAPL-phase in the aquifers underlying ABC One-Hour Cleansers and Tarawa Terrace and vicinity appears in several places within the NRC report, I will revisit this topic again in my discussion below.

In the aforementioned statement on page 33 of the NRC report, I also noticed that the NRC committee acknowledged that the PCE source was discharged to the unsaturated zone of the aquifer underlying ABC One-Hour Cleaners and Tarawa Terrace and vicinity. However, given that observation, the NRC committee fails to provide a justifiable critique of the use of the MODFLOW family of codes that only considers a saturated groundwater zone to analyze the physical problem at the site. On the contrary, the NRC committee considers the MODFLOW family of codes to be an acceptable modeling choice. This is probably because the NRC committee considers these MODFLOW family codes as accepted state-of-the-art tools for typical groundwater pathway modeling. This is a perfect example of a typical case of fitting a physical problem to a code "concept" I referenced in my response statement "A-1" above, which the ATSDR water-modeling team tried to avoid as much as possible.

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In recognition of this problem and also in recognition of the general perception that prevails in the scientific community that the MODFLOW family of codes is an accepted procedure, the ATSDR water-modeling team first utilized the MODFLOW and MT3DMS codes in their simulations. In addition, to enhance our understanding of conditions at the site, ATSDR has and extended their analyses. The ATSDR water-modeling team applied the TechFLOW^{MP} software to understand and evaluate the unsaturated zone injection conditions that are implemented at the site. TechFLOW^{MP} is a public domain code that can be accessed from the Georgia Tech website for individual use without a fee (http://mesl.ce.gatech.edu/). The NRC report attempts to discredit this extra effort and the steps taken by the ATSDR water-modeling team to simulate the proper source disposal conditions at the ABC One-Hour Cleaners site by classifying: (i) the TechFLOW^{MP} code as a research tool; and, (ii) a proprietary code that is not verified. Again, this is very puzzling and a misrepresentation of the scientific and public domain facts of this case by the NRC committee. These NRC statements that appear in several places in the NRC report ignore a scientifically sound attempt by the ATSDR water-modeling team to properly solve a physical problem, above and beyond a traditional MODFLOW and MT3DMS application which the NRC review committee accepts (NRC 2009, p. 43). Additionally, these NRC statements misrepresent the public domain information of the status of a model used in the analysis. NRC committee remarks in this regard misrepresent a public domain code as a proprietary code without checking with the authors of the code or the web site where this code can be accessed freely by anybody without a fee. Further, the NRC committee also failed to check the current technical literature and peer reviewed scientific publications containing substantial evidence of publications involving the TechFLOW^{MP}. Contained in this technical literature and scientific publications is evidence where the TechFLOW^{MP} code has been tested and verified against other applications that appeared in the literature. (see web site : http://mesl.ce.gatech.edu/PUBLICATIONS/Publications.html) This lack of due diligence by the NRC committee is also very puzzling.

I am very familiar with the expertise of the scientists who prepared the NRC report, many of whom I know personally and respect. What I do not understand is how they reached these puzzling and in some cases erroneous conclusions, which are not justified in the NRC report they prepared. Misrepresentation of these scientific and public domain facts is extremely bothersome and, in my opinion, sheds a dark cloud over the scientific credibility and integrity of the overall NRC report.

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B. REVIEW COMMENTS ASSOCIATED WITH SCIENCE-BASED TOOLS, ANALYSES, AND INTERPRETATION OF STUDY RESULTS:

1. **Comment on p. 43:** *For example, MT3DMS can predict the transport only of dissolved contaminants, so a key approximation was made to represent the mass dissolved from the DNAPL source. To apply MT3DMS, ATSDR replaced the highly complex DNAPL contaminated source zone with a hypothetical model node where PCE was injected directly into the saturated aquifer formation at a constant rate (1.2 kg/day).*

   Response: This NRC report statement relies on their unsubstantiated and undocumented source characterization concept (see my review comment "A-2"). Using this conceptualization as an undisputable fact, the NRC committee then attempts to discredit the groundwater-modeling study conducted by ATSDR at the ABC One-Hour Dry Cleansers site and Tarawa Terrace and vicinity. This statement is a hyperbole, wherein first an "assumption" is made and then that "assumption" is considered to be a "fact" to critique the findings of a study. This approach in a critique does not even deserve a scientific response; and again, brings about more questions as to the scientific credibility and integrity of the NRC report.

2. **Comment on p. 43:** *Unlike the MODFLOW and MT3DMS codes, the PSOpS and TechFlowMP codes lack validation by a broad spectrum of practicing geoscientists in an open-source environment.*

   Response: I have addressed the path the NRC committee chose in reference to the misrepresentation of TechFLOW^{MP} as a proprietary code in my aforementioned response A-2. I will not repeat that here again. In reference to the PSOpS model developed by the Georgia Tech group, I would like to enquire of the NRC committee the following: Can a reference to a public domain code be provided by the NRC that is available through the published literature which would substitute for the PSOpS application? Has such a public domain code been developed for, and applied to, any study that they are aware of to manage pumping-schedule operations in an optimal manner for a complex system such as the one at Tarawa Terrace? The answer to these questions is obvious and the answer is: "This type of public domain model does not exist."

   PSOpS is an optimization application that was developed by the Georgia Tech group participating in the ATSDR water-modeling analysis to yield answers to specialized uncertainty-related questions pertinent to the current health study at Camp Lejeune. The analysis is based on the MODFLOW family of codes in the generation of the database used to solve an optimization problem. The development of this optimization model was necessary to respond to scientific questions raised by the ATSDR Expert Panel (March 2005) whose members guided our study and contributed significantly to its quality. The

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members of this ATSDR Expert Panel are well known and respected scientists in the field and their names are listed in the Expert Panel report (Maslia 2005) that is also available on the ATSDR website. The question ATSDR Expert Panel members raised in this case was related to the uncertainty of a pumping-schedule operation that may be implemented at the site and the characterization of its effects on the study outcome. The PSOpS model that was developed for the purposes of this analysis and used in the ATSDR water-modeling analyses to address this question became part of the PhD thesis of a graduate student at Georgia Tech. In that sense, the theoretical background of the model is reviewed and accepted by an independent PhD thesis committee at Georgia Tech and the detailed documentation of this model can be found in the PhD thesis of Dr. J. Wang, which is public domain information (Wang 2008)

In conclusion, the NRC committee is most likely aware of the following: (1) specialized models such as PSOpS are not available in the technical public-domain literature; and, (2) codes such as PSOps are developed for the specialized purposes of the current study to find answers to specialized questions that are raised by the current water-modeling analysis. The concept of using an optimization algorithm that is fed by a database through the MODFLOW family of models, which is a common and routine procedure, is both scientifically sound and scientifically necessary in a study such as the one ATSDR is conducting at Camp Lejeune.

I also provide a paragraph below that is extracted from an USEPA report (USEPA 2009) that indicates that NRC opinion in this case is also at odds with the USEPA recommendations on the use of proprietary or research codes when necessary:

"This guidance defines proprietary models as those computer models for which the source code is not universally shared. To promote the transparency with which decisions are made, EPA prefers using non-proprietary models when available. However, the Agency acknowledges there will be times when the use of proprietary models provides the most reliable and best-accepted characterization of a system."

If the NRC committee can provide us with a reference to another public domain model that can be used for our study and that would serve the same purpose, instead of the PSOpS model, we would be glad to use that model instead of the PSOpS model. To my knowledge, such a model is not available. In my opinion, the NRC committee also should recognize that the ATSDR water-modeling effort is not a run-of-the-mill work product and the problem at hand is not a routine problem that can be or should be analyzed using only routine models. In such cases it is expected that specialized methods can be developed and implemented—this should not be shunned by the NRC, but instead, it should be applauded (USEPA 2009). It is most puzzling to see, that under the name

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"NRC," this approach is not encouraged, but instead, it is criticized when USEPA is recommending the use of these procedures.

3. **Comment on p. 44:** *The DNAPL source zone was represented by using a model node where PCE was injected continuously into the unconfined model layer-1 of the saturated zone at a constant rate of 1.2 kg/day (Faye 2008).*

Response: Again, in this statement, the NRC committee is asserting that the DNAPL source zone was misrepresented in the current study. I refer the reader to my previous comments in my response A-2 in reference to the DNAPL source mischaracterization by the NRC committee.

To reiterate, we have not represented a DNAPL source zone as an injection point in our models because according to our understanding of the site conditions there is **no DNAPL source zone** in the aquifer underlying the ABC One-Hour Dry Cleaners site and Tarawa Terrace and vicinity. If the claim of the NRC committee can be substantiated by any field data, not only we will modify our modeling study efforts, **but also we would strongly recommend that the U.S. Environmental Protection Agency (USEPA), their consultants, and the North Carolina Department of Environment and Natural Resources (NCDENR) should immediately abandon their current remediation efforts at the ABC One-Hour Dry Cleaners site and Tarawa Terrace and vicinity and adopt remediation strategies that would yield more effective results for a DNAPL source contaminant.** The remediation technology currently used at the site is a pump-and-treat system which is not effective in DNAPL remediation. There are other remediation techniques that would be more effective in remediating DNAPL conditions. USEPA and NCDENR field consultants who are currently not implementing these DNAPL remediation technologies at the site is additional evidence that these agencies and their consultants also does **not** agree with the NRC committee as to the characterization of the contamination source as DNAPL-phase PCE.

4. **Comment on p. 48:** *Because insufficient historical pumping data were available to constrain the model predictions from 1953 to 1980, the ability of the advanced optimization models to estimate the dates* ***accurately*** *is questionable.*

Response: There are obvious uncertainties in the physical problem being studied at ABC One-Hour Dry Cleaners and Tarawa Terrace and vicinity. The NRC committee would most likely agree with this statement. If we accept this statement, then the question becomes, should one completely ignore uncertainty effects in the analysis or, should one try to develop techniques that would provide an estimate of the effects of the uncertainty on the solution in a systematic way? We have chosen the second route.

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The NRC committee should accept the fact that answers to uncertainty questions cannot be answered "**accurately**" as the report states in the above statement. Expecting that from an uncertainty analysis outcome would be **scientifically irresponsible**. Our uncertainty analyses are not provided to give "**accurate**" answers to the problem studied. Instead, our uncertainty analyses are used as estimates that would indicate the variability range of the deterministic results provided earlier. The domain of uncertainty analysis is a scientific field which is not in the realm of the traditional groundwater fate and transport analysis expertise and should be viewed using a different microscope and expertise.

5. **Comment on p. 48:** *(5) there is no spatial variation in the microbiologic or geochemical characteristics.*

Response: The NRC committee correctly identified that in the application of the TechFLOW^{MP} model to the aquifers underlying the ABC One-Hour Dry Cleaners site and Tarawa Terrace and vicinity, we assumed no spatial variation of microbiologic characteristics. If the NRC committee is familiar with the finite element procedures used in the TechFLOW^{MP} model, they would acknowledge that this is not a restriction of the model but a restriction of the available field data for the site. If the microbial distribution in an aquifer can be accurately characterized, which we doubt can be accomplished in this case or any case, we can certainly include that heterogeneity in our modeling effort.

Having pointed out this fact, I would also like to question issues pertaining to levels of acceptable homogeneity considered in our modeling effort and compare it with levels of unacceptable homogeneity that are shunned in our modeling analysis based on the critique presented in the NRC report. For example the assumption of uniform infiltration across the model domain when the MODFLOW family of model codes is utilized was not critiqued in the NRC report, but the assumption of uniform microbial distribution in the multilayer aquifer domain is critiqued. Between these two processes, which would be the easier process to characterize? The distribution of microbial colonies in the multilayer aquifers of Camp Lejeune, or infiltration due to rainfall events on the top aquifer layer. I think the answer to this question is obvious—based on the available data—the infiltration process would be easier to consider as a distributed process. Thus, although both processes are characterized by heterogeneity in the aquifer, accepting the homogeneity assumption for the infiltration case but not accepting homogeneity assumption for the microbial distribution case would be setting the bar too high and would be scientifically irresponsible considering the levels of data that may be available to characterize either process. A scientific review committee should be able to make these distinctions easily and come up with appropriate conclusions in their prepared review comments.

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6. **Comment on p. 49:** *However, there are some important limitations in ATSDR's modeling efforts because of the sparse set of water quality measurements, the need to make unverifiable assumptions, and the complex nature of the PCE source contamination.*

   Response: There are limitations of the modeling analyses conducted by ATSDR water-modeling team. We would be the first to acknowledge these limitations. This is evident by the level of detail of the uncertainty analysis conducted as part of the water-modeling analysis to envelope the effect of those uncertainties on the outcome presented. However, in my opinion, characterizing the uncertainty analysis outcome as not "**accurate**" as previously stated (see response B-4) or, that uncertainty analysis only should be conducted in "**verifiable**" cases as stated above is not a scientifically sound assessment or procedure. **An uncertainty that can be verified is no longer uncertain.**

7. **Comment on p. 49 first bullet:** *The effects of the DNAPL in both unsaturated and saturated zones have not been included in the studies.*

   Response: The NRC report brings back the DNAPL issue here again. Please see my response in A-2 and other comments above.

8. **Comment on p. 49 second bullet:** *Constant values of dispersivity (longitudinal dispersivity of 25 ft and transverse 2.5 ft) were used in the transport model.*

   Response: Although dispersivity is considered to be constant, based on the definition of the hydrodynamic diffusion coefficient, the hydrodynamic diffusion coefficients are variable because they depend on the velocity field at the site. This is a common assumption in most studies where field data are not available to support spatially variable dispersion coefficients. This comment again is related to my discussion of acceptable homogeneity and unacceptable homogeneity conditions at a site study (see my response B-5).

9. **Comment on p. 49 bullet four:** *The numerical codes TechFLowMP and PSOpS used in the modeling are research tools and are not widely accepted public-domain codes, such as MODFLOW and MT3DMS, so their validation is important.*

   Response: This characterization is a misrepresentation of the aforementioned models, clearly identified in my response A-2. As the NRC committee may acknowledge, the availability of codes with the capabilities of these models are very limited (see my response A-2 and B-2). In my opinion the use of these models in complex analysis should not be shunned by NRC, but instead, it should be encouraged since they are providing

supplemental information beyond MODFLOW family of code applications (see my response in B-2) (USEPA 2009).

10. **Comment on p. 49 bullet five:** *The PSOpS modeling study is based on the premise that an optimization model can be used to evaluate pumping stresses. Without site-specific pumping and water-quality data, the results will be nonunique and uncertain.*

Response: PSOpS modeling concept is based on the effort of estimating the effects of uncertainty on the modeling outcome. This analysis is approached in a systematic manner following a well accepted process such as an optimization analysis based on some constraints to satisfy the demands. The PSOpS model uses the MODFLOW family of codes as its database engine. We are not claiming that the outcome provides the exact conditions representing the problem at the site. But the outcome of the analysis provides us with an envelope which bounds our deterministic analysis. This is a standard uncertainty analysis procedure similar to, for example, Monte Carlo analysis that is routinely used in uncertainty analysis. Monte Carlo analysis, according to a well established procedure, systematically evaluates the effects of uncertainty on the problem solution. In such an application, it is not **certain** that the random numbers generated would **exactly** represent the actual conditions for the problem at the site. However, the bounding limits of the analysis are the ultimate goal. The application of PSOpS, in essence, is very similar to that analogy.

As I have stated earlier, this goes back to the NRC report statement about the "**accuracy**" of the uncertainty analysis results that cannot be justified scientifically. Please see my response in B-4. Also, I have to emphasize again what I stated earlier: The domain of uncertainty analysis is a scientific field which is not in the realm of the traditional groundwater fate and transport analysis expertise and should be viewed using a different microscope and expertise.

11. **Comment on p. 49 bullet seven:** *The TechFlowMP model predicted very high vapor concentrations. For example, TechFlowMP predicted that the PCE vapor concentration in the top 10 ft of soil beneath the Tarawa Terrace elementary school should be 1,418 µg/L. Studies of PCE vapor concentrations in buildings that house or are near a dry-cleaning facility have reported measured concentrations around 55 µg/L.*

Response: This reference to a vapor concentration at 1,418 µg/L is another example of **misrepresentation** of the results of the modeling analyses by the ATSDR water-modeling team. This aforementioned information was taken from Chapter A of the ATSDR Tarawa Terrace report series (Maslia et al. 2007, p. A44). The statement provided in the ATSDR report reads as follows:

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"b. the maximum simulated PCE concentration in groundwater (model layer 1) at the Tarawa Terrace elementary school was 1,418 µg/L (Figure A15b), whereas the maximum simulated vapor-phase PCE (in the top 10 ft of soil) was 137 µg/L (Figure A20a)"

The above sentence, taken directly from the ATSDR report submitted to NRC, clearly states that the groundwater (not vapor) concentration of PCE in layer "1" is 1,418 µg/L concentration. Vapor concentration is given separately in the same paragraph towards the end of that sentence. For the NRC report to represent this number (1,418 µg/L) as the vapor concentration that is simulated at the site in order to discredit a study does not fit to any norm of a scientific review. I will provide a more detailed analysis of this case using simulation results to bring clarity to the concern raised in the NRC report.

In this case, the work product referred to are the TechFLOW^{MP} modeling results and the particular analysis mentioned was conducted by the Georgia Tech group participating in the ATSDR water-modeling analysis of the ABC One-Hour Cleaners site and Tarawa Terrace and vicinity (Jang and Aral 2007). In order to provide the reader with clear evidence of scientific **misrepresentation** of the facts which seem to appear too frequently in the NRC report, the actual data reported in our report is presented below in sufficient detail—unlike the other responses I have provided to other comments in this document.

In the numerical study of the multispecies, multiphase groundwater contamination at ABC One-Hour Dry Cleaners and Tarawa Terrace and vicinity, TechFLOW^{MP} simulations used two boundary-conditions to characterize the ground surface under the original pumping schedule: (1) GSBC = 0.01 and (2) GSBC = 1.0 (Jang and Aral 2007, p. G15). Here the acronym "GSBC" stands for the Ground Surface Boundary Condition. For the in-/out-flux of gas between the atmosphere and the unsaturated zone, if the ground surface does not have low-permeable zones or hindrances due to pavement, lakes, or buildings, the GSBC value is set to be 1.0. This implies that the soil gas can be freely released into the atmosphere from the unsaturated zone. However, when some objects, including roads, buildings, ponds, or highly water-saturated areas, are present at the ground surface, the soil gas can not be released into the atmosphere freely. Under such a condition, GSBC is set to be 0.01 in the current study. Actually any number between these two extremes can be considered in the analysis. However, just to show the bounds of the results, the discussion here will be confined to these two extreme cases.

In order to analyze the concentration distribution around the school area as it is referred to in the aforementioned NRC report comment, the location of the school at Tarawa Terrace has to be identified, and it shown in Figure 1.

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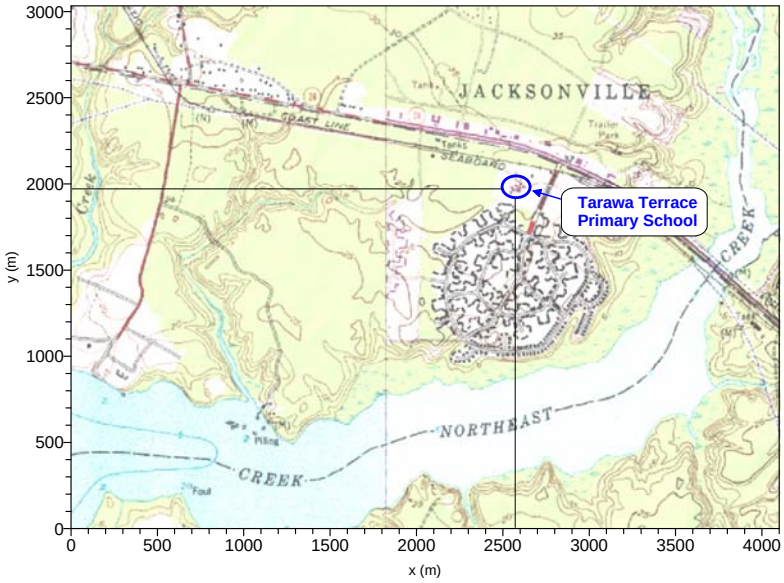

Figure 1. Location of the Tarawa Terrace Elementary School

In the school area, the groundwater table is near the ground surface (CH2MHILL 2007). In this study, the ground surface is at z = 7.6 meters (m, z = 25 ft), and the groundwater table is around z = 2.4 – 4 m (z = 8 – 13 ft) (Jang and Aral 2007, Figure G3, p. G10). Thus, the concentration distributions of the vaporized PCE at z = 6 m are presented below, where the unsaturated zone is at this location.

As shown in Figure 2, under GSBC = 0.01, which is more representative of an area where there are buildings and pavements, the predicted vaporized PCE concentrations in the pore space of the soil at the center of the school area (x = 2,580 m, y = 1,975 m) are about 15.5 µg/L during December 1984 (Figure 2a) and 3.7 µg/L during December 1994 (Figure 2b). Within the school area (marked with the circle in this figure), the PCE concentration ranges 0.1-100 µg/L during December 1984 (Figure 2a) and 0.1-50 µg/L during December 1994 (Figure 2b).

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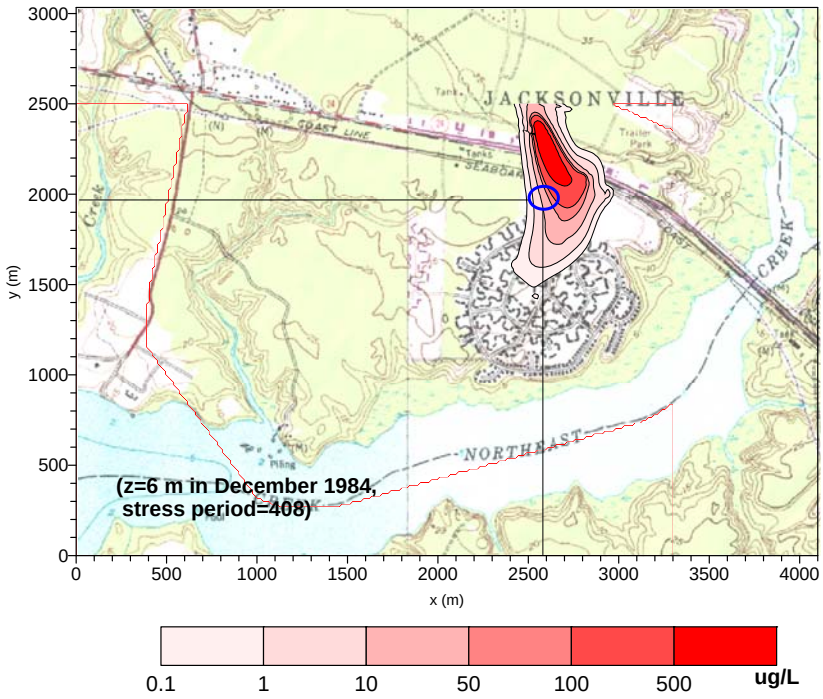

Figure 2a. Vaporized PCE concentrations in the gas phase under the original pumping schedule (PS-O) with GSBC=0.01, at z=6, December 1984.

In Figure 2, the vaporized PCE concentrations near the ABC One-Hour Cleaners site are very high where the contamination source is located. This is expected, but the vapor concentrations decrease sharply with the distance away from the ABC One-Hours Cleaners site. Furthermore, the simulated concentration of PCE in the gas phase, ranging from 0.1 to 100 µg/L, is not significantly different from the value of 55 µg/L, given in the NRC report.

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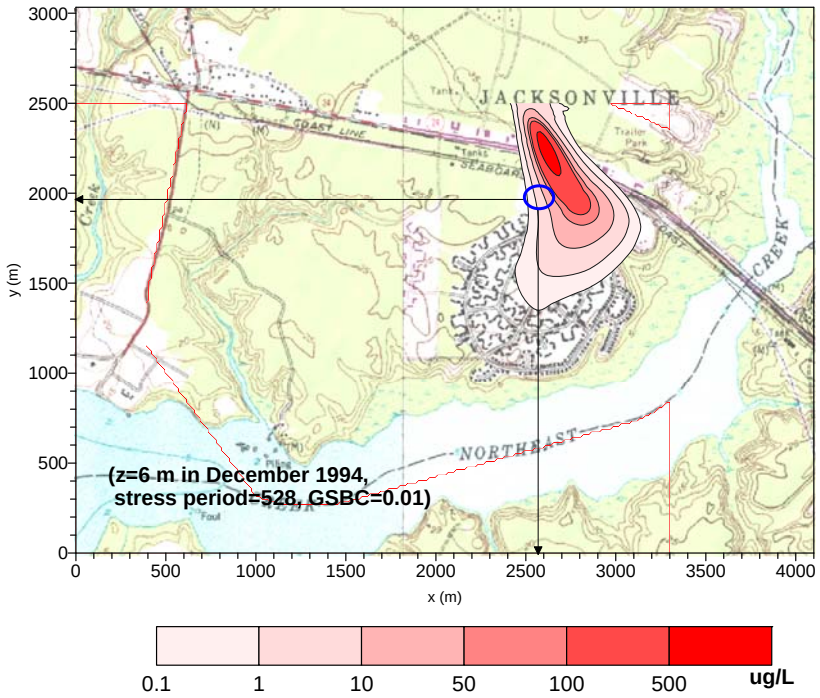

Figure 2b. Vaporized PCE concentrations in the gas phase under the original pumping schedule (PS-O) with GSBC=0.01, at z=6, December 1994.

Having provided this comparison, **I also question the source of the reference number, 55 µg/L, that is used in the NRC report**. The NRC report provides a reference in this case and this reference is McDermott et al. (2005). I was curious about this reference; therefore, I located and obtained a copy of the referenced paper. In the McDermott et al. (2005) study, the authors are analyzing and reporting data on the PCE vapor concentrations in a building where dry-cleaner operations are housed in New York City. Does the NRC committee expect us to accept the concept that what is observed (measured) as vapor concentration in a building that houses a dry-cleaner facility in New York City should also apply to the pore space of the soils at the site of an elementary school area in Camp Lejeune, North Carolina? Or do they expect that what we have simulated in the pore space of the soils at a site in North Carolina should also confirm the observations made in New York City, 17–20 years beyond our final simulation date (2001–2003), in some dry-cleaner facility building? **In my opinion, these types of comparisons, expectations, and assertions are scientifically not acceptable and credible; they discredit the NRC report in its entirety.**

In the groundwater contamination study that utilized TechFlow^{MP} (Jang and Aral 2007), the local equilibrium of contaminant partitioning between the water and gas phases is

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implemented while calculating the contaminant distribution between the two phases (gas and liquid). Thus, we can use the Henry coefficient, *H*, in estimating PCE concentration in the gas phase from the concentration in the groundwater phase as follows:

$$C_{Vapor,PCE} = HC_{GroundWater,PCE}$$

For PCE, *H* is 0.35 (Jang and Aral 2007, Table G2). Using the dissolved PCE concentration in the groundwater shown in Figure G5 of Jang and Aral (2007) (in the unsaturated and saturated zones), the overall concentration distribution of the vaporized PCE within the gas phase in the unsaturated zone can also be estimated. This simple calculation could have been done by the NRC committee to confirm the vapor concentration numbers they are reporting in their statement. In Figure G5 of Jang and Aral (2007), the dissolved PCE concentration in the groundwater is 100-500 µg/L near the ground surface at the location of the elementary school (*x = 2,580* m, *y = 1,975* m). Therefore, the vaporized PCE concentration will be approximately 35-175 µg/L in the unsaturated zone near the school area. The cross section line A-A' in Figure G5 is located at *x* = 2,606 m.

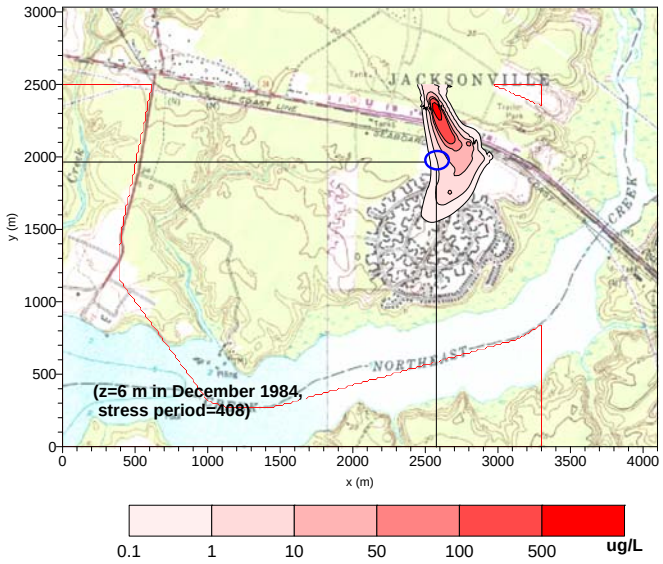

Figure 3a. Vaporized PCE concentrations in the gas phase under the original pumping schedule (PS-O) with GSBC=0.01, z=6, December 1984.

Let us also analyze the results of the other boundary condition that is used in the TechFLOW^{MP} model out of curiosity and see if the vapor concentration value of 1,418 µg/L reported in the NRC report was referring to that case. The results reported in (Jang

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and Aral 2007) under the condition GSBC = 1 are shown in Figure 3. The predicted vaporized PCE concentrations at the center of the school area ($x = 2580$ m, $y = 1975$m) are about 0.99 during December 1984 (Figure 3a) and 0.1 µg/L during December 1994 (Figure 3b) (i.e. more PCE vapor is released to the atmosphere and less is remaining in the pore space when compared to the previous results). Within the school area (marked with the circle in the figure), the concentration ranges 0.1-10 µg/L in December 1984 (Figure 3a) and less than 5 µg/L in December 1994 (Figure 3b).

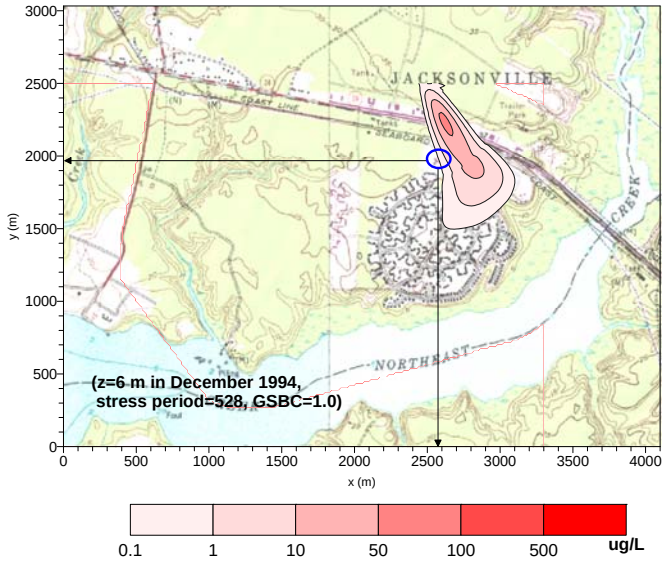

Figure 3b. Vaporized PCE concentrations in the gas phase under the original pumping schedule (PS-O) with GSBC=0.01, z=6, December 1994.

As can be seen from these results the number reported in the NRC report does not exist in the ATSDR water-modeling analysis as vapor concentration. This is a clear misrepresentation of the ATSDR water-modeling results. The purpose of this misrepresentation is not clear to us.

The field investigation during 2007 (CH2MHILL 2007) it was reported that the vaporized concentrations of PCE near the ground surface were below detection limits or very low, 3.9 ppbv (parts per billion volume), which is equivalent to 0.028 µg/L. Considering the time gap between the end of the historical simulation time (December 1994) and the field investigation time (July 2007), the simulation results that are provided in the Chapter G report of the ATSDR Tarawa Terrace report series (Jang and Aral 2007) provide reasonable modeling results and represent acceptable levels of expected vapor concentration near the Tarawa Terrace elementary school. Are we asserting that this is absolutely the case? The answer to that question is absolutely "No." This outcome is only

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an estimate based on the assumptions and limitations of the models considered in the ATSDR water-modeling analyses and the assumptions and limitations are based on our best judgment of the conditions that may exist at the ABC One-Hour Dry Cleaners site and Tarawa Terrace and vicinity.

The ATSDR water-modeling reports do not report such high concentration of vaporized PCE concentration in the gas phase. The vaporized PCE concentration of 1,418 μg/L is equivalent to a dissolved PCE concentration of 4,051 μg/L, in the groundwater:

$$C_{Vapor,PCE} = HC_{GroundWater,PCE}$$
$$C_{GroundWater,PCE} = 1418 / 0.35 = 4051.4$$

I also note that the unsaturated zone is located at a very thin layer near the ground surface (z = 7.6 m (25 ft)) in Jang and Aral (2007, Figure G5) which is characterized in terms of several layers in water-modeling analysis. The maximum thickness of the unsaturated zone is about 7.6 m.

In conclusion the data, the associated discussion of the vapor levels near the Tarawa Terrace elementary school area, and also the reference provided in the NRC report (McDermott et al. 2005) are far from the facts of the case and the results that are presented by the ATSDR water-modeling team. Again I see here a misrepresentation of the data reported in a study to discredit a study. The purpose of that approach is not clear to me. However, I can declare without hesitation that this approach does not have any scientific credibility and place in any scientific document.

12. **Comment on p. 49 bullet eight:** *The biodegradation model used within the TechFlowMP code is based on an untested preliminary research model.* and also,
Comment on p. 50: *The TechFlowMP simulations assumed that the biodegradation byproduct of TCE is trans-1,2-DCE. However, the scientific literature indicates that cis-1,2-DCE is the predominant product of TCE reduction under in situ groundwater conditions.*

Response: The detailed description of "why *trans*-1,2-dichloroethylene is chosen as the representative byproduct of TCE bioreaction at the Tarawa Terrace area instead of *cis*-1,2-DCE" is given in page G4 of the report, Chapter G (Jang and Aral 2007). Additional explanation regarding this issue is given below.

As shown in Figure G2 of the report (Jang and Aral 2007), the anaerobic biological degradation of trichloroethylene (TCE) generates three isomers, *cis*-1,2-dichloroethylene (*cis*-1,2-DCE), *trans*-1,2-dichloroethylene (*trans*-1,2-DCE), and 1,1-dichloroethylene (1,1-DCE). As discussed in the report (Jang and Aral 2007), *cis*-1,2-DCE (1,2-cDCE) is

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the most common byproduct among the three DCE isomers produced theoretically (Wiedemeier 1998). Even though *cis*-1,2-DCE has been often used as a primary byproduct of TCE-biodegradation under the anaerobic conditions in contaminant-transport modeling of chlorinated ethenes (Clement et al. 2000; Jang and Aral 2008), but the primary byproduct of the TCE bioreaction highly depends on the chemical-biological conditions (especially, microorganisms and nutrients) at the contaminated sites (Bradley 2003), implying that the biological reaction of TCE is highly site-specific. For example, Christiansen et al. (1997) and Miller et al. (2005) reported the anaerobic biological degradation of TCE produced more *trans*-1,2-DCE than *cis*-1,2-DCE. At the TCE-contaminated site in Key West, Florida, the ratio of *trans*-1,2-DCE to *cis*-1,2-DCE was greater than 2 (SWMU9 2002). Griffin (2004) reported that the ratio could reach up to 3.5, based on field data for several sites, including Tahquamenon River, MI; Red Cedar River, MI; Pine River, MI; and Perfume River, Vietnam.

In the modeling of contaminant transport at a contaminated site, the field measurement data at the site are very important in validating the numerical models and in obtaining more accurate simulation results. For the numerical study at the Tarawa Terrace area, we had limited field data regarding the concentrations of PCE, TCE, and *trans*-1,2-DCE. This is indicated in the following statement of the ATSDR report: Review of degradation byproduct data analyses, provided to ATSDR by the Department of the Navy, U.S. Marine Crops, the North Carolina Department of Environment and Natural Resources, and others indicated that the predominant degradation byproduct of TCE at Tarawa Terrace and vicinity was *trans*-1,2-DCE (Faye and Green 2007, Tables E2 and E7).

As mentioned above, since the primary byproduct of the biological degradation of TCE depends on site-specific conditions, it is more reasonable to select *trans*-1,2-DCE instead of *cis*-1,2-DCE as a primary TCE-bioreaction-byproduct in the study on the groundwater contamination at the Tarawa Terrace area.

The NRC critique, therefore, ignores site-specific TCE degradation byproduct data pertinent to Tarawa Terrace and vicinity, listed in Chapter E of the Tarawa Terrace report series. This statement again clearly demonstrates the lack of due diligence by the NRC review committee in their review of the data that exists at the Tarawa Terrace, Camp Lejeune site and their lack of understanding of the facts of the site specific case based on this data. This is very bothersome.

13. **Comment on p. 50 next to last bullet:** *In the absence of data, historical reconstruction efforts that use groundwater models can only provide a general conceptual framework for what happened at the site and why.*

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Response: Historical reconstruction is a procedure that is accepted in the literature. It uses models to predict the past in a conceptually similar manner the models are routinely used to predict the future in engineering studies. The references to these studies are Hanford study, Toms River study, Woburn study and the like.

14. **Comment on p. 65:** *Therefore, the committee recommends the use of simpler approaches (such as analytic models, average estimates based on monitoring data, mass-balance calculations, and conceptually simpler MODFLOW/MT3DMS models) that use available data to rapidly reconstruct and characterize the historical contamination of the Hadnot Point water-supply system. Simpler approaches may yield the same kind of uncertain results as complex models but are a better alternative because they can be performed more quickly and with relatively less resources, which would help to speed-up the decision-making process.*

    Response: Use of simpler models may be easier to implement. We are already proceeding in that direction for the Hadnot point study. However, how the detailed questions that are raised in the NRC report could be answered using simpler models is not clear to me.

CONCLUSIONS:

Examples of the scientific evidence presented in this response statement and the discussion of this evidence herein clearly indicate that the data and the analysis presented in the NRC report (NRC 2009) are misrepresentations and mischaracterizations of the findings of the ATSDR water-modeling analyses conducted at the ABC One-Hour Cleaners site and Tarawa Terrace and vicinity. The conceptual characterizations made by the NRC committee also do not fit available field data or reported field conditions by the USEPA, their consultants, or the NCDENR which are guiding current remediation efforts at ABC One-Hour Cleaners and Tarawa Terrace and vicinity. Thus, what is in question here is the credibility of the complete contents of the NRC report as a scientific document of any value.

As I have said earlier, I know and respect many of the NRC committee members. What I do not understand is, how they reached these puzzling and in some cases erroneous conclusions in their review.

Thus, I believe, due to the presence of numerous errors, misrepresentations and mischaracterization of the scientific facts of the ATSDR water-modeling analyses, the NRC report cannot be used as a guidance document. In light of the concerns that I have raised in this response statement, I recommend that the NRC should be asked to: (i) prepare a supplemental document in which detailed correction of all the facts of the case would be included without misrepresentation or mischaracterization; (ii) based on these facts a reanalysis and

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reinterpretation of the ATSDR water-modeling analysis should be conducted and documented; and, (iii) the outcome be reissued in a report immediately to serve the public health concern of the former Marines at Camp Lejeune, North Carolina. Otherwise, in the opinion of many scientists who will review the contents of the NRC report and the responses they will receive to the report, such as this one, what will be in question is the overall credibility of the effort undertaken by NRC in this case.

This response statement is respectfully submitted to ATSDR to document my scientific evaluation of the findings of the NCR report.

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