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Scientific and Technical Advisory Committee Review of the Chesapeake Bay Program Partnership's Climate Change Assessment Framework and Programmatic Integration and Response Efforts

Maria Hermann

Scott Doney

Tal Ezer
Old Dominion University, tezer@odu.edu

Keryn Gedan

Philip Morefield

See next page for additional authors

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Authors
Maria Hermann, Scott Doney, Tal Ezer, Keryn Gedan, Philip Morefield, Barbara Muhling, Douglas Pirhalla, and Stephen Shaw

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Maria Herrmann[1], Scott Doney[2], Tal Ezer[3], Keryn Gedan[4], Philip Morefield[5], Barbara Muhling[6], Douglas Pirhalla[7], Stephen Shaw[8]

[1]The Pennsylvania State University, [2]University of Virginia, [3]Old Dominion University, [4]George Washington University, [5]US EPA, [6]University of California Santa Cruz, [7]NOAA, [8]State University of New York – Environmental Science and Forestry (SUNY-ESF)

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About the Scientific and Technical Advisory Committee

The Scientific and Technical Advisory Committee (STAC) provides scientific and technical guidance to the Chesapeake Bay Program (CBP) on measures to restore and protect the Chesapeake Bay. Since its creation in December 1984, STAC has worked to enhance scientific communication and outreach throughout the Chesapeake Bay Watershed and beyond. STAC provides scientific and technical advice in various ways, including (1) technical reports and papers, (2) discussion groups, (3) assistance in organizing merit reviews of CBP programs and projects, (4) technical workshops, and (5) interaction between STAC members and the CBP. Through professional and academic contacts and organizational networks of its members, STAC ensures close cooperation among and between the various research institutions and management agencies represented in the Watershed. For additional information about STAC, please visit the STAC website at www.chesapeake.org/stac.

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STAC Administrative Support Provided by:

Chesapeake Research Consortium, Inc.
645 Contees Wharf Road
Edgewater, MD 21037
Telephone: 410-798-1283
Fax: 410-798-0816
http://www.chesapeake.org

Abbreviations

BCSD – Bias Corrected Spatial Disaggregation
BMPs – Best Management Practices
CBP – Chesapeake Bay Program
CCAF – Climate Change Assessment Framework
CMIP – Coupled Model Inter-comparison Project
CO-OPS – Center for Operational Oceanographic Products and Services
CRWG – Climate Resiliency Workgroup
DEM – Digital Elevation Model
ENSO – El Niño–Southern Oscillation
ET – Evapotranspiration
GCM – General Circulation Model or Global Climate Model
GPCC – Global Precipitation Climatology Centre
LOCA - Localized Constructed Analogs
MACA – Multivariate Adaptive Constructed Analogs
MD DNR – Maryland Department of Natural Resources
NAO – North-Atlantic Oscillation
NOAA – National Oceanic and Atmospheric Administration
NGS – National Geodetic Survey
NWF – National Wildlife Federation
PET – Potential Evapotranspiration
PRISM – PRISM Climate Group, Oregon State University, http://prism.oregonstate.edu
RCP – Representative Concentration Pathway
SLR – Sea-level Rise
STAC – Scientific and Technical Advisory Committee
SLAMM – Sea Level Affecting Marshes Model
USACE – U.S. Army Corps of Engineers
USGS — U.S. Geological Survey
WARMER – Wetland Accretion Rate Model of Ecosystem Resilience
WM – Watershed Model (CBP)
WQSTM – Water Quality and Sediment Transport Model (CBP)

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Executive Summary

The following report presents a synthesis of reviewer responses from the Scientific and Technical Advisory Committee's (STAC) panel on the Chesapeake Bay Program Partnership's Climate Change Assessment Framework (CCAF) and Programmatic Integration and Response Efforts. The enclosed findings and recommendations are in response to the 16 questions delivered to the panel (Appendix A).

In summary, given the current state of knowledge, the combination of using climate model projections and downscaling provides an acceptable baseline for estimating changing climate conditions for the Chesapeake Bay, and the panel finds the CCAF approach to be fundamentally sound. However, the panel members have a number of concerns pertaining primarily to the current lack of complete formal documentation on the details of the approach. In the responses to the questions that follow in the body of the report, the panel has outlined several areas where more details or further investigations are suggested and has also provided some specific recommendations for CBP consideration in regard to future use and application of the CCAF.

The core findings and recommendations of the report are summarized below.

- The CBP's approach to select projections and global circulation models largely follows accepted practices in the climate change impacts research community. However, the CBP team could consider excluding strongly biased models by comparing them to longer-term multi-decadal monthly climatologies of temperature and precipitation.
- The CBP's use of Representative Concentration Pathways (RCPs) is in line with best practices of the climate science community as of the most recently available climate model inter-comparison project (CMIP5). RCP8.5 and RCP4.5 are reasonable choices bracketing a "business as usual" high climate change scenario and a moderately aggressive mitigation strategy, respectively. While different RCP scenarios are not likely to diverge strongly at the 2025 timescale, more difference in the RCPs should be expected in the projections out to 2050.
- The choice to use Bias Corrected Spatial Disaggregation (BCSD) downscaling approach is reasonable and justified, as BCSD has become a relatively standard approach to downscaling native climate model output. More generally, the use of readily available downscaled product rather than creating a customized downscaling procedure for the Chesapeake domain seems appropriate and justified. Ideally, it may be advisable to conduct a review and an inter-comparison of other available downscaled products over the Chesapeake domain.
- The panel agrees with the conclusions of the CBP that there remains uncertainty in the response of tidal wetlands, but that the Sea Level Affecting Marshes Model (SLAMM)

provides the most useful and applicable tool available for the geographic region at this time.

- While the current treatment of relative and global mean sea level rise (SLR) in the framework of the CBP modeling suite (i.e., WQSTM) seems appropriate, the potential overall impacts of SLR on the Chesapeake Bay most likely will go beyond what is included in CCAF (e.g., accelerated minor flooding), and should be discussed in the documentation, and checked for consistency.
- The panel has concerns related to the decision to extrapolate precipitation from the last 100 years out to 2025. The STAC Workshop report appropriately observed that precipitation is highly influenced by decadal-scale variations in climate, and recommended against extrapolation over the short term. However, it is not clear how extrapolating over the full record corrects for this decadal-scale, natural variability.
- The Delta Approach is well-designed to address changes in mean conditions but is not fully capable of analyzing future changes in variability and extreme events. To a large degree, the magnitude of the future precipitation events is being dictated by the 1991-2000 baseline period used as the template for daily variability. Even if the high percentile precipitation values are modified, the extremes will be set by the very specific conditions that occurred in 1991-2000. While the panel does not necessarily suggest that the use of this 10-year period to set the variability is invalid, it seems essential to clearly document that this choice likely has a large role in establishing the magnitude of future extremes.
- The full uncertainty in future climate effects is underestimated by the current set-up of the Delta Approach, and in particular by the choice to use the 10^{th} and 90^{th} percentiles. Excluding a subset of models with unacceptably high bias against recent historical observations before considering their projections is a reasonable practice; however, excluding models based solely on their forward projections implies that one has reason to believe that these models are flawed, or their projections are unlikely. Unless there is some justification for this, the full ensemble should be included. Another major point of concern is the selection of "high precipitation/high temperature" and "low precipitation/low temperature" climate futures, which do not capture the full potential range of the boundary conditions for evapotranspiration.
- The provided written documentation is lacking in detail and organization, which significantly limited how well each of the questions could be addressed by the panelists. The panel strongly recommends that a substantial effort should be dedicated to improving the comprehensive description of the overall CCAF strategy in the documentation, including a clear statement of the central goals and overarching strategy of the CCAF effort and the specifics of how the climate simulations are incorporated into the CBP modeling suite.

Introduction

The following report presents a synthesis of the reviewer responses to the 16 charge questions (Appendix A) delivered to the panel in the main CCAF document [01] (Appendix B) dated 30 June 2017. The Climate Change Assessment Framework (CCAF), under current review, was presented to the panel by the Chesapeake Bay Program (CBP) as a collection of information sources – including technical reports and briefing documents – which are listed in Appendix B together with the referencing codes assigned by the panel and used for citation purposes throughout this report. Additional and clarifying information on CCAF was presented by CBP directly to the panel in the form of a two-hour webinar ([04] on 8 August 2017 (information webinar, hereafter). Thus, the panel formed the following recommendations based on multiple sources of information, rather than a single comprehensive document targeted to CCAF.

Review Panel Responses to Charge Questions

1. Please comment on the overall approach to incorporate projected 2025 and 2050 climate change into the Watershed Model and Water Quality and Sediment Transport Model.

The panel greatly appreciates the effort that was taken to incorporate the latest climate science into the CBP modeling suite. Given the state of knowledge, the combination of using historical trends, climate model projections, and downscaling provides a solid baseline for estimating changing climate. The panel's main concern, however, is that the provided written documentation is insufficient in its current state, and does not present a comprehensive description of the overall CCAF strategy. The panel strongly recommends that a substantial effort should be dedicated to improving the documentation, including a clear statement of the central goals and overarching strategy of the CCAF effort. Once the central objectives are clarified and explicitly defined, it will become easier to identify the most desirable structure for climate change assessments within the CBP modeling suite, as well as what treatment of uncertainty is most suitable for the task at hand – that is, whether the analysis should focus on most probable events or on the extremes. The documentation of the CCAF approach would also benefit from attention to overall organization and detail. For example, the decisions of which climate models, support tools, and climate scenarios were chosen is clearly outlined in document [1]. However, the specifics of how these climate simulations are incorporated into the CBP modeling suite are absent from the documentation. It is difficult, therefore, to evaluate the incorporation of climate change into the models when the implementation is not known. As an example, in the case of tidal wetland loss due to sea level rise and its impact on nutrient loads, what is the relationship between tidal wetlands and nutrient loads in the WQSTM? Are tidal wetlands defined simply by overall area, by bands of elevation (low marsh, high marsh), by subtypes (forested, fresh tidal, brackish, salt), etc.?

2. How well do the global circulation models used for producing 2025 and 2050 climate scenarios show skill in hindcasting the actual climate and hydrological changes that have happened in the Chesapeake Bay watershed over the past decades?

The evaluation of the Coupled Model Intercomparison Project, Phase 5 (CMIP5) model skill for the Chesapeake Bay watershed is a crucial aspect of the climate projection effort, but one that should be addressed to the CBP modeling team. A more appropriate phrasing of the question for an external review team would be: "Did the CBP team adequately assess and utilize model hindcasting skill in the incorporation of CMIP5 climate model projections as drivers for the Watershed and Water Quality-Sediment Transport Models?" Based on the slides from the information webinar [04], the CBP team has begun conducting a zeroth-order skill analysis highlighting some clear model deficiencies, but it was not fully clear the extent to which model skill (or lack thereof) influenced the design of the climate forcing future projections. This is a definite gap in the written documentation provided to the review committee.

It may be useful here to refer to past studies and reviews in order to address the general question of hindcasting skill, and to clarify appropriate validation techniques for GCMs. The first point to note is that GCM runs (as used in this study) are not constrained by boundary conditions that would force them to match observed year-to-year variability (e.g., Hayhoe et al. 2007; Overland and Wang 2013). This indicates that they will not be able to reproduce the specific timing of a warm or a cold year (or a wet or a dry year) in the recent past, or in the future. Observed historical patterns are a product of both anthropogenic (man-made) trends, as well as natural variability resulting from cycles such as ENSO, the PDO, the NAO, and others. As Stock et al. (2011)[1] note, variability at the regional-scale is often most strongly driven by these internal/natural processes, while the anthropogenic signal is typically strongest over larger areas (e.g., global). As a result, changes or trends in the Chesapeake Bay watershed over timescales of the past few decades or less will be primarily a result of natural variability, which is not replicated on a year-to-year basis by the GCMs. However, there are other methods by which GCMs historical runs can be compared with observations. It is common to compare the mean and standard deviation of observed vs. modeled values of key variables such as temperature or precipitation over the region of interest. Nonetheless, it is important to note that mean values should be calculated over a period of at least ~30 years or so, otherwise (as above), the observed signal may be too confounded by natural variability. Muhling et al. (2017) completed this exercise using historical runs from 33 (un-downscaled) CMIP5 GCMs over the Chesapeake Bay and watershed, comparing observations to model outputs for the period 1956-2005. The authors assumed that GCMs with mean annual air temperature within 2°C of observed values, and mean annual precipitation within 400 mm of observed, constituted "acceptable" bias. Seven GCMs

[1] Stock et al. (2011) is an excellent, "plain English" review of the issues associated with using GCM projections for living resource management, and the panel would strongly recommend it as further reading in addition to the enclosed comments.

were removed using these criteria: FGOALS-S2, MIROC-ESM, MIROC-ESM-CHEM, CAN-ESM2 and ACCESS1-3 were excluded for warm temperature bias, FGOALS-G2 was too cool, and rainfall in CMCC-CESM was too high. Four of these seven GCMs were included in the 31-member ensemble used by the CBP (Bhatt & Shenk), and so they may wish to ensure that projections from these models are not strong outliers. While Muhling et al. (2017) used different statistical downscaling methods compared with the CBP, they found that where GCMs were strongly biased against the observed seasonal cycle (e.g., winters too warm, summers too cold), and downscaling often did not fix this problem. Thus, the panel recommend that GCMs downscaled using BCSD are still checked for unacceptably high seasonal bias before inclusion in the ensemble.

Moreover, it is not necessarily true that GCMs with closer agreement to observed 20th century climatologies will result in more reliable future projections (see Stock et al. 2011). While there is no accepted best practice for choosing GCMs for any particular study or purpose, many researchers proceed by first culling models with unacceptably high bias, or strong outliers. Remaining models can be averaged in an ensemble (e.g., Overland et al. 2011), or used to represent different plausible future scenarios (e.g., Muhling et al. 2017). Some studies (e.g., Giorgi and Mearns 2002) weight GCM projections in ensembles in proportion to their skill in reproducing historical observed climate characteristics. However, many climate scientists argue against this practice.

3. Please comment on the appropriateness of the methodology to account for uncertainty in 2025 and 2050 climate projections.

The documentation on the analysis and interpretation of uncertainty results is rather incomplete in its current form (though there were some slides in the information webinar [04] presentation). The panel strongly recommend that the written documentation be expanded to include a discussion of different types of uncertainty that have different impacts on different timescales and processes (e.g., model errors in hindcasting; initial conditions and natural climate variability for decadal-projections to 2025; structural model differences and uncertainties in RCP for projections to 2050; evapotranspiration treatment in the watershed model). Another important point that should be emphasized in the documentation is that the arbitrary selection of the 10th and 90th percentiles gives the false impression of statistical certainty.

The primary sources of uncertainty in GCM projections are dependent on the time horizon and the variable being projected. By 2025, uncertainty is mostly expected to result from GCM spread and natural variability, with a much smaller contribution of RCP scenario (see Kirtman et al. 2013). The CBP projections have accounted for the former by including a large ensemble of GCMs, as is generally recommended. It is much more difficult to account for natural variability, as it cannot yet be well predicted by decadal-scale forecasts. The CBP projections for 2025 should not be expected to quantify natural variability, but it should be more strongly emphasized that the conditions in the Chesapeake Bay region by 2025 will primarily result from natural variability, and not from a strong anthropogenic signal. By 2050, it should be expected that the RCP scenario will contribute much more to uncertainty in projections, and natural variability substantially less.

In terms of the variable being projected, an anthropogenic warming signal is likely to be more apparent, but precipitation is much more uncertain. Muhling et al. (2017) found that in some GCMs, there was no statistical trend in projected precipitation over the Susquehanna River watershed by the end of the 21st century. It should therefore be expected that many GCMs will show no significant trend in mean precipitation by 2050, and this uncertainty should be more clearly stated. The method used for statistically downscaling GCM projections can influence the magnitude of projected future change. The CBP projections appear to use the USGS BCSD products, which are commonly used. It is likely beyond the scope of the study to comprehensively consider the uncertainty contributed by the selection of statistical downscaling method, however, the CBP team may wish to compare their results to Muhling et al. (2017), who used a selection of quantile mapping methods as well as a cumulative distribution function transform (Muhling et al. found that the choice of downscaling method within this small subset contributed less uncertainty than did choice of GCM. However, it could provide significant spread at the upper end of the temperature distributions, due to the different way that each method handled extrapolation to conditions not seen in the historical period. This may be even more important if attempts are made to model extreme events at the upper tails of temperature and precipitation distributions).

In regard to the methodology for precipitation projections for 2025 and the associated uncertainty, the decision to extrapolate precipitation from the last 100 years out to 2025 is based on the recommendation of the 2016 STAC workshop [02] with the rationale that such short-term estimates may be best deduced from the historic record instead of the climate models. However, the panel has several concerns related to this decision. The STAC workshop report appropriately observed that precipitation is highly influenced by decadal-scale variations in climate and recommended against extrapolation over the short term. However, it is not clear how extrapolating over the full record corrects for this decadal-scale, natural variability. First, the natural variability itself can generate the appearance of a long-term trend. If one were to synthetically generate precipitation data in such a way as to include "persistence" in the signal that leads to sustained wetter and drier periods, by random coincidence one could get a dry period early in the record and a wet period late in the record that gives the impression of a long-term upward trend. In the actual record, it is difficult to determine whether any observed trend is due to long-term changes driven by anthropogenic climate change or random coincidence in timing of wet and dry periods. Second, because precipitation is strongly influenced by decadal scale variability, it is seemingly possible that by 2025, the Chesapeake Bay region could reenter a drier period. Recent work has promoted the idea of looking for change points – rapid shifts in the mean value of time series – in place of assuming that climate variables change in gradual ways that can be predicted by looking at trends (Sagrika et al. 2014, Ivancic and Shaw 2017). By adopting a mentality of looking for change points, one is acknowledging that natural variability can cause relatively rapid shifts in the magnitude of climate variables. Thus, extrapolating the long-term record provides little information on how natural variability may be influencing the climate in 2025. As 2025 approaches, it seems that the most reasonable estimate of 2025 precipitation is that of recent precipitation (say from the last 10 years). As an error bound, one could possibly recognize some high and low percentile threshold from the historical record. As a qualitative bounding of error, it could also be useful to present the approximately 100-year precipitation records with a moving average line (say for a 5- or 10-year window) that highlights decadal scale variability in wetness, not just the long-term trend. Figure 1 below is generated from 1927-2017 precipitation data at State College, PA. As noted earlier, given periodic variability in decadal means, one could just as easily be below the trend line in 2025 as above.

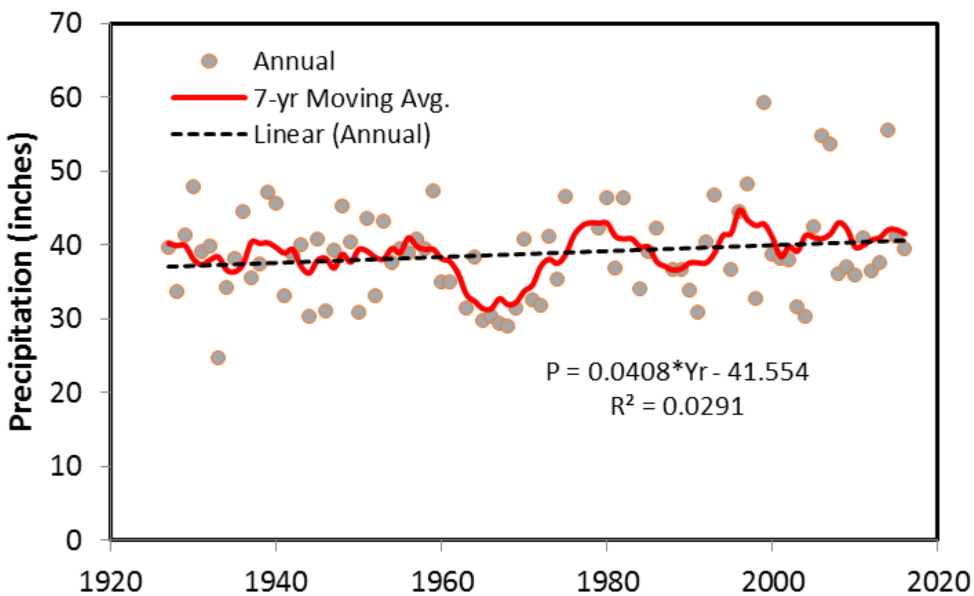

Figure 1. Precipitation data at State College, PA from 1927-2017

In generating this sample figure, it also became quite clear that much of the linear slope over the last 100 years was driven by several high precipitation years post 1995. In most cases, these high annual precipitation totals were due only to brief wet weather periods occurring as a single storm or at most as a multi-week wet period. For instance, 1996 (the largest annual value in the record) had a storm that brought over 4 inches of rain in two days in June and a two week stretch in September that brought over 9 inches of rain. These two events alone shifted the annual total far above normal levels, but it is hard to attribute these two events to any systematic upward increase in annual precipitation driven by a process specifically connected to anthropogenic climate change. This reinforces the opinion that it is not justifiable to simply extrapolate the 100-year record. A paper by Smith et al. (2010) reviews three different mesoscale causes of three consecutive near-record floods in the Delaware Basin in 2004, 2005, and 2006. While nearby but not in the Chesapeake Bay watershed, the paper reinforces the diversity of causes of extreme flows in the eastern U.S. and the difficulty in attributing these recent extreme flows to any trend in climate drivers.

The panel strongly recommends that thorough discussion of uncertainty of the 2025 projections be added to the CCAF documentation and discussed in any potential future use of these projections. More specifically, it is important to clarify that 2025 projections should not be treated as singe-year projections, but rather as plausible expectations of future conditions, centered on 2025. This is important for any projection, but especially for 2025, because the reality will soon be known for comparison with the projections, and because natural variability is likely to dominate observed conditions in the near future (e.g., Deser et al. 2012). Without clear discussion of the uncertainty of these near-term projections (from which deviations are likely due to natural short-term climate variability), stakeholders who follow the discussions may quickly lose confidence in the validity of longer-term climate change projections.

4. Please comment on the CBP's use of multiple Representative Concentration Pathways (RCP's) and their associated 10th, 90th percentiles and the median projections to derive 2025 and 2050 temperature estimates and 2050 precipitation estimates?

The use of Representative Concentration Pathways (RCPs) is in line with best practices of the climate science community as of the most recently available climate model intercomparison (CMIP5). RCP8.5 and RCP4.5 are reasonable choices bracketing a "business as usual" high climate change scenario and a moderately aggressive mitigation strategy, respectively. RCP2.6 seems overly optimistic given present-day global discussions on climate mitigation, but could perhaps be useful to include to highlight benefits to the Bay of aggressive mitigation. At the 2025 timescale, the anthropogenic signal is expected to be weak, and the different RCPs will likely not diverge strongly. More difference in the RCPs should be expected in the projections out to 2050.

The availability of common RCP scenario simulations from multiple climate modeling groups reduces the reliance on any single climate model, and in many cases the use of multi-model ensembles (median and range) for climate projection appears to provide more robust results. The use of the model median seems appropriate; the choice of the 10^{th} and 90^{th} percentiles is somewhat arbitrary and the panel is unsatisfied by the justification provided in the current documentation for the selection of percentiles. The arbitrary selection of percentiles gives the false impression of statistical certainty, that is, that the tails of the distribution are so unlikely that they can be safely ignored. The panel know of no evidence to support that claim and thus, strongly recommend that a detailed discussion is added to documentation to address this issue.

Another major point of concern is that the selection of "high, high" and "low, low" climate futures (named "Ensemble_P90" and "Ensemble_P10" in slide #13 of the information webinar [04]) seems inappropriate given the specific research questions and modeling goals. The "extremes", for lack of a better term, with respect to hydroclimatic conditions would be: (i) low/decreasing precipitation coupled with increasing temperatures ("Ensemble_P10T90") and (ii) increasing precipitation coupled with decreasing/low increase temperature ("Ensemble_T10P90"). The panel concludes that full potential range of the boundary conditions for evapotranspiration are not captured by the current climate change selection approach. The documentation does not provide any rigorous justification for the existing choices and the panel recommends that this issue be addressed in depth.

As a general remark, the panel would also note that most climate scientists recommend against treating an ensemble of GCMs as anything like a probability distribution, given that the GCMs cannot be considered to be independent from each other (because many use the same or similar sub-models). The scientific community has no real information yet on which of the RCPs is most likely to be realized. Thus, the panel recommends that the documentation highlights that GCM ensemble projections represent a range of plausible futures, rather than a probabilistic estimate.

With regard to the documentation, the written documentation for the RCPs addresses only the RCP4.5 results; the RCP8.5 results discussed in the information webinar [04] need to be included. In the webinar material for the recommended 2050 modeling climate inputs, it appears that only the RCP4.5 value of atmospheric CO_2 is mentioned, but one would want to use consistent atmospheric CO_2 and climate anomalies for evapotranspiration from the different climate scenarios (e.g., RCP2.6 vs. RCP8.5). Another suggestion to improve the documentation: as the natural/internal variability will likely be dominant out to at least 2025, particularly for precipitation, the CBP team could consider showing some projections at an annual time-step from some selected GCMs as time series out to 2050, to emphasize this point, in addition to the mean % change from each GCM (such as shown in slide 33 of the information webinar [04]). Fig. 8 in Muhling et al. (2017) shows something similar for 4 GCMs under RCP 8.5, and the importance of internal variability in the models is quite clear.

5. Please comment on the CBP's selection of the downscaling approach, Bias Corrected Spatial Disaggregation (BCSD) downscaling methodology to derive 2025 and 2050 temperature estimates and 2050 precipitation estimates?

BCSD has become a relatively standard approach to downscaling native climate model output. This is a reasonable and justified choice. As discussed in the documentation, these downscaled outputs have been used extensively without any concerns of technical errors or reliability. More generally, the use of readily available downscaled product rather than creating a customized downscaling procedure for the Chesapeake domain seems appropriate and justified. Ideally, it may be advisable to conduct a review and an inter-comparison of other available downscaled products (e.g., MACA or LOCA) over the Chesapeake domain.

6. Is the interpretation of downscaled climate data from a gridded product (⅛° resolution) to a county-scale within the Watershed Model sufficient to represent changing climatic patterns and assess load responses at a larger regional scale?

To answer this question, one would need to have a better sense of the spatial scale of the information content in the downscaled gridded product (1/8 deg.). Frequently, downscaled products at high spatial resolution from coarse resolution climate models do not fully capture real-world spatial variability. This issue was not discussed in great detail in the documentation and the panel recommends that the CBP team conduct further analysis on this topic in the future.

7. Given limitations of modeling resources, policy and governance, is the applied Delta Approach for precipitation, temperature and evapotranspiration adequate to represent a range of potential changes in climatic forcing variables? Are there limitations in the ability to capture potential variability of precipitation intensity, temperature swings, or timing of extreme events (e.g., storm occurring early in growing season vs. late fall) that would affect the ability to assess the impact of less probable but higher magnitude events (e.g., Hurricane Isabel)?

The documentation should be substantially expanded to clarify the details of the Delta Approach. Document [1] gives the following description of the Delta Approach procedure: "… downscaled climate model historical values are compared against future projected values and the average percentage or degree change is then applied to an observation dataset used in the model of study". The changes can be calculated and applied to the forcing data in multiple ways, e.g., at daily, monthly, seasonal, or annual, resolution, with and without averaging, and so forth. Each one of the decisions made along the way must be carefully described and justified in the documentation.

This additional documentation may also help clarify how BCSD downscaling is being used in conjunction with the Delta Approach. As noted, BCSD has become a relatively standard approach to downscaling native climate model output. However, other downscaling approaches are still used, one of those being the "delta" or "change factor" approach (Ho et al. 2012). Thus, by mentioning BCSD and the "delta" approach it may imply that two different methods are being used to downscale the native climate model output. In actuality, it appears to the panel that the delta approach is being used to further analyze the already downscaled data. It may avoid confusion by simply saying that the relative change in historic and future BCSD downscaled data is calculated and avoid using the terminology of a "delta" approach.

The panel appreciates that an important incentive for using the Delta Approach must be the reduced number of required model runs and thus a more economical use of computational and human resources. However, two major artifacts of this approach are that, (1) it considers only mean changes in climate variables, while variability remains fixed by the base-line period of the forcing; and (2) the use of the 10th and 90th percentiles (rather than the full model spread) seriously underestimates the uncertainty in future climate conditions. This approach may be reasonable for certain applications as it likely captures the range of potential futures for the Chesapeake Bay watershed in terms of mean temperature and precipitation. Extreme events are more difficult to represent in GCM projections, as they are realizations of the tails of the probability distributions (see Flato et al. 2013). In addition, many extreme events are associated with processes that operate at a finer spatial structure (particularly for precipitation), and so these are often represented more accurately as GCM resolution increases (Wehner et al. 2010). However, most GCM projections agree that the Mid-Atlantic Bight region will see more warm temperature extremes in the future, and more extreme precipitation events overall (Romero-Lankao et al. 2014). The potential effects of these extreme events on future water quality targets should be discussed (e.g., Lee et al. 2014, 2016), but it would be beyond the scope of the CCAF assessment to try and model them explicitly. In contrast, the representation of future extreme precipitation events specifically from tropical cyclones in GCMs is highly uncertain. While lower-resolution models can reproduce the observed frequency and distribution of tropical cyclones with some skill, higher resolution models are needed to reproduce observed intensity of stronger storms. Recent studies project that there may be overall fewer tropical cyclones by the end of this century, but that average cyclone intensity and precipitation rates will increase (Knutson et al. 2015). Given the relatively short time horizons for the CBP analyses (2025 and 2050), there is likely insufficient information to project changes in precipitation from tropical storms with any confidence.

The presented documentation does not provide much detail on extreme events with the exception of some discussion of trends in precipitation intensities. An unanswered question is whether the coarse spatial resolution and temporal output scales (and variance metrics) for the CMIP5 simulation archive are adequate. The answer will of course change for different extremes depending on scales (e.g., drought and heatwaves are perhaps better captured than convective rainfall, flash flooding, hurricanes etc.). Climate projections of extreme events is an active area of research. If extreme events are important for the CBP modeling effort simply taking existing CMIP5 archive simulations is likely inadequate. This would require some dedicated research funding to assess the sensitivities of the CBP watershed and water quality-sediment transport models to different types of extreme events, which would then guide the choice and treatment of climate projection model results.

8. Is the use the Karl and Knight (1998) estimates of precipitation intensity appropriate for modifying 2025 precipitation intensity? Is it sufficient to apply these estimates to the entire watershed based on their central Mid-Atlantic derived trends?

The treatment of precipitation intensity is not explained in sufficient detail in the provided CCAF documentation in its current state. Foremost, the term "precipitation intensity" can be interpreted in many different ways depending on the context, and so when used as a quantitative metric, it should be clearly defined. Karl and Knight (1998) constructed probability distributions of daily precipitation events and defined intensity as the amount of precipitation associated with specific percentiles of the probability distribution. Their study documents intensity changes over the twentieth century, demonstrating that the proportion of total precipitation derived from the extreme events (the upper tail of the probability distribution) is increasing relative to more moderate events (middle of the probability distribution). Modifying higher percentile precipitation events more than lower percentile events does not seem unreasonable. It is generally well documented that because a warmer atmosphere can hold more moisture, precipitation intensity will increase. This increase would scale at roughly 7% per unit change in Celsius in accordance with the Clausius-Claperyron relationship. Additionally, because mean rainfall will scale with changes in latent heat exchange as dictated mainly by the energy budget, annual rainfall will increase more slowly than extreme precipitation (Held and Soden 2006). This therefore implies (as many others have argued) that high percentiles should indeed be scaled upward more and the panel agrees with this rationale. However, the panel is unable to comment on the implementation details, as there is no information available in the documentation.

A related concern is how daily variability is introduced into the projected future climate. To a large degree, the magnitude of the future precipitation events is being dictated by the 1991-2000 baseline period used as the template for daily variability. Even if the high percentile precipitation values are modified, the extremes will be set by the very specific conditions that occurred in 1991-2000. This ten-year period is a small sample of the range of possible extremes and not necessarily representative of actual future conditions. While the panel does not necessarily suggest that the use of this 10-year period to set the variability is invalid, it seems essential to clearly explain that this choice likely has a much larger role in establishing the magnitude of the extremes than the additional scaling.

9. The models (both the P5 and new P6 versions) use a 10-year average hydrology for the simulation. The 10-year period that is used is 1991-2000. The TMDL and planning targets are also based on a hydrologic critical period (1993-1995) for meeting WQ standards. With the latest information we have about climate science and given the methods that being used to incorporate changing temperature, precipitation, and sea-level into the models, are these periods still appropriate, when the hydrologic averaging period is 17 years old and the critical period is 23 years old?

When comparing two time periods for climate change impact studies, most climate scientists recommend averaging over at least 30 years (e.g., compare 1970-1999 to 2035-2065). Some studies use 50 year means, but others use 20 years, or occasionally as little as 10. The wider the period over which observations and model outputs are averaged, the smaller the contribution of natural variability to the difference is likely to be. However, these long averaging periods tend to be at odds with natural resource management goals, which often operate on time scales of several years to a few decades at most (i.e., most managers do not want to hear what may happen by 2070-2100, but this timescale is the one where climate change impacts are best represented by GCMs).

The main questions being asked by the CBP appear to relate to how future conditions will change compared to the "present". How the present-day baseline is defined is a decision for the program and natural resource managers, but averaging over at least 20 years to define present-day and future time periods would be more in line with accepted practices in climate change impact science. Natural cycles such as the NAO, ENSO and Pacific-North American (PNA) teleconnection influence temperature and precipitation in the Mid-Atlantic (e.g., Notaro et al. 2006). Moving to wider baseline and future periods would help to negate the effects of these on climate change analyses.

For the CBP modeling, preferably one would want to use an up to date baseline period that incorporates ongoing climate change over the past several decades. The caveat is that one wants a baseline period with adequate forcing and validation data, and need to assess carefully whether aspects of the model parameterizations are tuned to a particular baseline time period. The existing documentation requires a careful review to clarify the different baseline periods used for model assessment, validation, climate change forcing, etc.

10. Was the use of a modified Hargreaves-Samani evapotranspiration methodology sufficient to capture expected changes due to projected temperatures? In addition, should other ET methodologies be considered to develop a comparison of ET estimates?

Reasonable estimation of evaporation in future climate conditions is critical for adequate predictions of the water balance in the Chesapeake Bay system. The advantage of using existing PET parameterizations is that they allow approximation of the ET process from readily available variables, such as air temperature and humidity (e.g., Lu et al. 2005). The major shortcoming of relying on the existing parameterizations of PET is that the parameterizations are based on the empirical relationships between atmospheric variables that will not necessarily hold under future climate conditions. Ideally, explicit parameterization of PET should be avoided and replaced instead by a surface energy balance model. The panel's long-term recommendation is to investigate the feasibility of the energy-balance approach for the CBP modeling suite (see Liou and Kar 2014 as an example).

Besides suggesting that the Hargreaves-Samani potential ET equation may be more in-line with a method such as Penman-Monteith, there is no indication in the CCAF documentation why Hargreaves-Samani was selected in place of other available methods, e.g., Priestly-Taylor. The panel presumes that the modeling team picked a PET model that was only temperature dependent and that did not require an estimate of net radiation (since this is not readily available as an output from the BCSD downscaling). The specific rationale for picking Hargreaves-Samani instead of other methods which could have been used in place of the Hamon method should be clearly stated.

While Hargreaves-Samani is more favorable than the Hamon method (formerly used) in terms of producing physically reasonable estimates of ET in a changing climate, recent research suggests that Hargreaves-Samani is not necessarily the most physically reasonable PET formulation. Recent work by Milly and Dunne (2017) found that a simple net radiation model came closest to reproducing future ET during periods of negligible water stress as predicted by a GCM, which is arguably a more complete representation of ET than Penman-Monteith. Milly and Dunne (2017) indicate that when averaged globally, the Hargreaves-Samani PET equation overestimates changes in future ET relative to GCM estimates by approximately 0.2 mm day^{-1} (see Milly and Dunne 2017, Fig. 4). Thus, Hargreaves-Samani may still be overestimating ET (although less so than Hamon). In light of the recent work by Milly and Dunne, it would seem reasonable to acknowledge that ET estimates may still be biased high.

11. Please comment on the appropriateness of the methodology to select 2025 and 2050 sea level rise scenarios for application in the WQSTM?

The 2025 and 2050 projections based on Kopp et al. (2014) and presented in document [02] are quite reasonable, though there are differences in projections from different research groups. The uncertainties arise from the unpredictable variations in the Gulf Stream and atmospheric patterns such as ENSO and NAO; the rate of SLR acceleration in the Chesapeake Bay region is also not settled. One may also acknowledge that there are spatial variations in SLR rate along the Chesapeake Bay, which are not been considered in CCAF.

Document [03] provides a good summary of the current knowledge, and the recommendations by Kopp for selection of scenarios are reasonable. Kopp's projections for SLR in Baltimore and Sewells Point were compared to those calculated by the USACE projections (http://www.corpsclimate.us/ccaceslcurves.cfm). It was found that Kopp's projections for "likely" SLR for 2050 are consistent with USACE's projections of middle to high range (2000-2050 SLR at Sewells of 34-53 cm for Kopp vs. 31-60 cm for USACE). However, the 2100 projections were significantly higher for USACE than Kopp's, which is probably due to uncertainty in the acceleration rate. While variations in relative SLR within the Bay are ignored in the modeling of the projections, one should at least acknowledge that parts of the lower Bay with its large subsidence may experience more effects of SLR than other parts.

The panel notes inconsistency in the CCAF documentation in the definition of 'relative SLR'. In document [03], SLR is defined relative to 2000. In document [01], Fig. 7 gives SLR relative to 1992, while the table and Fig. 9 are relative to 1995. This inconsistency is confusing and can cause errors, so all the projection numbers must be carefully checked to see that they relate to the same reference level. Also, the documentation should clearly define "Background SLR" which the panel assumes refers to mostly land subsidence (Boon et al. 2010, Karegar et al. 2016) with additional contributions from variations in atmospheric and oceanic patterns, such as a potential slowdown of the Gulf Stream (Ezer et al. 2013).

The section on "Relative Sea Level Rise" in document [01] is very brief (one paragraph, two figures, and one table) and does not give enough detail to the treatment of this major issue that is likely to affect the Chesapeake Bay as much (or more) as other climate drivers, such as changes in precipitation or air temperature. Acceleration in SLR (Ezer and Corlett 2012), acceleration in flooding, coastal erosion of barrier islands, and saltwater intrusion into marshes, are already being observed in the region. In particular, the acceleration of minor tidal flooding (Ezer and Atkinson 2014; see for example., http://www.ccpo.odu.edu/~tezer/NorfolkFloods_2016_1ft.png ; http://www.ccpo.odu.edu/~tezer/Flood_projection_0.3mNorfolk.png) and the impacts on cities and facilities along the Chesapeake Bay (Boesch et al. 2013, Ezer and Atkinson 2015) are not considered in CCAF documentation. In addition, additional background (besides reference to the workshop) explaining the large land subsidence in the region (Boon et al. 2010; Karegar et al. 2016) and the impact on local SLR from climatic changes in ocean currents like the Gulf Stream (Kopp 2013, Ezer et al. 2013, Boon and Mitchell 2015) would be useful to consider.

In summary, while the treatment of relative and global mean sea level rise (SLR) in the WQSTM seems appropriate, the potential overall impacts of SLR on the Chesapeake Bay most likely will go beyond what is included in CCAF (e.g., accelerated minor flooding), which should be discussed in the documentation. Additionally, the parts of documentation relevant to SLR should be checked for consistency.

12. Given limitations on available data sets and modeling products, as well as uncertainty about how wetlands within differing geographies may adapt to changes in sea level over-time, please comment on the appropriateness of the methodology to project 2025 and 2050 tidal wetland change?

The panel agrees with the conclusions of the CRWG that there remains uncertainty in the response of tidal wetlands, but that the SLAMM provides the most useful and applicable tool available for the geographic region at this time. The updates to SLAMM in v6 over v5 are substantial, particularly with respect to accounting for different types of tidal wetlands (e.g., v6 incorporates specific accretion rates for marshes of different salinity classes and initial marsh elevations). This is relevant because the 2008 NWF SLAMM run used v5, while the 2012 MD DNR SLAMM run used v6. Additionally, the 2008 NWF SLAMM study used a 30 m DEM while the 2012 MD DNR SLAMM study used a 10 m DEM. This difference in spatial resolution is large when it comes to understanding topography and upland suitability for tidal marsh migration, the key process for predicting marsh resilience and future habitat area. This raises the question for the panel as to why the CRWG recommended using the 2008 NWF results instead of the 2012 MD DNR results.

The CRWG also recommends the use of NOAA's Sea Level Rise Marsh Impacts and Migration Tool, which (based on the documentation of the tool available online) uses an approach similar to SLAMM and incorporates best available DEM data. Unfortunately, it is not clear to the panel exactly how up to date the tool's data is for our region of interest. This information needs to be discussed. Also, how much will the WQSTM runs rely upon SLAMM vs. NOAA's tool? Recent efforts by USGS scientists use a model of tidal wetland response called WARMER (described in Swanson et al. 2014) to predict change in wetland area across the major estuaries on the Pacific coast. WARMER is a model of mineral and organic sediment accumulation and decay. As of now, there are not enough data available to parameterize WARMER for the Chesapeake Bay wetlands without further study, so the panel concurs with the decision to rely on SLAMM or other similar tools for CCAF. However, it is recommended that the CBP considers developing WARMER parameterizations for future applications.

Finally, recent studies (Enwright et al. 2016, Kirwan et al. 2016) have highlighted the importance of the transition of human-dominated uplands in the response of tidal marsh to sea level rise. In areas of favorable topography and when upland barriers (e.g., developed areas, seawalls, dikes) are removed, there are higher probabilities of marsh migration and conservation (or even expansion) of marsh area in response to low to moderate levels of sea level rise. When upland barriers are maintained, these probabilities diminish and marsh loss becomes more likely. SLAMM v6 offers an option to "protect developed" uplands, which prevents any developed uplands from converting to wetlands. If developed uplands are unprotected in the model runs, the results likely overestimate future marsh area (at least some protection is highly likely). If developed uplands are protected in the model runs, the results are likely more conservative. It is important to clarify the treatment of developed uplands in CCAF documentation.

13. Does the applied methodology reflect the latest and best scientific understanding of the influence of climate on watershed processes and estuarine responses; is there any additional scientific information that should be included?

The analysis presented in the CCAF captures many of the key climate factors driving changes in watershed processes (e.g., precipitation, ET) and estuarine processes (e.g., river flow, temperature, sea level). While the panel did not identify any glaring gaps and omissions for short-term application, a number of other concerns and suggestions are outlined below.

1) **Tidal datums, SLR projections, and tidal wetland modeling.** The current tidal datum epoch is based on data from 1983 to 2001 and will be revised based on new data around 2021. Will the projections of sea level rise and tidal wetland change still be adequate? The SLAMM model runs on tidal datum reference points, with sea level rise estimates layered on top of tidal datum reference levels. It would be worth consulting with someone at NOAA CO-OPS or NGS about this issue to ensure an issue will not arise with predicting increases in tidal datums to 2025 and 2050 when the baseline of these datums will change during that period. Perhaps this warrants a brief analysis of how far our tidal datums have already shifted since 2001 for several Chesapeake sites.
2) **Feedback between sea level rise and watershed loadings.** While SLR is incorporated into the WQSTM, the effects of SLR are not included in the nutrient loads simulated by the WM. Saltwater intrusion due to rising sea level is leaching nutrients from coastal uplands, including coastal farmland. These nearshore coastal watersheds will therefore contribute disproportionate nutrient loads relative to inland watersheds, and the rate of loading will be affected by the nearshore topography of the coastal watershed in terms of the area of land that will be affected by saltwater intrusion for a given rate of sea level rise. Based on the provided documentation, the panel concluded that the watershed model does not currently account for this effect of SLR.
3) **Accounting for increases in CO_2 in the watershed model.** While the documentation mentions the accounting for increases in CO_2 concentration within the modeling framework, the details of how this is being done are not sufficient. Document [03] refers to adjustment of a hydrologic model parameter related to the vadose zone (lower vadose zone evapotranspiration), but it is not clear how this relates to changes in plant regulation of stomatal opening and closing. Is the lower vadose zone evapotranspiration parameter just a knob that can be used to adjust ET but which does not intrinsically have any connection to plant's response to changes in CO_2? Land surface models use standard equations to compute changes in stomatal resistance (e.g., Franks et al. 2017). Has any formulation such as these from the land surface models been considered? Admittedly, the land surface models often calculate changes in stomatal resistance iteratively, looking

at the balance between maximizing photosynthesis while minimizing moisture loss. There is not necessarily a single closed-form equation that can be readily incorporated into an existing, basic hydrologic model, but this area should be explored.

4) **Other potential feedbacks.** Other issues that potentially might be important but currently do not have a strong scientific consensus:
   - Changes in vegetation and soil dynamics associated with warming and higher CO2 may result in changes to nitrogen mobilization and release (Lee et al. 2014, Lee et al. 2016);
   - SLR may lead to changes in tidal characteristics in the bay (Lee et al. 2017, Pickering et al. 2017) which may potentially affect mixing and biogeochemical cycling;
   - Changes in water temperature, salinity, and acidification status may lead to changes in water column and benthic nutrient cycling.

14. Many of the plans to incorporate climate change into programmatic efforts are using more qualitative information. To what extent is there reliable quantitative information on which land uses and BMPs are going to be impacted by climate change? Is there quantitative information on modification that can be made to land use and BMPs that are effective in addressing climate change?

The panel is not aware of any extensive, comprehensive sources of quantitative information as described in this question. While the CBP modeling framework appears to be well-positioned to address the major land-use changes (i.e., urbanization, deforestation, etc.), quantification of the effects of climate change on the specific land use categories and BMPs is an area that should be targeted by new research efforts. The panel recommend that as far as feasible, CBP should facilitate synthesis activities and new research efforts in this area.

15. For longer term CBP considerations, how can the overall approach and procedures be improved and what alternative approaches and data would be recommended?

It is not entirely clear from this question whether "longer term" refers to consideration of the effects of climate change at timescales beyond 2050, or to similar modeling exercises to the present one being repeated at a later time. Below, the panel provides a list of comments relevant for both interpretations.

- Due to the (understandable) focus on near-term endpoints of 2025 and 2050, the current analyses clearly underestimate the long-term effects of climate change on water quality and living resources in the Chesapeake Bay, as scenarios that run to 2100 will likely become much more extreme, particularly those pertaining to sea level rise and tidal wetland loss. The sum effect of the documentation in its present form is slightly mollifying, when the long-term effects of climate change threaten to do much greater damage to water quality gains made earlier in the century. It would be worthwhile to have a separate section discussing end century scenarios, even if they are not investigated in the same level of detail.

- The panel recommends a more holistic assessment of the various sources of uncertainty (see Hawkins and Sutton 2011) in climate change projections, with a particular focus on the timescale of decisions, and carefully heeding the conclusions of that assessment. It is concerning that current analyses seem to intentionally ignore the full range of climate change projections by excluding the models which fall farthest from the ensemble median. Excluding a subset of models with unacceptably high bias against recent historical observations before considering their projections is a reasonable practice and not uncommon, as described in the response to Question 2. However, excluding models based solely on their forward projections implies that one has reason to believe that these models are flawed, or their projections are unlikely. Unless there is some justification for this, the full ensemble should be included. The panel suggest that future assessments consider the broadest possible range of climate change projections, inclusive of other downscaled data sets.
- A more complete investigation of future daily to monthly variability in the predicted climate signal is advisable. This could be done by comparing multiple runs from within the same model and emission scenario as well as across models. Part of this might also entail investigating what type of meso-scale meteorological process a given GCM can generate. For instance, if one knows that the largest precipitation amounts have historically been related to tropical cyclone, it makes sense to evaluate how well the GCM and downscaling process can even generate something that resembles a tropical storm. As GCMs progress to a point where they can more fully resolve a given phenomenon such as a tropical cyclone – GCM's are getting there but still have some issues (e.g., Emmanuel 2013), it may be more reasonable to let the GCM directly generate the variability in precipitation and to more probabilistically consider the likelihood of different extremes over a number of different runs.
- Future selection of climate change projections could be more focused on the relevant drivers. For example, since a particular point of emphasis in the current modeling effort was PET, the panel recommend that the scatterplots show in the information webinar [04] slides be updated to relevant climatic variables; the MACA dataset currently offers these, and LOCA will soon offer them as well.
- There is a need to evaluate how well the downscaling from global climate models really works, and how well it may work for longer term changes, say 2100 and beyond, with much larger uncertainties than 2025-2050. More analysis on the fine scale spatial pattern of changes of the different coasts along the CB are needed. In the long run, it might be useful to include consideration of nested regional climate models rather than simple downscaling.

- For long-term modeling work, the panel recommends investigating the feasibility of adding energy-balance module to the CBP modeling suite, which would potentially eliminate the need for PET parameterizations.
- The GCMs and other climate models continue to evolve, and CMIP6 is well underway. If possible, a closer connection to the federal scientists at NOAA GFDL and scholars at the Princeton University Program in Atmospheric and Oceanic Sciences (AOS) may facilitate faster access to cutting-edge models and analysis techniques. Moving forward, the CBP should keep an eye out for other emerging products that could be useful including more detailed regional climate assessments (e.g., 4th National Climate Assessment), high resolution regional climate model projections (e.g., from nesting into global scale model), and integrated assessment model projections for the region of land-use/climate trends.

16. Please comment on the climate change modeling documentation. Is it clear, well organized, concise and complete?

The documentation provided to the panel was scattered across multiple technical reports, briefings, letters, and cited peer-review articles as well as the information webinar [04] slides. While the primary written documents (Appendix B) are generally clear and concise, there are sections of the text that are significantly deficient in detail in their current state. The information webinar [04] and accompanying slides were very informative, providing additional critical background information, graphics, and clarifications on the incorporation of climate change projections into the CBP modeling process. For example, the webinar materials went into more detail that the original written documentation on the RCP8.5 results, as well as model skill and uncertainty. The panel's recommendation is to integrate the written documentation, in particular documents [01], [02], and [03] into a single document and fold the information webinar [04] materials into the written documentation prior to the next steps in the process. Particular attention should be paid to clearly communicating which historical baseline is used to assess climate change magnitude and, where feasible, magnitude of change already present in this baseline relative to a pre-industrial baseline (to avoid the shifting baseline problem).

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Appendix A. Review Request

Chesapeake Bay Program Partnership's Climate Change Assessment Framework and Programmatic Integration and Response Efforts

Request for STAC Peer Review 06.30.17

The Chesapeake Bay Program (CBP) partnership is undertaking a midpoint assessment of progress to ensure that the seven Chesapeake Bay watershed jurisdictions are on track to meet the 2025 Chesapeake Bay Total Maximum Daily Load (TMDL) goal. A key element of this effort is the incorporation of the latest climate science, data, tools, and BMPs into the partnership's decision support tools to help guide implementation and to use this new information to facilitate and optimize implementation of the jurisdictions' Watershed Implementation Plans (WIPs).

The CBP's Scientific and Technical Advisory Committee (STAC) has conducted several assessments of climate science and recommended processes to integrate the consideration of climate change into the Bay Program's management framework (DiPasquale, 2014; Johnson et al 2016; Pyke et al 2008; Pyke et al 2012; STAC, 2011; Wainger, 2016). These reviews and recommendations assessed the latest climate science and impacts to the Chesapeake Bay watershed and highlighted the need to more effectively embed climate change among partnership goals in decision making, identify and prioritize vulnerabilities of restoration efforts and management actions, and utilize partners' ongoing research efforts to better assess and evaluate responses to changing climatic conditions.

Along with culminations of past STAC assessments as well as stand-alone peer reviews of the general approach to incorporate projected 2025 and 2050 climate change variables into the Watershed Model (WM) and estuarine Water Quality and Sediment Transport Model (WQSTM) modeling processes (currently underway), the Modeling and Climate Resiliency Workgroups request a more thorough evaluation of the Partnership's climate change assessment framework and plans for incorporating climate change into programmatic efforts.

Questions for STAC Peer Review:

Question 1) Please comment on the overall approach to incorporate projected 2025 and 2050 climate change into the Watershed Model and Water Quality and Sediment Transport Model.

Question 2) How well do the global circulation models used for producing 2025 and 2050 climate scenarios show skill in hindcasting the actual climate and hydrological changes that have happened in the Chesapeake Bay watershed over the past decades?

Question 3) Please comment on the appropriateness of the methodology to account for uncertainty in 2025 and 2050 climate projections.

Question 4) Please comment on the CBP's use of multiple Representative Concentration Pathways (RCP's) and their associated 10^{th}, 90^{th} percentiles and the median projections to derive 2025 and 2050 temperature estimates and 2050 precipitation estimates?

Question 5) Please comment on the CBP's selection of the downscaling approach, Bias Corrected Spatial Disaggregation (BCSD) downscaling methodology to derive 2025 and 2050 temperature estimates and 2050 precipitation estimates?

Question 6) Is the interpretation of downscaled climate data from a gridded product (⅛° resolution) to a county-scale within the Watershed Model sufficient to represent changing climatic patterns and assess load responses at a larger regional scale?

Question 7) Given limitations of modeling resources, policy and governance, is the applied Delta Approach for precipitation, temperature and evapotranspiration adequate to represent a range of potential changes in climatic forcing variables? Are there limitations in the ability to capture potential variability of precipitation intensity, temperature swings, or timing of extreme events (e.g., storm occurring early in growing season vs. late fall) that would affect the ability to assess the impact of less probable but higher magnitude events (e.g., Hurricane Isabel)?

Question 8) Is the use the Karl and Knight (1998) estimates of precipitation intensity appropriate for modifying 2025 precipitation intensity? Is it sufficient to apply these estimates to the entire watershed based on their central Mid-Atlantic derived trends?

Question 9) The models (both the old P5 and new P6 versions) use a 10-year average hydrology for the simulation. The 10-year period that is used is 1991-2000. The TMDL and planning targets are also based on a hydrologic critical period (1993-1995) for meeting WQ standards. With the latest information we have about climate science and given the methods that being used to incorporate changing temperature, precipitation, and sea-level into the models, are these periods still appropriate, when the hydrologic averaging period is 17 years old and the critical period is 23 years old?

Question 10) Was the use of a modified Hargreaves-Samani evapotranspiration methodology sufficient to capture expected changes due to projected temperatures? In addition, should other ET methodologies be considered to develop a comparison of ET estimates?

Question 11) Please comment on the appropriateness of the methodology to select 2025 and 2050 sea level rise scenarios for application in the WQSTM?

Question 12) Given limitations on available data sets and modeling products, as well as uncertainty about how wetlands within differing geographies may adapt to changes in sea level over-time, please comment on the appropriateness of the methodology to project 2025 and 2050 tidal wetland change?

Question 13) Does the applied methodology reflect the latest and best scientific understanding of the influence of climate on watershed processes and estuarine responses; is there any additional scientific information that should be included?

Question 14) Many of the plans to incorporate climate change into programmatic efforts are using more qualitative information. To what extent is there reliable quantitative information on which land uses and BMPs are going to be impacted by climate change? Is there quantitative information on modification that can be made to land use and BMPs that are effective in addressing climate change?

Question 15) For longer term CBP considerations, how can the overall approach and procedures be improved and what alternative approaches and data would be recommended?

Question 16) Please comment on the climate change modeling documentation. Is it clear, well organized, concise and complete?

Appendix B. List of Primary Review Materials

Reference #	Document Title	Location of Materials
[01]	Preliminary Phase 6 Watershed Model (WSM) and Chesapeake Bay Water Quality Sediment Transport Model (WQSTM) Climate Change Assessment Procedures for the 2017 Midpoint Assessment. STAC Peer Review Documentation; 06.30.17 Draft.	http://www.chesapeake.org/stac/presentations/279_CCAF_STACPeerReviewDocumentation_Draft_063017.pdf
[02]	The Development of Climate Projections for Use in Chesapeake Bay Program Assessments. STAC Workshop Report; March 7-8, 2016 Annapolis, MD; STAC Publication 16-006.	http://www.chesapeake.org/pubs/360_Johnson2016.pdf
[03]	Recommendations on Incorporating Climate-Related Data Inputs and Assessments: Selection of Sea Level Rise Scenarios and Tidal Marsh Change Models. Climate Resiliency Workgroup; August 5, 2016.	http://www.chesapeake.org/stac/presentations/279_CRWG_SLR_climate_data_recommendations_final_080516.pdf
[04]	Chesapeake Bay Program Partnership's Climate Change Assessment Framework and Programmatic Integration and Response Efforts. STAC Peer Review Webinar; August 8, 2017	http://www.chesapeake.org/stac/presentations/279_STAC_PeerReviewClimateChangeWebinar_080817.pdf

Additional Review Materials Provided:

Chesapeake Bay TMDL 2017 Mid-Point Assessment: Guiding Principles and Options for Addressing Climate Change Considerations in the Jurisdictions' Phase III Watershed Implementation Plans

CBP Climate Resiliency Workgroup
Briefing Document - 12/13/16
http://www.chesapeake.org/stac/presentations/279_Briefing%20Document_climate_options_for_phase_iii_wips_12.13.16.pdf