On Critiques of "Stationarity is Dead: Whither Water Management?"

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Abstract We review and comment upon some themes in the recent stream of critical commentary on the assertion that "stationarity is dead," attempting to clear up some misunderstandings; to note points of agreement; to elaborate on matters in dispute; and to share further relevant thoughts.

1. Introduction

Anthropogenic climate change (ACC) is occurring and growing in influence. Basic thermodynamics tells us that warming associated with ACC must have hydrologic implications [*IPCC*, 2014] due to temperature-driven changes in such characteristics of the water cycle as precipitation type and amounts of water stored in snowpacks, the water-holding capacity of the atmosphere, amounts of precipitable water in the atmosphere, rainfall intensities, and evapotranspiration rates, as well as infiltration rates and runoff rates on various time scales, with presumed implications for droughts and flooding. Additional impacts of ACC on the water cycle are driven by systematic shifts in atmospheric circulation, affecting the geographical distribution of both precipitation and radiation, the two main drivers of land water balances.

Many water professionals are struggling with the question of how to account for ACC in hydrologic design. In view of the worldwide hydrologic change presumed to result from ACC, *Milly et al.* [2008] asserted that "stationarity is dead." This assertion has been echoed widely in the water community, and beyond: in ecology [*Wolkovich et al.*, 2014], in conservation and natural resource management [*Wiens et al.*, 2012], and in environmental and natural resource law [*Craig*, 2010; *Ruhl and Salzman*, 2013]. Investments are being made (indeed, were being made even before publication of *Milly et al.* [2008]) to account in some way for ACC in water planning [*Klijn et al.*, 2004; *Kundzewicz et al.*, 2008; *Obama*, 2015]. However, neither those echoes nor those investments should be accepted as scientific evidence of the validity of the proposition that "stationarity is dead." In fact, the proposition has been prominently questioned in a series of thoughtful papers presenting an opposing viewpoint [*Lins and Cohn*, 2011; *Matalas*, 2012; *Koutsoyiannis and Montanari*, 2014; *Montanari and Koutsoyiannis*, 2014]. In view of the importance of this topic and the apparent differences within the literature, herein we review and comment upon some themes in the recent stream of critical commentary, attempting to clear up some misunderstandings; to note points of agreement; to elaborate on matters in dispute; and to share further relevant thoughts.

2. Exposition of Our Terminology

Many hydrologic time series can be viewed (i.e., modeled) as combinations of deterministic (including periodic) and stochastic components. Such combinations obtain randomness from their stochastic component(s), so those combinations are also stochastic. For a very simple example, if $a(t)$ and $b(t)$ are deterministic and $z(t)$ is stochastic, then $Z = a + bz$ is also stochastic. The functions $a(t)$ and $b(t)$ might be determined by anthropogenic forcing of a system, while $z(t)$ might be the result of chaotic nonlinear internal dynamics and unpredictable natural external forcing of the system. Stationarity is a property of a stochastic process; we understand a process to be stationary if and only if the joint probability distribution

function (pdf) of its state is invariant over time. A (necessarily stochastic) combination of deterministic and stochastic components (e.g., Z) can be nonstationary even though its stochastic component (z) is stationary. Indeed, many useful nonstationary stochastic models of hydrologic processes decompose the hydrologic signal into deterministic (where change is encapsulated) and stationary stochastic parts. Such decomposition allows application of statistical tools based on stationarity in the characterization of the stationary stochastic component(s) of observed time series.

Koutsoyiannis and Montanari [2014] assert that *Milly et al.* [2008] misused the term "stationarity." In view of the considerations laid out above, we think it is valid to say that, as a result of ACC, representations of hydrologic processes in the 21st century generally need to consider nonstationarity. In fewer and simpler words, suitable for the title of a *Science* "Policy Forum," and readily accessible to the reader (scientist or non-scientist), "Stationarity is dead." We doubt that many readers misunderstood our message, and we suspect that the simplicity of the title helped to convey the message.

3. ACC in the Historical Hydrologic Context

Planning related to water has always depended (and always should depend) on assessment of past data to inform projections of future conditions. Over the past century or two, for a number of reasons, water professionals involved in planning needed to introduce into their process descriptions various changes in the deterministic components, the stochastic components, or both. Thus, hydrology has a long history of introducing concepts of nonstationarity to address a variety of human actions, including land-use and land-cover change (e.g., deforestation, urbanization); land drainage; dams, diversions and water withdrawals; and groundwater depletion. In such cases, the impact on mean values, seasonality, and variability on a full range of time scales from subseasonal to multiyear at the downstream end of a watershed (or larger area in the case of groundwater development) can be projected using information about that individual watershed and a toolkit of science and engineering methods—often developed in other watersheds–that have become relatively well understood. In addition, the quality of this kind of forecasting capability can be reasonably assessed through the use of hindcast studies or results from other catchments.

Consideration of ACC's influence on hydrologic variables is an entirely different type of problem, because we do not have "tried and true" analytical tools for treating ACC, and because the relevant "control volume" of interest (i.e., to be simulated) is no longer confined to a single watershed (or somewhat larger area), but rather is global, even if the impact being considered is of watershed scale. Furthermore, we do not have other globes on which to experiment and from which to gain experience, nor is the time scale of ACC short enough or its signal strong enough for us to observe it well before managing it. In their visionary work, *Revelle and Suess* [1957] made the following observation about release of CO_2 by burning of fossil fuels: "Human beings are now carrying out a large scale geophysical experiment of a kind that could not have happened in the past." It is a one-of-a-kind event, which simply does not fit the template of our historical encounters with hydrologic nonstationarity, nor has such rapid change in greenhouse-gas forcing been found in the paleo-record.

This uniqueness and immensity of ACC as a driver of hydrologic change—its grand space and time scales and its pervasiveness within the water cycle—led us to state that "stationarity is dead" due to ACC. It is expected to influence processes even in the most remote corners of the earth, where humans may be exerting virtually no local effect on the local hydrology.

4. Discussion of Critiques

Matalas [2012] stated that nonstationarity implies absence or secondary status of stationary random components. As can be understood from the exposition of our terminology and the example process Z, we do not see why this would be. And *Montanari and Koutsoyiannis* [2014] say that use of a nonstationary model does not "allow" one to get rid of stationarity. We agree that the stationarity concept is useful in the characterization of the z component of Z. One might counter that this implies stationarity is very much alive and well. This is a matter of semantics, but it seems clear from the context (e.g., reference to annual streamflow as an example of the process), that *Milly et al.* [2008] were declaring the death of the stationarity of stochastic representations of such bottom-line variables as annual streamflow (an example of a nonstationary

process, analogous to Z), rather than the expiration of a useful analytic concept (statistical tools useful for characterizing a stationary process, z).

Thus, we have no fundamental argument with the statement [*Montanari and Koutsoyiannis*, 2014] that "any hydrological model . . . is affected by uncertainty and therefore should include a random component that is stationary." We do not presume "perfect knowledge" of hydrologic systems–far from it–nor do we "deem future scenarios obtained with deterministic [climate] models [to be] credible predictions of the distant future." In view of the wide disagreement among climate models, their collective bias, their imperfect parameterizations of unresolved processes, and their chaotic sensitivity to initial conditions, we understand physical climate-model projections to be samples of possible futures, subject to errors, and conditioned on assumed trajectories of atmospheric composition. Many uses of such projections in making a decision will need to include a reasonable estimation of the statistical properties of those errors and estimates of the probabilities of those trajectories. Other uses may explore selected examples or constructed scenarios from the outer limits of the available projections without assigning specific probabilities (e.g., along the lines of *Stern et al.* [2013] and *Vermeulen et al.* [2013]) in order to identify vulnerabilities and system failures that need to be avoided regardless of the probabilities. In either case, there is still value in the projections even with their uncertainties, if the uncertainties are acknowledged and accommodated.

Admission of the uncertain nature of climate projections does not preclude their use in a nonstationary model applicable in the future. Such a model could be viewed as the combination of a stationary component, representing the process in the absence of ACC, and an ACC component that contains both deterministic and stochastic parts, representing climate-model (or other deterministic) projections and their uncertainty, respectively.

It has been noted that "long-term persistence" [*Lins and Cohn*, 2011], "long memory processes" [*Matalas*, 2012], or "conditioning" [*Koutsoyiannis and Montanari*, 2014] can contribute to a stationary process being mistaken for a nonstationary one, as a result of sampling error. If the possibility of long-term dependence is allowed, it can be exceedingly difficult to overturn a null hypothesis of stationarity on the basis of observed data alone [*Villarini et al.*, 2009]. Even so, if one accepts the prevailing scientific view of ACC, it follows from thermodynamics that the water cycle is now undergoing fundamental change, albeit one whose structure is poorly known. We find ourselves in a situation where the science suggests a substantial and growing ACC signal, yet the observable change may currently be indistinguishable from the chaotic internal variability and naturally (e.g., volcanically) forced variability of the climate system. However, because the ACC effects are growing in magnitude, they cannot readily be assumed to be negligible over the decades-long design horizon of engineered water systems. In such a situation, because there is reason to suspect that a trend exists, one should be sensitive to the substantial possibility of type-II errors: the probabilities of failing to recognize a trend––a signal hidden amidst overwhelming noise––when it does actually exist. Arguably, the world is in such a situation now. *Vogel et al.* [2013] point out that the type-II error "informs us about the likelihood of whether or not society is prepared to accommodate and respond to" a trend that might not yet be detectable by the usual null-hypothesis significance test.

It has been noted [*Montanari and Koutsoyiannis*, 2014] that use of a nonstationary model adds parameters and thereby increases estimation variance. We agree, but should we ignore the uncertainty introduced by climate change, just because it is difficult to quantify? Should the enormous body of research on ACC amount to nothing when water decisions are made? It is not our suggestion that ACC effects should be identifiable from the observational record alone; this would be quite a challenge [*Serinaldi and Kilsby*, 2015]. Rather, we envision a major role for estimates of ACC trends that are informed by combination of climate theory, models, empirical analysis of hydrologic data, and expert opinion.

Montanari and Koutsoyiannis [2014] also state that "Reliability of [deterministic model] projections is a necessary condition for obtaining less biased estimates, and therefore better defined mitigation policies for environmental risks." The difficulty with this statement is that "reliability" is a relative term. No model is perfectly reliable even for changes that are as straightforward as those created by urbanization. We suggest that the current generation of global climate models and their linkage with hydrologic models remain uncertain and insufficiently tested. But just because those models don't achieve a high level of "reliability" by some measure, it does not follow that the processes that they were designed to represent should be ignored in the planning process. We would argue that assessment of the reliability of these current models

(with respect to hydrologic variables) is an important goal for the hydrologic sciences, and this can only be accomplished through the use of accurate data and innovative data analysis approaches that are mindful of the nature of stationary but highly persistent stochastic systems. This assessment of reliability of the models (with respect to hydrology) involves not only assessing the biases of modeled mean values, but at least as importantly assessing the variances, the measures of persistence, and the presence of trend-like behaviors of accurate hydrologic data as compared to the outputs of models driven by the historic record of greenhouse forcing.

It is perhaps worthy of note that commentators apparently acknowledge the possible utility of a nonstationary stochastic model to account for ACC. *Lins and Cohn* [2011] suggest that (italics our own) "one might want to recognize the *increased* uncertainty that potential anthropogenic influences on climate change introduce," while *Koutsoyiannis and Montanari* [2014] allow that (italics our own) "In absence of credible predictions of the future, admitting stationarity (and *larger* uncertainty) provides a more consistent and more effective modeling option [than nonstationarity]." We understand both of these suggestions, in essence, to imply the assumption of a deterministic temporal change in the pdf (specifically, the second moment) of a random process (evidently, from historical observations to future analysis period). As defined in the exposition of our terminology, the temporal change in the pdf constitutes a nonstationarity in the stochastic process.

We agree with our critics on the importance of data. We have very little knowledge about what the outcome of ACC will be. Precisely for this reason we stated the following [*Milly et al.*, 2008]: "In a nonstationary world, continuity of observations is critical." The idea here is that we must use long-term hydrologic observations to help us evaluate, on an ongoing basis, how the changing atmosphere is changing hydrologic processes.

5. Concluding Remarks

We reiterate that "now is an opportune moment to update the analytic strategies used for planning [water] investments under an uncertain and changing climate" [*Milly et al.*, 2008]. Finding approaches for dealing with the qualitatively different situation presented by ACC during the so-called "Anthropocene" [*Crutzen*, 2002; *Steffen et al.*, 2015] is a major challenge. These approaches must combine various tools that are at our disposal, including deterministic, process-based science, and stochastic methods. We agree with *Montanari and Koutsoyiannis* [2014] that the way forward must "bridge the gap between physically based models without statistics and statistical models without physics." We acknowledge well-founded concern in the community that wholesale abandonment of accepted tools and techniques in favor of speculation and untested and (or) poorly understood methods could introduce substantial risk of far-from-optimal solutions (i.e., gross overdesign or catastrophic system failures). In this regard, resorting to an entirely deterministic approach relying on one or more climate-model projections is a far-fetched idea that we neither proposed nor advocated. Neither did we suggest that the parameters of relevant nonstationary stochastic models could be identified solely on the basis of historical observations. Rather, we need an approach that is informed by advances in understanding and computation regarding the coupled global land-ocean-atmosphere system and by the pursuit of empirical analysis that is aimed at better evaluation of the deterministic and stochastic components of the hydrologic system we observe.

We do not suggest that the "suitable successor" [*Milly et al.*, 2008] to stationarity has been found. Rather we continue to regard finding such a suitable successor as a major challenge to the community of hydrologists and water resource engineers.

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