



## Climate, water, and conflict: Commentary on Selby et al. 2017



Peter H. Gleick

Pacific Institute, 654 13th Street, Oakland, CA 94612, United States

### ARTICLE INFO

#### Article history:

Received 18 June 2017

Accepted 23 June 2017

Available online 30 June 2017

I appreciate the effort of the authors of this paper (hereafter [Selby et al., 2017](#)) to dig into the complicated and important subject of the connections among climate, extreme hydrologic events, social and political disruption. Unfortunately, the paper is badly flawed, logically inconsistent, and fails to support its fundamental hypothesis, which is that there is insufficient evidence to support a link between climate change, the severe drought that afflicted the Eastern Mediterranean region over the past decade, and subsequent economic, social, and political disruption, especially related to the Syrian civil war.

Part of the problem appears to be a determination on the part of the authors to refute rather than carefully critique the work of several previous papers, especially ([Kelley, Mohtadi, Cane, Seager, & Kushnir, 2015](#)) and – for full disclosure – one of mine ([Gleick, 2014](#)). Some of the specific criticisms they level against those other papers are useful and should help future assessments, but too often they push their own arguments past the point where the facts support them. At the invitation of the journal's editor, I was asked to provide two separate reviews of versions of this paper. Some of my specific concerns were addressed, and the paper is improved, but major flaws remain, and the editor solicited this commentary on the final version.

The authors focus their work on analyzing three key themes. In their own words, assessing whether:

1. “anthropogenic emissions of greenhouse gases contributed to Syria's drought;
2. this drought led to large-scale migration; and

3. this drought-related migration was an important factor in Syria's early unrest.”

A wide series of studies published over the last three to five years have evaluated portions or variants of these, but a fundamental flaw in the current paper is precisely the wording and assumptions behind the last two of these questions, as well as their assessment of the first question. For example, asking whether or not the drought “led” to large-scale migration is a strawman argument. The focus of much of the previous literature is on whether the drought “contributed” to migration, not whether it caused (i.e., “led to”) it, and there is acknowledged ambiguity in the extent of that influence. This is addressed further, below. Similarly, the authors focus their analysis of the third question on whether internal migration was “an important” factor in the unrest in Syria, but “important” is a subjective, unquantifiable term.

If the questions are rephrased more along the lines of the questions addressed in earlier studies, the authors would have asked whether:

1. anthropogenic emissions of greenhouse gases contributed to Syria's drought (“climate change-to-drought”);
2. the drought add to/contribute to migration within Syria (“drought-to-migration”); and
3. population displacements contribute to Syria's early unrest (“migration-to-conflict”).

Posed this way, the academic literature is extremely clear, answering “yes” to all three points.

That literature is part of a rapidly growing body of evidence addressing the broader issues of whether anthropogenic climate changes are influencing extreme events, such as floods and

E-mail address: [pgleick@pipeline.com](mailto:pgleick@pipeline.com).

droughts, and whether, as a result, some of the consequences of those extreme events – including social and political unrest and violence – can now be identified as explicit climate impacts.

Returning to the focus of their three questions, the authors begin by taking issue with the substantial and growing body of evidence that explores the influence of climate changes on extreme hydrologic events using climate models, statistical assessments of hydrologic extremes, and regional hydrologic analysis (i.e., “climate change to drought”). Despite difficult and well-catalogued issues associated with data limitations, the natural variability of climate, and imperfections in climate models, several key papers have concluded that the severe drought that affected the watersheds of the Tigris and Euphrates rivers in Turkey, Syria, and Iraq fell outside of the range of conditions that can be explained by natural variability alone (Hoerling et al., 2012; Kelley et al., 2015) and that further, when the influence of climatic factors are included, they better explain the drought’s severity.

The key argument Selby et al. use to dispute the climate-drought link relies on an overly narrow definition of drought – focused only on precipitation. The Syrian drought was actually manifest through changes in rainfall, runoff, temperature (which determines water demands, especially by agriculture), groundwater levels, soil moisture, and other factors. Yet even using their narrow focus on rainfall, data from precipitation stations identified in the authors’ own citations show clear indications of severe drought and strong evidence the severity of the drought was related to human influence on climate.

It is worth noting that the literature supporting the severity of the Eastern Mediterranean drought and its links to climate change continues to expand. This is described in more detail in the other invited commentary that accompanies this article. For example, a new analysis by (Cook, Anchukaitis, Touchan, Meko, & Cook, 2016), not cited by the Selby et al., states:

“we conclude that there is an 89% likelihood that this drought [in the Levant/Eastern Mediterranean region] is drier than any comparable period of the last 900 years and a 98% likelihood that it is drier than the last 500 years. These results confirm the exceptional nature of this drought relative to natural variability in recent centuries, consistent with studies that have found evidence for anthropogenically forced drying in the region.”

On the second question (“drought-to-migration”), the authors first acknowledge but then dispute the scope of the role played by the drought in contributing to internal population displacements in Syria. As noted above, their strawman argument is to test whether the drought “led to” “large-scale” migration. They repeat this strawman when they state, “At the very least, it should not be assumed that drought was the only, or dominant, catalyst of migration.” They raise the possibility that the population disruptions seen in Syria may have been the result of institutional or regulatory changes in the agricultural sector and they provide some interesting new information on these factors. But this is not a new idea, nor is it ignored by other studies. Earlier assessments do not typically argue the drought was the “only” catalyst of migration; and most do not claim it was “dominant” – only that there was influence. There is legitimate uncertainty around the “scale and importance” of that connection, but the evidence for a connection remains strong.

Moreover, several of the factors the authors postulate are other drivers of migration are also drought related. For example, they note that wheat production dropped because of lack of surface and groundwater for irrigation – precisely the kind of impact we also see related to drought. This strengthens rather than weakens the drought-migration argument.

Another problem with their assessment of the drought-to-migration connection is that the authors cherry-pick evidence that supports their position while ignoring comparable evidence that doesn’t. The authors describe interviews conducted with 30 Syrian refugee families that draw no links between drought and population displacement. Such interviews are interesting, but they have no validity scientifically (or from a political science perspective) in whether or not other groups had a different experience, or whether the drought-migration connections are real. How were they chosen? How representative are they of the population? How relevant is the experience of this particular set of people to the kinds of connections described in other studies? Why are their opinions more valid than the opinions and official statements from other individuals quoted in the other papers (such as UN officials and Syrian ministry experts)? These interviews are touted as an important counter to the positions in the Kelley et al. and Gleick papers, but I disagree that they serve such a purpose and they do not constitute quality social science analysis.

Moreover, a growing body of work using similar interviews and social science assessments *do* show such links between water scarcity and population displacements. A 2012 study from the International Organization for Migration provides a detailed survey of Iraqis displaced by conflict who cited water as a key reason for both their displacement and their inability to return home (International Organization for Migration, 2012). This study (while for Iraq rather than Syria) adds on-the-ground support for the idea that water problems can be directly tied to migration and displaced populations.

On the third question, whether the internal migration and population displacement “was an important factor” in Syria’s unrest, the authors similarly fall back on first acknowledging a connection but then disputing the severity or strength of that connection. Here, a key flaw in the paper is their choice of metrics to evaluate the strength of the connections among these factors. They try to parse the difference between whether something is a “significant” cause or a “contributory” factor and judge based on “significance.” The difficulty in this approach is that “significant” is a meaningless term without quantification, but the authors do not attempt to quantify it. Presumably it means less than “primary” and somehow more than “contributory,” but ultimately this is a subjective standard.

At the same time, the authors regularly suggest that they agree that there was some non-zero role or link among these factors. If the only real complaint is a disagreement about the relative contribution of the many, complex factors involved, that is a far simpler and more justifiable paper.

Many of the flaws in the paper can be traced to a fundamental failure to consistently distinguish between “causality” and “influence” related to the role of climate change in the severe drought in the region and the subsequent social and political unrest. While the authors note in a few places that the research studies they critique do not typically claim that climate change “caused” the Syrian unrest, they themselves regularly repeat that very argument as a strawman that they then try to debunk.

In summary, the authors critique earlier works in this field and raise some important and interesting points, but their conclusions do not hold up, their logic is inconsistent, and their main point boil down to a dispute about how “significant” different influences were. Arguing which factors were most or less important may be interesting, though extraordinarily difficult given problems with both data and attribution, but this paper fails to debunk earlier studies that identify that such links existed and fails to provide new insight to how better to quantify these connections.

I hope these criticisms will be seen as an effort to understand and analyze the points and arguments the authors are trying to

make and will lead to more, not less dialogue and discussion about these important issues, especially as climate impacts worsen and widen and are felt in more and more aspects of society and politics.

No conflicts of interest. This commentary was requested by Buhaug (the journal editor), after he also requested my review of the manuscript and a re-review. He was informed that I had a conflict in the sense that the authors were, in part, addressing some of the issues raised in a previous peer-reviewed publication of mine (Gleick, 2014). He acknowledged that and requested a review anyway.

## References

- Cook, B. I., Anchukaitis, K. J., Touchan, R., Meko, D. M., & Cook, E. R. (2016). Spatiotemporal drought variability in the Mediterranean over the last 900 years. *Journal of Geophysical Research: Atmospheres*, 121(5), 2060–2074. <http://dx.doi.org/10.1002/2015JD023929>.
- Gleick, P. H. (2014). Water, drought, climate change, and conflict in Syria. *Weather, Climate, and Society*, 6(3), 331–340.
- Hoerling, M., Eischeid, J., Periwitz, J., Quan, X., Zhang, T., & Pegion, P. (2012). On the increased frequency of Mediterranean drought. *Journal of Climate*, 25(6), 2146–2161.
- International Organization for Migration. (2012). *IOM Iraq special report: Water scarcity*. International Organization for Migration. <http://reliefweb.int/map/iraq/iom-iraq-special-report-water-scarcity>.
- Kelley, C. P., Mohtadi, S., Cane, M. A., Seager, R., & Kushnir, Y. (2015). Climate change in the fertile crescent and implications of the recent Syrian drought. *Proceedings of the National Academy of Sciences*, 112(11), 3241–3246.
- Selby, J., Dahi, O. S., Fröhlich, C., & Hulme, M. (2017). Climate change and the Syrian civil war revisited. *Political Geography*, 60, 232–244.