As an opinion piece, it is perhaps less useful to evaluate this paper on its scientific merits, but rather, its ability to make its case and the need for that case to be made.

Within the abstract, lines 9-13 inform the reader of what this paper intends to accomplish. Four objectives are outlined:

1. "…examine the history…"
2. "…discuss the benefits…"
3. "…examine some practical challenges…"
4. "…provide perspectives on issues…"

As this paper is, at its core, a compilation of qualitative arguments rather than quantitative results, it seems most appropriate to assess the efficacy of this paper in terms of the four objectives outlined by the authors.

1. Examining the History

Beginning by arguing that with a sufficiently narrow spatial and temporal sample and sufficient parameters, nearly any model can perform well, this paper does enumerate applicable papers outlining the need to incorporate more nuanced features as well as the PUB initiative of the past decade. Pg. 9149, line 19 argues, "what is needed is to begin taking advantage of extensive datasets…" However, given the multiple works published in HESS in the fall of 2012 (Cheng et al., Ye et al., Coopersmith et al., and Yaeger et al.) attempting to draw broader insights from 400+ diverse catchments within the MOPEX database, this comment seems to ignore the work of numerous younger hydrologists over the past few years.

The authors rightfully discuss the previous computational challenges associated with calibration or cross-application. Numerous young researchers are attempting to address these issues and the authors' plea is encouraging.

Between pg. 9152, line 16 and pg. 9153, line 10, the authors discuss Linsley (1982) and Klemes (1986). These arguments largely rehash what is stated when these two papers are cited within the introduction. Unlike a standard research paper in which an idea is mentioned briefly in an introductory section, then expounded upon as quantitative analyses are performed, this paper is an opinion piece (and a somewhat long one at that). Passages like the one described above are somewhat redundant and should be shortened or removed.

Section 2.2, a pseudo-literature review, either requires substantial additions, or more likely, a shortening and relocation towards the introduction. It lists a variety of modeling studies from the 1980s, then mentions the PUB initiative, but skips the various classification studies that occur between them. Mentioning MOPEX, DMIP, and NLDAS as potential national-scale data sources is important in terms of their facilitation of large-scale studies (though they are biased perhaps in terms of their focus upon those regions of the globe that are more affluent and more densely populated (North America, Western Europe, etc).

The authors rightly describe the prevailing trends to eschew "data fitting" (pg. 9156, line 18) in favor of understanding developed by signatures. However, as mentioned previously, the works published in HESS this past fall do exactly this, focusing on the flow duration curve (Cheng et al), the annual regime curve (Ye et al; Coopersmith et al), and the hydrologic relationship between the two (Coopersmith et al; Yaeger et al).

2. Discussing the Benefits

This is the objective most convincingly achieved within this paper. The authors, omitted papers notwithstanding, nicely frame the importance (pgs. 9157-9160) of large scale studies in terms of the holistic understanding, generalization, cross-applicability of models, and uncertainty estimation they facilitate. These four pages are perhaps a bit longer than they need to be, as some of these concepts are discussed earlier in the paper and/or discussed thereafter. However, in terms of making one's case for large-scale studies, this is the strongest piece herein.

3. Examining Practical Challenges

This is also an objective that is relatively effectively achieved by this paper. The need for data availability is well-described, though perhaps some redundancy occurs in the re-introduction of MOPEX and DMIP. The authors correctly note that these large hydrologic datasets are focused upon inputs and responses rather than the underlying subcomponents. I might argue that MOPEX, with its geologic, edaphic, vegetation coverage, and numerous other features, makes some attempt at unearthing these insights. One might point towards NASA's upcoming SMAP (remotely sensed soil moisture) or GPCP (global precipitation) missions as potential sources of widely-available data for hydrologists in the years to come.

The notes regarding appropriate reporting of data quality and errors therein is an important discourse – probably one could write an article exclusively focused upon this issue. The issues associated with soil texture and topography are appropriately mentioned, though perhaps the potential solutions should be discussed in terms of national soil databases (SCAN, SoilWeb from the USDA, etc) and elevation datasets becoming increasingly ubiquitous through LiDAR data, etc.

4. Providing Perspective

Personally, I agree with the general opinion espoused – more broadly-scoped hydrologic research is tremendously important. However, the perspective provided in the introduction argues (pg. 9149, lines 23-24) that "the context of much current hydrological practice is a focus on *depth* rather than breadth" continuing to discuss the development of generalized models where parameters are specified as a function of location. In turn, this leads to the "uniqueness of place" reference from Beven (2000) and the challenges associated with generalization. This perspective seems to caution against the idea that one conceptual framework can be applied at a diversity of locations, though this is, to some extent, the "holy grail" of which the authors speak. The authors rightly note that this challenge is increased when additional features are incorporated in the name of realism, but fail to mention that it is likely that the "holy grail," were it ever to be found, is likely to be a relatively simple model of the type they warn against (one we try to apply everywhere).

With regards to the specifics of the subsection associated with perspectives, the first section discusses barriers to sharing data. This is a real concern, but seems more aligned with the previous section's discussion of practical challenges. The next subsections discussing hydrologic processes and the need for classification are somewhat redundant and again, omit numerous recent classification papers. Section 5.4 also seems a bit redundant, but section 5.5 is a very important point that requires substantial attention.

Overall

This is a well-written manuscript raising several issues worthy of attention within the hydrologic community. However, as this is an opinion paper (and not a research article), the length seems a bit unwieldy, especially as numerous arguments therein are either revisited, expanded upon at length or repeated outright. Considering that there are likely to be several papers in the years to come that cite a paper like this one (as it does attempt to encapsulate the highlights of many decades of hydrologic work), it is important that this work incorporate the comparative hydrological work of the past five-to-ten years.

I would advocate that this work, ideally, should be revised to 50-75% of its current length, address some of the newer papers that focus on signatures and/or hydrologic classification, and avoid repeating arguments (however persuasive they may be!).