Review of "*Calculating the average natural recharge in large areas as a factor of their lithology and precipitation*" (doi:10.5194/hessd-8-4753-2011)

GENERAL EVALUATION:

This manuscript is fundamentally flawed. It makes an unreasonable assumption, turns it into a new method, and then mislabels its application (where falsification is neither defined nor rigorously possible) a "test." The manuscript ignores a large body of literature that invalidates its underlying approach on observational and theoretical grounds. The manuscript's interesting results (relation of spring flow to lithology) have already been published. Its new results (application of recharge coefficients to large areas) are empirical, poorly supported, and of little demonstrated value.

The writing style is obscure and difficult to follow. Only by spending considerable effort is it possible to discern the underlying logic. In a convoluted series of steps, previously reported spring discharge in mainland Spain is used to produce lithology-dependent recharge factors for six rock types. The assumed equation has the form $R = c\,P$, where R is groundwater recharge, P is precipitation, and c is the recharge factor (constant) for a given lithology. The final result is a set of six c's that are claimed to be applicable far beyond the calibration area. Multiply the respective lithologic areas and annual precipitation amounts in a target region by the corresponding universal c's, sum the result, and voila—regional recharge is obtained.

The manuscript begins by implying that groundwater recharge = spring flow when changes in storage can be neglected; e.g., for annual periods. This is tantamount to saying that the only form of groundwater discharge is spring flow. This is obviously untrue [*Healy*, 2010], even in karst terrains. In non-karst terrains the implication is even farther removed from reality—groundwater discharges primarily as base flow to streams, wetlands, etc., in humid regions; and as evapotranspiration from shallow water tables on basin floors and as sub-basin outflow, in arid regions [e.g., *Meinzer*, 1923; *Rorabaugh*, 1964; *Maxey*, 1968; *Risser et al.*, 2005].

Eventually, on page 4672 (203 lines into the manuscript!), a single fudge factor is developed to account for the missing recharge. It amounts to a sizeable portion of the recharge even for the karst-skewed sample. The factor should be considerably larger elsewhere (previous paragraph). Yet this same factor is boldly assumed to apply to the world at large.

It is mentioned in passing that in order to accurately apply the method one probably needs to know the answer in advance, to develop adjusted factors. This sort of method is not useful.

The assumption that recharge is a fixed percentage of precipitation at sufficiently large scales has been shown to be false again and again. In dry terrains, virtually all precipitation below a sizeable threshold returns to the atmosphere (as shown in hundreds of Maxey-Eakin studies; closely related to the current study but not discussed). When things get really wet, the infiltration capacity of the land surface frequently is exceeded and additional precipitation runs off (per Hortonian dynamics). Entire low-lying desert countries have measureable precipitation but essentially no recharge [UNESCO; see figure 11 of *Flint and Flint*, 2007 for several North American deserts]. Thus there is a threshold that must be taken into account at the dry end, and an asymptote, or sill, at the wet end (e.g., for tropical island nations).

The new method assumes that co-controlling factors like topography, vegetation, and soils can be ignored because they ‘‘cancel out’’ at large scales (p. 4755), despite a preponderance of evidence to the contrary (e.g., large-scale studies of *Cook and Walker*, which the manuscript mentions but then neglects, and *Scanlon et al.*, 2005, and special collections on the topic, e.g., in *Water Resources Research*—which the manuscript does not mention). No rigorous evidence is presented to support such assumptions, i.e., there is no quantitative analysis where these factors are evaluated and shown to be unimportant. Far from cancelling out, such factors are correlated.

On a basic level, studies of *Watson et al.* [1976] and *Avon and Durbin* [1994] reveal the limitations of recharge reductionism in the context of Maxey-Eakin empiricism. This literature is relevant, but missing from action.

The current method's highly reduced empiricism causes it to produce results that lack significance. For example, the amounts of groundwater recharge estimated by the current method, like those produced by Maxey-Eakin methods, do not dictate groundwater availability. As demonstrated by *Theis* [1940], and many others since, recharge estimates have value only to the degree they provide insight into groundwater dynamics. The when, where, and why of recharge is all important. Groundwater recharge estimates of the sort afforded by the proposed method are simply ‘‘irrelevant’’ for water-resources decisions [*Bredehoft*, 2002].

Extraordinary claims require extraordinary evidence. When purely empirical relations developed for the driest country in Europe are claimed to apply for its wettest parts (Ireland), the only rational response is skepticism. The manuscript rehashes previously published material used in developing the recharge factors at length but fails to provide any of the critical calculations used in the ‘‘test’’ of the new method. The poorly documented (and undocumented) descriptions of the test data (section 3.2) do not suffice in a scientific context. The method's applicability was not demonstrated. Nor was its worth.

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SPECIFIC ISSUES:

The manuscript suffers from inconsistencies, careless errors, and unclear development. Excessive levels of significant figures appear throughout.

Heavy use of materials from prior publications, even one's own publications, raises copyright issues. Entire portions of tables are repeated. Where numbers have changed, it is not clear why. The n = 17305 to 875 logic of pages 4756–7 is unclear. The presentation of previously published work, in addition to making up an undue portion of the
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manuscript, is confusing. Too much bulk in the paper is in tables and verbiage that seem to serve as ballast, rather than to inform.

The assumption that lithology is independent of topography and hence precipitation is illogical (loose alluvium does not make mountains; geology and gravity work the other way around such that permeable alluvium forms lowlands and the hardest and least permeable of rocks form mountains) and contraindicated by the current data. This can be seen by inspection of Figures 2 and 3 in the heavily exploited 2001 paper. Where is the quantitative demonstration? The GIS data are clearly available. Why are they used only for selected empiricism, and not to test operating assumptions?

The Table 1–Table 5 comparison (section 2.4) seems largely circular.

Spain's expansive agricultural areas are intensively irrigated with groundwater. What makes this heavily perturbed system "natural" (per the title)?

Italy, e.g., has significant areas of volcanic rocks. Spain does not. Not one of the coefficients applies.

Climatic patterns (e.g., monsoons, ENSO, AMO, PDO) clearly affect large scale groundwater recharge [e.g., *Pool*, 2005], but disappear in the current approach.

Mentioning clearly flawed assumptions and model failures in fleeting discussions is not the same as treating them in a meaningful way.

Additional issues of style and substance include the mathematical gibberish of the second equation (a summation, not an integral, is called for here; at the risk of seeming harsh the only purpose of the calculus seems to be to impress the uncritical reader); a failure to define terms clearly and use them consistently (e.g., "inflow" versus "recharge" versus "spring flow;" and "surface lithology" versus "lithology") and to clearly name, identify, and give appropriate units as a part of variable definitions); vague and unpersuasive arguments; and problems with typesetting (e.g., subscripts that aren't subscripted properly, a stray period in an inline equation, the space that sometimes is and sometimes isn't between the kilo symbol "k" and the cubed meters "m^3"-- a million-fold difference(!); and other problems with units; missing equation numbers, etc).
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My recommendation to the editors is that the manuscript be advanced no further in anything close to its current form. My recommendation to the authors is to identify and account for all major components of the water budget in the hydrologic systems of interest. Develop conceptualizations that account for system dynamics and structure research accordingly. Results with generality will stem from this approach.

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