

Interactive comment on “Screening of sustainable groundwater sources for integration into a regional drought-prone water supply system” by T. Y. Stigter et al.

Anonymous Referee #1

Received and published: 17 February 2009

General comment

The manuscript deals with a topic of great relevance for planning of sustainable water management options. With regard to a water-scarce region the paper report an interesting groundwater screening as a part of a developing DSS for IWRM. The overall methodology is intrinsically valuable and the scope of the paper is well explained. GW availability estimations and GW flow simulations under different scenarios are adopted for safe yield evaluations. Despite the importance of the topics for regional water resources management the paper the presentation of novel concepts, ideas, or tools, is non-relevant. The paper, though well written, is more close to a technical report than

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to a science paper. It seems not matching with the journal target. Furthermore, most of the findings presented, particularly those related to the GW flow model (including model output and parameterization), are poorly validated.

Detailed comments

The introduction section is well arranged in sub-section 1.1 which expresses the needs for IWRM in the target region. Then in sub-section 1.2 the authors introduce a DSS and an optimization tool which are yet under development. Hence, what is the use of this subsection 1.2? The manuscript is quite too long and some parts seem not necessary and misleading. A similar comment arises for the hydrogeological setting of the region (sub-section 2.2): for the parsimony of the paper the knowledge coming from other studies should be focused on the more recent findings since this is not a hydrogeology paper. In sub-section 1.3 the acronym MPWSS is first reported without expressing its meaning.

In sub-section 2.3 it is not clarified what are the water uses satisfied with the public water supply. Is that only for civil uses or also for agricultural water use? In the latter case probably the MPWSS can only assure part of the total irrigation demand, the remaining part being supplied with groundwater. These aspects of multiple resources and multiple uses have to be properly documented.

The well screening in section 3 is sufficiently reasonable though the concept of the SVI is not innovative (see also authors's references). Consequently the results and discussion (sub-section 3.2) appears as technical report focused on a GW quality assessment study.

The section on sustainability assessment (section 4) is also well organized in principle but the overall results are dependent on the natural recharge evaluation which is absolutely questionable in terms of hydrological process description (Kessler, 1965). This is a crucial term in water balance of semi-arid region, particularly if safe-yield thresholds are pushed up to 60% or 100%. Validation of recharge estimation should be presented

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or at least adequately discussed.

The development of a GW flow model to test the satisfaction of the safe-yield hypothesis is a good point of this paper. Nevertheless the need of inverse modelling to estimate the aquifer hydrodynamic parameters for a sufficient hydraulic head reconstruction cannot prevent to discuss the physical feasibility of the adopted parameterizations. The delineation of homogeneous T sub-regions should be adequately discussed and validated with the hydrogeological information available. In absence of a clear model validation the presented results are no more than a mathematical model output.

Technical comments On page 91 line 23, evaporates should be changed into evapo-rates. Figure 1 is not clear at least at the size suggested by the journal editor, Figure 3 also is not clear; the picture has mixed symbols and shaded polygons. What do white dots mean?

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 6, 85, 2009.

HESSD

6, S129–S131, 2009

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