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eEarth Discussions

Interactive comment on "Use of remote sensing and GIS in mapping the environmental sensitivity areas for desertification of Egyptian territory" *by* A. Gad and I. Lotfy

Anonymous Referee #1

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General Comments

Gad and Lotfy present a paper on integrating spatial data from different sources to map areas susceptible to desertification in Egypt. They follow the methodology proposed by the EC-funded research project MEDALUS. The authors use a variety of concepts and approaches to integrate highly divergent data sources and provide a spatially explicit estimation. It is rightfully claimed that such information may be of high relevance for land managers and planning authorities. Consequently, the topic is relevant and warrants a detailed investigation.

However, the paper is not well-written, both in terms of the level of language and num-

ber of minor or major typo errors, as well as in the way it describes the implementation of general assumptions to concrete data processing strategies. Also, the conclusions appear to be overstated given the level of analysis, data used and results shown.

The following sections provide some points of criticism that partially follow the structure of the manuscript.

Specific comments

-Title

The title tries to combine the MEDALUS terminology with reference to the desertification topic in a rather cumbersome manner, why not reduce it to "...mapping areas sensitive to desertification in Egypt" and introduce the "ESA" terminology in the text or a subheading?

-Abstract

The abstract appears somewhat diffuse in that it mixes general statements, formulae, technical information and some results without a clear logical flow. Following general definitions the authors should start from an identification of the major driving processes to present clearly-defined objectives and justify the adopted methodology in relation to these. This could be followed by a brief reference to the results.

In particular, the concluding paragraph of the abstract is rather vague. Are the authors referring to a spatial or temporal trend? If the latter is the case, it is not obvious how, given the data sources and index calculation, the approach should translate in a quantitative trend. In particular, no temporal information seems to be incorporated, so this statement should be reformulated to match with the actual approach and results. Perhaps the authors are referring to gradients, but this is not clear. Also, the conclusion commenting on the benefit from combining GIS and remote sensing data with regard to "fulfilling needed large computational requirements" is unclear and obsolete.

-Introduction

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As stated before, the authors should clearly identify which are the most relevant processes of desertification in the considered area, as there is a variety of processes and feedback loops that vastly differ between different areas. The identification of relevant processes should determine the indicator used and the methodology applied, which is not obvious in the present form.

The project referred to is called MEDALUS and should be identified as an EC-research project.

-Methodology/Results/Discussion

The following comments refer to both the Methodology and Results/Discussion sections.

The Methodology section only briefly introduces the individual parameters which are integrated into one single index for the three major factors (soil, vegetation, climate), upon which in turn the final index is built. The Results/Discussion section provides short descriptions of individual and integrated results. This is, however, done without given proper credit to the type and quality of input data, pre-processing steps applied, accuracies attributed to intermediate steps, final accuracies etc. Although such analyses may be difficult in some cases, the authors should have least provided an analysis of sensitivity to variations in input data (e.g. with different weights applied) to provide the reader with an idea of the confidence of the attained results. In addition, reference information for subset areas (e.g. from other studies, land use statistics for administrative regions,. . .) might have allowed providing an indication of plausibility.

Furthermore, the time periods/dates for which the individual data are representative remain obscure, although temporal congruence would be the foremost precondition for this type of analysis.

In principle, the approach is based on classifying qualitative information and then further integrating these classes using weighting factors. Yet, there is no indication of the

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manner in which the weights have been deduced. Obviously, choice of the weighting factors eventually determines the results. This applies both at the level of individual factors, but also when calculating the final ESAs under the assumption that each of the three indices contributes equally to the risk of desertification. The authors should provide proper reasoning in this context to make their approach scientifically credible.

In general, each study incorporating remote sensing data needs to state some essential facts about the data use, such as type of data, time/season of image acquisition, pre-processing applied (geometric rectification, sensor calibration, radiometric correction). Each of these steps strongly affects any subsequent analysis. In this particular context, it would be important to know of how many images the mosaic consists, if they all represent the same phenological conditions (a prerequisite for this type of analysis), if data are mono- or multi-temporal (only the latter allowing to safely conclude on different vegetation types), etc. In the present form, the remote sensing-related component of the study is not credible.

-Soil

Apparently, the individual factors were derived from a geological map, remote sensing data and a DEM. This raises several questions that are not addressed by the paper:

When and how was the geological map prepared, what is the scale, how is the quality rated?

How were geological units converted to soil properties?

Deriving textural information from satellite imagery and relating this to soil properties (in particular if only one date is considered?) is a major field of research in remote sensing, entailing considerable complexity. The authors only touch this with one sentence. What kinds of algorithms were used, or was this done based on visual mapping? This needs to be clearly stated in order to allow the reader to assess the quality of the work.

Apparently, the authors used topographic maps and a DEM to map slope. Again, what

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kind of map and DEM (e.g. source, resolution, way of production etc.) were used? Why was it not considered using SRTM elevation data as a consistent data source?

-Vegetation

Similar to the points raised before, a major point of criticism is that a single classification of vegetation classes is used to associate them with different characteristics and weights, without even mentioning the relevant characteristics of the data set (data type, pre-processing, multi- or monotemporal set,. . .) or the classification strategy applied (what approaches were pursued in the “hybrid” approach?). How was the accuracy/plausibility checked and what accuracy parameters were attained (e.g. kappa statistics). This is particularly important as the satellite image appears to represent the only dynamic element in the study, while soil data are based on a map of unclear date, and climate data are potentially based on long-term averages. Also, the authors should justify why they attributed a cover value for different vegetation classes, in spite of the availability of a variety of approaches to derive vegetation cover as a quantitative value (Spectral Mixture Analysis, Tasseled Cap, other Vegetation Indices,. . .).

-Climate

The authors name the number of stations, but their distribution is equally important. As interpolation of climate data can be a major scientific challenge, it would be essential to know about the interpolation method used, how reliable the records of the stations are, what period they cover at what resolution etc. Depending on the overall length, was the overall time-series used or only a certain interval? This is important to know, as longer series may themselves incorporate elements of recent change in general climatic properties (if existent). As an example for the above mentioned influence of the way the factors are weighted and integrated, figures 11 and 12 may serve. They provide very distinct spatial patterns, which are not discernible anymore in the resulting overall map. This would either point to the fact that relative weights are inappropriate, or that these factors do not contribute to desertification susceptibility. If the latter is the

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case, this should be discussed.

-Environmentally sensitive areas

Again, the way in which the individual factors are combined (equally weighted) needs to be critically discussed and justified in relation to the region of investigation. Some sentences are cumbersome and hence hard to understand. For instance, a region can not be sensitive to an index (p50/20).

-Conclusions and recommendations

Overall, I believe it is daring to call a rather simplistic classification/reclassification and intersection approach “quantitative” and rate it to be advantageous to descriptive approaches. Although this is a well-established mapping procedure, it is still rather qualitative when compared to actual spatially explicit mathematical models, despite it yielding numbers. Notwithstanding, the link to processes advocated in the introductory section only dimly appears in the conclusions. In this respect, and given the different points of criticism pointed out before, these seem to me overstated and not supported by the preceding analysis and results. Also, results are merely described in terms of area associated with different classes. There is no distinct spatial differentiation beyond major physiogeographic patterns of Egypt that appear reproduced by the approach and only some broad categories are presented and discussed. Again, the language in this section is sometimes ambiguous and somewhat confusing (e.g. “the merely quantitative aspect of desertification sensitivity demonstrates a clearer image”).

Technical comments

The quality of the figures should be improved, the maps are partially very coarse, map layout differs although the same area is covered, and there is no consistent legend.

There is a large number of typos and stylistic flaws of the text. The authors should refrain from their overly use of the term “rather”, which is often applied in an inappropriate manner. Below; some errors and typos are listed, but these are not exhaustive

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and a thorough revision of the manuscript is essentially required.

P41/1 ‘art’ should read ‘article’

P41/4 ‘consequence” should read ‘consequences”

P41/6 wrong blank character

P41/14 ‘algorism” should read ‘algorithm”

P41/22 ‘soil” should read ‘soils”

P42/4 ‘space data” should read e.g. ‘satellite imagery”

P42/4ff. This sentence is without meaning

P42/12 ‘MEDLUS” should read ‘MEDALUS”

P42/12 brackets not closed

P42/15 ‘loss of soil nutrient status”, delete ‘status” to read ‘loss of soil nutrients”

P43/27 introduce ‘is” to read ‘while there is still time. . . ”

P46/6 ‘image” should read ‘images”

P50/5 two points after sentence

P50/9 ‘The hyper arid. . . ” should read ‘Hyper-arid . . . ”

P50/12 ‘driven together”: awkward formulation

P50/23 ‘to plane” should read ‘to plan”

Figure 11: ‘precepetation” should read ‘precipitation”

Figure 12: ‘evap-transpiration” should read ‘evapotranspiration”

Interactive comment on eEarth Discuss., 3, 41, 2008.