**Atmospheric
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Interactive comment on "Technical Note: Minerals in dust productive soils – impacts and global distribution" *by* S. Nickovic et al.

K. Kandler (Referee)

kzk@gmx.de

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This paper deals with a database for soil composition, which can be incorporated into dust models. The work bases on the approach of Claquin et al. in 1999. It has a comprehensive introductory part addressing many impacts of the dust on climate, health, biosphere underlining the importance of the subject. The proposed soil database can be a valuable tool for the dust emission modeling.

However, some assumptions and uncertainties inherent to the database (outlined below) should be clearly addressed.

A database with a spatial resolution of $0.5°$ implicates a high exactness and high spatial knowledge of the base data. However, two major points of critics on that approach

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should be adressed: First, by common sense it can be seriously doubted that top soil measurements are available on a grid of 50 km across the desert regions. Particularly, the soil investigations available mainly rely on single samples taken from a vast area. Thus, many areas of this map must rely on averaging or interpolation. I suggest to add some comments to this point.

Second, for example Bristow et al. (2010, doi: 10.1029/2010GL043486) have shown shat even on scales as small as 2 km (inside the Bodélé depression) there is a very high variation in soil composition. This is largely beyond that what the soil classification used here can resolve. So, a single value of soil composition has to represent an area of 50km x 50km, which can be either an average composition of the soils inside or the modal one or whatever. But it can be assumed that dust emission favors particular soil types, due to their texture, humidity, location, composition. So, this single value is not necessarily representative for the actual dust emission. Please comment on that (in the paper).

Of course, these problems can't be solved within the scope of this publication, but I highly suggest addressing these drawbacks to aid the reader – and of course, user of the database – in addressing the relevance of his calculations and pointing out potential uncertainties. My concern is that otherwise these data are used unreflected, and model discrepancies are misinterpreted.

=== Comments ===

26017/26: Where do we know that from?

26018/11-12: Though the higher chemical reactivity due to surface roughness sounds plausible - is there any reference for that?

26018/15: Actually, carbonates themselves are not exactly highly soluble, if we are speaking of water, but they are the most reactive ones of the common mineral dust in an acidic environment. Please correct this paragraph accordingly.

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26018/23: But the Bodélé depression is not particularly rich in gypsum, according to Fig. 4h

26018/27: I do not think that this is a good idea, as hematite and goethite (or other iron oxide/hydroxide compounds, which are often not very well distinguished) do have different spectral behavior. If the information on iron speciation is available, I would suggest keeping it a long as possible.

26019/29: The phosphorus solubility is also depending on atmospheric processing (Nenes et al 2011, doi: 10.5194/acp-11-6265-2011) and in general the P cycle is not well understood (Okin et al. 2011, doi: 10.1029/2010GB003858), so I would suggest not giving a number here would be more favorable.

26020/9-10: I understand that there is not more data available, but I have serious doubts whether this is close to reality. Trace contributors to soils like P most probably depend on traces in the geological basement, but soils are a product of basement and climate, so at least a very large uncertainty with respect to the P content should be given as a caveat here.

26020/20: Up to here the paper is on soil composition, but now this soil composition is set equal to an atmospheric dust composition. However, the transfer function from soil composition to dust composition is not unity (e. g., Eltayeb et al. 2001, doi: 10.1023/A:1012272208129). In a work like that, I would expect at least some comments on this to aid the user in identifying potential uncertainties.

26030/Table 1: Please distinguish carefully between "fractions of clay minerals" (i. e. relative amount of minerals in the clays group) and "clay fraction" (particles with sizes smaller than 2 μm). I suggest - to improve uniqueness - to print explicitly "clay size fraction", if the particle size is referred. Same applies to "fraction of silt"

26030/Table 1: Some of these compositions are very similar. I understand that these numbers represent an ideal composition of a particular soil or a composition that rep-

resents and average over several samples of that soil. But how variable can be the composition of a single soil type? This would be particularly interesting, when addressing iron and phosphorus contents, which have been most probably not of primary concern when these soil classes have been established.

26030/Table 1: This table implies that clay minerals like kaolinite, illite are never present in grain sizes larger than 2 μm, and that feldspar and gypsum are never present in sub 2 μm particles. This is certainly not true (e. g., Leinen et al. 1994, doi: 10.1029/94JD01735; Reid et al. 2003, 10.1029/2002JD002935; Kandler et al. 2007, 10.1016/j.atmosenv.2007.06.047), but relies on the definition of "clay mineral size fraction" as an a priori assumption. However, for the representation of the true composition, this might introduce a significant error, and should be addressed here. At least, it should be mentioned that this clear-cut composition change is artificial.

26034: This map is in my opinion quite misleading. Looking at the map, I would assume that most of the dust consists of quartz and feldspars. Though this is probably true for the soils, the question of applicability to airborne dust arises again, as that contains considerable amounts of clay minerals. Instead, I would suggest displaying the maps separately for the silt size fraction and the clay size fraction.

Also, there are very large phosphorus deposits in NW Africa (Morocco), which are even mined. But the map doesn't show anything of it, so the relevance of the phosphorus concentrations estimated by that database should be addressed critically.

=== Corrections ===

26010/5: are represented?

26010/6: determined?

26012/19: It should be mentioned that "clay and silt" refer to particle size here, not to composition

26014/6: "In the ice nucleation process"

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26014/12: "of dust"?

26015/1: Though I like the expression "global worming", it should be "warming"

26015/4: cruise

26017/4: Isn't the process of lifting a little bit more complex? Though it is not scope of this paper, the reader might be misled by this notion.

26018/1: Please refer correctly, that this list contains a mixture of mineral names (calcite, gypsum) and mineral group names (feldspars, smectite)

26019/26: food production → nutrient supply

26021/16: "Not all of the area covered by . . . is necessarily dust-productive"

26030/heading "slit" → "silt", "gypsium" → "gypsum"

26033: The Xerosols can't really be identified in the map due to their black color. Please use another color

Lastly, I'm not a native speaker myself, but it feels like a lot of "the"s are missing from the text. I suggest having the manuscript screened for style by a native speaker.

Interactive comment on Atmos. Chem. Phys. Discuss., 11, 26009, 2011.