

Interactive comment on “Viticulture microzoning: a functional approach aiming to grape and wine qualities” by A. Bonfante et al.

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Review comments on: Bonfante et al. Viniculture minizoning: a functional approach aiming to grape and wine qualities [original title, may need re-phrasing]

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This paper presents interesting and new results as it combines up-to-date physical techniques for measuring soil physical parameters for modeling, EM and DSM/DTM with pedological soil characterizations. A good example of hydropedology. The paper is also quite interesting as it includes detailed analyses of the characteristics of grapes and must, which are linked to modelled CWSI values. Comparing on the one hand what the soil has to offer with, on the other hand, what the crop (the grape) requires is a key

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element of land evaluation and this paper moves beyond the often rather empirical and qualitative nature of traditional land evaluation. This reviewer would strongly support publication of this paper in the special SOIL issue of Geosciences and Wine, after comments of this reviewer are answered satisfactorily. The authors would be well advised to improve the presentation of their results. Language quality is a problem and the text needs screening by an english editor. Also, the text is wordy, repetitive and while some results and conclusions are repeated several times, some important aspects are not yet covered. But I am confident they can be when attended to. The following comments and suggestions are made: 1. The title of the paper needs rephrasing. Is the minizoning really the main item? The strength is more: “Using a quantitative analysis of soil water regimes to explain differences in grape and wine quality”. The paper only talks about Homogeneous Units, never about Viniculture Minizoning! 2. 2.1. Study area: mention that area is 3 ha (this is only mentioned now in the discussion). What does : “green manure management” imply? When mentioning “mean daily temperature” : for the growing season? Clarify. 3. The mapping procedure of the homogeneous zones raises a number of questions. There are lots of activities but ultimately there is only one “representative”profile for each one of the two HZ's. This corresponds with classic land evaluation. So what's new? The “representative”profiles for each HZ show standard deviations of their properties, which is new but this is not “translated” later when modeling. Does this imply that differences are so small that they are insignificant for the measured hydraulic characteristics? If so, this needs to be mentioned. The preliminary survey defines two mapping units. The EC analysis (Shown in Figure 3) shows clear patterns in both units. Why does this not result in more HZ's? Where is the proof that all those different images don't indicate different conditions for grape development? Why are these patterns different ? Clay content (see table 1), water content? (see different Ko values). 6 soil profiles were analysed. How where those locations selected? How about the locations of the 25 soil borings? I like to point out the fundamental approach taken by Van Alphen and Stoorvogel in 2000 (SSSAJ 64: 1706-1713) when defining a functional approach. They made a grid of borings, ran a model for each point defining

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relevant functional properties (moisture supply, N-leaching under standardized conditions in their case) and then using kriging to compose “functional “soil units , that were different from soil-map units. This was applied in precision agriculture. I don’t suggest something similar but the authors should better describe their procedure which is now unclear. Finally, why kriging GE data (= EMI data) (watch consistency in terminology!). This provides a continuous output, isn’t it? Why kriging? 4. The key CWSI index has been used before, by Kozak, 2006, we read. Explain how he approached this and what you do better now. Also, it would help if you would explain a bit more about what seems to be an important paper by Matthews et al 1990, who related water stress to a number of quality parameters of wine. What did he report and how thorough was his research? It would increase the consistency of the paper if the terms used here to express the quality of wine would better correspond with the terms in table 2. 5. In section 2.5 (Simulation modeling) more information is needed on rooting patterns. This has a major effect on the outcomes of the model. Now it states that :“it is specified by the user” and that it is noted during profile descriptions. But the SWAP model was developed for agricultural crops with the implicit assumption that in the rootzone 80% of all roots occur and that water is within diffusion distance of the roots (no lack of accessibility). How about the grape shrub? Is there not a single root going down with branches? This would not match with the model assumptions. And is there a relation between rooting and the development stage, as suggested in text? And did Taylor and Ashcroft (1972) define the terms for the sink term defined by Feddes?? For grapes? Explain. 6. The GIS section 2.6 is a bit confusing. DSM/DTM are applied to express:“variation within homogeneous units”. But the two HZ’s are assumed in the end to be homogeneous , each with one representative soil profile. This is classic land evaluation. “shadows”are expressed but how is that represented in SWAP? By reducing energy input by 55 kWhr/square meter, as reported later? Is that the same as “potential insolation”, mentioned later? (why “potential?”).And what are these “water proof”surfaces? They will certainly affect the soil water regime. How? How about the TWI, the Topographic Wetness Index, that suddenly pops up? This is all rather incoher-

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ent and confusing and it remains unclear what the relation is with SWAP. 7. In section 2.7 several advanced crop measurements are made. Were they also used to calibrate or, better, validate the SWAP model? Good opportunity to do so! 8. General comment on Results and Discussion: focus on results and avoid again extended description of methodologies used. That belongs in the methods section. Why speak of preliminary HZ’s? and “potential” CWSI? The real CWSI values have been simulated is it not? Quite confusing that the authors speak of Cambic Calcisols (CAL) and Eutric Cambisol (CAM) but also of Haplic Calcisols and Haplic Cambisols in text and Figure 4. Stick to CAL and CAM? Why the difference? 9. A conceptual question can be raised for chapter 3.2, Modelling Application. The standard deviations for CWSI are enormous. The values for CAL and CAM are not significantly different. The standard deviation for CAL is appr.80% of the average, for CAM it is appr.70%. This implies that CAM buffers a bit better? The authors are advised to pay more attention to the ability of the two soils to cope with varying weather conditions in different years (their buffering capacity or resilience) that appears to be a crucial factor not covered so far in this paper. 10. Section 3.4: don’t repeat all the numbers that can be seen in the table. Text is unreadable now. 11. In the discussion section the important point is made that physical characteristics should be measured, not calculated by pedotransferfunctions (PTF’s) because the textures of the soils “are the same”. This is, however, only true when considering surface soil (Table 1) .So rooting depth (discussed above) is very important. If roots go deeper, this statement about PTF’s is incorrect and it can, of course, only be proofed in any case when calculations using measured physical data are compared with those obtained with PTF’s. This was not done. Considering the major differences in weather among the years, the results of such comparisons are not clear at all beforehand! Perhaps those weather differences have a larger impact than differences between the hydraulic characteristics! 12. Interesting observations are made for the development of the Aglianico grape comparing both soils and practical advice is provided for the CAL soil. This is a strong point of this paper. Future developments are discussed but they could perhaps be a bit more focused. Having many locations with these types of data

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would allow establishment of statistical relations between soil characteristics or , better, calculated CWSI values, and wine quality parameters derived from different varieties. This way, different types of grapes could be characterized, allowing better suggestions for planting certain varieties in certain locations? The analysis also allows estimates of profitable drip irrigation at strategic moments to overcome some of the observed CWSI differences between the two soils. I would recommend to make a link with precision irrigation because this capital intensive business, where hydropedology can make an important contribution, provides an attractive window for future business. The authors mention the possibilities to estimate effects of future climates and that is a good point! And.never forget that not only CWSI affects grape characteristics but also, for instance, pH! 13. The conclusion section is far too long and repeats what has been said before. Present some short and concise points about the main aspects of this study: (i) using hydropedology to define CWSI; (ii) link wine and must characteristics with soil characteristics, particularly CWSI; (ii) need to transform the soil map into a functional map (As discussed above there are many questions about procedures used here that need to be addressed), and (iii) future prospects in terms of more effective grape variety selection, precision irrigation to overcome high CWSI values and expected effects of climate change.

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