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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]

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 element of land evaluation and this paper moves beyond the often rather empir-

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ical 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.

We are grateful to em. Prof. Bouma for his revision, we consider his point of view very important to improve the manuscript. As we replied to other referees on the general comments to the paper, we will try to improve the presentation of results and discussion, adding references to the other studies and correcting the English grammar.

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!

Following also the referee 3 request, we will try to improve the title of paper in according to the main objective of it.

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.

The dimension of study area (or experimental site) is already reported in the paraghraph 2.1. line 129 "This was done on an experimental site (3 ha over an homogeneous hilly slope) ..". The description of management ("green manure") was necessary to specify that the management during the three years was the same (an agricultural practice rooted in time). The "mean daily temperature" refers to the annual and not to the growing season. (we add this information in the reviewed text).

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 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 krige GE data (= EMI data) (watch consistancy in terminology!). This provides a continuous output, isn't it? Why krige?

First of all, we want to specify that the study was not oriented to the precision farming management. We were looking for a zoning procedure – potentially sustainable for high quality viticulture farmers- rooted on standard soil survey incorporating some geophysics... both issues easily found in most countries where viticulture is important. Morever we aimed to test the procedure, already published at district scale and based on a dynamic information produced by modelling application (CWSI) on Geoderma (Bonfante et al., 2011), to a farm scale on Aglianico grapevine. The questions raised by the referee about the spatial soil "micro-variability" inside of the HZ's identified by

the colour changing of ECA maps (nuances), is very interesting, but it is not the approach we have undertaken. Referring to the interpolation of ECA measurements, the instrument gives continuous points of measurement between the vine rows, than in order to extent the measured between to vine rows we need to interpolate the ECA measured. Of course, considering the distance between the two points of measure, the application of inverse of distance method to interpolate was also a good choice.

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 at 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 consistancy of the paper of the terms used here to express the quality of wine would better correspond with the terms in table 2.

We appreciate this comment. We will discuss the points in the revised manuscript. Specifically, Kozak et al,., 2006 discussed about the importance of the correct estimation of the actual transpiration in the determination of the Water Stress Index (Ta/Tp) in the RZWQM and RZ-SHAW models, when corn and soybean production were simulated. We quoted this also because a modelling approach was applied and because we believe that Transpiration is more realistic variable – in respect to Evapotranspiration – for defining the crop water stress. The model we used to simulate the soil water balance is different from that one of Kozak et al.. We used SWAP model based on the Richards' equation, that is it very robust to simulate the soil water balance and it was previously used and tested in Italy and in the same Campania Region (Bonfante et al., 2010; Bonfante et al., 2011). We will improve the literature about the water status and wine quality, not only by means of a better explaination of Matthews results but also adding other information from Van Leewen et al., 2009, Chapman et al., 2005; Acevedo-Opazo et al., 2010, Intrigliolo and Castel 2011 and Romero et al., 2013.

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.

The newer versions of swap model allow the user to describe the time course of the roots' depth and especially the Root density distribution along the profile. In such a way can be distinguished the depths where the roots are more efficient from those where tha uptake is less relevant or even zero. Moreover, after 3 years from planting, the root apparatus can be considered stable. We found some others papers modelling water uptake with swap in such a way: - Minacapilli, M., Agnese, C., Blanda, F., Cammalleri, C., Ciraolo, G., D'Urso, G., Iovino, M., Pumo, D., Provenzano, G., and Rallo, G.: Estimation of actual evapotranspiration of Mediterranean perennial crops by means of remote-sensing based surface energy balance models, Hydrol. Earth Syst. Sci., 13, 1061-1074, doi:10.5194/hess-13-1061-2009, 2009. - Rallo, G., Agnese, C., Minacapilli, M., and Provenzano, G. (2012). "Comparison of SWAP and FAO Agro-Hydrological Models to Schedule Irrigation of Wine Grapes." J. Irrig. Drain Eng., 138(7), 581–591. - Ben-Asher, J., van Dam, J., Feddes, R.A., Jhorar, R.K., 2006. Irrigation of grapevines with saline water II. Mathematical simulation of vine growth and yield. Agricultural Water Management, 83: 22-29

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 incoherent and confusing and it remains unclear what the relation is with SWAP.

The LIDAR information (DSM and DTM) were used for the geomorphological analysis to realize the preliminary landscape mapping units for the pedological survey. This information was reported in the results section but not in the materials and methods. Thus, we will add this information in M&M. The effect of different insolation was tested directly on the Penman Monteith equation. The results have shown a negligible effect on evapotranspiration and then on CWSI predicted by the model. We will add in the manuscript this information. The "waterproof surfaces" refer to evaluation of TWI with DTM. This information (TWI) is widely used in the terroir analysis, it is a steady state wetness index, commonly used to quantify topographic control on hydrological processes (to define the path flows). In our case study it was used only to compare the two HZs, whitout any inference to the hydrological behaviour of the slope. Thus, the section 2.6 was not correctly written in the manuscript, creating confusion in the reader, therefore we will write it clarifying all aspects raised by the referees.

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!

The model SWAP has been calibrated and validated in this case study on SWC at different depths. Moreover, a check of goodness of model application by means of data not simulated by the model (i.e. Leaf Water potential data) was performed. (Bonfante et al., Soil-plant water status and wine quality: a physically based approach to terroir analysis. Ixe International Terroir Congress 2 012.") However, we would like to stress that a crucial point concerns the potentiality of the use of physically based model information (CWSI) in a procedure of viticultural zoning at farm scale. This for using the model as a preliminary information to zoning and planning the plant of vineyard (e.g

without have a local data for calibrating and validating the model).

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?

Surely in order to improve the paper we will avoid to extent the description of methodology in the results and discussion section.

9. Why speak of preliminary HZ's? and "potential" CWSI? The real CWSI values have been simulated is it not?

Good question, the use of term preliminary create a misunderstanding in the reader and confusion. We will delete this term, because there are not two kind of HZ's (e.g. preliminary and final) but only one. We used the term "potential" CWSI, because it is determined on a long time period (2003-2013), and it is representative (as average value) of different capacity of two different soils to produce a crop water stress (under the same climate condition and crop management and crop development). It is also correct to talk about potential because the plant development (in terms of LAI) was considered the same in the different years of simulation (representative of crop development from data acquired in field in three years of crop monitoring). Then, in this way we have emphasized the different "potentiality" of these different soil physical systems, Calcisol and Cambisol (with different soil layering and hydraulic properties). We apologize for the errors reported in the figure 4 and in the text, we have only Cambic Calcisol (CAL) and Eutric Cambisol (CAM).

10. 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,

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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.

The statistical ANOVA analysis on CWSIcum over the season has shown a significant differences with alpha of 0.02 between the two soils. During the different phenological phases only the Berry ripening phase shows a significant differences with alpha of 0.02. This behavior could be explained because both soils start the growing season with an optimum water content, but during the season, the reduction of rainfall, the increase of ETo and the effect of water uptake by the plant, emphasize the physical differences of two SPA systems. Concerning to the high variability of SD, in the figure 1 is shown how these differences during the berry ripening depend from different climate conditions (rainfall ammount) during the 11 years analyzed. We clearly identify two very dry years (2003 and 2007) and to very wet years (2005 and 2010). During the simulated years, the average rainfall ammount in the period 18 August to 15 October was 105 mm with a SD of 46.9 (44% variation). This information will be discussed in the paper.

11. Section 3.4: don't repeat all the numbers that can be seen in the table. Text is unreadable now.

Ok, it will be done.

12. 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!

Surely the root depth is very important as the vertical soil horizon description (thickness and hydraulic properties). We have compared the effect of use of PTF or measured data in this case study, but not reported here because the focus of the paper was different. What we have found? The differences between the two soils are evident with measured data and light with PTF (see figures 2-3). Obviusly, because the PTF are "smooting engines" of hydraulic properties and then hydraulic beaviour. The differences in hydrological behaviour between the two soils hold under very different climatic conditions. In the figure 1 (point 10) the differences of CWSI responses of the two soils in some years is comparable with the soil intra-years variability of CWSI.

13. Interesting observations are made for the development of the Aglianico grape comparing both soils and practical advise is provided for the CAL soil. This is a strong point of this paper. Future developments are discussed but they could perehaps be a bit more focused. Having many locations with these types of data 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!

Of course future developments can be better discussed and we will try to do this. About the question on the use of many locations to verify the correlation between the

organoleptic quality of wine or must and CWSI, in order to allow the better suggestions for planting of certain varieties in certain location, we think that is very important question, but not so easy to realize. Bonfante et al. in the 2011, in a regional viticultural zonig, have evaluated the correlation between CWSI (determined with SWAP) and some plant responses (Plant production, Berries weight, Sugar, pH..etc..) of Falanghina grapevine on ten different soils (in different landscape systems), identifying some interesting correlation between the potential CWSIcum over the season and the plant production or berries weight. It was very interesting story, because in each soil the Falanghina vineyards was the same cultivar with the same age, management..etc.. under different climate conditions. Moreover, the study area of this researc is not so distant from the study area of Aglianico grapevine. Obviusly, is important to test the relation between CWSI and plant responses in different case studies, possibly with the same grape cultivar and climate condition but with different soils charachteristics. It is also important to point out that the Aglianico grapevine represents the most important red vine of Campania Region. Then, the study of the aglianico responses to water stress is important for the winegrowers of Campania Region, in particular (we think) if it is based on the use of SPA models because these last allow to do a prediction of crop responses to climate change. In this way, they allow to study if in the next future (or also actually) there will be the necessity in some areas of Campania Region, to use the drip irrigation to control the plant water status to improve the quality of most or to mantain the actual quality. But these important purposes are in conflict with the production regulations of Aglianico wine in the DOC area, "Demarcation of controlled production areas" or DOCG "Demarcation of controlled and guaranteed production areas", where the irrigation is forbidden and permitted only in extreme conditions (very dry season) as emergency irrigation.

14. 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.

We are in agreement with the reviewer and we will improve the conclusion following his advice.

Interactive comment on SOIL Discuss., 1, 1203, 2014.
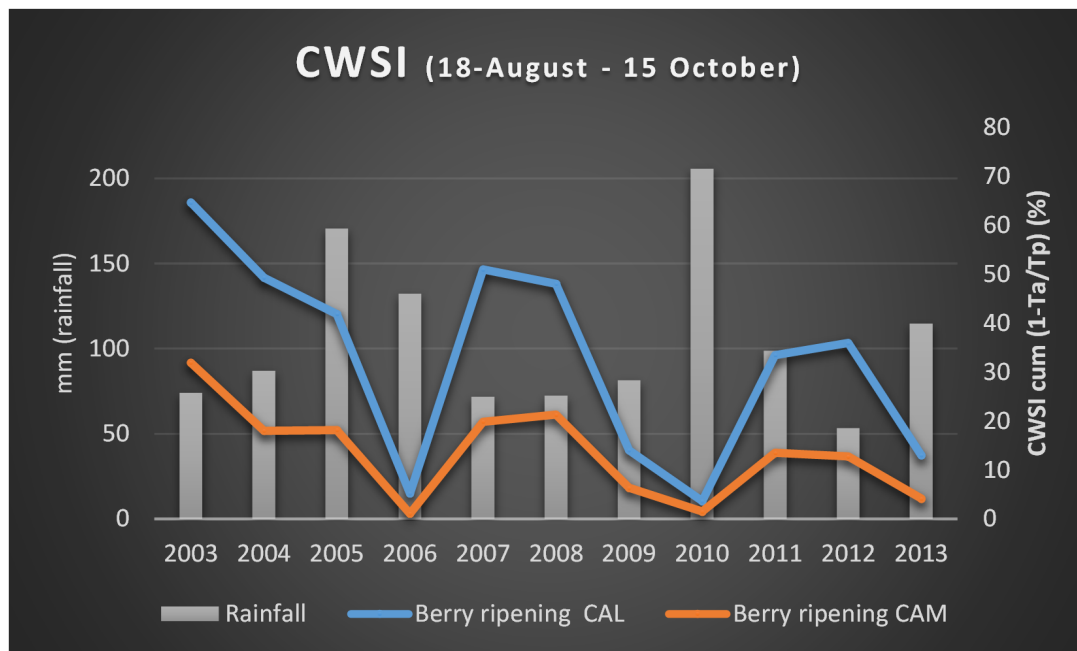
Fig. 1. CWSI during berry ripening
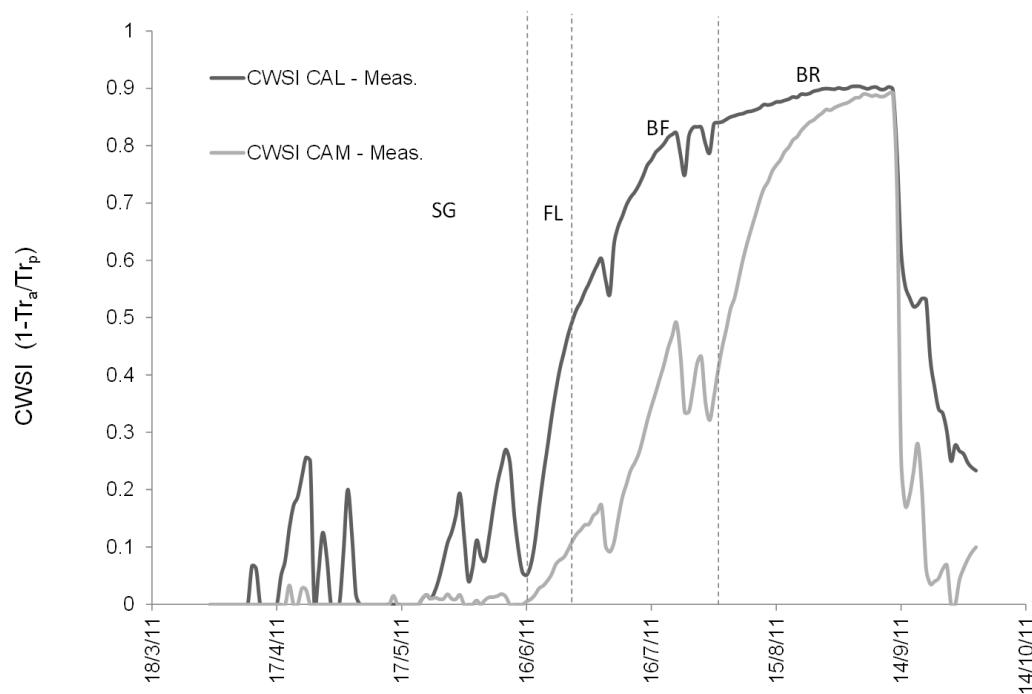
**Fig. 2.** CWSI obtained with hydraulic properties measured
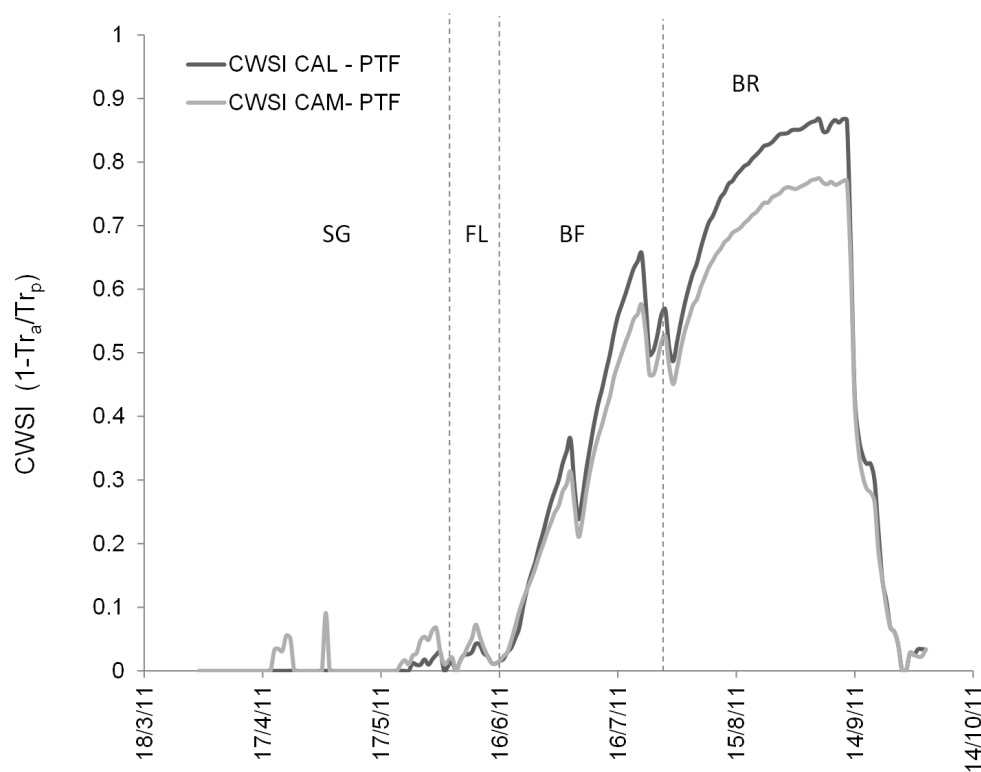
Fig. 3. CWSI obtained with hydraulic properties estimated (PTF)