

Interactive comment on “Assessment of strip tillage systems for maize production in semi-arid Ethiopia: effects on grain yield and water balance” by M. Temesgen et al.

Anonymous Referee #2

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General Comments:

This is a potentially interesting study of how soil conditioning is affecting the hillslope water balance and affecting yield in a semi arid region. The field trials were set up to give valuable information on runoff as well as soil moisture. There are, however, serious shortcomings of the experimental design, the modelling, the presentation of methodology and results. In fact I believe that the paper needs to be completely rewritten after the data have been completely reanalysed as there are indications that there actually no measurable difference between soil treatments concerning yield and water

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balance (with the exception of surface runoff only).

These are:

- The paper does not clearly communicate what was actually derived directly from experimental observation and what has been modelled with the conceptual model (eg Abstract – quantities such as Q_s and T are reported as results of the experiments – which is true for surface runoff Q_s and yield Y but not for T and W_{PET}). In fact, little effort has been made to derive system understanding from the measurements first before using a model to add value. The use of the TDR measurements is the best example: (i) It is not used directly to compare the different treatments and (ii) no evidence was provided to justify that the rooting depth is 1 m although it was claimed that water contents were measured to a depth of 1.8 m.
- The water balance remains uncertain as little or no effort was made to separate I, R, E and T based on observation. Furthermore the model parameters are not independently estimated – they are assumed with very little justification and explanation why these values are suitable for the system under investigation. Furthermore, soil properties such soil moisture storage at field capacity and wilting point were assumed to be same for all plots, but why wouldn't different ploughing techniques (in particular the subsoiling) affect field capacity for the 1 m soil profile? This is further exacerbated by assuming a 1m rooting depth for all plots without providing any experimental evidence for this.
- The soil characterisation is less than adequate to assess hydraulic response to rainfall and more important changes before and after treatment. Nine samples of 0-15 cm horizon were analysed before the experiment with no reporting of standard deviation (the most basic statistical information) and no efforts seem to have been made on capturing a comprehensive set of data for soil depth below

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0.15 m. Furthermore it seems that only three samples have been taken after the experiments. Moreover, the only useful parameter measured to assess changes in soil hydraulic properties was bulk density – and with such few data it is impossible to conclude that soil physical properties have not been affected by the different treatments. Texture and OM are properties that should remain relative constant over the experimental period. By just using the simplest infiltration test (for which we have so many methods such a ring infiltrometer test or auger hole permeameter method) during the experimental period could have provided us with some insights into the relative changes between the different plots. In top of all this some methods have not been reported at all such as field capacity and wilting point although they are listed in Table 1 as measured properties (how many samples, what depths, ...).

- Modelling could be improved to compare not only water content data but also surfaced runoff data with the model. Also, it not clear what the data points represent – are they averages of measurements or more???

Suggestions

To improve the paper I suggest going back to the data and clearly prove from experimental evidence that the different ploughing systems are significantly different. I believe that the surface runoff provides proof that they behave differently, but since surface runoff is a small component of the water balance, it seems to remain inconclusive how transpiration is affected. As yield for field crops such as maize have been shown to be strongly correlated with transpiration and in some cases evapotranspiration, the yield data may reflect transpiration better then the model, which may prove to be inconclusive when accounting for all parameter uncertainties. Therefore, I suggest to first exhaust all experimental evidence including TDR measurements to a depth of 1.8m and present this as the first part of the paper. Then use the model to assess all components of the water balance, but the authors need to formulate a clear objec-

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tive of what we can learn from the modelling. Furthermore, the discussion needs to pick up on the existing literature on yield – water balance relationships which has been extensively researched.

Specific Comments

Abstract

See first comment above, present first what the data have shown then what the model contributes to our knowledge and present modelled results as factual results.

Introduction

This section seems to have been written for an audience that has its main interest in tillage and issues in the developing world – and that is not necessarily concerned with fundamental and applied hydrology. I suggest adding some references and a paragraph on how soil management affects hillslope hydrology in order to capture a wider readership of this manuscript. As mentioned before, this is an interesting hydrological problem for which we don't have many comprehensive hillslope-scale data sets. Also, a clearer and if possible a hypothesis driven objective statement would add value to the paper.

Materials and Methods

There are serious omissions in this section so that many methods are not reproducible. Some examples are given below.

It would be good to present some more detailed soil profile description to get an idea of how hydrological processes work in this system. There is no information provided that could be used to deduce information on whether for example hydraulic conductivity decreases with depth, whether there are clayey subsoils that will prevent deep drainage. The information provided is insufficient.

Some further explanation should be given on the experimental design. It is claimed

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to have a completely randomized block designed presumably for the three soil treatments. But this is further complicated by having different maize varieties with additional treatments looking at open and closed furrows. Furthermore, fertilizer treatment is introduced and if effective it could also impact on transpiration (if fertilizer is in addition to water a limiting factor to growth). Are all these factors properly considered in the design to unequivocally prove that the different ploughing systems significantly affect water balance and crop yield? More detailed explanation of the design and the statistical treatment should be added.

The water balance section needs more detail. For example how were the tubes for TDR measurements installed so that bypass flow a long the tube was prevented? Give make and model and cite manual for example etc.

The modelling section is overall well described but the term “deep absorption” is an unusually term – deep drainage seems more appropriate. Also, parameter values should be justified here. Furthermore, I have a problem with the way validation is used here. If runoff is a small component of the water balance and has little effect on the soil water balance, then by calibrating (or fitting) the model to the first treatment and then comparing it to the others should obviously work very well – and that can be seen by looking at the water content data (they seem not very different).

In the grain yield section it is stated that SAS was used – this is secondary information – it should have a clear statement on what statistical method was used. Clearly the procedures are irreproducible with the information given.

Soil physical and chemical properties needs more detail as well. At which depth were the 9 samples taken? What increments and etc.? See above comments for more fundamental problems with the choice of analysis.

Results and Discussion

I assume the first section refers to figure 9 (not mentioned in text). The authors need to

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choose a statistical method that indicates whether the data are significantly different. Just by looking at the data – I am a bit worried to that the data may not be significantly different. The current analysis is not acceptable as the parameters listed in equations 12 to 14 would have very large error bars which in turn could likely produce large model errors. The section ends in speculation with little or no justification.

The sections Model outputs are commented on above. All the following sections are full of speculations and contain little substantiated statements I will comment on in detail. There has been no attempt to actually present the results of this study in the context of the existing literature on hillslope runoff and tillage. Furthermore there is a body of literature dealing with the relationship between transpiration (and ET) and yield – no citation and no discussion. There is some old work by Hanks and other son maize yield and ET.

Tables and Figures

Tables and and Figures are consistent with the rest of the paper and I will not comment any further.

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 4, 2229, 2007.

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