Response to the editor after its decision on August 17th, 2021.

Robustness of a parsimonious subsurface drainage model at the French national scale

(hess-2021-168)

By A. Jeantet et al.,

Dear Dr. Stamm,

Thank you for your valuable comments improving clearly the manuscript after receiving the revised version. They have been integrated in the revised manuscript and highlighted by a specific comment mentioning the corresponding order of appearance. The mention "Language" designates the comments from the Language part, "Calibration" for those from the Calibration part and "FC" for those from the "Further comments" part. The lines mentioned-below refer to the marked version:

"Language":

All the language issues have been corrected as requested.

1. "*L. 29: What the meaning of exacerbating any infiltration concern?*"
   Reply: The expression has been replaced at the line 29 by:

   "reducing the deep infiltration"

2. "*L. 31 - 33: The expression stone aggregates sounds strange. Actually, the sentence is not properly citing the literature. Actually the wording is literally copied from the reference [1] but it refers NOT to mole drains but to ditches separating the experimental plots. The article itself deals with mole drains and gravel mole drains. Accordingly, the sentence on L. 32 is factually wrong. Please correct.*"
   Reply: To avoid useless descriptions, the sentence at lines 33-34 has been removed and replaced by:

   "such as subsurface drainage and open ditch."

3. "*L. 52: ... thus complicating their parameterization on a large database. Weird sentence, which is not clear. Please rephrase.*"
   Reply: The line 54 has been complete by:

   "with large number of parameters. Their calibration on several study sites becomes difficult and time consuming (Beven, 1989)"

4. "*L. 275 - 275: This information is repeated for the third time. Please skip and avoid unnecessary repetitions throughout the manuscript.*"
   Reply: The line 275 has been removed.

5. "*L. 301 - 309: This information has already be provided in the previous paragraph. Skip.*"
   Reply: The paragraph from the lines 313 to 321 has been removed and merged with the previous paragraph, from the lines 302 to 312:

   "Table 1 lists the performance over the entire calibration period obtained from all 22 sites and Table 2 classifies the model performance from each soil texture according to the score ranges. Performance varies across the three soil textures, with both unsatisfactory KGE' values, e.g. for the *Courcival_P3* site, and some "very good" KGE' values, e.g. *Parisot*. For 21 of the 22 referenced plots, the calibration KGE' lies above 0.5, thus revealing at least "acceptable" KGE' values. The silty plots show values ranging from 0.54 to 0.83, including the best model performances, such as *La_Jaillière_P4* plot, with a KGE' of 0.83. They compile three "acceptable" scores, reaching "good" for six of them and "very good" for another six. The silty-clayey plots exhibit relatively homogenous KGE' values, ranging from 0.54 to 0.76. As regards

the clayey plots, KGE' values display a wider range than on the silty-clayey plots, i.e. from 0.44 at the *Courcival_P3* plot to 0.76 at *Saint_Laurent_P2* but the model performance remains at least "acceptable" on most of them (including one "very good"). *Courcival_P3* is the only one indicating an "unsatisfactory" KGE' value"

6. "*L. 353 - 361: Not the Q05 are close to zero, but the respective biases! Please adapt the wording for all quantiles!*"
   Reply: The paragraph from the lines 366 to 377 has been reworded:

   "Regarding the Q05 quantiles (Fig. 7a), results show that for the three textures, bias between simulated and observed Q05 ranges from -0.020 to 0.030 mm/d, with some extreme points (mainly on the silty texture). The medians of biases all lie close to zero as well (from -0.002 to 0.002 mm/d), thus revealing that the model correctly predicts low flows. Regarding the Q95 quantiles (Fig. 7b), the median values from boxplots are once again close to zero (from -0.247 to -0.040 mm/d); however, the ranges of the Q95 biases lie above those of the Q05 quantiles. On silty soils, the boxplot limits of Q95 biases range from -1 mm/d to + 0.5 mm/d, and the whiskers range from -3 mm to +3 mm. Similarly, for silty-clayey soils, the Q95 biases vary from -3 mm to -2 mm; the discrepancies are larger on clayey soils, where the Q95 biases varies from -4 mm to +4 mm. Figure 7c shows that the boxplot medians for the Q_{mean} biases also lie close to zero (from 0.007 to 0.057 mm/d). The Q_{mean} biases range from -0.5 mm to +0.5 mm for silty soils, from -0.3 mm to +0.6 mm for silty-clayey soils, and from -0.8 mm to +0.9 mm for clayey soils. SIDRA-RU performs at a level of good agreement with respect to the average drainage discharges. The deviation on Q_{mean} biases is higher on clayey soils, thus reflecting the greater difficulties of the SIDRA-RU model in simulating Q_{mean} on this texture."

7. "*L. 430: This sentence is not correct. I assume it should be: Not all regions .... are well represented.*"
   Reply: The sentence at lines 444-445 has been replaced by :

   "Not all regions with a high drainage rate are well represented."

8. "*L. 452: respected: this is not the adequate expression here.*"
   Reply: "respected" has been replaced by:

   "reproduced with a good agreement"

9. "*L. 459: respects: Rephrase: simulates well the temporal ...*"
   Reply: The sentence at lines 476-477 bas been reworded:

   "Also, the SIDRA-RU model allows simulating the temporal variation of drainage discharge for both dry and wet periods;"

10. "*L. 467: Reword. There are not just two extreme discharges.*"
    Reply: Line 484, the expression "at the two" has been replaced by the word "on".

11. "*L. 490: Why should model calibration (be) only relevant if the calibrated K and μ are probable according to the case study soil type? The word relevant seems inappropriate here.*"
    Reply: the editor is right; the word "relevant" has been replaced by "reliable" at the line 508.

12. "*L. 516: relevant: Not an appropriate term: it is also relevant if the performance is poor at a given site. Relevant has the meaning of be connected with the matter at hand (see e.g., https://www.wordreference.com/definition/relevant).*"
    Reply: The editor is right; the word "relevant" has been replaced by "good".

13. "*L. 547: What is the robustness approach? Strange wording, please rephrase.*"
    Reply: As requested, the lines 567-569 have been reworded as follow:

"Another limitation of this model is the RU parameters are slightly less temporally robust than the SIDRA parameters, showing fewer stable values between calibration sub-periods than the SIDRA parameters."

14. "*L. 551: What is a conventional period?*"
    Reply: Here, a conventional period refers to a common hydrological period without extreme events. To improve the clarity of the discussion, the sentence at lines 571-573 has been reworded:

    "Compared to a common hydrological period without too much extreme discharge events, a dry calibration period conducts to increase S_{inter} values"

15. "*L. 566: Adopting assumptions on model performance would then offer a relevant alternative: What does this mean?*"
    Reply: The sentence at lines 590-591 has been reworded:

    "Making assumptions, e.g. grouping soils by categories, to address this weaknesses would then offer a good alternative"

16. "*L. 568: Reword… : that also limit the study. (I assume that the assumptions were not made in order to serve as limitations!).*"
    Reply: The sentence from the line 593 to 596 has been reworded:

    "Moreover, the model is based on some rather important assumptions, such as neglecting both the surface runoff and recharge to groundwater, or the ones made on the parameters a and α, which limit its field of application. Indeed, "

17. "*L. 571: Re-word: in every other context. Your current version tells that nowhere else the assumption were true while you want to say that not everywhere it was true.*"
    Reply: As requested, the sentences at lines 597-598 have been reworded:

    "up to now there is no evidence of their relevance on other sites. This limits the use of SIDRA-RU to the specific conditions outlined herein"

18. "*L. 612: matricial compartment: replace by soil matrix.*"
    Reply: As requested, the term "matricial compartment" has been replaced by "soil matrix" at the line 641.

<u>"Calibration":</u>

1. "*L. 65 - 72: What are the arguments for selecting KGE in your case?*"
   Reply: The argument justifying to KGE' is detailed at lines 245-246:

   "We have thus introduced the KGE' criterion (Kling et al., 2012), an evolution of KGE that is more relevant than NSE in reproducing internal flow rate variability (Santos et al., 2018)"

   To avoid repetitions, we did not integrate this argument in the introduction part.

2. "*L. 227 - 231: Where can one see the priors? Provide these data.*"
   Reply: The editor is right; the characteristics of distribution are missing. A table grouping the mean and standard deviation bas been integrated in the Appendix part and the following sentence has been integrated at lines 237-240:

   "The mean and standard deviation of each parameter are available in Appendix A. The ones for K and μ were extracted per soil texture from the aforementioned reference drainage areas. The ones for S_{inter} and S_{max} were numerically fixed after many calibration tests."

3. "*L. 441 - 442: Is it actually necessary to use these priors? Wouldn't the calibration process find the appropriate values anyway? Please test this aspect if haven't done already.*"
   Reply: Using the priors is necessary, the calibration algorithm searches in each parameter space to find the best combination. This requires a prior knowledge of the corresponding distributions. The calibration process cannot be performed without this information.

4. "*Line 578: What about the priors?*"
   Reply: The calibration algorithm is designed to be as automatic as possible (Mathevet, 2005). However, it requires the distributions of each parameter, which might introduce biases. This aspect is highlighted at lines 605-606: "external decisions influencing results are still necessary" and developed at lines 614-616 from "These measurements are subjected to uncertainty" to "that bias the mean and standard deviation".

5. "*L. 586 - 587: Where can one see these data*"
   Reply: These data are extracted from 96 reports on reference drainage tests, provided by INRAE on request. Vincent (1989) provides a list of technical datasheets of all these experiments and is available as requested or in university libraries.

6. "*L. 587 - 592: Are the calibrated parameters indeed constraint by the priors*"
   Reply: The editor is right; the calibrated parameters are constraint by the distributions used in the calibration algorithm. To detail this aspect, we propose to include the following sentence at the line 617 after "the calibrated parameters" and before "might be biased":

   ", constraint by the distributions a priori defined,".

Further comments:

1. "*L. 43: This re-design of drainage system is not further elaborated on in the manuscript. Can you comment on that aspect in the discussion when talking about the benefits of the model?*"
   Reply: The editor is right; this aspect is not further specifically mentioned in the manuscript after this sentence. The authors consider that it deals with the evolution of subsurface drainage under climate change, which is not the topic of the manuscript. Another paper dealing with this aspect is currently under correction and will specifically analyse the becoming of French subsurface drainage under climate change.

2. "*L. 53: What is semi-conceptual? Please be consistent in your terminology. Often you describe the model as conceptual (e.g., L. 258; which seems clear to me).*"
   Reply: The "semi-conceptual" term refers to the fact that the SIDRA-RU model is composed by a physically-based module (the historical SIDRA module) coupled with a conceptual module (the RU module) (Beskow et al., 2011). Consequently, the SIDRA parameters K and μ are physically-based instead of the RU parameters S_{inter} and S_{max} are conceptual. To improve the clarity of this aspect, the following correction have been applied:
   - to rewrite the sentence at lines 56-58 in the Introduction part:

   "The model is semi-conceptual (Beskow et al., 2011), being composed of one physically-based part (the SIDRA module) coupled with a conceptual part (the RU module) and parsimonious".

   - to remote the sentence "such as SIDRA-RU" at the line 267.

3. "*L. 54: You should make it explicitly clear that the model has six parameters. In your application described here, you simply kept two parameters fixed at values obtained from prior knowledge. This is important for applications elsewhere (see L. 173 - 188, also L. 439).*"
   Reply: The editor is right; the manuscript has been corrected as follow:
   - At the line 58, the expression "4 parameters" has been replaced by "six parameters";

- The sentence at lines 191-192 has been rewritten as follow: "The parameter α defines the proportion of $P_{net}(t)$ being converted to recharge R(t), while the remainder updates the water level, i.e. Eq. (3)";
- The sentence at lines 215-216 has been rewritten as follow: "due to the aforementioned assumptions dealing with the parameters a and α, a calibration process is only necessary for four parameters" ;
- The sentence at lines 454-455 has been rewritten as follow: ", initially requiring the calibration of six parameters, the assumptions made in this study allow to reduce this number to four parameters";
- The following sentence at the line 595 has been included: ", or the ones made on the parameters a and α,".

4. "*L. 90: Provide examples of what you do not consider. Discuss what the implications could be.*"
   Reply: The editor is right, some soil characteristics are not considered. To improve this aspect, the following paragraph from the line 92 to the line 101 has been rewritten as follow:

   "A multitude of materials constitute French soils, as defined by their geological context, textural evolution and regional climate. All of the above characteristics serve to determine the uniqueness of a soil. Making generalizations about soil diversity becomes then a necessary step. Indeed, grouping them by soil category facilitates their modeling. Several official classifications serve to group soil types (FAO, 1988; Krogh and Greve, 1999; Driessen et al., 2000). In this study, we are proposing to classify them by texture, thus making it possible to sort the database into three categories (see Fig. 1 & Table 1). Let's note that here we do not consider the geological context or the regional climate to classify soils."

   Section 4.7 already discusses of the consequences of such oversights, highlighting that grouping soils only using their soil texture introduces biases. To avoid repetitions, no other sentence has been integrated dealing with this aspect.

5. "*L. 95 - 96: But this would also be possible with other classifications?*"
   Reply: The editor is right; theoretically, the classification can be made using other characteristics as discussed in the previous comment. However, this would be in practice difficult because the classification would be too restrictive, e.g. to obtain groups with one soil which is not relevant. Regarding the number of available sites in the used database, this risk is non-negligible.

6. "*L. 108 - 109: where drained soils are predominantly silty-clayey and composed of fine sediment with heavy clay: this is confusing. Is the texture silty-clayey or heavy clay? It cannot be both at the same time, can it?*"
   Reply: The editor is right; in our classification, a plot cannot be simultaneously silty-clayey and composed of heavy clayey. However, two plots of the same study site can be defined by two different soil textures as *La Bouzule*. To avoid confusions, the sentence at lines 113-114 has been corrected as follow:

   "where drained soils are either predominantly silty-clayey or heavy clayey soils."

7. "*L: 119: drainage modalities: what does this mean?*"
   Reply: The term "Drainage modalities" refers here to the technical aspects of drainage networks, such as depth, spacing or the material used for the pipes. The reference drainage tests were performed to define what practice best suits to the conditions of a given site. We propose to include the following sentence after "drainage modalities" at lines124-125:

   ", i.e. what depth, space or pipes best fit to the field conditions".

8. "*L. 204 - 208: In your response you mention that the model could also deal with leaky subsoils. Please be specific about that and briefly describe how the model could represent this situation.*"

Reply: The editor is right; the model could deal with leaky soils integrating the depth infiltration with Hooghoudt's equation, according to the principle of equivalent depth (Zimmer, 1992), introducing the term "D_s", designating the deep seepage rate, in the equation to reduce the recharge term to the drains. This aspect is already mentioned at lines 599-600, and we propose to replace the reference by (Zimmer, 1992) and complete the sentence by:

"A term "D_s", designating the deep seepage rate, is introduced in the Boussinesq's equation to reduce the recharge rate to the drains."

9. "*L. 218 - 219: In this context, the model calibration allows estimating parameters based on a comparison between observations and model simulations. This is trivial and can be skipped.*"
Reply: The editor is right; the sentence has been deleted.

10. "*L. 322: Is this a general finding or site-specific? If the peaks are underestimated but the overall flux is ok, it implies that the recession curve overestimates water flow. Is this correct and a general observation or is it site-specific*"
Reply: The editor is right; it is a general trend. The model quite underestimates peak flows and simulate longer recession periods. However, the intensity of this phenomenon varies from one site to another and we cannot relate it to any soil characteristic. The phenomenon seems to depend on the calibration quality. Let's not that the model better simulates peak flows at the hourly time step introducing a new term in the Boussinesq's equation (Bouarfa and Zimmer, 1998).

11. "*Fig. 8. I assume you have two data points per site from the split test. Please indicate (by specific symbols) which data points belong to the same site.*"
Reply: The editor is right; there are two points per site from the split sample test. Initially, the purpose was to illustrate each site by a single symbol. However, the split sample test is performed on 9 sites, which imposes 9 different symbols, not facilitating the understanding of the graph. Eventually, only the soil texture has been represented. If the reader wants to compare the KGE' values specific to a single site, he can refer to Table 1 which provides the KGE' values in calibration and in evaluation from the split-sample test.

12. "*L. 437: Why is a simple model better for generalizing? A physics-based model should be better suited because it can accommodate more different situations while a conceptual model imposes more implicit model constraints. This view is supported by your description of the conceptual limitations of SIDRA-RU. On L. 521 - 522 you explicitly state that the model was primarily designed for silty soils and the results clearly demonstrate the limitations that go with that. You illustrate this also very clearly in Fig. 12. This all seems to contradict you claim that a simpler conceptual model is better suited for general applications.*"
Reply: The editor is right; a physically-based model is theoretically better suited to represent a large soil diversity. However, this kind of model is generally composed of many parameters representing the complexity of a study site such as current crop, root depth, saturated water content (i.e. unsaturated one), hydraulic parameters or water holding capacity. Performing such a model on a large database requires providing all these characteristics for each site, which is in practice very difficult, because the measurement technics are globally expensive. The calibration might be useful but it is time consuming due to their large number of parameters of such models. Indeed, a simple model offers significant advantages, as it requires few information and faster calibration, thus becoming in practice suitable for generalizing. Let's note that, in line with what the editor remarks, a simple model is limited by the assumptions made on its design.

13. "*L. 451: more local studies: provide references.*"
Reply: As requested by the editor, the following references are provided at the line 467: (Gowda et al., 2012; Skaggs et al., 2012; Muma et al., 2017; Revuelta-Acosta et al., 2021).

14. "*L. 471 - 472: This rationale might partially explain the observed delay.: You can simply test this explanation instead of being speculative.*"
Reply: The editor is right; it would be better to accurately highlight the origin of bias at the start of the drainage season performing tests. However, this starts is controlled by many factors depending on the climate conditions of the past year, the actual ones and the soil conditions. All of these factors influence the S_{inter} parameter, which is calibrated on the entire calibration period. Consequently, to prove this phenomenon is quite difficult. As proposed in Section 4.6 at lines 578-580, to make S_{inter} dependent on

these annual conditions, we can adjust it on every year according to the aforementioned factors. To avoid any confusion, we propose to remove the sentence at the line 488-489: "This rationale might partially explain the observed delay".

15. "*L. 500: What is the metric for consistency? There are quite some deviations.*"
Reply: The editor is right; there are some quite deviations. However, the distributions are in the same order of magnitude. Regarding the hydraulic conductivity K, except for three calibrated sites (20% of the calibrated sites) showing high values compared to the reference sites, the distributions on the remaining 12 sites are similar. The deviation in mean value does not exceed 0.05 m/d. In this regard, the histograms from the calibrated K are congruent with the ones from reference drainage tests. We propose to correct the sentence from the lines 516-517 by:

"However, over the remaining range of values, the histogram from the calibrated K are congruent with the one from the reference drainage tests".

16. "*Fig. 11: If I understand correctly, you have a single parameter value for each site from the reference data base and the calibration (actually, from the calibration you should have two from the split test). Accordingly, it would be more informative to compare the site-specific values.*"
Reply: The editor is right; we have one value per reference site and one value per calibrated site and it would be better to directly compare the site-specific values. However, we do not have a sufficient number of measured values of K and μ from the calibrated sites to do so.

17. "*L. 519: N. Jarvis commented on the good performance of MACRO on such soils. This has to be mentioned and referenced to avoid a wrong impression.*"
Reply: The performance of the MACRO model on clayey soils has already been mentioned at lines 559-561 as requested by N. Jarvis (see comment 18 from the "Further Comments" part): "Among them, there is the MACRO model which showed its efficiency to simulate drainage discharge in Europe, including structured soils like heavy clayey soils (Köhne et al., 2009)".

18. "*L. 544: Why should that be the case? As you describe, SIDRA-RU is not that general because it performs not very well on heavy clay soils (due to conceptual constraints, see also L 521 - 522, Fig. 12). Why should MACRO perform poorly on non-heavy clay soils? The preferential flow part of MACRO is not a static feature but depends on site-specific parameters.*"
Reply: The editor is right; to avoid confusions, we propose to correct the sentence at lines 559-561 by:

"Among them, there is the MACRO model which showed its efficiency to simulate drainage discharge in Europe, including structured soils like heavy clayey soils (Köhne et al., 2009)"

19. "*L. 550 - 551: Why should the calibration period a dry period? Where can one see this?*"
Reply: Dry periods might appear due to climate conditions, observing a larger occurrence of dry events on a same period. In practice, the model might be calibrated on such a period. Figure 5 shows that at *Courcival_P3*, the period 1991 to 1995, on which the model is calibrated for the split-sample test, is drier than the one from 1986 to 1990.

20. "*L. 552 - 554: This is a hypothesis that can be tested on your data. Please do so.*"
Reply: The editor is right. Regarding *Courcival_P3*, the split-sample test showed that S_{inter} is stronger on the second period from 1991 to 1995 (175 mm) than on the first period 1986 to 1990 (202 mm). As above-mentioned, the second period is drier. We propose the following correction:
    - To remove "and Smax" at the line 575;
    - To integrate this example at lines 575-576:

"Regarding *Courcival_P3*, the split-sample test showed that S_{inter} decreased from 175 mm on 1986-1990 to 202 mm on 1990-1995, a dry period (see Fig. 5)."

21. "*L. 558 - 561: Above you argue that crops are not relevant. This is now confusing.*"
Reply: The editor is right; to avoid confusions, we propose to remove the entire paragraph from lines 582 to 585.

22. "*L. 572 - 573: Is this already implemented or does it need a change in the model code?*"

Reply: The current version of SIDRA-RU requires a change in the model code to integrate deep seepage. The latter is managed in an older version of the SIDRA module (see comment 8 in the "Further Comments" part).

23. "*L. 604 - 605: Thus, a good model can be used as a decision-making tool, for example to restrict pollutants' application during flow period for the case of pesticides: It is a typical claim to state that a model is required to support decision making. But is it actually true? Practitioners generally know very well flow periods and if they were to restrict e.g., pesticide or fertilizer applications during such periods, models weren't necessary, I'd argue. In such a situation it most probably not the lack of knowledge regarding the flow regime that limits such restrictions but the crop-specific timing and needs for nutrients or crop protection. Please be more precise in describing what can be achieved for practice for which there is hardly an alternative for models.*"
Reply: The editor is right; a better knowledge dealing with the crop-specific timing and needs for nutrients or crop protection could be an efficient way to protect agricultural water against pollutants. However, to be able to anticipate the start of flow might also have a major contribution in such a context. Lewan et al. (2009) attested that this is the one of the most efficient strategies helping farmers to reduce pesticide transfer risk. Some solutions recommend to use restriction based on water content instead of restriction timing or ban (Brown and van Beinum, 2009; Lewan et al., 2009). We proposed to add the following sentence at lines 633-635:

"Indeed, using water content as an indicator to anticipate the start of drainage flow in order to reduce pesticide applications is a recommended strategy instead of restriction timing (Brown and van Beinum, 2009; Lewan et al., 2009)"

24. "*L. 608 - 609: How do transfer function solve this problem? You need to come up with an adequate transfer function model. A key aspect is that water flow is not identical to solute transport. This implies that conceptually, one has to add components such as to account for the transport aspect. Important in this context: fast transport is of high relevance for sorbing (and degrading) compounds such as P or pesticides. Hence, even if surface runoff may be irrelevant for simulating the water fluxes and the water balance, it may be essential to account for runoff (and preferential flow to tile drains) in the model concept. Often this implies that one has to introduce a shallow top-soil layer to account for the crucial processes (sorption, degradation, mobilisation) controlling the fate of agrochemicals. Refer to relevant conceptual models in the literature to provide some more depth to the discussion*."
Reply: The editor is right; we totally agree with this summary of solute transport in tile-drained soils. The transfer functions will not fully solve the problem, compared to the more physically-based models mentioned-above, but constitute a simple approach to be coupled with the SIDRA-RU model. In this framework, choosing an adequate transfer function is strongly required. Regarding the PESTDRAIN module (Branger et al., 2009), two reservoirs are integrated, one for fast transport and one for slow transport, and both of them use their own transfer functions, being exponential type. Furthermore, PESTDRAIN already integrates surface runoff, required step as highlighted by the editor. We believe that citing Magesan et al. (1994) and Branger et al. (2009) is sufficient.

25. "*L. 618: The term exhaustive seems inadequate given the model limitations on e.g., clay soil that are mentioned above*."
Reply: The term "exhaustive" has been replaced by "large" at the line 648.

We hope this version fulfils your requests. We added a sentence in the Acknowledgments part to thank you and the reviews for your relevant and valuable comments, which strongly improved the quality of this paper.

Best regards,

Alexis Jeantet

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