Reply for comment of Reviewer #1 on “Estimating the degree of preferential flow to drainage in an agricultural clay till field for a 10-year period”.

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1 Introduction

We have addressed and carefully considered the constructive comments/inputs by Reviewer #1 and made the suggested revisions and modifications where we find them appropriate. We very much appreciate the extensive review of our manuscript provided. Altogether we believe the manuscript with the modifications has improved with the revisions made and we very much hope that you will consider the revised manuscript for publication. Due to the policy of HESS, the revised manuscript will be not uploaded, until further approval.

2 Detailed response to the major concerns

2.1 Major concern 1.: Figure referencing, grammatical error and mixed section titles

A thorough textual and grammatical revision of the complete manuscript has been conducted as advised by Reviewer 1. This includes

- General check of the references in the text and a reordering of the figure numbers. Unfortunately we overlooked that the Latex code applied has mixed up the addresses of the references for Figure S2-S4.
- Revision of the entire text regarding the setting of paragraphs including headings and misphrased sentences. We do, however, not agree with the reviewer regarding the headings not fitting the content of section 2.1 (Field characteristics) and section 2.2 (Field-scale monitoring and field management). Section 2.1 describes the geografical location, pedological composition and hydrological characteristic of the field. The part highlighted by the reviewer added the comment "Mixing with field methods" has been rephrased to emphasise that this is additional field-specific hydrogeological information available in scientific literature to be applied in this study.

Section 2.2 describes the applied monitoring setup, such as the equipment used for measuring the climate variables (precipitation, air temperature, global radiation, wind speed and relative humidity), the timely amount of water being transported by the tile drain system of the field out of the field, depth to the groundwater table and TDR (Time Domain Reflectometry) for the continuously monitoring of the soil water content profile of the field. Some geological and pedological references, details and methodology were included into this section, alongside the deployed equipment, in order to explain the purpose and limitations of the monitoring data in regards to their use in this modelling study. No changes have been made to this section. Additional, we have

A) included the former section 3.1 into section 2.4 (Model concepts) to focus the section 3 on the optimization of the water and bromide balance;

B) changed the title of section 3.2 to "3.1 The impact of macropores on model performance", 3.3 to "3.2 Sensitivity analysis of the six macropore models", 3.4 to "3.3 Calibration of the water balance of the six macropore models", 3.4.2 to "3.3.2 Soil water content at 25 and 60 cm depth", 3.4.3 to "Drainage dynamics", and 3.5.2 to "3.4.2 Macropore settings";

C) split the Introduction section into paragraphs to make it easier to read.

2.2 Major concern 2.: Misleading title

The reviewer has a point. We have therefore:

- changed the title into "Estimating the degree of preferential flow to drainage in an agricultural clay till field based on 10 years of monitoring data".

- revised the final part of the Introduction to clarify, which data is applied for what purpose (calibration or "validation"). The revision are as follows:
  "The purpose of this study was, based on 10 years of monitoring data (1st April 2000 to 1st April 2010), to evaluate whether it is possible with the DAISY model to capture the water balance of a well-described agricultural clay till field that is tile-drained and prone to preferential transport (Ernstsen et al., 2015) and, if so, to estimate the degree of macropore flow and the associated transport of bromide to the drains.

  Seven different conceptual models were set up having realistic coherent upper and lower boundary conditions: one without macropores and fluctuating groundwater table measured in downstream well and three with different macropores settings (macropores to: tile drain, + just below tile drain, + fracture) being exposed to either the fluctuating groundwater tabel measured in the downstream or upstream well. The periode 1st April 2003 to 31st March 2008 was selected for the calibration of the models given it's large variation in climate. The rest of the time period was applied to evaluate on the ability of the different calibrated model concept to capture the water and bromide balance of the field."

We have presented one season from the development dataset (2006-2007) and the two season for evaluation purposes (2000-2001 (water + solute) and 2008-2009). On the other hand, we understand the concern of Reviewer #1, in regard of presenting the outcome of the entire drainage period. Therefore, we can incorporate Figure R1 with some additional description of the complete time-series.

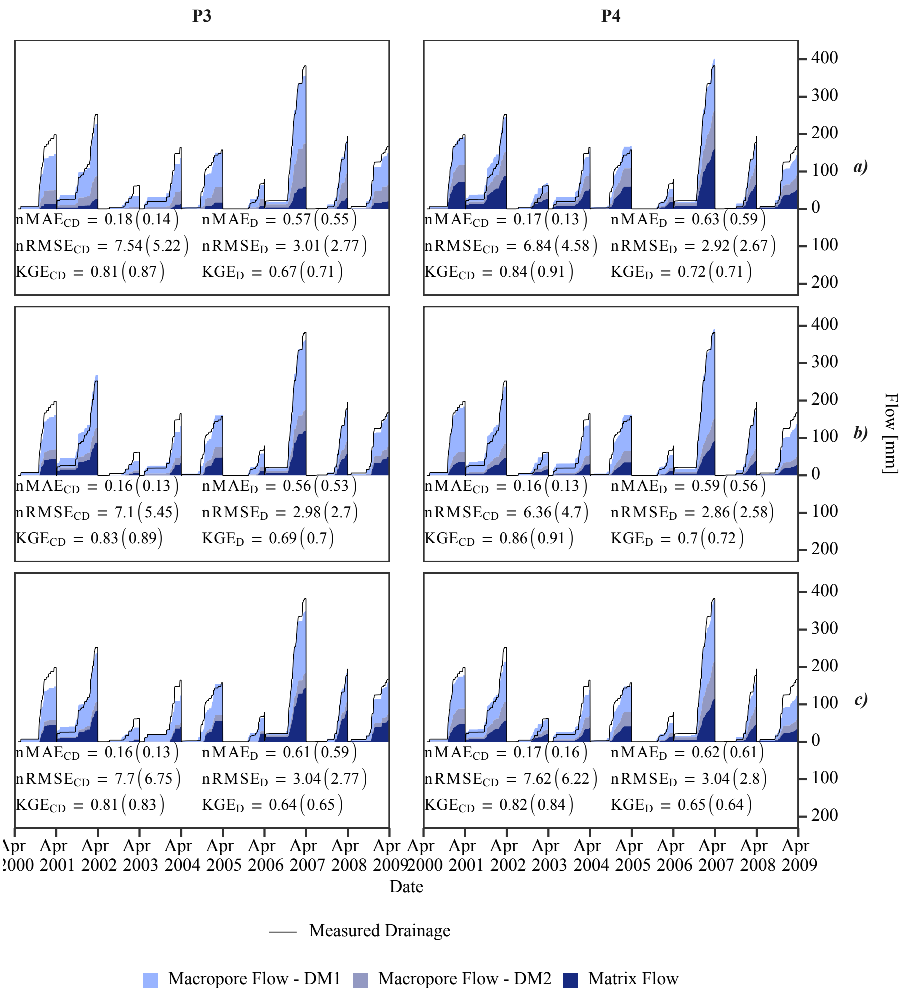

Figure R1. Measured and simulated (from matrix, macropore type DM1 and DM2) accumulated drainage for each of the hydrogeological years of the period of April 2000 until the end of 2009 for the three macropore settings (a, b and c) being exposed to the fluctuating groundwater table measured in well P3 or P4. The corresponding nMAE, nRMSE[%] and KGE for 2000-2009 drainage at 1.1 m drain level for all the six model setups with values in brackets for the whole calibration drainage period (2003-2008).

2.3 Major concern 3:There is some misfit throughout the entire manuscript between the presented and required information.

Again, the reviewer has a point. The following revisions have been conducted:

- Figure S1-S4 have been combined into Figure 1 taking outset in Figure S3 though only showing the points with measurements applied in this study and the point for soil sampling as suggested by the reviewer.
- Figure 1 are changed into Figure S1.
- Figure 4 and 5 have been combined into Figure 3 to facilitate a hydrogeological overview.
- Figure 6 and 7 have been combined into Figure 4 to visualise that the six model concepts capture quite well the monitored drainage in both the period of calibration and two periods of "validation".
- Figure 8 will now be converted into Figure 5.
- Figure 9 is now given as the new Figure 6 shown above.
- Figure 10 is now Figure 7.
- Figure 11 is now Figure 8.

2.4 Major concern 4: Critical review of the applied calibration procedure and equifinality

We recognize that equifinality, may pose credibility issues on the parameter search described in the manuscript. But we also would like to highlight, that our intention was to evaluate the conceptual description of preferential flow module adopted in Daisy, as well as to evaluate the conceptual uncertainty of describing the Silstrup field.

In order to reduce uncertainty and decrease the possibility of equifinality of these models, several measures can be taken. We tried to adopt the recommendation of Refsgaard et al. (2007):

- Such as multiple model simulation to identify uncertainty about model structure. Instead of doing an assessment using one stand-alone model,
- the modelling was carried out using different models of the same system, which was done by having different conceptual models based on different geological, in this study case pedological, interpretations.

To further reduce the uncertainty of the model sensitivity analysis was carried out based on different concept. Therefore we could exclude parameters which has relatively low impact on the model outcome, indirectly reducing the problem of equifinality.

We also want to emphasize that we aimed to provide effective parameter sets for each conceptual approach, which are capable to describe the field hydro-mechanics in relation to preferential transport of water and solute. Of course, this study could be brought further by shrinking the prior distribution to generalized posterior distribution, which can be utilized for predictive modelling, but again this was not the aim of this study.

2.5 Major concern 5: What are the lessons learned from this study and what is the novelty of this modelling approach?

We thank the reviewer for these eyeopeners. Yes, we fully agree with you that this dataset applied is unique. For fields with no or less monitoring taking place we have added the following recommendations extracted from this study to section 3.6:

This study based on an extensive dataset and knowledge regarding a field reveals that if conducting leaching risk assessment modelling for clay till agricultural fields it is a necessity to be able to account for water and solute transport through macropores such as biopores and fractures.

The degree of preferential flow and transport of contaminants (like nitrate and pesticides) through drained clay till field to tile drain systems and groundwater can easily be underestimated applying the current models approaches. In this regard it is important to be aware of the choice of model to be applied and its limitations especially in its representation of macropores. With the one-dimensional DAISY model it was not possible to allow precipitation to enter the macropore domain directly at the soil surface nor was it possible to include a fracture domain in a physical acceptable manner here among allowing water and solute to leave the fracture domain directly at the bottom of the model domain like in the model study by Rosenbom et al. (2008). Having such drawbacks in the modelling code lead to that the macropores initiated at the plough pan had the highest impact on leaching in this study. Even though the model outcome may not be completely realistic given the conceptual setting of the macropore and matrix domain, the model have been restricted by a realistic upper and lower boundary condition, soil water contents in the profile and sink-term (measured drainage from the field), it emphasis the high degree of preferential flow and transport to drainage.

For clay till fields that are not or scarcely monitored, these findings emphasise the high value of having coherent timeseries of climate (the upper boundary condition) and fluctuating groundwater table (lower boundary condition), since such monitoring data gives an indication of the effective porosity of the clay till setting and hence the degree of effective transport pathways in the soil profile.

As observed by Rosenbom et al. (2008) and Urbina et al. (2019), the results reveal a build up of water and solute in the dead-end macropores (MM) minimizing the leaching to the active hydraulic system of the clay till. The choice of MM properties in this study, however, seems to allow this type of macropore to fill completely up with water during the some fall-winter periods as observed by Rosenbom et al. (2008) making them hydraulic inactive in the soil profile and leaving water and solute to be transport through either the soil matrix or as observed by Rosenbom et al. (2008) the biopores being connected to and drained by deeper fractures. The model concepts applied can therefore be underestimating the degree of rapid flow and solute transport to groundwater via macropores by not being able to cope with biopores-fracture connections making a rapid by-pass on the soil matrix possible and hence store the excess of water and solutes in the low-permable matrix.

3 Detailed response to the minor concerns

Line 4: We can use "parametrization" instead to avoid word repetition, but we are using "concept" throughout the manuscript.

Line 37-38: The sentence has been deleted - it did not provide extra value.

Line 44-45: As demonstrated by Germann (2018) this is not straightforward, since the hydromechanics of the macropores and the interaction with the lower-permeable soil matrix still pose a scientific challenge. The " *in terms of avoiding too crude a description thereof.*" has been deleted.

Line 49: Agree it is only the water flow, which is described by Richards (1931) by "and solute transport" has been deleted.

Line 59: No changes has been made since as Simunek et al. (2003) state, "one disadvantage is that the kinematic wave equation is limited to vertical gravity-driven flow, since capillarity is ignored."

Line 70-71: The experiment of Tofteng et al. (2002) showed that macropore flow appeared below -20 to -30 cm pressure potential, which is outside of the the expected 0 to -10 cm. Therefore, it cannot be described by a general retention - conductivity function such as the Mualem - van Genuchten retention model. The following text has been added instead "be initiated under much drier conditions in the matrix domain and hence in longer periods of the year than earlier foreseen."

Line 87: The sentence has been modified into *The field monitoring was started in 1999 and prior to that, it has been used for agricultural purposes since 1942 and systematically drained since 1966.*

Line 93 The top layer refers to the upper part of the clay-silt mixture found by the geological survey (Abiltrup, 1999). No changes.

Line 94: Clay-rich glacial deposits, covers large areas of Denmark. The upper layers of these deposits are typically weathered and fractured, which can increase the otherwise low hydraulic conductivity of the deposit Rosenbom et al. (2008). No changes.

Line 98-99: This sentence has been rephrased into "The depth to the groundwater measured in several piezometers and monitoring wells surrounding the field indicated that the groundwater table measured in the Oligocene clay-silt deposit represents the field conditions including the measurements of drainage optimal".

Line 101: No changes. According to Freeze and Cherry (1979), "The definitions of aquifer and aquitard are purposely imprecise with respect to hydraulic conductivity. This leaves open the possibility of using the terms in a relative sense. For example, in an interlayered sand-silt sequence, the silt may be considered the aquitard, whereas in a silt-clay system, they are aquifers."

Line 103: The most common horizon subdivision was Ap-Bv-Bt(g) and Ap-BC-Cc, was based on the field description of Lindhardt et al. (2001). This has been added.

Line 107-116: : We think, soil sampling and DUALEM measurements and their methodology is tightly related to the field characteristic, therefore the highlighted part should be the inherent part of Section 2.1..

Line 128: The reference of the Figure has been fixed.

Line 138: The NO3 measurements of the drainage information is not relevant for this study and has hence been deleted as suggested by the reviewer.

Line 162: *transport* has been deleted from the sentence.

Line 189: "interaction between macropores and surface water" has been rephrase to "infiltration of surface water to the macropore".

Line 257-259: Sentence has been deleted and added in a rephrased version to the 3.6 Perspective section.

Line 261.a: For macropore parameterization we use the synonym of macropore setting (MSET). The "concept" refers to the composition of different lower boundary and MSET model, e.g. MSET *a)* +P3. Also this is indicated by the title of Secction 2.4.

Line 261.b: Rephrased to *...the desired performance by fitting simulated processes to the monitoring data,..*

Line 290: "is considered satisfactory" is grammatically and contextually correct. No changes has been made.

Line 291: Half of the standard deviation of the measured data, in term of nMAE and nRMSE, and 0.5 in terms of KGE, called threshold(TH) and it is indicated in Table 3. The sentence has been rephrased into "These values are called thresholds (TH) in the following.

Line 309-310: Here we wanted to emphasize, the need of a "pre-calibration" certain crop parameters in order to avoid calibration on a crop system which are in stage of water stress. We have rephrased the sentence to "depend on many factors such as soil-matrix-crop conditions and the climate".

Line 345: The following information has been added to section 2.6. for clarification: *The Morris screening method is a One-At-the-Time (OAT) method, by evaluating the impact of the input parameters by changing their value one at the time. The measure of the impact is called Elementary Effect (EE). By calculating the EE of each input parameters, it can be determined whether the input parameter has a (a) negligible, (b) linear and additive, or (c) non-linear effect on the model output or a parameter is involved in interactions with other input parameters. The two measures of the sensitivity method is the mean μ^* of EE, which represent the overall importance of a parameter and standard deviation σ, which describes the interaction and non-linear effects of a parameter. For further information see Morris (1991) and Campolongo et al. (2007).*

Line 348: It is a short summary of the sensitive parameters based on the model concept. In a tabular way looks the following.

Line 353: We meant, that out of the 3 horizon (A, B, and C) retention parameters, the most sensitive parameters were yielded from horizon A and B.

	MSET + *a)*	MSET + *b)*	MSET + *c)*
P3	14	13	19
P4	16	15	19

Line 358: "The MM macropores contribute very little thereafter to the rest of the hydraulic environment." We also agree, that the dead-end macropore can and will increase the soil water storage. But the statement of our refers to the MM macropores and their parameters involvement in the model outcome according to the sensitivity analysis (Figure S6-S8). The output of the sensitivity is mentioned in Line 348. Additionally, new text has been added in the "Perspective" section regarding this.

Line 369: The whole sentence including "not being in direct contact with the tile drain has no significant impact on the drainage." has been rephrased into "This may be because the 1D-concept c) can not include a realistic 2D-fracture setting and hence not the influence of a fracture on the drainage in a right 3D manner represented in 1D". We wanted to indicate, that the implemented fracture concept (FR) in model MSET *c)*+P3, and MSET *c)*+P4, give no benefit to the model outcome, since it has no direct connection to the drain pipe.

Line 399-404: We agree with the reviewer that the sentence is not clearly phrased.

It has been reformulated from Line 396 until Line 404:

The ability of the mode to describe the SWC measurements at 60 cm *depth was not as good as for the upper depth, due to the measured soil water content at 60* cm *depth did not respond to plant water uptake and the groundwater changes in the same way as at 25* cm *depth. This was especially pronounced for the growing seasons 2000, 2003, 2007, and 2009 and there was a delayed reaction from the TDR in the growing season of 2005 according to the model results. However, a good agreement between model and measurements were found for seasons 2004 and 2006 (Fig. S10 and S11, middle graph). The reason why the TDR measurements stayed at this high water content during the growing seasons 2000, 2003, 2007, and 2009 is not clear, especially as in some years a clear response with lowering of the water content due water abstraction by the crops, were measured. A possible explanation may be that the actual crop in these years did develop a water abstraction pattern not sensed by the 30 cm TDR probe, e.g. roots tend to concentrate their growth in macropores, a mechanism not included in the model.*

Line 417: "may be explained partly by too low a precipitation input to the model." As Figure R2 shown, the intensity of the rainfall (4 $\mathrm{mm}/\hat{\mathrm{h}}$) in early March, before the peak drain event (2 mm/h), is similar to the intensity occurring in January multiple times. None of these January rainfall events causing such a large drainage $[\mathrm{mm}/\mathrm{h}]$, which appeared in March. As it can be also seen, the model is able to simulate the drainage in January, therefore we can assume, that the too low precipitation input is causing the under prediction of the model in March. During the precipitation in March event the temperature varies between 0$°C$ - 6 $°C$, therefore we can assume it fell as snow and the Geonor rain gauge were malfunctioning and under-measuring the actual precipitation.

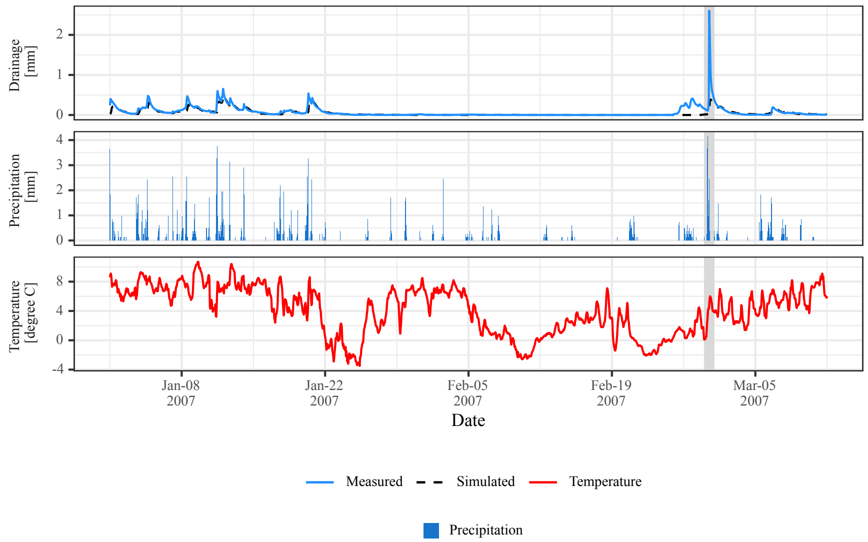

Figure R2. Measured hourly precipitation [mm], air temperature and measured drainage [mm] with the simulated drainage of the model MSET *b)* + P3 between 1st January 2007 and 12th March 2007.

Line 438: Agreed, out of this four evaluation case (Fig. 7) the MSET *a)* and MSET *c)*, performs better than MSET *b)* during the season of 2000-2001 with P3 lower boundary condition, but in all other cases MSET *b)* outperform MSET *a)* and *c)*. Thereby, we can state that "MSET *b)* performed better than MSET (*a)* and *c)*". This has been added to the manuscript.

Line 450: We agree with the reviewer, additional information has been added. "a distinct heavy rain event *(sum: 40.6* mm*, mean: 10.2* mm*,max: 25.1* mm*)* was screened for model response"

Line 457a: As the reviewer states, it is crucial, when we account for macropore flow, that the temporal scale of the preferential flow can be minutes or seconds. In Daisy, the transport mechanism inside the macropore is instantaneous, thereby, when the soil matrix steps over the entry pressure ($\psi_i nit$) of the macropore region, then yes the model is capable to simulate the influence of preferential flow on the soil drainage, as it is seen in Figure 8.

Line 457b: In Line 190, there was a contextual error, we meant The macropore model does **not** express viscous flow, **in contrast** as Germann (2018) suggested,"

Line 467: The 70% refers to the amount of drainage originated to the macropore flow. We understand, that it is difficult to see from Table 4. Therefore a following text has been added to the manuscript. "...estimating a high macropore contribution to drainage (113 mm, 70% of total drainage *(160 mm = From matrix 47 mm + From macropore + 113 mm) Table 4.*).

Line 481-486: In order to not add extra Figures and Table to this already extensive manuscript, we decided to express the amount of crop uptake of BR^- in the percentage of the applied amount.

	MSET + *a)*		MSET + *b)*		MSET + *c)*	
	% of 20.14 $\mathrm{kg\,Br^-\,ha^{-1}}$	$\mathrm{kg\,Br^-\,ha^{-1}}$	% of 20.14 $\mathrm{kg\,Br^-\,ha^{-1}}$	$\mathrm{kg\,Br^-\,ha^{-1}}$	% of 20.14 $\mathrm{kg\,Br^-\,ha^{-1}}$	$\mathrm{kg\,Br^-\,ha^{-1}}$
P3	67	13.5	57	11.5	52	10.5
P4	59	11.9	60	12.1	54	10.9

Line 490: The sentence is corrected.

Figure 11: The dashed lines describe the horizon division. For better understanding extra notation added to the figure, indicating the name of the horizon.

Table 1. The gray rows are there, but for the better distinction, we darkened the rows.

Table 2. The gray rows are there, but for the better distinction, we darkened the rows. Example:

Macropore parameters	Initial	Uncertainty boundaries		Best calibration MSET b)		MSET	Sensitivity	
		Min.	Max.	P3	P4		P3	P4
$Depth_A$ [cm]	31	20	40	-39.29	-38.93	*a), b), c)*	*a), b), c)*	*a), b), c)*
$Depth_B$ [cm]	140	130	150	140	140	*a), b), c)*		
$\psi_{init/term}$ [cm]	-5	-15	-1	-2.93	-14.73	*a), b), c)*	*a), b), c)*	*a), b), c)*
$\psi_{barrier}$ [cm]	-5	-15	-1	-5	-14.62	*a), b), c)*	*a), c)*	*a), b), c)*
ρ_{DM1} [$\mathrm{m^{-2}}$]	3.5	1	20	5.489	11.33	*a), b), c)*	*a), b), c)*	*a), b), c)*
ρ_{DM2} [$\mathrm{m^{-2}}$]	3.5	1	20	12.643	1.71	*a), b), c)*	*a), b), c)*	*a), b), c)*
d_{DM1} [mm]	3	1	5	3	3	*a), b), c)*		
d_{DM2} [mm]	3	1	5	3	3	*a), b), c)*		
ρ_{MM1} [$\mathrm{m^{-2}}$]	100	1	100	100	100	*b), c)*		
ρ_{MM2} [$\mathrm{m^{-2}}$]	11.5	1	20	11.5	16.79	*b), c)*		*b)*
ρ_{MM3} [$\mathrm{m^{-2}}$]	11.5	1	20	11.5	11.5	*b), c)*		
d_{mm1} [mm]	2	0.1	3	2	2	*b), c)*		
d_{mm2} [mm]	3	1	5	3	3	*b), c)*		
d_{mm3} [mm]	3	1	5	3	3	*b), c)*		
$K_{s,D}$ [$\mathrm{cm\,h^{-1}}$]	10	1	100	10	10	*c)*	*c)*	*c)*
ρ_{FR} [$\mathrm{m^{-2}}$]	3.5	1	20	3.5	3.5	*c)*	*c)*	*c)*
d_{FR} [mm]	0.1	0.01	1	0.1	0.1	*c)*	*c)*	*c)*

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