Reply to Reviewer1

Thank you very much for your thoughtful comments. Our replies to your comments are as follows. We will revise the manuscript to incorporate all of these discussions.

Comment 1: necessity of two layer model

Thank you for your important comment. As mentioned in section 5.4, in a turbidity current flowing over hundreds of kilometers, the upper layer of the current is predicted to be continuously diluted while the lower layer remains highly concentrated, thus maintaining the current over long distances. Existing one-layer shallow water equation models are insufficient to reproduce such phenomena. The forward model of this study is not an exception.

However, the focus of this study is on rapidly decelerating sedimentary turbidity currents. Normally graded turbidites are considered to be deposited from such decaying flows. In this study, the distribution of turbidites is assumed to be limited to about several tens km at most, and the separation of the lower and upper layers that occurs in sustained turbidity currents after flowing tens of kilometers does not need to be considered when calculating such relatively small-scale turbidity currents. In fact, Kostic and Parker (2006), on which the forward model of this study is based, has been verified to reproduce turbidity currents at experimental and small natural scales (e.g., Fildani et al., 2006). This suggests that the inverse model in this study is well suited to analyze a single bed of turbidites that generally exhibit normal grading in strata.

It is expected that turbidity currents maintained over long distances do not leave turbidites and create a bypassing zone. Otherwise, the concentration in the lower layers of turbidity currents decrease, and therefore the currents stop within a relatively short distance. Thus, a two-layer model of turbidity currents is not always necessary for inversion of bed-scale turbidites. However, modeling of continuos sustained turbidity currents is necessary for inverse analysis of the development of submarine fans and channel-levee systems in a larger scale. This is an interesting research topic in the future.

We fully describe this aspect in Section 2 and Section 5.4 of our revised manuscript (Lines 113–123, 490–495).

Comment 2: the reason why the inverse model reconstructs initial conditions

As you pointed out, our objective of the inverse analysis is the local conditions of a turbidity current (velocity, concentration, etc.) that resulted in a turbidite. That is why we used inverse analysis to estimate the initial conditions of the flow. This is because, as shown in Figure 9, we can obtain the state of the turbidity current at any location and time by calculating the time evolution of the forward model from the initial conditions. On the other hand, if we conduct inverse analysis to reconstruct all the values of the local flow conditions at a certain location, we have to estimate an almost infinite number of parameters because the flow velocity and concentration at the point of interest keep changing with time. If we consider all the hydraulic conditions at any given time as independent parameters, the number of values to be obtained in the inverse analysis will be the same as the number of time steps in the forward model. To avoid this situation, we decided to reconstruct the initial conditions at the onset of the flow. In this way, we can obtain the behavior of the flow with a relatively small number of parameters. This approach has already been tried successfully by Lesshafft et al. (2011), and Falcini et al. (2009) also reconstructed flow conditions of turbidity currents by obtaining boundary conditions of the model. Thus, the approach we adopted here is a standard procedure in this research field.

This is an essential point for the inverse model in this study, but we acknowledge that it has not been fully explained yet in the current text. In the revised manuscript, we explained these points in the Section 3 (Lines 184–188).

Comment 3: length of slope

In this study, the length of the slope located upstream was not changed. The slope from the point of flow occurrence to the bottom works as a hypothetical generator of turbidity currents. The hydraulic conditions on the slope do not necessarily have to be realistic because the role of the slope is to allow the sediment cloud to acquire the structure of a turbidity current and the flow conditions on the slope are not subjects to be estimated. If the length of the slope is changed, the flow initial conditions on the slope may vary. However, our aim is to obtain sufficiently realistic values of the velocity, concentration and thickness of the flow on the sedimentary basin. In other words, if the hydraulic conditions at the upstream end of the basin are the same, we do not need to worry about the differences in velocity and thickness of the flow on the slope caused by differences in slope length. To clarify that the slope length is not significant in this study, we conducted new numerical experiments to show the effect of the slope length on the results of the inverse analysis in the revised manuscript (Lines 281–287, 346–364, 423–432).

Reply to Reviewer 2

Thank you very much for your thoughtful comments. Our replies to your comments are as follows. We will revise the manuscript to incorporate all of these discussions.

Comment 1: Bed amalgamation and disturbance

Thank you for pointing out a very interesting issue. Our inverse model produced robust results when the artificial data was subjected to quite large noise. Therefore, even if localized and small-scale scouring and sedimentation occur due to some processes such as bottom currents after the deposition of a turbidite, results of inverse analysis will not be seriously affected. However, if deposits of multiple events are amalgamated to form a single thick massive sandstone, the hydraulic conditions reconstructed from the bed will be considerably different from the actual conditions. To avoid this situation, it is important to identify the erosional surface inside the bed carefully at the actual outcrop. In addition, it is safer not to analyze massive sandstones that are more than several meters thick, because they are likely to be amalgamated deposits. These precautions were described in the Discussion section of the revised manuscript (Lines 417-422).

Comment 2: Self-acceleration

Thank you for your interesting comment. Actually, it is in the case of self-acceleration that the inverse analysis of the initial conditions becomes easier. The relationship between turbidity currents and characteristics of turbidites is nonlinear. Especially when the flow is self-accelerating, a small difference in the initial conditions can result in very different sedimentary characteristics. This means that it is easy to find the initial conditions of the flow by inverse analysis, because even if the characteristics of the deposits are very different, the initial conditions of the flow should not be so different. Thus, the inverse results in this case are expected to be robust even if there are some measurement errors in characteristics of deposits. In other words, there is a tradeoff between the robustness of the forward and inverse modeling.

This property of the inversion can be understood when we consider the opposite case. If the initial conditions of the flow are different but the characteristics of the turbidites are exactly the same, it is impossible to estimate the flow conditions from the turbidites. The inverse analysis of hydraulic conditions is possible because the depositional characteristics are sensitive to conditions of turbidity currents. The self-acceleration of turbidity flow is an extreme example of the sensitivity of turbidites to the flow initial conditions. This issue was explained in section 5.1 of the revised paper (Lines 380–390).

Comment 3: Applicability to field scale problem

The applicability of the method to actual turbidites is a main topic of this paper. It is unlikely that our model will have the same results as Parkinson et al. (2017) because their model has an essential difference from ours, and this is the unequivocal reason why their inversion results were not realistic. Their model does not consider resuspension (entrainment) process of sediment, where as suspended sand in turbidity currents is maintained by balancing the effects of particle settling and diffusion from the bottom (i.e. entrainment). Their model only considers advection and settling of particles, so that the suspended sediment quickly settles and be lost over short distances at realistic flow thicknesses and concentrations. The only way to transport large amounts of suspended sediment for long distance and to deposit thick turbidites without resuspension is to make the flow extremely thick or to suppose unusually high velocity or concentration. This is the reason for that the extremely thick flow depth (more than 3000 m) was obtained in their results. Their inversion method requires iterations that cannot be parallelized, so that the forward model needs to be simplified for this purpose. Our inverse model, on the other hand, can completely parallelize the forward model calculations, and we do not need to obtain an analytical solution for the gradient of the objective function unlike their method. Therefore, we were able to adopt "full model" that incorporate the entrainment process of suspended sand into our model without any problems. As a result, our inversion did not produce any anomalous reconstructions even though most of our test data exhibit thickness and grain size distributions similar to realistic turbidites. This strongly suggests the robustness of our inverse model and its applicability to real turbidites.

Of course, the analysis of ancient turbidites is an important issue in the future. However, even if ancient turbidites are analyzed, it is not possible to verify that the results obtained are correct, because the hydraulic conditions for ancient turbidity currents are unknown. Another way to verify the validity of the method is to reconstruct the hydraulic conditions of experimental turbidity currents from the turbidites deposited in the flume, and compare them with the measured values. The turbidity currents measured in the modern submarine canyons and their deposits would be another candidate to be used for the model verification. However, before proceeding to these steps, we suggest that it is important to thoroughly examine the validity of the method using artificial data.

We have already described some of these points described above in the discussion section, and described them in more detail in the revised manuscript (Lines 451–458, 460–463, 501–505).

Comment 4: Novelty in methodology

This paper has a methodological novelty in that it achieves inversion of unsteady and nonuniform flows. Our research group was the first to develop a neural network based inversion method for event deposits, and the first application of this framework was the inversion of tsunami deposits by Rimali et al. (2020). This is described in the introduction section. However, the forward model used in their study was based on the assumption of quasi-steady flow, and thus our work is the first time to perform the inverse analysis using a neural network with completely unsteady flow. In addition, there are various differences in the properties of tsunamis and turbidity currents. In the case of turbidity currents, the amount of suspended sediment is linked to the driving force of the flow, while in the case of tsunamis, the two are independent of each other. Therefore, an increase in the concentration of suspended sand does not affect the flow dynamics of tsunamis. Because of these differences, there was no guarantee that the inverse analysis of turbidites would give good results, even if a similar inverse analysis framework was used for tsunami deposits. The success of the inverse analysis for turbidity currents, which exhibit quite different properties from those of tsunamis, indicates the wide applicability of our inversion framework for event deposits.

These novelty in our methodology was described in the Introduction of the revised manuscript (Lines 99–106).

Line-by-line comments

Line 7

We agree to unify the terminology to "layer averaged model", and revised the manuscript.

Line 11

We used the number 3500 to mean a relatively small number. However, for the sake of clarity, we will remove this sentence in the revised paper. Instead, we will revise the sentence in line 8 as follows:
A reasonable number (3,500) of repetition of numerical simulation using one-dimensional shallow water equations under various input parameters generates a dataset of the characteristic features of turbidites.

Line 279–280

We added the following explanation (Lines 311-316):
We do not fully understand why the results are not stable for sampling windows shorter than 5 km, but it probably indicates that the training results fall into a local optimum solution depending on the initial values of the weight coefficients of the neural network (given by random numbers) due to incomplete information. In any case, the loss function is very good (less than 0.01), so that even turbidites that can be tracked for less than 5 km are likely to give good results if the outcrop spacing is sufficiently narrow and detailed observation of beds is possible.