Dear anonymous referee #1,

thank you for the very detailed and profound review of our manuscript and the additional recommendations on the literature. We really appreciated your effort and insights. In fact, we found an error in our data, due to an unfortunate isotope dampening event, leading to the restructuring of our reasoning in the results and discussion segment. Two samples are now excluded from the analysis, and the description of the discharge dynamics now paints a more intuitive picture. We applied changes to all affected plots and tables.

Event water and solute concentrations Export patterns of N and P were analysed for total Q and event-Q. However, it is not explained which concentrations have been assigned to event-Q. Assuming that pre-event Q (or everything that is not event-Q) has a constant concentration, which (if I got it right) was also the underlying assumption for event-Q estimation via the isotopic signal, every change in concentration must be attributed to the event water. This way, even a small increase in the concentration could indicate a much higher concentration in event-Q compared to pre-event Q, especially if event-Q makes only a small percentage of total Q. How was that handled in the analysis? As there is nothing explained in this regard, I assume that the same concentrations as measured in total Q were also assigned to event-Q, and I find this conceptually questionable.

In line with that, I miss a paragraph discussing the uncertainties of the event-Q calculation and what that might mean for the interpretation of results. Also, what happens if water from shallow flow path gets mobilized that has a different isotopic signature? This should at least be discussed.

Further, I strongly recommend reading von Freyberg et al. (2018) on the topic of event water calculation using stable isotopes.

> We sincerely appreciate your literature recommendation and your comment on our approach, as it gives us the opportunity to clarify our reasoning in greater detail. We fully agree on the importance of discussing the uncertainties in event-flow (event-Q) calculations using stable isotopes, and we will add a corresponding paragraph to the manuscript. However, we were somewhat puzzled by your general critique of our conceptual framework, so let us clarify our rationale.
>
> In our analysis, we separated the event water from total discharge using stable isotopes and assumed a normally distributed nitrate load in the pre-event water (as was the case during the sampling campaign). Consequently, variations in nitrate load should be attributed to the incoming event water—regardless of whether this portion mainly represents “old” water from the saturated zone or overland flow. Distinguishing between these flow paths cannot be achieved within this setup without direct groundwater or overland flow sampling. Nevertheless, if the nutrient load signal remains stable prior to the event, changes during the event can be attributed to the event-water portion, under the assumption that total discharge (Q) and event discharge (Q_e) represent mixtures of pre-event and event water from different sources. This approach is not only widely accepted (Blume et al., 2007; Klaus and McDonnell, 2013; Semenov et al., 2015, Marin-Ramirez et al. 2024) but also the most suitable and logical choice when direct sampling of subsurface or overland flow is not feasible due to resource limitations.
>
> Furthermore, we acknowledge the valuable contribution by von Freyberg et al., who introduced an approach to quantify precipitation-to-discharge fractions to trace streamflow sources. While this is an elegant and insightful technique, von Freyberg et al.

(2018) also emphasize that it represents an alternative rather than a universally superior method. Their study was conducted in a different catchment and at a different temporal resolution. From our perspective, their work delineates the internal correlations between the Q_e/Q relationship, antecedent moisture, and other parameters derived from the Q_e signal. Conceptually, our study pursues a similar objective, but with a focus on the signal of nitrate load, allowing us to examine catchment-scale event dynamics at the same level of abstraction without introducing additional layers of complexity. We believe this conceptual clarity is a strength of our approach, and we see the integration of both frameworks as a promising direction for future research

We added von Freyberg et al. (2018) to our manuscript together with all other mentioned literature, thank you very much for providing this information. We also added a paragraph discussing the uncertainties of event Q calculation using stable isotopes.

1. Explanation of NCL and comparison to other approaches

While I enjoyed reading the section on how measurements were taken, other parts of the methods lack details that allow the reader to follow how exactly the methods were conducted. This is true for the event-Q calculation, but also for the NCL method. It would further help he readers to clearly distinguish what separates this method from the power law relationship between C and Q ($C = aQ^b$), or the respective L-Q relationship ($L = aQ^{b+1}$), for example. As C-Q and L-Q are, from my experience, more commonly applied, readers need a good reason to be convinced that the alternative method presented here is a good alternative, at least for specific cases or questions.

Thank you very much for pointing this out. From our perspective, the NCL (Normalized Cumulative Load) method is widely applied in hydrology—particularly in the subfield of water quality—for analyzing concentrations and loads of dissolved or suspended substances (see for example Obermann et al. 2007;Hathaway et al. 2012; Mamun et al. 2020). The strength of the NCL approach lies in its visualization of fitted power-law curves within the normalized [0,1] parameter space, allowing researchers to easily detect and interpret differences between water portions, catchments, or events at a low level of abstraction, which makes the method highly intuitive.

In our study, we applied the NCL function to loads, whereas the C–Q method you mentioned typically uses concentrations. We chose loads as a more integrative parameter for analyzing event-scale process dynamics in the Nesselbach catchment. While you correctly referred to the L–Q relationship, this method does not normalize the parameter space. If normalization were applied, the resulting behavior would be equivalent to that observed in our approach.

Moreover, NCL functions are cumulative by definition, whereas the examples you provided are not—this explains the observed differences. As illustrated by your example ($L=aQ^{b+1}$) this equation effectively represents the integral of the L–Q relationship when the coefficient a is a positive fraction between 0 and 1. Consequently, our approach focuses on load as an expression of catchment dynamics by implicitly integrating time through the cumulative nature of the NCL function.

We added some information about recent application of the method in the introduction.

2. b --> 0 (dilution)

I do not agree with the definition of b à 0 for dilution patterns, as it is currently indicated in Figure 3. I might be mistaken, but why should a negative value of b not be possible? For the C-Q relationship described as $C=aQ^b$, a negative b indicates dilution. Translated into loads, it becomes $L = aQ^{b+1}$, which means that $b < 0$ implies very strong dilution, so strong that despite discharge going up, loads go down. This can only happen if the baseflow (or pre-event Q) concentration decreases as well, which might be rather unlikely, but it is not entirely impossible. Consequently, it should be b --> -∞ for dilution. I am happy to be proven wrong, but I recommend checking this carefully

> Thank you very much for highlighting this point! We believe the misunderstanding arises from differences in the underlying modeling approaches. While you are absolutely correct for formulations such as $L = aQ^{b+1}$, negative values of b cannot occur in our case due to the nature of the normalized cumulative load (NCL) functions.
>
> By normalizing the parameter space to [0,1], we mathematically constrain the power function to pass through the fixed points (0,0) and (1,1) This transformation forces the curve to remain within the positive quadrant, effectively shaping the parabolic form into a smooth, bounded curve. Values of $b < -1$ would correspond to hyperbolic functions that diverge towards infinity and thus leave the normalized parameter space entirely, producing unrealistically large (infinite) areas under the curve. Such behavior would indicate errors in measurements or model setup rather than meaningful parameter estimates.
>
> Even moderate negative exponents (e.g., b=−0.5b) would place the function above the unit square, yielding an integral greater than one (≈2), which again is inconsistent with normalized cumulative data. Since NCL functions are monotonically increasing by definition —each time step accumulates more load than the previous one (or keeps the value if load suddenly drops to zero)—the corresponding fitted functions must also be monotonically rising. In this setup, negative exponents cannot occur.
>
> This does not contradict the general mathematical properties of power functions but rather reflects the specific characteristics and constraints of our normalized model. We added some additional information for better clarity.

3. Start and end point of events

I could not find a description of how the start and end points of events were defined. However, I find this important, as this has the potential to severely influence the results, especially with respect to the percentage of event-Q. This needs to be clarified, and its impact on the results should be carefully checked

> A description of event delineation was added in the method section (2.2 sampling and measurement.

4. Discussion relevance and implication

I would appreciate to hear a little more about the relevance of the topic. Why does it matter? Yes, these patterns were observed, but what does that imply? This applies to the abstract, the discussion and conclusion.

> We added some more thoughts about the relevance of the topic in the abstract, the discussion and especially the conclusion.

5. Data availability

I do not see a reason why data from this study should be available upon request and not uploaded to an open repository. If there is an acceptable reason for that, it should be stated in the data availability section. Otherwise, I advocate for a transparent and easily accessible provision of the data so others can replicate he presented results

> We added the data to the supplement as an excel file, thank you for asking!

6. First flush

I got confused by the use of the term first flush and flushing behaviour in the manuscript. It appeared to me that both were used as a synonym for what in other studies is called an enrichment or accretion pattern, meaning that concentrations increase with increasing discharge. In other cases, it appeared to describe an earlier peak of concentrations as compared to discharge, which one could call first flush, or which others have described by clockwise hysteresis. I might have overread things in this regard, but the manuscript would benefit from a clear definition of what term refers to what and how these different patterns are distinctively characterised via NCL. It would also be good to clearly distinguish "first flush" from enrichment (or flushing?) behaviour, but also distinguishing it from the "first flush" that describes a disproportionally high concentration increase during the first event(s) after a drought (e.g., Winter et al., 2022).

> Thank you very much for highlighting this, we applied consistency throughout the document. You are completely correct, that flushing behavior does not necessarily also mean that a first flush had been observed. We changed parts of the manuscript accordingly, also we did not found a first-flush that would fulfill the criteria of Bertrand-Krajewski (1998) or Lee (2002).

7. Linear vs. chemostatic

In the manuscript, constant (or chemostatic) solute dynamics are described as "linear". While I understand that this term makes sense from the perspective of the NCL approach, it is somewhat confusing to readers who are more familiar with C-Q or L-Q relationships in the form of a power law relationship. There, enrichment, chemostasis, as well as dilution are linear in the log-log space. Hence, I suggest using a different terminology.

The wording was changed to “constant” or “constant behavior”.

8. Loads vs. concentrations

Throughout the manuscript, solute dynamics are often referred to as nitrate or total phosphorus, without indicating whether this is about loads or concentrations. As this makes a huge difference, also in the way results are interpreted, I recommend clarifying this throughout the manuscript.

We applied consistency.

Minor comments:

Title: I suggest either saying “The combination” or “Combining traditional…” with a tendency to the second for brevity reasons. As it is now, it reads a little odd. No point is needed at the end of a title.

Thank you very much, we did apply the changes to the title!.

Abstract

I struggle with the causal relationships in the first sentence. Is a first flush or dilution an effect of solute export dynamics? Also, see my major comment regarding the use of the term “first flush”.

We will revise the sentence, first flush is of course not an effect of solute export but a phenomenon.

L15: NO3- should be formatted to NO_3^- throughout the manuscript

We changed the format throughout the document.

L19: what are “discharge processes” – I suggest referring to hydrological processes here, if this should refer to transport processes and not biogeochemical ones.

We rephrased to “hydrological processes”

Introduction

L26: “Nutrient cycle”? This sounds like a cycle of biochemical transformations. I assume this should rather be something like nutrient storage and transport within and from catchments?

This is not an uncommon description, but we can adjust it for higher precision.

L28: “nutrient and other solutes concentrations” needs adjustment: it is nutrient/solute concentrations that are measured, and nutrients are solutes as well, not either or.

We did clarify the wording for this sentence.

L32. The point is missing.

Thanks! The point was found again!

L46: Musolff et al. and Ebeling et al. use the exponent of the power law relationship between C and Q. Not NCL, this should be distinguished.

> We removed the part this part for less confusion.

L50-51: Include insights from von Freyberg et al. (2018)?

> We now include the insights from Freyberg in the Introduction and also in the discussion. Unfortunately we don't have a long and consistent timeseries of isotope data, therefore a direct comparison with the findings of von Freyberg et al. and application of their methods is difficult.

Methods

L67: Central Uplands, Germany.

> Thanks, was added!.

L72: I suggest referring to the world reference base and not to the German one (i.e., brown earth). I guess it is Cambisol?

> We changed all soil names to the world reference base

L75: Please add the year for which the mean was calculated

> We added the year (2021)

Fig.1: What are the black lines in the land use map? Are these tile drains or just the borders between polygons? If the latter case, they should be removed. Also, the north arrow is missing

> The north arrow was added. Black lines are now correctly labeled as "farm lane". Land use is no more differentiated for the agricultural areas and we added the country road.

L82: besides à except

> Thanks! We changed this.

L87: What is the quality of the calibration? I would like to see a plot comparing sensor measurements and grabs samples with a 1:1 line and R^2 or similar in the supplement

> We provide this plot in the supplement, together with the calculation of multiple R^2. The presented calibration curve is most suitable for the range of 50 to 100 mg NO_3^-

L93: I appreciate Fig S1. Still I would have liked to see something that gives me an idea of how noisy discharge data was and how it looked like after the correction.

> We added an example to the supplement material, it can be found under S2.

L98: Where was precipitation measured and how? Can this be displayed on the map as well?

> Precipitation was measured north of the catchment. We give the coordinates of the rain gauge in the method section. We did not put it on the map for aesthetic reasons because it was slightly outside the catchment area.

L104: How does this compare to the methods of von Freyberg et al. (2018)? How were deuterium concentrations in P estimated? As a weighted mean? Also, how was the pre-event concentration estimated? Is it a mean across several values, if so which values?

> Deuterium concentrations in P were estimated by using a sequential precipitation sampler based on the design of Fischer et al 2019. Precipitation was collected as

weighted mean in 5 mm portions and measured individually. The pre-event concentration was estimated by using biweekly grab samples and samples anticipated before incoming storm events. It is the median value of those samples.

Information about the sampling of stable water isotopes was added to the method section.

L110-112: Nice!

Thanks!

L115: “using” not “by using” here and elsewhere. It does not really help to know that it is a “classical method”, common is enough.

Recommended changes were made.

L123: what is the R base package? If it is an additional package, it should be cited. Otherwise, “computed in R (R core team…)” is enough.

Changed as recommended.

L125: what is the unit of the mean rate of change?

We clarified the units for mean rate of change and volatility (its [L/s²]).

L133: With k equal to

Was changed, thanks!

L139 here and elsewhere: “=” should be written out outside of an equation

Consistency was applied.

L152: Terminology is not consistent. It should be either “nitrate” or “NO_3^-“. Further, if the minus is added, minus and plus also need to be added to all other ions (e.g. Ca^{2+}, etc.)

We applied consistency throughout the manuscript, nitrate should now be NO_3^-, anywhere. We added the charge numbers for major ions in the sentence and, also in Fig. 10 and Tab. 2.

L153: “Beginning with” sounds odd. Maybe just: Event water and total discharge from automated sampling were compared…

The sentence was revised, its starts now as recommended, thanks!

Eq.: 6: and F(X) is the concentration? Or the load? Why not say this directly? Also, Q would be the more intuitive abbreviation than X

F(x) is the load, because it is all about cumulative load function. We choose X because we try to explain which changes we applied to the underlying concept of NCL-functions, so we rooted for consistency with Bertrand-Krajewsky (1998). We increased the precision of the formula description.

Eq. 7: and here y is used instead of f(x), right? I recommend using the same (and ideally more intuitive) symbols in all equations.

Thanks for the Question, in this regard, the formula is just following publication standards. Y is not equal to f(x) because we don’t want to calculate the root mean square error of the function f(x) but from its values (y_i). While there is conceptual overlap, that would not be formally correct.

L186-188: This statement is too general. Especially as it is underlain by the citation of two studies that only span a hand full of catchments. If any, a large sample study should be cited here. For example, across Germany, Ebeling et al., (2021) show that N tends towards enrichment patterns (increase in C with increasing Q, due to the mobilisation of diffuse sources with increasing catchment wetness) and dilution P towards dilution patterns (decrease in C with increasing Q), due to the dilution of point sources. This is, if I get it right, the opposite of your statement. Note that Ebeling et al. looked at long-term patterns from low-frequency data, and that patterns between these time scales can diverge (Winter et al., 2024). However, Winter et al. (2024) showed that the tendency towards enrichment or dilution remains the same, only less pronounced, during events.

> Thank you very much for providing additional information on the literature. We did revise those sentences and expanded the section.

Results & Discussion

L192: DWD 2024 à I could not find that reference in the reference section. I am not sure if it would make more sense to name this data source in the method section and remove it here?

> Thanks for highlighting, we will added the reference and moved the sentences to the method section.

Table 1: It would be beneficial to add total Q to the table as well and to specify if "date" refers to the starting date of an event.

> We added the parameter to table 1 and specified that the date column shows the beginning of an event.

L232: From the literature (von Freyberg et al., 2018) and also intuitively, I would have expected a larger event water fraction during larger storms, as during smaller events, a higher percentage of the water fills up empty storages. The manuscript would benefit from a more detailed discussion on why the results diverge from this and from comparing their results to the literature.

> Thank you very much for reading this part profoundly, we mixed thinks up, during internal review. We removed two heavy outliers, what resulted in very different results. Our results are now way more intuitive, the events with the most precipitation now also deliver the most and not the least event water on average. This changed our reasoning and made the discussion section more stringent, especially for the description of discharge dynamics. We corrected the complete result and discussion segment and applied all changes to affected plots and tables.

L249: e.g. seems to be missing in the citation, as these are just exemplary references

> e.g. was added to the citation

Figure 6 & L257: I guess it is API_{14}?

> We adjusted this.

L264-265: see my comment above (L232). Was there a seasonal difference in the events analysed? If not, I am not entirely convinced by this argument.

> We revised this part of the discussion, for example:
> **". Because the temporal distribution of precipitation during the sampling period was unusual, with only just one event big enough to sample in autumn of 2021 and frequent precipitation in summer, the observed pattern is likely biased by the**

overweight of summer and spring events. General behavior of the catchment cannot be inferred from the available data. "

L283-284: Why is it likely to be mobilised, and why would it "normally" have a chemostatic export behaviour? Winter et al. is a good citation here, as it comes from a study comprising a comparably high sample size. However, the authors showed that event patterns are closer to chemostasis as compared to long-term patterns, not necessarily that all events are chemostatic.

We agree, and we are not assuming that every event is chemostatic, but we revised the sentence.

L290-294: I assume this is largely because the event water shows a different dynamic compared to total Q?

Yes, this should be the case since event water is only available at events and total Q is also available during baseflow. Part of the analysis aims at separating event water portion from total discharge, so we assumed that the event water signal has a governing effect on the total Q during events-

L305: I assume P_{tot} and not phosphorus?

Was corrected.

L306: Is that necessarily a dependency?

We changed dependency to "robust correlation".

Fig.7: The labels on the right are not needed, as all information is already provided on the left. Maybe lines in the plot could be colored for the different events so that readers can see if the direction of changes remains similar for the same events.

Thanks, we adjusted the plot, removed the labels on the right and introduced a color/line type based coding for events.

L315: I am not convinced the difference is unexpected (see my comment to L290-294 and my major comment regarding event-Q).

We are convinced, that the difference is unexpected. Please keep in mind, that we did not sample any event water portions independently from the total discharge portion. Without hydrograph separation the transport signal of total discharge would have overwritten the transport signal stemming from the event water.

L344: A deeper look into the literature would show quite a few studies where such patterns have been found (e.g., Dupas et al., 2016; Winter et al., 2021, …)

Here, we discuss the pattern arising from our method found in Fig. 7. Since this method was not used before, it is also not possible that this pattern has been observed before. We changed the wording for more clarity

L332-334: This explanation is needed earlier in the manuscript + additional explanations (see major comments on first flush)

We moved the explanation to the introduction and elaborated further. Thanks a lot for mentioning!

L351: Shouldn't this be introduced in the method section already?

It is introduced in the method section, see line 129.

Table 2: I recommend adding the ratio (CV_C/CV_Q) here as well. It would enable a nice comparison to studies such as those from (Musolff et al., 2015).

This table does not refer to (CV_c/CV_Q) ratios, introduced by Musolff et al. Here we just present the coefficient of variation used as a statistical metric for comparing distributions of ion loads in the discharge and linking it to differing patterns for the cumulative load functions.

L370: The drainage system is likely to have a strong impact on the results observed. A deeper discussion on this, also in comparison with other studies with and without such systems, could potentially add much value to the discussion.

Unfortunately, the data on the drainage system in this catchment is very scarce and obscure, we have no reliable information. We added some explanation in the method section.

L382: What is “sufficiently large”? Can this be specified?

We revised the sentence, sufficiently large is somewhat subjective and depends on many factors.

L394-396: And this would not be possible with other methods?

It would of course be possible, but as highlighted, our proposed method is quite easy to use and to interpretate, which makes it a good candidate for comparing study sites.

References:

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Dear anonymous referee #2,

thank you for your review, we very much appreciate the indications for improving our manuscript.

Response to detailed comments:

Line 69: If applicable, please change "rural" to "agricultural area".

We changed the wording to "agricultural area".

Line 81: Please revise the heading, possibly change to "Sampling and measurements" to illustrate the content.

Thank you for highlighting that, we revised the heading to "Sampling setup".

Lines 124-125: Please check that all parameters are listed correctly and use the same terms as in line 126.

Thanks, we revised accordingly.

Line 103: Please revise the heading, as this section also covers the clustering of events, not just hydrograph separation. Alternatively, move the second part to a separate section.

Thanks, the heading was changed to, "hydrological event analysis".

Line 196/ Table1: The pre-event wetness in the catchment (API) appears to be a very important parameter for the subsequent analysis. Please add this to the table.

Thank you very much for this input, the API was added to Table 1

Line 215-217: Please revise the sentences to avoid repetition.

The sentence was revised.

Line 225/ Fig. 5: Add a * if the clusters are significantly different.

We reworked our cluster analysis and identified 3 clusters. This improves our analysis overall but there are not enough datapoints in each sample size for not risking heavy β-error.

Line 265: It is mentioned here that the red events might have taken place in seasons when no crops were grown or the fields were compacted. However, based on Fig. 4b and Table 1, the red events (3,5 and 11) took place in May, June and November so I cannot see any clear pattern. Please elaborate on this reasoning in the text, and be careful not to draw conclusions based on a very limited sample size.

Also, the events in Table 1 were not equally distributed throughout the year, which may introduce a bias towards spring and summer samples. Please discuss this briefly.

Thank you very much for your observation, there is indeed a bias towards spring and summer: The measurement took place from 02/2021 till 06/2022 due to institutional restriction regarding our field setup. Unfortunately, the autumn of 2021 was very dry and did not bear any major precipitation event we could sample from. In the winter of 2021 snowfall scrambled the isotope measurement and the small stream was completely frozen, making it impossible to measure discharge. The following winter of 2022 left us with three usable events, so we get a sample bias towards spring and summer.
We revised our reasoning regarding the influence of crops and season on the event dynamics, especially considering limitations in sample size and resulting bias of the database:

“The observed pattern is likely biased by the overweight of summer and spring events, as discussed above, and may not be inferred to general behavior of the catchment outside the monitored timespan.”

Lines 274-276: Please revise sentence.

Sentence was revised, removed redundancy.

Line 349: Could the influence of the drainage system on nutrient export be quantified? Is there any data available indicating the percentage of the catchment area with a drainage system? Similarly, this question could be extended to the crop type in the catchment over the years. In general, more information on the catchment’s specific agricultural land use would be helpful in order to understand the possible nutrient export.

Thank you for mentioning these points. Unfortunately, there are no more details about the drainage system available. While we identified 2 clearly visibly drainages, there is no information of how large the drained area is, how often those drainages are activated or which amount of discharge is contributed by the drainages at an event. More detailed knowledge about the agricultural land use in the catchment can only be derived from occasionally field observations, there is no official database containing a timeseries of crop types for every field in the catchment. However, we observed that most of the slopes in the catchment were planted with grain (mostly winter wheat and rye) and the shallower fields near the stream were planted with mostly cauliflower and kale. We now present the land use in greater detail by breaking down certain parts of the agricultural area, where we observed meadows and vegetable farming during the sampling period.

Lines 375-389: Possibly add recommendations for transferring this method to other catchments and sampling campaigns.

We added some recommendations for inferring the method to other catchments, for example the increase of precision coming with a more detailed knowledge about land use and crops types, or practical experience like the cleaning interval of the devices:

“Additionally, we recommend increasing the cleaning interval of the Spectrolyzer, both automated cleaning with pressure and manual cleaning to ensure less instrument failure. Additionally, we recommend increasing the cleaning interval of the Spectrolyzer, both automated cleaning with pressure and manual cleaning to ensure less instrument failure. Finally, the method would profit from a higher temporal and spatial resolution and a near continuous timeseries of isotopes in discharge and precipitation, like for example achieved by Freyberg et al. 2018.”.

Line 399: Do you mean general knowledge or is there a specific point, that you would like to highlight to decisionmakers?

We added some points for more clarity:

“Focusing on the event water fraction for mitigation measures for solute export in affected catchments could result in a more targeted and cost-effective strategy than concentrating on the total discharge. This would not only apply to nutrients, but to all types of solutes mobilized by the event water, like pesticides, microplastics, or pharmaceuticals.”

Line 407: “given the method’s sensitivity to limited sample size”: was this tested here?

This was not tested in this approach, but since the core of the NCL function is a statistical fitting technique, it will profit from a bigger sample size. We revised the sentence.