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Revisiting causality using stochastics: 2. Applications

Demetris Koutsoyiannis, Christian Onof, Antonis Christofidis and Zbigniew W. Kundzewicz

Article citation details
Proc. R. Soc. A **478**: 20210836.
http://dx.doi.org/10.1098/rspa.2021.0836

Review timeline

Original submission:	30 October 2021
1st revised submission:	16 February 2022
2nd revised submission:	20 April 2022
Final acceptance:	22 April 2022

Note: Reports are unedited and appear as submitted by the referee. The review history appears in chronological order.

Review History

RSPA-2021-0836.R0 (Original submission)

Review form: Referee 1

Is the manuscript an original and important contribution to its field?
Good

Is the paper of sufficient general interest?
Good

Is the overall quality of the paper suitable?
Acceptable

Can the paper be shortened without overall detriment to the main message?
Yes

Do you think some of the material would be more appropriate as an electronic appendix?
No

Do you have any ethical concerns with this paper?
No

Recommendation?
Major revision is needed (please make suggestions in comments)

Comments to the Author(s)
Comments on Koutsoyiannis et al.: Revisiting causality using stochastics: 2. Applications
RSPA-2021-0836

This paper presents some example applications of the methodology presented in Part 1 of the paper. It would seem as if both Part 1 and the first part of Part 2 are all really leading up to Section 2.3 and the suggestion that the authors' analysis appears to suggest that temperature rise precedes rise in CO2, as already suggested by Koutsoyiannis and Kundzewicz (2020). Clearly this is not so logically obvious and might well what led them into this study of causality identification in the first place.

The authors note that the direction of causality for the modern, satellite derived, data seems to be in the direction □T □ □CO2 with a lag or about 6 months but that the explained variance is only of the order of 30% such that this "suggests that the two processes have a behaviour that is much more complex and affected by additional geophysical processes." (P18.L51). The differenced ice core proxy data are more ambiguous, suggesting a Hen or Egg conclusion, the aggregated data suggesting more strongly that □T □ □CO2 with a time lag of about 1000 years. The authors do suggest some possible mechanisms for why the causality might be in this direction in the discussion section.

These results suggest it really would be useful to have some estimates of uncertainty in the identification to assess how much confidence we should have in the inference. And since the IRF is essentially a model of the data it would be of interest to show how well the data are represented by the model (often instructive, for example, in the precipitation-runoff case).

They also suggest in the discussion that: "Further, the requirement that the IRF coefficients be nonnegative is, on its own, sufficient to enable the correct direction of causality to be inferred. When the roughness condition is added, this enables the correct system dynamics to be recovered." This surely will not always be the case where the "correct system dynamics" is nonlinear or oscillatory in ways not subject to simple transformations?

Other comments

Figs 3, 4 – see note on sensitivity of IRF and roughness condition in Part 1.

P4.L7. See comments on analytical solution in Part 1. Surely a problem even with a "small" number of ordinates (41). Would be of interest to see what the unconstrained "rough" solution looks like relative to the smoothed solution. There are other ways of smoothing (e.g. see Young, 2011 again).

P14.L31. We have a hydrological cycle but that involves many processes other than precipitation and runoff. So not really logical for series of observations to suggest that this is cyclical or potentially HoE. This is an example of identifying the obvious (as is reflected in the use of causal IRF in hydrology for 90 years or so in the form of the unit hydrograph).

This is also an example of nonlinearity in the responses and changing time delays with magnitude of the storm. This is not reflected in the analysis here except in so far as a simple transformation of the input and output variables is made.

Figure 11 shows that the relationship between the transformed precipitation and runoff variables is more scatter than correlation – yet the roughness constrained IRF is presented as if a well determined relationship without any uncertainty. It would surely be the case that the IRF Should be considered as rather uncertain in such a case and the uncertainty be evaluated to avoid false inference (the inference might be absolutely obvious here in the rainfall-runoff case, but presumably the authors are suggesting their methodology would serve in less obvious cases in order to actually learn something). Should the uncertainty estimation (and how it might interact with the roughness constraint) not be a expected part of the process (especially given Figs 3 and 4 and the artifacts of Figure 13).

P23L49 However, the avenues are not necessarily point to a type of "resolving" that makes the result compatible with what intuition dictates. In the history of science, the avenues were often created when established ideas were overturned by new findings. English not clear here. Perhaps "However, these avenues of research might not resolve the issue in a way compatible with what intuition dictates".

Review form: Referee 2

Is the manuscript an original and important contribution to its field?
Acceptable

Is the paper of sufficient general interest?
Good

Is the overall quality of the paper suitable?
Acceptable

Can the paper be shortened without overall detriment to the main message?
No

Do you think some of the material would be more appropriate as an electronic appendix?
No

Do you have any ethical concerns with this paper?
No

Recommendation?
Major revision is needed (please make suggestions in comments)

Comments to the Author(s)
See Appendix A.

Decision letter (RSPA-2021-0836.R0)

28-Jan-2022

Dear Professor Koutsoyiannis

The Editor of Proceedings A has now received comments from referees on the above paper and would like you to revise it in accordance with their suggestions which can be found below (not including confidential reports to the Editor).

Please submit a copy of your revised paper within four weeks - if we do not hear from you within this time then it will be assumed that the paper has been withdrawn. In exceptional circumstances, extensions may be possible if agreed with the Editorial Office in advance.

Please note that it is the editorial policy of Proceedings A to offer authors one round of revision in which to address changes requested by referees. If the revisions are not considered satisfactory by the Editor, then the paper will be rejected, and not considered further for publication by the journal. In the event that the author chooses not to address a referee's comments, and no scientific justification is included in their cover letter for this omission, it is at the discretion of the Editor whether to continue considering the manuscript.

To revise your manuscript, log into http://mc.manuscriptcentral.com/prsa and enter your Author Centre, where you will find your manuscript title listed under "Manuscripts with Decisions." Under "Actions," click on "Create a Revision." Your manuscript number has been appended to denote a revision.

You will be unable to make your revisions on the originally submitted version of the manuscript. Instead, revise your manuscript and upload a new version through your Author Centre.

When submitting your revised manuscript, you will be able to respond to the comments made by the referee(s) and upload a file "Response to Referees" in Step 1: "View and Respond to Decision Letter". Please provide a point-by-point response to the comments raised by the reviewers and the editor(s). A thorough response to these points will help us to assess your revision quickly. You can also upload a 'tracked changes' version either as part of the 'Response to reviews' or as a 'Main document'.

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Once again, thank you for submitting your manuscript to Proc. R. Soc. A and I look forward to receiving your revision. If you have any questions at all, please do not hesitate to get in touch.

Yours sincerely
Raminder Shergill

proceedingsa@royalsociety.org

on behalf of
Professor Graham Hughes
Board Member
Proceedings A

Reviewer(s)' Comments to Author:
Referee: 1
Comments to the Author(s)
Comments on Koutsoyiannis et al.: Revisiting causality using stochastics: 2. Applications
RSPA-2021-0836

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Referee: 2
Comments to the Author(s)
See Appendix A.

Board Member:
Comments to Author(s):
See comments for companion manuscript.

Author's Response to Decision Letter for (RSPA-2021-0836.R0)

See Appendix B.

RSPA-2021-0836.R1 (Revision)

Review form: Referee 1

Is the manuscript an original and important contribution to its field?
Excellent

Is the paper of sufficient general interest?
Excellent

Is the overall quality of the paper suitable?
Excellent

Can the paper be shortened without overall detriment to the main message?
Yes

Do you think some of the material would be more appropriate as an electronic appendix?
No

Do you have any ethical concerns with this paper?
No

Recommendation?
Accept with minor revision (please list in comments)

Comments to the Author(s)
P2L49 and throughout. 2022a, b again – do not need to separately reference supplementary information

P9L15 replace owed by due

P14L54 replace cannot stand by cannot hold

P16L28. Only 2022

P15L18. I should perhaps have raised the issue in my earlier review as to whether the paper by Richtet be mentioned if it was retracted (footnote 2)? It is not needed for the argument here. If the authors think that the retraction was unjustified – even if the inference of lag is different from their own – then they should explain why. If they think that retraction was justified – then surely it should not be included here.

P15L31. in 1979 and continues to date

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Review form: Referee 2

Is the manuscript an original and important contribution to its field?
Acceptable

Is the paper of sufficient general interest?
Good

Is the overall quality of the paper suitable?
Acceptable

Can the paper be shortened without overall detriment to the main message?
Yes

Do you have any ethical concerns with this paper?
No

Recommendation?
Accept as is

Comments to the Author(s)
I guess the authors have misunderstood my words in the previous review. Technically the applications are sound. My concern is for the authors' good.

To make the proposed method influential, it would be better to choose well-established problems for validation. But in this manuscript three big problems (particularly the 2nd one on CO2 vs. global mean temperature) are chosen, with conclusions drawn in a rush. This may do more harm than good. As well known, each application itself would make an individual paper.

If the authors are determined, I have no objection here. The publication of this part is at their own discretion.

Decision letter (RSPA-2021-0836.R1)

12-Apr-2022

Dear Professor Koutsoyiannis,

On behalf of the Editor, I am pleased to inform you that your Manuscript RSPA-2021-0836.R1 entitled "Revisiting causality using stochastics: 2. Applications" has been accepted for publication subject to minor revisions in Proceedings A. Please find the referees' comments below.

The reviewer(s) have recommended publication, but also suggest some minor revisions to your manuscript. Therefore, I invite you to respond to the reviewer(s)' comments and revise your manuscript. Please note that we have a strict upper limit of 28 pages for each paper. Please endeavour to incorporate any revisions while keeping the paper within journal limits. Please note that page charges are made on all papers longer than 20 pages. If you cannot pay these charges you must reduce your paper to 20 pages before submitting your revision. Your paper has been ESTIMATED to be 23 pages. We cannot proceed with typesetting your paper without your agreement to meet page charges in full should the paper exceed 20 pages when typeset. If you have any questions, please do get in touch.

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When submitting your revised manuscript, you will be able to respond to the comments made by the referee(s) and upload a file "Response to Referees" in Step 1: "View and Respond to Decision Letter". Please provide a point-by-point response to the comments raised by the reviewers and the editor(s). A thorough response to these points will help us to assess your revision quickly. You can also upload a 'tracked changes' version either as part of the 'Response to reviews' or as a 'Main document'.

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2) A separate electronic file of each figure (tif, eps or print-quality pdf preferred). The format should be produced directly from original creation package, or original software format.

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*Write simple English: this is intended for the general public. Please explain any essential technical terms in a short and simple manner.
*Describe (a) the study (b) its key findings and (c) its implications.
*State why this work is newsworthy, be concise and do not overstate (true 'breakthroughs' are a rarity).
*Ensure that you include valid contact details for the lead author (institutional address, email address, telephone number).

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Once again, thank you for submitting your manuscript to Proceedings A and I look forward to receiving your revision. If you have any questions at all, please do not hesitate to get in touch.

Best wishes
Raminder Shergill
proceedingsa@royalsociety.org
Proceedings A

on behalf of
Professor Graham Hughes
Board Member
Proceedings A

Reviewer(s)' Comments to Author:
Referee: 1
Comments to the Author(s)
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Referee: 2
Comments to the Author(s)
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If the authors are determined, I have no objection here. The publication of this part is at their own discretion.

Board Member
Comments to Author(s):
Both reviewers are happy for your paper to proceed to publication after addressing a few minor comments. However, it is apparent that Reviewer 2's original comments were intended to aid clarity and I encourage you to consider if any rationalisation would be helpful in that regard. Ultimately, Reviewer 2 is happy to leave that to your judgement.

Author's Response to Decision Letter for (RSPA-2021-0836.R1)

See Appendix C.

Decision letter (RSPA-2021-0836.R2)

22-Apr-2022

Dear Professor Koutsoyiannis

I am pleased to inform you that your manuscript entitled "Revisiting causality using stochastics: 2. Applications" has been accepted in its final form for publication in Proceedings A.

Our Production Office will be in contact with you in due course. You can expect to receive a proof of your article soon. Please contact the office to let us know if you are likely to be away from e-mail in the near future. If you do not notify us and comments are not received within 5 days of sending the proof, we may publish the paper as it stands.

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On behalf of the Editor of Proceedings A, we look forward to your continued contributions to the Journal.

Sincerely,
Raminder Shergill
proceedingsa@royalsociety.org

on behalf of
Professor Graham Hughes
Board Member
Proceedings A

Appendix A

Review of "Revisiting causality using stochastics" by D.Koutsoyiannis, C. Onof, A. Christofides, and Z.W. Kundzewicz

As I said in the review of the first paper, authors propose to inference the causal relation between two variables, say, x & y, by connecting them in a convolution equation.

$$y(t)=\int_{-\infty}^{\infty} g(h)x(t-h)dh+v(t)$$

The method boils down to finding the impulse response function g(h). Given finite observations, theoretically this has infinitely many solutions (somehow similar to the overfitting problem in machine learning). To find g(h), the authors impose some restrictions on "roughness" and minimize the variance. (The roughness restriction makes the scheme different from the traditional linear regression.) Then, by some proposed criteria, they claim that the causality can be determined. This is then applied to three real world problems: precipitation-runoff, atmospheric temperature vs. CO2 concentration, atmospheric temperature vs. ENSO.

I would not make further comment here on the method. Authors can refer to the review of paper 1 for details. A suggestion: The dimensionality may be greatly reduced is g(h) is expanded with respect to some basis. Authors may try this technique.

Given that I commented in the first review, here this part can be greatly shortened and should be adapted into the first paper. Particularly, the three applications, if only for the purpose of validation, should be shortened or removed; otherwise it is far from enough for a conclusion to be drawn in their respective fields.

2.2 Precipitation vs. runoff

This is still a puzzle in some local system, where precipitation does not need to precede runoff. Unless this is a well-known case, it does not serve to validate the method.

2.3. Atoms. Temp. vs. CO2 concentration

To get a one-way causality does not need to show that the formulation here is robust. In fact, the opposite, i.e., the causality from CO2 to T has indeed been identified from paleoclimate series, particularly the ones drawn from the Antarctic ice cores. Besides, the impact is spatially inhomogeneous (see below).

2.4. Atmos. Temp. vs. ENSO

This is, again, a very disputative issue. A simple calculation that results in a one-way direction causality from SOI to the globally averaged atmosphere temperature series does not help for the validation purpose. Particularly, the impact of ENSO on the atmospheric temperature is shown to be inhomogeneous in space; such a globally averaged series cannot be representative. A recent publication addresses this issue and shows that this kind of averaged series could result in totally wrong result (Liang 2022: The causal interaction between complex subsystems. Entropy, 24, 3).

Minor points

Figures 3, 4 are not good, while in Figures 5, 6, 8 there always exists nonzero estimated IRF at some negative time lag. Why?

Appendix B

Response to review comments on "Revisiting causality using stochastics: 2. Applications"

by Demetris Koutsoyiannis, Christian Onof, Antonis Christofides and Zbigniew W. Kundzewicz

Summary: Version 1 of our manuscript "Revisiting causality using stochastics: 2. Applications" received two anonymous reviews and an additional assessment by an anonymous Board Member. Based on them, the paper thankfully received a favourable decision with an invitation to revise the manuscript in accordance with the suggestions of the reviewers. As seen below, where all review material is reproduced, the level of criticism we received is appropriate and made with a scientific spirit. Also, the reviewers' suggestions are constructive and thus helped us to improve our paper and clarify our methodology and its limitations, as we explain in detail below. In addition, the reviewers' critiques helped us to enhance our confidence about our method and results, and, in particular, to strengthen our confidence that our work is scientific, posited at the front of knowledge inquiry, where the ground is still under exploration—not completely known. The two reviewers' suggestions are generally in agreement with each other. However, there is one exception clearly distinguished below: While Reviewer's 1 comments imply expansion of our work to address them, Reviewer 2 suggests cutting a part of the companion paper and merging the two papers into one. Given that there was not an editorial directive for such a substantial cutting (see Board Member's Comments to Authors below), we preferred to follow the advice of Reviewer 1 and not to follow the suggestion to merge as as, in our view this would distort, truncate and devalue the entire study. At the same time, as each of the two papers already exceeds the length limitation of the 20 pages (21 pages each according to the Editor's letters), we have put a lot of the expansion material into a new Supplementary Information report, so as to keep the main papers as short as possible.

Key:

Review comment.

Response.

Quotation from manuscript.

Quotation from other papers or from the reviews.

Note: The list of references contained in the bottom of this report is for the report per se and does not coincide with the references contained in the paper.

Board Member

Comments to Author(s):
See comments for companion manuscript.

We are grateful to the Board Member for the positive assessment. We appreciate the reviewers' comments and we have addressed them in the revised manuscript as we explain in detail in the report that follows.

Reviewer 1

This paper presents some example applications of the methodology presented in Part 1 of the paper. It would seem as if both Part 1 and the first part of Part 2 are all really leading up to Section 2.3 and the suggestion that the authors' analysis appears to suggest that temperature rise precedes rise in CO2, as already suggested by Koutsoyiannis and Kundzewicz (2020). Clearly this is not so logically obvious and might well what led them into this study of causality identification in the first place.

We appreciate the Reviewer's judgement on our motives of the study. Indeed, the preceding study by Koutsoyiannis and Kundzewicz (2020) was one of the main motives in conducting this study. And indeed, its results were not logically obvious and thus led these two authors to further investigate them. On the other hand, there were additional discussions on philosophical aspects on causality and its mathematical representation among the four of us (all coauthors) with the main motive being scientific curiosity for an important philosophical and scientific issue. (We note here the Reviewer's 2 comment in the review of the first part that "causal inference becomes an arena of enormous interest").

The authors note that the direction of causality for the modern, satellite derived, data seems to be in the direction ΔT → ΔCO2 with a lag or about 6 months but that the explained variance is only of the order of 30% such that this "suggests that the two processes have a behaviour that is much more complex and affected by additional geophysical processes." (P18.L51). The differenced ice core proxy data are more ambiguous, suggesting a Hen or Egg conclusion, the aggregated data suggesting more strongly that ΔT→ ΔCO2 with a time lag of about 1000 years. The authors do suggest some possible mechanisms for why the causality might be in this direction in the discussion section.

The phrase "only of the order of 30%" may imply that this percentage is not enough to capture the causality direction and its key characteristics. However, we believe it is more than enough. To illustrate this, in the new Supplementary Information report, section SI2.1, *Assessment of uncertainty in the identification of the impulse response function and its characteristics*, we have intentionally increased (with respect to the original illustration in the main paper, part 2) the intensity of the noise so that the explained variance ratio decrease to about 1/3 (i.e. close to the actual one in the particular case of $\Delta T \rightarrow \Delta\ln[\mathrm{CO_2}]$). As the new illustration shows, the results are always satisfactory, even with a rough shape of the IRF.

The following text in the revised manuscript provides this information:

> Remarkably, however, the explained variance ratio of $e = 0.31$ is low and suggests that the two processes have a behaviour that is much more complex and affected by additional geophysical processes. However, insofar the relationship of these two processes is concerned, this explained variance ratio is adequate to detect the main characteristics, i.e. direction and time lags. Indications for this adequacy are provided by the precipitation – runoff case study (section 2.2), in which $e = 0.17$, as well in the additional controlled (synthetic) case study that is provided in Supplementary Information (section SI2.1, $e = 1/3$).

These results suggest it really would be useful to have some estimates of uncertainty in the identification to assess how much confidence we should have in the inference. And since the

IRF is essentially a model of the data it would be of interest to show how well the data are represented by the model (often instructive, for example, in the precipitation-runoff case).

We have now provided additional material on uncertainty assessment in the new Supplementary Information report for part 2, section SI2.1, *Assessment of uncertainty in the identification of the impulse response function and its characteristics*. We have chosen not to use the precipitation-runoff case as an example for the illustration, because we believe that the illustration would be more instructive if we choose a system with dynamics fully known a priori. Therefore, the system for this illustration is a modification of that in case studies #8 and #10, but with much increased intensity of the noise, so that the explained variance be about 1/3—in between the values of 17% and 68% which were found for the precipitation-runoff case for untransformed and transformed variables, respectively.

Overall, we are particularly thankful for this comment of Reviewer 1: it inspired us to make this additional analysis, which we believe added value to the paper.

They also suggest in the discussion that: "Further, the requirement that the IRF coefficients be nonnegative is, on its own, sufficient to enable the correct direction of causality to be inferred. When the roughness condition is added, this enables the correct system dynamics to be recovered." This surely will not always be the case where the "correct system dynamics" is nonlinear or oscillatory in ways not subject to simple transformations?

While we recognize that there exist oscillatory nonlinear systems, we would avoid subsuming them under the concept of causality, particularly when this is inferred from data in an inductive manner. Therefore, we deem this issue out of the scope of our paper. To clarify it we have added the following text:

> (Recall from the companion paper—Koutsoyiannis et al., 2022a—that we do not subsume oscillatory nonlinear systems, in which the sign of $g(h)$ could alternate, under the causality notion, which accords with Cox's (1992) conditions for causality and in particular the monotonic relation of the cause with the effect.)

In terms of nonlinearity, we agree that there could be an issue in certain cases but we are making a linear approximation as we clarify in Supplementary Information of part 1, section S1I.2, *Justification of the linear causal system*.

Other comments
Figs 3, 4 – see note on sensitivity of IRF and roughness condition in Part 1.

We believe that with the new investigation in the Supplementary Information report for part 2, section SI2.1, *Assessment of uncertainty in the identification of the impulse response function and its characteristics*, this is fully clarified now.

P4.L7. See comments on analytical solution in Part 1. Surely a problem even with a "small" number of ordinates (41). Would be of interest to see what the unconstrained "rough" solution looks like relative to the smoothed solution. There are other ways of smoothing (e.g. see Young, 2011 again).

To address this comment, we have added the following text in section 1:

> As also explained in the companion paper (Koutsoyiannis et al., 2022a), the proposed method for the determination of the ordinates g_j based on the minimization of the variance of the error process, is, by its construction, nonparametric. A well-known weakness of determining numerous ordinates is that it is an over-parameterized problem. Alternative techniques may overcome this problem using a parametric model (such as a Box-Jenkins model or an autoregressive moving average exogenous—ARMAX—model; Young, 2011, 2015). In our nonparametric approach the over-parameterization problem can also be tackled—and here lies the usefulness of imposing constraints (a)-(c) discussed above. For comparison, an additional parametric method, formulated in terms of parameterizing the IRF per se in continuous time is also discussed and compared to the proposed non-parametric method in the Supplementary Information, section SI2.3. This method is also applied in one of the case studies in the Supplementary Information, section SI2.4.

P14.L31. We have a hydrological cycle but that involves many processes other than precipitation and runoff. So not really logical for series of observations to suggest that this is cyclical or potentially HoE. This is an example of identifying the obvious (as is reflected in the use of causal IRF in hydrology for 90 years or so in the form of the unit hydrograph).

We are aware of this fact and we are among those who have used the unit hydrograph professionally. The cyclical element we refer to is for the global hydrological cycle. To make it clearer, in the revised manuscript we have put emphasis (italics) at the phrases "*global scale*" and "*local scale*":

> At the *global scale,* the hydrological cycle is obviously a family of processes that act in a cyclical manner (Koutsoyiannis, 2020b), precipitation – runoff – precipitation – …, and therefore can be thought of as a hen-or-egg case of causality. However, if we specify a particular location on Earth, the situation is different and it is well known that at a *local scale* runoff is caused by past precipitation upstream in the drainage basin in a mono-directional fashion.

This is also an example of nonlinearity in the responses and changing time delays with magnitude of the storm. This is not reflected in the analysis here except in so far as a simple transformation of the input and output variables is made.

We are also aware of nonlinearities and we believe the particular cases studies (with untransformed and transformed input and output) are adequate for our causality framework. Thus is not identical with accurate reconstruction of the system dynamics. As we have stated in the revised manuscript for part 1, section 3.3:

> Here we clarify that the problem of identifying causality is different from that of recovering the full system dynamics. The former and not latter, is the scope of our study.

Figure 11 shows that the relationship between the transformed precipitation and runoff variables is more scatter than correlation – yet the roughness constrained IRF is presented as if a well determined relationship without any uncertainty. It would surely be the case that the IRF Should be considered as rather uncertain in such a case and the uncertainty be evaluated to avoid false inference (the inference might be absolutely obvious here in the rainfall-runoff case, but presumably the authors are suggesting their methodology would serve in less obvious cases in order to actually learn something). Should the uncertainty estimation (and how it might interact with the roughness constraint) not be a expected part of the process (especially given Figs 3 and 4 and the artifacts of Figure 13).

Please refer to our reply to the third general comment above. In particular, the new Figures SI2.1 and SI2.2, reproduced below, expand the information on the Figures 3 and 4 referred to in the comment.

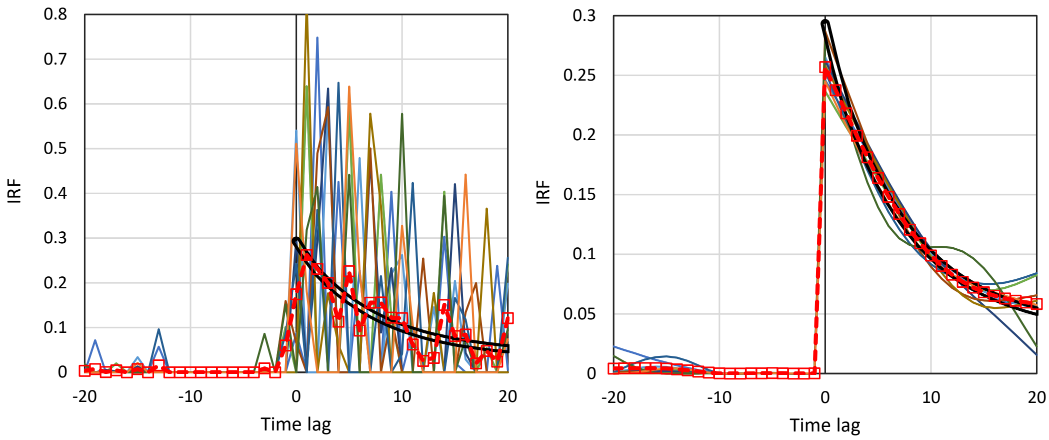

Figure SI2.1 Spaghetti plots of the IRFs for synthetic case studies similar to cases #8 and #10 (left and right, respectively) of the main paper (Koutsoyiannis et al., 2022b) but with variance of the error term $\underline{u}_\tau$ twice that of $\underline{x}_\tau$. Each panel shows ten different IRFs calculated from ten different Monte Carlo realizations of the process $\underline{y}_\tau$, each one with a different realization of the noise $\underline{u}_\tau$, and the same realization of $\underline{x}_\tau$ (see more details in the text). The double black line shows the true IRF and the dotted line with squares the average of 10 ten Monte Carlo realizations.

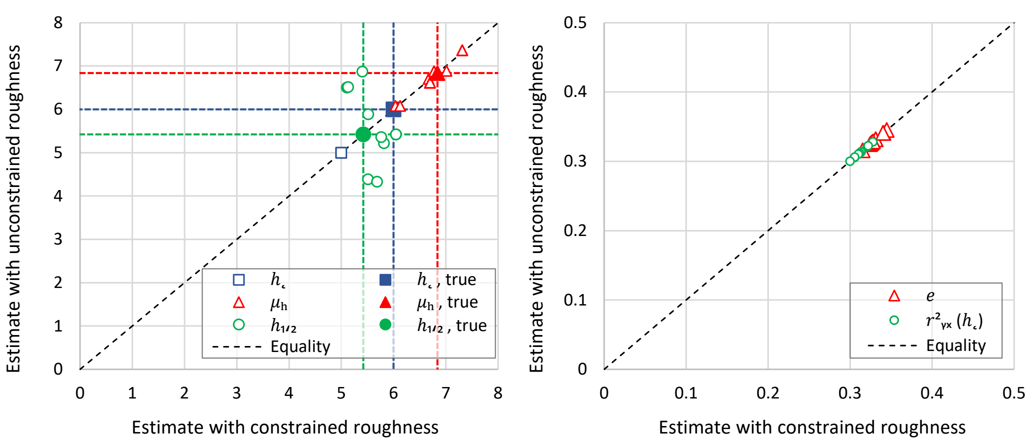

Figure SI2.2 Scatter plots of characteristic indices of the estimated IRFs shown in Figure SI2.1 for the ten generated time series of the processes $\underline{x}_\tau$, $\underline{y}_\tau$. The coordinates in each plot are the estimates of the indices using the nonnegativity constraint only (vertical axis) and using both the nonnegativity and the roughness constraint (horizontal axis). (Left) Time indices of the IRFs, where h_c is the time lag maximizing the cross-correlation $r_{yx}(h)$, μ_h is the mean (time average) of the function $g(h)$ and $h_{1/2}$ is the median of the function $g(h)$. (Right) Indices of strength of the causal relationship, where e is the explained variance ratio and $r_{yx}^2(h_c)$ is the explained variance in the simplified case that the causality relationship had been determined in terms of an IRF with only one nonnegative ordinate (at lag h_c).

As per the "artifacts of Figure 13" we explain in the paper that (a) these artifacts are due to the limited range of lags we considered and (b) that the scope of the paper is not to construct a model, which would need a wider range of lags, but to illustrate and test the methodology proposed. Obviously, with a wider range of lags the "artifacts" would disappear.

Finally, with respect to the effect of the transformation, please notice that, while it substantially improves the explanatory power, increasing the explained variance from 17% to 68% (Table 1), it does not change the characteristic lags much. This means that even without the transformation the causal relationship and its key characteristics are well captured.

P23L49 However, the avenues are not necessarily point to a type of "resolving" that makes the result compatible with what intuition dictates. In the history of science, the avenues were often created when established ideas were overturned by new findings. English not clear here. Perhaps "However, these avenues of research might not resolve the issue in a way compatible with what intuition dictates".

Suggestion thankfully adopted.

Reviewer 2

As I said in the review of the first paper, authors propose to inference the causal relation between two variables, say, x & y, by connecting them in a convolution equation.

$$y(t) = \int_{-\infty}^{\infty} g(h)x(t-h)dh + v(t)$$

The method boils down to finding the impulse response function g(h). Given finite observations, theoretically this has infinitely many solutions (somehow similar to the overfitting problem in machine learning). To find g(h), the authors impose some restrictions on "roughness" and minimize the variance. (The roughness restriction makes the scheme different from the traditional linear regression.) Then, by some proposed criteria, they claim that the causality can be determined. This is then applied to three real world problems: precipitation-runoff, atmospheric temperature vs. CO2 concentration, atmospheric temperature vs. ENSO.

We appreciate the Reviewer's summary of our work. On the other hand, we do not endorse the statement "they claim that the causality can be determined". In several parts in the two papers we stress the fact that what we study are necessary conditions for causality—and this is also recognized by the Reviewer in her or his statement of the first part: "only necessary conditions can be proposed to falsify a hypothesis". The necessary conditions do not enable us to say that "causality can be determined". Rather, what the necessary conditions can help us to do is is summarized in our following statement in part 1:

> Therefore, here we focus on simpler problems, such as falsifying an assumed genuine causality and adding statistical evidence, in an inductive context, for potential causality and its direction.

A similar statement appears in the Conclusions of part 1:

> The methodological framework proposed herein features substantial differences from existing methods, such as those discussed in section 2.2. A first difference is in its

> epistemological background which leads to a less ambitious objective, that of seeking necessary conditions of causality rather than sufficient ones. The usefulness of this objective lies in its ability to falsify an assumed genuine causality and to add statistical evidence, in an inductive context, for potential causality and its direction.

Overall, we believe that we have done much more than merely to "claim" anything. Furthermore, the Reviewer refers here only to the real-world case studies and overlooks the fact that the majority of our case studies (18 out of 30) are for synthetic examples, with dynamics known a priori. These enabled us to validate our proposed methodology in depth and in detail, so that our assertions are well supported by evidence.

> I would not make further comment here on the method. Authors can refer to the review of paper 1 for details. A suggestion: The dimensionality may be greatly reduced is g(h) is expanded with respect to some basis. Authors may try this technique.

We are grateful for the Reviewer's comment about the alternative of determining $g(h)$ as an expansion with respect to some basis functions. Actually, this was the first option we had initially investigated, but we excluded it from the paper as, after we had developed the nonparametric method based on the nonnegativity and roughness constraints, we deemed the latter methodology more general and hence superior.

Nonetheless, following the Reviewer's comment, in the revised paper we have also presented this option in the new Supplementary Information report, section SI2.3, *Parametric approach to identification of the impulse response function*. Furthermore, we provided a detailed case study using this method in section SI2.4, *Example of application of the parametric approach to modern temperature and CO_2 datasets*. From the applications we had originally studied, we chose the temperature – CO_2 case to study further with this option, because, as Reviewer 1 had pointed out, "Clearly this is not so logically obvious". As can be seen in the new Supplementary Information report, the results of the parametric setting of the method are in full agreement with those of the nonparametric method presented in the main paper (part 2).

This certainly enhanced our confidence to our results and we thank the Reviewer for making this comment.

> Given that I commented in the first review, here this part can be greatly shortened and should be adapted into the first paper. Particularly, the three applications, if only for the purpose of validation, should be shortened or removed; otherwise it is far from enough for a conclusion to be drawn in their respective fields.

Here we have to repeat (from our reply to the comments of part 1) our arguments about why we did not follow this Reviewer's suggestion:

- Reviewer 1 makes several points that require an expansion, rather than shortening of the work.
- Even Reviewer 2 includes comments that require expansion (see above and review of part 1), while she or he explicitly recognizes that "causal inference becomes an arena of enormous interest". On the other hand, the Reviewer suggests cutting the real-world case studies, which however may be related to this "enormous interest". The extent of these case studies is less than 10 pages. Therefore, even if we adopted this

suggestion, the final outcome, after merging the two papers, would substantially exceed the length restrictions set by the journal (https://royalsocietypublishing.org/rspa/for-authors#question2).

- In the decision letter and the Board Member's Comments to Authors there was no editorial directive for such radical cutting and merging.
- We strongly disagree to erase the real-world case studies, which were our inspiration in developing the methodology and preparing this work. We, all four authors, strongly believe that the real value of any methodology is assessed by confronting it with real-world data. While we certainly appreciate the philosophical and mathematical aspects related to causality—and this is reflected in the paper and also recognized by the reviewers—we are convinced that the removal of real-world examples would severely distort, truncate and devalue the entire study.
- The two parts could raise interest of two separate audiences. Part 1 could be of interest to theorists, statisticians, time-series experts, philosophers and system scientists, while part 2 to applied geophysicists, hydrologists, and climatologists.
- In our reply to the specific comments below, we provide additional information about our disagreement with the particular review comments for each of the case studies.

Therefore, to address the entire set of review comments, we were obliged to expand the study. On the other hand, to keep the "formal" part of the study as short as possible (the part that would hopefully be accepted and published in journal pages), we have put a lot of the expansion material into a second Supplementary Information report (new in part 2 of the study).

2.2 Precipitation vs. runoff
This is still a puzzle in some local system, where precipitation does not need to precede runoff. Unless this is a well-known case, it does not serve to validate the method.

The supposed puzzle that the Reviewer is alluding to is not clear to us. What we state in the paper is this:

> At the *global scale*, the hydrological cycle is obviously a family of processes that act in a cyclical manner (Koutsoyiannis, 2020b), precipitation – runoff – precipitation – …, and therefore can be thought of as a hen-or-egg case of causality. However, if we specify a particular location on Earth, the situation is different and it is well known that at a *local scale* runoff is caused by past precipitation upstream in the drainage basin in a mono-directional fashion.

We believe this statement is clear and correct. We distinguish what happens at the global scale (cyclical behaviour, involving further processes, most notably evaporation) and at a local scale. In a particular catchment, there is a clearly directional behaviour, from precipitation as the cause to runoff as the effect. Being experienced in hydrology, all of us (coauthors), we have difficulty understanding what the Reviewer means by "precipitation does not need to precede runoff". In fact, the causal link precipitation → runoff is a very "well-known case" and thus, it does "serve to validate the method". This is also recognized by Reviewer 1 who states "This is an example of identifying the obvious (as is reflected in the use of causal IRF in hydrology for 90 years or so in the form of the unit hydrograph)." And indeed, we chose this example as a validation case of the method, because the causality direction is obvious and indeed broadly recognized.

In conclusion, we think that the precipitation – runoff case constitutes an ideal example to illustrate and validate the method and that there is no need at all to exclude this study from out work.

2.3. Atoms. Temp. vs. CO2 concentration
To get a one-way causality does not need to show that the formulation here is robust. In fact, the opposite, i.e., the causality from CO2 to T has indeed been identified from paleoclimate series, particularly the ones drawn from the Antarctic ice cores. Besides, the impact is spatially inhomogeneous (see below).

We do not know how the Reviewer justifies her or his opinion that "the causality from CO2 to T has indeed been identified from paleoclimate series, particularly the ones drawn from the Antarctic ice cores", as she or he does not provide any reference. If she or he did provide some, we would include that in the revised manuscript. From the references we have in mind, we believe that the opposite is the case, as seen in the following quotation from Koutsoyiannis and Kundzewicz (2020; with references listed below)—a study cited in the paper, even though the paragraph below is not included as we believe the reference to it suffices:

> Despite falsification of some of Arrhenius's hypotheses, his line of thought remained dominant. Yet, there have been some important studies, based on palaeoclimatological reconstructions (mostly the Vostok ice cores [Jouzel et al., 1987; Petit et al., 1999]), which have pointed to the opposite direction of causality, i.e., the change in temperature as the cause and that of CO_2 concentration as the effect. Such claims have been based on the fact that temperature change leads and CO_2 concentration change follows. In agreement with Roe [2006], several papers have found the time lag to be positive, with estimates varying from 50 to 1000 years or more, depending on the time period and the particular study [Caillon at al. 2003; Soon, 2007; Pedro et al., 2012; Liu et al., 2018; Koutsoyiannis, 2019; Beeman et al., 2019]. Claims that CO_2 concentration leads (i.e., a negative lag) have not generally been made in these studies. At most, a synchrony claim has been sought on the basis that the estimated positive lags are often within the 95% uncertainty range [Beeman et al., 2019], while in one publication [Pedro et al., 2012], it has been asserted that a "short lead of CO2 over temperature cannot be excluded". With respect to the last deglacial warming, Liu et al. [2018], using breakpoint lead–lag analysis, again find positive lags and conclude that the CO_2 is an internal feedback in Earth's climate system rather than an initial trigger.

Furthermore, the Reviewer does not mention that our study mainly uses the modern data that are more reliable than proxy paleoclimatic data.

As Reviewer 1 correctly points out, this case study was a key motivation in the development of our methodology: "Clearly [the fact that temperature rise precedes rise in CO2] is not so logically obvious and might well what led [the authors] into this study of causality identification in the first place." The fact that this case study is a key element of our study is highlighted in the concluding paragraph in the Conclusions section of our paper in this way:

> By letting the geophysical records speak for themselves, with the help of our original methodology, we discovered a regularity that apparently contradicts common opinion. Our innovative findings should be given considerable attention as well as careful and critical scrutiny in the form of public discussion by the scientific community, which will undoubtedly improve understanding. If the methodology we proposed in the companion paper (Koutsoyiannis et al., 2022a) stands up to scrutiny, then our novel, high-impact results, i.e. those of cases #23 – #28 in the present paper, will have to be taken seriously and interpreted. Further research on the regularities of the causal behaviour of the climate system reported herein, being of considerable importance and relevance, is urgently needed.

For these reasons, we believe that our study would lose much of its value if we erased this section, and hence we prefer to keep it. Indeed, this section is relevant and likely to attract considerable attention and interest.

> 2.4. Atmos. Temp. vs. ENSO
> This is, again, a very disputative issue. A simple calculation that results in a one-way direction causality from SOI to the globally averaged atmosphere temperature series does not help for the validation purpose. Particularly, the impact of ENSO on the atmospheric temperature is shown to be inhomogeneous in space; such a globally averaged series cannot be representative. A recent publication addresses this issue and shows that this kind of averaged series could result in totally wrong result (Liang 2022: The causal interaction between complex subsystems. Entropy, 24, 3).

We are really glad that the Reviewer found "disputative" our section 2.4, *Atmospheric temperature and ENSO*. We believe that disputing published theories and results is a healthy practice, known from ancient times as the main driver of scientific progress. Therefore, if our study is published along with its potentially disputable (yet well supported, we believe) elements, we will be happy to receive formal Discussions by the Reviewer or any other interested colleague, in which they would dispute what they think is disputable.

As per the recent publication by Liang (2022), we thank the Reviewer for pointing it out, but we did not find in it anything related to ENSO. It is a theoretical study without any real-world application. We agree with the Reviewer that globally "averaged series could result in totally wrong result" yet we wish to point out that global averaging has been the standard practice in climate research. The series of globally averaged temperature that we use are not ours—they are highly used datasets in climate research provided by well-known organizations.

Nonetheless, in the revised version we have cited the suggested reference by adding the following text:

> Here we use only the global average (noting that Liang, 2022, disputes the use of averages as he claims that generally they do not work correctly) on the monthly scale for the lowest level, referred to as the lower troposphere.

> **Minor points**
> Figures 3, 4 are not good, while in Figures 5, 6, 8 there always exists nonzero estimated IRF at some negative time lag. Why?

As we understand it, the Reviewer's phrase "Figures 3, 4 are not good" must mean that she or he did not like their rough shape. We too did not like it and that is why we introduced the roughness constraint and produced the next figures. We are afraid that we cannot give a one-line explanation about the importance of the roughness constraint but in section 2.1 we explain every detail of the synthetic applications. In the same section we reply to the second part of this comment, about the cases of negative lags.

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Appendix C

Response to round 2 review comments on "Revisiting causality using stochastics: 2. Applications"

by Demetris Koutsoyiannis, Christian Onof, Antonis Christofides and Zbigniew W. Kundzewicz

Summary: Version 2 of our manuscript "Revisiting causality using stochastics: 2. Applications" received two anonymous reviews and an additional assessment by the Board Member Graham Hughes. Based on them, the paper thankfully received a favourable decision of acceptance after minor revisions. All reviewers' suggestions for minor changes have been implemented in the revised Version 3.

Key:

> Review comment.

Response.

> Quotation from manuscript.

Board Member

> Comments to Author(s):
> Both reviewers are happy for your paper to proceed to publication after addressing a few minor comments. However, it is apparent that Reviewer 2's original comments were intended to aid clarity and I encourage you to consider if any rationalisation would be helpful in that regard. Ultimately, Reviewer 2 is happy to leave that to your judgement.

We are grateful to the Board Member for the positive assessment. We appreciate the reviewers' comments and we have addressed them in Version 3 as we explain in detail below. We are also thankful for the clarification about the Reviewer 2's original comments. (Please see our reply to Reviewer 2 below.)

Reviewer 1

> P2L49 and throughout. 2022a, b again – do not need to separately reference supplementary information

Done.

> P9L15 replace owed by due

Done.

> P14L54 replace cannot stand by cannot hold

Done.

> P16L28. Only 2022

Done.

P15L18. I should perhaps have raised the issue in my earlier review as to whether the paper by Richtet be mentioned if it was retracted (footnote 2)? It is not needed for the argument here. If the authors think that the retraction was unjustified – even if the inference of lag is different from their own – then they should explain why. If they think that retraction was justified – then surely it should not be included here.

Whether the retraction was justified or not is not something we are prepared to discuss. After all, our results do not agree with those of that paper, but we generally find it useful to discuss contradicting results from different studies, some of which are necessarily incorrect.

What attracted our interest and impressed us in this case is that the journal announced that the paper was rejected, rather than retracted, even though it was published before by that journal in final form.

Anyhow, after this Reviewer's comment, we have removed the footnote and the reference.

P15L31. in 1979 and continues to date

Done.

P15L36. Perhaps better (given response to referees) would be: that generally they conceal regional patterns of change.

Done.

P15L54 …. Though this does seem to leave open the question as to whether the potential causality might be different for the other two satellite levels? Has this already been addressed in the earlier publications by the authors?

We have added the following statement:

> We additionally note that we also examined the temperature data for the other two satellite levels of the troposphere and the results were very similar to those reported here for the lower troposphere.

Because of the similarity of results, there is no meaning in including quite similar graphs in the paper. There are good reasons for this similarity. For example, the dataset of differenced temperature of the mid troposphere are synchronous and highly correlated with those of the lower troposphere—those reported in the paper. Namely, the correlation coefficient between ΔT of mid troposphere and lower temperature is maximum at lag zero and takes on a value as high as 0.96!

Overall, we are grateful for all these detailed suggestions and we thank the Reviewer for the impressively attentive reading.

Reviewer 2

I guess the authors have misunderstood my words in the previous review. Technically the applications are sound. My concern is for the authors' good.

> To make the proposed method influential, it would be better to choose well-established problems for validation. But in this manuscript three big problems (particularly the 2nd one on CO2 vs. global mean temperature) are chosen, with conclusions drawn in a rush. This may do more harm than good. As well known, each application itself would make an individual paper.
>
> If the authors are determined, I have no objection here. The publication of this part is at their own discretion.

We are grateful to the Reviewer for her of his explanation of her of his stance in the previous review and we are very glad that she or he found our applications sound. We agree with her or him that particularly the second problem is both difficult and important. Actually, the reason that we included this is to showcase the usefulness of the method in studying important and non-trivial problems. We also agree with the Reviewer that "each application itself would make an individual paper". In fact, we are looking forward to such individual papers as we do not claim that we have said the last word on the subject. Rather, we clearly state the opposite in the two concluding paragraphs:

> We believe that counterintuitive results, such as those about the causal link between temperature and CO_2 concentration conveyed in this paper, can indeed open up avenues of research. However, these avenues of research might not resolve the issue in a way compatible with what intuition dictates. In the history of science, such avenues were often created when established ideas were overturned by new findings.
>
> By letting the geophysical records speak for themselves, with the help of our original methodology, we discovered a regularity that apparently contradicts common opinion. Our innovative findings should be given considerable attention as well as careful and critical scrutiny in the form of public discussion by the scientific community, which will undoubtedly improve understanding. If the methodology we proposed in the companion paper (Koutsoyiannis et al., 2022) stands up to scrutiny, then our novel, high-impact results, i.e. those of cases #23 – #28 in the present paper, will have to be taken seriously and interpreted. Further research on the regularities of the causal behaviour of the climate system reported herein, being of considerable importance and relevance, is urgently needed.

As per "well-established problems for validation", we are confident that we have also dealt with this in the most appropriate manner, i.e., by including (a) an extended set of synthetic applications with a priori known dynamics and (b) a geophysical problem (rainfall-runoff), with well understood mechanisms and dynamics.