Peer Review Report

Review Report on Revisiting transfer functions: learning about a lagged exposure-outcome association in time-series data.

Hints and Kinks, Int J Public Health

Reviewer: Benedict Armstrong
Submitted on: 07 Apr 2022
Article DOI: 10.3389/ijph.2022.1604841

EVALUATION

Q 1 **Please describe the new method reported in this manuscript, and its purpose.**

See detailed review.

Q 2 **Please highlight the limitations and advantages.**

See detailed review.

Q 3 **Are there objective errors or fundamental flaws? If yes, please detail your concerns.**

See detailed review.

Q 4 **Check List**

Is the English language of sufficient quality?
Yes.

Is the quality of the figures and tables satisfactory?
No.

Does the reference list cover the relevant literature adequately and in an unbiased manner, including the primary manuscript(s) that describe the methodology?
No.

Are the quantitative or qualitative methods sufficiently explained and documented?
No.

Are the quantitative methods valid and correctly applied? (e.g. sample size, choice of test)
Yes.

Are the qualitative methods valid and correctly applied? (e.g. sample selection, method of data collection)
Yes.

Are the data underlying the study available in either the article, supplement, or deposited in a repository?
No.

Does the study adhere to ethical standards including ethics committee approval and consent procedure?
Not Applicable.

Q 5 **Please provide your detailed review report to the editor and authors (including any comments on the Q4 Check List):**

This reviewer is a biostatistician with experience in distributed lag models as usually used in Public Health (PH) research, but little prior knowledge of transfer functions.
This paper briefly introduces transfer functions (TFs) as an approach to formulating and fitting distributed lag models (DLMs) with the advantage over methods more widely used in PH that no maximum lag (lag length) needs be specified. TFs seem to have been developed and are used principally be econometricians; they are not well known to PH researchers. I presume the paper aims to inform such PH researchers.
I thought this objective a useful one to address, and considered the content, as far as I could judge, essentially correct. However, I believe the paper it could be considerably improved by making the its message more accessible to its target audience.
1. Ref 2 is an odd one to give for an example of PH use of DLMs - it addresses a specific issue in time series regression not particularly focused on the DLM component. I would suggest Schwartz Epidemiol 2000 as an early PH paper focused on DLMs and/or Gasparrini Stats in Med 2010 as one explaining their use for non-linear associations (DLNMs).
2. L15. "pre-specified window after which the lagged association diminishes". As currently written this suggests that lag length is set at the lag where the effect in a DLM is maximum (eg lag 0 for exponential decay), which is clearly nonsense. I expect that something like "pre-specified window after which the lagged association diminishes to a negligible level" was meant.
3. L17. It would be useful when introducing TFs to give a reference to a general description of them, as accessible as possible.
4. L20. I did not find it clear that the exposure referred to was time-varying. I suggest something like ".. a CHANGE IN obesogenic food environment" or "INTRODUCTION OF an exposure impacting the obesogenic ..."?
5. The link between the structural variable E[subt] and the outcome, say Y[subt], should be explained (Et =E(Yt)?)
6. The Koyck (exponential?) decay model (L32) looks like it could be formulated as DLM in the PH conventional way, though I do not think it is available as standard in any of the "PH-standard" packages I am aware of (eg R's dlnm). If so a note to this effect would be helpful. Also a note as to whether this is true of any transfer function (I suspect so, though the formulation would be less elegant than the TF formulation).
7. Perhaps the nature of the paper limits the number of figures, but I would think it very helpful if the plots in eFigures 1a and 1b, 2a and b (could all be in one figure) could be brought into the main text, even if that means relegating the current Figure to the SM. Though I can believe that the simple exponential decline is common, it would be a poor fit to many PH applications (eg temperature-health associations, where DL(N)Ms are widely used). The more general TF models illustrated in eFigures 1A-2b on the other hand seem to capture most DLMs found in PH.
8. Discussion: The authors are up-front and honest about the cost in using TFs rather than the more PH conventional DLMs of being able to avoid setting a maximum lag length. This being the need to pre-specify shape of the DL functions (the p and q parameters). I think this trade-off could be further informed by amplifying a bit the implicit assumptions of TFs: If I understand correctly, all TF models imply decay of effect towards zero at some lag length. If this is true I think it would be helpful to acknowledge it.
9. Discussion. There has been some discussion of choice of maximum lag length in the conventional PH DL literature. I know of that in Armstrong Epidemiol 2006, but strongly suspect there are others.
10. Discussion? I am aware of some other approaches that similarly (if I am right - see previous comment) assume an eventual decay in effect towards 0. Eg Welty and Peng Biostatistics 2009, Heaton and Peng Agric Biol Environ Stat. 2012. An acknowledgement and references would be helpful.
11. Discussion? It would be useful if it could be indicated whether if so how TF models can be extended to non-linear exposure-response dependences (ie to DLNMs).
12. I was pleased to see the relevant analysis code in the SM. And pleased that, although in a language (Stan) not known to many PH researchers, it was explained that it could be run in R (BTW, I found a package rstan, not rStan). However: (a) the code as written seems to be Stan without the necessary the R (rstan) interface code, and (b) no data was supplied. Without both of those things few if any PH researchers will be able to reproduce this analysis. If these could be supplied (preferably in an easy-to-un format), the chance of this approach getting used by PH researchers would increase substantially. The Gasparrini 2010 ref I mentioned above is an example.

QUALITY ASSESSMENT

Q 6	Rigor	■■■■□
Q 7	Method validated by effective results	■■■■□
Q 8	Applicability	■■□□□
Q 9	Significance to the field	■■■□□
Q 10	Interest to a general audience	■□□□□
Q 11	Quality of the writing	■■■■□

REVISION LEVEL

Q 12 What is the level of revision required based on your comments:

Moderate revisions.