Dear Dr Añel,

thank you very much for handling the review process. Due to the substantial comments of both referees, we decided to largely overhaul the manuscript and invested in further refining and improving our methodology. This took time and we apologize for the delay of submission of the revised version of the manuscript. We are now confident that our manuscript is in much better shape, the methodology is more robust and better explained, and we address all comments of the reviewers adequately. For reasons of conciseness, we decided to only present one counterfactual dataset (GSWP3-W5E5) in the manuscript. We now better explain how it builds on GSWP3 and which issues are present in both datasets that can hinder their use for attribution. We also added a new Figure 2 to better explain our impact attribution approach rooted in the definition of IPCC WGII. Due to the major overhaul of the methodology and its underlying code we decided to increase the version number to v1.1, so that both versions can be easily referred to and accessed. We linked the git commit to the code that produced all results in the code and data availability statement but did not yet trigger the v1.1 release as we would like to wait for the final versions of the manuscript (and its DOI to insert in README.md) and the code. We hope that you share our view on the improvements and we look forward to your decision.

All the best,
Matthias Mengel on behalf of the author team

Referee comments in italic

Responses in blue

"Quotes from the revised manuscript as plain text"

Anonymous Referee #1

Received and published: 26 August 2020

This paper describes the construction of so-called counterfactual datasets, using parametric statistical models to describe high frequency variability in the observed climate, removing trend from the parameters of those distributions via what is essentially a lowpass filtering approach, and then mapping observed quantiles onto the quantiles of the distributions with trend removed. Some basic results are show illustrating the removal of trend and the retention of variability. The method is applied to two gridded datasets.

We kindly thank the referee for the evaluation of the manuscript. Due to the substantial comments from both referees, we substantially overhauled both manuscript and methodology.

Things that I found to be missing from this paper include
1) Justification for the various choices that are made in setting up the probability model (e.g., why Gaussian for several variables, Weibull for wind speed, and why the particular choice of low-pass filtering approach based on singular spectrum analysis).

The manuscript now includes a justification for the choice of each distribution in section 3.2, see lines 330 to 364. We based most of our choices on existing work from the field of bias correction. For example, using Weibull to model the wind speed distribution is a common choice for parametric quantile mapping of that variable (e.g., (Lange 2019; Li et al. 2019)). We use singular spectrum analysis (SSA) to smooth (or low-pass filter) the predictor time series of global mean temperature because it is a widely applied and long-standing method in the field, see for example (Ghil and Vautard 1991), (Schlesinger and Ramankutty 1994) or (Rahmstorf 2007) for applications and (Ghil et al. 2002) for a review of the method. We have also added the original GMT time series to Figure 3 (former Figure 2) to show the effect of the smoothing. We only smooth the predictor and not the spatial climate data from which the counterfactual is derived. We now write, see lines 244 to 258:

"Here, T is GMT change since 1901 smoothed by singular spectrum analysis (Michael Ghil et al. 2002) with a smoothing window of 10 years (Fig. 3). The smoothing of the predictor is applied because we only want to remove long-term trends from the regional climate time series. Natural climate variability on shorter time scales due to phenomena such as the El Niño–Southern Oscillation is retained. Using T as the predictor means that we remove long-term trends in regional climate to the extent that those are correlated with GMT change,

but irrespective of the cause of global warming."

2) Motivation for the decision to fit the model using a Bayesian approach, which then requires that priors of various kinds be chosen -- and justified.

We now provide a motivation and justify our prior choices, see lines 297 to 306:

"We use a Bayesian approach to estimate all of these parameters. This requires the specification of prior distributions of the model parameters. Similar to regularization techniques in frequentist approaches, the prior allows to focus the model fitting on plausible parameter values. This is particularly important for numeric stability when the logit and logarithm link functions are applied. We use a zero-centered Gaussian prior for all parameters and all climate variables because we normalize the data before parameter estimation. We use a standard deviation of 1.0 for $a_0^{(intercept)}$, a standard deviation of $1/(2k-1)$ for $a_k^{(intercept)}$, $k = 1, ..., 4$, and a standard deviation of 0.1 for $a_k^{(slope)}$, $k = 0, ..., 4$. Our choice of priors for $a_k^{(intercept)}$ is based on the assumption that the first mode with a period of one year explains the largest part of the annual cycle and higher-order modes have decreasing influence. However this is only a prior assumption, i.e. if the data show different patterns, they can still be captured by our model. For $a_k^{(constant)}$ we use the same priors as for $a_k^{(intercept)}$. We use the same priors for the parameters b_k."

We reworked our model formulation, now fitting into the class of Generalized Linear Models, to unify it across variables and make it easier to grasp for the reader. This also influenced choices of priors.

3) Discussion about how one goes about ensuring that dependence between variables is respected (which needs to be well represented for many impacts models).

We use quantile mapping for detrending the time series. Quantile mapping preserves ranks, which means that relatively high values before the mapping are also relatively high after the mapping and similarly for relatively low values. This is now explained in the main text, see lines 507 to 514:

"Our detrending approach does not guarantee the maintenance of physical consistency of different climate variables in the counterfactual datasets in terms of, e.g., energy closure or water budgets. However, the applied quantile mapping preserves ranks, which means that relatively high values before the mapping are also relatively high after the mapping and similarly for relatively low values. Statistically speaking, univariate quantile mapping independently applied to all climate variables preserves the multivariate rank distribution (the copula) over all variables. In that sense the statistical dependence between variables is preserved by our detrending method and the risk of producing physically inconsistent counterfactual climate data is at least limited. This is critical for the attribution of the extreme event magnitude to observed

climate trends (see introduction) because several climate variables can contribute to impact extremes."

4) Discussion about maintaining physical consistency between variables (also required by many impacts models, e.g., to ensure closure of energy and water budgets) and how it is maintained in the heavily processed counterfactual datasets that are produced.

That is an important point. While our approach preserves statistical dependence between variables (see previous point) it does not imply maintenance of physical consistency. This is similar to the inconsistencies introduced by bias correction methods, which are widely applied in the field. Yet we think that the preservation of statistical dependence at least limits the risk of producing physically inconsistent counterfactual climate data (see argument given above).

5) Sufficient discussion of the homogeneity (or lack thereof) of the underlying factual dataset and its impacts. The authors attempt to minimize potential problems, for example, by pointing to a basis in a long reanalysis (line 70) and by trying to argue that early data have limited influence (lines 282-283), but I don't find these arguments very convincing.

The factual climate data are certainly subject to regional artefacts that also affect the counterfactual data. In the revised manuscript we now make very explicit that the counterfactual data should not be used blindly for the attribution of historical changes in natural, human or managed systems. Instead we recommend a number of control plots to check for potential artefacts in the observed trends in climate variables. For brevity, we now present counterfactuals only for the GSWP3-W5E5 dataset. We now write, see lines 370 to 375:

"In addition, we propose to plot the entire time series for regionally averaged annual (or seasonal) mean values for both the original and the counterfactual climate data. Here, we do so for annual regional averages over 21 world regions (Giorgi and Francisco 2000), see left panels of Figs. 7-10 and supplementary figures, but propose to adjust the regions and season for each attribution study individually according to its focus. For our specific observational dataset we add annual regional averages of the original GSWP3 data to check if the homogenisation of GSWP3 with W5E5 has introduced artificial trends in the factual GSWP3-W5E5 data."

In the paper we provide these plots for all world regions defined by (Giorgi and Francisco 2000) and all climate variables covered. The results section now includes an exemplary discussion for a subset of regions and variables. In addition, we highlight already in the introduction that each impact attribution study based on the proposed factual and counterfactual climate forcing first needs an evaluation of the factual impact simulation that additionally reduces the risk of erroneous attribution of impacts, see lines 105 to 112 and the newly added Fig. 2:

"In a first step the climate impact model forced by observed climate and socio-economic drivers has to demonstrate to be able to reproduce the observed changes in natural, human and managed systems as measured by an impact indicator (comparison of black and blue solid lines

in Figure 2). The attribution of the observed changes in natural, human and managed systems is built on a high explanatory power of the factual simulations. Then, in a second step, that factual simulation can be compared to a counterfactual simulation, forced by counterfactual climate but otherwise the same input as in the factual simulation. Such a comparison allows for a quantification of the contribution of climate change to both the observed trend in the impact indicator (CT in Fig. 2) and the observed magnitude of an individual impact event (CE in Fig. 2)."

and lines 140 to 142:

"Low-quality factual climate forcing data is expected to result in a low-quality reproduction of observed variations in the impact indicators of interest. If that is the case, the simulation set-up outlined here does not allow for an attribution of the observed changes in impacts to climate change."

6) Cross-validation using references from the literature concerning differences between factual and constructed counterfactual climates that are found. For example, have others written about changes in South American wind speeds, and have causes been explored and tested in models? Surely, this kind of validation is the minimum that should be expected to ensure that the datasets that are produced are fit-for-use.

The focus of our paper is on the detrending approach and less on the detailed evaluation of the factual climate forcing data. This is now more explicitly stated in the text:

"In this paper we introduce a detrending method tailored to support impact attribution and illustrate its application to one of the observational climate datasets provided within ISIMIP3a (https://protocol.isimip.org/protocol/ISIMIP3a, see data section below). The quality of the associated impact attribution studies will critically depend on the quality of that observational dataset. Deficits in the observational data may lead to artefacts in the derived historical trends. For the dataset used here, we identify some of those artifacts. Since it is expected that other artifacts will be found for other observational datasets, impact attribution studies should ideally be based on a range of different observational datasets to facilitate a quantification of the contribution of observational climate data uncertainty to the uncertainty of the attribution results. This is also planned within ISIMIP3a."

Other observational datasets that could also be used in the described attribution set-up are the Princeton data (PGMFD v2.1, Sheffield, Goteti, and Wood 2006) and the WATCH data (WFD, Weedon et al. 2011), and the combined data set WATCH-WFDEI (Weedon et al. 2011, 2014) that have already been used within ISIMIP2a. We make our methods freely available, so they can readily be applied to other datasets to explore data uncertainties in impact attribution results.

To make it easier to judge the fitness of the GSWP3-W5E5 data for attribution, we expanded

"Examples of temporal inhomogeneities in GSWP3 that are relevant for this study include artificial drying trends over northwest China and the Tibetan Plateau over the first half of the 20th century (Fig. 10) that are inherited from GPCC (Chen and Frauenfeld 2014), and spurious trends in shortwave radiation and wind speed over Alaska, Northern Canada and Greenland over the first half of the 20th century (Figs. 9 and S12), which are related to artificial extratropical cyclone trends in 20CR over that time period (Wang et al. 2013). Generally, the quality of 20CR, and hence GSWP3, becomes relatively stable around mid-century over the Northern Hemisphere, earlier over Europe and later over the Southern Hemisphere, in line with variations in the availability of pressure observations for data assimilation in the reanalysis (Compo et al., 2011)."

We additionally exemplify major deficits in the result section.

It is a critical part of attribution to evaluate the impact model simulation against the observed changes in natural, human or managed systems (see response to previous comment) and identify the best data set for specific regions or variables. We encourage evaluation tailored to the specific region and variable, see the new section 3.3, lines 366 to 378:

"To evaluate the detrending method and the counterfactual GSWP3-W5E5 data we use the difference between multi-year averages of each climate variable over the beginning of the time period (1901-1930) and multi-year averages over the end of the time period (1987-2016) as a measure of the trend. We compare this trend measure between the observed data and the counterfactual data, for which it should be close to zero (Figs. 5 and 6). In addition, we propose to plot the entire time series for regionally averaged annual (or seasonal) mean values for both the original and the counterfactual climate data. Here, we do so for annual regional averages over 21 world regions (Giorgi and Francisco 2000), see left panels of Figs. 7-10 and supplementary figures, but propose to adjust the regions and season for each attribution study individually according to its focus. For our specific observational dataset we add annual regional averages of the original GSWP3 data to check if the homogenisation of GSWP3 with W5E5 has introduced artificial trends in the factual GSWP3-W5E5 data. To evaluate the performance of the detrending method for each day of the year we propose to compare the 1987-2016 regional mean climatology of the counterfactual data to the 1901-1930 regional mean climatology of the factual data for each region of interest (right panels of Figs. 7-10 and supplementary figures)."

7) Discussion of what applications might, or might not be suitable.

In principle the application of the approach is only limited by our ability to reproduce, i.e. to explain the observed changes in the considered human natural and managed systems. Explanatory power could be constrained by the quality of the observational climate data,

"In addition to 'climate attribution', research on 'impact attribution' addresses the question: To what degree are observed changes in natural, human and managed systems induced by observed changes in climate (Fig. 1, second arrow). In the WGII contribution to the IPCC-AR5, an entire chapter was dedicated to the topic including the following definition: An impact of climate change is 'detected' if the observed state of the system differs from a counterfactual baseline that characterizes the system's behavior in the absence of changes in climate (IPCC 2014, chap. 18.2.1) and 'attribution' is the quantification of the contribution of climate change to the observed change in the natural, human or managed system. In both cases "climate change refers to any long-term trend in climate, irrespective of its cause" (IPCC 2014, chap. 18.2.1).

While in principle changes in natural, human and managed systems could also be attributed to anthropogenic climate forcing ('impact attribution to anthropogenic climate forcing', first and second arrow in Fig. 1, (Pall et al. 2011; Schaller et al. 2016; Mitchell et al. 2016)), we focus on 'impact attribution to climate change' as described in the WGII definition and introduce a climate dataset that can be used as input to climate impact models to characterize the system's behavior in the absence of climate change."

There are also some aspects that I found to be somewhat confusing. An example is the consideration of daily temperature skewness – which I found confusing given that daily mean temperature is modelled as a Gaussian random variable (and thus has zero skewness, by assumption). What is being discussed using some kind of shorthand that is known to the authors, but perhaps not others, is the nature of the diurnal temperature cycle – but how that diurnal cycle, and its variation in time, is represented by daily minimum temperature, daily maximum temperature and a measure of "skewness" of some kind is not made clear.

skewed in the sense that tasskew = (tas - tasmin) / (tasmax - tasmin) may deviate from 0.5. We have now clarified this part in the text, see lines 334 to 341:

"**Daily minimum and maximum near-surface air temperature.** They provide a measure of the diurnal temperature cycle in the daily resolved dataset. We do not estimate counterfactual time series for tasmin and tasmax directly to avoid large relative errors in the daily temperature range as pointed out by (Piani et al. 2010). Instead we construct counterfactuals for the auxiliary variables tasrange = tasmax - tasmin and tasskew = (tas - tasmin) / tasrange that then determine the tasmin and tasmax counterfactuals (Piani et al. 2010). We use the Gamma distribution to model tasrange since it has a lower bound at zero. The expected value is modeled according to Eq. (1). The skewness of daily near-surface temperature tasskew is modeled by a Gaussian distribution. While theoretically bounded, tasskew is never close to its bounds of zero and one. This justifies the Gaussian model choice."

Another example is the choice to represent relative humidity (which has a strong diurnal cycle that can be important for some impacts models, and is confined to values between 0 and 100%) as a Gaussian variable – and then to clip that distribution if quantile mapping happens to produce values outside the 0-100% interval.

Based on the comment we reworked the model and now use a beta distribution to describe the relative humidity. We now write, see lines 360 to 363:

"Near-surface relative humidity *hurs* is positive and less than or equal to one. We assume *hurs* to follow a beta distribution. Its expected value is allowed to vary with T and t. The associated coefficients are estimated using a beta regression model (Ferrari and Cribari-Neto 2004) and Eq. (1) for the expected value while the dispersion parameter, φ , is assumed to only vary with t."

The diurnal cycle of relative humidity is not covered in our input datasets. Only the daily mean relative humidity is included and we therefore only provide counterfactuals for the daily mean.

Finally, it seems to me that this paper is not a terribly good fit for GMD; it might be a better suited for a data journal in my view.

The focus of the paper is actually on the detrending method itself and on the introduction of the general framework for impact attribution within the third phase of the Intersectoral-Impact model Intercomparison Project ISIMIP3a. In the revised version of the manuscript we only discuss one of the datasets that were included in the original submission (GSWP3-W5E5) as an example application. The illustrative character has been made more explicit in the main text, see lines 128 to 135:

"In this paper we introduce a detrending method tailored to support impact attribution and illustrate its application to one of the observational climate datasets provided within ISIMIP3a (https://protocol.isimip.org/protocol/ISIMIP3a, see data section below). The quality of the associated impact attribution studies will critically depend on the quality of that observational dataset. Deficits in the observational data may lead to artefacts in the derived historical trends. For the dataset used here, we identify some of those artifacts. Since it is expected that other artifacts will be found for other observational datasets, impact attribution studies should ideally be based on a range of different observational datasets to facilitate a quantification of the contribution of observational climate data uncertainty to the uncertainty of the attribution results. This is also planned within ISIMIP3a."

We refer to the editor to make the decision of journal fit.

Anonymous Referee #2

Received and published: 27 August 2020

The authors present a new methodology to derive a counterfactual climate, relevant for impact studies. This is a very relevant and topical area to be looking at, and the PIK team are world leading in this area, so I was excited to see this paper from their team. However, I do think the paper is not exactly up to their usual standard, and in particular a thorough understanding of the attribution science is not clear in this paper.

We thank the reviewer for this thorough and helpful review. We modified the text to better embed our approach into the context of the existing literature of attribution science, in particular how our work relates to the definition of attribution as used in IPCC WGI and WGII.

My main concern is the framing and interpretation of the question being posed. I have to confess that it took me reading the entire paper to fully understand what they were doing, and how it is different to other methods. I think the authors need to work on posing the problem much earlier, and discussing how it fits into the wider attribution methods. In IPCC WG1 attribution is often split into trend attribution, and event attribution – the authors mention this at one point, but do not really explain how their methodology fits into this concept.

This is absolutely right, and we thank the reviewer for this important remark. We now outline our framing of attribution in the abstract and we have reworked the introduction including Figure 1 and added a new Figure 2. We clarify that the approach is designed to support 'impact attribution to climate change' in the WGII sense and point out that this perspective differs from the WGI lens. The main question being posed is: *how large is the influence of climatic trends on observed changes in natural, human and managed systems that also respond to other time-evolving drivers of change?* We aim to allow for the quantification of the contribution of climate trends in comparison to potential other non-climate related drivers. The influence of anthropogenic forcing to the climate trend is not investigated. We now write, see lines 61 to 76:

"In addition to 'climate attribution', research on 'impact attribution' addresses the question: To what degree are observed changes in natural, human and managed systems induced by observed changes in climate (Fig. 1, second arrow). In the WGII contribution to the IPCC-AR5, an entire chapter was dedicated to the topic including the following definition: An impact of climate change is 'detected' if the observed state of the system differs from a counterfactual baseline that characterizes the system's behavior in the absence of changes in climate (IPCC 2014, chap. 18.2.1) and 'attribution' is the quantification of the contribution of climate change to the observed change in the natural, human or managed system. In both cases "climate change refers to any long-term trend in climate, irrespective of its cause" (IPCC 2014, chap. 18.2.1).

While in principle changes in natural, human and managed systems could also be attributed to anthropogenic climate forcing ('impact attribution to anthropogenic climate forcing', first and second arrow in Fig. 1, (Pall et al. 2011; Schaller et al. 2016; Mitchell et al. 2016)), we focus on 'impact attribution to climate change' as described in the WGII definition and introduce a climate dataset that can be used as input to climate impact models to characterize the system's behavior in the absence of climate change. As the response of natural, human or managed systems to climate and socio-economic forcings is commonly considered deterministic (or at least simulated in this way) the attribution of impacts to the observed realisation of climate change does not necessarily have to be addressed in a probabilistic way and gives more weight to the separation of climate change from direct human influences as potential drivers of changes in the considered systems."

I was also very surprised not to see any of the available attribution impact work cited in this paper. I feel this is a very large omission from the paper, and the authors need a paragraph or two, maybe in their discussion section, introduction section, or both, that describes these papers, and explain how the author's current views and concepts fit into that. From the top of my head, these papers are all very relevant: Hydrology: Pall et al, 2011, Schaller et al, 2014; Health: Astrom et al, 2013, Mitchell et al, 2016; economy: Frame et al, 2020.

We revised the manuscript to better embed the proposed approach into existing work on impact attribution, see lines 62 to 73:

"In the WGII contribution to the IPCC-AR5, an entire chapter was dedicated to the topic including the following definition: An impact of climate change is 'detected' if the observed state of the system differs from a counterfactual baseline that characterizes the system's behavior in the absence of changes in climate (IPCC 2014, chap. 18.2.1) and 'attribution' is the quantification of the contribution of climate change to the observed change in the natural, human or managed system. In both cases "climate change refers to any long-term trend in climate, irrespective of its cause" (IPCC 2014, chap. 18.2.1).

While in principle changes in natural, human and managed systems could also be attributed to anthropogenic climate forcing ('impact attribution to anthropogenic climate forcing', first and second arrow in Fig. 1, (Pall et al. 2011; Schaller et al. 2016; Mitchell et al. 2016)), we focus on 'impact attribution to climate change' as described in the WGII definition and introduce a climate dataset that can be used as input to climate impact models to characterize the system's behavior in the absence of climate change. "

as well as lines 114 to 127:

"Process-based impact models such as those taking part in the ISIMIP project (www.isimip.org) are ideal tools to address impact attribution as they generally describe the response of natural,

human or managed systems not only to climate but also direct (human) drivers. For example, crop models can simulate the response of crop yields to changes in land use, irrigation patterns, fertilizer input and crop varieties (Lobell, Schlenker, and Costa-Roberts 2011; Challinor et al. 2014; Minoli et al. 2019). Similarly, hydrological models can be used to simulate how dam construction and water withdrawal affect river discharge (Veldkamp et al. 2017, 2018). In addition, those models allow for a process-based representation of the extent of e.g. river floods and droughts that can be combined with maps of asset distribution and empirical damage functions to estimate the direct economic damages induced by weather extremes. The impact attribution framework could then be used to approximate the contribution of climate change to observed trends in reported damages. Using process-based climate impact models, this contribution can be explicitly separated from changes in damages driven by changes in exposure or vulnerability. In this regard it goes beyond available approaches of damage attribution that attribute to anthropogenic climate forcing but simply estimate the contribution of anthropogenic climate forcing to observed damages by multiplying the fraction of attributable risk associated with weather extremes by the observed damage (Frame et al., 2020). In the same way, it could improve the attribution of health impacts (Mitchell et al. 2016)."

We do not refer to Astrom et al. (2013) as their work is on future health impacts and not on observed historical changes.

In a similar context, I also believe the authors need to highlight the weaknesses in their approach, as compared to other approaches. Two obvious ones are:

1) *any attempt at making a counterfactual climate is difficult, and can be done multiple , ways. Many other methods therefore provide an uncertainty in their modelled climate, but you do not. I understand why you don't, but the implications of this are important.*

We consider the potential deficits of the observational climate data as the main source of uncertainty in the proposed impact attribution framework and propose to address this aspect by using more than one factual-counterfactual pair of climate forcing data for impact attribution, see lines 129 to 142:

"In this paper we introduce a detrending method tailored to support impact attribution and illustrate its application to one of the observational climate datasets provided within ISIMIP3a (https://protocol.isimip.org/protocol/ISIMIP3a, see data section below). The quality of the associated impact attribution studies will critically depend on the quality of that observational dataset. Deficits in the observational data may lead to artefacts in the derived historical trends. For the dataset used here, we identify some of those artifacts. Since it is expected that other artifacts will be found for other observational datasets, impact attribution studies should ideally be based on a range of different observational datasets to facilitate a quantification of the contribution of observational climate data uncertainty to the uncertainty of the attribution results. This is also planned within ISIMIP3a. For the dataset considered here and potential additional

ones we propose a collection of control plots that should be used to scan the observational climate data for artifacts in preparation of each individual impact attribution study. While we provide the control plots for a set of large-scale world regions and all climate variables covered by our observational climate dataset, they should be adjusted to the regions and variables of interest in an impact attribution study as part of the analysis of the factual impact simulation (Fig. 2). Low-quality factual climate forcing data is expected to result in a low-quality reproduction of observed variations in the impact indicators of interest. If that is the case, the simulation set-up outlined here does not allow for an attribution of the observed changes in impacts to climate change."

We realized that it is important to better understand our attribution set-up and the role of climate therein. We now better describe the set-up in lines 61 to 67:

In addition to 'climate attribution', research on 'impact attribution' addresses the question: To what degree are observed changes in natural, human and managed systems induced by observed changes in climate (Fig. 1, second arrow). In the WGII contribution to the IPCC-AR5, an entire chapter was dedicated to the topic including the following definition: An impact of climate change is 'detected' if the observed state of the system differs from a counterfactual baseline that characterizes the system's behavior in the absence of changes in climate (IPCC 2014, chap. 18.2.1) and 'attribution' is the quantification of the contribution of climate change to the observed change in the natural, human or managed system. In both cases "climate change refers to any long-term trend in climate, irrespective of its cause" (IPCC 2014, chap. 18.2.1)."

In such set-up 'observed climate change' is defined as the specific realized long-term trend in climate captured by the central estimates of the model parameters. Therefore the parameters are not varied to capture the uncertainties of the 'true' parameters given potential other realisations of the historical data. In this regard the attribution of the magnitude of an extreme impact event to the 'realized' trend in climate is similar to the quantification of the 'Contribution of Observed Trend to Event Magnitude' discussed by (Diffenbaugh et al. 2017).

To highlight the difference to the probabilistic approach used climate attribution to anthropogenic forcing we have added the following paragraph to the main text, see lines 69 to 76:

"While in principle changes in natural, human and managed systems could also be attributed to anthropogenic climate forcing ('impact attribution to anthropogenic climate forcing', first and second arrow in Fig. 1, (Pall et al. 2011; Schaller et al. 2016; Mitchell et al. 2016)), we focus on 'impact attribution to climate change' as described in the WGII definition and introduce a climate dataset that can be used as input to climate impact models to characterize the system's behavior in the absence of climate change. As the response of natural, human or managed systems to climate and socio-economic forcings is commonly considered deterministic (or at least simulated in this way) the attribution of impacts to the observed realisation of climate change does not necessarily have to be addressed in a probabilistic way and gives more weight

to the separation of climate change from direct human influences as potential drivers of changes in the considered systems."

    2) *Many authors have argued that impacts are felt in the extremes of climate more so than elsewhere, that is why counterfactual attempts are often made with very large model simulation sizes. This is not so easy in your methodology, although I can see ways forward for it – this should be discussed.*

Thank you very much for the comment. Indeed, we do not apply a probabilistic approach but rather extend the so called quantification of the 'Contribution of Observed Trend to Event Magnitude' discussed for weather extremes by Diffenbaugh et al. (2017) to the impacts of these weather extremes. This is now discussed in detail in the revised manuscript, see the revised introduction and lines 45 to 54:

"Climate attribution can refer to observed long-term trends (WGI contribution to IPCC 2013, chap. 10) or individual events (Trenberth, Fasullo, and Shepherd 2015; NAS 2016; Stott et al. 2016). Given the probabilistic setting, results are often formulated as statements such that 'Anthropogenic climate forcing has increased the probability of occurrence of the observed trend or the intensity or duration of a specific extreme event'. In a non-probabilistic framework the intensity of an observed event can be attributed to the observed realisation of climate change by comparing the event magnitude in the observed time series to the magnitude of the same event in a detrended version of the observed time series (quantification of the 'contribution of the observed trend to event magnitude', (Diffenbaugh et al. 2017)). This type of attribution to climate change does not address the reasons of the observed climate trend."

as well as lines 73 to 76:

"As the response of natural, human or managed systems to climate and socio-economic forcings is commonly considered deterministic (or at least simulated in this way) the attribution of impacts to the observed realisation of climate change does not necessarily have to be addressed in a probabilistic way and gives more weight to the separation of climate change from direct human influences as potential drivers of changes in the considered systems."

The authors have worked in IPCC WG2 for a long time, and maybe a bit in WG1, but they need to be aware that their readers might be solely in one WG (or none at all), so the subject specific language needs to be very simple for a paper like this.

We thank the reviewer for the helpful comment and hope that we have managed to adjust the language accordingly.

Other corrections

- *Title "counterfactual climate for impact attribution" – I see why you have this title, ´ but it seems that your work would be very useful*

We could not fully grasp this comment, and therefore have no proper reply.

- *Line 9: "anthropogenic" is needed ´ before climate change*

In most general terms, attribution is about the quantification of drivers of change. Impact attribution does not necessarily trace changes in impacts back to the anthropogenic causes of climate change. We rewrote the abstract to make our concept clearer.

- *Line 9: "Other drivers change according to observations". ´ Actually I think the other drivers should remain the same according to observations.*

This may be a misunderstanding. We aim to address problems with time-evolving impact drivers other than climate. They thus follow the observations, but still change. We rewrote the abstract to prevent this confusion.

- *Line 19-21: This sentence is very confusing without reading the paper, I suggest ´ making it stand alone.*

We rewrote the sentence and hope it is clearer now.

- *Line 26: Citation needed. Haustien et al, 2017 is a good ´ one, but there are others.*

We now cite the IPCC 1.5°C report, which has this statement in the Summary for Policymakers.

- *Figure 1: In this figure you show "climate change" as the affected quantity – this should just be "climate". Likewise for the driver in the second panel. I also do not agree with the caption that this is how the IPCC frame attribution.*

We updated the figure accordingly, with the aim to also clarify the link between attribution approaches and the potential to attribute impacts to emissions. We adapted the caption to be less prescriptive on the IPCC framing.

- *Lines 31-38: I like this description, and it is now clearer in my head ´ what you are doing. If this section can be summed up on 1 line for the abstract, that would really help make things clear from the start.*

Thank you for this remark! We now provide the description at the beginning of the abstract:

'Attribution in its general definition aims to quantify drivers of change in a system. According to IPCC WGII a change in a natural, human or managed system is attributed to climate change by quantifying the difference between the observed state of the system and a counterfactual baseline that characterizes the system's behavior in the absence of climate change, where "climate change refers to any long-term trend in climate, irrespective of its cause".'

- *Line 63-66: This section is ´ confusing me, in much the same way the end of the abstract did. Specifically, you say impact attribution does not need to address the causes of climate change. So, what is it addressing? You could state that explicitly here. I also think the attribution community would see this differently, and there is a danger that people will now be confused over what this term means.*

This is a key point to understand the presented concept, and we thank the reviewer for pointing this out. We now make this clearer at several points in the manuscript, including the abstract:

"Attribution in its general definition aims to quantify drivers of change in a system. According to IPCC WGII a change in a natural, human or managed system is attributed to climate change by quantifying the difference between the observed state of the system and a counterfactual baseline that characterizes the system's behavior in the absence of climate change, where "climate change refers to any long-term trend in climate, irrespective of its cause". … Attribution of climate impacts to anthropogenic forcing would need an additional step separating anthropogenic climate forcing from other sources of climate trends, which is not covered by our method."

See also lines 61 to 73 of the introduction:

"In addition to 'climate attribution', research on 'impact attribution' addresses the question: To what degree are observed changes in natural, human and managed systems induced by observed changes in climate (Fig. 1, second arrow). In the WGII contribution to the IPCC-AR5, an entire chapter was dedicated to the topic including the following definition: An impact of climate change is 'detected' if the observed state of the system differs from a counterfactual baseline that characterizes the system's behavior in the absence of changes in climate (IPCC 2014, chap. 18.2.1) and 'attribution' is the quantification of the contribution of climate change to the observed change in the natural, human or managed system. In both cases "climate change refers to any long-term trend in climate, irrespective of its cause" (IPCC 2014, chap. 18.2.1).

While in principle changes in natural, human and managed systems could also be attributed to anthropogenic climate forcing ('impact attribution to anthropogenic climate forcing', first and second arrow in Fig. 1, (Pall et al. 2011; Schaller et al. 2016; Mitchell et al. 2016)), we focus on 'impact attribution to climate change' as described in the WGII definition and introduce a climate dataset that can be used as input to climate impact models to characterize the system's behavior in the absence of climate change. "

- *Data section: What is the spatial scale of the dynamically ´ downscaled data? How much do we trust this data, especially in poorly observed parts of the world? What are the implications for these problems on the questions posed?*

The dynamical downscaling was done to a target resolution of approximately 0.5°. We expanded the data description section so that caveats are now better traceable. Concerning attribution studies using our data we now propose i) a series of control plots, ii) suggest to consider additional pairs of factual-counterfactual climate forcing data to better capture the

uncertainty induced by different climate forcing data sets and iii) suggest to always provide a detailed assessment of the explanatory power of the factual impact model simulations to overall reduce the risk of erroneous impact attribution induced by deficits in the observational data.

- *Line 101: Lots of work has been done on pattern scaling recently, so I think a more modern approach should be cited here. E.g. Herger, 2015, although many others exist as well.*

This is indeed true. We added the citation with a short explanation, see lines 242 to 244.

- *Line 101-105: This paragraph makes it much clearer in my head what you are doing. I would use some of this text to explain this earlier, especially about the non-causal link with global temperature.*

This is a good hint. We now state in the abstract that our approach aims at attribution to climate change, irrespective of the cause of climate change. We state in the introduction, see lines 78 to 80:

"The method proposed here is designed to generate a stationary climate without long-term changes. The statistical model used to produce this counterfactual climate removes the long-term change correlated with (but not necessarily caused by) large scale climate change, represented by GMT change instead of a simple temporal trend (see Methods). "

- *Page 7: Why are the distribution names bolded?*

We removed the bold font.

- *Line 144: I understand why you are including hurs, and commend it, but it is a bounded quantity (i.e. nominally constrained between 0-100), so would that cause problems when modelling with a Gaussian?*

We reworked the model for relative humidity (hurs) and now use a Beta distribution, which better represents the nature of this variable.

- *Figures 4 and 5: These are very nice and informative figures.*

Thanks for this positive judgement! They are now Figures 5 and 6.

- *Lines 255-256: I agree it is rare, but there are still numerous studies that have done this (see major comment).*

We now discuss these studies in the introduction section and set them in context to our work. See response to major comment.

- *Line 277-279: You should state clearly here why your data is useful in a complementary way to Gillett et al*

We now set our work in context to Gillett et al in the introduction, see lines 42 to 54. We also rewrote the section in the discussion section to clarify the complementarity of our dataset, see lines 470 to 484.

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