Reply to Short Comment by Ivan Bogoev

We thank Ivan Bogoev (IB hereafter) for his consideration of our manuscript, although we didn't particularly appreciate the repeated reference to instrumentations and protocols developed and commercialized by IB's company, in a context where no specific instrumentation is discussed and without strong relevance to the topics discussed in the manuscript.

Following are our replies, breaking down the text into individual comments.

Comment 1:
The scope of the paper is too narrow and addresses only a certain class of eddy covariance (EC) systems with instruments providing data to the data logging system via Ethernet or serial communication protocols and does not mention an important digital communication protocol, called Synchronous Devices for Measurement (SDM) that has been in use for more than 18 years. This protocol was specifically developed to meet the stringent requirements for time synchronization between the EC sensors. Two sensor manufacturers, Licor Inc. and Campbell Scientific, Inc. collaborated and implemented this protocol in some of their instruments. The SDM protocol allows for external instrument triggering at precise moment in time, so multiple sensors can be measured synchronously. Some sonic anemometers, like the CSAT3 (Campbell Scientific, Inc.) can accept a trigger and provide almost instantaneous measurement when prompted by the datalogger. This approach does not require the complexity of high level of clock synchronization between individual devices.

In our manuscript we intentionally avoided to mention and discuss existing commercial solutions (including the SmartFlux™ system released by LI-COR Biosciences, to which two of the Authors are affiliated). Rather, we focused our attention on the description and quantification of the timing-related errors potentially involved in fully-digital acquisition system, and on protocols and solutions that are scalable and likely to be applicable to a wide range of instrumentations and gas species. It is correct to say that a data collection strategy based on instrument triggering would avoid STEs (though not necessarily RTEs). However, implementing a triggering-based system is much more complicated - when even possible at all - for typical research groups that design and realize their own EC acquisition system. For example, to the best of our knowledge most sonic anemometers and gas analysers (also beyond CO_2 and H_2O) do not support a triggering signal. In particular, SDM is a Campbell Scientific Inc. proprietary protocol, implemented in just a handful of instruments (manufactured by only LI-COR Biosciences and Campbell Scientific) and very unlikely to ever be implemented in new instrumentation, other than Campbell's. For these reasons, we consider SDM a commercial solution of interest in specific cases, but of little relevance in general, for future EC systems, and for the scope of our manuscript.
Nonetheless, prompted also by Referee #2, in the revised manuscript we will add a note on SDM and discuss triggering-based systems (see also Replies to Referee #2).

Comment 2:
The authors should include more details about the fundamental principle of operation of the devices used in EC systems and more specifically the widely used NDIR analyzers and sonic anemometers.
Both of these sensors require some finite amount of time to make and process each measurement. They cannot provide a continuous analog signal, but rather discrete measurements. Consequently, the analog voltage outputs provided through the digital to analog converters (DAC) are discrete in time and magnitude. A unique feature, specific only to the gas analyzer, is the use of a rotating optical filter wheel to multiplex the desired infrared bands used in the gas concentration measurements. The gas analyzer can produce a single measurement of CO2 and H2O per each rotation of the filter wheel. This makes the frequency of the concentration measurements dependent on the rotational speed of the chopper wheel. If a precise timing is required the speed of the filter wheel need to be controlled precisely. Also, because of the dependency between the measurements and rotation of the wheel, the gas analyzer cannot be triggered to provide CO2 and H2O readings at a precise moment in time. So, the only option to provide a reading at a given moment in time is to spin the filter wheel fast (like 150 rotations per second for the Li-7500) and report the measurement made immediately on the next rotation of the filter wheel. With this approach the gas readings could be at best synchronized to 1/150 second (6.7 ms), which is proven acceptable for most EC applications. Similar approach should be implemented with sonic anemometers that do not have a trigger mode and the ability to provide a measurement upon a request from the data logging system.

We agree with this comment and thank IB for the suggestion. We will elaborate on this concept in the Introduction of the revised manuscript. We note, however, that the uncertainty deriving from the time-discrete nature of the measurement is random and does not accrue with time. Therefore, it can be assimilated to an RTE, whose effect on fluxes we verified to be negligible, as long as the precision of the measurement cycle timing is sufficiently smaller than the sampling frequency. This is certainly the case in NDIR-based analyzers.

Comment 3:
A new generation of EC systems has been available for the last several years. These EC systems include a gas analyzer and a sonic anemometer, as co-located or as stand-alone devices, that share a common set of electronics so that wind, temperature and gas concentration measurements can be made simultaneously. This approach does not require precise clock synchronization and still provides sub millisecond timing between individual measurements.

As already mentioned in the original manuscript, "*Commercial solutions implementing sound engineering practices do exist for long-established EC of CO_2 and H_2O, which guarantee data synchronicity within specifications that meet EC requirements.*" [Pag. 1, Lines 29-30]. The ones referred to by IB (implicitly here, and explicitly several times later) are just a few among those. Our manuscript is concerned primarily with EC systems assembled by research groups or individuals, to address needs which are either not yet met by industrial partners, or are met with solutions not scalable to other existing EC instrumentation.

Comment 4:
The study examines only the errors in the covariance of vertical wind and sonic temperature. The errors in the scalar fluxes could be strongly modulated by the density effects (WPL) associated with temperature and humidity.

While we indeed used sonic temperature in our simulations, the intention was not that of estimating errors in "covariance of vertical wind and sonic temperature". A sonic anemometer always measures sonic temperature, too, and therefore there is no timing issue there. The simulation using modified sonic temperature is intended to describe what happens to a scalar measured by a *separate* instrument under various scenarios of RTEs and STEs.
Of course, WPL effects modulate fluxes. If timing issues affect covariances between *w* and *c*, everything downstream is affected, including the H_2O-related WPL terms (but not the T_s-related terms). Resulting errors could be somewhat smaller or larger, depending on factors such as the relative error in *w*-*c* covariance and the magnitude of the dilution-related WPL terms relative to the uncorrected flux.

Comment 5:
The scope of the paper can be extended to characterize the effect of timing errors on H2O and CO2 fluxes which are of most interest in energy and carbon balance studies. The ability of the IRGASON to provide synchronous temperature, wind, H2O and CO2 readings makes it a suitable instrument to study the reduction of co-variance not only on vertical wind and temperature, but on the other scalars. The implications of underestimated sensible heat flux due to systematic timing errors and its effects on the WPL terms and ultimately on the CO2 flux could be addressed.

As explained in the reply to the previous comment, our manuscript is *already* concerned with gas fluxes. Considering all other effects playing a role in the flux computation (flow distortion, for an example) is outside the scope and in the specific case would actually *reduce* the scope by concentrating on the specifics of CO_2/H_2O, while the study is agnostic to the gas species under observation and only relies on the spectral similarity assumption.
This is important, in that we expect synchronization issues to be a significant concern in, for example, CH_4 and N_2O flux measurements.

Comment 6
Additional information about the data sets used in the study should be included, like sample rate, sensor path length and anti-aliasing filter bandwidth.

We will consider adding this information in the revision of the manuscript, if proved to provide any additional insights.

Comment 7
The validation of the simulation design is not convincing, because the experimental conditions for the 100 Hz sonic data are unknown. (Co)spectral plots should be shown to verify the spectral content of the validation signals. It would have been more appropriate to use 100 Hz data from one of the EC sites.

Given the virtually identical results obtained using 10 or 100 Hz data, we think that elaborating on experimental conditions and adding plots and details on the validation procedure detracts from the clarity and straightforwardness of the message, without adding anything useful. Though not stated explicitly (and we can fix that in the revision), the 100 Hz data was collected under conditions representative of typical EC measurements. As IB is probably aware of, 100 Hz turbulence data are not straightforwardly available, especially to prospective readers of our manuscript (EC practitioners). We wanted to setup a simulation that could easily be replicated by others, using also the source code that we plan to make available. For this reason we used 10/20 Hz data, and only used 100 Hz data to validate the procedure.

Other comments

IB added a number of more specific comments referring to specific pages and lines. Most of them are related to the points already discussed above, so we consider them answered and clarified.
However there is one comment that we would like to address. IB wrote: *"There is a conflict between promoting collaboration between manufacturers and protecting technical ideas with patents. The authors should not recommend that the scientific community collaborate with manufacturers of EC equipment unless they are willing to share the ideas that they patented with United States Patent 9,759,703 B2. This patent claims protection of an IEEE adopted standard, Precision Time Protocol, for synchronization between eddy-flux instrumentation."*

Firstly, we need to point out that the Authors of the manuscript are from different Institutions - not only from LI-COR Biosciences - and therefore they collectively don't own any patent. We urge more rigour and respect for the role and work of each individual.
The statement "*There is a conflict between promoting collaboration between manufacturers and protecting technical ideas with patents*" is factually incorrect. The patent system is designed to protect the inventor from infringement, and it does not impede collaboration (please refer to e.g. GitHub or any public software repository for an example). In fact, we argue that this manuscript and the patent referenced by IB *are exactly* a way of sharing ideas with the academic research community, while protecting intellectual property.

To the point of the manuscript under discussion, the Authors, and in particular G.F and K.E. of LI-COR Biosciences, think that using open and widely adopted protocols in conjunction with proprietary firmware to implement scalable and “agnostic” data acquisition systems strikes the perfect balance between: (1) allowing flexibility while guaranteeing high-quality measurements and (2) protecting commercial interest.

Finally, IB points out that:
Page 17, Figure 7: The authors should explain the cluster of points at the high end of the frequencies for the time series with the 180 microseconds STE. It would be more appropriate to include Ogives plots to show if the two signals become out of phase at some frequencies causing negative values for the covariance.

We addressed this point in the context of a reply to the Short Comment by W. Eugster (see Point 3, second bullet in that reply).