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Interactive comment on "Evaluating stream CO_2 outgassing via Drifting and Anchored flux chambers in a controlled flume experiment" *by* Filippo Vingiani et al.

Anonymous Referee #1

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The authors assess different types of flux chambers and different modes of deployment for measuring CO2 fluxes and corresponding gas exchange velocities in running waters. They performed a set of measurements in laboratory flumes, where flow velocity and slope could be adjusted, and compared static chambers (held at a fixed locations), drifting chambers (drifting with the flow), regular chambers (where the chamber edges penetrate into the water) and flexible foil chambers (chamber is sealed to the atmosphere using an adhesive foil). The main focus of their analysis is on estimating gas exchange velocities (k600), which are estimated from measured fluxes and dissolved gas concentrations. Exchange velocities are primarily controlled by near-surface turbulence in water, which has been measured during the experiments.

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Discussion paper

In line with previous research, the study results suggest that application of standard chambers in results in chamber artifacts, particularly in static deployments. The novel aspect of the present study is in the detailed analysis of systematic and statistical errors when chamber measurements are used for estimating gas exchange velocities. Unfortunately, there have been no reference measurements of fluxes and exchange velocities in the present study, so that the conclusions are based on comparisons of chamber performance. Nevertheless, the manuscript fills an important gasp, as the effect of chamber design and deployment mode on measuring gas fluxes from aquatic systems received very little scientific attention.

The manuscript is very well written and easy to follow. I have a few suggestions and technical comments, which may improve clarity:

- dissipation rates of turbulent kinetic energy were estimated using a bulk approach (from channel slope and flow depth), as well as from local measurements of turbulent velocity fluctuations. I suggest to add and to discuss a comparison of both dissipation rate estimates, as the bulk approach can be more easily applied to field conditions.

- I suggest to mention the range and variability of measured CO2 fluxes 8in addition to dissolved concentration and gas exchange velocity.

- Fig. 2: why not using a scaled x-axis (instead of a categorical axis), where significant regressions could be added to the graph.

- Comparison of the scaling coefficient (alpha) in the equation relating k600 to dissipation rates to other studies: energy dissipation rates depend on the depth at which measurements were taken (see e.g. Esters et al. 2017, doi: 10.1002/2016JC012088 for wind-driven systems). In streams, turbulence is driven by bed friction, leading to a different depth-dependence of dissipation rates (and values of the "constant" alpha). Dissipation rates from bulk scaling (see above), in contrast, assume uniform distributions. This issue could be discussed further when comparing the results to other studies.

- line 452: I assume that the ADV velocities were rotated into a vertically-oriented coordinate system before all subsequent analyses? line 483: lower bound of the wave number for spectral fitting: the lower bound should not be defined by water depth, but by the distance of the ADV sampling volume from the water surface (as this defined the larges isotropic eddy).

- line 501: estimates of energy dissipation rates are typically log-normally distributed (see e.g., Baker et al. 1987, https://doi.org/10.1175/1520-0485(1987)017<1817:STITSO>2.0.CO;2). Arithmetic averaging may therefore be not appropriate.

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