

Point-by-point response to referee comments

We thank the reviewers for taking the time to read over and give constructive feedback on our paper titled “Inverse energy transfer in three-dimensional quantum vortex flows”, submitted to PRL. Please see below the point-by-point response to the comments made by the reviewers.

Referee A

1) p.1, right, “helicity, which is also an inviscid invariant”: During the reconnection in a finite temperature, the total helicity including mutual friction force and normal fluid velocity is conserved? Normal fluid is handled as a viscous flow. Please describe to readers.

The presence of the mutual friction force introduces an **additional source** of dissipation into the normal fluid. By **also** computing the helicity in the superfluid component, we observe that during the reconnection event the total helicity (**sum of normal fluid helicity and superfluid helicity**) is not conserved. We omitted **the plot of the superfluid helicity** from the **original** manuscript so as to **avoid introducing the further concept** of superfluid helicity to a paper whose narrative is **dedicated to the normal fluid**. In **the revised manuscript** we have addressed the referee’s concern by inserting (in page 4) an additional statement to clarify that the total helicity during the reconnection event is **not** conserved.

2) p.3, right: Two pairs of initially orthogonal vortices are considered in the present configuration. This paper reports that one pair of vortices yield a negative helicity to the normal fluid velocity. Please mention whether the other pair of vortices yields the positive helicity or not. If both of them yield the negative helicity, the initial setup already causes a symmetry breaking.

In our manuscript, we indeed only describe the results referring to one pair of reconnecting vortices. We confirm that the helicity injected by the other pair is indeed positive.

3) Fig.3: In Fig.3(b), at $t = t_R$ the energy transfer at $T = 2.1K$ is higher than that at $T = 1.9K$. However, the energy at $T = 2.1K$ is lower than that at $T = 1.9K$. This is inconsistent. Please describe what happens. Is it better to normalize the $E(k)$ by E_R ?

We appreciate the referee’s careful reading of the manuscript and their observation regarding a perceived inconsistency between the plots of the energy and energy flux spectra. The perceived inconsistency arises from the scalings employed. While the energy spectrum is in arbitrary units (in particular, in the non dimensional units **used in** the code), the energy flux is normalised with quantities which depend on the temperature. If, in fact, we plot the spectra in dimensional units (Figures 1 attached), no inconsistency emerges. In the revised version of the code, following the referee’s suggestion, we have scaled $E(k)$ by $E_R L_R$.

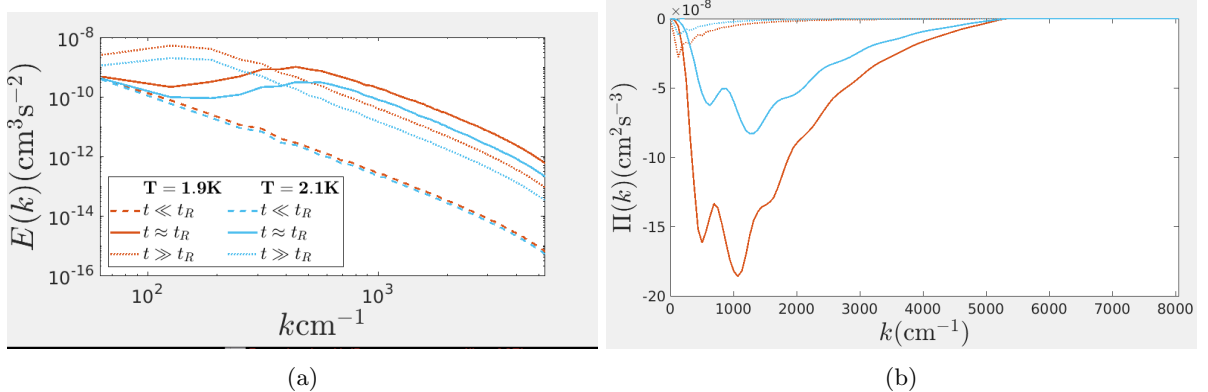


Figure 1: (a): Normal fluid kinetic energy spectrum $E(k)$ before reconnection (dashed lines), at reconnection (solid lines) and after reconnection (dotted lines) for $T = 1.9K$ (red) and $T = 2.1K$ (blue). (b): Spectral normal fluid kinetic energy flux, $\Pi(k)$.

4) p.4, right, “This inverse energy transfer arises from the helical character of the friction generated by the Kelvin waves released by the reconnecting cusp”: Is the inverse

energy transfer due to the Kelvin wave? This reviewer thought that it is due to the mutual friction force being helical as described on the left of p.4.

The mutual friction force is helical as a result of the presence of Kelvin wave packets released along the vortices after the relaxation of the cusp generated at reconnection. The two elements, the helical character of the mutual friction force and the presence of Kelvin wave, are therefore inter-twinned. In the revised manuscript we have underlined this aspect by slightly modifying the sentence pointed out by the referee.

5) Figure caption in Fig.1, $(t - t_0)/\tau_R$: t_0 is t_R ?

The value t_0 should in fact be t_R , the typo has been corrected.

6) Horizontal label in Fig.2: $(t - t_R)/\tau_R$ would be better than $(t - \tau_R)E_R^{1/2}/L_R$?

The authors agree with the comment of the referee that the current notation is not ideal. Instead, the figures have been changed to use the timescale in the horizontal axis of all time plots. Additionally to note, we have replaced the vertical axis label of Fig. 3b with $E_R L_R / \tau_R$, simply a change of symbols to make clear the non-dimensionalisation of units.

7) Figure 3(a): Is there a power law for the slope of $E(k)$?

The figure has been updated with a power law of k^{-3} .

8) p.4, left, "From Fig. 4 we determine the non-dimensional timescale...": Please describe the meaning of t^ .*

The time t^* was introduced as the time at which we observe a sufficient decay of the helical force ratio, where the difference is around 5% between the positive and negative modes and remains relatively stable afterwards. In order to make this clearer, we have added a sentence in the text to explicitly state this.

9) p.4, left, "corresponding dimensionally to $\tau = 0.1s$ for both temperatures.": Please describe where " $\tau = 0.1s$ " comes from or what that means. Is the time when the helicity is changing?

The value of 0.1s comes from dimensionalising the values of $\tau = 0.01$ for $T = 1.9K$ and $\tau = 0.005$ for $T = 2.1K$, using the values of $\tilde{\tau}$ listed in the supplementary materials.

10) Reference 53: B. ME should be M. E. Brachet. The year of 2026 should be 2017.

The incorrect reference noticed by the referee has been corrected.

Referee B

The authors' main motivation, as far as I understand, is to propose a mechanism for the inverse energy cascade which is numerically observed in counterflow superfluid turbulence [Polanco and Krstulovic, PRL (2020)].

We want to clarify to the Referee that the purpose of our work is not at all to explain Polanco & Krstulovic PRL 2020. Indeed, this article is only cited at the end of our manuscript as a possible physical setting where the new physical phenomena described in our manuscript could be relevant, and it could perhaps contribute to the origin of the inverse cascade reported in that paper (that one of us co-authors). To avoid possible confusion, we have reformulated the only paragraph in the conclusion where we cite Polanco&Krstulovic.

As recalled by the authors, inverse energy cascades show up when helicity symmetry is explicitly broken by external forcing as in [Plunian et al., JFM (2020)]. Their claim, then, is that vortex reconnection events in superfluid turbulence will similarly break helicity symmetry and trigger, in this way, an inverse energy cascade in the flow.

The referee correctly understood that the most important claim of our paper concerns the sign-defined helicity injection. We would like to highlight that we have consciously avoided talking about an inverse energy cascade, as we are well aware that our system is not turbulent. We have used the words *inverse energy transfer* instead.

While the paper's idea is interesting and worth pursuing, I believe the authors fail to provide even minimal evidence to support their case. Below, I address my main points. (i) First, the use of the word "turbulence" in the very first line (and used 16 times in other parts) of the manuscript is misleading: the flow studied by the authors is just a collision of vortex filaments. There is no turbulent flow at all here. It is all about a specific problem on superfluid flow instability.

As the Referee is certainly aware, vortex reconnections play an important role in turbulence. We have never claimed that our system is turbulent, but we do think that our findings have a strong impact on quantum turbulence. In the text, we have only referred to 'turbulence' in the introduction to motivate our work and to explain its relevance to a broad audience, and at the very end to comment on its implications in the conclusions. Explaining cascades, turbulence and invariants is necessary to understand the role of helicity on transfer towards large scales. We still believe that the use of the word turbulence is appropriate in our manuscript to put our results into the most relevant context.

Nevertheless, to take the referee's comment into account, we have added a sentence in the introduction (page 1) to remind the reader of the importance of vortex reconnections in quantum turbulent flows, and a comment in the conclusions about when our finding might be important for quantum turbulence.

(ii) The work of Polanco and Krstulovic [PRL (2020)] gives strong indication that counterflow superfluid turbulence behaves (for large enough counterflow velocities) as a quasi two-dimensional system and, therefore, an inverse energy cascade is expected to occur. Polanco and Krstulovic have also observed, by the way, the energy spectrum decay of the direct enstrophy cascade, in further qualitative agreement with the Batchelor-Kraichnan picture of 2D turbulence.

We do agree with the Referee's understanding of Polanco& Krstulovic main result. However, we would like again to point out that the purpose of our work is not at all to explain Polanco & Krstulovic PRL 2020. Indeed, this article is only cited at the end of our manuscript, as a possible physical setting where the new physical phenomena described in our manuscript could be relevant. The relevance of the large counterflow velocities in our mechanism for inverse energy transfer is in addition addressed in the next point.

(iii) The helicity production for an individual vortex reconnection event can be, of course, positive or negative. However, it is unlikely that this will be the case for the global helicity production associated to the complex vortex tangle of counterflow turbulence. I note that the previous results of [Plunian et al., JFM (2020)] refer to flow regimes where

helicity symmetry is broken across all scales by the external forcing.

Indeed, the helicity injection produced by random vortex reconnections is not sign-defined, but the helicity injected by a single **reconnection** is. In isotropic turbulence, the helicity injection produced by vortex reconnections is certainly positive and negative **and the overall helicity injected might cancel out.** **However,** in the case where the system is highly anisotropic and the vortex density is sufficiently large (as it is the case when counterflow velocities are large, where vortices tend to lie in a plane perpendicular to the heat flow), such symmetry breaking induced by the counterflow velocity could favour an asymmetry in the reconnection events triggering the overall production of helicity of one given sign. For this reason, at the end of the conclusion, we mentioned counterflow turbulence as such an example and raised the question of whether the findings of Polanco&Krstulovic could be ultimately related to the new mechanism reported in our Letter. Providing a definitive answer to this open question is undoubtedly challenging and beyond the scope of our work. In the revised version, we have softened and reformulated the sentence referring to Polanco&Krstulovic.

(iv) The vortex reconnection mechanism for the inverse energy cascade would imply that energy would flow from very small scales (around the atomic sizes of the vortex cores) towards the integral length scales. That is not what is observed, as discussed by Polanco and Krstulovic [PRL (2020)].

We remind the Referee that Polanco&Krstulovic used the HVBK model. This model does not describe **individual** quantised vortices **and their reconnections,** but only a continuous density of vortex lines, thus providing information only at length scales much larger than the average vortex separation. In the HVBK model the energy injection **occurs** via an external term whose acting scale can be chosen arbitrarily. **In the HVBK model** the inverse cascade is triggered by the nonlinear term when the counterflow is strong enough, which leads to a strong anisotropy. In our coupled model, the normal fluid energy injection is produced by vortex reconnections, and it is indeed transferred towards large scales by non-linear terms, at $k < 40$ (Fig. 3b), *i.e.* at scales much larger than the vortex core (we recall that our box is 1mm^3). We remind the Referee that one needs to be careful **when** comparing **the HVBK model** and our coupled model, as they describe the physics at very different scales. Again, by writing the sentence about Polanco&Krstulovic, we simply raise a question that needs further investigation.

(v) Finally, the “inverse energy cascade” detected by the authors may be unrelated to helicity production. One could just as easily argue that the perturbation/production of the standard component of the fluid through vortex reconnection is initially localized around the point where the filaments come into close contact. Subsequently, this normal component perturbation is strained by the specific background velocity configuration of the surrounding normal fluid. Helicity would not play any relevant dynamical role here.

The Referee raises a good point. In a recent paper [arXiv:2501.08309], three of us studied the wakes generated by moving superfluid vortices (with respect to the normal fluid). In this case, however, energy results to be transferred to *small* scales, discarding the scenario proposed by the Referee. **We attach here (Fig. 2) the plot of the energy flux spectrum corresponding to the development of the wakes: all values are positive, showing a direct energy cascade.**

For the above reasons, I do not recommend the paper for publication in the PRL.

We trust that we have clarified to the Referee the goal and main results of our Letter, and at the same time, provided enough arguments to emphasize the impact of our Letter and to make .

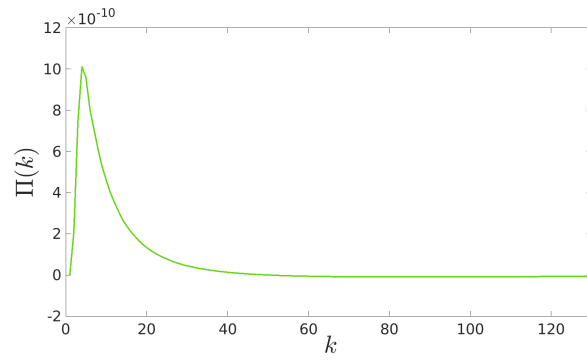


Figure 2: Spectral normal fluid kinetic energy flux, $\Pi(k)$ for the wakes generated in the normal fluid, data from [arXiv:2501.08309].