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Peer Review File

Self-reverting vortices in chiral active matter

Reviewers' comments:

Reviewer #1 (Remarks to the Author):

The authors in their manuscript "Self-reverting vortices in chiral active matter" combine theory and computer simulations to investigate chiral active particles with isotropic attractions but without implied alignment interactions. The authors report the formation of vortices with permanent or oscillating vorticities (self-reverting vortices) depending on the parameters of the chiral motion.

The paper is timely as the chiral active particles and their collective behavior are currently at the forefront of active soft matter research. The work is very detailed and clearly written. The theoretical arguments and details of the simulations are well articulated. Overall, I feel comfortable recommending the paper for acceptance.

Some minor comments:

- while the results are quite remarkable even without the alignment interactions, it would be eventually interesting to investigate the behavior of the system in the presence of those interactions (often driven by hydrodynamics) as is the case of the majority of the experimental systems dealing with chiral colloidal ensembles.

- for the case of oscillating vorticity: almost sinusoidal behavior of the \tau_{\Omega} in Fig 2c suggests the presence of a corresponding time-scale. A short discussion on the nature of this time-scale would be beneficial

- Symbol \tau is overloaded. It is used for time-scales and also for spatial average chirality \tau_{\Omega} in Fig. 2 b,c.

Reviewer #2 (Remarks to the Author):

In this work, Caprini, Liebchen and Löwen investigate an interesting system which has been recently under the focus of active matter community, namely the collective dynamics of chiral self-propelling particles. Here the novelty, compared to recent experimental and theoretical works in the field, is the absence of alignment interactions, combined with an algebraic-like long range attraction coming from a Lennard-Jones potential. With this system, the authors combine numerical simulations and theory to demonstrate the emergence of three dynamic states where the novel one is the self-reverting. In this case, the active particles form clusters that spontaneously and periodically revert the vorticity. In principle, a similar type of dynamic states (vortices that switch chirality) were experimentally observed by the group of A. Snetzko [Ref. 49]. But, of course, the system in the current work is different as different are the individual self-propelling units and hydrodynamic interactions do not play a role. Thus, I believe the findings in this work are original and the physical ideas put forward to explain them are well posed, taking into account the different, and necessary, approximations involved. The manuscript is also well written, easy to read, and the vast citations reflect that the authors have done a good job in looking at this emerging research field. For all these reasons, I support publication of this work in Comm. Phys., and have some questions/comments for the authors, here not in order of importance:

1. On page 1 first column are “self-propel” and “self-rotate” not related one to each other? If the particle is active it can take directed motion and, at the same type, display some spinning motion due to, for ex., asymmetric distribution of chemical. So I wonder why the author treat as two independent mechanism.

2. With Reference 3 I would add also Bibette microswimmer that was pioneering the field. Indeed, the Thomas E. Mallouk nanorod, to be precise, has not been exactly the first prototype in the field of catalytic swimmers. Few previous works from Whiteside group point along this direction (chemical propulsion) earlier, see for ex. Angew. Chem. Int. Ed. 41, 652 (2002).

3. The model is formulated with the inertia term, but at the end the theory works in the overdamped case with negligible thermal noise. I wonder what happen when $\tau/\tau_i \lessim 1$, i.e. small persistent time.

4. Dynamic states in Fig.2(a): are the boundaries between the different states (not thermodynamic phases!) really sharp? I.e. there is a sharp, first order-like transition between them?

5. The mapping is nice and lead to physical intuition, of course it works for the lattice case, i.e. strong attraction, and formation of compact cluster. I wonder if, for weak interaction, one can introduce small perturbation and see if the dynamic state s become unstable.

6. Again on the approximation used for the mapping, I understand that a basic hypothesis is that interparticle forces are balanced within the bulk, neglecting thus multi-body effects. However, as the cluster are finite size, I wonder how the effect of boundary will influences/break this approximation.

7. Figs.2d,e probably for the sake of the explanation of the self-reverting vortex vs full rotating one, it will be clearer to draw the dumbbells with different lengths in 2d and 2e.

8. Finally, the authors put forward a connection with odd viscous fluids. It will be possible to calculate/measure from simulation some odd viscosity?

Reviewer #3 (Remarks to the Author):

The paper deals with the emergence of collective vortex states in a model of chiral active matter, namely the active Brownian particle model with chirality. The main results are numerical finding of permanent and self-reverting vortex states (Fig.2a), which emerge despite the absence of the explicit alignment interaction. The authors give intuitive theoretical accounts on how these can happen, based on an effective alignment interaction that results from the interaction force.

Whereas chiral active matter is a subject of considerable attention and the reported results are sound and of interest, I was not convinced that the depth and the novelty of the results warrant publication in Communications Physics. First, the emergence of the effective alignment interaction in such systems was already reported by a subset of the authors in Ref.[50]. The submitted work is a chiral version of that, but since there is no cross effect between the chirality and the effective alignment interaction, the extension is quite straightforward. Then the emergence of permanent vortex state is expected, as a similar phenomenon was already reported in Ref.[20] for a model with alignment interaction. In contrast, perhaps the reverting vortex state is characteristic of the present model without explicit alignment interaction, as it requires the oscillation of individual particles at a frequency different from that of the cluster, but the underlying mechanism is rather trivial, and the authors' analysis did not go farther than what one can immediately understand from the sketch of Fig.2e. For these reasons, I consider that the present work is suitable for a more specific journal, such as Phys. Rev. E, Phys. Rev. Res., etc.. Scientific Reports also seems to be an option.

I also have the following concern on the model definition: the interaction potential U(r) seems to have a discontinuity at r = 3\sigma. This may result in unphysical singular behavior of particles. This is also worrisome in view of their effective theory, which assumes differentiability of the interaction potential.

Below I attach a list of typos and expressions that do not seem to make sense to me. I would suggest the authors consider these to improve the manuscript for future publication (elsewhere, in my opinion).

- 8th line in the section "Mechanism behind self-reverting vorticity" (p.4), "smaller" should be "greater". In the next line, the inequality should be $v_0 / \omega > \sigma$. A similar mistake also appears 5 lines below.

- 4 lines below Eq.(4), the sentence "if r_i rotates with a period smaller..." seems to be erroneous. This should be the condition for the reverting vortex state.

- last line of p.4, "This scaling law suggests that a larger cluster size favors the self-reverting vorticity state over the permanent vorticity state, with the latter becoming dominant as the cluster size becomes sufficiently large." This sentence does not make sense.

- Eqs.(7) and (8)b, the factor τ is missing in the chirality term.

- It is confusing to use Einstein's convention in some places (e.g. Eq.(9)) but not always (e.g., Eq.(12) contains explicit sum symbols).

- I think the last term of Eq.(11) should not have $-1/\tau$.

- The RHS of the first line of Eq.(12) looks strange, with inconsistent indices k,l,m and undefined function $U(x_l, x_m)$.

- It is misleading to say "τ does not play any role" (p.7, left, in the middle), as the velocity does exhibit rotational diffusion with time scale τ.

REPLY to Referee 1:

The authors in their manuscript "Self-reverting vortices in chiral active matter" combine theory and computer simulations to investigate chiral active particles with isotropic attractions but without implied alignment interactions. The authors report the formation of vortices with permanent or oscillating vorticities (self-reverting vortices) depending on the parameters of the chiral motion.

The paper is timely as the chiral active particles and their collective behavior are currently at the forefront of active soft matter research. The work is very detailed and clearly written. The theoretical arguments and details of the simulations are well articulated. Overall, I feel comfortable recommending the paper for acceptance.

Our reply: We thank the referee for the positive comments on our paper and, in particular, for the following sentence *The work is very detailed and clearly written. The theoretical arguments and details of the simulations are well articulated*. In the new version of the manuscript, we have addressed all the points raised by the referee for which we are grateful.

Some minor comments:

- while the results are quite remarkable even without the alignment interactions, it would be eventually interesting to investigate the behavior of the system in the presence of those interactions (often driven by hydrodynamics) as is the case of the majority of the experimental systems dealing with chiral colloidal ensembles.

  Our reply: We agree with the referee. In future studies, it could be interesting to investigate the behavior of chiral systems at high density additionally characterized by the presence of alignment interactions and/or hydrodynamics interactions. Given the experimental relevance of this case, we have added a paragraph in the conclusions of our manuscript about this open problem.

  *Even if here collective phenomena spontaneously emerge without alignment interactions, it could be interesting to evaluate the effects of explicit alignment mechanisms on chiral active particles at high density, in cluster configurations. This is a rather common scenario in self-propelled colloids that can behave as chiral microswimmers by simply introducing a rotational asymmetry in their body [**?**].*

- for the case of oscillating vorticity: almost sinusoidal behavior of the τ_Ω in Fig 2c suggests the presence of a corresponding time-scale. A short discussion on the nature of this time-scale would be beneficial.

  Our reply: We thank the referee for outlining this point that we have clarified in the new version of the paper. In Fig,2c, the average vorticity $\langle \Omega \rangle \tau$ (normalized by the persistence time τ), shows an oscillating behavior with a typical time-period T_{osc} which is numerically investigated in Fig.3c.

  In the new version of the paper, we have clarified the nature of this time scale that arises from the typical time due to chirality, i.e. $\sim 1/\omega$. We have added a sentence below Eq.(3):

  This phenomenon is a consequence of the additional time scale introduced by chirality, as confirmed by the oscillation period which scales as $\sim 1/\omega$.

  and further commented on this point below Eq.(6):

  Before completing a full rotation, the orientation of chiral active particles is reversed after a time period $\sim 2\pi/\omega$. This implies that these periodic oscillations are uniquely induced by chirality.

- Symbol τ is overloaded. It is used for time-scales and also for spatial average chirality τ_Ω in Fig. 2b,c.

  Our reply: We thank the referee for this comment. In Fig. 2b,c we have plotted the average vorticity $\langle\Omega\rangle$ (that is the inverse of a time), normalized by the time-scale τ, i.e. $\tau\langle\Omega\rangle$. However, we agree that our notation is misleading and we have replaced $\tau\langle\Omega\rangle$ with $\langle\Omega\rangle\tau$, to make clear that in Fig. 2b,c we plot a vorticity and not a time.

REPLY to Referee 2:

In this work, Caprini, Liebchen and Lwen investigate an interesting system which has been recently under the focus of active matter community, namely the collective dynamics of chiral self-propelling particles. Here the novelty, compared to recent experimental and theoretical works in the field, is the absence of alignment interactions, combined with an algebraic-like long range attraction coming from a Lennard-Jones potential. With this system, the authors combine numerical simulations and theory to demonstrate the emergence of three dynamic states where the novel one is the self-reverting. In this case, the active particles form clusters that spontaneously and periodically revert the vorticity. In principle, a similar type of dynamic states (vortices that switch chirality) were experimentally observed by the group of A. Snetzko [Ref. 49]. But, of course, the system in the current work is different as different are the individual self-propelling units and hydrodynamic interactions do not play a role. Thus, I believe the findings in this work are original and the physical ideas put forward to explain them are well posed, taking into account the different, and necessary, approximations involved. The manuscript is also well written, easy to read, and the vast citations reflect that the authors have done a good job in looking at this emerging research field. For all these reasons, I support publication of this work in Comm. Phys., and have some questions/comments for the authors, here not in order of importance:

Our reply: We thank the referee for the positive comments on our paper which is denoted as *original and well-posed*, for what concerns the results, and also *well-written* and *easy to read*.

In the new version of the manuscript, we have addressed all the points raised by the referee for which we are grateful.

1. On page 1 first column are self-propel and self-rotate not related one to each other? If the particle is active it can take directed motion and, at the same type, display some spinning motion due to, for ex., asymmetric distribution of chemical. So I wonder why the author treat as two independent mechanism.

   Our reply: We thank the referee for this comment. In the case of active colloids, self-propulsion and self-rotation often occur simultaneously. However, there are several physical systems that exhibit either pure translation or pure rotation. This is the case of active granular particles with an intrinsic chirality, for instance hexbug particles which are characterized by intrinsic chirality due to the asymmetric position of their battery in the particle body. Another example is provided by 3D-printed spinners, where the chirality can be tuned by twisting the legs as in the following paper: Scholz, Engel, Pöschel. Nat. Commun. 9, 2018 *Rotating robots move collectively and self-organize*. Here, self-propulsion is even absent.

   In the new version of the paper, we have commented on this point in the subsection *Model for chiral particles*.

   We remark that our model considers self-propulsion and chirality, i.e. self-rotations, as two independent mechanisms. Indeed, even if these two propulsions are often related in chiral active colloids, this is not the case in other physical systems, for instance active granular particles with an intrinsic chirality and spinners where the self-propulsion is even absent.

2. With Reference 3 I would add also Bibette microswimmer that was pioneering the field. Indeed, the Thomas E. Mallouk nanorod, to be precise, has not been exactly the first prototype in the field of catalytic

swimmers. Few previous works from Whiteside group point along this direction (chemical propulsion) earlier, see for ex. Angew. Chem. Int. Ed. 41, 652 (2002).

Our reply: We thank the referee for mentioning these previous studies in the field of active colloids. In particular, in the new version of the paper, we have cited the paper mentioned by the referee Angew. Chem. Int. Ed. 41, 652 (2002) and Nature 437, 862-865 (2005).

3. The model is formulated with the inertia term, but at the end the theory works in the overdamped case with negligible thermal noise. I wonder what happen when $\tau/\tau_i \lesssim 1$, i.e. small persistent time.

Our reply: We thank the referee for raising this point. In the small persistence time regime, when the inertial time corresponds to the smaller time scale, the cluster does not show any coherent motion and simply diffuses. Indeed, in this case, we can approximate the active force $\gamma v_0 \mathbf{n}$ with an effective Brownian motion because the active force direction changes fast in time. Therefore, in this regime, chirality plays a negligible role and consequently, the three states we have identified, i.e. non-permanent vorticity state, permanent vorticity state, and self-reverting vorticity state, cannot be observed.

This conclusion can be analytically taken by evaluating the activity equation (i.e. Eq.(1b) of the main text) in Cartesian coordinates:

$$\begin{aligned} \dot{\mathbf{n}} &= -\frac{\mathbf{n}}{\tau} + \omega \mathbf{n} \times \mathbf{z} + \frac{\sqrt{2}}{\sqrt{\tau}} \boldsymbol{\xi} \\ &= -\frac{\mathbf{B}}{\tau} \cdot \mathbf{n} + \frac{\sqrt{2}}{\sqrt{\tau}} \boldsymbol{\xi} \end{aligned} \tag{1}$$

where $\boldsymbol{\xi} = (0, 0, \xi)$ is a vector hortogonal to the plan of motion and $\mathbf{B}$ is a matrix with components

$$\mathbf{B} = \begin{bmatrix} 1 & \tau\omega \\ -\tau\omega & 1\,. \end{bmatrix} \tag{2}$$

In the small persistence time regime, $\tau \ll \tau_I$, τ is the faster time scale and we can take the overdamped limit in the equation for $\mathbf{n}$, by setting $\dot{\mathbf{n}} = 0$:

$$\mathbf{n} = \tau \mathbf{B}^{-1} \cdot \frac{\sqrt{2}}{\sqrt{\tau}} \boldsymbol{\xi} \tag{3}$$

where $\mathbf{B}^{-1}$ is the inverse of $\mathbf{B}$ with components

$$\mathbf{B}^{-1} = \frac{1}{1+\tau^2\omega^2} \begin{bmatrix} 1 & -\tau\omega \\ \tau\omega & 1\,. \end{bmatrix} \tag{4}$$

By substituting Eq.(3) in Eq.(1a) of the main text (the dynamics of $\mathbf{v}$), we obtain

$$m\mathbf{v} = -\gamma \mathbf{v} + \sqrt{2T\gamma}\boldsymbol{\eta} + \mathbf{F} + \gamma v_0 \sqrt{2\tau} \mathbf{B}^{-1} \cdot \boldsymbol{\xi}\,. \tag{5}$$

As a consequence, the active force simply behaves as a white noise which cannot induce the non-equilibrium collective motion observed in the regime of large persistence time.

In the new version of the paper, we have briefly commented on this point in the first paragraph of the subsection *Simulations unveil self-reverting vortices* and we have discussed this limit in an additional section of the methods.

4. Dynamic states in Fig.2(a): are the boundaries between the different states (not thermodynamic phases!) really sharp? I.e. there is a sharp, first order-like transition between them?

   Our reply: We thank the referee for this question. The boundaries between different states are not sharp. This is confirmed by Fig.3a where the time-averaged value of the spatial average vorticity is plotted as a function of the reduced chirality $\omega\tau$ for different cluster sizes. In all the cases, such a function shows a peak at a given value of $\omega\tau$ that is never sharp. However, we have distinguished the different states, for instance in Fig.2a, by defining a threshold that delineates the different states from each other (see the methods section, i.e. Eq.(31) and Eq.(32)).

   We have further commented on this point in the main text in the section *state diagram* (above Eq.(6):

   We remark that the crossover between different states is not a sharp transition but occurs smoothly. Indeed, the regions in Fig.2a are obtained by following the threshold criterion defined in the methods section.

   More details on this point are reported in the methods section.

5. The mapping is nice and lead to physical intuition, of course it works for the lattice case, i.e. strong attraction, and formation of compact cluster. I wonder if, for weak interaction, one can introduce small perturbation and see if the dynamic states become unstable.

   Our reply: We thank the referee for raising this question. Our theory assumes the lattice structure and we are not aware of any methods to relax this hypothesis and still be able to develop a theoretical prediction. Numerically, if we decrease the attraction strength, particles are characterized by larger fluctuations in their lattice positions. By further decreasing the attraction strength, the cluster will become unstable because self-propulsion could dominate attraction due to other particles, such that particles could leave the cluster. In this latter regime, collective motion of any kind cannot be achieved because the cluster itself is not stable. In the new version of the paper, we have added a comment on the role of interaction strength in the subsection *Simulations unveil self-reverting vortices*:

   In addition, the dynamical states shown here are obtained only in the regime of large attractions compared to thermal noise strength and activity, i.e. when the typical potential energy due to the inter-particle interactions is large compared to the thermal energy and the kinetic energy associated with self-propulsion $\approx mv_0^2/2$*. Indeed, without this condition, the cluster is not stable because particles are able to leave it and therefore it is not possible to observe collective motion. Here, to investigate the influence of chirality, we vary the associated dimensionless parameter, the reduced chirality* $\omega\tau$*, and examine different cluster sizes* L_c*.*

   We also remark that in our theory, we basically resort to a linearization of the force between different particles. This is possible only because of the solid structure. In principle, we believe that perturbation theory can be applied to non-linear potentials with a weak non-linearity $\mathbf{F} \approx -k_0\mathbf{x} - k_1|\mathbf{x}|^2\mathbf{x}$, with $k_1/k_2 \ll 1$. In this case, we expect to quantitatively provide a correction to our result without changing the observation of the three dynamical states. It is of course an intriguing perspective that we could explore in future studies.

   In the methods section, we have added the following sentence:

   We also remark that in our theory, we resort to a linearization of the force between different particles. This is possible because of the solid structure. In principle, perturbation theory can be applied to non-linear potentials with a weak non-linearity, such that the force $\mathbf{F} \approx -k_0\mathbf{x} - k_1|\mathbf{x}|^2\mathbf{x}$*, with* $k_1/k_2 \ll 1$*.*

In this case, we expect that the theory could quantitatively provide a correction to our result without changing the observation of the three dynamical states.

6. Again on the approximation used for the mapping, I understand that a basic hypothesis is that interparticle forces are balanced within the bulk, neglecting thus multi-body effects. However, as the cluster are finite size, I wonder how the effect of boundary will influences/break this approximation.

   Our reply: The referee is right. The theory cannot be straightforwardly applied to particles on the cluster boundary where the number of neighbors is different from six. Unluckily, it is not easily possible to extend the theory to include the contribution of the particles placed on the outer layer which could strongly on the cluster shape. However, if N_c is the total number of particles in the cluster, particles at the cluster boundary are $\approx \sqrt{N_c}$ and, thus, their relative contribution decreases as $1/\sqrt{N_c})$, such that for large clusters, their contribution is negligible. Still, it would of course be interesting to explore boundary effects for small or moderately sized clusters in the future.

   In the new version of the paper, we have added a comment on this limitation of our theory in the Methods:

   In this way, every particle is characterized by six neighbors. This implies that we consider systems sufficiently large to neglect the contribution of the outer layer of particles, whose number scales as $\approx \sqrt{N_c}$.

7. Figs.2d,e probably for the sake of the explanation of the self-reverting vortex vs full rotating one, it will be clearer to draw the dumbbells with different lengths in 2d and 2e.

   Our reply: We thank the referee for the suggestion. However, here, the dumbbell size represents the cluster size that is independent of the chirality. This is why we have chosen equal dumbbell sizes in Fig.2d and Fig.2e, while we have changed the sizes of the radius of the circular trajectory.

   However, by following the idea of the referee and making this figure more clear, we have changed the color and the symbol which denotes the chiral radius so that it can be distinguished from the typical dumbbell size.

8. Finally, the authors put forward a connection with odd viscous fluids. It will be possible to calculate/measure from simulation some odd viscosity?

   Our reply: We thank the referee for this interesting suggestion which could surely be central for a future study. Indeed, we prefer keeping the manuscript focused on the observation and characterization of the three dynamical states on which we focused, rather than introducing a coarse-grained, effective hydrodynamic description to estimate odd viscosity.

REPLY to Referee 3:

The paper deals with the emergence of collective vortex states in a model of chiral active matter, namely the active Brownian particle model with chirality. The main results are numerical finding of permanent and self-reverting vortex states (Fig.2a), which emerge despite the absence of the explicit alignment interaction. The authors give intuitive theoretical accounts on how these can happen, based on an effective alignment interaction that results from the interaction force.

Whereas chiral active matter is a subject of considerable attention and the reported results are sound and of interest, I was not convinced that the depth and the novelty of the results warrant publication in Communications Physics. First, the emergence of the effective alignment interaction in such systems was already reported by a subset of the authors in Ref.[50]. The submitted work is a chiral version of that, but since there is no cross effect between the chirality and the effective alignment interaction, the extension is quite straightforward. Then the emergence of permanent vortex state is expected, as a similar phenomenon was already reported in Ref.[20] for a model with alignment interaction. In contrast, perhaps the reverting vortex state is characteristic of the present model without explicit alignment interaction, as it requires the oscillation of individual particles at a frequency different from that of the cluster, but the underlying mechanism is rather trivial, and the authors' analysis did not go farther than what one can immediately understand from the sketch of Fig.2e. For these reasons, I consider that the present work is suitable for a more specific journal, such as Phys. Rev. E, Phys. Rev. Res., etc.. Scientific Reports also seems to be an option.

Our reply: We thank the referee for the careful reading and, in addition, we appreciate the referee's remarks and concerns that help us to improve the new version of the paper.

However, we do not agree with the referee's judgment on our work. The main argument of the referee is the absence of a cross effect between chirality and effective alignment interactions that does not justify an advance compared to previous work, such as Ref.[50] and Ref.[20]. Actually, this cross effect is present and gives rise to the self-reverting vortex state, which dominates for large cluster sizes, and spontaneously emerges from the competition between effective alignment and chirality.

In the new version of the paper, we have stressed this point, by mentioning the cross effect, both in the introduction of the paper to outline the novelty compared to previous studies, such as Ref.[50] and Ref.[20]. In particular, in the introduction, we write:

This state can be viewed as the superposition of the translational motion characterizing the previous state and an additional collective rotation due to chirality, which transfers from the single particle to the collective level. and an additional sentence: *The occurrence of this state is a consequence of the competition between chirality and isotropic interactions, which suppresses the tendency of a cluster to collectively rotate.*

In addition, we believe that the self-reverting vorticity state is not intuitive at variance with the permanent vorticity state, and we believe that its observation enriches the phases typical of chiral systems at high density. Theoretically, we were able to understand such a state by resorting to a mechanical argument which sheds light on the physical origin of this effect and allows us to predict the transition from the permanent vorticity state and self-reverting vorticity state. In addition, this argument was further supported by considerations of the total torque acting on the cluster.

I also have the following concern on the model definition: the interaction potential U(r) seems to have a discontinuity at $r = 3\sigma$. This may result in unphysical singular behavior of particles. This is also worrisome in view of their effective theory, which assumes differentiability of the interaction potential.

Our reply: We thank the referee for raising this point, which was a typo in the previous version of the paper. The interacting potential $U(r)$ is given by $\tilde{U}(r) = U_{LJ}(r) - U_{LJ}(3\sigma)$ for $r \leq 3\sigma$ and zero otherwise. Here, $U_{LJ}(r) = 4\epsilon\left[\left(\frac{\sigma}{r}\right)^{12} - \left(\frac{\sigma}{r}\right)^{6}\right]$ is the Lennard Jones potential while the constant $-U_{LJ}(3\sigma)$ shiftes the potential $U(r)$ to zero for $r = 3\sigma$. Thus, the potential is continuous.

In the new version of the paper (section *model for chiral particles*), we have corrected the expression for the potential:

The shape of the interaction potential $U(r)$ is obtained by truncating and shifting an attractive Lennard-Jones potential, $U_{LJ}(r) = 4\epsilon\left[\left(\frac{\sigma}{r}\right)^{12} - \left(\frac{\sigma}{r}\right)^{6}\right]$. The potential $U(r)$ is therefore defined as $U(r) = U_{LJ}(r) - U_{LJ}(3\sigma)$ for $r \leq 3\sigma$, and zero otherwise

By contrast, the force is continuous but not differentiable at 3σ. Actually, this absence of differentiability is not a problem for the theoretical analysis for the following reason: We resorted to the first-neighbors approximation, which allows us simply to select the interactions between the six-neighboring particles, which are roughly at distance $\approx \sigma$. If this condition is fulfilled, there are no theoretical issues: the second derivative of the potential (appearing in the theoretical prediction) is evaluated only at distance $\approx \sigma$.

In addition, we remark that the first-neighbors approximation is justified here because the second shell of the triangular lattice is formed by 12 particles: 6 of them at distance $\sqrt{3}\sigma$ and the other 6 at 2σ. The first-neighbors approximation works if $|U''(\sigma)| \gg |U''(\sqrt{3}\sigma)|$. This condition is verified in our case since, for $r > \sigma$, the potential scales as $\sim 1/r^6$.

We have reported this explanation in the methods sections.

Below I attach a list of typos and expressions that do not seem to make sense to me. I would suggest the authors consider these to improve the manuscript for future publication (elsewhere, in my opinion).

Our reply: We thank the referee for the careful reading: All the points raised by the referee have been addressed in the new version of the paper.

- 8th line in the section "Mechanism behind self-reverting vorticity" (p.4), "smaller" should be "greater". In the next line, the inequality should be $v_0/\omega > \sigma$. A similar mistake also appears 5 lines below.

  Our reply: We are grateful to the referee for the careful reading and for outlining this typo. We have corrected the condition outlined by the referee in the section "Mechanism behind self-reverting vorticity".

- 4 lines below Eq.(4), the sentence "if r_i rotates with a period smaller..." seems to be erroneous. This should be the condition for the reverting vortex state.

  Our reply: We are grateful to the referee for the careful reading. Indeed, the argument applies when $\mathbf{n}$ rotates with period π/ω that is smaller than the period of $\mathbf{r}$, say T_r. When this condition is fulfilled, i.e.

$\pi/\omega < T_r$, we can observe the self-reverting vorticity (regime of large ω) and, indeed, the total torque $\mathbf{M}$ changes sign periodically because $\mathbf{n}$ does.

In the new version of the paper, we have corrected the sentence.

- last line of p.4, "This scaling law suggests that a larger cluster size favors the self-reverting vorticity state over the permanent vorticity state, with the latter becoming dominant as the cluster size becomes sufficiently large." This sentence does not make sense.

  Our reply: We agree with the referee and we are grateful for the careful reading. Here, we simply mean that, for large cluster sizes, the self-reverting vorticity state dominates over the permanent vorticity state. Therefore, we have simply removed the last sentence *with the latter becoming dominant as the cluster size becomes sufficiently large*.

- Eqs.(7) and (8)b, the factor τ is missing in the chirality term.

  Our reply: We are grateful to the referee for the careful reading. In the new version of the paper, we have replaced the term $\omega\mathbf{n}_i \times \mathbf{z}$ with $\tau\omega\mathbf{n}_i \times \mathbf{z}$.

- It is confusing to use Einstein's convention in some places (e.g. Eq.(9)) but not always (e.g. Eq.(12) contains explicit sum symbols).

  Our reply: We thank the referee for the comment, however, the sum appearing in the first line of Eq.(12) is not a sum over repeating indices but simply a sum over all the pair of particles, i.e. the definition of the total potential $U_{tot} = \sum_{l<m} U(|\mathbf{x}_m - \mathbf{x}_m|)$. This is why we prefer keeping the sum explicit. In a similar spirit, we prefer keeping the sum $\sum^*$ running only on the first neighboring particles.

- I think the last term of Eq.(11) should not have $-1/\tau$.

  Our reply: We thank again the referee for the careful reading. We have removed the extra-factor $-1/\tau$ in the last term of Eq.(11).

- The RHS of the first line of Eq.(12) looks strange, with inconsistent indices k, l, m and undefined function $U(x_l, x_m)$.

  Our reply: We agree with the referee. The index k was a typo, which we have replaced by m to be consistent with the definition of the total potential $U_{tot} = \sum_{l<m} U(|\mathbf{x}_m - \mathbf{x}_m|)$, reported in the main text below Eqs.(1).

- It is misleading to say "τ does not play any role" (p.7, left, in the middle), as the velocity does exhibit rotational diffusion with time scale τ.

  Our reply: We agree with the referee and therefore we have replaced the sentence τ *does not play any role* with the sentence τ *plays a negligible role* which is actually what we intended before.

REVIEWERS' COMMENTS:

Reviewer #1 (Remarks to the Author):

The authors adequately addressed all the comments raised by the reviewers. I recommend the paper for acceptance.

Reviewer #2 (Remarks to the Author):

I have read again the manuscript and the answers of the authors to the queries of the Referees. The authors have satisfactorily addressed all these queries. Thus, I support publication of this manuscript in Comms. Phys. as it is.

Reviewer #3 (Remarks to the Author):

This revision mainly consists of the addition of descriptions requested by referees and the correction of minor mistakes. Regarding my main concern on the depth and the novelty of the results, let me first cite my criticisms:

> First, the emergence of the effective alignment interaction in such systems was already reported by a subset of the authors in Ref.[50]. The submitted work is a chiral version of that, but since there is no cross-effect between the chirality and the effective alignment interaction, the extension is quite straightforward. Then the emergence of a permanent vortex state is expected, as a similar phenomenon was already reported in Ref.[20] for a model with alignment interaction. In contrast, perhaps the reverting vortex state is characteristic of the present model without explicit alignment interaction, as it requires the oscillation of individual particles at a frequency different from that of the cluster, but the underlying mechanism is rather trivial, and the authors' analysis did not go farther than what one can immediately understand from the sketch of Fig.2e.

About the first point, my point was clearly on the extension of their theory to derive the effective interaction, but the authors' reply remained a general statement that all of the translation, the effective alignment and the chirality are important to the phenomenon reported here. As for the theoretical derivation I discussed, it is clear that the main part of the derivation [Eqs.(12)-(22)] is about the potential term which is decoupled from the chirality, so that the presence of the chirality simply results in adding a chiral term (the last term of Eq.(23)) to the effective equation. Although I appreciate this effective equation itself, since it was essentially already published in Ref.[50], the addition of chirality does not make this part of the theory nontrivial or novel.
So the main point of the paper is the finding of self-reverting vortices and its theoretical account, but then my evaluation (the second criticism in the cited text above) is that the mechanism is rather trivial in my taste, and the analysis did not provide more insights than what one can understand from the sketch of Fig.2e. The authors' following rebuttal on this point was rather consistent with this view of mine.

> we believe that the self-reverting vorticity state is not intuitive at variance with the permanent vorticity state, and we believe that its observation enriches the phases typical of chiral systems at high density. Theoretically, we were able to understand such a state by resorting to a mechanical argument which sheds light on the physical origin of this effect and allows us to predict the transition from the permanent vorticity state and self-reverting vorticity state. In addition, this argument was further supported by considerations of
> the total torque acting on the cluster.

For the rest of my remarks, which were about minor typos and expressions that required rephrasing, the authors made an appropriate revision. However, since my main concern was not resolved, my overall evaluation has not changed from the previous conclusion:

> I was not convinced that the depth and the novelty of the results warrant publication in Communications Physics.

and

> I consider that the present work is suitable for a more specific journal, such as Phys. Rev. E, Phys. Rev. Res., etc.. Scientific Reports also seems to be an option.

REPLY to Referee 1:

The authors in their manuscript "Self-reverting vortices in chiral active matter" combine theory and computer simulations to investigate chiral active particles with isotropic attractions but without implied alignment interactions. The authors report the formation of vortices with permanent or oscillating vorticities (self-reverting vortices) depending on the parameters of the chiral motion.

The paper is timely as the chiral active particles and their collective behavior are currently at the forefront of active soft matter research. The work is very detailed and clearly written. The theoretical arguments and details of the simulations are well articulated. Overall, I feel comfortable recommending the paper for acceptance.

Our reply: We thank the referee for the positive comments on our paper and, in particular, for the following sentence *The work is very detailed and clearly written. The theoretical arguments and details of the simulations are well articulated*. In the new version of the manuscript, we have addressed all the points raised by the referee for which we are grateful.

Some minor comments:

- while the results are quite remarkable even without the alignment interactions, it would be eventually interesting to investigate the behavior of the system in the presence of those interactions (often driven by hydrodynamics) as is the case of the majority of the experimental systems dealing with chiral colloidal ensembles.

  Our reply: We agree with the referee. In future studies, it could be interesting to investigate the behavior of chiral systems at high density additionally characterized by the presence of alignment interactions and/or hydrodynamics interactions. Given the experimental relevance of this case, we have added a paragraph in the conclusions of our manuscript about this open problem.

  *Even if here collective phenomena spontaneously emerge without alignment interactions, it could be interesting to evaluate the effects of explicit alignment mechanisms on chiral active particles at high density, in cluster configurations. This is a rather common scenario in self-propelled colloids that can behave as chiral microswimmers by simply introducing a rotational asymmetry in their body [**?**].*

- for the case of oscillating vorticity: almost sinusoidal behavior of the τ_Ω in Fig 2c suggests the presence of a corresponding time-scale. A short discussion on the nature of this time-scale would be beneficial.

  Our reply: We thank the referee for outlining this point that we have clarified in the new version of the paper. In Fig,2c, the average vorticity $\langle\Omega\rangle\tau$ (normalized by the persistence time τ), shows an oscillating behavior with a typical time-period T_{osc} which is numerically investigated in Fig.3c.

  In the new version of the paper, we have clarified the nature of this time scale that arises from the typical time due to chirality, i.e. $\sim 1/\omega$. We have added a sentence below Eq.(3):

  This phenomenon is a consequence of the additional time scale introduced by chirality, as confirmed by the oscillation period which scales as $\sim 1/\omega$.

  and further commented on this point below Eq.(6):

  Before completing a full rotation, the orientation of chiral active particles is reversed after a time period $\sim 2\pi/\omega$. This implies that these periodic oscillations are uniquely induced by chirality.

- Symbol τ is overloaded. It is used for time-scales and also for spatial average chirality τ_Ω in Fig. 2b,c.

  Our reply: We thank the referee for this comment. In Fig. 2b,c we have plotted the average vorticity $\langle\Omega\rangle$ (that is the inverse of a time), normalized by the time-scale τ, i.e. $\tau\langle\Omega\rangle$. However, we agree that our notation is misleading and we have replaced $\tau\langle\Omega\rangle$ with $\langle\Omega\rangle\tau$, to make clear that in Fig. 2b,c we plot a vorticity and not a time.

REPLY to Referee 2:

In this work, Caprini, Liebchen and Lwen investigate an interesting system which has been recently under the focus of active matter community, namely the collective dynamics of chiral self-propelling particles. Here the novelty, compared to recent experimental and theoretical works in the field, is the absence of alignment interactions, combined with an algebraic-like long range attraction coming from a Lennard-Jones potential. With this system, the authors combine numerical simulations and theory to demonstrate the emergence of three dynamic states where the novel one is the self-reverting. In this case, the active particles form clusters that spontaneously and periodically revert the vorticity. In principle, a similar type of dynamic states (vortices that switch chirality) were experimentally observed by the group of A. Snetzko [Ref. 49]. But, of course, the system in the current work is different as different are the individual self-propelling units and hydrodynamic interactions do not play a role. Thus, I believe the findings in this work are original and the physical ideas put forward to explain them are well posed, taking into account the different, and necessary, approximations involved. The manuscript is also well written, easy to read, and the vast citations reflect that the authors have done a good job in looking at this emerging research field. For all these reasons, I support publication of this work in Comm. Phys., and have some questions/comments for the authors, here not in order of importance:

$\boxed{\text{Our reply:}}$ We thank the referee for the positive comments on our paper which is denoted as *original and well-posed*, for what concerns the results, and also *well-written* and *easy to read*.

In the new version of the manuscript, we have addressed all the points raised by the referee for which we are grateful.

1. On page 1 first column are self-propel and self-rotate not related one to each other? If the particle is active it can take directed motion and, at the same type, display some spinning motion due to, for ex., asymmetric distribution of chemical. So I wonder why the author treat as two independent mechanism.

   $\boxed{\text{Our reply:}}$ We thank the referee for this comment. In the case of active colloids, self-propulsion and self-rotation often occur simultaneously. However, there are several physical systems that exhibit either pure translation or pure rotation. This is the case of active granular particles with an intrinsic chirality, for instance hexbug particles which are characterized by intrinsic chirality due to the asymmetric position of their battery in the particle body. Another example is provided by 3D-printed spinners, where the chirality can be tuned by twisting the legs as in the following paper: Scholz, Engel, Pöschel. Nat. Commun. 9, 2018 *Rotating robots move collectively and self-organize*. Here, self-propulsion is even absent.

   In the new version of the paper, we have commented on this point in the subsection *Model for chiral particles*.

   We remark that our model considers self-propulsion and chirality, i.e. self-rotations, as two independent mechanisms. Indeed, even if these two propulsions are often related in chiral active colloids, this is not the case in other physical systems, for instance active granular particles with an intrinsic chirality and spinners where the self-propulsion is even absent.

2. With Reference 3 I would add also Bibette microswimmer that was pioneering the field. Indeed, the Thomas E. Mallouk nanorod, to be precise, has not been exactly the first prototype in the field of catalytic

swimmers. Few previous works from Whiteside group point along this direction (chemical propulsion) earlier, see for ex. Angew. Chem. Int. Ed. 41, 652 (2002).

Our reply: We thank the referee for mentioning these previous studies in the field of active colloids. In particular, in the new version of the paper, we have cited the paper mentioned by the referee Angew. Chem. Int. Ed. 41, 652 (2002) and Nature 437, 862-865 (2005).

3. The model is formulated with the inertia term, but at the end the theory works in the overdamped case with negligible thermal noise. I wonder what happen when $\tau/\tau_i \lesssim 1$, i.e. small persistent time.

Our reply: We thank the referee for raising this point. In the small persistence time regime, when the inertial time corresponds to the smaller time scale, the cluster does not show any coherent motion and simply diffuses. Indeed, in this case, we can approximate the active force $\gamma v_0 \mathbf{n}$ with an effective Brownian motion because the active force direction changes fast in time. Therefore, in this regime, chirality plays a negligible role and consequently, the three states we have identified, i.e. non-permanent vorticity state, permanent vorticity state, and self-reverting vorticity state, cannot be observed.

This conclusion can be analytically taken by evaluating the activity equation (i.e. Eq.(1b) of the main text) in Cartesian coordinates:

$$\begin{aligned}\dot{\mathbf{n}} &= -\frac{\mathbf{n}}{\tau} + \omega \mathbf{n} \times \mathbf{z} + \frac{\sqrt{2}}{\sqrt{\tau}}\boldsymbol{\xi} \\ &= -\frac{\mathbf{B}}{\tau} \cdot \mathbf{n} + \frac{\sqrt{2}}{\sqrt{\tau}}\boldsymbol{\xi}\end{aligned} \tag{1}$$

where $\boldsymbol{\xi} = (0, 0, \xi)$ is a vector hortogonal to the plan of motion and $\mathbf{B}$ is a matrix with components

$$\mathbf{B} = \begin{bmatrix} 1 & \tau\omega \\ -\tau\omega & 1\,. \end{bmatrix} \tag{2}$$

In the small persistence time regime, $\tau \ll \tau_I$, τ is the faster time scale and we can take the overdamped limit in the equation for $\mathbf{n}$, by setting $\dot{\mathbf{n}} = 0$:

$$\mathbf{n} = \tau \mathbf{B}^{-1} \cdot \frac{\sqrt{2}}{\sqrt{\tau}}\boldsymbol{\xi} \tag{3}$$

where $\mathbf{B}^{-1}$ is the inverse of $\mathbf{B}$ with components

$$\mathbf{B}^{-1} = \frac{1}{1+\tau^2\omega^2}\begin{bmatrix} 1 & -\tau\omega \\ \tau\omega & 1\,. \end{bmatrix} \tag{4}$$

By substituting Eq.(3) in Eq.(1a) of the main text (the dynamics of $\mathbf{v}$), we obtain

$$m\mathbf{v} = -\gamma\mathbf{v} + \sqrt{2T\gamma}\boldsymbol{\eta} + \mathbf{F} + \gamma v_0\sqrt{2\tau}\mathbf{B}^{-1} \cdot \boldsymbol{\xi}\,. \tag{5}$$

As a consequence, the active force simply behaves as a white noise which cannot induce the non-equilibrium collective motion observed in the regime of large persistence time.

In the new version of the paper, we have briefly commented on this point in the first paragraph of the subsection *Simulations unveil self-reverting vortices* and we have discussed this limit in an additional section of the methods.

4. Dynamic states in Fig.2(a): are the boundaries between the different states (not thermodynamic phases!) really sharp? I.e. there is a sharp, first order-like transition between them?

   Our reply: We thank the referee for this question. The boundaries between different states are not sharp. This is confirmed by Fig.3a where the time-averaged value of the spatial average vorticity is plotted as a function of the reduced chirality $\omega\tau$ for different cluster sizes. In all the cases, such a function shows a peak at a given value of $\omega\tau$ that is never sharp. However, we have distinguished the different states, for instance in Fig.2a, by defining a threshold that delineates the different states from each other (see the methods section, i.e. Eq.(31) and Eq.(32)).

   We have further commented on this point in the main text in the section *state diagram* (above Eq.(6):

   We remark that the crossover between different states is not a sharp transition but occurs smoothly. Indeed, the regions in Fig.2a are obtained by following the threshold criterion defined in the methods section.

   More details on this point are reported in the methods section.

5. The mapping is nice and lead to physical intuition, of course it works for the lattice case, i.e. strong attraction, and formation of compact cluster. I wonder if, for weak interaction, one can introduce small perturbation and see if the dynamic states become unstable.

   Our reply: We thank the referee for raising this question. Our theory assumes the lattice structure and we are not aware of any methods to relax this hypothesis and still be able to develop a theoretical prediction. Numerically, if we decrease the attraction strength, particles are characterized by larger fluctuations in their lattice positions. By further decreasing the attraction strength, the cluster will become unstable because self-propulsion could dominate attraction due to other particles, such that particles could leave the cluster. In this latter regime, collective motion of any kind cannot be achieved because the cluster itself is not stable. In the new version of the paper, we have added a comment on the role of interaction strength in the subsection *Simulations unveil self-reverting vortices*:

   In addition, the dynamical states shown here are obtained only in the regime of large attractions compared to thermal noise strength and activity, i.e. when the typical potential energy due to the inter-particle interactions is large compared to the thermal energy and the kinetic energy associated with self-propulsion $\approx mv_0^2/2$. *Indeed, without this condition, the cluster is not stable because particles are able to leave it and therefore it is not possible to observe collective motion. Here, to investigate the influence of chirality, we vary the associated dimensionless parameter, the reduced chirality* $\omega\tau$, *and examine different cluster sizes* L_c.

   We also remark that in our theory, we basically resort to a linearization of the force between different particles. This is possible only because of the solid structure. In principle, we believe that perturbation theory can be applied to non-linear potentials with a weak non-linearity $\mathbf{F} \approx -k_0\mathbf{x} - k_1|\mathbf{x}|^2\mathbf{x}$, with $k_1/k_2 \ll 1$. In this case, we expect to quantitatively provide a correction to our result without changing the observation of the three dynamical states. It is of course an intriguing perspective that we could explore in future studies.

   In the methods section, we have added the following sentence:

   We also remark that in our theory, we resort to a linearization of the force between different particles. This is possible because of the solid structure. In principle, perturbation theory can be applied to non-linear potentials with a weak non-linearity, such that the force $\mathbf{F} \approx -k_0\mathbf{x} - k_1|\mathbf{x}|^2\mathbf{x}$, *with* $k_1/k_2 \ll 1$.

In this case, we expect that the theory could quantitatively provide a correction to our result without changing the observation of the three dynamical states.

6. Again on the approximation used for the mapping, I understand that a basic hypothesis is that interparticle forces are balanced within the bulk, neglecting thus multi-body effects. However, as the cluster are finite size, I wonder how the effect of boundary will influences/break this approximation.

   Our reply: The referee is right. The theory cannot be straightforwardly applied to particles on the cluster boundary where the number of neighbors is different from six. Unluckily, it is not easily possible to extend the theory to include the contribution of the particles placed on the outer layer which could strongly on the cluster shape. However, if N_c is the total number of particles in the cluster, particles at the cluster boundary are $\approx \sqrt{N_c}$ and, thus, their relative contribution decreases as $1/\sqrt{N_c})$, such that for large clusters, their contribution is negligible. Still, it would of course be interesting to explore boundary effects for small or moderately sized clusters in the future.

   In the new version of the paper, we have added a comment on this limitation of our theory in the Methods:

   In this way, every particle is characterized by six neighbors. This implies that we consider systems sufficiently large to neglect the contribution of the outer layer of particles, whose number scales as $\approx \sqrt{N_c}$.

7. Figs.2d,e probably for the sake of the explanation of the self-reverting vortex vs full rotating one, it will be clearer to draw the dumbbells with different lengths in 2d and 2e.

   Our reply: We thank the referee for the suggestion. However, here, the dumbbell size represents the cluster size that is independent of the chirality. This is why we have chosen equal dumbbell sizes in Fig.2d and Fig.2e, while we have changed the sizes of the radius of the circular trajectory.

   However, by following the idea of the referee and making this figure more clear, we have changed the color and the symbol which denotes the chiral radius so that it can be distinguished from the typical dumbbell size.

8. Finally, the authors put forward a connection with odd viscous fluids. It will be possible to calculate/measure from simulation some odd viscosity?

   Our reply: We thank the referee for this interesting suggestion which could surely be central for a future study. Indeed, we prefer keeping the manuscript focused on the observation and characterization of the three dynamical states on which we focused, rather than introducing a coarse-grained, effective hydrodynamic description to estimate odd viscosity.

REPLY to Referee 3:

The paper deals with the emergence of collective vortex states in a model of chiral active matter, namely the active Brownian particle model with chirality. The main results are numerical finding of permanent and self-reverting vortex states (Fig.2a), which emerge despite the absence of the explicit alignment interaction. The authors give intuitive theoretical accounts on how these can happen, based on an effective alignment interaction that results from the interaction force.

Whereas chiral active matter is a subject of considerable attention and the reported results are sound and of interest, I was not convinced that the depth and the novelty of the results warrant publication in Communications Physics. First, the emergence of the effective alignment interaction in such systems was already reported by a subset of the authors in Ref.[50]. The submitted work is a chiral version of that, but since there is no cross effect between the chirality and the effective alignment interaction, the extension is quite straightforward. Then the emergence of permanent vortex state is expected, as a similar phenomenon was already reported in Ref.[20] for a model with alignment interaction. In contrast, perhaps the reverting vortex state is characteristic of the present model without explicit alignment interaction, as it requires the oscillation of individual particles at a frequency different from that of the cluster, but the underlying mechanism is rather trivial, and the authors' analysis did not go farther than what one can immediately understand from the sketch of Fig.2e. For these reasons, I consider that the present work is suitable for a more specific journal, such as Phys. Rev. E, Phys. Rev. Res., etc.. Scientific Reports also seems to be an option.

$\boxed{\text{Our reply:}}$ We thank the referee for the careful reading and, in addition, we appreciate the referee's remarks and concerns that help us to improve the new version of the paper.

However, we do not agree with the referee's judgment on our work. The main argument of the referee is the absence of a cross effect between chirality and effective alignment interactions that does not justify an advance compared to previous work, such as Ref.[50] and Ref.[20]. Actually, this cross effect is present and gives rise to the self-reverting vortex state, which dominates for large cluster sizes, and spontaneously emerges from the competition between effective alignment and chirality.

In the new version of the paper, we have stressed this point, by mentioning the cross effect, both in the introduction of the paper to outline the novelty compared to previous studies, such as Ref.[50] and Ref.[20]. In particular, in the introduction, we write:

This state can be viewed as the superposition of the translational motion characterizing the previous state and an additional collective rotation due to chirality, which transfers from the single particle to the collective level. and an additional sentence: *The occurrence of this state is a consequence of the competition between chirality and isotropic interactions, which suppresses the tendency of a cluster to collectively rotate.*

In addition, we believe that the self-reverting vorticity state is not intuitive at variance with the permanent vorticity state, and we believe that its observation enriches the phases typical of chiral systems at high density. Theoretically, we were able to understand such a state by resorting to a mechanical argument which sheds light on the physical origin of this effect and allows us to predict the transition from the permanent vorticity state and self-reverting vorticity state. In addition, this argument was further supported by considerations of the total torque acting on the cluster.

I also have the following concern on the model definition: the interaction potential U(r) seems to have a discontinuity at $r = 3\sigma$. This may result in unphysical singular behavior of particles. This is also worrisome in view of their effective theory, which assumes differentiability of the interaction potential.

Our reply: We thank the referee for raising this point, which was a typo in the previous version of the paper. The interacting potential $U(r)$ is given by $\tilde{U}(r) = U_{LJ}(r) - U_{LJ}(3\sigma)$ for $r \leq 3\sigma$ and zero otherwise. Here, $U_{LJ}(r) = 4\epsilon\left[\left(\frac{\sigma}{r}\right)^{12} - \left(\frac{\sigma}{r}\right)^{6}\right]$ is the Lennard Jones potential while the constant $-U_{LJ}(3\sigma)$ shiftes the potential $U(r)$ to zero for $r = 3\sigma$. Thus, the potential is continuous.

In the new version of the paper (section *model for chiral particles*), we have corrected the expression for the potential:

The shape of the interaction potential $U(r)$ is obtained by truncating and shifting an attractive Lennard-Jones potential, $U_{LJ}(r) = 4\epsilon\left[\left(\frac{\sigma}{r}\right)^{12} - \left(\frac{\sigma}{r}\right)^{6}\right]$. The potential $U(r)$ is therefore defined as $U(r) = U_{LJ}(r) - U_{LJ}(3\sigma)$ for $r \leq 3\sigma$, and zero otherwise

By contrast, the force is continuous but not differentiable at 3σ. Actually, this absence of differentiability is not a problem for the theoretical analysis for the following reason: We resorted to the first-neighbors approximation, which allows us simply to select the interactions between the six-neighboring particles, which are roughly at distance $\approx \sigma$. If this condition is fulfilled, there are no theoretical issues: the second derivative of the potential (appearing in the theoretical prediction) is evaluated only at distance $\approx \sigma$.

In addition, we remark that the first-neighbors approximation is justified here because the second shell of the triangular lattice is formed by 12 particles: 6 of them at distance $\sqrt{3}\sigma$ and the other 6 at 2σ. The first-neighbors approximation works if $|U''(\sigma)| \gg |U''(\sqrt{3}\sigma)|$. This condition is verified in our case since, for $r > \sigma$, the potential scales as $\sim 1/r^6$.

We have reported this explanation in the methods sections.

Below I attach a list of typos and expressions that do not seem to make sense to me. I would suggest the authors consider these to improve the manuscript for future publication (elsewhere, in my opinion).

Our reply: We thank the referee for the careful reading: All the points raised by the referee have been addressed in the new version of the paper.

- 8th line in the section "Mechanism behind self-reverting vorticity" (p.4), "smaller" should be "greater". In the next line, the inequality should be $v_0/\omega > \sigma$. A similar mistake also appears 5 lines below.

  Our reply: We are grateful to the referee for the careful reading and for outlining this typo. We have corrected the condition outlined by the referee in the section "Mechanism behind self-reverting vorticity".

- 4 lines below Eq.(4), the sentence "if r_i rotates with a period smaller..." seems to be erroneous. This should be the condition for the reverting vortex state.

  Our reply: We are grateful to the referee for the careful reading. Indeed, the argument applies when $\mathbf{n}$ rotates with period π/ω that is smaller than the period of $\mathbf{r}$, say T_r. When this condition is fulfilled, i.e.

$\pi/\omega < T_r$, we can observe the self-reverting vorticity (regime of large ω) and, indeed, the total torque $\mathbf{M}$ changes sign periodically because $\mathbf{n}$ does.

In the new version of the paper, we have corrected the sentence.

- last line of p.4, "This scaling law suggests that a larger cluster size favors the self-reverting vorticity state over the permanent vorticity state, with the latter becoming dominant as the cluster size becomes sufficiently large." This sentence does not make sense.

  Our reply: We agree with the referee and we are grateful for the careful reading. Here, we simply mean that, for large cluster sizes, the self-reverting vorticity state dominates over the permanent vorticity state. Therefore, we have simply removed the last sentence *with the latter becoming dominant as the cluster size becomes sufficiently large*.

- Eqs.(7) and (8)b, the factor τ is missing in the chirality term.

  Our reply: We are grateful to the referee for the careful reading. In the new version of the paper, we have replaced the term $\omega \mathbf{n}_i \times \mathbf{z}$ with $\tau\omega \mathbf{n}_i \times \mathbf{z}$.

- It is confusing to use Einstein's convention in some places (e.g. Eq.(9)) but not always (e.g. Eq.(12) contains explicit sum symbols).

  Our reply: We thank the referee for the comment, however, the sum appearing in the first line of Eq.(12) is not a sum over repeating indices but simply a sum over all the pair of particles, i.e. the definition of the total potential $U_{tot} = \sum_{l<m} U(|\mathbf{x}_m - \mathbf{x}_m|)$. This is why we prefer keeping the sum explicit. In a similar spirit, we prefer keeping the sum $\sum^*$ running only on the first neighboring particles.

- I think the last term of Eq.(11) should not have $-1/\tau$.

  Our reply: We thank again the referee for the careful reading. We have removed the extra-factor $-1/\tau$ in the last term of Eq.(11).

- The RHS of the first line of Eq.(12) looks strange, with inconsistent indices k, l, m and undefined function $U(x_l, x_m)$.

  Our reply: We agree with the referee. The index k was a typo, which we have replaced by m to be consistent with the definition of the total potential $U_{tot} = \sum_{l<m} U(|\mathbf{x}_m - \mathbf{x}_m|)$, reported in the main text below Eqs.(1).

- It is misleading to say "τ does not play any role" (p.7, left, in the middle), as the velocity does exhibit rotational diffusion with time scale τ.

  Our reply: We agree with the referee and therefore we have replaced the sentence τ *does not play any role* with the sentence τ *plays a negligible role* which is actually what we intended before.