Response to Anonymous Report 2

Strengths
1) Methodology: Authors use a microscopic model to investigate the interplay between disorder and vortex formation. For strong disorder this is superior to 'conventional' phenomenological approaches.
Considered system sizes are much larger than in previous investigations.

2) Careful and physically sound discussion on the various aspects of their findings.

3) Good introduction into the subject.

Weaknesses
1) At few places it is hard to correlate the discussion with the results shown in the figures.

2) Figure labeling

Report
In this paper authors investigate the interplay of disorder and vortex formation on the basis of an attractive Hubbard model with on-site disorder which is coupled to a magnetic field and solved within a Bogoliubov-de Gennes approach. Different regimes in the field-disorder phase space are identified. These comprise the conventional Abrikosov lattice in the small disorder regime, the transition toward a rectangular lattice at 'intermediate' fields, and the loss of translational invariance at even higher fields. Also the superconducting properties as a function of the field are studied where it is found that up to intermediate disorder strengths the critical magnetic flux is enhanced. Moreover, for large magnetic fluxes disorder can even enhance the average superconducting order parameter.

This is an interesting paper which provides new insight into the actual and complex problem which makes a step forward to understand the influence of disorder on the vortex formation in superconductors. The paper is well written and meets the criteria for publication in SciPost.
I therefore recommend publication of the manuscript in SciPost after the points in "Requested changes" have been considered.

Our response:
We thank the referee for his/her time and effort in reviewing our manuscript and for his/her positive and constructive report. Some of the referee's requested changes have certainly led to a better manuscript. Below is a detailed response to the referee's comments and questions:

Requested changes
1.) According to Abrikosov theory the 'size' of the vortex core is determined by the coherence length. Despite that it is a central quantity in vortex physics the term 'coherence length' appears only once in the caption to Fig. 1. In my opinion it should be straightforward to evaluate the coherence length as a function of disorder (e.g. from the current-current correlations) and then compare with the vortex profile shown in Figs. 7-10.

Our response:
We agree with the referee that the coherence length is a central quantity in vortex physics, that it is straightforward to obtain it from the mentioned correlation function and that, in principle, a

comparison with the vortex profile could be a natural check of our results. It is indeed a meaningful check but only for sufficiently weak disorder. Our results indicate that as disorder increases, the vortex profile is increasing determined by the details of the random potential while the superconducting coherence length is less sensitive to it. For instance, the vortex core becomes asymmetric and it is located in regions where disorder fluctuations heavily suppress the order parameter while the coherence length still reflects global properties of the superconductor. Precisely because in our case there is no in general a direct relation between the two observables we decided not to present explicit comparison.

However, after the referee's comment, and given that many readers may not be familiars with all the details of the physics of disordered superconductors, we feel we should be more explicit about this point in the manuscript. For that purpose, in this update, we have included an explanation why the two quantities are in general different and therefore, the coherence length is not really suitable to characterize the vortex profile.

In order to support this statement, and for the sake of completeness, we present below an explicit comparison. We follow a previous study [PHYSICAL REVIEW B 92, 064512 (2015)] to extract the coherence length ξ_D from fitting the intrinsic superconducting response $\Delta D_s(q_y) = D^{SC}_s(q_y) - D^{M}_s(q_y) = D_s [1-(\xi_D q_y)^2]$, where $D^{SC}_s(q_y)$ is the superconducting component, and $D^{M}_s(q_y)$ is the transverse current response of the normal state. ξ_D is the superconducting coherence length related to the current response. Here, we want first to note that in this calculation we didn't consider the vertex corrections, which can be important in the strong disorder limit around $q_y \sim 0$. We are not sure whether the concave shape in the strong disorder limit is due to absence of vertex corrections, which includes different fluctuation channels. Considering all vertex corrections in such large system is numerically demanding and not realistic with the computational resource we have. However, we expect that even including the vertex correction, the curvature does not change significantly.
The results are illustrated in Figure.R1. ξ_D decreases fast with disorder. In the clean limit, when V=0, ξ_D=14, which is similar to the vortex size r_0 = 12.9. In the weak disorder V=0.5, ξ_D=9.5, which is also consistent with the vortex size r_0 = 10.1. Note that r_0 is the radius of the vortices. However, when the disorder is stronger, **the SC coherence length ξ_D decreases significantly** (ξ_D = 4 when V=1), while **the vortex size decreases slightly**. Fig. 20 in the manuscript shows that r_0=9.35 when V=1.5. In this range of disorder, ξ_D <<r_0. When V>=1.5, the shape becomes concave around $q_y \sim 0$, which cannot be fitted with the parabolic prediction. In Figure.R2, we have depicted $D^{SC}_s(q_y)$ and $D^{M}_s(q_y)$ under various disorder. Both show decreasing curvature with increasing disorder.
In Figure.R3, we tried to just fit $D^{SC}_s(q_y)$ with formula $D^{SC}_s(q_y) = D_s [1-(\xi_D q_y)^2]$ to obtain an approximate ξ_D in the strong disorder limit. The curvature is dominated by $D_s \xi_D^2$, which is smaller for strong disorder V=2.25. We hope that those results provide evidence that the SC coherence length ξ_D <2 when V>=1.5, which is much smaller than the corresponding vortex size.
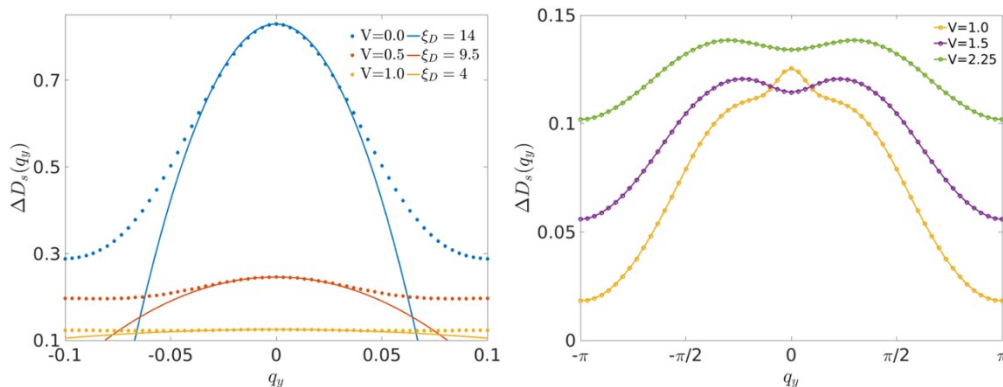
Figure.R1: The intrinsic superconducting current response \Delta D_s(q_y) as a function of disorder. Left panel: Only shows the results for small q_y, which can be fit to obtain the SC coherence length \xi_D. Right panel: \Delta D_s(q_y) with respect to the stronger disorder. The system size is N=60\times 60. The results are averaged over five samples when V>=1, but only one sample when V<=0.5. The other parameters are |U|= 1, <n> = 0.875, the magnetic flux \phi/\phi_0 = 0. The results in Figure.R2 and Figure.R3 are the same configurations.
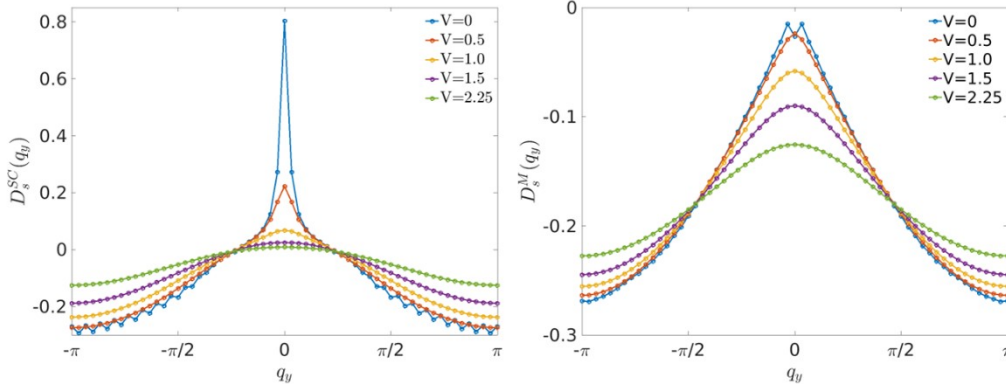

Figure.R2: The transverse current response for the superconducting system D^{SC}_s(q_y) (Left panel) and the normal metal system D^{M}_s(q_y) (Right panel)
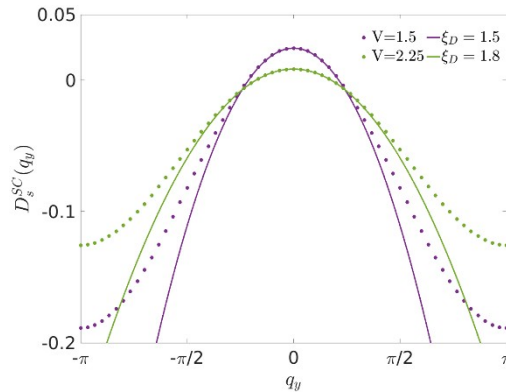

Figure.R3: The transverse current response for the superconducting system D^{SC}_s(q_y) and the corresponding fitted \xi_D.

2.) For the clean system the vortex lattice is only shown for values of the flux up to \phi/\phi_0=18. It would strengthen the discussion when authors would add to Fig. 3 a row with V=0. In fact, Fig. 1 seems to indicate that there is also a transition to a rectangular structure for V=0 whereas on page 14 (2nd row) it is claimed that this structure results from a compromise between disorder and magnetic flux. The question is therefore, whether for the clean case the lattice stays triangular up to high fields.

Our response:
Yes, we agree with the referee. However, in the clean limit, the vortex lattice breaks down quickly. We can see from Fig. 19 in Appendix C in the updated manuscript that superconductivity breaks down when the flux is 14. So, it is not possible to increase the flux much more than we did in Fig.3 in the manuscript. However, following the referee comment, we feel we didn't explain sufficiently well in Fig. 1 that for V=0, there is no such transition from triangular to rectangular. This has been corrected in the updated manuscript.

We cannot rule out that tuning the shape of the sample and the number and size of the vortices that a rectangular lattice is observed. However, it may require some fine tuning so it would not be a generic transition.

By contrast, in the weak disorder limit, even for different system sizes and a wide range of flux number, the transition is always observed, see Fig. 24 and Fig. 25. We think this transition is robust in this case because it results from the combined effect of disorder and the long-range magnetic interactions between vortices.

In the clean limit, since the size is finite and the system is symmetric, only configurations with a certain number of vortices respect the symmetry. Therefore, when there are 8 or 10 vortices in a square lattice, it would be of course impossible to form a perfect Abrikosov vortex lattice. When there are 12 vortices, a compressed Abrikosov lattice is reproduced, see Appendix C in the updated manuscript. That's why we only consider the size N=100x100 in the main text in order to reproduce the Abrikosov lattice. We also add some results with system size N=60xW, where W varies. For our choice of parameters, the Abrikosov lattice is also well reproduced, although in some cases, depending on the value of W, the Abrikosov lattice is compressed or stretched.

3.) Page 12, last paragraph: The quantity \xi_0 is introduced as the vortex separation in the clean limit. I don't understand this definition because the vortex separation should depend on the flux. Does \xi_0=12 refer to the same flux where the rectangular lattice is observed? Please clarify!

Our response:
We agree with the referee that the discussion about \xi_0 was confusing. \xi_0 here means the vortex size, which is also close to the coherence length in the clean or weak disorder limit. The vortex separation is represented by L_v. What we meant is that when the vortex separation L_v is close to \xi_0, which means that vortices start overlap each other, the triangular-rectangular transition happens. We have rewritten the corresponding paragraph and the caption of Fig.5(b) in order to convey this idea more clearly.

4.) page 17, 2nd paragraph: "It is expected that the profile of the order parameter should match with the magnetic field inside the vortex.....". This statement and the following is misleading. The profile of the order parameter is determined by the coherence length whereas the decay of the magnetic field is ruled by the penetration depth and the functional forms of both quantities do not necessarily coincide. Eq. 4 is rather an Ansatz which allows to fit the order parameter profile but I would not relate this to a functional form for the magnetic field.

Our response:
We agree completely with the referee that the profile of the order parameter and the decay of the magnetic field are two completely different things. The Ginzburg-Landau theory predicts that the vortex profile is $\Delta(r)\sim \tanh(r)$ in the absence of disorder. In the updated manuscript, we have changed the theoretical model accordingly and updated the comparison with the numerical results in the updated manuscript. Although the functional form is different, the agreement between the numerical and the theoretical prediction is very good and similar to the one achieved with the previous expression.

5.) page 16: The correct limits for the definition of the superfluid stiffness are \omega=0 and the transverse momentum q->0. At the end of the same paragraph it is stated that (for V=2.25) "the superfluid stiffness becomes zero for a much smaller field strength \phi/\phi_0=16." However, Fig. 13a still reveals a finite stiffness of D_s=10^-3 - 10^-2 for this value of the flux.

Our response:

Yes, we agree with the referee that for V=2.25 there is still a finite stiffness D_s \sim 10^-3 when \phi/\phi_0=16 in Fig.13. However, the method of computing D_s in our case is not very accurate to predict the exact location of the phase transition though a sharp drop by a small change of disorder or flux is a rather precise indication of the critical region close to the transition. Moreover, we didn't consider the quantum phase fluctuations, which further suppresses the stiffness. As a consequence, the very small value of D_s for \phi/\phi_0=16 and V=2.25 likely signals that the superfluid stiffness is already zero.

Another source of uncertainty is that our mean field results are less reliable in this region of strong disorder and relatively large magnetic field. In the updated manuscript, we add some details about the range of applicability of our method of computing D_s in order to illustrate the limitations of the strong disorder (V = 2.25) results which are likely at the transition or already in the insulating region.

6.) Fig. 12: Why the correlation function is not periodic? Does the plotted r-range correspond to half of the lattice size?

Our response:
We thank the referee for raising this issue. Indeed, the correlation function was not properly defined in the manuscript. Since the disorder is not periodic, we didn't consider the periodicity when we calculate the correlation function. When we calculate the correlation function, only the sites with a specific distance r from the chosen site are considered. We then do average over all sites to get the final correlation function. We have provided a precise definition of the correlation function in the updated manuscript so that it is clear now why the correlation function is not periodic.

Minor issues:

a) Eq. 1: Either the hamiltonian is defined for arbitrary hopping parameters, then one should replace -t-> t_{ij}. Or one introduces nearest-neighbor hopping from the beginning. Then this should be indicated in the sum over "i" and "j".

Our response:
We thank the referee for this suggestion. We only consider the nearest- neighbor hopping, so we replaced t_{ij} with t.   In the updated manuscript, we clarified this issue in the very definition Hamiltonian and in the text around it as well.

b) Eq. 3: Replace t_{ij} -> t_{i,i+\delta} and put it under the sum.

Our response:
We thank the referee for spotting this typo. It has been corrected in the updated manuscript.

c) The results in Sec. IV are for 60x60 lattices. This is only specified at the end of Sec. IV but should be already defined at the beginning.

Our response:
We thank the referee for this suggestion. In the updated manuscript we state at the beginning of the Sec. IV that all results are for 60\times60 lattices. Moreover, we explain explicitly the different parameters that we use in the paper and justify the choice of parameters.

Our response:
We agree with the referee. We have improved the figures of the Fourier transform.