
Response to Anonymous Report 3

Strengths
1. Results are sound and interesting, and may open a new pathways for future research
2. Results were obtained in a novel fashion
3. Sufficient details are presented
4. Mostly clear presentation

Weaknesses
1. Some assumptions and parameter values are not stated clearly and should be justified more, especially the uniform filed approximation.
2. One of these assumptions is the form of the order parameter around the vortex under Eq. (4), which is at odds with the well-known Abrikosov vortex solution in the Ginzburg-Landau theory, $\Delta(r) \sim \tanh(r)$.
3. Organizational issues: paper somewhat lacks focus. For example, introduction refers to Appendix figures multiple times.
4. Least important, but grammar and sentence structure could generally be improved throughout the text.

Report
With some necessary changes I believe this manuscript may in principle be acceptable for publication in SciPost. Specifically I believe this research can open a new pathway for further follow-up work. The authors have carried out a numerical study of vortex lattices in a square lattice superconductor in the presence of disorder by solving real-space BdG equations for a previously inaccessible lattice size. One of the main results is an observation of a phase transition from the usual triangular vortex lattice to a square lattice as the magnetic field increases.

Our response:
We thank the referee for his/her time and effort in reviewing our manuscript and for his/her constructive and detailed report. We have tried to implement all the requested changes which we think have improved the manuscript.

There are several issues the authors should address, which I think can significantly improve the manuscript. First, the Hamiltonian studied in this paper, Eq. (1), is the Harper-Hofstadter model with addition of disorder and interactions. Superconductivity has been studied in this model in the absence of disorder in some earlier studies that the authors should cite. More importantly, the model assumes that the uniform magnetic field fully penetrated the superconductor, which is a valid approximation only close to a second order phase transition. This assumption should be made more explicit. Related to this, there is band reconstruction in the Hofstadter model and the electrons form Hofstadter bands separated by gaps. In the regime considered by the authors these are essentially Landau levels. Another important assumption the authors appear to make is that the superconducting gap Δ is much larger than the separation between the Hofstadter bands/Landau levels, since U is taken to be on the order of the full bandwidth at zero field. Is this correct? If so, this should be stated. Reentrant superconductivity has been predicted when this assumption no longer holds (see Rev. Mod. Phys. 64, 709 and references therein), a potential direction for future research that may be relevant to mention.

It may also be worthwhile to note that in the Hofstadter model in the Landau gauge the unit cell is extended into the magnetic unit cell, which for the parameter values used would be of length at least

625 if I am not mistaken. This is much larger than the lattice size used in the numerical calculations, which means that the true Hofstadter regime is likely not being accessed.

Our response:

The referee is right that the true Hofstadter regime cannot be accessed within the range of parameters that we can reach numerically. This is the reason why we do not mention in the manuscript. The minimum required magnetic field would still be far too strong to support superconductivity with our choice of band structure and coupling constants. Moreover, our main interest is the interplay between disorder and magnetic field but disorder is detrimental of the Hofstadter regime because momentum is no longer a good quantum number which would make hard to observe the intricate band pattern typical of this regime.

Having said that, it would be an exciting research problem to explore the robustness of (reentrant) Hofstadter superconductivity to disorder effects in 2d moire lattices where the Landau regime can be reached with comparatively weak magnetic field. However, we feel that is well beyond the scope of the present paper. In order to avoid any confusion, in the updated manuscript, we state that the range of parameter we investigate is far from the Hofstadter regime, and we give a few references on this topic as suggested by the referee, see Ref [66–69]. Moreover, we mention that this would be an interesting topic for future research.

Second, and related to this, in the introduction the authors state that they studied 100x100 lattices, but later a 60x60 lattice is used for some parameter values. This should be mentioned earlier in the paper. Similarly, ranges of other parameter values like U and V should be mentioned earlier, and when different values are used some explanation should be given as to why the particular values were chosen.

Our response:
We thank the referee for raising this issue. In the section II of the updated version of the paper, we explain in more detail the different parameters of the model and its purpose.

Third, the most interesting finding in the paper is probably the phase transition between the triangular and square lattices. It is stated that no transition occurs in the absence of disorder, but numerical data is not presented to support this claim. Could the authors provide this data for completeness?

Our response:
We agree with the referee that it is necessary to provide evidence to support this claim. In the Appendix C of the updated version of the paper, we present this data. An important comment, not specific of our paper, is that a systematic study of vortex lattices at zero disorder reveals that for certain system shapes and magnetic fluxes is hard to observe the expected Abrikosov lattice because the number of resulting vortices is too small or the shape is such that it cannot accommodate the vortices in a triangular fashion. For instance, for twelve vortices, we observed only a clearly compressed triangular lattice. We cannot study a much larger number of vortices because superconductivity breaks down for just fourteen vortices. Therefore, we cannot rule out that, after some fine tuning, we could observe something resembling a square vortex lattice but that would not be generic.

Fourth, concerning the vortex profile study, I am not sure about the validity of the form of $\Delta(r)$ used by the authors as stated below Eq. (4), at least for weak disorder. It is well-known that in the absence of disorder the profile is $\Delta(r) \sim \tanh(r)$ in the Ginzburg-Landau theory, and this seems to fit the numerical data better than some of the fits in Figures 7-9. Also, the parameter Δ_0 is not defined, and I would suggest changing A and B parameters to something like a and b, to avoid confusion with the vector potential and magnetic field. In general, there is an apparent contradiction in introducing Eq. (4) as the magnetic field is assumed to be uniform in the model studied by the authors; this should be explained more clearly. In this section $|U|=1$ is used instead of 1.25 used in the previous section, is there any particular reason for this? Finally, in Fig. 10 some sharp peaks in Δ are seen around r=15 and -20, do the authors have any explanation for their origin?

Our response:
We thank the referee for raising this interesting issue. Indeed, Ginzburg-Landau theory predicts that the vortex profile is $\Delta(r)\sim \tanh(r)$ in the absence of disorder. We have changed our theoretical model and updated the results accordingly. We also changed the notation to avoid confusion. The agreement between numerical and theoretical results is still very good.

The reason to choose a weaker coupling, $|U|=1$ is that in this section we only focus on the spatial profile of a single vortex. The weaker the coupling, the larger the vortex core. A larger vortex size makes easier to study the impact of disorder in the vortex profile which is the main motivation of this section. Since the main goal of the paper is to describe the interplay between disorder and magnetic flux, we aim to reach the weaker coupling possible.

The sharp peaks in Δ vortex profile are only observed in the strongly disordered sample. This is due to the spatial distribution of order parameter is quite inhomogeneous. If there are some significant enhanced superconducting islands around the vortex core, we would observe the sharp peaks at the island location. If we do sample average over many vortices, those sharp peaks would disappear and the vortex profile would be smoother.

Fifth, concerning the study of correlations in Sec. VI, one question that does not appear to be addressed in the correlation between the disorder potential V_i and the gap function distribution. I think the authors should present some data to address this issue. Also, concerning Fig. 13a, it is stated that D_s becomes zero for V=2.25, but this appears at odds with the insert in the figure.

Our response:

Regarding D_s, we agree with the referee that the computed superfluid density is still finite at V=2.25 however its value is already very small and decreases very fast even with small changes in V. Taking into account that we are not considering quantum fluctuations that further weaken phase coherence, we believe that at V = 2.25 the superfluid density is already around the superconductor-insulator transition.

Regarding the correlation between the disorder potential V_i and the gap function distribution, the truth is that we do not fully understand what the referee means. In Fig. 13, and in the main text, we comment on the relation of the gap correlation function and the strength of disorder V. If what the referee means is the correlation between the value of the order parameter and the potential at site i see Fig. R4 below for a comparison. The random potential has a box distribution so it is completely uncorrelated. Therefore, its correlations are unrelated to those of the order parameter.

Sixth, in Fig. 18 and 19 in Appendix C, are (a), (d), (g), (j) and (m) different disorder realizations?

Our response:
Yes, we have clarified it in the caption in the updated manuscript.

Seventh, in terms of organization I wonder if section III should be simply incorporated into section IV. Since figures from appendices are referred to extensively in the introduction, it also seems appropriate to move them into the main text, along with any relevant explanations. Alternatively, they should not be referred to in the introduction.

Our response:
We have moved the results of the clean sample with different aspect ratios from the appendix to section III.

Finally, though this is not a huge issue I think the grammar can be improved significantly in the text. For example, on page 5, paragraph 1: “it has also been identified a range of parameters where...” is incorrect grammar. It should read instead "A range of parameters has been identified where..." I saw multiple errors of this form throughout the text, in addition to other typos. The authors also refer to 'vortices overlap' several times, which should instead read 'vortex overlap.' These should be easy issues to address.

Our response:
We thank the referee for this suggestion. We have carefully proofread the manuscript which hopefully has fixed these problems.

Overall, I think the manuscript is interesting and will meet the criteria for publication once these issues are addressed.

Our response:
We hope to have addressed all the issues raised by the referee and that the paper is therefore suitable for publication.

Requested changes
1. Identify Eq. (1) as the Harper-Hofstadter model and state the assumptions behind it more explicitly, as well as clarify the parameter regime being studied.

Our response:
As argued earlier, our model is effectively very different from the Harper-Hofstadter model. In the range of parameter, we are interested in, both models have completely different properties. Therefore, we think it would be confusing to call it that way. In the updated version of the paper, in order to avoid any misunderstanding, we mention explicitly why our model cannot describe Harper-Hofstadter physics and refer to the relevant literature suggested by the referee.

2. In general, state which ranges of parameters were used and why earlier in the text.

Our response:
We have explained it in section II in the updated manuscript.

3. Present data showing no phase transition between triangular and square lattices occurs in the absence of disorder.

Our response:
We thank the referee for this suggestion. We have presented the data in Appendix C in the updated version.

4. Justify the use of Eq. (4) better, and why it is assumed that $\Delta(r)$ is not linear in r for small r at weak disorder.

Our response:
Following the fourth point in referee's report, we changed the fitting model. We agree that the Ginzburg-Landau theory prediction is more suitable for this purpose so we have fit the vortex profile with it. The agreement between the numerical results and the theoretical prediction is very good in the clean and weak disorder where we expect that the Ginzburg-Landau theory applies. One of the main results of the paper is the observation that this theory fails in the strongly disorder region where the agreement is much worse.

5. Study the correlation between the disorder potential V_i and the gap function distribution, as several claims appear to be made about it without supporting evidence.

Our response:
As mentioned earlier, we do not fully understand this comment. From our limited understanding, since our random potential is uncorrelated and box distributed, we do not think that there is any strong reason to compare it with the correlation function of the order parameter. We think it might distract the readers if we also study the correlation between disorder potential and order parameter. We present the density scatter plot of the order parameter as a function of disorder in Figure R4 below, from V=0.5 to V=2.5. In the weak disorder V=0.5, the role of disorder is limited. However, when V>=1, disorder potentials play an increasingly important role. It is expected that maxima of the random potential suppress superconductivity further. However, the order parameter is enhanced significantly at sites with weaker potentials in those strongly disordered sample.

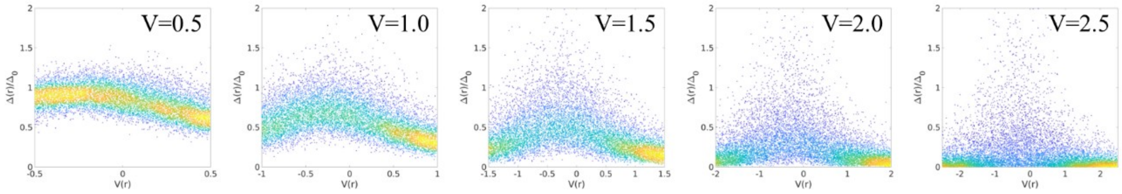

Figure R4: Density scatter plot of the order parameter as a function of disorder (Yellow means higher density). To make the comparison more directly, we only show the data between 0 and 2\Delta_0.

6. Clarify captions in Fig. 18 and 19 in appendix C.

Our response:
We thank the referee for this suggestion. We have improved the captions in the updated version.

7. Move figures from appendix to main text, or change discussion in the introduction. Possibly move section III into section IV as it is too short.

Our response:
We thank the referee for this suggestion. We have moved figures, which reproduce the Abrikosov lattice, from the appendix to section III.

8. Proofread the manuscript carefully and fix typos and grammar issues.

Our response:
We thank the referee for this suggestion. We have proofread the manuscript carefully and fixed typos and grammar issues