Dear Editors,

Thanks for your mail attached with reports on our manuscript (NJP-110230).

We also thank the two referees very much for their helpful comments and suggestions. Following their comments and suggestions, we have made some changes on the text and figures. We sincerely hope that now both referees will find satisfactory in this version and recommend it for publication in “New Journal of Physics”.

The replies to the questions raised by the referees and the main changes in the manuscript following the referees’ suggestions are listed below.

Best wishes,

Zhao-Long Gu, Kai Li, and Jian-Xin Li

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Main changes:

1. **Main text:**
2. In the revised manuscript, we add a new paragraph (the 4th paragraph) of Section 2 to introduce the technique used to calculate the edge states in the framework of CPT/VCA.
3. In the last section of the revised manuscript, more discussions on the NMI phase are made. In particular, we discuss the possibility that the NMI phase arises from the artifact of CPT/VCA method, and also mention other possibilities. Meanwhile, we emphasize that whether or not the NMI phase exists does not affect the main issue we focus.
4. **Figures**
5. In the revised manuscript, in Figs. 2(a), 3(a) and 6(a), the range of t'/t is extended from 0.1 < t'/t < 0.3 to 0< t'/t<0.3.
6. **Reference**
7. Refs. 11, 12, 13, 20, 31, 35, 37, 38, 40, 63, 64, 65, 66 in our revised manuscript are added to cover the first papers and available reviews of this field, as well as the newly discussed topics.

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Response to the referee 1:

We thank the referee very much for his/her valuable comments and suggestions, which is very helpful for us to improve this manuscript. Following is our reply.

(1) Quantum cluster approaches have been actively used in the past years to study correlated topological band insulators (in particular, on the honeycomb lattice); yet, the papers which are cited, are random papers, neither the first papers on this topic nor the most relevant ones – not even the available reviews are mentioned.

We thank the referee for pointing out this. After a careful investigation of the available literature, we have reorganized our references in our revised manuscript.

(2) If the authors had invested the time to study the available literature they would be aware of the following problem: the symmetric six-site cluster (and presumably the same might be true for the two-site cluster?) on the honeycomb lattice produces an erroneous insulating phase ("MNI") for infinitesimal interaction strength.  
Back in 2012-2014 this was motivated by the "discovery" of an intermediate-U spin liquid phase on the honeycomb lattice, which was later falsified by numerically exact quantum Monte Carlo methods (Assaad and Herbut etc.). Yet there are several papers published where the presence of a putative spin liquid phase ("NMI") was found within quantum cluster approaches on the honeycomb lattice.  
The authors fail to address this at all - instead they cite Ref. [17] which is one of the few papers which claims that this (erroneous) NMI phase could be a spin liquid. I think it is very likely that the NMI phase found in the paper under consideration is connected to the erroneous NMI of Ref. [17]. Thus, it might be that the NMI phase is an artifact after all (disagreeing with the authors conclusion).

And here comes the most problematic part of the paper: the authors are only showing results for t'/t > 0.1, thus the region 0 < t'/t < 0.1 which would reveal whether or not the erroneous (previously mentioned) NMI phase is present here as well is not shown.

In the following I'm going to explain briefly why I believe the NMI phase is an artifact. Within real-space quantum cluster approaches, the hoppings within the cluster become renormalized in the presence of interactions. The hoppings between the clusters are, however, the bare hoppings. Even if they are treated variationally, they will be different from the hoppings on the cluster. Now the honeycomb lattice is known to be unstable towards on plaquette anisotropy, this has been discussed first in Laubach et al [Phys Rev B 90, 165136 (2014)], there might be also other papers. Consequently, for any finite interaction strength on the honeycomb lattice quantum cluster approaches will find a gapped phase. Even in the presence of a gapful term (such as the Haldane term) this phase is still present. I suspect that the NMI phase discussed in this paper is the very same phase (an artifact of the quantum cluster approach).

I would suggest that the authors show in their revised version of the manuscript how the phase diagram and the observables behave in the regime t'/t<0.1. That might clarify the situation.

We thank the referee for pointing out the fault of the choice with a symmetric six-site cluster. In fact, we were aware of this before the writing of this paper and one of the authors even participated in the historical debate in the community on whether the “spin liquid” phase exists in the simple honeycomb Hubbard model. Following the referee’s suggestion, we have extended the phase diagram to the regime t'/t<0.1. The NMI phase does connect to the fictitious “spin liquid” phase with t'=0. We agree with the referee that there does exist the possibility that the whole NMI phase is just an artifact of the CPT/VCA method. But we also want to argue that the experiences of the case with t'=0 may not be definitely generalized to that with a finite t'. In view of the effective spin model incorporated with finite-U effects, a nonzero t' term introduces ring-exchange spin interactions and next nearest neighbor spin interactions, which could frustrate magnetic order and stabilize spin liquid phases [See PRB 91, 134414(2015) and PRL 116, 137202 (2016)]. Besides, charge fluctuations are known to be possible to suppress magnetic order and favor nonmagnetic phases in the moderate interacting regime [e.g. See PRL 100, 136402 (2008) and PRL 114, 167201 (2015)]. We have added these discussions in the last section in our revised manuscript.

We want to emphasize that the main issue of this paper is the correction of previous computing scheme of applying quantum cluster approaches to the calculation of interacting Chern numbers in correlated Chern insulators, but not what the true phase diagram of the Haldane Hubbard model is. In fact, as is explained in the last section in our revised manuscript, whether the NMI phases exists is irrelevant to the issue we stress here because the same fault leading to the false interacting Chern number doesn’t affect the calculation of the quantities (i.e. the single particle gap and the antiferromagnetic moment) used to identify the NMI phase at all! Therefore, compared to the question about the existence of the NMI phase raised by the referee, what we want to stress is a different issue. And, as is stated both in the revised and the previous manuscript, the existence of intermediate phases in the Haldane Hubbard model has still been highly debated. We have no intention to settle down this difficult question due to the limited power of CPT/VCA. Considering that the Haldane Hubbard model has sign problem in quantum Monte Carlo method, the settlement of this question may involve other sophisticated numerical analyses, such as large-scale density matrix renormalization group simulations, which is apparently beyond the scope of this paper. However, the reported fault in this paper that can lead to false interacting Chern number is common in most quantum cluster approaches in the study of correlated Chern insulators and is not only limited to the Haldane Hubbard model, but has been long ignored. The key point of this paper is that we reveal this fault for the first time in the community and propose an alternative computing scheme to get a consistent interacting Chern number.

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Response to the referee 2:

We thank the referee very much for his/her valuable comments and suggestions, which is very helpful for us to improve this manuscript. Following is our reply.

(1) It is not clear how the correlated edge states were calculated in Fig. 5. I guess the authors did not really calculate a ribbon with 15\*6 correlated sites but rather used the self-energy of the 6-site cluster to artificially build the ribbon. It should be made clearer how the authors did this calculation.

The referee’s guess is correct. We agree that this technical procedure should be made clearer in the paper. Therefore, we have added a new paragraph (the 4th paragraph) in Sec. 2 in our revised manuscript to introduce it.

(2) Page 12, line 21, “spinless free” is confusing.

Sorry about the confusion. We have changed this phrase to “noninteracting spinless”.