nature portfolio

Peer Review File

Topological flat bands in a family of multilayer graphene moire' lattices

Corresponding Author: Professor Matthew Yankowitz

This file contains all reviewer reports in order by version, followed by all author rebuttals in order by version.

Version 0:

Reviewer comments:

Reviewer #1

(Remarks to the Author)
The authors study the electronic properties of two Bernal-stacked graphene samples consisting of M- and N-layers, respectively, which are twisted with respect to each other by a specific twist angle, so-called tM+N devices. This is done by measuring the longitudinal resistivity as well as the Hall resistance in the presence of a relatively small magnetic field. Moreover, a top and a back gate were used to change the electronic density of the moiré bands as well as to tune a perpendicular displacement field across the sample. The twist angle was mainly chosen such that the first conduction band became flat and correlation effects were to be expected.

For t1+2, t2+2 and t1+3, insulating states mainly emerge at filling factor nu=2; for t2+3, though, gaps also clearly open up for nu=1 and nu=3. In fact, it is shown that the device with M=2 and N=3 exhibits a novel correlated insulating state at nu=0.25 as complete isospin degeneracy breaking for nu<0.2 and two-fold isopspin degeneracy breaking for nu>0.3 is observed via Shubnikov-de Haas oscillations. For t1 + 4, t2 + 4 and t2 + 5, no correlated states were observed.

Finally, the anomalous Hall effect for t1+3 and t2+3 was discussed. For the former sample, a quantized hysteresis loop for Rxy was clearly seen for filling factor nu=3. For the latter one, this was only seen below nu=1, it was also less pronounced and no further discussion was given.

The study contains several new and interesting results for a "new" family of moiré systems. However, the interpretation of the results is sometimes a bit vague and might be even confusing. This is also due to a rather basic theoretical support only based on a basic non-interacting continuum model. The following points should thus be addressed, before acceptance can be considered:

1) According to the non-interacting continuum model, the tM+N devices are mainly characterized by three layers between the two samples as the local density of states is strongly enhanced around the interface. However, the longitudinal resistivity of t1+2/t2+3 and t1+3/t2+4 is quite different, as one can observe an approximate mirror-symmetry between positive and negative displacement field for an odd number of layers which is absent for an even number of layers. Also devices with a larger number of layers do not show correlated states. All this contrasts the universality picture proposed by the authors.

2) For devices other than t2+3, only one twist angle was discussed and the value was inferred from the theoretical modelling. However, if there is universality due to the enhanced density of states around the interface, why is the twist angle not kept constant? Moreover, for devices with more layers, it seems that smaller twist angles are needed. However, smaller twist angles lead to a larger unit cell which might also be the reason why correlated states for t1 + 4, t2 + 4 and t2 + 5 are not seen (since there are simply too many bands which overlap).

3) As mentioned before, the modelling is quite basic and it is not clear why e.g. alpha=0.5 is used and how the results would change for say alpha=0.8. The modelling could also be improved by including e.g. a momentum-dependent tunneling term in H_int. But in principle, tight-binding calculations should be performed as they would give more reliable results and which appear to be straightforward since out-of-plane corrugations should be negligible.

4) A Chern number of C=2 is reported for t1+3 which is an interesting result on its own. However, no calculations are shown which should be included in the SM together with the results of the Chern number of the flat bands for other devices.

Minor remarks:

1) The discussion on the anomalous Hall effect of t2+3 is difficult to follow and
the results are less clear compared to the ones for t1+3. Since there is a separate paper by the same authors it might be more consistent to remove this discussion together with Fig. 4d,e,f.

2) In order to interpret the experimental data in the strongly interacting regime, more detailed calculations are necessary which should be at least on the Hartree-Fock level. Several of the conclusions on the nature of the symmetry-broken correlated insulator states might thus either be misleading or even wrong.

Reviewer #2

(Remarks to the Author)
The authors have investigated twisted M- and N-layer Bernal-stacked graphene devices. They first analyzed the band structures of various multilayer configurations and found that adding graphene sheets above and below the well-studied t1+3 and t2+2 structures does not significantly alter the low-energy band structure. This finding was confirmed by transport measurements that revealed insulating states at the charge neutrality point and full filling of the lowest moiré valence and conduction bands ($\nu = \pm 4$). Moreover, the authors identified multiple similarities in the correlated phase diagrams of the different tM+N structures and revealed symmetry-broken phases and anomalous Hall states. In the t2+3 devices, the authors further discovered a previously unobserved quarter-metal state that is not directly associated with the $\nu = 1$ insulator. Additionally, they identified an unexpected insulating state at $\nu = 0$ near $D = 0$, which shows similarities to the layer antiferromagnetic (LAF) state observed in Bernal bilayer graphene.

Overall, I believe the authors provide a comprehensive overview of the single-particle and correlated states of twisted M- and N-layer Bernal-stacked graphene devices. The paper is well-written and easy to follow. While it is not entirely surprising to observe certain similarities within this family of twisted multilayer graphene structures, the authors' systematic exploration of the different multilayer configurations adds valuable insights to the field. Nevertheless, I have a few questions and remarks that need to be addressed before I can recommend this work for publication in Nature Communications:

1. I highly appreciate the large number of devices included in this work. However, I would appreciate a comprehensive device overview in the main text or at the beginning of the methods section.

2. It is unclear how the authors have chosen the respective twist angles for each layer configuration. Furthermore, the influence of the twist angle should be discussed in more detail.

3. The authors present a summary phase diagram in Fig. 3a. However, the assignment of the different states is not very clear. Could the authors add a figure to the extended data section to clarify these assignments, i.e. by drawing the lines into the R_{xx} and R_{xy} maps? How did the authors determine which features in their phase diagram correspond to van Hove singularities?

4. Correlated metallic states with different isospin degeneracies were identified at finite magnetic field from the spacing between Shubnikov-de Haas (SdH) oscillations or from the Fourier transform of SdH oscillations collected by sweeping the magnetic field at fixed gate voltage. What magnetic field ranges were used for the Fourier transformations of the SdH oscillations? Additionally, is the reduced spacing between SdH oscillations also present at much lower magnetic fields? Could the degeneracy of quantum Hall states be reduced simply due to the high magnetic fields applied to the system, rather than due to spontaneous symmetry breaking of the isospin degeneracies?

5. Do the authors have any insights into why the correlated insulating state arises at D=0? What role does the t2+3 structure play in facilitating this state compared to Bernal bilayer and rhombohedral multilayer graphene?

Further small remarks concerning the figure (captions):

• Could the authors note the magnetic field used to measure Extended Data Fig. 6a in the figure caption?
• Could the authors also specify the twist angle in each figure, as they have done in Fig. 1, but which is missing in subsequent figures?

Reviewer #3

(Remarks to the Author)
In this manuscript, Waters et al. report a new family of moiré graphene structures composed of M- and N-layer Bernal stacked graphene flakes with a small interfacial twist. By performing low-temperature transport measurements and continuous model calculations, they identified several commonalities in both the single-particle and correlated states across this moiré graphene family. Although the t1+2 and t2+2 moiré graphene systems have been thoroughly studied before, this work extends the investigation to t1+3, t2+3, t1+4, and t2+4 moiré graphene systems, highlighting the potential significance

of the t2+3 system. Overall, the study is interesting and there exists novelty in the manuscript. I recommend the publication of this manuscript in Nature Communications, after the authors carefully address the following points:

1. For the insulating state at v = 0 near D = 0 observed in t2+3 devices, can the energy gap of the insulating state be determined from the thermal activation fitting?

2. In Fig. 4c, the authors only show the Landau fan diagram of measured Rxy, and a Chern number of C = -2 is deduced based on the Streda formula. What about Rxx? The local minimum of Rxx or the temperature dependence of Rxx is needed to establish the Chern gap forming in the bulk.

3. The authors show the AH signals for t1+3 and t2+3 devices. What about the AH signals in other combinations, such as t1+4 and t2+4?

4. A minor point: There are two independent papers reporting the conclusive transport evidence of fractional quantum anomalous Hall effects in twisted bilayer MoTe2, one published in Nature and another in PRX. The authors only cite the Nature paper as Ref. 39. The PRX paper (PHYSICAL REVIEW X 13, 031037) should also be cited.

Reviewer #4

(Remarks to the Author)
I co-reviewed this manuscript with one of the reviewers who provided the listed reports. This is part of the Nature Communications initiative to facilitate training in peer review and to provide appropriate recognition for Early Career Researchers who co-review manuscripts.

Version 1:

Reviewer comments:

Reviewer #1

(Remarks to the Author)
We appreciate the author's careful response to the referees' questions and remarks. And we agree that the experimental observations are interesting and timely.

Still, in our opinion, the paper would benefit from including the citations of Chen et al., Nature Physics 17, 374 (2021) and M. He et al., Nature Communications 12, 4727 (2021) along with a discussion on the absence of mirror symmetry and the different nature of the insulators for D>0 and the correlated metals for D<0. In addition, a comment from the authors regarding the evident differences between the 1+2 and 2+3 and the 1+3 and 2+4 structures in Fig.1 and their possible explanation would improve the paper and help avoid incorrect interpretations by the reader.

Apart from that, as a side-remark, the inclusion of more momentum shells and non-local tunneling terms in H_int, see Phys. Rev. B 107, 075408(2023), should be possible without great effort and could improve the theory in future works. In particular, the bandwidth of the state at \nu=0, D=0 could be modified.

There are still a couple of typos in the section 'Topological states in tM + N graphene': the references to Fig.3 should be changed to Fig.4.

Reviewer #2

(Remarks to the Author)
The authors have improved their manuscript and have addressed my comments, as well as those from the other referees, thoroughly. I particularly appreciate the inclusion of a summary of the different devices and the expanded Extended Data Fig. 6, which clarifies the assignment of the different states. As a result, the revised manuscript now appears well-suited for publication in Nature Communications.

Reviewer #3

(Remarks to the Author)
The authors have properly responded to all of my questions. I would recommend the publication of this work in Nature Communications.

Reviewer #4

(Remarks to the Author)
I co-reviewed this manuscript with one of the reviewers who provided the listed reports. This is part of the Nature Communications initiative to facilitate training in peer review and to provide appropriate recognition for Early Career Researchers who co-review manuscripts.EEE

Reviewer Response

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We thank the reviewer for their careful review and detailed summary of our manuscript.

The following points should thus be addressed, before acceptance can be considered:

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The reviewer raises an interesting observation about the possible similarities of correlated transport states upon flipping the sign of the displacement field, and how this potentially connects to the parity of the layer number. Strictly speaking, only t2+2 has this exact behavior owing to the mirror symmetry of the overall lattice structure. All other *M* and *N* combinations in this study break mirror symmetry, and any apparent transport similarities are thus approximate at best. As studied previously (e.g., S. Chen et al., Nature Physics 17, 374 (2021) and M. He et al., Nature Communications 12, 4727 (2021)), the correlated states in t1+2 are actually very different depending on the sign of *D*, despite any superficial similarities between them in a *n*-*D* map.

The reviewer is correct that we do not observe correlated states for structures with six or more total graphene layers. This is probably due to some combination of the smaller energy gaps between the lowest moiré conduction band and the nearby remote bands, a larger dispersion of the moiré conduction band, enhanced screening from the additional graphene layers, and non-uniform potential drops across the many graphene layers (which is not accounted for in our theoretical modeling).

However, we would like to stress that we do not claim any sharp "universality" in our work. Our main message is that many of the t*M*+*N* constructions share common transport features, all of which can be understood using the t1+2 structure as a "building block" for thicker systems. We thus do not agree that the features noted by the reviewer are inconsistent with our claim that these t*M*+*N* structures share a set of obvious similarities. To summarize, there are two distinct ways in which the behaviors of the t*M*+*N* family are related. First, the basic symmetry-breaking properties – including a robust spin-polarized insulator at $\nu = 2$ along with weaker correlated insulators at $\nu = 1$ and 3, each surrounding by regions of half- and quarter-metals – is quite similar for t1+2, t2+2, t1+3, and t2+3 as detailed in Fig. 2. Second, the basic non-interacting physics is also quite similar, with *D*-tunable band insulators arising at the CNP ($\nu = 0$) and full-filling of the lowest moiré bands ($\nu = \pm 4$). Figure 1 shows that this behavior holds for structures at least as thick as 6 layers (t2+4), although the precise details depend on *M*, *N*, and Θ. This is the extent to which we hope to establish the similarities across the t*M*+*N* structures in this study.

2) For devices other than t2+3, only one twist angle was discussed and the value was inferred from the theoretical modelling. However, if there is universality due to the enhanced density of states around the interface, why is the twist angle not kept constant? Moreover, for devices with more layers, it seems that smaller twist angles are needed. However, smaller twist angles lead to a larger unit cell which might also be the reason why correlated states for t1 + 4, t2 + 4 and t2 + 5 are not seen (since there are simply too many bands which overlap).

According to our band structure calculations, the details of the interlayer hopping terms away from the moiré interface for different layer number combinations can slightly shift the "optimal" twist angle for observing correlated states (see Extended Data Fig. 9 and many of the figures in the SI showing band structure calculations). Indeed, we seem to find experimentally that there are slightly different "optimal" twist angles for at least t1+2, t2+2, and t2+3. Fully characterizing this is an experimentally demanding task, requiring the fabrication and study of as many devices as possible. It is quite possible that yet more correlated and topological states will emerge within ranges of twist angles we have not yet covered. Unfortunately, it was impractical to make enough devices to determine the optimal angle for all the new t*M*+*N* structures we studied here. It could be an interesting problem for future work to establish quantitatively how the optimal twist angle depends on *M* and *N*. We have added this discussion to the Methods section.

3) As mentioned before, the modelling is quite basic and it is not clear why e.g. alpha=0.5 is used and how the results would change for say alpha=0.8. The modelling could also be improved by including e.g. a momentum-dependent tunneling term in H_int. But in principle, tight-binding calculations should be performed as they would give more reliable results and which appear to be straightforward since out-of-plane corrugations should be negligible.

Tight binding calculations including all the many thousands of atoms in each moiré unit cell are exceptionally time consuming. The standard approach, even in purely theoretical papers, is to use a simple continuum model (i.e., an extension of the Bistritzer-MacDonald continuum model), which has

been shown to capture the essential features of the band structure at the single-particle level. Within such models, α is a phenomenological parameter capturing the effects of lattice relaxations. Since there is currently no microscopic derivation of this parameter, we chose to use a fixed $\alpha = 0.5$ for illustration. From our experience, the choice of the value of α does not meaningfully influence the salient qualitative features of the bands, but indeed matters at the quantitative level.

We agree with the referee that there is opportunity for improving the modeling, and are aware that the continuum model does not necessarily faithfully capture all details of realistic systems. However, as this is an experimental paper, we use the standard continuum model calculation to establish the overall qualitative trend of the dependence of the moiré bands on twist angle and the displacement field. It is not our intention to develop a completely satisfactory theoretical modeling of the system, which should be devoted to a separate theoretical work.

4) A Chern number of C=2 is reported for t1+3 which is an interesting result on its own. However, no calculations are shown which should be included in the SM together with the results of the Chern number of the flat bands for other devices.

We have added the calculation of the Chern number to the Methods section. We have also included the specific value we calculate for each isolated moiré conduction band shown throughout the manuscript (Fig. 1a-d and Supplementary Information Figs. 6-19).

Minor remarks:

1) The discussion on the anomalous Hall effect of t2+3 is difficult to follow and the results are less clear compared to the ones for t1+3. Since there is a separate paper by the same authors it might be more consistent to remove this discussion together with Fig. 4d,e,f.

We agree that the properties of the AHE are more complicated in t2+3 than in the other t*M*+*N* constructions where it is seen. However, we feel this is an important aspect of our results. The measurements are clear, even if the origin of this behavior is not perfectly understood. Our other paper is primarily focused on the behavior of the Chern insulator at $\nu = ¼$, and only discusses the AHE insofar as it is needed to understand the behavior of that state. We thus prefer to leave these results in our manuscript, as we feel they provide valuable information about the ways in which t2+3 appears to differ in its correlated states from all other t*M*+*N* systems. We have also made efforts to clarify our discussion in the revised manuscript.

2) In order to interpret the experimental data in the strongly interacting regime, more detailed calculations are necessary which should be at least on the Hartree-Fock level. Several of the conclusions on the nature of the symmetry-broken correlated insulator states might thus either be misleading or even wrong.

We would like to stress that our analysis of the symmetry-broken states comes purely from interpreting the experimental observations. We do not make any attempt in our work to compare with Hartree-Fock calculations. This is a considered choice that we feel is a positive aspect of the manuscript. As an example, even after years of study the nature of the symmetry breaking at $\nu = 1$ and 3 in t2+2 is not totally settled. Hartree-Fock predicts that intervalley coherent states compete closely with spin-valley polarized states, but is unable to reliably determine the ground state. The gold standard is therefore performing experiments and carefully interpreting the results. Although we could in principle perform

HF calculations for all our t*M+N* systems, we feel that doing so would lead to a higher risk of reporting misleading/wrong results.

Overall, we have done our best to carefully study the correlated states we see and infer the underlying ground state from experimental observations. We strongly prefer to let the results speak for themselves, rather than try to bias with potentially misleading predictions from HF. We hope our work will attract more careful theoretical attention to these systems in the future.

Referee #2:

The authors have investigated twisted M- and N-layer Bernal-stacked graphene devices. They first analyzed the band structures of various multilayer configurations and found that adding graphene sheets above and below the well-studied t1+3 and t2+2 structures does not significantly alter the low-energy band structure. This finding was confirmed by transport measurements that revealed insulating states at the charge neutrality point and full filling of the lowest moiré valence and conduction bands (ν = ±4). Moreover, the authors identified multiple similarities in the correlated phase diagrams of the different tM+N structures and revealed symmetry-broken phases and anomalous Hall states. In the t2+3 devices, the authors further discovered a previously unobserved quarter-metal state that is not directly associated with the ν = 1 insulator. Additionally, they identified an unexpected insulating state at ν = 0 near D = 0, which shows similarities to the layer antiferromagnetic (LAF) state observed in Bernal bilayer graphene.

Overall, I believe the authors provide a comprehensive overview of the single-particle and correlated states of twisted M- and N-layer Bernal-stacked graphene devices. The paper is well-written and easy to follow. While it is not entirely surprising to observe certain similarities within this family of twisted multilayer graphene structures, the authors' systematic exploration of the different multilayer configurations adds valuable insights to the field. Nevertheless, I have a few questions and remarks that need to be addressed before I can recommend this work for publication in Nature Communications:

We thank the reviewer for their careful review and detailed summary of our manuscript

1. I highly appreciate the large number of devices included in this work. However, I would appreciate a comprehensive device overview in the main text or at the beginning of the methods section.

We thank the reviewer for the very helpful suggestion. We have added a table to the Methods section summarizing the basic properties of all the devices we studied in this work.

2. It is unclear how the authors have chosen the respective twist angles for each layer configuration. Furthermore, the influence of the twist angle should be discussed in more detail.

We were guided by our single-particle band structure calculations to pick a target twist angle for each t*M+N* configuration. Since we often miss the intended twist angle in making the devices due to layers slipping slightly atop each other, the actual twist angles we study are essentially random fluctuations around these targets. As we discuss in more detail in our response to Referee #1, Comment 2, it seems that more experiments will be needed to understand anything meaningful about the twist angle dependence of each configuration (especially since the theory is not yet reliable enough to make completely correct predictions without better guidance). We have added this discussion to the Methods section.

3. The authors present a summary phase diagram in Fig. 3a. However, the assignment of the different states is not very clear. Could the authors add a figure to the extended data section to clarify these assignments, i.e. by drawing the lines into the R_xx and R_xy maps? How did the authors determine which features in their phase diagram correspond to van Hove singularities?

We thank the reviewer for the helpful suggestion. Although we showed many such details in Extended Data Fig. 6 of our initial submission, we have expanded ED Fig. 6 in our present submission to include additional analysis. In particular, the longitudinal and Hall resistance maps are shown in **a** and **b**. Features used to extract the approximate boundaries in the summary phase diagram (Fig. 3a) are indicated by solid and dashed lines. We associate van Hove singularities with contours of metallic R_{xx} and $R_{xy} = 0$, bounded by regions of R_{xy} with opposite signs. "R_{xy} jumps" indicate features where R_{xy} deviates from the expected linear dependence on v but does not change sign, typically indicating an abrupt change in the spectral weight of the system because of interaction-inducing band renormalization.

4. Correlated metallic states with different isospin degeneracies were identified at finite magnetic field from the spacing between Shubnikov-de Haas (SdH) oscillations or from the Fourier transform of SdH oscillations collected by sweeping the magnetic field at fixed gate voltage. What magnetic field ranges were used for the Fourier transformations of the SdH oscillations? Additionally, is the reduced spacing between SdH oscillations also present at much lower magnetic fields? Could the degeneracy of quantum Hall states be reduced simply due to the high magnetic fields applied to the system, rather than due to spontaneous symmetry breaking of the isospin degeneracies?

We show a few representative Landau fans used to perform the fermiology analysis in Extended Data Fig. 6. We restrict our attention to $B = 0 – 1$ T, the same range used in prior fermiology analyses of crystalline multilayer graphene. Although it is possible for the magnetic field to lift isospin degeneracies, at such small field values the dominant frequency in the FFT analysis reflects the geometry of the $B = 0$ Fermi surface.

5. Do the authors have any insights into why the correlated insulating state arises at D=0? What role does the t2+3 structure play in facilitating this state compared to Bernal bilayer and rhombohedral multilayer graphene?

The reviewer raises excellent questions. Unfortunately, we do not yet know the answers. The $D = 0$ insulator is unambiguously a robust feature of t2+3, seen across multiple devices over a range of twist angles of at least three tenths of a degree. We feel that including any additional claims at this stage would be premature and potentially incorrect, and prefer to leave this result as an intriguing observation for future studies.

Further small remarks concerning the figure (captions):

• Could the authors note the magnetic field used to measure Extended Data Fig. 6a in the figure caption?
• Could the authors also specify the twist angle in each figure, as they have done in Fig. 1, but which is missing in subsequent figures?

We have updated the figures and captions appropriately.

Referee #3:
In this manuscript, Waters et al. report a new family of moiré graphene structures composed of M- and N-layer Bernal stacked graphene flakes with a small interfacial twist. By performing low-temperature transport measurements and continuous model calculations, they identified several commonalities in both the single-particle and correlated states across this moiré graphene family. Although the t1+2 and t2+2 moiré graphene systems have been thoroughly studied before, this work extends the investigation to t1+3, t2+3, t1+4, and t2+4 moiré graphene systems, highlighting the potential significance of the t2+3 system. Overall, the study is interesting and there exists novelty in the manuscript. I recommend the publication of this manuscript in Nature Communications, after the authors carefully address the following points:

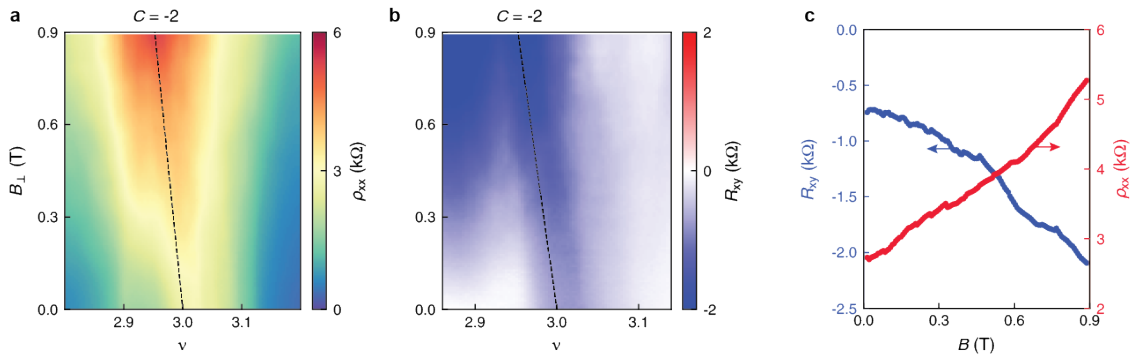
We thank the reviewer for their careful review and publication recommendation.

1. For the insulating state at v = 0 near D = 0 observed in t2+3 devices, can the energy gap of the insulating state be determined from the thermal activation fitting?

We have included the extracted energy gap and included it in the manuscript. However, we note that we only have complete thermal activation measurements for the 1.41° device, corresponding to the data now shown in Supplementary Information Fig. 5.

2. In Fig. 4c, the authors only show the Landau fan diagram of measured Rxy, and a Chern number of C = -2 is deduced based on the Streda formula. What about Rxx? The local minimum of Rxx or the temperature dependence of Rxx is needed to establish the Chern gap forming in the bulk.

There is no clear local minimum in R_{xx} in our measurements, which is shown in below (a). This almost certainly reflects the disordered nature of the Chern insulator at $\boldsymbol{\nu} = 3$, consistent with the fact that R_{xy} is very far from its anticipated quantized value of $h/(2e^2)$ (**c**, which show line cuts along the dashed trajectory in **a** and **b**). Such disorder is, unfortunately, quite common amongst moiré Chern insulators. Local magnetometry measurements, such as Grover et al., Nature Physics 18, 885 (2022), detail the origins of this effect arising from the "Chern mosaics" – mesoscopic domains of contrasting orbital magnetization – formed in samples prone to twist-angle disorder. Although there is no local minimum in R_{xx}, we feel the clear anomalous Hall hysteresis loop coupled with the dispersion of the R_{xy} maximum with Streda slope consistent with $C = -2$ is compelling evidence of an incipient Chern insulator.



3. The authors show the AH signals for t1+3 and t2+3 devices. What about the AH signals in other combinations, such as t1+4 and t2+4?

We do not observe clear signatures of isospin symmetry breaking at $B = 0$ in t1+4 and t2+4, and do not see the AHE in those samples. The AHE requires spontaneous valley polarization to break time-reversal symmetry, so its absence is not surprising.

4. A minor point: There are two independent papers reporting the conclusive transport evidence of fractional quantum anomalous Hall effects in twisted bilayer MoTe2, one published in Nature and another in PRX. The authors only cite the Nature paper as Ref. 39. The PRX paper (PHYSICAL REVIEW X 13, 031037) should also be cited.

We thank the reviewer for catching this oversight. We have included the PRX reference.

Reviewer Response

Referee #1:
We appreciate the author's careful response to the referees' questions and remarks. And we agree that the experimental observations are interesting and timely.

We thank the reviewer for positive assessment of our work.

Still, in our opinion, the paper would benefit from including the citations of Chen et al., Nature Physics 17, 374 (2021) and M. He et al., Nature Communications 12, 4727 (2021) along with a discussion on the absence of mirror symmetry and the different nature of the insulators for D>0 and the correlated metals for D<0. In addition, a comment from the authors regarding the evident differences between the 1+2 and 2+3 and the 1+3 and 2+4 structures in Fig.1 and their possible explanation would improve the paper and help avoid incorrect interpretations by the reader.

We have added brief discussions of these points to the main text and to a new section in the Methods ("Symmetries of t*M*+*N* structures").

Apart from that, as a side-remark, the inclusion of more momentum shells and non-local tunneling terms in H_int, see Phys. Rev. B 107, 075408(2023), should be possible without great effort and could improve the theory in future works. In particular, the bandwidth of the state at \nu=0, D=0 could be modified.

We thank the reviewer for the helpful suggestion and will take this into consideration going forward.

There are still a couple of typos in the section 'Topological states in tM + N graphene': the references to Fig.3 should be changed to Fig.4.

We have fixed these typos.

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