Referee reply: Meson formation in mixed-dimensional *t-J* models

Dear Editors,

we would like to resubmit our paper "Meson formation in mixed-dimensional *t-J* models" to SciPost. We are grateful for the referees careful assessments, and after taking into account their recommendations and criticism, we believe that our work is now ready for publication. A detailed point-by-point response to the referee reports is provided below. We would like to emphasize that the specific changes requested by both referees were very limited, and given the duration of the first round of the refereeing process we would like to ask for a timely decision on the suitability of our manuscript for publication in SciPost.

Yours sincerely,

Fabian Grusdt
Zheng Zhu
Tao Shi
Eugene Demler

<u>Report 1, 2018-9-28</u>

First of all, we would like to thank the referee for his/her careful reading of our manuscript and the generally positive assessment of our work.

***Referee**: 1. The authors discuss a model which may be useful for exploring the physics of spin-charge separation in 1D in presence of coupling to other degrees of freedom.*

Response: The referee's assessment of our work differs from our own perspective. The main focus of our paper is to study systems without spin-charge separation and beyond 1D. While the motion of the dopants is restricted to one direction, the system should be considered as truly 2D because the couplings of the spins extend in all directions. Moreover, as we demonstrate in our work, the motion of the charges couples to the spins, and hence the charges are also effectively coupled in both directions. The case when the couplings of the spins perpendicular to the motion of the charges is zero is special: Here the system is truly 1D. However, this is just one single point in parameter space.

***Referee**: 2. A variational wave-function suggested in the paper seems to provide a good qualitative (and even reasonable quantitative description) of spin-charge correlators.*

Response: We agree and thank the referee for this positive assessment.

Referee*: 3. Results could be relevant to cold-atom experiments*

Response: We certainly believe that this is the case. In fact, we are already talking to experimentalists working with ultracold fermions in optical lattices about possible experimental realizations of the proposed setup, which is absolutely possible with current technology.

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Referee*: 1. The content of the paper in terms of new physical results seems to be rather confined to the identification of the meson size from the spin-charge correlators, and to some results on dimensional crossover.*

Response: We agree with the referee, that our work **enables the determination of the meson size in our system**. The interplay of spin and charge in the doped Hubbard model is a long-standing problem, and as we demonstrate in our work, the microscopic perspective afforded by quantum gas microscopy provides an entirely new perspective: In our view, the mere fact that we can identify a meson-like structure forming in this system, is an important result. In the context of cuprate compounds there is an ongoing discussion whether the pseudogap phase can be understood from microscopic constituents which are bound states of spinons and holons / chargons, see [Beran et al., Nuc. Phys. B, 473, 707 (1996)] and [Punk et al., PNAS 112, 9552 (2015)] and [G. Baskaran, arXiv:0709.0902]. The microscopic structure of such constituents — if they exist — remained poorly understood so far. We believe that the meson-like constituents we identify in our work, even if limited to a toy model so far, qualify as candidates for the spinon-chargon bound states discussed in the context of the pseudogap before. Hence our results provide a significant step forward towards a more complete microscopic understanding of the doped Hubbard model.

We would also like to emphasize our highly non-trivial result in Fig. 4 of our paper: There we introduce an entirely new way of analyzing doped quantum magnets, by studying the full counting statistics of certain string patterns. This method, first proposed in our arXiv pre-print in June this year, has motivated a recent experimental analysis of the doped 2D Fermi-Hubbard model, see [Chiu et al., arXiv:1810.03584], and goes beyond the capability of traditional solid state experiments. Beyond the introduction of a new method for analyzing doped quantum magnets in general, we would like to emphasize the remarkable quantitative agreement we obtain in Fig. 4 (b) between full numerical simulations and a highly simplified semi-analytical description of the mixD t-J model. Such level of quantitative agreement indicates how accurate our description of meson-like bound states in the mix-D t-J model is.

We have now added a reference to the recent paper [Chiu et al., arXiv:1810.03584] in the discussion at the end of our manuscript.

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Referee*: 2. The authors address a large number of different points. However, it would have been good to see a more systematic analysis of the results, as well as limitations of their analytical variational approach.*

Response: We are confused about the referee's conflicting statements: In the last point she/he criticized that our results are limited in their extend. Here the referee starts off criticizing that our paper has too much content, before criticizing again that we should include even more systematic results concerning the variational wavefunction and its limitations.

When composing our manuscript, we have carefully weighed which of our results should be combined into this paper. Since we would like to address the cold atom community and the experimentalists who can actually perform the proposed measurements, we have decided against including the full systematic analysis of our variational wavefunction, although we did include sufficient information in the Methods section for an interested reader to be able to reproduce our results. We have performed a more systematic and in-depth analysis of our variational wavefunction and its limitations, including a calculation of the meson dispersion and of the variational energy as a function of *t/J*. However, a discussion of these results — which, moreover, are beyond todays experimental capabilities in ultracold atoms — would go far beyond the scope of our current manuscript.

As the referee noted himself/herself, we already discuss an extensive number of points in our paper. Therefore, we will publish our results concerning the variational wavefunction in a forthcoming work, which we are currently finalizing. In our view, establishing the existence of meson-like constituents in a doped quantum magnet is a significant result, and doing so requires careful analysis of different signatures in various experimentally accessible quantities and including limiting regimes, such as the well-known 1D system: This is what we provided in the present manuscript.

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***Referee**: The authors of the paper study the mechanism of spin-charge separation in a one-dimensional t-J model doped with one hole, that is coupled via exchange interactions to a 2D lattice of spins, the latter described by a Heisenberg model. This paper is an extension of some of the authors' earlier work related to cold-atom experiments measuring hidden antiferromagnetic correlations in a doped 1D Hubbard chain, Ref. [36].*

Response:

- As eluded to above, our main objective is not to study the mechanism of spin-charge separation: Mesons are formed in a regime where spinons and holons are not separated, but instead they are bound to one another.
- We think that the referees description of our mixD *t-J* model as just "a one-dimensional *t-J* model" is inaccurate: As also eluded to above, the couplings of the spins extend in both directions, and this is very important for the physics of the doped model: In stark contrast to the 1D case with a single hole (see e.g. [Bohrdt et al., PRB *97*, 12511 (2018)]) we find no fractionalization and the the holon is bound to the spinon to form a meson-like bound state with a finite size: We demonstrate this explicitly by studying the dimensional cross-over in Fig. 5 of our manuscript.
- We believe that our manuscript should be considered as more than just "an extension of some of the authors' earlier work related to cold-atom experiments measuring hidden antiferromagnetic correlations in a doped 1D Hubbard chain, Ref. [36]." As explained above, our work is NOT about a 1D model, and the existence of a very finite length scale on which hidden anti-ferromagnetic correlations can be observed, extending no further than very few lattice sites, clearly demonstrates that the nature of charge carriers has changed drastically. Indeed, in Ref. [36] a true 1D model was studied, where the sign flip which we observe in Fig. 2 a) and b) on a finite scale extends all the way across the entire system. Physically, the

situation with free fractionalized holons could not be more different from our present case where spinons and holons form bound states, which drastically modifies the properties of charge carriers in the system.

We would also like to draw the referees attention once more to our Fig. 4: There we analyze short-range hidden order by studying string patterns revealed in individual snapshots of the wavefunction. Such measurements were not performed in Ref. [36], but instead our prediction in this work has already lead to the recent identification of similar string patterns in the doped 2D Fermi-Hubbard model, see [Chiu et al., arXiv:1810.03584].

Of course the referee is correct that several of the observables which we use in our present manuscript, are identical to the observables considered in Ref. [36]: But this is due to our effort to make our work (i) easily accessible and (ii) realistic to implement experimentally. Indeed, the work in Ref. [36] has demonstrated that the observables we consider here can be measured in ultracold atom experiments, even though they remain inaccessible in traditional solid state experiments.

Finally, we would like to emphasize that, at the end of our work, we also discuss precursors of stripe formation in mixD. This relates our results to a ubiquitous phenomenon observed at finite doping in the 2D doped Fermi-Hubbard model, well beyond the scope of Ref. [36].

***Referee**:*

The authors present the result of ED, and DMRG calculations for spin-charge correlation functions, and compare these with the results obtained using variational Monte-Carlo based on a trial wave-function describing a bound state of a spinon and a holon. They find a quantitatively reasonable agreement between these approaches. In addition, the authors study dimensional crossover, and stripe formation using similar approaches, as well as suggest how to identify the bound states in cold atom experiments.

The model suggested in this paper is interesting as it allows one to study how the coupling to spin-background affects the physics of spin-charge separation. Specifically, the authors identify the size of the meson from the spin-charge correlation function calculated using ED and DMRG, and found how meson size scales as the function of inter-channel Heisenberg coupling.

From this perspective, and from its potential relevance to cold-atom experiments, I think this paper is suitable for publication in SciPost, perhaps after some modifications, see questions and comments below.

Response: We would like to thank the referee for this positive assessment, with which we fully agree.

***Referee**: 1. On page 2 the authors say "… we show that spinons and holons are confined and form bound states". This statement does not seem to be fully justified or explained.*

Response: This sentence is part of the introduction, where we describe and summarize in a few words our main results. Naturally, we cannot provide a detailed explanation or justification at

this point. We believe that it is important to use easily accessible language, rather than providing precise mathematical definitions at this stage of the manuscript.
We do not fully understand the referees question, since the referee confirmed in a previous comment that he/she believes that we identify the size of the meson. The finite meson size is a direct reflection of the fact that spinons and holons form bound states.

***Referee**: 2. It would have been useful to see results on the spin-correlators not only along the chain where the hole can hop, but also for the spins in the nearby chains, to see how the hole modifies the spin background.*

Response: We agree with the referee that this is another interesting observable. In our view it does not reveal additional physical insights and for the sake of clarity and compactness we did not include a discussion of these effects. As the referee pointed out himself/herself, the paper is already extensive, and we decided against the addition of such analysis. We do not know if the referee has any specific effect in mind that she/he is expecting to see from such data.
Moreover, we would like to point out that string correlations between the chains do play a role in our protocol for revealing the string patterns in our individual snapshots, see Fig. 4 and Methods. So in this sense, the effect on inter-chain spin correlations is addressed in our present manuscript.

***Referee**: 3. It would be good to see some tests of the FSA approximation, and specifically a discussion of applicability of the basis states discussed on page 2. Note, that these states are not the ground states of the Heisenberg Hamiltonian with one site removed and will have dynamics.*

Response: This test of the FSA approximation is an important part of our results: It is presented in Fig. 4 b). The prediction by the FSA approximation [red dotted line in Fig. 4 b)] agrees remarkably well with exact numerical results [solid blue line in Fig. 4 b)]. The referee is correct that the new basis states are no eigenstates or ground states of the pure Heisenberg Hamiltonian. But, importantly, we consider a doped system, and the kinetic energy t exceeds the Heisenberg couplings J; This leads to the creation of a highly excited spin state, relative to the ground state of the Heisenberg model, and as we demonstrate in Fig. 4 b) these excitations capture extremely well the quantum state of the mobile hole.
Of course we would agree with the referee that there are many measures to determine how well the FSA approximation works. The most rigorous may be the calculation of wavefunction overlaps, but it is also well known that overlaps as measures of how accurate a trial wavefunction is can be misleading in a quantum many-body problem with an exponentially large Hilbert space. Hence, we restricted our test of the FSA to the study of various multi-point spin-charge and spin-spin-charge correlation functions, as well as the full counting statistics of the string length in Fig. 4 which goes beyond simple quantum mechanical averages.
To address this comment by the referee we have added a sentence in our manuscript on page 2: "We will confirm below, in Fig. 4, that the FSA is a reliable approximation."

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***Referee**: 4. While the variational wave-function presented in page 2 seems to be reasonable for a 2D system, the results suggest that it also provides a good description even for 3-leg ladders. This is unexpected, and perhaps requires some explanations.*

Response: We thank the referee for this interesting comment. In our view, the 3-leg ladder with a single dopant moving only on the central leg is already fairly close to a full 2D situation. We checked that the local spin-spin correlations of the undoped three-leg ladder (NN, diagonal NNN and straight NNN) are already very close to the local spin-spin correlations expected in the ground state of the full 2D Heisenberg model. As also mentioned in the our manuscript, the three-leg ladder has no long-range order — in contrast to the ground state of the 2D Heisenberg model — and we assume that this is the reason why the referee is surprised about the accuracy of our variational wavefunction even for the three-leg ladder.

We believe that this has to do with the local spin-spin correlations, which are fairly similar to the local correlations in the bulk of a 2D system: When the mobile hole moves through the system, it distorts the spins and, from the point of view of the spin system, creates a highly excited state. Within the FSA approximation, the (average) energy of this excited state is determine by the local spin-spin correlations, which have already converged to values close to their analogues in a true 2D system. Because of the fast fluctuations of the holon, the excited state is coherently de-excited before it has a chance to decay into an incoherent mixture of elementary spin excitations. I.e. the highly excited states are only virtually populated for a short time, and hence the nature of low-energy spin excitations — which is very different indeed for a three-leg ladder and a 2D system — does not play an important role.

Nevertheless, we would not want to claim that all properties of the meson in the three-leg ladder are as well converged to their full 2D values. For example, the quasiparticle weight of the single hole — which can include dressing with low-energy spin excitations in the system — could be very different in a three-leg ladder than in a full 2D system. Calculating such quantities requires more work, so we do not yet have these results available yet, but it is most definitely an interesting question to address in the future.

Finally, the precursors of stripe formation which we observe in small ladders also highlight that some properties of ladders are clearly distinct from full 2D systems, where the formation of stripe-like patterns for doping by a single hole is impossible because it would cost an extensive amount of energy.

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***Referee**: 5. On page 2 the authors cite the values for staggered magnetic flux and staggered field. Do the authors use these same values for all geometries, including finite-size ladders?*

Response: In Fig. 2 b) we used those values. We have now indicated this explicitly in the figure caption. As described in the last part of the Methods, we have used an optimized value for the staggered magnetic field at \Phi=0.5 π in the dimensional cross-over, Fig. 5. We have included an explicit reference to the Methods section now. We have also added an explanation in the caption of Fig. 9 in the Methods. Finally we note that we have not performed any calculations with our variational wavefunction for a 3-leg ladder. The system sizes used in the trial wavefunction are always indicated explicitly in the figure captions.

***Referee**: 6. For consistency it would have been good to see a comparison with 1D results and for a e.g. a 2-leg ladder, for example in Fig.3.*

Response: 1D results for similar observables as considered here, e.g. Fig. 2, are discussed in Ref. [36], as the referee has pointed out before. In Fig. 3 a) for a 1D case the result depends on the number of spins because this controls the number of spinon excitations in the system. Without any spinon excitations, S^z —> - S^z symmetry shows that $C_{SH}^z = 0$. With one spinon excitation, the result depends on the shape of the spinon wavefunction in the system. To avoid such subtleties, which would need to be discussed, and keep the paper focused on our main message, we decided agains including a comparison to 1D in Fig. 3. Fig. 4 is based on 2D correlators and cannot be generalized to 1D or 2-leg ladders in a straightforward manner. Fig. 5 already includes 1D as a limiting case.

In general, we believe that the physics in a 2-leg ladder is quite different from the physics in a 3-leg ladder. For example, the string tension is smaller because the hole motion only frustrates spins along one direction. Moreover, the formation of a valence-bond solid is favored in this case even at zero doping. While we agree with the referee that this is a very interesting direction, we believe it goes beyond the scope of our present manuscript.

In summary, we believe that our comparison with a three-leg ladder, where the main spatial symmetries of the full 2D model are preserved, is the smartest choice to lear about the full mixD model while keeping the DMRG calculations efficient.

***Referee**: 7. In Fig.5 it would be good to see the same correlators, and the same system sizes when comparing DMRG and variational MC results. Why does the discrepancy in the ground state energy grows with J_y/J_x?*

Response: We agree with the referee that it would be nice to have a direct comparison of identical system sizes. However, there are some technical limitations which would mean that producing such plots would require significantly more effort: The trial wavefunction is always constructed in a system with periodic boundary conditions in all directions. In principle we could generalize our method to open boundaries, but then momentum is no longer conserved and some quite serious modifications need to be added to our theoretical analysis. On the other hand, simulating periodic boundary conditions by DMRG is significantly more complicated. The most realistic choice would be to use a 4-leg ladder with periodic boundary conditions along the short direction only. This would be possible, but it would require a significant amount of additional numerical simulations. Here we have decided to present results for the three-leg ladder, which is experimentally realistic in its own right (in contrast to the 4-leg ladder with periodic boundaries), and compare to the trial wavefunction in this regime. Of course we agree with the referee that this means that a direct comparison of the energies has to be taken with a grain of salt.

The fact the observed discrepancy of the energies grows with J_y / J_x is easily understood by considering the two limiting cases: In the 1D limit, when $J_y =0$, the trial wavefunction is essentially exact: The holon is free and it is well known that the Gutzwiller projected MF state of spinons is an extremely accurate trial wavefunction for the 1D spin chain. This explains why the energies match when $J_y =0$. On the other hand, when J_y is sizable, we do not expect the trial wavefunction to work perfectly. For example, there is expected to be some back-action: When the string forms, the surrounding spins will adjust to the modified configuration. Moreover, the spinon at the opposite end of the string than the holon can move along y-direction if J_y is non-zero. This will lead to non-straight strings which have not been included in our trial wavefunction so far. We expect both effects to lead to deviations on the order of J_y, consistent with our observations. These effects are also discussed in our manuscript already: On page 6 we wrote: *"We expect that the dominant factors contributing to this discrepancy are (i) the use of only straight strings along x in $\ket{\Psi_{\rm MP}}$ and (ii) our neglect of spin-hole correlations in squeezed space. Both effects should lead to corrections of order J_y."*

***Referee**: 8. The paper contains some jargon e.g. "geometric strings", "squeezed space", etc. which is not explained clearly. This jargon can also be avoided.*

Response: We have ensured that the terms mentioned by the referee are properly defined now: On page 2 we wrote: "*In analogy with 1D, see Refs.~\cite{Ogata1990,Kruis2004a}, we call the space defined by the spins on these lattice sites $\tilde{\vec{j}} \neq (j_x^s,0)$ squeezed space.*" The lattice sites *$\tilde{\vec{j}} \neq (j_x^s,0)$* are clearly defined above this sentence and we do not understand why the referee thinks this term has not been clearly defined. Also on page 2 we have now clarified what we mean by geometric strings: "In the approximate set of basis states constructed so far this corresponds to a displacement of all spins along Σ, referred to as the geometric string, connecting j_x and j_x^s." This is where the term "geometric string" is used for the first time in this manuscript. We use this notation (i) to emphasize that the lattice geometric has been changed along this string, where the spins are displaced by one lattice site, irrespective of their quantum state; and (ii) to have a consistent notation as in [Grusdt et al., PRX 8, 011046 (2018)] and [Chiu et al., arXiv:1810.03584].

***Referee**: 9. In Fig. 7 the correlators do not seem to converge as a function of the magnetic field, why?*

Response: We believe that the correlators do in fact converge as a function of B. Note the scale used in our legend: B=0, 0.05 J, 0.1 J, 0.2 J and finally 1J. Firstly, we increase B in a logarithmic scale; Second, the last step corresponds to a factor of 5 rather than 2 as before, while the difference of the resulting correlators is even smaller.

***Referee**: 10. In general, the paper could be shortened, the main statements sharpened and moved to the top of the paper.*

Response: In the introduction, at the top of the paper, we do summarize the main statements of our paper: (i) We propose to realize experimentally a mixed dimensional toy model of strongly correlated matter; (ii) We study this toy model at low dopings, and reveal the physical nature of individual charge carriers: They can be understood as meson-like bound states of spinons and holons; (iii) The connection of these partons to the fractionalized excitations of 1D chains are revealed by studying a dimensional crossover; (iv) We introduce methods to directly "observe and characterize" these meson-like bound states, based on existing experimental capabilities; (v) We discuss precursors of stripe formation, expected at finite doping.
We believe that the rest of our paper is written in a concise and self-contained form. All necessary details are provided for our results to be reproducible, and some technical details are relegated to the Methods section.
We don't know how the referees vague request to shorten our paper should be understood. Since the referee did not provide any specific examples how, in his/her view, our paper should be shortened, while his/her earlier comments actually demanded MORE rather then less details to be included in our paper, we decided against any additional changes.

In summary, we thank the referee for his/her valuable time. We hope that our detailed response has convinced the referee to fully recommend our manuscript for publication in SciPost now.

Report 2, 2018-11-5

We would like to thank the referee for carefully reading our manuscript and recommending our work for publication in SciPost.

***Referee**: The authors introduce a simplified model to describe a doped 2D Heisenberg model, which may shed some new light on the persisting open problem and enable researchers to study spin correlations in experimental context.*

Response: We agree and thank the referee for this positive assessment of our work. We would like to emphasize that some of our analysis even goes beyond standard two or three point spin correlation functions, and we analyze the full counting statistics of the size of non-trivial string patterns revealed in individual snapshots of the wavefunction in Fig. 4 of our manuscript.

***Referee**: The authors should clarify the applicability of the model for higher doping (beyond a single hole) and what are the possible efficient extensions of it.*

Response: We are unsure what the referee means by “the applicability of the model”: Does he/she refer to the mixD model Hamiltonian in Eq. (1), or our model of meson-like bound states forming when the system is doped?

If the referee refers to the former, the model can be implemented and applies at arbitrary doping levels. We clarify this point, we have included a sentence in discussion section now: “*Our model Hamiltonian can be implemented at arbitrary doping levels using ultracold atoms in optical lattices.*”

If the referee refers to the latter, we cannot give a definite answer. As we discuss at the end of the paper: “*we expect that the mixD $t-J$ model can provide new insights into the interplay of superconductivity and stripe phases. At finite doping we also expect that the relation of our meson approach with the fractionalized Fermi liquid theory of the pseudogap phase \cite{Punk2015PNASS} or the phase string effect \cite{Sheng1996,Weng1997,Zhu2015a} can be explored.*” The nature of the pseudogap phase remains elusive, but one scenario that has been theoretically discussed by several authors is that it is constituted by meson-like bound states of spinons and chargons (holons) of some sort, see [Beran et al., Nuc. Phys. B, 473, 707 (1996)] and [Punk et al., PNAS 112, 9552 (2015)] and [G. Baskaran, arXiv:0709.0902]. We believe that the meson-like bound state we discuss in our manuscript at very low doping could be directly related to the bound states relevant to the pseudogap phase. But clarifying the applicability of our microscopic model to this regime will definitely require significantly more work, in particular due to the lack of controlled numerical methods allowing to describe the pseudogap phase.

In this regard, we would like to point out that the mixD model has the interesting advantage that the statistics of the holes can be modified by Jordan-Wigner strings, since the holes can only move along one direction. As we emphasize in the introduction, “*being mappable to a problem of hard-core bosons, the model is sign-problem free, thus enabling efficient quantum Monte Carlo simulations for arbitrary doping values.*” This provides a realistic avenue for testing our model at finite doping.

We are unsure we understand what the referee means by “possible efficient extensions” of our model. Does she/he think our implementation is inefficient? Or that the trial wavefunction is inefficient? Or does the referee refer to efficient approximations to the 2D Hubbard model?

As discussed in some detail in the introduction and discussions sections of our manuscript, we believe that studying our model at finite dopings and exploring the analogies with the doped Hubbard model provides (i) a rich and (ii) an interesting extension of our work. We also believe that the toy model is efficient, in the sense that (i) semi-analytical approximations can be developed, applied and tested and (ii) we expect that our model can be efficiently simulated by numerical Monte Carlo methods.

***Referee**: The authors study the structure of an object (a "meson") when a hole is created in a two-dimensional Heisenberg antiferromagnet. The topic of the doped 2D Heisenberg model is challenging and despite of its origin in 1980's it's still an important open question that needs to be solved. The authors introduce a simplified model, which may shed some light on the problem and enable researchers to study spin correlations in experimental context. I* *recommend the publication of the manuscript in SciPost, after the authors address the comments given below.*

Response: We thank the referee for this positive assessment of our work.

***Referee**: As far as I understand, the setup and model under investigation is useful to study the "meson" structure. I wonder what else one can learn from the model when going to higher doping (anything beyond a single hole). In particular, in case of more holes there are many possible configurations how to design the "filling" of chains. The subtlety of the competition between superconductivity and the stripe phase, as the authors mention in discussion, is that the emergence of "hole-rich" and "hole-depleted" regions eventually takes place in a homogeneous 2D lattice. In this model, however, the distribution of holes should be done by hand. It hence seems that the model should be modified, e.g., by allowing a perpendicular hopping, but then the numerical difficulty again starts to be a problem. I think the authors should clarify for what exactly, beyond the studies of the "meson" structure, the particular model should be useful, or what are the possible efficient extensions of it.*

Response: We agree with the referee that the distribution of holes to the different legs of the mixD model has to be done 'by hand'. A precise mathematical way of formulating the same fact is to say that our model has an extensive number of conserved quantities, namely the number of holes $N_h(y)$ in each leg at coordinate *y* is conserved.
Rather than being disadvantageous and requiring the addition of hole tunneling along *y* for gaining new physical insights, we believe that these conserved quantities provide a rich set of tuning knobs: The mixD t-J model can be studied theoretically, and realized experimentally, in any desired sector $[\dots, N_h(-1), N_h(0), N_h(1), \dots]$. Naturally, we expect that the physics of the ground states in different sectors can differ significantly.
Of course some sectors are more natural candidates to study than others: To make the system translationally invariant along *y*-direction, the most natural choice is to consider a case where $N_h(y)$ is independent of y, and in this case there is only a single doping value $n_h = N_h / L_x$ that should be tuned. Candidate states in this case include a superconducting ground state, stripe phases with hole-rich and hole-depleted regions as described by the referee could form, and analogues of the pseudo phase are conceivable at elevated temperatures. Note that the motion of holes along the x-direction still allows the formation of clusters of holes, and no tunneling along y-direction is required. At the end of our manuscript we have also discussed that pairing of two holes from the same chain appears to be suppressed. In the revised version of the manuscript, we have clarified that both holes in the three-leg ladder move on the central chain. In the future it will be interesting to investigate the possibility of pairing of holes from two neighboring chains, which may be relevant to superconductivity in the homogeneously filled scenario described at the beginning of this paragraph. We have already performed DMRG simulations at finite doping, with an equal number of holes per chain. In this case we observe clear indications for the formation of stripes: Across hole-rich lines oriented along y-direction the AFM order parameter switches sign. These results will be published in a separate

publication, and we devote studies of the competition of such stripes with possible other phases to the future.
Another natural candidate, whose relation to cuprates is less obvious, corresponds to a staggered regime where every second chain is hole-rich and the doping in every other chain is small: $N_h(2y) > N_h(2y+1)$. The precursors for stripe formation which we report for a single hole already in the present manuscript suggest that stripe formation is also possible in this case. It would be very interesting to tune the ratio of the two filling factors, $N_h(2y) / N_h(2y+1)$, and study effects of commensurability, for example. The case when every second chain is empty, $N_h(2y+1) = 0$, could be interesting to study stripe phases with less than one hole per chain.

To make the reader aware of the richness of our system at finite doping, we have included a remark on page 7 of our manuscript: "*Note that the mixD Hamiltonian has many independent sectors of individually tunable doping levels per chain, which need to be studied separately.*" A complete discussion of the finite doping problem will be devoted to future work.

***Referee**: Looking at the SciPost webpage, the Referee report on 2018/Sept/28 poses some relevant questions that should be addressed in a modified version of the manuscript.*
Response: We would ask the referee to read our reply above to the first referee, where we have addressed all points raised in that first report.

***Referee**: Additional comment is related to a better explanation of numerical results in Fig. 2: In (a), why are results from a 6 x 3 cluster plotted as 6 x 6 density plot, in (b), why are results from a 16 x 8 cluster plotted as 9 x 9 plot.*
Response: In Fig. 2 (a) the distance d=6 is equivalent to d=0 due to the periodic boundary conditions used along the x-direction in the ED simulations, as specified in the figure caption. Hence we plot all values d=0,1,2,3,4,5. The same argument applies for the distance d_h to the hole, where we plot all non-equivalent values d_h=0,1,2,3,4,5. This leads to the 6x6 grid we use. With the same reasoning, the results for the 16x8 cluster could in principle be plotted on a 16x16 grid. Surely the referee understands that in such a presentation of our results, it would be difficult to resolve the interesting structures at small distances, and to directly compare them with the ED results in Fig. 2(a). This explains why we have decided to show only a limited window of data values in Fig. 2 (b) which guarantees an appealing graphical form of our figure. We would like to emphasize that we did not, of course, exclude data points based on any other reason than mere graphical appeal. We don't think that an explicit explanation of this reasoning is required in the manuscript, also in light of the first referees recommendation to shorten our manuscript.

In summary, we thank the referee for his/her valuable time. We hope that our detailed responses have convinced the referee to fully recommend our manuscript for publication in SciPost in its current form.