

Resonant fractional conductance through a 1D Wigner chain



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Reviewers' comments:

Reviewer #1 (Remarks to the Author):

The manuscript presents a compelling mechanism that explains the recently observed phenomenon in the transport of quasi-one-dimensional quantum wires. The observation of fractional conductance of plateaus within a 1D channel, where the width is increased, was an unexpected discovery; however, the authors in this paper have proposed a robust theoretical explanation for this observation. This work is highly interesting and undoubtedly deserves publication in this journal. Though the backscattering theory was utilised previously, Ref 11, to explain the odd denominator fractional states, this manuscript presents explanations on the even denominator fractional conductance of a 1D channel, which is currently lacking a theoretical explanation. These new insights can contribute significantly to advancing our understanding of this intriguing phenomenon.

The authors have presented an interesting model that takes into account the interactions between 1D electrons in the spinful case. They have managed to replicate fractional conductance values of $1/2$, $1/32$, and $1/8$ in the absence of a magnetic field, which is similar to what was reported in Ref 5, based on holes in germanium. However, I found it a bit unclear how they define filling factors within the 1D channel in the absence of a magnetic field and what causes the formation of the filling factor.

In the research reported in Ref 6, a hierarchy of non-magnetic fractional conductance plateaus was reported. The authors, in this paper, perhaps could have commented on the fractional conductance reported for electrons in GaAs. It may be helpful if they could comment on why electrons showed different results compared to holes in germanium.

This referee notes that the number of electrons in the channel is crucial to observe the fractional plateaus. This is in agreement with the experimental setup, where an additional gate is utilized to manipulate carriers in the channel. However, it may not be possible to estimate the exact number of electrons in the channel. From the experimental results, the density of carriers, and device layout, it appears that the 'effect' occurs when around 10 electrons/holes are in the system. This number seems to be somewhat larger than the one used by the authors. Therefore, it would be appreciated if the authors could comment on this.

It may have been beneficial for the authors to consider incorporating conductance traces, akin to those utilised in experiments, to aid readers in understanding their quality work. It is uncertain if this was attempted or if there were any complexities in their simulated system.

In addition, the referee is interested in the authors' thoughts on the influence of working temperature on the interacting electrons with spins. It is known that at higher temperatures, the interaction effects and spins tend to smear out, thus leading to the disappearance of the interaction-induced fractional states. It would be helpful to know if the authors have attempted to address this issue, and any comments on the matter would be appreciated.

In summary, the referee acknowledges that the research paper is original, interesting and addresses an important finding in the quantum condensed matter system, and deems it suitable for publication after appropriate modifications have been made.

Reviewer #2 (Remarks to the Author):

The paper studies zero-temperature zero-bias conductance of one-dimensional fermionic chains. The authors focus on the limit of strong nearest-neighbor interaction and weak coupling to the leads. With

this, the authors calculate the conductance of the spinless model and spinful model. For spinless model, the results are consistent with the well-known results of resonance. For spinful model, the results are probably new which indicate no perfect resonance and fractional peak at non-zero chemical potential. In my opinion, this work with novelty can be interesting to specific audience for correlated quantum transport, thus could be suitable for the Comm. Phys. However, the author need to address the following concerns.

1. In this work the authors always work on the strong-coupling limit. To make connection to the experiments, the authors need to comment on the deviation due to weaker interaction.

2. For the spinless resonance as well as the weak interaction (might be genuine interaction), there is a quantitative theory, e.g., PRB 86, 121302; PRB 105 165120 . It would be nice to understand the result of resonance absence by this approach. If there is a more intuitive physical explanation, then it could be better than doing this. To me, the current presentation does not include much more than strong coupling calculation.

3. While Eq.23 does not include the information of onsite interaction, I have the feeling that the author also set this as infinity. If this is true, the authors should be clear about this.

4. Ref.11 is based on the picture of particle(s)-hole(s) bounded liquids, which are very different from the Wigner crystal here. Given both theory of fractional conductance, the authors should discuss if two theory explain different experiments.

We are grateful to both Reviewers for carefully reading the manuscript and providing positive responses and constructive suggestions. Below we give our responses and describe the appropriate changes in the manuscript.

RESPONSE TO REVIEWER 1.

1. "... a bit unclear how they define filling factors within the 1D channel in the absence of a magnetic field..."

The ratio N/ℓ plays the role similar to that of the filling factor in the Quantum Hall Effect. We have added this explanation in the footnote [18].

2. "The authors, in this paper, perhaps could have commented on the fractional conductance reported for electrons in GaAs (ref [6]). It may be helpful if they could comment on why electrons showed different results compared to holes in germanium."

The fractions depend on the choice of the filling factors and chemical potential. In this manuscript we have chosen the simplest possible scenario to demonstrate the principle. Under the same conditions, electron and hole systems might be described by different choices of parameters. We have inserted the appropriate clarification in the last paragraph on p.1, lines 71-78.

3. "From the experimental results, the density of carriers, and device layout, it appears that the 'effect' occurs when around 10 electrons/holes are in the system. This number seems to be somewhat larger than the one used by the authors. Therefore, it would be appreciated if the authors could comment on this."

According to the authors of Ref. [6], the number of electrons was 6 to 10 in the samples they used. But again, our aim in the manuscript is to demonstrate the principles.

4. "... the referee is interested in the authors' thoughts on the influence of working temperature on the interacting electrons with spins."

Our considerations have been at zero T. As we have limited to the ground states of N electrons, it remains valid as long as T is smaller than the gap to the excited states. We have inserted the appropriate explanation after Eq.(13) – lines 155-158.

RESPONSE TO REVIEWER 2.

1. "In this work the authors always work on the strong-coupling limit. To make connection to the experiments, the authors need to comment on the deviation due to weaker interaction."

Indeed, since the weak interaction models produce non-universal conductance values, we turned to the opposite scenario of extremely strong interactions. To make this fact clear, we have now inserted various comments stressing the importance of our assumption: lines 82-84, 213-217, and 291-297.

2. "For the spinless resonance as well as the weak interaction (might be genuine interaction), there is a quantitative theory, e.g., PRB 86, 121302; PRB 105 165120 . It would be nice to understand the result of resonance absence by this approach. If there is a more intuitive physical explanation, then it could be better than doing this. To me, the current presentation does not include much more than strong coupling calculation."

We are thankful to the reviewer for bringing these papers to our attention. We have added new references [19,20] and comments aiming to differentiate between the models. In the Ref. [20] there is numerics for non-interacting leads connected to a 'device', which is either a single- or double-impurity Anderson model, which is not applicable to the experiment we are discussing. In the Ref. [19], the model used includes forward-scattering interaction only, which does not describe formation of the Wigner type crystal. Also, they calculate conductance for a constriction longer than the appropriate correlation length, which is opposite to the limit required. We suggest a different model that may explain universal fractions, not observed in these papers. In general, we do not expect universality in the weak coupling limit, as we now stressed in lines 82-84.

3. "While Eq.23 does not include the information of onsite interaction, I have the feeling that the author also set this as infinity. If this is true, the authors should be clear about this."

We have stated at the very beginning that on-site interaction is assumed to be infinite both for spinless (where it is automatic) and for the spinful case. For clarity, we put now this description before Eq. (23), lines 225-226.

4. "Ref.11 is based on the picture of particle(s)-hole(s) bounded liquids, which are very different from the Wigner crystal here. Given both theory of fractional conductance, the authors should discuss if two theory explain different experiments."

In our model, the electrostatics of the setup dictates well-defined non-fluctuating electron numbers in the constriction, which results in a universal fractional conductance that changes only with a chemical potential (gate voltage). In the Ref. [11], the Luttinger-liquid description results only in partial freezing of modes with inevitable fluctuations due to remaining gapless modes, which is certainly not a Wigner crystal. Both models might describe the experiment. Whether or not the setup electrostatics requires electron number to quantise, is an open question. As we have noted in the introduction (lines 29-33) Ref. [11] describes only fractions with odd denominator, while our model could deliver, e.g., $\frac{1}{2}$. We have added the corresponding description at the end of the discussion and results section, lines 288-297.

We once again thank the referees for taking the time to read our manuscript producing extensive helpful comments.

Yours Sincerely,
Rose Davies, Igor Lerner, Igor Yurkevich.

REVIEWERS' COMMENTS:

Reviewer #1 (Remarks to the Author):

Authors have modified the manuscript satisfactorily, I recommend it for publication.

Reviewer #2 (Remarks to the Author):

The authors have addressed my question and now I believe that the paper can get published.