

Electrical control of the metal-insulator transition in a one dimensional device

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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 paper reports on electrical control of the band gap of a suspended metallic carbon nanotube (CNT) by introducing a potential modulation with a gate array underneath. The authors measured the transport characteristics where the data showed an opening of the band gap in addition to the Coulomb diamonds. The authors attributed the band gap to electrically induced potential modulation in the CNT channel; the single-electron-transistor model featuring a quantum dot with spatially modulated potential is used to explain the experimental data. Although I find the paper proposing an interesting model with a set of experimental data and thereby worth publication in some suitable journal, My opinion is that the paper is not conclusive enough and would not reach the criteria for the nature communications for the following reasons.

1) One of my big concerns about the present paper is how it is evidenced that the disorder can be washed out as a specific gate average voltage. As show in Fig.3 of SI, the energy gap shows quite a variety of complicated behaviors as a function of the amplitude of the gate modulation, which indicates that the CNT channel suffers from disorder. Although I find it interesting that the result in Fig 4 can be modeled by the Mathieu equation, if the result itself is intentionally chosen to from the data of various patterns resulting from the random disorder, it could not be a proof of the gate-modulation induced the band gap.

2) There are no descriptions on what kind of single-wall metallic CNT (with chirality, diameter). As very briefly mentioned in the manuscripts, there have been many papers studying the energy gap of metallic CNTS such as ref [5] of the main text, Mitchell J. Senger et al. PRB 97 035445 (2018), and so on, where they showed more systematic study. I would like to see more substantial discussions on how the present work can be compared to the previous work. I would like to see a basic characteristic such as I-Vg curves with the same voltage to 15 gates, which I think it could a standard way to check if there is a band gap.

3) As noted in the paper, the model featuring a modulating potential is rather simple and it does not consider the screening effect in the metallic CNT. Therefore, I cannot judge if the interpretation of the result is correct quantitatively. I wonder if the authors can discuss more using the density of states of the metallic CNT.

4) Minor comment:

I do not understand how the authors calculated the band gap for "Theory with varying alpha" in Fig. 4 c. Did the authors introduce varying potential to the sinusoidal function in the Mathieu equation and did numerical calculations?

Reviewer #2

(Remarks to the Author)

In this paper, the authors propose a way to open a gap in the spectrum of a carbon nanotube by specifically engineered externally applied gate voltages. The experiment described in the manuscript constitutes an impressive technological achievement. It is however less clear to us what precisely the fundamental or conceptual contribution of this work is. We propose below a list of points that the authors should address in order both to clarify the

above-mentioned concern and to improve the quality of the manuscript.

We separate the comments in four groups and we provide a list of typos to be corrected.

Introduction and motivation

- The work is motivated by the fact that being able to artificially open a gap is useful in many different mesoscopic systems, for various reasons. Here, the mechanism has been implemented in a suspended carbon nanotube. It is however not clear how this could be transferred to other systems. This is particularly hard to grasp since the physical mechanism leading to the opening of the gap is not clearly explained in the manuscript (see comments about the Mathieu equation).
- It would help in understanding the context to cite more clearly in the introduction the previous theoretical works on using a static CDW to open a gap in the bulk and create edge states. Also a clearer connection of the presented work to Refs. 1 and 2 would be helpful. It feels indeed that the presented method is a generalization of their top-down approach to more gates.
- The authors claim that the goal of this generalization is to prevent local perturbations from destroying the desired effect. But the interest of opening up a gap seem to usually be the topologically protected edge states that come with it, which is not discussed here. Therefore, the references to the topological features related to gap opening in condensed matter systems seem a bit misleading.
- An analogy with Peierls systems is proposed in the introduction. It would therefore be nice to add a sentence or a reference to say in which sense this analogy is justified and what are its limits.

Physical mechanism and theory

- The information about the master equation approach is very detailed (perhaps a bit unnecessarily detailed). At the same time it does not become clear why the system can be modeled as a large metallic island, instead of a chain of quantum dots created by the gates.
- Section IV: Mathieu equation. This part, which is highly relevant to this work, is very much reduced with respect to the lengthy discussion of the master equation (see previous comments).
- It would be helpful to write down the charge density used to solve the Poisson equation numerically in Figure 4 (which needs a caption). What is neglected when decoupling Poisson and Schroedinger? Is that the backaction of the electrons from the CNT on the electric potential, and is it justified to neglect this? This would be a nice place to justify the need for 15 gates, for example by showing what happens for fewer gates.
- I feel that stating clearly that the authors only model the bulk properties would also help the reader.
- What is meant by "the equation can be solved asymptotically" (in which parameter? is it justified?). In the same sentence it is mentioned that the gap is obtained numerically, this seems contradictory. Also, as the semiconductor gap is used to find the value of δ , what is its gap Δ_g and how was it chosen? Please explain in particular their relative magnitude!
- The most important in this part would be to explain the physical reason for the appearance of the gap. This seems to have been thoroughly discussed already in Phys. Rev. B. 87, 807 (1952) by Slater. According to him, the potential creates the equivalent of "local" atomic potentials, or harmonic oscillators around each gate, in which the electrons are "trapped", hence the gap. The formula found for the frequency of these harmonic oscillators is $\sqrt{V_0 \delta}$, which directly enters in the expression for the gap that the authors found in the large gate voltage regime. Related to this, why do the authors keep the δ in the large η solution of the Mathieu equation? I feel that this simple physical picture by Slater should be included in the main paper as it gives an intuitive touch to most of the results. Notably, the fact that, in the large gate bias regime and if the only objective is to open a gap, the periodicity of the potential does not crucially matter, as each gate creates an "independent" harmonic oscillator. Furthermore, in a wave-particle duality view, δ looks like the quantum of kinetic energy of electrons in the "periodic lattice" created by the drive. Therefore, the relevant energies for the gap opening are the height of the potential barriers at each "site", V_0 , and the kinetic energy of electrons in such a "crystal", δ . This then confirms that the way to increase the gap size is to increase the gate modulation amplitude or to

decrease its wavelength (or the effective mass of the electrons).

- It would be nice to clarify the topological nature of the setup. Indeed, as the authors mentioned when referring to the bulk-boundary correspondence (and as is known from the surface state literature), on the edge of a periodic crystal, one would expect to see a surface state (Shockley state?). This edge state neither shows up in the conductance plots, nor in the top right panel of Fig. 4 where one could expect the gap to be zero for the 1 and 15 gates. Looking for edge states might be out of scope but it would be nice to say a few words about this.

Specifically about the device and the measurements

- It is not clear to me why the device is embedded in a waveguide cavity.
- The authors mention that the robustness of the gap is due to the periodic setting of the spatial modulation. Is this why many gates are required (the local imperfections in the potential landscape would average out). It seems to me that this is not the case as the gap opening seems to be a single particle property, in the neighborhood of each gate. Is 15 the maximum number of gates the authors could achieve because of experimental constraints, or are there other reasons for this choice?
- When saying that the charge density wave might not be stable enough to prevail, it would be nice to mention the competing effect (the impurities?).
- The authors mention charging effects many times, but it is not clear where or how they are supposed to explain the discrepancies in the results, in particular how they break the left-right symmetry in the nanotube. In the results section the authors mention previously observed charging effects (where? Which reference?). Also in Fig. 4 there are large discrepancies between the red and black dashed curves, which deserve more explanations.
- When experimental results do not match the modelling, the authors invoke inhomogeneous disorder or charging effects without providing any details. It would be nice however to give an intuitive explanation as to why this should change the expected behavior as observed. For example the asymmetry for positive and negative values of ΔV_g appears a few times, and would deserve being commented more. More importantly, when trying to take into account the disorder, in Figure 4 of the main text, it does not seem to give a better match with experimental data.
- Section B, comparison of different gate points: The authors talk about "same qualitative behavior", but apart from the gap not being constant as a function of ΔV_g I do not see a same qualitative behavior. Also I do not see the gap closing, it just gets smaller.

Some missing, more detailed explanations

- In Figure 2, the names of panels b) and c) seem to be reversed in the caption. Why is only $V_{g,7}$ modulated? Also the axis on Figure 2 (mentioned in the results section) are only explained in section Discussion, two pages later. Especially, V_{SD} , is not mentioned in the caption either. In the same Figure, the authors mention that $\angle V_g$ is fixed to a certain value for all panels. But it also appears as the x-axis on the right-hand Figures. This is a bit confusing: even though the model is explained in Supplementary material, the main text should be self-contained. Also it would be nice to point to Figure 3 of the Supplementary material to show that there can be unpredictable complications.
- In Figure 4 of the main text, the labels of the sub plots are missing and "lever arm" should be outside the parenthesis in the caption.
- The last paragraph of page 4 (which ends in page 5) looks like it should either go in introduction or be removed. In this paragraph "the" chain is mentioned, so it should be made clear that your gate potentials create the equivalent of an atomic chain in the CNT. When citing Ref 13 it would be helpful to mention which part of the book is referred to.
- Introducing the textured density of state in the Results section should not be justified by "nothing forbids", and indeed, what does it add to the discussion? Also mentioning superconductors can be misleading, if I understand correctly, the only thing they have in common is a gap?
- The second paragraph of page 5 starts by "The expected electronic potential"
- What is it expected from (this might be due to the previous paragraph not being at the right place)? In Fig 3 one could actually put the Potential found in the Supp. mat. on Figure 4 for comparison.

Typos:

- mass of the electron: mass of the electrons (p.5)
- semiconducting gap of 80 mV -> 80 meV?
- gap of $E_g=6\text{mV} \rightarrow 6\text{meV}$
- In Figure caption 4 of the supplementary material, please replace "poisson" by "grenouille".
- In section "Additional data" of the Supp. mat.: Please, fix the capitalization. (Figures take a capital F, Coulomb as well)
- Equation 22 of the supplementary Material is not an equation.
- Bottom of page 6, "in the tube" is repeated in the same sentence.
- Caption of Fig 3 of Supp. mat.: Figure (a)-(d), should be plural.

Reviewer #3

(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.

Reviewer #4

(Remarks to the Author)

This interesting manuscript describes the experimental observation of a tuneable energy gap in a suspended carbon nanotube (CNT), created by spatially modulating the local potential of multiple (15) gates below the CNT. The potential is created by modulating voltage by a fixed value, above and below an average value, from one gate to the next. It is proposed that such a potential creates a Peierls charge density wave (CDW), with an associated signature – the formation of an energy gap, varying from $200\mu\text{eV}$ to 20meV , in transport measurements at low temperature. Single-electron effects are observed following sweeping of one of the gates, leading to the observation of 'Coulomb diamond' (CD) characteristics in the drain-source conductance vs. gate and drain-source voltage. Such a system can act as a synthesised Peierls system, with potentially stable quantum states. Furthermore, small and large modulation amplitude on the gates lead respectively to CDs which close to zero width in drain-source voltage, or CDs with a low current gap at low-drain-source voltage, which cannot be modulated by the swept gate. It is argued that the gate voltage modulation leads to the gap, due to the Peierls CDW.

The paper has an interesting approach, in my view, to the generation of a CDW in a CNT. This requires difficult fabrication challenges to be overcome, e.g. ensuring a stable CNT which does not collapse onto the underlying gates and appropriate gate dimensions with respect to CNT length. Furthermore, a nice supporting theoretical and experimental analysis is conducted to show the plausibility of the proposed CDW mechanism, e.g. calculation of energy scales, which support experimental observation, and in particular, experimental measurements of the induced energy gap using different gates. In the latter case, the variation in gap width is small, and I agree that this is likely to indicate a homogeneous system along the CNT.

I think the results presented are very interesting, and the supporting analysis demonstrates that a CDW induced energy gap mechanism is certainly plausible. The results also have potentially high impact for the generation of tailored, stable quantum states. I also like the fabrication work, which is non-trivial. Given the above, while I am happy to see that manuscript published, I would like the point below to be addressed.

I am somewhat concerned that the observed experimental result, i.e. the formation of a low-bias gap in the CD characteristics, not modulated by the gate, might occur through other mechanisms as well. Very similar behaviour often occurs in 1-D semiconductor based single-electron systems, due to additional quantum dots being induced by a change in a given gate voltage (in the case of the current manuscript, this might be due to the change in modulation amplitude, perhaps near the CNT edges), with these additional QDs coupling poorly to a swept gate. This would form a multiple tunnel junction / QD system with varying gate coupling. As the manuscript appears to consider a metallic CNT, this effect may perhaps be less likely. However, I would like the authors to consider and discuss this possibility in the manuscript, it would be nice to rule this out – this would in my view strengthen the paper.

Version 1:

Reviewer comments:

Reviewer #2

(Remarks to the Author)

The authors have provided detailed answers to the reviewers' concerns. Importantly they have added a substantial amount of new data and streamlined the presentation of the theoretical background.

I think that the paper is now in principle of interest for publication in Nature Communications.

My main remaining concern are clarity issues. I think the paper would overall benefit from an additional round of streamlining. For example:

- Figures are often not easy to understand. In Fig. 3, the grey coloured surfaces are not explained. Lines are a bit difficult to discern, not only but in particular with the black background.

- I found comparisons with different physical systems (superconductivity, topological states, quantum Hall effect) rather confusing since the underlying physics is different.

- there are quite some typos, which are sometimes confusing, for example in the center of page 9 it says “the larger the gates”, but this should be “the larger the number of gates”. Or in the end of this page there is an “anyway” which should be “away”.

Reviewer #4

(Remarks to the Author)

Thank you very much for addressing my concerns, particularly regarding alternative mechanisms such as addition charging dots. I am therefore happy, from my side, for the manuscript to be accepted for publication.

Reviewer #5

(Remarks to the Author)

As a reviewer of the manuscript, and after considering the comments of the other reviewers, I agree that while certain aspects of the manuscript may be subject to debate, it represents a significant scientific advance and is worthy of publication.

My comments/suggestions are listed below.

1. There is a mismatch between the description in the text and the caption for Figure 2. It was pointed out by reviewer # 2 but not addressed in the text.

Figure 2 contains four subpanels:

(a) Differential conductance map for weak modulation (experimental)

(b) Differential conductance map for strong modulation (experimental)

(c) Modeling of the nearly metallic case

(d) Modeling of the gapped case

In the main text, Figures 2a and 2c are described as experimental, while Figures 2b and 2d are presented as simulations. This is inconsistent with the caption. Clearly, panels b and c are swapped in the caption.

The corrected caption should read:

(a) Differential conductance map for weak modulation (experimental)

(b) Modeling of the nearly metallic case

(c) Differential conductance map for strong modulation (experimental)

(d) Modeling of the gapped case

Corresponding references within the figure caption should also be revised accordingly.

2. I strongly recommend that the authors provide additional clarification of the experimental setup. As reviewer #4 suggested, a clear sentence should be included to explain that the topic concerns “the experimental observation of a tunable energy gap in a suspended carbon nanotube (CNT), created by spatially modulating the local potential of multiple (15) gates below the CNT. The potential is formed by applying alternating voltages, offset above and below an average value, from one gate to the next.”

In addition, the description of the gate voltages is unclear. The manuscript states (page 4) that in Figure 2 the average gate value is $\langle V_g \rangle = 0.2\text{V}$ with a modulation amplitude of $\Delta V_g = -50\text{ mV}$. It is not evident how these values translate into the experimental configuration. For instance, does a reported modulation of $\Delta V_g = -50\text{ mV}$ indicate that odd (or even?) gates are set to the average value $\langle V_g \rangle$, while the even (or odd) gates are offset by -50 mV ? Furthermore, how is the average gate value $\langle V_g \rangle$ determined? Similarly, in the strong modulation case with $\Delta V_g = +250\text{ mV}$, does this imply that odd (or even) gates are set to the average $\langle V_g \rangle$, while the remaining gates are offset by $+250\text{ mV}$? Clarification of these points is necessary to ensure that readers can accurately interpret the experimental setup. I suggest a paragraph to be added to the supplementary information to clearly explain it.

3. In the caption for Figure 4b, "Dependence of the extracted energy gap as a function of the gate number for three different gap values," the symbols for $\Delta V_g = 50$ mV and $\Delta V_g = 300$ mV appear to be swapped; otherwise, this contradicts the overall narrative and Figure 4c. This should be corrected to accurately reflect the data.

Version 2:

Reviewer comments:

Reviewer #5

(Remarks to the Author)

The manuscript can be accepted in its present form. Nevertheless, I recommend adding an explicit reference to Figure 3 in the Supplementary Materials, which will help clarify the non-monotonic dependence of the gap value on ΔV_g discussed in the explanation of Figure 4b in the main text, rather than simply stating that "the corresponding measurements are shown in the supplementary."

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We would like to thank the reviewers for their comments and their work. We note that the general tone of the reviews is quite positive. We also thank the reviewer for their constructive remarks and careful reading. We provide below a point by point answer to the comments/questions which allows us to clarify the points raised by the reviewers. We have substantially revised the manuscript with extensive discussions added about the interpretation, new modeling and a lot of new data, which further confirm the validity of our interpretation and reinforce the strength of our conclusions.

The main changes are:

1. Added additional data in figure 4 from a new cool down of the device to support the opening and closing of gap on a wider scale.
2. Added figure showing the variation on a wider scale of the gap for different chemical potential.
3. Added figure showing the site by site construction of the gaped chain (figure 6).
4. Modified the discussion and the presentation of the data in figure 4 and the corresponding figures of the supp mat. We have completely rewritten this part and removed the “varying α ” model which was confusing (as raised by reviewer #1). We have now included an extension of the previous modeling and allows one to make a more direct comparison with the Mathieu equations.
5. Refined the modeling using Mathieu equation using numerical solving in the presence of weak disorder. This leads to a much better agreement between theory and experiments.
6. Modified figure 3 to incorporate the discussion on the Slater paper and to illustrate better our gap mechanism using the harmonic picture. Added to that figure plots from the modelling with both tight binding density of states and Mathieu equations with or without disorder.
7. Added numerical data for a 15-gate quantum dot chain using a Green’s function formalism to make a connection with the continuum picture of Mathieu equations.
8. Added discussion related to the harmonic oscillator along the discussion of the paper by Slater (reviewers 2 and 3).
9. Added new data in the supplementary where we show the construction site by site of the chain by the side to rule out the “uncoupled dots” mechanism.
10. Added I-Vg data for the 15 gates at room temperature to show the homogeneity of the response of the device and I-Vg for all gates at room temperature and low temperature in the supplementary.
11. Added a discussion about the connection between Peierls physics, topological gaps and band gaps and the gap protection of quantum states (topological or not).
12. Made an explicit link with the initial additional data in the supplementary and the refinement of the modelling.
13. Added references to the paper by J.C. Slater Phys. Rev. 87, 807 (1952) and to M. J. Senger et al. PRB 97 035445 (2018), J. Klinovaja and D. Loss PRL 110, 126402 (2013), L. Contamin et al. npjqi 7, 121 (2021), E. Laird et al. RMP 87, 703 (2015).
14. Reorganized the discussion in the main text to a great extent.

Reviewer #1 (Remarks to the Author): *The paper reports on electrical control of the band gap of a suspended metallic carbon nanotube (CNT) by introducing a potential modulation with a gate array underneath. The authors measured the transport characteristics where the data showed an opening of the band gap in addition to the Coulomb diamonds. The authors attributed the band gap to electrically induced potential modulation in the CNT channel; the single-electron-transistor model featuring a quantum dot with spatially modulated potential is used to explain the experimental data. Although I find the paper proposing an interesting model with a set of experimental data and thereby worth publication in some suitable journal, My opinion is that the paper is not conclusive enough and would not reach the criteria for the nature communications for the following reasons.*

We thank the reviewer for the comments which has helped us to improve our manuscript. The reviewer seems to compare our contribution with respects to previous work on carbon nanotubes with observed gaps. We would like to point out that our work is markedly different from these early works. In particular, in these works, the focus was more on the effect of interactions. While interesting, these effects cannot lead to an in-situ electrical tuning of the energy gap. In addition, many features such as Kondo-like anomalies of unknown origin were observed quite systematically in these measurements. This calls for the use of different, simpler mechanism to induce in a controlled way an energy gap in a one-dimensional system which would not rely in particular on electron-electron interactions. This could turn out to be crucial to protect quantum states. This is one of the goals of our work. More precisely, we focus on a non-interacting mechanism which could ultimately be used to protect quantum states with topological properties or not, which is a completely different focus from these early studies. We have added this discussion to highlight better the novelty of our work.

1) One of my big concerns about the present paper is how it is evidenced that the disorder can be washed out as a specific gate average voltage. As show in Fig.3 of SI, the energy gap shows quite a variety of complicated behaviors as a function of the amplitude of the gate modulation, which indicates that the CNT channel suffers from disorder. Although I find it interesting that the result in Fig 4 can be modeled by the Mathieu equation, if the result itself is intentionally chosen to from the data of various patterns resulting from the random disorder, it could not be a proof of the gate-modulation induced the band gap.

We are strongly concerned by the presentation of all the data we have and this is why we had presented the full complexity of our findings in the SI. In order to address this question, we have done a new cool down and pushed further our analysis by studying the gap on a wider ΔV_g , for different average voltage and with a different number of sites. We now show that at large ΔV_g , the residual disorder which induces the gap fluctuations is washed out and that the gap vs ΔV_g curves essentially overlap at large ΔV_g . Therefore, we recover a very simple picture at large oscillating gate amplitude as expected. This is quantitatively explained by the simple theory based on Mathieu equations. The agreement is in fact better than in the original version of the manuscript.

2) There are no descriptions on what kind of single-wall metallic CNT (with chirality, diameter). As very briefly mentioned in the manuscripts, there have been many papers studying the energy gap of metallic CNTS such as ref [5] of the main text, Mitchell J. Senger et al. PRB 97 035445 (2018), and so on, where they showed more systematic study. I would like to see more substantial discussions on how the present work can be compared to the previous work. I would like to see a basic characteristic such as I - V_g curves with the same voltage to 15 gates, which I think it could a standard way to check if there is a band gap.

We thank the reviewer for pointing out this reference which infers the gap in carbon nan-

otubes through temperature dependence of the resistance of carbon nanotube devices. In all the works cited and mentioned by us, a high energy gap was considered, essentially above 100 meV, which were attributed to correlation effects. For example, reference 5 considered Mott physics. These correlation gaps are not electrically tunable and cannot be used to generate synthetic edge states. On the contrary, our work is closer to the Peierls physics which is the other main mechanism to induce a gap besides Mott as stated in reference 1. By generating an incommensurate spatial modulation, we engineer an electrically tunable gap at a much lower energy which could ultimately be used to generate exotic edge states with topological properties, like proposed in reference 16.

We have also included in the supplementary material room temperature characterization on all the gates as requested where we only see a weak dependence of the current for all gates. We have also included an all gate scan at room temperature and low temperatures which shows the evolution of the current on a wide average gate scan. While for each gate, the room temperature response is weak, there is a down-turn of the current above 0.5 V for the all gate scan at room temperature. At low temperature, the behavior becomes clearer and negligible current is observed for $\langle V_g \rangle > 0.1V$, signalling the presence of a small gap. The main measurements are done away from this region at negative $\langle V_g \rangle$. The new data shown in figure 5 and 6, in particular the construction site by site of the chain, enables us to demonstrate experimentally that we are not probing an intrinsic property of the carbon nanotube but a gap induced by the 15 gates.

3) *As noted in the paper, the model featuring a modulating potential is rather simple and it does not consider the screening effect in the metallic CNT. Therefore, I cannot judge if the interpretation of the result is correct quantitatively. I wonder if the authors can discuss more using the density of states of the metallic CNT.*

4) *Minor comment: I do not understand how the authors calculated the band gap for "Theory with varying alpha" in Fig. 4 c. Did the authors introduce varying potential to the sinusoidal function in the Mathieu equation and did numerical calculations?*

We respond jointly to points 3) and 4) because they turn out to be related. We have removed the discussion on the varying alpha which was confusing. We have completely modified the discussion regarding the theory. We have also done tight binding modelling of the device using Green's function methods. In both cases, we have introduced disorder and solved numerically the related equations. This has allowed us to have a much better agreement with both the initial and the new experimental data.

Reviewer #2 (Remarks to the Author): *In this paper, the authors propose a way to open a gap in the spectrum of a carbon nanotube by specifically engineered externally applied gate voltages. The experiment described in the manuscript constitutes an impressive technological achievement. It is however less clear to us what precisely the fundamental or conceptual contribution of this work is. We propose below a list of points that the authors should address in order both to clarify the above-mentioned concern and to improve the quality of the manuscript.*

We thank the reviewers for these very positive comments.

We separate the comments in four groups and we provide a list of typos to be corrected.

Introduction and motivation

- The work is motivated by the fact that being able to artificially open a gap is useful in many different mesoscopic systems, for various reasons. Here, the mechanism has been implemented in a

suspended carbon nanotube. It is however not clear how this could be transferred to other systems. This is particularly hard to grasp since the physical mechanism leading to the opening of the gap is not clearly explained in the manuscript (see comments about the Mathieu equation).

We have now clarified the generality of our findings. The new discussion on the Mathieu equation has helped us to make a more direct connection with the other platforms which could take benefit of the presented method to open a gap. We have also added a discussion about the possibility to use such a setup for non-abelian braiding which makes a stronger connection to the Majorana works.

- It would help in understanding the context to cite more clearly in the introduction the previous theoretical works on using a static CDW to open a gap in the bulk and create edge states. Also a clearer connection of the presented work to Refs. 1 and 2 would be helpful. It feels indeed that the presented method is a generalization of their top-down approach to more gates.

We have completely rewritten that part. One important theoretical reference for our work is ref. 16. We now explain better how our present work relates to it and how we could use it in the context of reference 1 and 2.

- The authors claim that the goal of this generalization is to prevent local perturbations from destroying the desired effect. But the interest of opening up a gap seem to usually be the topologically protected edge states that come with it, which is not discussed here. Therefore, the references to the topological features related to gap opening in condensed matter systems seem a bit misleading.

The system which we have in mind is a Su Schrieffer Heeger (SSH) chain which has topological properties. It is considered as a very important benchmark for topological systems and bears strong analogies with the Majorana systems considered in refs 2 and 3. Indeed, both Majorana and SSH systems are enabled by modulating spatially a physical parameter in a 1D chain from which a gap, topological or “trivial” depending on the tuning, emerges. This gap is the prerequisite for the isolation of edge states with remarkable properties. It is therefore important to demonstrate that one can engineer a sizeable low energy gap in spatially modulated 1D systems. We have clarified this issue in the main text by extending this discussion and added relevant references.

- An analogy with Peierls systems is proposed in the introduction. It would therefore be nice to add a sentence or a reference to say in which sense this analogy is justified and what are its limits.

In a Peierls system induced by nesting properties of the Fermi surface, the wavelength of the emerging charge density wave is not necessarily commensurate with the lattice of the chain. However, the charge density wave is the new groundstate. In our case, we induce by the gates an a priori incommensurate charge density wave but without having to rely on the fact that it corresponds to a minimum of the system energy. We have added a discussion along these lines.

Physical mechanism and theory

- The information about the master equation approach is very detailed (perhaps a bit unnecessarily detailed). At the same time it does not become clear why the system can be modeled as a large metallic island, instead of a chain of quantum dots created by the gates.

We have simplified the discussion on the master equation. The large metallic island limit is the

limit when correlation effects between the electron are neglected and one can assume that there is weak screening between the different electronic sites of the chain. In such a case, the Coulomb interaction acts globally on the full length of the chain yielding constant interaction model type charging term and hence a large metallic island with a global charging energy. We have expanded the discussion on this.

- *Section IV: Mathieu equation. This part, which is highly relevant to this work, is very much reduced with respect to the lengthy discussion of the master equation (see previous comments).*

We have expanded the discussion on Mathieu equations, in connection with the discussion of the paper by Slater. See below.

- *It would be helpful to write down the charge density used to solve the Poisson equation numerically in Figure 4 (which needs a caption). What is neglected when decoupling Poisson and Schroedinger? Is that the backaction of the electrons from the CNT on the electric potential, and is it justified to neglect this? This would be a nice place to justify the need for 15 gates, for example by showing what happens for fewer gates.*

There is a priori back-action on the CNT electric potential from the electrons. Along the lines of the discussion of the constant interaction model and in a mean field approach, we account for it simply by a gate lever arm. We have followed the suggestions by the reviewers and studied systematically site by site how the gap emerges. While 1, 3 or 5 gates are not enough to produce a gap in the conductance which is dominated by the spatial modulation, the minimum number of gates where the effect settles appears to be between 7 and 9. This is now clearly shown in a completely new set of data in figure 6 of the main text (construction by the middle) and in a new figure in the supplementary (construction by the side).

- *I feel that stating clearly that the authors only model the bulk properties would also help the reader.*

We include now a sentence in the main text stating that more clearly.

- *What is meant by “the equation can be solved asymptotically” (in which parameter? is it justified?). In the same sentence it is mentioned that the gap is obtained numerically, this seems contradictory. Also, as the semiconductor gap is used to find the value of δ , what is its gap Δ_g and how was it chosen? Please explain in particular their relative magnitude!*

The wording “asymptotically” was confusing. We apologize for that. We were referring to reference 15, Wilkinson et al. Physica E '18, which considers simple limiting cases in order to provide analytical formulae. We have replaced this discussion and explained with greater details its input parameters. We have also added a new modelling part by introducing explicitly weak disorder in the Mathieu equation and by introducing a tight binding Green's function modelling.

- *The most important in this part would be to explain the physical reason for the appearance of the gap. This seems to have been thoroughly discussed already in Phys. Rev. B. 87, 807 (1952) by Slater. According to him, the potential creates the equivalent of “local” atomic potentials, or harmonic oscillators around each gate, in which the electrons are “trapped”, hence the gap. The formula found for the frequency of these harmonic oscillators is $\sqrt{V_0\delta}$, which directly enters in the expression for the gap that the authors found in the large gate voltage regime. Related to this, why do the authors keep the -1 in the large η solution of the Mathieu equation? I feel that this simple physical picture by Slater should be included in the main paper as it gives an intuitive touch to*

most of the results. Notably, the fact that, in the large gate bias regime and if the only objective is to open a gap, the periodicity of the potential does not crucially matter, as each gate creates an “independent” harmonic oscillator. Furthermore, in a wave-particle duality view, δ looks like the quantum of kinetic energy of electrons in the “periodic lattice” created by the drive. Therefore, the relevant energies for the gap opening are the height of the potential barriers at each “site”, V_0 , and the kinetic energy of electrons in such a “crystal”, δ . This then confirms that the way to increase the gap size is to increase the gate modulation amplitude or to decrease its wavelength (or the effective mass of the electrons).

We thank the reviewers for this very important point and for pointing out this important paper by Slater. This paper is equivalent to the “transmon” paper of Wilkinson et al. but it is more directly related to the gap opening in electronic systems indeed. The Mathieu equation in the large amplitude limit can be approximated by a series of harmonic traps in which each harmonic oscillator has a quantized spectrum. This quantization is by construction gapped with a gap which is the result of the curvature of the harmonic well and the kinetic energy. Like pointed out in the paper by Slater, this provides a level spacing of these harmonic oscillators of the form $\sqrt{V_0\delta}$. A bulk gap emerges if all these oscillators are equivalent, hence yielding the same V_0 . This explains qualitatively why the periodic modulation is needed in order to obtain a robust bandgap. It is worth noting that a similar mechanism is at work in the Quantum Hall effect which is insulating in the bulk and conducting in the edges. The Landau levels are the harmonic oscillators in that case. The gap of the bulk is simply the level spacing of these Landau levels. We have expanded this discussion in the main text and modified figure 3 to bring forward this important analogy.

- It would be nice to clarify the topological nature of the setup. Indeed, as the authors mentioned when referring to the bulk-boundary correspondence (and as is known from the surface state literature), on the edge of a periodic crystal, one would expect to see a surface state (Shockley state?). This edge state neither shows up in the conductance plots, nor in the top right panel of Fig. 4 where one could expect the gap to be zero for the 1 and 15 gates. Looking for edge states might be out of scope but it would be nice to say a few words about this.

As discuss above, we have in mind the topological SSH chain which could be implemented in our setup. We would expect indeed Shockley states or Jackiw-Rebbi topological states. The possible emergence of localized states in our setup is for the moment out of the scope of our study. We have added a discussion on this point.

Specifically about the device and the measurements

- It is not clear to me why the device is embedded in a waveguide cavity.

The reason for constructing the device inside a microwave cavity is to probe the compressibility of the potential localized edge or bulk states, a bit like a previous work Desjardins et al. Nature’17. The joint compressibility/conductance measurements has proven to be very discriminating. This, like the discussion on localized states, is out of the scope of the present study. We have nevertheless added a discussion on this point and, in particular, a discussion on the possibility to use the microwave cavity to perform braiding operations as theoretically proposed by some of us (ref Contamin et al. npjq’21).

- The authors mention that the robustness of the gap is due to the periodic setting of the spatial modulation. Is this why many gates are required (the local imperfections in the potential landscape would average out). It seems to me that this is not the case as the gap opening seems to be a single particle property, in the neighborhood of each gate. Is 15 the maximum number of gates the au-

thors could achieve because of experimental constraints, or are there other reasons for this choice?

Yes, the main reason is the robustness of the gap. The maximum number of gates which does not result in collapsing of the nanotube is about 15. Since the devices are already challenging, we have limited our study to 15-gates devices. This echoes also now with the construction site by site of the gap in figure 6. We find experimentally that a minimum of 7 gates are needed to produce a robust gap.

- When saying that the charge density wave might not be stable enough to prevail, it would be nice to mention the competing effect (the impurities?).

The main competing effect in solid state systems is interactions like mentioned in reference 1. In our case, we have indeed another competing effect which is specific to synthetic setups, which is disorder. Disorder can induce a localization gap which will indeed compete with the synthetic charge density wave gap which we induce via the gates. We have now included explicitly disorder in our modelling which has allowed us to quantify the effect of disorder. We have added these points in the general discussion of the setup.

- The authors mention charging effects many times, but it is not clear where or how they are supposed to explain the discrepancies in the results, in particular how they break the left-right symmetry in the nanotube. In the results section the authors mention previously observed charging effects(when? Which reference?). Also in Fig. 4 there are large discrepancies between the red and black dashed curves, which deserve more explanations.

- When experimental results do not match the modelling, the authors invoke inhomogeneous disorder or charging effects without providing any details. It would be nice however to give an intuitive explanation as to why this should change the expected behavior as observed. For example the asymmetry for positive and negative values of ΔV_g appears a few times, and would deserve being commented more. More importantly, when trying to take into account the disorder, in Figure 4 of the main text, it does not seem to give a better match with experimental data.

The charging effects were not the focus of our study but we agree that we should at least make connection with the large literature about this in carbon nanotube devices. We have added a reference to E. Laird et al Rev. of Mod. Phys. (2015). We now also discuss more thoroughly the discrepancies due to disorder, but also the biasing of the device which produces the peculiar shifted and truncated Coulomb diamond pattern. The main reason for the discrepancies between our experimental findings is the residual disorder. The effect of disorder can be modelled using Green's function techniques in a chain of 15 dots as included in the Supplementary Information. We model it by changing the relative position of the dot levels. The main features induced by disorder are a shift of opening and closing point of the gap and an asymmetry of the dependence of the gap as a function of ΔV_g . We now also show that we can quantitatively model the gap opening with the numerical solving of the Mathieu equation with disorder added. Note that the measurement of the gap on a wider ΔV_g scale has allowed us to show the robustness of our setup experimentally and to refine the analysis in terms of Mathieu equation gap. It turns out that the experiment is best fit with higher energy gap in the Mathieu spectrum and not the "cuspy" gap function of the lowest gap in Mathieu equation.

- Section B, comparison of different gate points: The authors talk about "same qualitative behavior", but apart from the gap not being constant as a function of ΔV_g I do not see a same qualitative behavior. Also I do not see the gap closing, it just gets smaller.

In order to address this question, we have now conducted a much more systematic study

on a wider ΔV_g scale which shows the robustness of our gap opening for different average V_g . This is now shown in a completely new set of data in the new figure 5 in the main text.

Some missing, more detailed explanations

- In Figure 2, the names of panels b) and c) seem to be reversed in the caption. Why is only $V_{g,7}$ modulated?

Thanks for pointing out this typo. Regarding the modulation vs $V_{g,7}$, it turns out that we had to choose one gate to show the transport colorscale map. The systematics versus all gates is done in figure 4. We now refer directly to this section in the text when explaining the results of figure 2.

Also the axis on Figure 2 (mentioned in the results section) are only explained in section Discussion, two pages later. Especially, V_{SD} , is not mentioned in the caption either. In the same Figure, the authors mention that $\langle V_g \rangle$, is fixed to a certain value for all panels. But it also appears as the x-axis on the right-hand Figures. This is a bit confusing: even though the model is explained in Supplementary material, the main text should be self-contained. Also it would be nice to point to Figure 3 of the Supplementary material to show that there can be unpredictable complications.

- In Figure 4 of the main text, the labels of the sub plots are missing and "lever arm" should be outside the parenthesis in the caption.

We hope we have fixed all these issues in the new version of the manuscript. This discussion is now replaced by the 4 new data sets shown in figure 4, 5, 6 and in the supplementary.

- The last paragraph of page 4 (which ends in page 5) looks like it should either go in introduction or be removed. In this paragraph "the" chain is mentioned, so it should be made clear that your gate potentials create the equivalent of an atomic chain in the CNT. When citing Ref 13 it would be helpful to mention which part of the book is referred to.

We acknowledge that the previous organization of the main text was somewhat confusing. We have reorganized the main text along those lines.

- Introducing the textured density of state in the Results section should not be justified by "nothing forbids", and indeed, what does it add to the discussion? Also mentioning superconductors can be misleading, if I understand correctly, the only thing they have in common is a gap?

- The second paragraph of page 5 starts by "The expected electronic potential"

- What is it expected from (this might be due to the previous paragraph not being at the right place)? In Fig 3 one could actually put the Potential found in the Supp. mat. on Figure 4 for comparison.

We have completely rewritten that part and include now a discussion related to the paper by Slater. We hope this is OK now.

Typos:

- mass of the electron: mass of the electrons (p.5)

- semiconducting gap of 80 mV $\rightarrow$ 80 meV?

- gap of $E_g = 6$ mV $\rightarrow$ 6 meV.

- In Figure caption 4 of the supplementary material, please replace "poisson" by "grenouille".

- In section "Additional data" of the Supp. mat.: Please, fix the capitalization. (Figures take a capital F, Coulomb as well).
- Equation 22 of the supplementary Material is not an equation.
- Bottom of page 6, "in the tube" is repeated in the same sentence.
- Caption of Fig 3 of Supp. mat.: Figure (a)-(d), should be plural.

We apologize for these typos. We have fixed them now.

Reviewer #3 (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.

We thank the reviewer for the very constructive remarks.

Reviewer #4 (Remarks to the Author): This interesting manuscript describes the experimental observation of a tuneable energy gap in a suspended carbon nanotube (CNT), created by spatially modulating the local potential of multiple (15) gates below the CNT. The potential is created by modulating voltage by a fixed value, above and below an average value, from one gate to the next. It is proposed that such a potential creates a Peierls charge density wave (CDW), with an associated signature – the formation of an energy gap, varying from 200 μeV to 20 meV , in transport measurements at low temperature. Single-electron effects are observed following sweeping of one of the gates, leading to the observation of "Coulomb diamond" (CD) characteristics in the drain-source conductance vs. gate and drain-source voltage. Such a system can act as a synthesised Peierls system, with potentially stable quantum states. Furthermore, small and large modulation amplitude on the gates lead respectively to CDs which close to zero width in drain-source voltage, or CDs with a low current gap at low-drain-source voltage, which cannot be modulated by the swept gate. It is argued that the gate voltage modulation leads to the gap, due to the Peierls CDW.

The paper has an interesting approach, in my view, to the generation of a CDW in a CNT. This require difficult fabrication challenges to be overcome, e.g. ensuring a stable CNT which does not collapse onto the underlying gates and appropriate gate dimensions with respect to CNT length. Furthermore, a nice supporting theoretical and experimental analysis is conducted to show the plausibility of the proposed CDW mechanism, e.g. calculation of energy scales, which support experimental observation, and in particular, experimental measurements of the induced energy gap using different gates. In the latter case, the variation in gap width is small, and I agree that this is likely to indicate a homogenous system along the CNT.

We thank the reviewer for the careful reading and positive assessment. We have extensively revised the paper in order to further improve its readability, clarity and to show the robustness of our findings. We now include completely new data sets from a new cool down in figure 4, 5, 6 and in the supplementary.

I think the results presented are very interesting, and the supporting analysis demonstrates that a CDW induced energy gap mechanism is certainly plausible. The results also have potentially high impact for the generation of tailored, stable quantum states. I also like the fabrication work, which is non-trivial. Given the above, while I am happy to see that manuscript published, I would like the point below to be addressed.

I am somewhat concerned that the observed experimental result, i.e. the formation of a low-bias gap in the CD characteristics, not modulated by the gate, might occur through other mechanisms

as well. Very similar behaviour often occurs in 1-D semiconductor based single-electron systems, due to additional quantum dots being induced by a change in a given gate voltage (in the case of the current manuscript, this might be due to the change in modulation amplitude, perhaps near the CNT edges), with these additional QDs coupling poorly to a swept gate. This would form a multiple tunnel junction / QD system with varying gate coupling. As the manuscript appears to consider a metallic CNT, this effect may perhaps be less likely. However, I would like the authors to consider and discuss this possibility in the manuscript, it would be nice to rule this out – this would in my view strengthen the paper.

We would like to thank the reviewer for pointing out this important point. One argument which in our view is justifying that we do not have uncoupled dots (or spurious dots) in the CNT is the homogeneity of the response of the CNT for all the gates in the device. However, in order to address specifically this important question at low temperatures, we have constructed site by site the chain either starting from the middle (shown in figure of the main) or from the side (shown in the supplementary) in the new cooldown. We observe qualitatively the same behavior as in the 15-gate case and the gap opening goes converges gradually to the full gate case and saturates above 7 to 9 gates actuated (from the middle). This strengthens the interpretation in terms of the modulation induced gap. A spurious dot would either show on adding a specific gate or be triggered differently upon starting the construction by the edge or by the middle. We have also added new data sets on a wider ΔV_g scale to show better the robustness of the gap opening.

Note in addition that our nanotube appears to have a small gap. In order to better characterize this, we have now included in the supplementary material room temperature characterization on all the gates as requested by reviewer 1 where we only see a weak dependence of the current for all gates. We have also included an all gate scan at room temperature and low temperatures which shows the evolution of the current on a wide average gate scan. While for each gate, the room temperature response is weak, there is a down-turn of the current above 0.5 V for the all gate scan at room temperature. At low temperature, the behavior becomes clearer and negligible current is observed for $\langle V_g \rangle > 0.1V$, signalling the presence of a small gap. The main measurements are done away from this region at negative $\langle V_g \rangle$. As stated above, and although our nanotube appears to have a small gap, the new data shown in figure 5 and 6 (and in the supplementary), in particular the construction site by site of the chain, enables us to demonstrate experimentally that we are not probing an intrinsic property of the carbon nanotube but a gap induced by the 15 gates.

We would like to thank the reviewers for now recommending publication of our manuscript in Nature Communications. We have addressed all the remaining points in our revised manuscript. We reply point by point to the recommendations of the reviewers below.

Reviewer #2

The authors have provided detailed answers to the reviewers concerns. Importantly they have added substantial amount of new data and streamlined the presentation of the theoretical background. I think that the paper is now in principle of interest for publication in Nature Communications.

We would like to thank the reviewer for these positive comments and for helping us to further improve our manuscript.

My main remaining concern are clarity issues. I think the paper would overall benefit from an additional round of streamlining. For example:

- Figures are often not easy to understand. In Fig. 3, the grey coloured surfaces are not explained. Lines are a bit difficult to discern, not only but in particular with the black background.
- I found comparisons with different physical systems (superconductivity, topological states, quantum Hall effect) rather confusing since the underlying physics is different.
- there are quite some typos, which are sometimes confusing, for example in the center of page 9 it says “the larger the gates”, but this should be “the larger the number of gates”. Or in the end of this page there is an “anyway” which should be “away”.

We have now fixed all these issues. We have, in particular, clarified the discussion on the different physical systems at the bottom of page 5. We have also changed the contrast in figure 3 on the black background and explained the grey coloured surfaces.

Reviewer #4:

Thank you very much for addressing my concerns, particularly regarding alternative mechanisms such as addition charging dots. I am therefore happy, from my side, for the manuscript to be accepted for publication.

We would like to thank the reviewer for these positive comments

Reviewer #5:

As a reviewer of the manuscript, and after considering the comments of the other reviewers, I agree that while certain aspects of the manuscript may be subject to debate, it represents a significant scientific advance and is worthy of publication.

We would like to thank the reviewer for these positive comments and for helping us to further improve our manuscript.

My comments/suggestions are listed below.

1. There is a mismatch between the description in the text and the caption for Figure 2. It was pointed out by reviewer # 2 but not addressed in the text.

Figure 2 contains four subpanels:

- (a) Differential conductance map for weak modulation (experimental)
- (b) Differential conductance map for strong modulation (experimental)
- (c) Modeling of the nearly metallic case
- (d) Modeling of the gapped case

In the main text, Figures 2a and 2c are described as experimental, while Figures 2b and 2d are presented as simulations. This is inconsistent with the caption. Clearly, panels b and c are swapped in the caption.

The corrected caption should read:

- (a) Differential conductance map for weak modulation (experimental)
- (b) Modeling of the nearly metallic case
- (c) Differential conductance map for strong modulation (experimental)
- (d) Modeling of the gapped case

Corresponding references within the figure caption should also be revised accordingly.

We would like to thank the reviewer for pointing out these remaining issues. We have now fixed them.

2. I strongly recommend that the authors provide additional clarification of the experimental setup. As reviewer #4 suggested, a clear sentence should be included to explain that the topic concerns “the experimental observation of a tunable energy gap in a suspended carbon nanotube (CNT), created by spatially modulating the local potential of multiple (15) gates below the CNT. The potential is formed by applying alternating voltages, offset above and below an average value, from one gate to the next.”

We would like to thank the reviewer for suggesting this sentence. We have now included a slightly modified version of it at the beginning of our main text. The corresponding paragraph at the top of page 3 starting by « Here, we demonstrate.. » now includes « The gap is created by spatially modulating the local potential of the multiple gates below the CNT. The potential is formed by applying alternating voltages, shifted by ΔV_g above and below an offset value $\langle V_g \rangle$, from one gate to the next. ».

In addition, the description of the gate voltages is unclear. The manuscript states (page 4) that in

Figure 2 the average gate value is $\langle V_g \rangle = 0.2\text{V}$ with a modulation amplitude of $\Delta V_g = -50\text{ mV}$. It is not evident how these values translate into the experimental configuration. For instance, does a reported modulation of $\Delta V_g = -50\text{ mV}$ indicate that odd (or even?) gates are set to the average value $\langle V_g \rangle$, while the even (or odd) gates are offset by -50 mV ? Furthermore, how is the average gate value $\langle V_g \rangle$ determined? Similarly, in the strong modulation case with $\Delta V_g = +250\text{ mV}$, does this imply that odd (or even) gates are set to the average $\langle V_g \rangle$, while the remaining gates are offset by $+250\text{ mV}$? Clarification of these points is necessary to ensure that readers can accurately interpret the experimental setup. I suggest a paragraph to be added to the supplementary information to clearly explain it.

We would like to thank the reviewer for these comments. We have clarified these remaining issues. We have changed the terminology « average » in favor of « offset » gate which is more accurate for our experimental setup. This solves all the questions raised above by the reviewer.

3. In the caption for Figure 4b, “Dependence of the extracted energy gap as a function of the gate number for three different gap values,” the symbols for $\Delta V_g = 50\text{ mV}$ and $\Delta V_g = 300\text{ mV}$ appear to be swapped; otherwise, this contradicts the overall narrative and Figure 4c. This should be corrected to accurately reflect the data.

We would like to thank the reviewer for these comments. Figure 4b corresponds to a different offset gate voltage than figure 4c. This is why the delta V_g 's appear swapped. But the symbols are indeed correct. The differences are fully consistent with the offset gate dependence shown for example in figure 5.