Reviewers' comments:

Reviewer #1 (Remarks to the Author):

The manuscript by Pankin et al is an extensive work in which Bound States in the Continuum are found in 1D systems. The authors first describe a toy model showing BICs: an electron in a 1D potential. BIC signatures can be found by adding a magnetic field, that splits spin in energies, and changing its direction in the potential zone (tilted in the x-z plane) to couple the two spin modes. The model neglects orbital motion of the electrons in the magnetic field, but its perfectly illustrative. BICs are observed in the simulated reflection of spin up electrons as a function of energy and length of the potential and tilting angle of the magnetic field.

The main result of the paper is its photonic equivalent, that is designed, simulated and experimentally demonstrated. Polarization takes the role of spin, and anisotropy the role of the magnetic field.
An extensive analytical derivation of all the field profiles, couplings and reflection responses can be found in the supplementary information, thing that I appreciate, although it is most of the times involved and details hard to check. Some of the simulations are checked with Berremann transfer matrix calculations and standard FDTD simulations.
An intelligent experimental is developed, making the effect observable.
Experimental and Theoretical comparison is convincing although it could be improved.

I find the idea and the results worth publication, they have high impact and will be appreciate by the community.
In the other hand, the text is difficult to read. The structure is complex and full of repetitions. Details, like inconsistent nomenclature and archaic language difficult the understanding. The authors should improve the presentation of their good results, maybe by looking for professional advice.

Specific comments:

In Fig 1. The authors show reflections of the spin-up electrons in (b) and (d). Could the authors comment briefly the reflection spectrum of spin down electrons? Is it comparable to the TE reflection of figure 4 (c) where there is no BIC signature?
Are all the BICs shown in Fig 1. FW BICs? Is it possible to find symmetry-protected ones? Where?

In Fig 2 and 3 \phi is plotted with an ellipse close to the incident TM-Wave. The text defines \phi as the orientation of the optical axis in the ADL. Plotting the scheme there is misleading in my opinion, as it might be confused with the polarization of the incident wave (always TM) or the returning light (elliptical in general).

The introduction of anisotropy mixes the TE and TM polarizations, as stated by the authors in the text. The calculations separate straight between TE/TM incident and TE/TM reflected light. The experiment sketched in Figure 7 of the supplementary information, though, has only a polarizer in the incident beam. Some light converts its polarization after reflection creating a background that blurs the effects one wants to observe. That means that after TM incidence, for example figure 4., there will be some light converted to TE, that is not eliminated and could eventually disturb the comparison, although it’s contribution should be small, almost neglectable.

The authors should clarify how they did the FDTD calculations, maybe just by saying they use commercial software of some sort. I suppose Lumerical by the looks of Fig 5 in the supplementary information.

There are plenty of typos scattered in the text (ex: 23?-25, porblem …) and the articles are missed in front of some words and forced in in some others.

Some letters are used in equations without definition.

Eq(7) of supplementary information (SI) has a typo (qs^2 should ks^2).

Amplitudes “a” and “b” of equations (10) and (11) of the SI should be redefined using a1,a2,b1 and b2 for every case…

Reviewer #2 (Remarks to the Author):

In this manuscript, the authors presented some theoretical and experimental results on bound states in the continuum (BICs) in 1D structures that depend on one spatial variable z. The theoretical results are close to, but not identical to those of Ref. [24], but this manuscript contains original experimental results. Overall, this is a valuable contribution to the understanding of novel physical phenomena associated with BICs which have important applications. The paper deserves to be published, but some revision is necessary. My main concern is that the manuscript is not well organized, and it is tedious and confusing to read. I would suggest the authors to consider revising the manuscript to improve the presentation.

(1) This is a manuscript without sections. If too many topics are put into this manuscript, it can be difficult to read. Is it really necessary to have the "quantum toy model"? The problems of 1DPhC+ADL+1DPhC, or 1DPhC+ADL+Metal, are easy to understand, there is no need to compare them with the quantum toy model. My suggestion is to remove the quantum toy model, and add back some supplementary materials. If the quantum toy model has important physical consequences, I would suggest the authors to write a separate paper.

(2) Introductory materials, the quantum toy model, 1DPhC+ADL+1DPHC, 1DPhC+ADL+Metal, should be well separated.

(2a) The first two paragraphs are introductory materials, but the 2nd paragraph on page 3 is also kind of introductory.

(2b) Somewhere near the top left column of page 3, there should be a new paragraph, to break the results on quantum toy model, and start to introduce 1DPhC+ADL models.

(2c) The 1DPhC+ADL+Metal structure is introduced in the 2nd paragraph of right column on page 3. But it only started from the 3rd sentence, with "In this study". I think a new paragraph should start with "In this study". The first two sentences should be rearranged.

Reviewer #3 (Remarks to the Author):

This manuscript presented Friedrich-Wintgen BICs by coupling polarizations. It is a nice work inspired by the analogy between optical polarizations and electron spins. I suggest minor improvements before it gets published:

1. It is still unclear to me why there are symmetry-protected BICs at \theta=0 and 90 degrees. To my knowledge, symmetry-protected BICs arise from the mismatch of the symmetry property between radiation channels and local resonances. It requires the system (including boundary conditions) to be subject to some geometric symmetry. Therefore, symmetry-protected BICs should be observed at normal incidence (kx=ky=0). But in the manuscript, incident-wave vector still has a non-zero kx component for Brewster angle, so the overall system is not symmetric. It

would be helpful if a physical explanation (in addition to the math) can be added in the main text or supplementary section D.

2. Metal loss lowers the upper limit of quality factor. But in supplementary Fig. 10, why is FW BIC's quality factor (\theta=40) obviously smaller than the quality factor of symmetry-protected BICs (\theta=0 & 90)? Since the presence of loss breaks the symmetry of the system, symmetry-protected BICs should be damaged more seriously than the relatively robust FW BIC. Yet the result implies the opposite. This is another reason for which I question whether theta=0 BICs are symmetry-protected or not. An analysis is needed to show whether material loss wipes out destructive interference.

3. Supplementary Fig.10's text refers to eq(10), which is obviously not the right equation for the figure. Also, the curves need a legend.

RESPONSE TO REFEREE 1

Reviewer 1 (Remarks to the Author):

General comment:

The manuscript by Pankin et al is an extensive work in which Bound States in the Continuum are found in 1D systems. The authors first describe a toy model showing BICs: an electron in a 1D potential. BIC signatures can be found by adding a magnetic field, that splits spin in energies, and changing its direction in the potential zone (tilted in the x-z plane) to couple the two spin modes. The model neglects orbital motion of the electrons in the magnetic field, but its perfectly illustrative. BICs are observed in the simulated reflection of spin up electrons as a function of energy and length of the potential and tilting angle of the magnetic field.

Author response:

We are grateful to the Referee for his/her high evaluation of a spin toy model. Currently we develop this model to apply to electron transmission in real structures.

General comment:

The main result of the paper is its photonic equivalent, that is designed, simulated and experimentally demonstrated.

Polarization takes the role of spin, and anisotropy the role of the magnetic field. An extensive analytical derivation of all the field profiles, couplings and reflection responses can be found in the supplementary information, thing that I appreciate, although it is most of the times involved and details hard to check. Some of the simulations are checked with Berremann transfer matrix calculations and standard FDTD simulations. An intelligent experimental is developed, making the effect observable. Experimental and Theoretical comparison is convincing although it could be improved. I find the idea and the results worth publication, they have high impact and will be appreciate by the community. In the other hand, the text is difficult to read. The structure is complex and full of repetitions. Details, like inconsistent nomenclature and archaic language difficult the understanding. The authors should improve the presentation of their good results, maybe by looking for professional advice.

Author response:
We are pleased very much by this comment.

Specific comments:
The Referee: In Fig 1. The authors show reflections of the spin-up electrons in (b) and (d). Could the authors comment briefly the reflection spectrum of spin down electrons? Is it comparable to the TE reflection of figure 4 (c) where there is no BIC signature? Are all the BICs shown in Fig 1. FW BICs? Is it possible to find symmetry-protected ones? Where?

Author response:
Owing to choice of the potential step ($U_0 = 20$) as depicted in Fig. 1a by green both spin channels are open in the central layer while outside only the spin up continuum is open for $E < B$. Therefore only the electron with spin up can propagate outside and participate in transmission and reflection by the central layer. The spin state with spin down in the central layer is orthogonal to the continuum state with spin up and therefore is the symmetry-protected (SP) BIC. In order to realize this SP BIC it is necessary to fulfil the energy preservation for spin down state $E = k_{\uparrow}^2 + B + U_0$ 2 if the magnetic field inside the defect layer is directed up, i.e., if $\phi = 0$. An alternative is $E = k_{\downarrow}^2 - B + U_0$ when the magnetic field inside the defect layer is directed down, i.e., $\phi = 180^o$.

The Referee: In Fig 2 and 3 ϕ is plotted with an ellipse close to the incident TM-Wave. The text defines ϕ as the orientation of the optical axis in the ADL. Plotting the scheme there is misleading in my opinion, as it might be confused with the polarization of the incident wave (always TM) or the returning light (elliptical in general).

Author response:
We thank the Referee for this comment. We have redrawn Figures 2a and 3a to eliminate a misleading.

The introduction of anisotropy mixes the TE and TM polarizations, as stated by the authors in the text. The calculations separate straight between TE/TM incident and TE/TM reflected light. The experiment sketched in Figure 7 of the supplementary information, though, has only a polarizer in the incident beam. Some light converts its polarization after reflection creating a background that blurs the effects one wants to observe. That means that after TM incidence, for example figure 4., there will be some light converted to TE, that is not eliminated and could eventually disturb the comparison, although it's contribution should be small, almost neglectable.

Author response:
We are grateful to the reviewer for this valuable comment. In fact, we did not use polarizer for reflected beam. The finite number of alternating layers in the PhC gives rise to a small leakage of TE-polarized light. We performed calculations by use of the Berreman method for photonic structure with polarizer and analizer, both. The attached figure 1 demonstrates that indeed the TE contribution is negligible for the frequency within the band gap.

The authors should clarify how they did the FDTD calculations, maybe just by saying they use commercial software of some sort. I suppose Lumerical by the looks of Fig 5 in the supplementary information.

Author response:
The results obtained by Berremans method were confirmed by the use of the Lumerical software. We have informed about that in Supplementary Information and Methods in the main text.

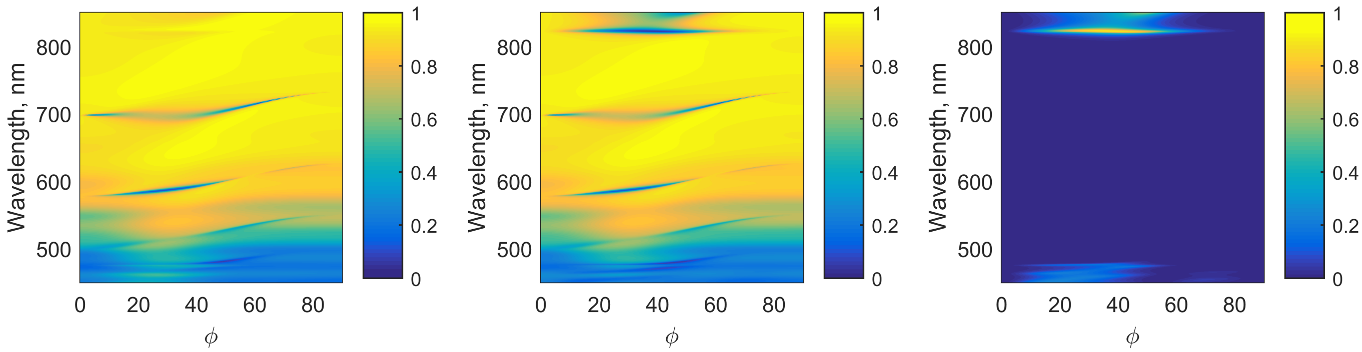

FIG. 1: Reflectanse spectra for TM-polarized incident light without analizer (left), with parallel analizer (TM-polarized reflectance only) (center) and cross analizer (TE-polarized reflectance only) (right).

There are plenty of typos scattered in the text (ex: 23?-25, porblem) and the articles are missed in front of some words and forced in in some others.

Author response:the typos have been corrected.

Some letters are used in equations without definition.

Author response: we have corrected the definitions.

Eq(7) of supplementary information (SI) has a typo (q_s^2 should k_s^2).

Author response: we have corrected Eq(9) in last version.

Amplitudes a and b of equations (10) and (11) of the SI should be redefined using a1,a2,b1 and b2 for every case

Author response: we have corrected Eqs (14-16) in last version.

RESPONSE TO REFEREE 2

Reviewer 2 (Remarks to the Author):
In this manuscript, the authors presented some theoretical and experimental results on bound states in the continuum (BICs) in 1D structures that depend on one spatial variable z. The theoretical results are close to, but not identical to those of Ref. [24], but this manuscript contains original experimental results. Overall, this is a valuable contribution to the understanding of novel physical phenomena associated with BICs which have important applications. The paper deserves to be published, but some revision is necessary. My main concern is that the manuscript is not well organized, and it is tedious and confusing to read. I would suggest the authors to consider revising the manuscript to improve the presentation.

Author response: We are thankful to the Referee for positive evaluation of our MS. However we would like to clarify the difference between the present MS and paper published in PRB 97, 24306 (2018) (citation [28] in present corrected MS). The principal difference is the type of BICs. In [28] the symmetry protected BICs were considered in the single continuum of 1D PhC arms specified by zero angle of incidence. This MS reports the Fridrich-Wintgen BICs due to full destructive interference of TE and TM polarizations in the ADL. Moreover in the present paper the PhC structure consists of only single PhC arm plus ADL terminated by metal mirror. That allowed to illuminate the structure under finite angle of incidence and respectively observe FW BICs experimentally.

(1) This is a manuscript without sections. If too many topics are put into this manuscript, it can be difficult to read.

Author response: We have introduced sections.

(2) Is it really necessary to have the "quantum toy model"? The problems of 1DPhC + ADL + 1DPhC, or 1DPhC + ADL + Metal, are easy to understand, there is no need to compare them with the quantum toy model. My suggestion is to remove the quantum toy model, and add back some supplementary materials. If the quantum toy model has important physical consequences, I would suggest the authors to write a separate paper.

Author response: Taking into account that the journal Nature Physics Communications is intended for wide circle of readers we consider that the quantum spin model is relevant even in such toy consideration. Moreover the spin model was highly appreciated by other Referees. However we agree that the spin model is to be considered separately with account of orbital motion. We are pleased that our plans coincide.

(2) Introductory materials, the quantum toy model, 1DPhC+ADL+1DPHC, 1DPhC+ADL+Metal, should be well separated.

Author response: We have corrected.

(2a) The first two paragraphs are introductory materials, but the 2nd paragraph on page 3 is also kind of introductory.

Author response: The editorial guidelines restrict the Introduction to less than 1000 words. Because of that we shifted some introductory material into the main text.

(2b) Somewhere near the top left column of page 3, there should be a new paragraph, to break the results on quantum toy model, and start to introduce 1DPhC+ADL models.

Author response: We have corrected.

(2c) The 1DPhC+ADL+Metal structure is introduced in the 2nd paragraph of right column on page 3. But it only started from the 3rd sentence, with "In this study". I think a new paragraph should start with "In this study". The first two sentences should be rearranged.

Author response: We have corrected

RESPONSE TO REFEREE 3

Reviewer 3 (Remarks to the Author):
This manuscript presented Friedrich-Wintgen BICs by coupling polarizations. It is a nice work inspired by the analogy between optical polarizations and electron spins. I suggest minor improvements before it gets published:

Author response:
We are highly thankful the Referee for high evaluation of our MS and careful reading not only the MS but Supplementary information.

1. It is still unclear to me why there are symmetry-protected BICs at $\theta = 0$ and 90 degrees. To my knowledge, symmetry-protected BICs arise from the mismatch of the symmetry property between radiation channels and local resonances. It requires the system (including boundary conditions) to be subject to some geometric symmetry. Therefore, symmetry-protected BICs should be observed at normal incidence (kx=ky=0). But in the manuscript, incident-wave vector still has a non-zero kx component for Brewster angle, so the overall system is not symmetric. It would be helpful if a physical explanation (in addition to the math) can be added in the main text or supplementary section D.

Author response: The specific choice of the 1D PhC consisted of alternating layers gives the band structure shown in Fig. 2(b). We can see that the TM continuum shown at left by red is open while the TE continuum shown at right by blue is closed for the Brewster angle of incidence. Then the TE state (with components E_y, H_x, H_z) in the ADL is the SP BIC irrespective to the choice of the TM continuum (with components H_y, E_x, E_z) specified by the angle θ owing to a geometry of structure.

2. Metal loss lowers the upper limit of quality factor. But in supplementary Fig. 10, why is FW BIC's quality factor ($\theta = 40$) obviously smaller than the quality factor of symmetry-protected BICs ($\theta = 0$ and 90)? Since the presence of loss breaks the symmetry of the system, symmetry-protected BICs should be damaged more seriously than the relatively robust FW BIC. Yet the result implies the opposite. This is another reason for which I question whether theta=0 BICs are symmetry-protected or not. An analysis is needed to show whether material loss wipes out destructive interference.

Author response:
We are very thankful to the Referee for this nice question. First, the losses affect the Q factor equally if to follow usual equation $1/Q = 1/Q_{BIC} + 1/Q_{losses} + 1/Q_{fluct}$ or $1/Q = 1/Q_{BIC} + 1/Q_{fluct}$ for the lossless case where the last contribution is a result of structural fluctuations, for example, imperfectness of ADL. In the case of metal mirror at the right side of the ADL the situation is more subtle. The losses in metal surface are result of skin effect when electromagnetic field penetrates by some depth into a metal (see attached Figure 2). For ideal case the metal surface would be exactly at nodal surface of the solution. However in real case this nodal surface is smeared altogether with the boundary condition for the BIC solution as shown in applied Figure. In other words, the metal mirror damages the boundary condition for the FW BIC and thereby slightly changes the BIC point. This effect is quite similar to damage of the FW BIC in experimental set up where the ADL has a curvature.
The same refers to the SP BIC whose solution also slightly penetrates into metal. However the metal does not change polarization. Thereby the TE solution remains orthogonal to the TM continuum at the left side and therefore does not lead to additional polarization leakage.

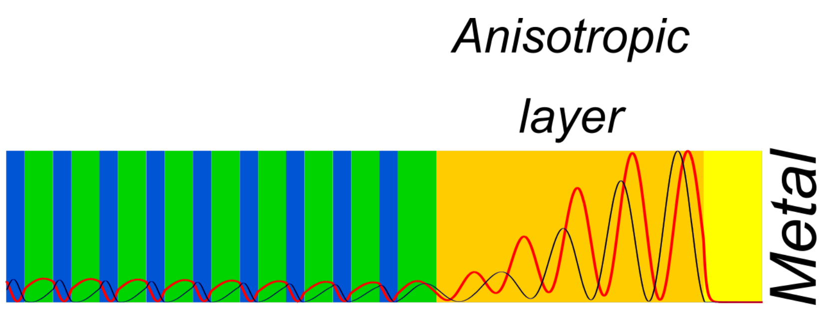

FIG. 2: Field distribution for TM-polarized wave near the FW BIC position for low loss metal $n_M = 0.14 + 20i$ (black) and for real case $n_M = 0.14 + 3.697i$ (red).

3. Supplementary Fig.10 text refers to eq(10), which is obviously not the right equation for the figure. Also, the curves need a legend.

Author response: We have corrected.

With our kindest regards,
Pavel Pankin, Bing-Ru Wu, Jhen-Hong Yang, Kuo-Ping Chen, Ivan V. Timofeev and Almas F. Sadreev.

REVIEWERS' COMMENTS:

Reviewer #1 (Remarks to the Author):

My technical questions have been answered appropriately.
My opinion about the quality of the results and the impact on the field remains high.

Although the presentation of the results can still be improved, I hope this can be done at the next stage, with the editors help. The language and part of the structure could be improved and simplify.

I recommend publication of the manuscript in its current form.

Reviewer #3 (Remarks to the Author):

The authors made one-by-one responses to the reviewers' comments and revised the manuscript accordingly. My questions about SP BICs and metal loss were fully answered. The paper is now ready for publishing.