Reviewers' comments:

Reviewer #1 (Remarks to the Author):

The paper of Gurtubay and Eiguren studies the magnetization induced by phonons
in a material with non-collinearity due to spin-orbit coupling. The paper is potentially interesting but needs substantial rewriting as several points are for me unclear. I outline here below the main weaknesses that should be improved to have a readable and complete paper.

1) The Magnetic oscillations the authors are referring to are similar (if not identical) to what proposed
by Qian Niu and coworkers, see Zhang and Niu, PRL 112, 085503 (2014), PRL 115, 115502 (2015).
In particular Eq. 4 (and the paragraph below Eq. 4) in PRL 112, 085503 (2014) is very similar to what the authors are proposing,
albeit in a model Hamiltonian approach. In these papers it is referred as spin-phonon coupling.
The authors should explicitly refer to these works and point out explicitly the relation between the two approaches. In particular, what is the relation between the variation of the SCF potential
and the Hamiltonian in Eq. 4 (and the discussion in below Eq 4 and in the two columns of the same page)
in PRL 112, 085503 (2014) and the current paper ?
It seems to me that
most of the claims done by the authors are already included in Niu's work, which reduces the novelty of the paper.

2) The authors should detail a bit how they obtain Eq. 2. In particular, I understood they perform a phonon linear response calculation with non-collinear magnetism (converging to a non magnetic state). This calculation
for a given q gives a static and complex four components delta V_q,scf. So the questions are:
(i) are the authors taking the modulus of the average of delta M or the square of the average of delta M ?

(ii) what are the author considering as time dependence in Eq. 2,a simple e^{i w_q nu t}? Is the symbol
< ... >_t meaning the average over a period ?

Reviewer #2 (Remarks to the Author):

The manuscript makes the case for an interesting new effect where phonons in nonmagnetic materials can give rise to spin textures in real space.
The finding has potentially implications for the fundamental understanding of electron-phonon interaction.

The authors motivate their work with an analogy to spin-orbit driven spin splitting at the surface of noble metals. In Au (111) this splitting has been measured via photo emission spectroscopy in 1996. The paper referenced by the authors states that this spin splitting depends critically on breaking of the inversion symmetry. In the Au(111) sample this breaking of inversion symmetry originates from the surface.
For their phonon driven effect the authors investigate the 2D material WSe2 which is surface only. However, I did not see an explicit mentioning for the need of broken spatial inversion. As key mechanism different electron-phonon interaction between time reversed electron states has been mentioned. If broken spatial inversion or surfaces are not required, this seems to be a significant difference to the effect in noble metals and should be discussed in more detail.

The authors make an effort to point out how this interesting effect can be measured. It would be useful to get an idea of the magnitude of the effect that can be expected when measuring, e.g., the dynamic structure factor.

Reviewer #3 (Remarks to the Author):

The manuscript reported that similarly to electrons in the presence of spin-orbit coupling, the propagating vibrational modes are also accompanied by a well-defined magnetic oscillation even in non-magnetic materials. Although this effect is illustrated by considering a single layer of the WSe2 dichalogenide, the phenomenon may be valid for any non-magnetic material with spin-orbit coupling. However, some issues should be carefully analyzed in detail.
1.The color map for the magnitude of in-plane magnetization in real space is missing in Figure 1.
2.Figure 3 shows that the direction of magnetization is perpendicular to the motion of W atoms. The authors should give more clear discussions about it. And why do the magnetizations for all phonon modes at q=Gamma become zero in Figs. 3b and 3c?
3.Overall, it seems that the acoustic branch can induce stronger magnetization than that of optical branch. This phenomenon should be discussed in detail.
4.The author claims that "The magnetic character associated inherently to phonons as proposed in this work is therefore accessible by means of any experimental setup probing the spin components of the dynamical structure factor in the energy ranges corresponding to phonons.". In fact, the temperature dependent dynamical structure factor was detected in MoS2 (Nano letters 15, 6889, 2015) with the structure similar to WSe2. So I encourage the authors to analyze the experimental results in MoS2 to further verify their theoretical calculation.

The manuscript in its present form cannot be accepted for publication.

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1) The Magnetic oscillations the authors are referring to are similar (if not identical) to what proposed by Qian Niu and coworkers, see Zhang and Niu, PRL 112, 085503 (2014), PRL 115, 115502 (2015). In particular Eq. 4 (and the paragraph below Eq. 4) in PRL 112, 085503 (2014) is very similar to what the authors are proposing, albeit in a model Hamiltonian approach. In these papers it is referred as spin-phonon coupling. The authors should explicitly refer to these works and point out explicitly the relation between the two approaches. In particular, what is the relation between the variation of the SCF potential and the Hamiltonian in Eq. 4 (and the discussion in below Eq 4 and in the two columns of the same page) in PRL 112, 085503 (2014) and the current paper ? It seems to me that most of the claims done by the authors are already included in Niu's work, which reduces the novelty of the paper.

Answer:
The physical property considered in PRL 112, 085503 (2014) and in our manuscript are completely different although both are related to magnetism and vibrational modes. In fact, both effects should coexist.

In PRL 112, 085503 (2014) the angular momentum due to the displacement of the atom out of the equilibrium is studied and Eq. 4 refers to a model hamiltonian for the crystal lattice (atoms) in a uniform magnetic field.

In our work we treat the response of the electron gas to the perturbation generated by a phonon of finite q in the crystal. In our theory the key point is that electrons with momentum k and -k couple differently to phonons with finite q and the existence of a finite population of spin polarization is due to the symmetry breaking within the Brillouin zone. Therefore, the angular momentum of individual atoms is not considered in relation to the magnetism as in PRL 112, 085503 (2014). For example, the spin response of the electron gas is null for a phonon with q=0 whereas PRL 112, 085503 (2014) would describe a finite contribution. It is clear that both works correspond to different physics.

The vector potential in Eq. 4 of PRL 112, 085503 (2014) describes the coupling of the angular momentum of individual atoms to an external magnetic field. Though interesting, we do not consider this contribution but a new one related to the response of the electron gas. We calculate the SCF potential due to the phonon propagation considering the full non-collinear response of the electron gas in first order of perturbation theory.

In order to make this clear we have added the following to the end of the long paragraph starting in page 3 (extending to the beginning of page 4)

“At this point it is worth to mention that the magnetic polarization of the electron gas as described in this manuscript does not have a relation with the angular momentum of the atoms as described by Zhang et al. in \cite{Niu2014}. In our theory linearly polarized phonons with null angular momentum give a finite and meaningful contribution to the magnetization. It is therefore clear that the physics described in \cite{Niu2014} is different and not connected to the spin response of the electron gas as described in the present work.”

2) The authors should detail a bit how they obtain Eq. 2. In particular, I understood they perform a phonon linear response calculation with non-collinear magnetism (converging to a non magnetic state). This calculation for a given q gives a static and complex four components delta V_q,scf.

The referee understood correctly. The variation of the 4-component density is

$$\delta \mathbf{n}_{\mathbf{q},\nu}(\mathbf{r},t) \quad = \quad \mathbf{Re}\left[\left(\delta\tilde{n}^0_{\mathbf{q},\nu}(\mathbf{r})\sigma_0 + \delta\tilde{\mathbf{M}}_{\mathbf{q},\nu}(\mathbf{r})\boldsymbol{\sigma}\right)e^{-i\omega_{\mathbf{q},\nu}t}\right] = \delta n^0_{\mathbf{q},\nu}(\mathbf{r},t)\sigma_0 + \delta\mathbf{M}_{\mathbf{q},\nu}(\mathbf{r},t)\boldsymbol{\sigma}.$$

Therefore, the magnetic fluctuation should be

$$\overline{\delta M^\alpha_{\mathbf{q},\nu}} = \sqrt{\int_\Omega \frac{d^3\mathbf{r}}{\Omega}\left(\langle \delta M^\alpha{}_{\mathbf{q},\nu}(\mathbf{r},t)^2\rangle_T - \langle \delta M^\alpha{}_{\mathbf{q},\nu}(\mathbf{r},t)\rangle_T^2\right)}.$$

But as the average of the trigonometric function is null

$$\langle \delta M^\alpha{}_{\mathbf{q},\nu}(\mathbf{r},t)\rangle_T^2 = 0\,,$$

we have simply that

$$\overline{\delta M^\alpha_{\mathbf{q},\nu}} = \sqrt{\int_\Omega \frac{d^3\mathbf{r}}{\Omega}\langle \delta M^\alpha{}_{\mathbf{q},\nu}(\mathbf{r},t)^2\rangle_T}.$$

Expanding the above equation considering the trigonometric terms and the real and imaginary components of the density response one obtains the following,

$$\begin{aligned}
\left(\overline{\delta M^\alpha_{\mathbf{q},\nu}}\right)^2 &= \int_\Omega \frac{d^3\mathbf{r}}{\Omega}\langle \delta M^\alpha{}_{\mathbf{q},\nu}(\mathbf{r},t)^2\rangle_T = \langle\langle \mathbf{Re}\left[\delta M^\alpha{}_{\mathbf{q},\nu}(\mathbf{r})e^{it\omega_{\mathbf{q},\nu}}\right]^2\rangle_T\rangle_r \\
&= \langle \mathbf{Re}\left[\delta M^\alpha{}_{\mathbf{q},\nu}(\mathbf{r})\right]^2\rangle_r\langle\cos^2\left(t\omega_{\mathbf{q},\nu}\right)\rangle_T + \langle \mathbf{Im}\left[\delta M^\alpha{}_{\mathbf{q},\nu}(\mathbf{r})\right]^2\rangle_r\langle\sin^2\left(t\omega_{\mathbf{q},\nu}\right)\rangle_T. \\
&= \frac{1}{2}\left(\langle \mathbf{Re}\left[\delta M^\alpha{}_{\mathbf{q},\nu}(\mathbf{r})\right]^2\rangle_r + \langle \mathbf{Im}\left[\delta M^\alpha{}_{\mathbf{q},\nu}(\mathbf{r})\right]^2\rangle_r\right)
\end{aligned}$$

Where we have taken into account that

$$\langle\cos^2(t\omega_{\mathbf{q},\nu})\rangle_T = \langle\sin^2(t\omega_{\mathbf{q},\nu})\rangle_T = \frac{1}{2} \quad ; \quad \langle\sin(t\omega_{\mathbf{q},\nu})\cos(t\omega_{\mathbf{q},\nu})\rangle_T = 0$$

Thought the calculations are correct there is typo in our Eq. 2 where the 1/2 was missing in the previous version of the manuscript. We have corrected it in the present version.

So the questions are:

(i) are the authors taking the modulus of the average of delta M or the square of the average of delta M ?

We take the modulus of the average of complex delta M which is the meaningful quantity as shown above.

(ii) what are the author considering as time dependence in Eq. 2,a simple e^{i w_q nu t}? Is the symbol < ... >_t meaning the average over a period ?

The referee is right and we have changed t with T in Eq. 2 in order to avoid confusion.

Reviewer #2 (Remarks to the Author):

The manuscript makes the case for an interesting new effect where phonons in nonmagnetic materials can give rise to spin textures in real space. The finding has potentially implications for the fundamental understanding of electron-phonon interaction.

The authors motivate their work with an analogy to spin-orbit driven spin splitting at the surface of noble metals. In Au (111) this splitting has been measured via photo emission spectroscopy in 1996. The paper referenced by the authors states that this spin splitting depends critically on breaking of the inversion symmetry. In the Au(111) sample this breaking of inversion symmetry originates from the surface. For their phonon driven effect the authors investigate the 2D material WSe2 which is surface only. However, I did not see an explicit mentioning for the need of broken spatial inversion. As key mechanism different electron-phonon interaction between time reversed electron states has been mentioned. If broken spatial inversion or surfaces are not required, this seems to be a significant difference to the effect in noble metals and should be discussed in more detail.

We are very grateful to Referee 2 for the positive comments and acknowledging the importance of our contribution.

The referee is right. The role of broken spatial inversion is to generate a non-trivial spin polarization and WSe2 is already non-centrosymmetric. The only requirement to obtain a meaningful spin response of the electron gas as we describe it in this manuscript is to have a spin polarization in the Brillouin zone and a phonon mode with finite momentum.

In the new version of the manuscript we mention this point and the importance of the absence of inversion symmetry at the end of page 2 where we have added the following:

" ...is completely general for crystals with spin-orbit coupling since the only requirement is a non-trivial pattern of the spin-polarization within the BZ associated to the absence of inversion symmetry [1]."

The authors make an effort to point out how this interesting effect can be measured. It would be useful to get an idea of the magnitude of the effect that can be expected when measuring, e.g., the dynamic structure factor.

We thank the referee for this comment. In fact the main effort in our manuscript it to show that the effect is meaningful. Comparing the scalar charge density (Fig. 2b) with the magnetic contributions (Fig. 2a) is the most straightforward way to see the expected effect. In the case of WSe2 we find that the magnetic contributions only one order of magnitude smaller than the scalar one, as mentioned in the main paragraph of page 3 (it is actually only 5 times smaller).

Reviewer #3 (Remarks to the Author):

The manuscript reported that similarly to electrons in the presence of spin-orbit coupling, the propagating vibrational modes are also accompanied by a well-defined magnetic oscillation even in non-magnetic materials. Although this effect is illustrated by considering a single layer of the WSe2 dichalogenide, the phenomenon may be valid for any non-magnetic material with spin-orbit coupling. However, some issues should be carefully analyzed in detail.

We are very grateful to Referee 3 for acknowledging the relevance of our work.

1. The color map for the magnitude of in-plane magnetization in real space is missing in Figure 1.

To avoid confusion with the colorbar of the displacement of the W atom in the z direction, we have intentionally omitted this colorbar. The arrows have been scaled to make them visible in the unit cell. We have modified the caption of the figure as follows:

“The coloured vector field is proportional to the in-plane magnetization at each point in real space, with yellow/light (blue/dark) arrows representing the largest (smallest) values. These arrows as well as the displacements of the Se atoms have been scaled to make them visible in the unit cell.”

2. Figure 3 shows that the direction of magnetization is perpendicular to the motion of W atoms. The authors should give more clear discussions about it.

The main paragraph of page 3 discusses in detail all the atomic polarization-vector dependence and many other details related to physical phenomena described in the present work. We would be grateful to the Referee if he suggested any additional point he considers relevant but missing in the present version.

And why do the magnetizations for all phonon modes at q=Gamma become zero in Figs. 3b and 3c?

At q=Gamma the spin contributions from k and -k cancel each other, implying no net magnetization. A phonon with a finite q propagating in the crystal breaks this symmetry and is the origin of the whole physics described in our work.

3.Overall, it seems that the acoustic branch can induce stronger magnetization than that of optical branch. This phenomenon should be discussed in detail.

The distinction of acoustic and optical modes is meaningful only for small momenta where all modes give a similar contribution in order of magnitude. In the border of the BZ the lowest energy modes (already without acoustic character) give a significant contribution but this is related mainly to the importance of the components of W to the polarization vector as mentioned in the caption of Fig. 3. The arrows in the insets of Figs 3b and 3c (for q=K) are proportional to the atomic displacements of the atoms. The larger the displacement of the atom (as it happens in the acoustic branches), the larger the magnetization the corresponding phonon induces.

We understand that in this journal the captions of figures play an important role and caption of Fig.3 discusses this point but if the referee or the Editor considers it relevant we could add an additional comment in the main text.

4.The author claims that "The magnetic character associated inherently to phonons as proposed in this work is therefore accessible by means of any experimental setup probing the spin components of the dynamical structure factor in the energy ranges corresponding to phonons.". In fact, the temperature dependent dynamical structure factor was detected in MoS_2 (Nano letters 15, 6889, 2015) with the structure similar to WSe_2. So I encourage the authors to analyze the experimental results in MoS_2 to further verify their theoretical calculation.

The manuscript in its present form cannot be accepted for publication.

The experiment described in Nano letters 15, 6889, 2015 use femtosecond electron scattering as an atomic-scale probe to scan large amplitude in-plane and out-of-plane atomic displacements. The use of a new source of relativistic femtosecond electron pulses enables the study of weakly scattering monolayers, such as MoS_2. However, although the authors suggest the possibility of controlling the electronic and optical responses coupled to the atomic displacements, the measurements reported in there don't seem to probe the spin components of the electronic response associated to those structural changes.

We find that Fig. 3 of Nano letters 15, 6889, 2015 is the only place where the authors mention the dynamical structure factor but there is not any relevant information in the typical energy scales of phonons. In this sense an important goal of our manuscript is to guide experimental groups to design new experiments which could probe the physics described in our theory.

Reviewers' comments:

Reviewer #1 (Remarks to the Author):

The authors correctly addressed the difference with the work of Zhang et al. and replied to all questions raised by the referees. The paper can now be accepted for publication.

Reviewer #3 (Remarks to the Author):

The authors have revised the manuscript according to the comments from the reviewers. But the authors claimed that an important goal of their manuscript is to guide experimental groups to design new experiments which could probe the physics described in the theory. To improve the quality and novelty of the manuscript, I thus strongly suggest the authors do the cooperative study with experimental groups to confirm the physics described in the author's theory.

Reviewer #4 (Remarks to the Author):

Review Report
Re: COMMSPHYS-19-0189A-Z
Magnetic oscillations induced by phonons in non-magnetic materials
by Idoia G. Gurtubay, Asier Eiguren

In this manuscript, the authors found that similar to electrons when spin-orbit coupling exists, phonons are accompanied by magnetic oscillations even in non-magnetic systems. This seems to be a very interesting job, but I have a few questions about this article and hope the authors consider it:
(1) The authors declare the universality of the conclusion, but the WSe_2 applied here is a lattice with high symmetry. What are the similarities and differences in other complex lattices, can the authors give a brief statement?
(2) Γand M points are invariant under time reversal symmetry. In Fig.3 (b), onlyΓpoint has zero magnetization, but in Fig.3 (c), bothΓand M points have zero magnetization, can they explain it?
(3) Fig.3 (b) and Fig.3 (c) represent the magnetization in the plane and perpendicular to the plane, respectively. Why the distribution of point M to K and K' is symmetrical, whileΓ to K and K' is not, resulting in the net magnetization mentioned by the authors?
(4) The authors mention that the system can generate a net magnetization, whether there is a relevant experiment, can they discuss the net magnetization comparing with the experiment?

Reviewers' comments:

Reviewer #1 (Remarks to the Author):

The authors correctly addressed the difference with the work of Zhang et al. and replied to all questions raised by the referees. The paper can now be accepted for publication.

We are very grateful to the Referee for the careful study our work.

Reviewer #3 (Remarks to the Author):

The authors have revised the manuscript according to the comments from the reviewers. But the authors claimed that an important goal of their manuscript is to guide experimental groups to design new experiments which could probe the physics described in the theory. To improve the quality and novelty of the manuscript, I thus strongly suggest the authors do the cooperative study with experimental groups to confirm the physics described in the author's theory.

We are very grateful to the referee for appreciating the novelty of our contributions and the recommendation to collaborate with research teams in the experimental field.

Our manuscript gives the clues to observe this phenomenon in an experimental way, since the dynamic structure factor is accessible by means of several experimental techniques (inelastic xray scattering, neutron etc.) as we mentioned in our work. It is worth mentioning that we have already been in contact with some members of the research group “Structure and microscopic properties in solids” in our faculty (http://www.cryst.ehu.es/), and particularly with Prof. Gotzon Madariaga, who is a specialist in X-ray scattering techniques. We hope this will serve us to confirm the phenomenon. In a second step we are planing to contact some group working with the measurement of magnetic properties of coherent phonons.

Reviewer #4 (Remarks to the Author):

In this manuscript, the authors found that similar to electrons when spin-orbit coupling exists, phonons are accompanied by magnetic oscillations even in non-magnetic systems. This seems to be a very interesting job, but I have a few questions about this article and hope the authors consider it:

We thank the referee for carefully studying or manuscript and appreciating the novelty of the Physics explored in this work. Please find the replies of the questions below each inquiry.

(1) The authors declare the universality of the conclusion, but the WSe2 applied here is a lattice with high symmetry. What are the similarities and differences in other complex lattices, can the authors give a brief statement?

The phenomenon has its origin in the fact that phonons with finite momentum (q) interact differently with time reversed electron momenta. This continues being correct even in the total absence of any crystal symmetry, and depending on the system, we expect that it could even favor it.

(2) Γ and M points are invariant under time reversal symmetry. In Fig.3 (b), only Γ point has zero magnetization, but in Fig.3 (c), both Γ and M points have zero magnetization, can they explain it?

Phonons generate a spin dependent perturbation which is different for electrons at time reversed momenta, and therefore behave effectively as an external magnetic field by lifting the Krammers degeneracy. This happens only when spin-orbit coupling and a perturbation with finite wave-length are present, because in this way the change of the spin polarization is not compensated within the Brillouin zone (BZ).

In fact, all points of the BZ contribute to the induced spin-density,

$$\rho_{\mathbf{q}}^{\sigma,\sigma'}(\mathbf{r}) \sim \sum_{\mathbf{k}} \frac{g_{\mathbf{k+q,k}}}{E_{\mathbf{k}} - E_{\mathbf{k+q}}} \Psi_{\mathbf{k}}^{\sigma}(\mathbf{r})^* \Psi_{\mathbf{k+q},k}^{\sigma'}(\mathbf{r}) = \begin{pmatrix} \rho_{\mathbf{q}}(\mathbf{r}) + \delta m_{\mathbf{q}}^z(\mathbf{r}) & \delta m_{\mathbf{q}}^x(\mathbf{r}) - i\delta m_{\mathbf{q}}^y(\mathbf{r}) \\ \delta m_{\mathbf{q}}^x(\mathbf{r}) + i\delta m_{\mathbf{q}}^y(\mathbf{r}) & \delta\rho_{\mathbf{q}}(\mathbf{r}) - \delta m_{\mathbf{q}}^z(\mathbf{r}) \end{pmatrix}$$

and in principle nothing special can be deduced for TR invariant q points. The time reversal operation guarantees that the induced magnetization reverses in the same way as for electrons,

$$\Theta\left[\delta\rho_{\mathbf{q}}^{\sigma,\sigma'}(\mathbf{r})\right] = \begin{pmatrix} \rho_{\mathbf{q}}(\mathbf{r})^* - \delta m_{\mathbf{q}}^z(\mathbf{r})^* & -\delta m_{\mathbf{q}}^x(\mathbf{r})^* + i\delta m_{\mathbf{q}}^y(\mathbf{r})^* \\ -\delta m_{\mathbf{q}}^x(\mathbf{r})^* - i\delta m_{\mathbf{q}}^y(\mathbf{r})^* & \delta\rho_{\mathbf{q}}(\mathbf{r})^* + \delta m_{\mathbf{q}}^z(\mathbf{r})^* \end{pmatrix},$$

but we can not state anything general for TR invariant q points (other than Γ). This is already stated in the paragraph between equation (1) and (2).

(3) Fig.3 (b) and Fig.3 (c) represent the magnetization in the plane and perpendicular to the plane, respectively. Why the distribution of point M to K and K' is symmetrical, while Γ to K and K' is not, resulting in the net magnetization mentioned by the authors?

We thank the referee for this question as we were not sufficiently clear in this point. The induced magnetization is a Bloch wave including a periodic part,

$$\vec{M}_{\mathbf{q},\nu}(\mathbf{r}) = \vec{m}_{\mathbf{q},\nu}(\mathbf{r})e^{i\mathbf{qr}} \quad , \quad \vec{m}_{\mathbf{q},\nu}(\mathbf{r}) = \vec{m}_{\mathbf{q},\nu}(\mathbf{r}+\mathbf{R})$$

For visualization purposes we needed to include the full Bloch wave (as in Fig. 1 and Fig. 2) but the key quantity in the structure factor ($S(\mathbf{q}+\mathbf{G},\omega)$) is the periodic part $\delta\vec{m}_{\mathbf{q},\nu}(\mathbf{r})$. The $\mathbf{G}$ components of $S(\mathbf{q}+\mathbf{G})$, $\int d^3\mathbf{r} e^{-i(\mathbf{q}+\mathbf{G})\mathbf{r}} \delta M_{\mathbf{q},\nu}^{\alpha}(\mathbf{r}) = \int d^3\mathbf{r} e^{-i\mathbf{Gr}} \delta m_{\mathbf{q},\nu}^{\alpha}(\mathbf{r})$, express the local field details inside a unit cell.

This quantity is the same in the $\Gamma - K$ and the $\Gamma - K'$ directions and fulfills all the rest of expected symmetries. Even though the Bloch wave is periodic in reciprocal space, $\vec{M}_{\mathbf{q},\nu}(\mathbf{r}) = \vec{M}_{\mathbf{q+G},\nu}(\mathbf{r})$, we admit it may result misleading for representation in Fig. 3, as the extra phase affects the Bloch wave in a non-trivial way and causes an apparent lack of symmetry. For this reason and because it is the periodic part the one which enters the structure factor, we have represented this last quantity in the new version of our Fig. 3.

(4) The authors mention that the system can generate a net magnetization, whether there is a relevant experiment, can they discuss the net magnetization comparing with the experiment?

Strictly speaking, the real space oscillations produce a vanishing net magnetization due to the sinusoidal modulation. However, the effect is appreciable at microscopic scale as a fluctuation of the microscopic magnetization as expressed with the dynamical structure factor which is accessed by many experimental probes (Inelastic X-ray, neutron scattering, IEELS ect.). We also mention in the beginning of page 3 that the magnetization connected with phonons, as described in this article, may be directly traced to classical (coherent) phonons and probably a direct measurement may be possible also in this sense. Although we plan to continue investigating in this direction, we consider this subject is beyond the scope of this work.

REVIEWERS' COMMENTS:

Reviewer #4 (Remarks to the Author):

The authors found a well-defined magnetic oscillation by phonons even in non-magnetic materials, which can be generilized for any non-magnetic material with spin-orbit coupling. The emerging phonon-induced magnetic oscillation is a very interesting job, and they also address the questions from refereees, thus I suggest that it can be accepted for publication.