Reviewers' comments:

Reviewer #1 (Remarks to the Author):

The submitted manuscript presents a theoretical discussion about the existence of 3D superinsulators, which are predicted on the basis of a Bose condensate of non-elementary magnetic monopoles.
The formalism the authors use goes beyond the classical limit, and is based on standard techniques (and approximations) in the lattice formulation of quantum field theory.
The manuscript addresses a topic of interest, is written well, and the mathematical details of the work are discussed clearly.
As a minor suggestion, I was wondering if the authors could add a few brief statements about the dependence of their findings on the type of lattice that they are considering. In the lattice regularization of QCD and other high-energy theories, the lattice is not taken to be a physical structure, and is removed at the end of the calculation (by sending the lattice spacing to zero, i.e. the cutoff scale at which discretization effects become manifest to infinity) and lattices of different geometry are expected to give the same results in the continuum limit. Here, instead, if I understand correctly the discussion at the beginning of the "Phase transitions and phase diagram" section, the scale l is supposed to be physical. Hence it would be interesting to know if materials with a different underlying lattice structure can give rise to different physics.

Reviewer #2 (Remarks to the Author):

The article is written by three established physicists, two high energy field theorists and a condensed matter theorist. The field-theoretical part looks good but its relevance to condensed matter physics is obscure. The words said in the article assert that such relevance exists but they do not help to establish it. Starting with Eq. (1) no connection of vector and tensor variables that appear in the equations to the observable quantities has been provided. What are the proposed monopoles in material science language? What vortices are authors talking about? What strings? There appears to be no connection between mathematics of the paper and material science.

The paper should be revised as follows. The physical system must be described in the introduction. The words "Bosonic insulator" and references to condensed matter papers are not sufficient. Variables describing the system, such as vectors and tensors a, b, q, m, f, k, h, etc. that appear in the equations must be thoroughly explained and traced to measurable quantities. Indium oxide films have been mentioned in the Discussion section as candidates for predicted effects. The authors must explain how these effects can be observed, making a direct connection between their calculations and possible experiment? This is the only way to visualize and understand what they have done.

Reviewer #3 (Remarks to the Author):

In the manuscript "Quantum magnetic monopole condensate" by M.C.Diamantini, C.A.Trugenberger and V.M.Vinokur it is demonstrated that quantum magnetic monopoles can emerge as quasiparticle excitations in bosonic insulators. Authors argue that at low temperature these monopoles condense and drive system into superinsulating phase which is dual to ordinary superconducting state with condensate of Cooper pairs.

In general, the manuscript is well written and present a self-contained picture of the proposed monopole condensation scenario. However, three points remain unclear for me after the careful reading of the manuscript:

1. The first point is related to the physical meaning of the used degrees of freedom. What physics stays behind the Lagrangian (2) and how it is related to the ordinary description of a superconductor? Especially in connection with InO mentioned in the text.

2. The another point is related to the manifestation of monopole condensation on the experiment. What should be measured in order to separate three phases on Figure 2 and to evidentiate the condensate of monopoles?

3. The last point is about the main result eq. (7). What is the value of G(0) which enters the exponent? It seems that it can be evaluated for simple lattices. Why is it not infinite as for usual Coulomb potential? Does this quantity depend on lattice spacing and what physics stays behind this dependency?

Summarizing all stated above i think this manuscript can be published in Communications Physics if the Authors clarify these three points.

Point-by-point replies to Reviewers

Reviewer #1 (Remarks to the Author):

The submitted manuscript presents a theoretical discussion about the existence of 3D superinsulators, which are predicted on the basis of a Bose condensate of non-elementary magnetic monopoles. The formalism the authors use goes beyond the classical limit and is based on standard techniques (and approximations) in the lattice formulation of quantum field theory. The manuscript addresses a topic of interest, is written well, and the mathematical details of the work are discussed clearly.

As a minor suggestion, I was wondering if the authors could add a few brief statements about the dependence of their findings on the type of lattice that they are considering. In the lattice regularization of QCD and other high-energy theories, the lattice is not taken to be a physical structure, and is removed at the end of the calculation (by sending the lattice spacing to zero, i.e. the cutoff scale at which discretization effects become manifest to infinity) and lattices of different geometry are expected to give the same results in the continuum limit. Here, instead, if I understand correctly the discussion at the beginning of the "Phase transitions and phase diagram" section, the scale l is supposed to be physical. Hence it would be interesting to know if materials with a different underlying lattice structure can give rise to different physics.

Answer to Reviewer 1:

We are most happy that Reviewer #1 finds our work interesting and well written. S/he raises an excellent point which is of a prime importance but was not articulated sufficiently. Reviewer justly points out that the scale \ell in our model is a physical high energy cutoff and not only a regularization to be removed at a critical point. However, this scale is not related to crystalline lattice structure of a specific material. The original discrete model in which superinsulation was predicted was the square Josephson junction array (JJA), Reference [7] of our manuscript. The regularization scale in this case naturally appears as a size of the JJA plaquette. It was established in Fistul et al, PRL 100, 086805 (2008) (and then confirmed in many experiments) that films in the critical vicinity of the superconductor-insulator transition (SIT) acquire self-induced electronic granularity, i.e. turn effectively into a JJA in which the superconducting islands play the role of the granules connected by Josephson links. In this self-induced 'JJA's the universal characteristic plaquette size (i.e. the distance between the centers of superconducting islands coupled by Josephson links) is of order of the superconducting coherent length \xi. The crossover to continuous model is realized not by sending \xi to zero but on the contrary, when considering large spatial scales on which granularity becomes nonessential. That at low temperatures granular conductors are equivalent to continuous conducting media, was rigorously proved, see the review, Beloborodov *et al*, Rev. Mod. Phys. **79**, 469 (2007). The equivalence of JJA on large distances to superconducting films and relations between the granular characteristics and effective film's parameters are discussed, for example, in M. Tinkham's textbook, Introduction to Superconductivity, McGraw Hill (1996), and the geometry of the initial JJA manifests only in numerical factors relating these quantities. As a result, the very fact of the emergence of the monopole condensate which is the focus of the present work is not affected by the particular geometry of the JJA. Of course, the particular universality class of the Mott insulator (i.e. superinsulator) depends on the geometry. Furthermore, it will be most appealing to consider the model on other lattices, e.g. granular structures with frustration. This will bring in additional effects which add on top of the main properties of the superinsulating ground state discussed here. These interesting questions will be a subject of the forthcoming researches but for now they go well beyond the scope of the present work.

Reviewer #2 (Remarks to the Author):

The article is written by three established physicists, two high energy field theorists and a condensed matter theorist. The field-theoretical part looks good but its relevance to condensed matter physics is obscure. The words said in the article assert that such relevance exists, but they do not help to establish it. Starting with Eq. (1) no connection of vector and tensor variables that appear in the equations to the observable quantities has been provided. What are the proposed monopoles in material science language? What vortices are authors talking about? What strings? There appears to be no connection between mathematics of the paper and material science.

The paper should be revised as follows. The physical system must be described in the introduction. The words “Bosonic insulator” and references to condensed matter papers are not sufficient. Variables describing the system, such as vectors and tensors a, b, q, m, f, k, h, etc. that appear in the equations must be thoroughly explained and traced to measurable quantities. Indium oxide films have been mentioned in the Discussion section as candidates for predicted effects. The authors must explain how these effects can be observed, making a direct connection between their calculations and possible experiment? This is the only way to visualize and understand what they have done.

Answer to Reviewer #2:

Reviewer #2 raises a very important point. Before addressing the technical aspects of her/his comment, we would like to communicate that we are extremely pleased that Reviewer #2 attested our team as a team of established physicists. On a personal level, nothing can be more rewarding than recognition of our accomplishments by the recognized expert in the field.

Turning to technical detail, we note that being an expert, Reviewer #2 is certainly aware of our preceding works, quoted as References [10,11] of the present manuscript. Reference [10] constructed a field theory of the superinsulating state as a state where Cooper pairs are bound by the Polyakov’s electric strings connecting them and describes how Dirac’s monopoles arising in disordered superconducting films shape these strings. Reference [11] presents, in turn, the experimental measurements of physical characteristics of these strings. The fact that Ref. [10] was an important component which won the 2020 London Prize for one of the authors (one can find this mentioning on the Communication Physics website) certifies that the concepts discovered and introduced in that paper are recognized and widely accepted in the community. This explains why we decided to be brief with the already published and recognized results but focus on the present discovery, the fundamental importance of which goes, in our opinion, well beyond its immediate context. Namely, that magnetic monopoles that arise in Josephson junction arrays and superconducting films in the critical vicinity of the superconductor-insulator transition, form a **quantum Bose condensate**. This is a theoretical result for the derivation of which we employed an exemplary model (which allows for physical realization), the Josephson junction array.

The important feature of our theory is that it is a phenomenological long-wave theory based on the symmetries of the low-energy degrees of freedom. In a sense, it describes superconductor-insulator transition and the emerging phases in the same manner in which Ginzburg-Landau theory describes superconductivity. Reviewer #2 justly wishes to see also a microscopic theory of the superinsulation which would have related phenomenologically introduced GL parameters in the same manner as the BCS and subsequent Gorkov's theories did it for the superconductivity. This is a nice and ambition project, and we appreciate the insightful Reviewer's suggestion. The completion of this proposal goes well beyond the scope of the present manuscript restricted to the GL-like description and will become the subject of future publications.

Nevertheless, since the non-specialist reader may not indeed be aware of the details well known to experts, we articulated better the employed concepts. All the physical quantities that are described by gauge fields have already been defined and explained in the paragraph below Eq. (1), which is the standard action for the bosonic topological insulator extensively used in the literature. In the revised text we expanded the explanations. We have also included a more thorough discussion of the experimental consequences to make our results more graphic, as the referee's aptly points out.

Reviewer #3 (Remarks to the Author):

In the manuscript "Quantum magnetic monopole condensate" by M. C. Diamantini, C. A. Trugenberger and V. M. Vinokur it is demonstrated that quantum magnetic monopoles can emerge as quasiparticle excitations in bosonic insulators. Authors argue that at low temperature these monopoles condense and drive system into superinsulating phase which is dual to ordinary superconducting state with condensate of Cooper pairs.

In general, the manuscript is well written and present a self-contained picture of the proposed monopole condensation scenario. However, three points remain unclear for me after the careful reading of the manuscript.

Answer to Reviewer #3

We are most happy that Reviewer evaluates our paper as well written and presenting the self-contained picture of the monopole condensing. Reviewer #3 raises three excellent points. We now address her/his detailed technical comments.

1. Reviewer #3

The first point is related to the physical meaning of the used degrees of freedom. What physics stays behind the Lagrangian (2) and how it is related to the ordinary description of a superconductor? Especially in connection with InO mentioned in the text.

Answer to Reviewer #3:

We thank Reviewer for this comment. Indeed, it is insufficiently explained how Eq. (2) relates to the ordinary description of a superconductor. We omitted this description since it is contained in all details in our previous publication, Ref. [10] of the manuscript, and since we at first thought that excessive detail may distract the reader from the main focus of the present work. Essentially, when the electric current $j^{\mu} = (1/2\pi)= h^{\mu}$ defined in the paragraph below Eq. (1) is minimally coupled to the electromagnetic gauge potential A_{μ} as a an additional term $A_{\mu} j^{\mu}$ in Eq. (2) in the phase with a charge condensation ($m_{\mu\nu} =0$, q_{μ} summed over in the partition function) and all matter fields are integrated out to obtain the electromagnetic response, one obtains a gauge field mass term $\propto A_{\mu}A^{\mu}$ which implies the induced current $j^{\mu} \propto A_{\mu}$, i.e. the London equations. Although we were concerned that repeating what was described before might appear redundant, we completely rely on Reviewer's educated feeling. We like to thank Reviewer #3 for this suggestion and have introduced the corresponding brief paragraph to outline this proof, referring to our earlier work, to make our present paper more accessible for non-specialist reader.

2. Reviewer #3

Another point is related to the manifestation of monopole condensation on the experiment. What should be measured in order to separate three phases on Figure 2 and to evidence the condensate of monopoles?

Answer to Reviewer #3:

This important point is crucial, and we thank Reviewer for bringing this to our attention since its discussion in the text is not sufficient. A lion share of the experimental research in the systems exhibiting the superconductor-insulator transition (SIT) is done on the superconducting side of the SIT. The major problem with the experiments with superinsulators is the well below 100 mK temperature range in which the measurements are to be done to make sure that the system resides in the superinsulating state. The Reviewer's question is to be split into two separate issues: (i) Evidence for the phase diagram of Fig. 2 and (ii) Evidence of the monopole condensate.

The structure of the phase diagram can be viewed as reliably established in previous experiments, Refs. [17,18] of the revised version of the manuscript. Reference [17] identifies charge Berezinskii-Kosterlitz-Thouless (BKT) transition into the confined low-temperature superinsulating state, and Ref. [18] presents the measurements of the electromagnetic response of the superinsulating phase and of properties of electric strings. In particular, the double kinks in the *I*-*V* characteristics was identified there indicated the predicted electric Meissner effect characteristic to superinsulators. All the results were in a pretty fair agreement with the theoretical predictions. The existence of a bosonic insulator in the same samples was unambiguously established in the recent publication, Diamantini *et al*, *Physics Letters* A **384**, 126570 (2020). Comparison of the direct SIT scenario for TiN films corresponding to smaller values of the parameter \eta with the SIT going via the bosonic insulator in NbTiN films (with larger \eta) investigated in the above publication also complies with the predictions. Further similar investigations of both dc and ac responses of the superinsulators in different systems are in order, but the study of the ac response poses an experimental challenge and is not yet carried out. To conclusively evidence the Cooper pair-based nature of the bosonic insulator the shot noise measurements similar to those carried out in Zhou *et al*, *Nature* **572**, 493 (2019) are desirable. The challenge there is that so far this kind of measurements was done temperatures about 30 K and above, while the reliable studies of bosonic insulators should go into the below Kelvin temperature range. This is the task for the future, and we expect that our publication will stimulate various experimental groups to undertake the task.

The detection of the monopoles, on the other hand, is a daunting task. So far only one reliable experiment that identified monopoles using a SQUID-on-tip device in the graphene quantum Hall system has been carried out, Uri *et al*, Nature Physics **16**, 164 (2020). To carry out the similar study of the superinsulator, experimentalists should learn to do this kind of experiments at temperatures below 100 mK (the measurements on graphene were taken at 300 mK).

While extremely interesting, the excessively detailed discussion of already accomplished and planned in the future possible experiments might take us well away from communication of our major discovery. We, however, fully agree with the Reviewer that such a discussion is necessary having thus added a focused paragraph to discuss the benchmarking and smoking gun experimental signatures of the monopole condensate. We are most grateful to Reviewer for pointing out this deficiency of our manuscript.

3. Reviewer #3

The last point is about the main result eq. (7). What is the value of *G*(0) which enters the exponent? It seems that it can be evaluated for simple lattices. Why is it not infinite as for usual Coulomb potential? Does this quantity depend on lattice spacing and what physics stays behind this dependency?

Answer to Reviewer #3:

The value of G(0) for a square lattice is 0.155. It is not infinite because the Coulomb potential in the 4D Euclidean space has no infrared divergence and its ultraviolet divergence is regularized by the lattice. The exact value of the numerical constant G(0) is not crucial for our considerations based on an effective field theory valid at low energies. We can give an estimate of the string tension but computing an exact value requires a full microscopic theory of superinsulation that is beyond the scope of the present work. We have added a comment on that into the manuscript.

Reviewers' comments:

Reviewer #1 (Remarks to the Author):

The Authors revised their manuscript according to the suggestions. I recommend this version of the manuscript for publication.

Reviewer #2 (Remarks to the Author):

The authors' response to my criticism is misleading. They are trying to create an impression that ambiguities in the manuscript can be resolved if the reader goes to their previous publications on the superinsulating state, but publications they refer to contain same kind of ambiguity. To have their theory resonate with the readers, the authors should try to spell out its relevance of to the physics of superconductors with a textbook clarity.

My main concern has been the absence of any obvious connection between the field-theoretical part of the paper and condensed matter physics. The authors' revision states:
"Relating the phenomenological parameters of the theory to microscopic material characteristics rests on a microscopic theory of superinsulation which is beyond the scope of the present work."

I agree that the conceptual framework of the manuscript may be not easy to relate to a concrete material but it is not what I asked. The authors must be able to show how to connect it to the topological objects that exist in material science, superconductivity in particular. Words like duality, Polyakov strings, Noether charges, topological solitons, instantons, etc. do not help. They make the paper look like a mathematical exercise. Refs. 3,4 of the manuscript that discuss monopoles in spin ice serve as an example. They are written in a manner that is easy to comprehend and relate to experiment. This should be the goal of the manuscript. Objects shown in Fig. 1 must be explained in terms familiar to condensed matter physicists.

Reviewer #3 (Remarks to the Author):

I carefully read the revised version of the manuscript "Quantum magnetic monopole condensate" by M.C.Diamantini, C.A.Trugenberger and V.M.Vinokur and Authors response to reviewers comments. In my opinion, the second version of the manuscript is more clear and now suitable for the publication in Communications Physics. So I recommend Editor to accept the manuscript in the present form.

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Point by point reply to Reviewers' comments

Reviewer #2 (Remarks to the Author):

1. Reviewer: The authors' response to my criticism is misleading. They are trying to create an impression that ambiguities in the manuscript can be resolved if the reader goes to their previous publications on the superinsulating state, but publications they refer to contain same kind of ambiguity. To have their theory resonate with the readers, the authors should try to spell out its relevance of to the physics of superconductors with a textbook clarity.

While we strongly insist that there were no ambiguities even in the original version of the manuscript, we have used an additional opportunity and made further efforts to enhance articulating the relevance of our findings to the physics of superconductors. We have added an extra piece into the introductory section and spelled out clearly that we predict a new class of 3D superconductors with emergent electronic granularity in which monopoles play a crucial role. Finally we have added a paragraph in the final discussion to explain even more the relevance of our ideas for layered materials.

2. Reviewer: My main concern has been the absence of any obvious connection between the field-theoretical part of the paper and condensed matter physics. ... The authors must be able to show how to connect it to the topological objects that exist in material science, superconductivity in particular. Words like duality, Polyakov strings, Noether charges, topological solitons, instantons, etc. do not help. They make the paper look like a mathematical exercise.

While we strongly disagree with the Reviewer, we hope that the new section and paragraphs will help to make this connection clear(er). As far as words are concerned we disagree even more with the referee. Concepts like duality, Noether charges (we add the epithet 'Noether' to properly honour one of the most famous female physicists in history whose ideas cast the physics into its modern shape and we are deeply surprised by the Reviewer's strong negative reaction), solitons, instantons and gauge fields are by now well known in condensed matter literature as can be established by looking up, e.g. the references below. The only new concept we introduce are "Polyakov electric strings". These are one of the main results of the paper, however, and, as such, are unavoidable.

The words mentioned by Reviewer are nowadays well known in condensed matter physics. This is clearly demonstrated by the following examples:

(i) **The methods used in our work were pioneered and introduced in condensed matter physics by Xiao-Gang Wen from MIT about 30 years ago**. In 2017 Xiao-Gang Wen won for these methods the **2017 Oliver E. Buckley Condensed Matter Physics Prize,**

one of the most prestigious awards of the American Physical Society. This evidences that the field-theory concepts that constitute the base of our approach are well familiar to the condensed matter physics community and in **27 years of their extensive use by the community** proved to be the vital, essential, and significant component of condensed matter physics.

(ii) These methods did not remain unknown for broad condensed matter community. They were immediately and thoroughly explained in great detail in the textbook by E. Fradkin, "Field Theories of Condensed Matter physics, Addison Wesley Publishing Company, **1991**, stuffed with terms like "duality," "instantons," etc. This book enjoyed enormous popularity and was re-published two more times in three following years. As an example, this book has been extensively used by hard-core condensed matter physicist Prof. Nayana Shah during her M.Sc studies in **Indian Institute of Technology** in 1994 – 1996 (**i.e. already 25 years ago**).

(iii) These concepts and methods were not forgotten but were getting more and more a mandatory part of textbooks for students. A recent example is the textbook by A. Altland and B. Simons, Condensed Matter Field Theory, Cambridge University Press, 2010, full of exercises (see attached), i.e. meant as a mandatory course for students. This book was sent to authors by the **graduate student** at the **University of Basque Country (Bilbao)**, Irene de León, as her desktop guide. Examples like those above can be multiplied indefinitely.

(iv) The seminal papers by Fazio and Schön, Charge and vortex dynamics in arrays of tunnel junctions, PRB 43, 5307 (**1991**) [**i.e. 29 years ago**] and by experimental Mooij's group, Field-Induced Superconductor-to-Insulator Transition in Josephson-Junction Arrays, PRL 69, 2971 (1992) are based on the extensive use of the notion of duality. **In 2011, Mooij and Schön received the London Prize for these researches, evidencing that the condensed matter community was quite familiar with the duality and similar concepts.**

(v) **A more recent paper,** Ashvin Vishwanath and T. Senthil, Physics of Three-Dimensional Bosonic Topological Insulators: Surface-Deconfined Criticality and Quantized Magnetoelectric Effect, PRX, **3**, 011016 (2013) [**seven years ago**], targets a broad audience of nonexperts and was written by renowned **condensed matter physicists**. Their Eq. (2) and our Eq. (1) are identical and are, in both cases, starting equations. Equally similar are our and their language, terminology, and analytical apparatus. This is one more forensic evidence that the language and concepts that we employ in our paper are familiar and common for condensed matter community which disproves Reviewer #2's claim.

Examples like the enclosed articles and textbooks are not outliers and can be multiplied indefinitely.

Reviewers' comments:

Reviewer #5 (Remarks to the Author):

In this paper, the authors discuss that superinsulators in Josephson junction arrays and phase transitions can be understood from magnetic monopole condensations through the electric magnetic duality found in high energy physics context. It seems that the previous referee just couldn’t follow the logics, but I think that the idea in this paper is very interesting. However, there are several
unclear points and drawbacks, some of which may be crucial and I cannot recommend the paper to be published in its form. Before reconsidering this paper, the authors should clarify the following points.

(1) Underlying physics is in fact coming from the Nambu’s idea in his seminal paper, Phys. Rev. D 10, 4262 (1974) - Strings, monopoles, and gauge fields (aps.org) in which he suggested that color confinement in QCD can be understood as a dual Meissner effect, that is, quarks are confined by color magnetic fluxes as a dual to a superconductor in which monopoles are confined by magnetic vortices. I think that the present authors’ argument heavily relies on this but they don’t mention this at all, (although they cite Ref.27 as a textbook for quark confinement).

(2) The paper heavily relies on their previous results Ref.13. However, this paper should be self-contained. The authors shouldn’t assume knowledge of their previous papers.

(3) The terminology of the Dirac string used in the present paper is misleading (or in fact wrong) at least compared with the original terminology. The original Dirac string, attached to a Dirac monopole in a U(1) gauge theory, is unphysical and does not carries any physical quantity. In fact, its direction from the monopole depends on gauge choices. The "Dirac string" in this paper connects magnetic monopoles and seems to be physical.

(4) What is the gauge symmetry in the second kind? It is not explained anywhere.

(5) The phase diagram schematically drawn in Figure 2 resembles that of QCD if we identify the horizontal and vertical axes as a chemical potential and temperature, respectively in that context. This is in fact interesting similarity between two theories.

(6) The most crucial question is how monopoles are condensed. In Fig.1, a pair of monopole and anti-monopole is connected by one string. However, in Josephson junction arrays, a pancake vortex in the i-th insulator ends on a "monopole" and "anti-monopole" but the flux goes vortices in superconductors to the (i-1)-th and (i+1)-th insulators, where there appear "anti-monopole" and "monopole", respectively. In other words, a monopole as one endpoint of a pancake vortex should be an anti-monopole as one endpoint of another pancake vortex. Thus, at least to me, a picture is not like Figure 1 but it should be a chain of strings. Then, I suspect the whole discussion afterwords.

(7) An another related doubt is that in superconductors one magnetic monopole is attached by two Abrikosov vortices, as a result of the Dirac quantization condition and the fact that Cooper pairs have charge 2e. Thus, a monopole in Figure 1 should be half-monopole, shouldn’t it?

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Point by point reply to Reviewer's comments

Reviewer #5 (Remarks to the Author):

First and foremost, we would like to thank Reviewer #5 for her/his careful and thoughtful reading and the professional assessment of our manuscript. We most enjoy an opportunity for a scientific discussion.

Reviewer: In this paper, the authors discuss that superinsulators in Josephson junction arrays and phase transitions can be understood from magnetic monopole condensations through the electric magnetic duality found in high energy physics context. It seems that the previous referee just couldn't follow the logics, but I think that the idea in this paper is very interesting. However, there are several unclear points and drawbacks, some of which may be crucial and I cannot recommend the paper to be published in its form. Before reconsidering this paper, the authors should clarify the following points.

Answer: We are most grateful to Reviewer for high evaluation of our work. Our only comment at this point is that in the present manuscript the focus is slightly shifted. While indeed our previous works were focused on somewhat more general and at the same time concrete aspects of this exciting direction, like the topological nature of the superconductor-insulator transition, Ref. [24] of our revised manuscript, or the revealing the nature of the superinsulating phase, Ref. [16] of the manuscript, [former Ref. 13], our present manuscript focuses on the aspect that has been overlooked by other researchers studying manifestations of the monopoles in condensed matter systems. Namely that monopoles in solid systems are not only classical particles but may form quantum condensate.

1. Reviewer: Underlying physics is in fact coming from the Nambu's idea in his seminal paper, Phys. Rev. D 10, 4262 (1974) - Strings, monopoles, and gauge fields (aps.org) in which he suggested that color confinement in QCD can be understood as a dual Meissner effect, that is, quarks are confined by color magnetic fluxes as a dual to a superconductor in which monopoles are confined by magnetic vortices. I think that the present authors' argument heavily relies on this but they don't mention this at all, (although they cite Ref.27 as a textbook for quark confinement).

Answer: We fully agree with Reviewer. Accordingly, we have added not only the reference to Nambu, but also two other relevant authors, Mandelstam and 't Hooft.

2. Reviewer: The paper heavily relies on their previous results Ref.13. However, this paper should be self-contained. The authors shouldn't assume knowledge of their previous papers.

Answer: At this point we have to respectfully disagree. We are sincerely confused about this comment. We quote the work [13] (which is Reference [16] in the revised version) in the Introduction section, where we present a brief overview of the topic. But neither section "Results," nor in the section "Phase transitions and phase diagram," where indeed all the basic results of the present work are obtained, rely on Ref. [13]. The starting Lagrangian of Eq. (1) from which we derive comes from a general and well-known "textbook-like-work" Ref. [27]. The description of the discrete form of action (2) contains the reference [14] indicating where this form has appeared for the first time, but its explanation is given in detail in the "Methods" section. The phase structure is derived in its entirety from scratch, the string tension of the superinsulating phase with a monopole condensate is also derived from scratch. No previous knowledge is assumed, all computations are done from A to Z in this paper, most technical details are in Methods. The only moment where this Reference [13] (now [16]) appears in the main body of the paper is a brief discussion after Eq. (4). But there this is the reference to the London equations which is a basic equation in superconductivity and is the textbook knowledge for every student specializing in condensed matter physics. We agree that this reference is a bit misleading since our previous work [13] also does not “describe in detail” this textbook equation, so we have removed unnecessary reference from this place. We have also mentioned that the London equations are actually not the focus of this paper but, rather we want to focus on the new superinsulating state.

3. Reviewer: The terminology of the Dirac string used in the present paper is misleading (or in fact wrong) at least compared with the original terminology. The original Dirac string, attached to a Dirac monopole in a U(1) gauge theory, is unphysical and does not carries any physical quantity. In fact, its direction from the monopole depends on gauge choices. The "Dirac string" in this paper connects magnetic monopoles and seems to be physical.

Answer: This is a crucially important point which was definitely not stressed well, and we thank Reviewer for pointing this out. Indeed, the vortex connecting a monopole to an anti-monopole can be considered a Dirac string (for both of them) only when it is long and loose (tensionless, technically speaking), as it is illustrated in Fig. 1 (b). In this case, the string is unobservable, its position does not matter, and one can justly refer to it as to indeed “unphysical.” When, however, the same vortex acquires a tension, the string becomes a taut straight object that linearly confines monopoles. This point is related to the point raised in Reviewer’s comment 6) below. The Bose condensation phase transition occurs exactly at the moment when the “tensionful” vortex connecting a monopole with an anti-monopole becomes tensionless. Moreover, at this point the monopole’s world-lines become infinitely long which means that monopoles that got free now having only Dirac strings attached to them, condense to form Bose condensate. We have added the new paragraph to articulate this point more clearly. We believe that this resolves this just Reviewer’s concern caused by our insufficient attention to terminology at this point.

4. Reviewer: What is the gauge symmetry in the second kind? It is not explained anywhere.

Answer: Here we are a bit confused again. The gauge symmetry of the second kind is explained in the paragraph right after Eq. (1) defining the model. We thank Reviewer for attentiveness and to make it more clear for a general reader, we have added some explanative sentences.

5. Reviewer: The phase diagram schematically drawn in Figure 2 resembles that of QCD if we identify the horizontal and vertical axes as a chemical potential and temperature, respectively in that context. This is in fact interesting similarity between two theories.

Answer: We thank the referee for pointing out this interesting similarity. We have of course added a corresponding paragraph in the text.

6. Reviewer: The most crucial question is how monopoles are condensed. In Fig.1, a pair of monopole and anti-monopole is connected by one string. However, in Josephson junction arrays, a pancake vortex in the i-th insulator ends on a "monopole" and "anti-monopole" but the flux goes vortices in superconductors to the (i-1)-th and (i+1)-th insulators, where there appear "anti-monopole" and "monopole", respectively. In other words, a monopole as one endpoint of a pancake vortex should be an anti-monopole as one endpoint of another pancake vortex. Thus, at least to me, a picture is not like Figure 1 but it should be a chain of strings. Then, I suspect the whole discussion afterwards.

Answer: The graphic picture drawn by Reviewer is correct and we are fully agreeing on that. However, when the monopole at the one end of a pancake vortex collapses “on top” of the anti-monopole at the end of the other pancake vortices they “annihilate” to form one single, longer vortex. In other words, the connected stacks, or chains, as the referees calls them, of “short” vortices (i.e., the pancakes) form a long vortex with monopoles and anti-monopoles appearing only at the surfaces of the sample (see Ref. [25]). Monopole anti-monopole pairs appear in the interior of the sample exactly provided that this stack or chain breaks somewhere in the “middle.” The resulting shorter vortices connecting them is exactly what is depicted in Fig. 1. It can indeed be viewed as the connected chain of “elementary single units” with the monopoles at the endpoints, but this time in the interior of the sample. We have tried to articulate this picture better having added a new paragraph. We thank Reviewer for attracting our attention to this point since it helped indeed to improve our presentation of this important result of our work.

7. Reviewer: An another related doubt is that in superconductors one magnetic monopole is attached by two Abrikosov vortices, as a result of the Dirac quantization condition and the fact that Cooper pairs have charge 2e. Thus, a monopole in Figure 1 should be half-monopole, shouldn’t it?

Answer: This is yet one more point very well taken by Reviewer, which stems from some semantics differences. Namely there is a mismatch between the field theory and condensed matter notation habits. In the field theory a “unit” vortex carries the flux equal to 2\pi/e since the elementary charge is the electron charge e. In the condensed matter literature, Abrikosov vortices appear in a Cooper pair condensate context in which the unit charge is 2e and, therefore one considers a unit vortex having flux \pi/e, which, from the field theory point of view is a “half-vortex.” The same problem arises with monopoles. From the field theory point of view the referee is correct, and our objects are half-monopoles. However, since we are dealing with phases of matter in which the unit charge is always 2e, they are actual unit monopoles, exactly as \pi/e are unit vortices in superconductors. We have added a detailed explanation of this semantic fact to shift misunderstanding among the two major audiences that we target.

To conclude we would like to thank Reviewer again for her/his thoughtful constructive remarks that helped us to improve our presentation.

REVIEWERS' COMMENTS:

Reviewer #5 (Remarks to the Author):

The authors revised the manuscript taking into account the comments of my previous report. I think that all subtle points in the previous manuscript are improved. Now I can recommend the paper to be published in Communications Physics.