Reviewers' comments:

Reviewer #1 (Remarks to the Author):

The manuscript addresses an interesting question of inter-disciplinary use (connecting the gravitational-wave astronomy and particle physics) of gravitational-wave detectors - the LIGO interferometers - to test a certain dark matter hypothesis, an extension of Standard Model of particles called Dark Photon Dark Matter (DPDM). The Authors present results of their analysis using public LIGO O1 data and conclude that the derived upper limits on the coupling parameter of dark photons to matter are better than for other experiments (based on equivalence principle tests using a torsion balance) in a wide range of frequencies.

These results are, up to my knowledge, new, and definitely of interest to the broad astro-particle community, so the publication of this manuscript would be worthwhile. In addition, the results strengthen the case of the otherwise very successfully emerging gravitational-wave astronomy.

Within the limits of the assumptions of the particular model of DPDM studied by the Authors, I find the statistical analysis convincing and the description sufficiently descriptive. The methods are laid out clear and the data used for the analysis is publicly available in the Gravitational Wave Open Science Center (GWOSC).

Additionally, I have just a few minor comments/questions:

- the authors state that the DPDM model is non-relativistic. It would be informative to include an explanation what does it mean (especially useful for readers who are non-experts),
- why newer and more sensitive LIGO O2 data were not used in the analysis?
- would including a third (fourth, etc.) detector (Virgo, KAGRA, LIGO India) improve the limits, and if yes then how?
- how would the third generation detectors (Cosmic Explorer, Einstein Telescope) change the limits?
- Tiny font on the insets in Fig. 4 is borderline unreadable.

Reviewer #2 (Remarks to the Author):

This paper presents an interesting analysis of ultralight dark photon dark matter in LIGO data. The analysis appears sensible and correct. My main concerns are whether dark matter of this type is well motivated or not. Is it hard to imagine sensible cosmologies that give rise to dark matter of this type? Would other ultralight boson dark matter give signals of this type at LIGO?

The limits presented here surpass tests of gravity at small scales/searches for 5th forces over a narrow range of dark photon masses. Projected sensitivities with updated LIGO data would beat these current limits but it begs the question of what the prospects are for the 5th force searches and how they would compare.

In light of this, I don't believe the paper is of sufficiently wide interest to be published in Nature Communications Physics. I would encourage the authors to submit to PRL or similar journal.

Reviewer #3 (Remarks to the Author):

This paper derived the 95% CL limit on parameter region for dark photon dark matter using LIGO O1 data, which exceeds the sensitivity of previous experiments in some region. This is certainly an interesting study/result, and should be published somewhere. However, I have reservation for its publication on Nature Communications Phys., explained below along with the following questions for the authors.
1) I find the discussion for the source of noise, background for this search is not sufficient. Also, for O1 analysis and projection for future O4-O5, discussion about expected astro background from binary merger should be included, for instance, see https://arxiv.org/pdf/1612.02029.pdf I understand the signal is distinct from standard stochastic bkg but that is still relevant.
2) It'd be helpful to show a strain-frequency plot of signal and background for this type of search. Background can be rescaled to show on the same scale.
3) I'm curious what causes the many spikes in some of the lines in Fig.1, esp. the solid blue line, while the red solid is relatively smooth? Related to binning?
4) There have been recent studies suggesting notable deviation of DM velocity distribution from Maxwell. E.g. https://arxiv.org/pdf/1807.02519.pdf and many refs. in there. Can the authors at least comment on how such uncertainty would affect their results?
5) A minor thing: Fig. 2 has v0 showed up, which is defined much later (eq.6) in the paper…
6) One main impression is that, the foundation of this work lies in the published work (some of the authors are on that paper, and that is a very interesting one) arxiv: 1801.10161, the current work although probably solid (pending addressing some questions above), does not seem to have sufficient novelties for publication at Nature Commun. Phys. Meanwhile this is one of the many papers these days presenting constraints from various experiments on different DM models. This paper does not seem to stand out of the crowd in terms of importance to justify publication in this high profile journal. Of course if it shows a hint of potential discovery that is another scenario which is the not case here… But as said the paper is interesting, and (after some clarifications to the above qs.) should be worthwhile publishing somewhere, e.g. Phys. Review journals.

Reviewer #4 (Remarks to the Author):

The central result of the manuscript, Searching for Dark Photon Dark Matter in LIGO O1 Data, is a proof of concept analysis of LIGO data which sets constraints on dark photon dark matter coupled to baryon currents. The authors perform an analysis based on a previous proposal --- Searching for Dark Photon Dark Matter with Gravitational-Wave Detectors,
Aaron Pierce, Keith Riles, and Yue Zhao, PRL 121, 061102 (2018)--- of O1 LIGO data, and set a constraint in the range of 10 to 1000 Hz (~10^-13 to ~10^-11 eV in mass) on this type of dark matter. The constraint exceeds previous constraints on baryon-number-coupled dark photons in a range of a factor of 4 in mass, which is a new and interesting result.

This work and the proposal preceding it builds on several previous proposals which put forward the idea of using the LIGO detector as a means of searching for light bosonic dark matter, including the work by Asimina Arvanitaki, Junwu Huang, Ken Van Tilburg, Phys.Rev. D91 (2015) no.1, 015015 which estimated the reach of LIGO to dilaton (scalar) dark matter and the work of Peter W. Graham, David E. Kaplan, Jeremy Mardon, Surjeet Rajendran, William A. Terrano, Phys.Rev. D93 (2016) no.7, 075029 which explored several other methods to search for such dark matter candidates. Thus I do not believe that the work will greatly alter the direction of thinking in the field. Nevertheless, the novelty in the work is in the actual demonstration that such techniques can succeed and is therefore valuable.

The statistical analysis takes into account subtleties in the dark matter velocity and polarization distribution as well as the look elsewhere effect, robustly considers the details of the LIGO noise floor, and interfaces with existing LIGO detector pipelines. Whether the approximation of 10^3 particles in the simulation used to estimate the power is an adequate approximation is unclear given that in this range of parameter space there are ~10^50 particles per deBroglie volume. The authors mention a `interference' effect without explanation; given that it will be explored in future work a brief explanation would be valuable here. Whether one of both of these points lead to the deviation from the Maxwellian distribution as shown in fig.2 should be specified to provide a physical reason for the difference.

That said, I believe there are two important caveats which are not clearly presented in the paper. The first is that, as stated in the title, the bounds assume 100% of dark photon dark matter; however, the origin of this dark matter in the mass range the authors consider is unclear. The original production method (refs 1 - 3 in the manuscript) is quite general but can only produce dark photon dark matter for a range of masses much heavier than the one considered here (m> 10^-6 eV). More recently (refs 4-8) there have been other ideas for how to create dark matter in this range but they are not very generic, and it is not clear to me that they apply to the model in question: B-coupled dark photon at ~10^-12 eV with the range of couplings probed in this analysis. In comparison, the existing bounds from EP violation shown in figure 1 are derived without any assumption on dark matter density and thus are much more powerful in this region of parameter space.

The second point is that the authors perform an analysis on only one type of dark photon dark matter -- the dark photon coupled to baryon number. Generally, in theoretically complete models, the types of dark photon that are considered are those with a kinetic mixing to our Standard Model photon or the one coupled to baryon minus lepton number ('B-L'). The reason is these two (the kinetic and the B-L) photons correspond to anomaly-free currents and thus can easily be embedded in a complete theory without having to be concerned about other constraints. On the other hand, a dark photon coupled to baryon number or another non-conserved current typically acquires additional constraints in a complete theory, see for e.g. Jeff A. Dror, Robert Lasenby, Maxim Pospelov, Phys.Rev.Lett. 119 (2017) no.14, 141803 and following work. In fact, for a B-L photon I suspect that the bound would no longer be competitive with previous results (although based on PRL 121, 061102 (2018) it may be in the future), while for a kinetically mixed dark photon there will be nearly no bound at all as the LIGO mirrors are approximately neutral. Thus the impact of this work in the context of motivated models is somewhat reduced, and this should at the very least be made clear in the abstract and discussion (dark photon -> baryon number dark photon or vector).

If the authors are able to demonstrate a consistent model of baryon coupled dark photons which result in dark matter in the region of parameter space probed in their analysis, the impact of the work would be much increased. The work is sound and interesting, nevertheless I would not recommend it for publication in Nature Physics Communications at this stage given the very narrow applicability of the analysis to existing beyond the standard model constructions.

Reply Letter

Date: August 26, 2019

Dear Editor:

We thank the referees for very helpful comments on our paper. We made the following improvements to our manuscript according to their comments and suggestions:

Reviewer 1

- The authors state that the DPDM model is non-relativistic. It would be informative to include an explanation what does it mean (especially useful for readers who are non-experts),

  Reply: DM particles need to be non-relativistic in order to be gravitationally bounded in galaxies. The typical velocity of DM particles in our galaxy is about $10^{-3}c$, which is determined by the virialization. As a consequence, in the non-relativistic limit, the total energy of a DM particle can be written as $\omega = m(1+\frac{1}{2}v_0^2)$, where $v_0 \approx 10^{-3}$ is the virial velocity. Thus the oscillation frequency of the dark photon background field is almost a constant determined by the mass of dark photon with $\sim 10^{-6}$ deviation. Further, in the non-relativistic limit, the polarization vector and propagation vector of dark photon are independent of each other. In the new version, we added a more detailed discussion in order to explain the necessary background for a general audience.

- why newer and more sensitive LIGO O2 data were not used in the analysis?

  Reply: We are in the process of analyzing LIGO-O2 data. We aim to apply a more sophisticated search strategy, a refined template analysis, with the O2 dataset. One of the key points in this paper is to demonstrate the power of the LIGO experiment when used as a DM direct detector. Especially, with only LIGO-O1 data, we start to surpass existing limits, which we think is impressive. Despite limiting results here to the O1 data, we did present a future projection on what can be expected from future O4-O5 datasets.

- would including a third (fourth, etc.) detector (Virgo, KAGRA, LIGO India) improve the limits, and if yes then how?

  Reply: In principle, the inclusion of additional detectors can improve the limits derived here. However, the actual degree of improvement depends on the performance of each additional detector and their relative orientations, i.e. the relative antenna patterns of these detectors. Virgo sensitivity is not as good as that of LIGO detectors yet. In addition, overlap functions between the Virgo detector and two LIGO detectors are much smaller than that between two LIGO detectors due to relative orientations. The same argument on antenna pattern also applies to other detectors such as KAGRA and LIGO-India. We add comments in the new version to address this subtlety.

- how would the third generation detectors (Cosmic Explorer, Einstein Telescope) change the limits?

  Reply: The third-generation detectors should have an order of magnitude lower strain noise, offering a promising future. We have added a comment to the paper to address this point.

- Tiny font on the insets in Fig. 4 is borderline unreadable.

  Reply: This has been fixed in the new version.

Reviewer 2

- This paper presents an interesting analysis of ultralight dark photon dark matter in LIGO data. The analysis appears sensible and correct. My main concerns are whether dark matter of this type is well motivated or not. Is it hard to imagine sensible cosmologies that give rise to dark matter of this type?

  Reply: Dark photon is a well motivated candidate for DM particles. A light U(1) gauge boson naturally appears in many models, especially when extra dimensions are involved. Such a dark photon can get a mass by the Higgs or Stueckelberg mechanism. Second, there are several interesting mechanisms producing a dark photon through non-thermal process, such as misalignment mechanism, light scalar decay and production from cosmic string. This is a very active research direction in the particle community now, and many interesting ideas are under investigation. With these, an ultralight dark photon can be a natural and well motivated candidate of cold dark matter. Furthermore, there are increasing experimental efforts aiming to look for ultralight dark photon dark matter. Brand new experiments are proposed and built to test DPDM. However, our study demonstrates that an exceedingly good sensitivity of LIGO comes for free, without any modifications of the experiment.

- Would other ultralight boson dark matter give signals of this type at LIGO?

  Reply: Indeed, the $U(1)_B$ gauge boson is just one benchmark model we used in our study to illustrate how a gravitational wave detector can be used for dark matter direct detection. Many other models can be studied based on our results through a simple rescaling. This point has been clarified in the previous theory paper. In order to be self-contained, we add some discussions in the revised version for clarification.

- The limits presented here surpass tests of gravity at small scales/searches for 5th forces over a narrow range of dark photon masses. Projected sensitivities with updated LIGO data would beat these current limits but it begs the question of what the prospects are for the 5th force searches and how they would compare.

  Reply: First, we have demonstrated that LIGO-O1 data has already beaten other existing experiments with the current published result. In addition, LIGO-O2 data has been publically accessible and LIGO will further deliver more data with lower detector noise. We expect that LIGO results will significantly improve in the near future. On the other hand, we checked the recent Eot-Wash experiment and find that their systematic uncertainties are already comparable to the statistic ones. Especially some contributions of their systematic uncertainties are not easy to remove, such as temperature gradients etc. Thus we do not expect their result will largely surpass these initial resulsts in the near future, while the more sensitive results wil be obtained by LIGO in the next several years.

- In light of this, I don't believe the paper is of sufficiently wide interest to be published in Nature Communications Physics. I would encourage the authors to submit to PRL or similar journal.

  Reply: This letter lies in the interdisciplinary area of gravitational wave (GW) astronomy and dark matter (DM) physics. It neatly combines these two topical fields. Our study builds a bridge between GW astronomy and particle physics. Our work extends the scientific goals of GW detectors and demonstrates a novel direction for DM direct detection. It shows that a GW detector can be multi-purpose and directly relevant to many other fields. Our paper presents the first careful study of DPDM sensitivities using LIGO's O1 data which is publicly accessible. **For the first time, LIGO data has been used for a direct DM search.** We demonstrated that even LIGO's initial O1 data set has already provided a sensitivity to DPDM better than that of prior experiments and show well-grounded projections of substantial gains in sensitivity from future data sets. Thus we believe our work meets the criteria for publication in Nature Communications Physics.

Reviewer 3

- 1) I find the discussion for the source of noise, background for this search is not sufficient. Also, for O1 analysis and projection for future O4-O5, discussion about expected astro background from binary merger should be included, for instance, see https://arxiv.org/pdf/1612.02029.pdf. I understand the signal is distinct from standard stochastic bkg but that is still relevant.

  Reply: A detailed discussion of LIGO instrumental noise sources is beyond the scope of this paper, but in addition to the spectral line noise reference cited in the original version, we have now added a citation to another paper addressing broadband noise sources and added a very brief discussion to the text. The astrophysical background from binary mergers will indeed eventually become relevant to DPDM searches in future GW searches (beyond Advanced LIGO), once that background has been definitively detected, with its strength and spectral shape measured (both dependent on merger rates and mass distributions not yet well known). At that point, if DPDM has not yet been detected, then its measurement will become a search for an extremely narrow excess correlation over a smooth broadband merger background. The reality of a putative DPDM detection could be tested by varying coherence times used in cross-correlation. Since we do not provide projected DPDM sensitivities for detectors and data sets more sensitive than the Advanced LIGO O4-O5 data set, we do not attempt in this paper to quantify the search sensitivity in the presence of that astrophysical background. As seen in Figure 3 of the paper mentioned by the referee, it is only the integrated broadband merger signal that is likely to be (marginally) detectable in O4-O5 by pure cross-correlations methods (detection of the non-Gaussian, sporadic binary black hole merger stochastic background is better accomplished by other methods; see, e.g., arXiv:1712.00688). We have added text stating that we neglect the true GW stochastic background measurable via cross-correlation in a single frequency bin, an assumption that should be safe for Advanced LIGO (and may well still be safe for third-generation GW detectors like Einstein Telescope and Cosmic Explorer).

- 2) It'd be helpful to show a strain-frequency plot of signal and background for this type of search. Background can be rescaled to show on the same scale.

  Reply: We thank the referee for this helpful suggestion. We have updated our plot to include such information, see Fig. 2 in the new version. We plot both signal PSD, given a benchmark choice on DP parameters, as well as the noise. We note that the noise is expected to be flat in a narrow frequency window.

- 3) I'm curious what causes the many spikes in some of the lines in Fig.1, esp. the solid blue line, while the red solid is relatively smooth? Related to binning?

  Reply: This is simply a confusion caused by plotting style. The spikes are the same for blue and red lines. When we made the plot, the blue spikes covered the red ones which causes the confusion. We have added a comment in the figure caption to avoid potential confusion.

- 4) There have been recent studies suggesting notable deviation of DM velocity distribution from Maxwell. E.g. https://arxiv.org/pdf/1807.02519.pdf and many refs. in there. Can the authors at least comment on how such uncertainty would affect their results?

  Reply: We discussed the subtlety on velocity distribution in our original version (see our footnote 4 in the new version). The uncertainty due to the velocity distribution has a very minor effect on the eventual upper limit in the current single-bin search because the SNR depends on the integrated power of the signal and not much on its shape. As long as the averaged velocity is not too different from the virial velocity, we do not expect that the detailed distribution largely impacts our results since we are not using the signal spectrum for this study yet. It may cause a difference when we use a more sophisticated template search. This is under investigation now and hopefully we can report the results soon. We add more detailed discussions in the revised version and also include the paper suggested by the referee to our reference list.

- 5) A minor thing: Fig. 2 has v_0 showed up, which is defined much later (eq.6) in the paper...

  Reply: We thank the referee for pointing this out. We have made proper adjustment of the text.

- 6) One main impression is that, the foundation of this work lies in the published work (some of the authors are on that paper, and that is a very interesting one) arxiv: 1801.10161, the current work although probably solid (pending addressing some questions above), does not seem to have sufficient novelties for publication at Nature Commun. Phys. Meanwhile this is one of the many papers these days presenting constraints from various experiments on different DM models. This paper does not seem to stand out of the crowd in terms of importance to justify publication in this high profile journal. Of course if it shows a hint of potential discovery that is another scenario which is the not case here... But as said the paper is interesting, and (after some clarifications to the above qs.) should be worthwhile publishing somewhere, e.g. Phys. Review journals.

  Reply: As the referee pointed out, this paper does not aim to introduce the idea that a gravitational wave experiment can be used for DPDM detection. However, we emphasize that this is the **first search**

of this type using LIGO-O1 data, one that addresses systematics due to non-gaussian noise. In other words, this paper goes beyond idealized phenomelogy and confronts reality, including instrumental artifacts. The highlight is that we have started to probe unexplored parameter space even with O1 data. This shows how powerful a GW experiment can be, once it is used for DM direct detection. Further, the sensitivity is going to improve for LIGO's future runs, given a better noise control as well as a longer operation time. This is a pioneering study with a promising discovery potential in the coming years. We do not think that non-detection of dark matter in this first search disqualifies the paper from N-CP publication; experimental discovery of dark matter would be a demanding standard! There are many examples of null results published in Nature journals, including limits on stochastic GW background.

Reviewer 4

- Thus I do not believe that the work will greatly alter the direction of thinking in the field. Nevertheless, the novelty in the work is in the actual demonstration that such techniques can succeed and is therefore valuable.

  Reply: Indeed, we agree with the referee that the goal in this paper is not to alter the direction of thinking in the field because the theory proposal, as well as several other theory proposals, have been studied by various groups, including a subset of authors in this paper. The point is to perform **the very first and serious study using LIGO-O1 data, demonstrate the systematics with non-gaussian noise artifacts and illustrate the power of the LIGO experiment as a DM direct detector as well as the fact that even LIGO-O1 data starts to probe the unexplored parameter space.**

- Whether the approximation of 10^3 particles in the simulation used to estimate the power is an adequate approximation is unclear given that in this range of parameter space there are 10^{50} particles per deBroglie volume.

  Reply: As the referee points out, there are far more than 10^3 dark photon particles, given the local DM energy density and the interested DP mass. However, we have checked carefully that including 10^3 particles suffices to capture all qualitative features in the DPDM background. We performed the cross checks in the following manners: 1. The simulated DPDM background provides results consistent with the estimations on coherence time and coherence length. 2. The signal PSD, presented in our Fig. 2, does not change qualitatively by increasing the number of particles in numerical simulations. 3. We recently developed an analytical formula to calculate signal PSD, which easily gives the results with an arbitrarily large DP number. The analytic result agrees well with the results from numerical simulations with 10^3 DP particles. We will provide detailed analytic formula in our follow up paper in the near future. 4. We apply our DPDM simulation of 10^3 DP particles to the LIGO simulation package LALSuite and cross check the validity on our result presented in Fig. 1. The results from our simulation match well with that derived in Fig. 1. With all these crosschecks, we do not see a reason for worries. We add a detailed discussion in the revised version to give more information and clarify the potential worries.

- The authors mention a 'interference' effect without explanation; given that it will be explored in future work a brief explanation would be valuable here. Whether one of both of these points lead to the deviation from the Maxwellian distribution as shown in fig.2 should be specified to provide a physical reason for the difference.

  Reply: The deviation is mainly caused by the Earth's rotation, with a resulting broadening of the spectrum of about $\Delta f \approx 2f_E$, where f_E is the Earth rotation frequency. The signal is determined by both the directions of polarization and propagation vectors of DPDM, with respect to detector orientations. Thus the Earth rotation affects the signal and causes the broadening. In addition, the signal PSD is not simply proportional to Maxwell distribution($\equiv f(v)$), but rather proportional to $vf(v)$. This is not surprising since our signal vanishes when $v_{vir} \to 0$. We agree with the referee that the details were not fully explained in the previous version, and some discussion has been added for clarification. Furthermore, we briefly mentioned the 'interference' effect in the previous version, without a clear explanation. We think this is unnecessary, as well as potentially leading to confusion. Thus we removed the related discussion in the new version and will save it for a much detailed discussion in the future work.

- That said, I believe there are two important caveats which are not clearly presented in the paper. The first is that, as stated in the title, the bounds assume 100% of dark photon dark matter; however, the origin of this dark matter in the mass range the authors consider is unclear. The original production method (refs 1 - 3 in the manuscript) is quite general but can only produce dark photon dark matter for a range of masses much heavier than the one considered here ($m > 10^{-6}$ eV). More recently (refs 4 - 8) there have been other ideas for how to create dark matter in this range but they are not very generic, and it is not clear to me that they apply to the model in question: B-coupled dark photon at 10^{-12} eV with the range of couplings probed in this analysis. In comparison, the existing bounds from EP violation shown in figure 1 are derived without any assumption on dark matter density and thus are much more powerful in this region of parameter space.

Reply: The DM relic abundance for ultralight dark photon is less well studied compared to other DM candidates, such as an axion. However, as the referee pointed out, this question has attracted much attention and study in recent years. Many recent and on-going works are devoted to properly produce DPDM relic abundance. With these efforts, it becomes more and more plausible to realize the DPDM scenario with natural choices for parameters. This is very similar to the study of the QCD-axion DM relic abundance with large f_a. For axion with f_a much larger than 10^{12} GeV, a generic choice of initial angle, θ, leads to too much DM relic abundance and over-closes the universe. However, there are many interesting mechanisms introduced to naturally produce QCD-axion DM relic abundance with large f_a. With these efforts, important experiments, such as CASPER, DM Radio and ABRACADABRA, are built to look for QCD-axion DM relic abundance in the "unnatural" parameter space. Therefore, we believe that one should not use theory prejudice, especially naturalness, as a criterion to judge the importance of a work based on real data. In addition, we agreee with the referee that the result from EW experiment does not rely on DP being DM. This is emphasized in both the introduction and the caption of Fig. 1 in the new version.

- The second point is that the authors perform an analysis on only one type of dark photon dark matter – the dark photon coupled to baryon number. Generally, in theoretically complete models, the types of dark photon that are considered are those with a kinetic mixing to our Standard Model photon or the one coupled to baryon minus lepton number ('B-L'). The reason is these two (the kinetic and the B-L) photons correspond to anomaly-free currents and thus can easily be embedded in a complete theory without having to be concerned about other constraints. On the other hand, a dark photon coupled to baryon number or another non-conserved current typically acquires additional constraints in a complete theory, see for e.g. Jeff A. Dror, Robert Lasenby, Maxim Pospelov, Phys.Rev.Lett. 119 (2017) no.14, 141803 and following work. In fact, for a B-L photon I suspect that the bound would no longer be competitive with previous results (although based on PRL 121, 061102 (2018) it may be in the future), while for a kinetically mixed dark photon there will be nearly no bound at all as the LIGO mirrors are approximately neutral. Thus the impact of this work in the context of motivated models is somewhat reduced, and this should at the very least be made clear in the abstract and discussion (dark photon → baryon number dark photon or vector).

  Reply: Indeed, we agree with the referee that the results from papers by Jeff A. Dror et. al. impose very strong constraints on gauged $U(1)_B$ scenario. We are well aware of their papers. In fact, one of our authors was acknowledged in their paper for discussions. The results derived in these papers rely on the assumption on how to UV complete the model in order to cancel the anomalies associated to $U(1)_B$. In their study, they assume the anomaly cancellation is achieved by introducing heavy fermions which are chiral under $U(1)_B$ but vector-like under SM gauge groups. On the right column of page 2 in 1705.06726 or Sec. II B in 1707.01503, they discuss the details on this assumption and pointed out the potential ways to get around the constraints derived in their studies, for example introducing new fermions while extending the EWSB sector or other anomaly cancellation mechanism. On the other hand, if we take the model-independent constraint on an anomalous gauge symmetry, new particles need to be added at energy scale $O(\frac{4\pi m_A}{\epsilon e})$, which gives $O(\text{TeV})$ for the parameter space we are interested in. Since the required new particles carry only electroweak charges, they are safe from various collider searches. We agree with the referee that this is an important theoretical subtlety that needs to be addressed clearly in the paper. We added a detailed discussion in the revised version.

- If the authors are able to demonstrate a consistent model of baryon coupled dark photons which result in dark matter in the region of parameter space probed in their analysis, the impact of the work would be much increased.

  Reply: We hope our discussion in this reply, as well as the modifications in the revised version, have fully addressed the concerns from the referee. We thank the referee for catching these subtleties which were left to be addressed in the previous version. If the referee thinks additional clarifications are necessary, we are happy to take further efforts to improve the paper.

- The work is sound and interesting, nevertheless I would not recommend it for publication in Nature Physics Communications at this stage given the very narrow applicability of the analysis to existing beyond the standard model constructions.

  Reply: We note that this $U(1)_B$ DPDM study is only one of many ultralight DM models one can study using GW detectors such as LIGO. The point is to perform the very first and serious study using LIGO-O1 data, demonstrate the systematics with non-gaussian noise artifacts and illustrate the power of the LIGO experiment as a DM direct detector. We believe our work has built a bridge between particle physics and GW physics, and this paper is only the first step along a series of studies. Thus we believe our paper is qualified for all criteria to be published in Nature Physics Communications.

We hope that these clarifications and modifications have properly addressed the referees' concerns and that the referees will now find the paper suitable for publication.

Yours Sincerely,

Huaike Guo, Keith Riles, Fengwei Yang and Yue Zhao

Reviewers' comments:

Reviewer #1 (Remarks to the Author):

I have read the revised manuscript and am satisfied with the improvements.

I still maintain that this manuscript, being the first example of a novel type of DM search using a gravitational-wave detector, is a milestone important enough to be published in Nature Physics Communications.

Reviewer #3 (Remarks to the Author):

1), 2): The authors' response about background for the analysis seems adequate and informative.
3) I guess I was not clear: I meant the solid magenta line (not 'red') indicating 'Ave optimal O4-O5 limits (2 yrs)' which is much smoother than the solid red/blue line for O1. Can the authors explain this big difference?

4), 5) OK.

6) Let me clarify that I did not mean to say that a discovery is required for publication on Nature. But I still find that although this is a very interesting work with its novelties, the dark matter model possibility that the method can be applied to is very limited. So again this is one of the a good number of papers presenting constraints from various types of experiments on various DM models. This is also not the first paper on DM search using LIGO data (I understand that this paper may include more experimental analysis details than some other papers though). Other referees also raised similar concern. So I think this does not have broad enough appeal for Nature publication. The analogy of limits of GW background being published on Nature is not at all appropriate: the 2009 LIGO paper result applies to much more generic theoretical models. Btw the more recent LIGO paper on this is published on PRL. Related, as I read at the end of the authors' response to Referee 4, the authors seem to claim the work is the first step in building a bridge between particle physics and GW physics. This is not true and an obvious exaggeration for the significance of the work: the work is an interesting one indeed, but there are many high quality papers in this endeavour before this work... With all considered, I still cannot endorse this paper for Nature publication. Physical Review series, possibly PRL, would be a suitable place for its publication.

Reviewer #4 (Remarks to the Author):

I appreciate the changes and additional comments included by the authors. I still believe the paper is too narrow in scope to be published in this journal. However, if the decision is made to publish, I would insist on clarifying in the abstract that the exclusion is specific to U(1)B dark photon dark matter, since, as I emphasized before, there are various types of dark photon dark matter and this is perhaps the least interesting one. Thus the casual reader should not be mislead in thinking it is a more general bound than it actually is.

Reply Letter

Date: October 15, 2019

Dear Editor:

We thank the referees for very helpful comments on our paper. We made the following improvements to our manuscript according to their comments and suggestions:
Reviewer 3

- I guess I was not clear: I meant the solid magenta line (not 'red') indicating 'Ave optimal O4-O5 limits (2 yrs)' which is much smoother than the solid red/blue line for O1. Can the authors explain this big difference?

  Reply: Now we understand what causes the confusions for reviewer 3, we have added a sentence in the subsection "New constraints from LIGO O1 data" for clarification. The magenta line is smoother, as it uses a design sensitivity curve that shows only fundamental noise sources, while the blue curve includes additional, non-fundamental noise artifacts that have not yet been mitigated in LIGO detector commissioning, such as power mains contamination at 60 Hz, harmonics and environmental vibrations, etc.

- Let me clarify that I did not mean to say that a discovery is required for publication on Nature. But I still find that although this is a very interesting work with its novelties, the dark matter model possibility that the method can be applied to is very limited. So again this is one of the a good number of papers presenting constraints from various types of experiments on various DM models. This is also not the first paper on DM search using LIGO data (I understand that this paper may include more experimental analysis details than some other papers though). Other referees also raised similar concern. So I think this does not have broad enough appeal for Nature publication. The analogy of limits of GW background being published on Nature is not at all appropriate: the 2009 LIGO paper result applies to much more generic theoretical models. Btw the more recent LIGO paper on this is published on PRL. Related, as I read at the end of the authors' response to Referee 4, the authors seem to claim the work is the first step in building a bridge between particle physics and GW physics. This is not true and an obvious exaggeration for the significance of the work: the work is an interesting one indeed, but there are many high quality papers in this endeavour before this work...

  Reply: We have toned down some statements in the paper in order to comply with the suggestions by referee 3. We emphasize that we do not claim that our study is the first DM search based on LIGO data. However we do present the very first *direct* DM search at LIGO, as opposed to an *indirect* search for dark matter via its production of gravitational waves, such as the search for primordial black holes. This point is further clarified in the new version in order to avoid any confusion. In addition, we believe our work has a broad application since many other ultralight DM candidates can be searched directly using LIGO's data with exactly the same analysis strategy.

Reviewer 4

- However, if the decision is made to publish, I would insist on clarifying in the abstract that the exclusion is specific to U(1)B dark photon dark matter, since, as I emphasized before, there are various types of dark photon dark matter and this is perhaps the least interesting one. Thus the casual reader should not be mislead in thinking it is a more general bound than it actually is.

  Reply: In both the "Abstract" and "Discussion" of the new version, we clearly state that the dark photon we consider in this study is the gauge boson of the $U(1)_B$ group. We also emphasize in the new version that, with upcoming LIGO data as well as the growth of a global GW detector network, the same analysis can be applied to look for many other ultralight DM scenarios.

We hope that these clarifications and modifications have properly addressed referees' concerns and that referees will now find the paper suitable for publication.

Yours Sincerely,

Huaike Guo, Keith Riles, Fengwei Yang and Yue Zhao