Peer Review File

Manuscript Title: Measuring gravitational attraction with a lattice atom interferometer

Reviewer Comments & Author Rebuttals

Reviewer Reports on the Initial Version:

Referees' comments:

Referee #1 (Remarks to the Author):

I enjoyed reading this manuscript. On the face of it, precision measurements of potential gradients by atoms in a lattice is a crazy idea, since the optical potentials are very large and fairly inhomogeneous. I've heard of this idea before but did not imagine it could be as successful, so precise, as has been demonstrated in this manuscript. The results are quite impressive.

The discussion on systematic errors was very thorough and very compelling, and particularly welcome given what might be seen as the systematic-prone nature of trying to measure forces on confined particles. It is clear that this method is much more likely to be useful when one can readily modulate the sources of exotic forces, what the authors call the M switch.

MINOR COMMENTS AND QUESTIONS.
1. Figure 3 caption refers to parts "a…b…b….c". I think it should be "a…b…c….d".
2. Do you use different value of tau for differing delta z? It seems like the decoherence rate depends on delta z and thus one might get better results by re-optimizing tau for each of the values of delta z.

SIGNIFICANCE OF RESULTS PRESENT AND FUTURE.
1. Although our current understanding of the numerical value of big G is in a deplorable state, it is not clear that lattice-based atom interferometry can help. To make an important contribution in that business, one must be able to calculate the expected acceleration from a switchable mass distribution to a part in 10^4, which is to say, about 300 times better than in the current manuscript. It seems to me that progress in big G happens not so much with new ways of measuring acceleration such as lattice-based interferometry, but with clever ways of positioning source masses and of designing experimental protocol which are forgiving of measurement error and possible internal density fluctuations.
2. Over the past 25 years there have many attempts to observe or at least constrain "Weird Gravity" (any deviation from the usual 1/r^2 force law). Looking for deviations at short length scale is particularly difficult because not only the detector but also the effective size of the source must shrink, and at fixed source density the corresponding forces fall very rapidly as the length scale to be probed shrinks. The current paper seems to be another effort in this tradition, and for this reason I was surprised not to see a version of the plot which has appeared in almost all the previous efforts, with the exotic mass-mass coupling being modeled as a Yukawa potential and the exclusion being plotted as a limit on alpha, the coupling strength in units of gravitational constant G, versus lambda, the Yukawa length. These plots are as I say are ubiquitous -- in 30 seconds on google I was able to find e.g. figure 4 of Yang et al PRL 108, 091101 (2012) although there has since then been further

progress. In the end I had to more or less guess how the current data would fall on such a plot. The authors put a limit on "weird acceleration" at about 30% of gravity, so at the most favorable range of lambda, say around 2 cm, the exclusion would be for alpha > 0.3. The authors report the atoms are never closer than 4 mm to the source, so for a yukawa length of say 0.2 mm, their sensitivity is going to be greatly suppressed by the exponential in the Yukawa potential. I'm going to guess at 0.2 mm their sensitivity might only good enough to exclude alpha > 1000. In this way of parameterizing sensitivity, the current work is a factor of 30 or more times less sensitive than competing short-range gravity experiments based on solid-mass sensors.

With this in mind I had to stare a little at the three exclusion plots in the current paper, plots which show instead that actually the current experiment offers best-ever exclusions. I did not go all the way back to the theory papers but I did look at an earlier paper from the Mueller group, Nat. Phys 2017. The theory model apparently is that the exotic forces are mediated by an ether-like substance that does not care to penetrate deeply into solid objects. One thing that seems clear is that in constraining such a model, experiments where the detector is a dilute gas of cold atoms (such as in the current project) gain an advantage over experiments in which both the source and the detector are made of some solid material. I don't know enough about particle theory or cosmology to understand if the notion of these curiously unassertive forces has other compelling features beyond rescuing atom interferometry experiments from what might seem to be a lack of sensitivity to short-range mass-mass coupled forces. Still, my ignorance in this matter is not an argument against publishing!

3. This manuscript represents a first attempt to do serious metrology with lattice-based interferometry, and short-distance applications, lattice interferometry is now already better than free-flight atom interferometry. The authors argue that more than a factor of 100 improvement is possible. At that point lattice-based interferometry will be able to surpass the precision of solid-detector short-range force experiments, meaning they will be the best thing going for constraining not only specialized "chameleon" models, but across the board for models of short-range forces. This is important!

EVALUATION

The gravitational forces on individual atoms form a centimeter-sized source are extraordinarily small, and it is quite impressive that the lattice-based interferometer is able to resolve forces down to 20% of that value. The authors have done an excellent job of characterizing systematic errors. With this first effort at performing serious metrology, lattice-based interferometry has already surpassed free-flight interferometry for short-range applications, and it seems likely it will pull still further into the lead in the future.

To my mind this is an important advance, and worth publishing in Nature. I am willing to look at any revised manuscript but I don't feel I need to, in order to recommend publication.

Referee #2 (Remarks to the Author):

In this manuscript, C. Panda et al. report on the operation of a suspended, intra-cavity-trapped interferometer. It allowed them to measure the gravitational attraction of a miniature source mass, consistent with Newtonian gravity, ruling out the existence of screened (Chameleon-type) dark energy theories over their natural parameter space (with 95% confidence). The combined accuracy is four times as good as the best similar measurements, by the same group, probing the same dark energy model with freely falling atoms.

The originality of the paper consists in it reporting, to my knowledge, about the first trapped atom interferometer that delivers a metrologically-competitive measurement. This is a significant step towards the design and operation of compact quantum sensors. The interferometer scheme (without the source mass) was the subject of the previous paper of the group C. D. Panda et al. "Minute-scale gravimetry using a coherent atomic spatial superposition" Arxiv:2210.07289. Here, the authors added a source mass and measured the gravitational acceleration due to it. This allowed them to improve previous bounds on Chameleon dark energy theories by a factor of 4.
The paper is well-written and clearly put in context. The work and in particular the systematics study are of excellent quality and carried rigorously. The main limitations are connected to the temperature of the atomic cloud and the variance in the tilt of the cavity axis. This was also the conclusion of the previous paper "Quantum metrology by one-minute interrogation of a coherent atomic spatial superposition" by the same team. They propose generic techniques to overcome these issues, leading to the optimistic projections of Fig. 4. They however did not apply any of these techniques/improvements in this paper compared to the two above-mentioned ones. The data analysis methods and measurement campaigns are convincing.

I would request the authors to clarify the three following major points:

1/The position of the test mass (above the upper path or below the lower one) is crucial in the phase extraction procedure. The authors mention in the supplementary material that "This procedure provides a measurement of the two elevator atom positions with respect to the source mass with better than 1 mm accuracy". I, however, do not see a systematic effect due to this large uncertainty in the systematics budget. I expect it to have a sizeable effect on the phase accumulated along the interferometer's paths. The authors should clarify this crucial point and quantitatively support how it enters their combined accuracy of 6.2 nm/s².

2/If I understand well, in the M=1 mode, the atoms are moved between the two configurations thanks to the elevator method to be above or below the Tungsten source mass of a height of 1 inch. I appreciate the study done around Fig. S3 to assess the magnitude of the related systematics to the light beam divergence. I fail, however, to connect the uncertainty of 0.5 nm/s² reported in Table 1, entry "DC Stark Shift" to the results of Fig. S3. Could the authors clarify how do they get to this value?

3/The title of the paper is suggesting that the authors operated a gravimeter, which is not the case. It is clear to me that the experiment is well suited for studies involving a movable mass (such as AB-effects, G measurements, etc.) profiting from differential scheme operations. To operate a

(compact) gravimeter, it is not clear at all that the method is competitive.
Therefore, I suggest that the authors:
(i) compare their measurements to a G determination and where they stand in comparison with G. Rosi et al. Precision measurement of the Newtonian gravitational constant using cold atoms. Nature 510, 518–521 (2014).
(ii) add a paragraph clarifying what the limits are in the case of an absolute gravimeter and what would the way forward to be competitive with state-of-the-art. References to state-of-the-art gravimeters (A. Peters, Berlin and F. Pereira, Paris) are also missing.

In conclusion, in view of the previous published results of the group and the modest improvement (factor of 4) constraining the very particular Chameleon model of Dark Energy, I do not recommend the publication of this article in Nature.

Provided that the authors address my three major concerns above, I believe the manuscript would be appropriate for more specialised journals as Nature Physics.

Referee #3 (Remarks to the Author):

This paper presents the results of new atom interferometer experiment, and the bounds on chameleon and symmetron modified gravity theories that they imply.

The bounds that are obtained exclude new regions of parameter space, but I do not find this sufficient for publication in nature. Specifically, the paper does not meet the standard set out in the criteria for acceptance: "a paper should represent an advance in understanding likely to influence thinking in the field, with strong evidence for their conclusions. There should be a discernible reason why the work deserves the visibility of publication in a Nature Portfolio journal rather than the best of the specialist journals."

I have two reasons for reaching this conclusion.

First, this work does not represent an advance in understanding likely to influence thinking in the field. Specifically, the exclusion of the regions of parameter space shown in figure 4 does not contribute significantly to our understanding of these theories. There are no phenomena that can be explained by screened modified gravity with parameters in the excluded region. Consequentially, this work is unlikley to influence thinking in this field.

Second, I do not believe this work deserves the visibility of publication in a Nature Portfolio journal rather than the best of the specialist journals. The authors consistently refer to these theories as dark energy. This is incorrect. Chameleon and symmetrons have nothing to do with dark energy, especially in the parameter range that is accessible to atom interferometers. These theories are simply models for non-linear fifth-forces. It is often claimed that that black line at the "dark energy scale" that the authors show in their figure 4 is where the field can drive dark energy but this too is incorrect. It is merely a coincidence between the value of a parameter in the model and the density of dark energy. There is no relation between this model parameter and the physics of dark energy. Any claim of such is speculation.

The author's measurement is clearly a new milestone for atom interferometers, but, given the discussion above, I do not believe that the resultant bounds on chameleon and symmetron theories are sufficiently important to warrant publication in nature. I encourage the authors to take some time to understand the theoretical foundations of these theories, and their connections, or, rather, lack of connections, to cosmology.

Author Rebuttals to Initial Comments:

Referees' comments:

Referee #1 (Remarks to the Author):

I enjoyed reading this manuscript. On the face of it, precision measurements of potential gradients by atoms in a lattice is a crazy idea, since the optical potentials are very large and fairly inhomogeneous. I've heard of this idea before but did not imagine it could be as successful, so precise, as has been demonstrated in this manuscript. The results are quite impressive.

Response: We thank the referee for this comment. We added a remark in the abstract about how the optical cavity is instrumental in achieving a highly spatially consistent optical lattice potential.

The discussion on systematic errors was very thorough and very compelling, and particularly welcome given what might be seen as the systematic-prone nature of trying to measure forces on confined particles. It is clear that this method is much more likely to be useful when one can readily modulate the sources of exotic forces, what the authors call the M switch.

MINOR COMMENTS AND QUESTIONS.

1. Figure 3 caption refers to parts "a…b…b….c". I think it should be "a…b…c….d".

Response: Thank you, we have corrected the caption labeling.

2. Do you use different value of tau for differing delta z? It seems like the decoherence rate depends on delta z and thus one might get better results by re-optimizing tau for each of the values of delta z.

Response: The referee is correct that re-optimizing tau for each delta z could slightly improve the results. We use the same tau = 2.3 s for all separations because it simplifies data acquisition at little cost in performance.

The paragraph following Eq. 4, lines 119-123 describes the scaling of the sensitivity with \Delta z and \tau using empirical equations, which do indeed suggest a different optimal \tau for each \Delta z. For our use of \tau = 2.3 s, we find that the sensitivities are 97%, 99%, 86% of the optimum for \Delta z = 4.2, 6.6, 9.4 um, respectively. Since these are within 15% of ideal, there is little loss in sensitivity. We have added a sentence on line 147 that clarifies this point.

SIGNIFICANCE OF RESULTS PRESENT AND FUTURE.

1. Although our current understanding of the numerical value of big G is in a deplorable state, it is not clear that lattice-based atom interferometry can help. To make an important contribution in that business, one must be able to calculate the expected acceleration from a switchable mass

distribution to a part in 10^4, which is to say, about 300 times better than in the current manuscript. It seems to me that progress in big G happens not so much with new ways of measuring acceleration such as lattice-based interferometry, but with clever ways of positioning source masses and of designing experimental protocol which are forgiving of measurement error and possible internal density fluctuations.

Response: We appreciate this comment and agree that progress in measuring G requires source masses with well-characterized internal density and accurate positioning. The lattice atom interferometer has distinct advantages in these regards: In previous atom interferometers, with atoms in free-fall, the atoms moved on 0.1-1-meter-scale trajectories, which are hard to control with millimeter or even micrometer precision. Also, the meter-scale trajectories required using dozens of source masses totalling hundreds of kg. It is hard to control their density and position accurately. By contrast, in our approach the atoms do not fall and can be precisely positioned in the lattice in the vicinity of a single, small (0.1 kg to several kg) source mass. The density of this source mass is easier to characterize. We believe that this is crucial in order for atom interferometers to make a stronger contribution to the measurement of big G).

We thank the referee for the discussion and we have added a last paragraph describing these prospects for big G measurements, lines 305-310.

2. Over the past 25 years there have been many attempts to observe or at least constrain “Weird Gravity” (any deviation from the usual 1/r^2 force law). Looking for deviations at short length scale is particularly difficult because not only the detector but also the effective size of the source must shrink, and at fixed source density the corresponding forces fall very rapidly as the length scale to be probed shrinks. The current paper seems to be another effort in this tradition, and for this reason I was surprised not to see a version of the plot which has appeared in almost all the previous efforts, with the exotic mass-mass coupling being modeled as a Yukawa potential and the exclusion being plotted as a limit on alpha, the coupling strength in units of gravitational constant G, versus lambda, the Yukawa length. These plots are as I say are ubiquitous -- in 30 seconds on google I was able to find e.g. figure 4 of Yang et al PRL 108, 091101 (2012) although there has since then been further progress. In the end I had to more or less guess how the current data would fall on such a plot. The authors put a limit on “weird acceleration” at about 30% of gravity, so at the most favorable range of lambda, say around 2 cm, the exclusion would be for alpha > 0.3. The authors report the atoms are never closer than 4 mm to the source, so for a yukawa length of say 0.2 mm, their sensitivity is going to be greatly suppressed by the exponential in the Yukawa potential. I’m going to guess at 0.2 mm their sensitivity might only good enough to exclude alpha > 1000. In this way of parameterizing sensitivity, the current work is a factor of 30 or more times less sensitive than competing short-range gravity experiments based on solid-mass sensors.

With this in mind I had to stare a little at the three exclusion plots in the current paper, plots which show instead that actually the current experiment offers best-ever exclusions. I did not go all the way back to the theory papers but I did look at an earlier paper from the Mueller group, Nat. Phys 2017. The theory model apparently is that the exotic forces are mediated by an ether-like substance that does not care to penetrate deeply into solid objects. One thing that seems clear is that in constraining such a model, experiments where the detector is a dilute gas of cold atoms (such as in the current project) gain an advantage over experiments in which both the source and the detector are made of some solid material. I don't know enough about particle theory or cosmology to understand if the notion of these curiously unassertive forces has other compelling features beyond rescuing atom interferometry experiments from what might seem to be a lack of sensitivity to short-range mass-mass coupled forces. Still, my ignorance in this matter is not an argument against publishing!

Response: Indeed, using a dilute gas of cold atoms gives enhanced sensitivity to screened fields, leading to the sensitivity gains of atom interferometry compared to classical sensors mentioned by the referee.

We thank the referee for placing our results into context for fifth-force measurements. We have now explored in more detail current bounds and prospects for testing fifth-force physics in a more general framework, such as Yukawa-type potentials, see the new Figure 4d and associated discussion. Improving the performance of the lattice interferometer and using optimized geometries would enable competitive measurements of Yukawa deviations from the 1/r gravity law at short ranges <1 cm.

3. This manuscript represents a first attempt to do serious metrology with lattice-based interferometry, and short-distance applications, lattice interferometry is now already better than free-flight atom interferometry. The authors argue that more than a factor of 100 improvement is possible. At that point lattice-based interferometry will be able to surpass the precision of solid-detector short-range force experiments, meaning they will be the best thing going for constraining not only specialized "chameleon" models, but across the board for models of short-range forces. This is important!

EVALUATION

The gravitational forces on individual atoms form a centimeter-sized source are extraordinarily small, and it is quite impressive that the lattice-based interferometer is able to resolve forces down to 20% of that value. The authors have done an excellent job of characterizing systematic errors. With this first effort at performing serious metrology, lattice-based interferometry has already surpassed free-flight interferometry for short-range applications, and it seems likely it will pull still further into the lead in the future.

To my mind this is an important advance, and worth publishing in Nature. I am willing to look at any revised manuscript but I don't feel I need to, in order to recommend publication.

Response: We appreciate the careful review of the manuscript and thank the referee for the positive recommendation.

Referee #2 (Remarks to the Author):

In this manuscript, C. Panda et al. report on the operation of a suspended, intra-cavity-trapped interferometer. It allowed them to measure the gravitational attraction of a miniature source mass, consistent with Newtonian gravity, ruling out the existence of screened (Chameleon-type) dark energy theories over their natural parameter space (with 95% confidence). The combined accuracy is four times as good as the best similar measurements, by the same group, probing the same dark energy model with freely falling atoms.

The originality of the paper consists in it reporting, to my knowledge, about the first trapped atom interferometer that delivers a metrologically-competitive measurement. This is a significant step towards the design and operation of compact quantum sensors. The interferometer scheme (without the source mass) was the subject of the previous paper of the group C. D. Panda et al. “Minute-scale gravimetry using a coherent atomic spatial superposition” Arxiv:2210.07289. Here, the authors added a source mass and measured the gravitational acceleration due to it. This allowed them to improve previous bounds on Chameleon dark energy theories by a factor of 4.

The paper is well-written and clearly put in context. The work and in particular the systematics study are of excellent quality and carried rigorously. The main limitations are connected to the temperature of the atomic cloud and the variance in the tilt of the cavity axis. This was also the conclusion of the previous paper ”Quantum metrology by one-minute interrogation of a coherent atomic spatial superposition” by the same team. They propose generic techniques to overcome these issues, leading to the optimistic projections of Fig. 4. They however did not apply any of these techniques/improvements in this paper compared to the two above-mentioned ones. The data analysis methods and measurement campaigns are convincing.

Response: We thank the referee for the analysis and the positive commentary on our work.

I would request the authors to clarify the three following major points:

1/The position of the test mass (above the upper path or below the lower one) is crucial in the phase extraction procedure. The authors mention in the supplementary material that “This procedure provides a measurement of the two elevator atom positions with respect to the source mass with better than 1 mm accuracy”. I, however, do not see a systematic effect due to this large uncertainty in the systematics budget. I expect it to have a sizeable effect on the phase accumulated along the interferometer’s paths. The authors should clarify this crucial point and quantitatively support how it enters their combined accuracy of 6.2 nm/s².

Response: We thank the referee for the careful reading. The positioning error of 1 mm is accounted for in the error bar of the calculated Newtonian acceleration from the source mass, a^{calc}_{mass}, which is 1 nm/s^2, as described in the methods section “Determination of source mass Newtonian gravitation attraction”. This error bar is small because the atoms are positioned closely to the location that gives maximum gravitational acceleration from the source mass. At this location, the gravitational acceleration is only weakly dependent on the atom position. Figure S2 shows the variation of the acceleration as a function of position. We added a sentence in lines 497-499 clarify this.

2/If I understand well, in the M=1 mode, the atoms are moved between the two configurations thanks to the elevator method to be above or below the Tungsten source mass of a height of 1 inch. I appreciate the study done around Fig. S3 to assess the magnitude of the related systematics to the light beam divergence. I fail, however, to connect the uncertainty of 0.5 nm/s² reported in Table 1, entry "DC Stark Shift" to the results of Fig. S3. Could the authors clarify how do they get to this value?

Response: We appreciate the opportunity to clarify. The beam divergence systematic and the DC Stark shift systematic are indeed separate. The beam divergence effect includes the error from moving the atoms (the E-switch), as described in section "AC Stark shift difference between upper and lower atom positions, a^E" , in particular Eq. 6. It leads to a systematic error of 0.07 nm/s^2 [Table 1, entry "a^E (via dBz/dz)"]. The detailed mechanism is described in the supplement, section "Systematic investigation: a^E phase shift model ". The "DC Stark shift" systematic of 0.5 nm/s^2 is different. It arises from possible electric fields due to residual surface charges and is described in section "Source mass surface". We have added remarks on lines 205 and 232 and to specify the correspondence between the systematic error descriptions and Table I entries.

3/The title of the paper is suggesting that the authors operated a gravimeter, which is not the case. It is clear to me that the experiment is well suited for studies involving a movable mass (such as AB-effects, G measurements, etc.) profiting from differential scheme operations. To operate a (compact) gravimeter, it is not clear at all that the method is competitive.

Response: Our manuscript describes the first use of lattice interferometry to "measure the gravity" of a source mass, which we feel is consistent with the title. We use a differential measurement to subtract out Earth's gravity.

Therefore, I suggest that the authors:

(i) compare their measurements to a G determination and where they stand in comparison with G. Rosi et al. Precision measurement of the Newtonian gravitational constant using cold atoms. Nature 510, 518–521 (2014).

Response: Thank you for this suggestion. We have added a paragraph in conclusion, manuscript lines 305-310 that describes the prospects of using lattice interferometry for measuring the gravitational constant G.

(ii) add a paragraph clarifying what the limits are in the case of an absolute gravimeter and what would the way forward to be competitive with state-of-the-art. References to state-of-the-art gravimeters (A. Peters, Berlin and F. Pereira, Paris) are also missing.

Response: We thank the referee for this suggestion and have added the requested remarks to the penultimate paragraph, lines 298-302, including on reducing systematics from the divergence of the laser beam and increasing long-term stability by stabilizing the optical lattice tilts. We have also added two more references to state-of-the-art gravimeters used for mobile and industrial applications.

In conclusion, in view of the previous published results of the group and the modest improvement (factor of 4) constraining the very particular Chameleon model of Dark Energy, I do not recommend the publication of this article in Nature.

Provided that the authors address my three major concerns above, I believe the manuscript would be appropriate for more specialised journals as Nature Physics.

Response: We thank the referee for their perspective. However, our result will appeal to Nature's broad readership and have a broad and lasting impact on many counts, some of which have been pointed out by the reviewers:

1. [Highly visible topics] Atomic gravimetry and atomic measurements of short-distance gravity are highly visible topics (A Peters et al., Nature, 1999, 1206 citations; Rosi et al, Nature, 2014, 807 citations; Hamilton et al.,Science, 392 citations;...; as well as wide interest in the emerging quantum industry, e.g., AOSense, MuQuans, or Vector Atomic). Precision control of quantum states with many seconds of coherence is another highly visible topic, enabling quantum sensing, quantum metrology, and quantum simulation.
2. [Pioneering work] As mentioned by referees 1 and 2, we present the first quantum metrology with trapped-atom interferometry, based on a powerful and widely applicable strategy to overcome systematic errors. This accomplishment has been a long-standing goal in the community. For short-distance applications, it is already "metrologically competitive" (Referee 2), surpassing free-flight atom interferometry
3. [Future potential] More than a factor of 1000 improvement is possible. We believe that, on the path to that point, lattice-based interferometry may become the preferred method for a broad range of quantum sensing applications. Some of these exciting prospects are now discussed in the last paragraphs of the paper, thanks to the referee's input.

Referee #3 (Remarks to the Author):

This paper presents the results of new atom interferometer experiment, and the bounds on chameleon and symmetron modified gravity theories that they imply.

The bounds that are obtained exclude new regions of parameter space, but I do not find this sufficient for publication in nature. Specifically, the paper does not meet the standard set out in the criteria for acceptance: "a paper should represent an advance in understanding likely to influence thinking in the field, with strong evidence for their conclusions. There should be a discernible reason why the work deserves the visibility of publication in a Nature Portfolio journal rather than the best of the specialist journals."

I have two reasons for reaching this conclusion.

First, this work does not represent an advance in understanding likely to influence thinking in the field. Specifically, the exclusion of the regions of parameter space shown in figure 4 does not contribute significantly to our understanding of these theories. There are no phenomena that can be explained by screened modified gravity with parameters in the excluded region. Consequentially, this work is unlikely to influence thinking in this field.

Second, I do not believe this work deserves the visibility of publication in a Nature Portfolio journal rather than the best of the specialist journals. The authors consistently refer to these theories as dark energy. This is incorrect. Chameleon and symmetrons have nothing to do with dark energy, especially in the parameter range that is accessible to atom interferometers. These theories are simply models for non-linear fifth-forces. It is often claimed that that black line at the "dark energy scale" that the authors show in their figure 4 is where the field can drive dark energy but this too is incorrect. It is merely a coincidence between the value of a parameter in the model and the density of dark energy. There is no relation between this model parameter and the physics of dark energy. Any claim of such is speculation.

Response: We thank the referee but respectfully disagree with this position.

Our results represent an important advance in the long-standing experimental program aimed at testing gravity, including the search for deviations from Netwonian gravity, on various scales. We have edited the introductory paragraphs to emphasize this point. We acknowledge lively debates in the community about the impact of chameleon and symmetron theories on cosmology and dark energy. Since our manuscript is not the place to settle this, we have now removed the direct references to cosmology implications that the referee finds objectionable and added a statement acknowledging the debate. That said, the usefulness of chameleons/symmetrons as concrete and testable models of non-linear fifth forces, with rich phenomenology in the ultra weak-field regime, is by now well-established.

To highlight that optical lattice interferometry offers even broader opportunities, beyond chameleons and symmetrons, we have now added Figure 4d to show that proposed upgrades to the optical lattice interferometer will improve constraints on generic Yukawa-like deviations from the inverse-square law at short distances. We also added a discussion of the advantages of the optical lattice interferometer in measurements of the gravitational constant and beyond. We thank the reviewer for expressing the above critique and hope that these changes have addressed the concern.

The author's measurement is clearly a new milestone for atom interferometers, but, given the discussion above, I do not believe that the resultant bounds on chameleon and symmetron theories are sufficiently important to warrant publication in nature. I encourage the authors to take some time to understand the theoretical foundations of these theories, and their connections, or, rather, lack of connections, to cosmology.

Response: We thank the reviewer for stating that “the author's measurement is clearly a new milestone for atom interferometers", demonstrating the strengths of lattice interferometry in probing new physics. Indeed, the main contribution of our work is to demonstrate the power of lattice atom interferometry in testing the nature of gravity at short distances and in the “ultra weak-field” regime (where a force of gravitational strength is further suppressed by ‘screening’ as a function of the environment). In the words of Referee #1, precision measurements of potential gradients by atoms in a lattice were "thought to be a crazy idea”. For this purpose, screened theories represent a metric with which to compare our method to previous experiments with atoms in free-fall, which have been at the forefront of reducing available parameter space for such theories.

This is just a first step towards more applications within gravimetry and fundamental physics measurements, a demonstration of the opportunities opened by the new lattice interferometry technology. For this reason, and because of the very broad appeal described in our reply to reviewer 2, we believe that the paper is a very strong and timely contribution to Nature.

Reviewer Reports on the First Revision:

Referees' comments:

Referee #1 (Remarks to the Author):

The revised manuscript addresses all the concerns I had with the first version. I was in favor of publishing the original version, and I'm still in favor of publishing this revised version. I see that referees #2 and #3 have reservations about this paper appearing in Nature. In particular, they are worried that the main technical advances were presented in earlier publication, and that the modest exclusions presented here are for rather particular, perhaps not very well motivated. I'm sympathetic to both of these concerns. On the whole I come down on the side of publication, but I think the other referees, with their suggestions that the paper might belong in a more specialized journal, are not obviously wrong.

Referee #2 (Remarks to the Author):

The authors clarified my raised points 1-3 in a convincing manner.

Regarding my suggestions, they addressed the second one (ii) and added a useful discussion about the potential use of lattice atom interferometers as absolute gravimeters together with the appropriate references to state of the art, which makes it fair.

About the first suggestion (i) to anticipate G measurements with lattice atom interferometry, the modifications are more modest and on a qualitative level despite adding one author who made the state of the art measurement of G (Tino). I expected more quantitative comparisons and improvement scenarios here.

Recommendation: Previously, I recommended to publish the paper in a more specialised journal of the Nature family if my concerns 1-3 are addressed. I am pretty much convinced with the answers to these points. The authors gave 3 arguments on why their article should appear in Nature. It's true the topic of atomic gravimetry is highly visible but the advances they are making in this field with this study are not sufficiently impactful in terms of performance to be worth the visibility of the Nature journal.

Author Rebuttals to First Revision:

We thank the referees for their contributions to improving the quality of the manuscript. We add responses below:

Referee #1 (Remarks to the Author):

The revised manuscript addresses all the concerns I had with the first version. I was in favor of publishing the original version, and I'm still in favor of publishing this revised version. I see that referees #2 and #3 have reservations about this paper appearing in Nature. In particular, they are worried that the main technical advances were presented in earlier publication, and that the modest exclusions presented here are for rather particular, perhaps not very well motivated. I'm sympathetic to both of these concerns. On the whole I come down on the side of publication, but I think the other referees, with their suggestions that the paper might belong in a more specialized journal, are not obviously wrong.

Response: We thank the referee and appreciate the positive recommendation.

Referee #2 (Remarks to the Author):

The authors clarified my raised points 1-3 in a convincing manner.

Regarding my suggestions, they addressed the second one (ii) and added a useful discussion about the potential use of lattice atom interferometers as absolute gravimeters together with the appropriate references to state of the art, which makes it fair.

About the first suggestion (i) to anticipate G measurements with lattice atom interferometry, the modifications are more modest and on a qualitative level despite adding one author who made the state of the art measurement of G (Tino). I expected more quantitative comparisons and improvement scenarios here.

Response: We have reworked the final paragraph to clarify prospects for measuring G. The three orders or magnitude increased sensitivity along with the use of larger masses could give sensitivity compatible with or beyond the state of the art.

Recommendation: Previously, I recommended to publish the paper in a more specialised journal of the Nature family if my concerns 1-3 are addressed. I am pretty much convinced with the answers to these points. The authors gave 3 arguments on why their article should appear in Nature. It's true the topic of atomic gravimetry is highly visible but the advances they are making in this field with this study are not sufficiently impactful in terms of performance to be worth the visibility of the Nature journal.

Response: The performance shown here is for the first time sufficient to perform the most precise atom interferometric measurement of the gravitational field of a small source mass. Such tests, previously done with atoms in free-fall, have been instrumental in probing fifth forces, particularly in testing “screened potential” theories.