nature portfolio

Peer Review File

Photophoretic flight of perforated structures in near-space conditions

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This file contains all reviewer reports in order by version, followed by all author rebuttals in order by version.

Version 0:

Reviewer comments:

Referee #1

(Remarks to the Author)

This work studies photophoretic levitation on thin 1 cm plates with micrometer substructure and manufactured relevant physical properties of top and bottom sides. It combines models and measurements and shifts the possibilities somewhat. Future structures along these lines of optimising might be suitable small carriers for mesospheric probes. So this paper might be a step toward the next milestone of observing free flying plates in a solar like spectrum.
Here is a list of things that could be improved.

- Radiation is not well defined throughout the paper. A light flux of the unit „sun“ is often used. It kind of reads as solar constant later in the paper (1367 W/m2). However, it is unclear to what wavelength range this refers as „sun“ implies some solar spectrum.

- The measurements are carried out at one wavelength of 447 nm. This is not a solar spectrum. In fact, the manufactured structure has an interference layer specifically optimized for roughly this wavelength. Therefore, other parts of the solar spectrum are not absorbed as well by the plates. This implies that any levitation in the lab is much stronger than for solar insolation. It is not clear that this can easily be adapted by other surface layers for the whole spectrum. In fact, to show the applicability to the atmosphere a broader spectrum should be used.

- Using a laser, not only specific interference layers on each side are important but the radiative interaction has ample opportunities for destructive and constructive interferences with the structures in general. It is not clear to what effects this amounts. Also here, an incoherent light source might be an alternative.

- A general note on units: these are given and used kind of sloppy throughout. Apart from „sun“, which is not exactly an SI unit, torr is also not. Fig. 1d has „area force per light intensity“ which gives area force as g/m2 if I take out „sun“. This then comes with an „m“ in italics and a bold „g“, which implies g to be Earth’s acceleration. This area force should be defined as this is not a usual quantity throughout physical sciences. It might also be confused with the surface density g/m2 both in bold in the text later.
In fig. 2 f,g,h and others: the unit on the y-axis is given as (m). But it says as text log10 of the radius. A log function does not have a unit so it should be log10 (radius / m).

- The authors say that optimizing the optical properties is straightforward by adding various layers. This stays nebulous until the experiment is described. In view of one specific interference layer given later, this also sounds a bit exaggerated.

- It is not clear why eps_IR are different for day and night and top and bottom.
emissivity is usually just a physical property for a given wavelength (range). If it is already tied to Earth’s albedo or such, this would no longer be just an emissivity.

- Line 176 mentions the dependence of the dimensionless force on the rarefaction factor.
Again, it would be good to know if this is related to the pore radius or overall size.

- The dimensionless force is known to people working on photophoresis but the term needs an immediate few word explanation when it first shows up for other readers.

- It is hard to see in Fig. 2 h,i what the important differences might be. On first sight, substructure might not be the game changer.

- Line 265: The text says that the models agree well with the measurements below 1 Torr. Fig. 3f,g does not agree with this. They do only fit beyond 0.01 Torr. Somewhat below (line 295 ff) these data are again discussed, now agreeing with my observation that the fits are not so well. In fact, I only see the usual photophoretic peak at a rarefaction factor (or Knudsen number) of 1, which is not very model sensitive to my experience. If so, model and experiment do not really match or benefit from each other. The title says „model guided“. What did the model guide to do?

- It is hard to connect fig. 1d and fig. 3h. Both are on experimental measured lifts. Fig 1. has more than 2 at maximum (black circles) with the given units (see above), I would estimate to be the lift over gravity ratio for 1 „sun“. Fig. 3 h gives forces directly and gravity and at 1 „sun“ has less than two.

- The lift of a structure at sub-solar (at 447nm) is mentioned in the abstract but not much of a focus in the main text. In fact Fig. 4b s hard to see. No note is given how this would translate to solar spectrum.

- This work heavily seems to draw on references 5 and 9 from the same authors. It could be made clearer what the innovation in this work is.

- Ref. 8 is a 20 year old, non-reviewed reference.

Referee #2

(Remarks to the Author)
The paper presents exciting theoretical and experimental findings related to photophoretic structures for near-space exploration. The novel contributions include:

1) Design of structures with minimal ligaments to maximize the temperature difference and, consequently, the lift force.
2) Development of a new experimental setup for measuring lift force.
3) Measurements of lift force on cm-scale structures in various gases.

Before recommending the paper for publication, I request that the authors address the following questions and concerns:
1) Line 222: The authors state, "to maximize payload capacity independent of altitude, a structure should have no perforations." This assertion appears to contradict Fig. 2e, where the lift force is maximized at a hole fraction of approximately 0.7 and a temperature difference of delta = 100. Additionally, the sentence could be rephrased to avoid a possible misinterpretation that a no-perforation structure maximizes force at *every* altitude.
2) Figure 3g: The experimental results for the photophoretic force in helium (He) and other gases are compared with theoretical fits, none of which match the data very well. Can the authors achieve a better fit by using alternative values for the NEAC and TMAC?
3) Line 318: The authors write, "To our knowledge, this is the first reported photophoretic levitation of a macroscopic object under 1 sun of illumination and is a critical development in establishing the feasibility of sunlight-powered photophoretic aircraft." Although not yet published, a recent preprint (https://arxiv.org/abs/2312.15592) appears to report levitation at intensities below 1000 W/m².
4) Line 344: The authors propose using a "ground-based near-IR or microwave laser and small (3-10 cm) aperture telescope." How do the authors propose to track the position of the structures in the sky? What level of tracking accuracy is feasible, and what laser power would be required given the tracking accuracy? Additionally, the authors should address the challenges posed by the diffraction-limited angular divergence of microwave lasers/masers for such small apertures, which would require very high power. Finally, do the authors anticipate their structures will absorb microwave radiation effectively?
5) Line 349: The authors calculate that "a device with r = 1 cm, β = 0.5, and H = 125 μm would generate 0.5 g/m² at 75 km at night," suggesting that sustained lofting is possible for lightweight devices. Could the authors propose specific dimensions for a structure capable of remaining aloft both day and night? Additionally, in Fig. 4e, they model nighttime descent. Can they design a structure that achieves continuous levitation, perhaps by ascending during the day and descending at night?
6) Line 360: The authors suggest that "deployment might be achieved using high-altitude balloon technology that can lift kg-scale payloads to the 50 km stratopause." Can the authors be specific about the dimensions and payload capacities of a structure that is capable of generating sufficient lift at altitudes of 50 km (which is necessary for deployment from a balloon)?
7) Figure 4g: The predicted horizontal airspeeds for tilted structures reach 350 m/s, which is likely supersonic at 75–80 km altitudes and therefore implausibly high. The authors may want to revisit their drag assumptions in deriving these speeds. For instance, their use of subsonic, low-Reynolds-number drag in Eq. S25 may need reconsideration.
8) The proposed payload capacities are relatively low, limiting practical utility. Perhaps the authors could provide more quantitative insights into how this technology could outperform existing mesospheric tools, such as remote satellite observation or ground-based radar and lidar measurements. Including a comparison with existing technologies for atmospheric sensing and Martian exploration would contextualize the benefits of this approach.
9) Supplementary Material, Page 21: The authors write, "The antenna can be etched directly into the SS." It appears that "SS" refers to "silicon substrate," but this abbreviation is not defined in the text.

Referee #3

(Remarks to the Author)
The authors present a feasibility study for a photophoretic aircraft operating in the mesosphere. They introduce a microstructured (perforated) photophoretic surface and show that it outperforms previous designs by a factor of two in terms of areal force. The authors performed a ground-based levitation experiment and demonstrated that with a 300mW blue laser they are able to lift a 1cm2 sample with a weight of about 1mg. Based on this demonstration they discuss the requirements and challenges for the realization of a photophoretic aircraft operating in the mesosphere.

This work presents a technological advance, that is, the enhancement of the photophoretic force by means of an engineered surface. To me, the manuscript reads like a proposal, i.e. based on a preliminary study it presents ideas that have yet to be realized. Based on simulations the authors provide estimates, but in some places these are quite speculative. I struggled with the general presentation of this work, i.e. many terms are undefined in the introduction and require to jump back and forth in the manuscript. All in all, I think this manuscript presents a noteworthy technological advance and a bunch of clever ideas. In the following I list some of my concerns and questions.

1) The meaning of the ``normal energy accommodation coefficient” should be explained in the introduction. What are the units? How is it measured?

2) Key parameters of the mesosphere should be mentioned (temperature, pressure, distance to earth).

3) A reader has to be familiar with Shapirov (2024) in order to understand the terms. What is a dimensionless heat flux, what is a dimensionless force?

4) Many readers won’t understand light intensities in units of ``suns”. Also, this unit might be misleading, as it does not specify the wavelength. The authors used a blue laser for their experiment, which covers only a small fraction of the sun’s spectrum. But photophoretic propulsion relies on absorption, which depends on wavelength, and might be strongest in the blue spectral range. The use of a blue laser might therefore not reflect the performance under solar radiation.

5) It would help to compare the photophoretic force with the radiation pressure force. I estimate that it is 6 orders of magnitude stronger.

6) In Fig.3. the weight of the lofting surface has not been specified.

7) Propulsion with an earth-based laser during night times needs further discussion. I estimate you require a one Watt laser expanded to over one meter and then focused on the spacecraft. How will you track the spacecraft so it remains in the focus? What about clouds?

Version 1:

Reviewer comments:

Referee #1

(Remarks to the Author)
All my concerns were answered appropriately, which is much appreciated.
I have no significant further comment and would recommend publication.

Gerhard Wurm

Referee #2

(Remarks to the Author)
The authors have adequately addressed my prior concerns and I am happy to recommend the paper for publication, assuming the can address the minor comments below. I particularly appreciate the the description of the conditions under which the proposed structures can levitate even at night using IR emission from the earth. This is a conceptually new idea that would be interest to a broader audience.

on line 83, the authors write "characteristic size is typically its smallest 83 dimension perpendicular to ΔT or $\Delta \alpha n$,". While I understand what this sentence means, I think many readers may find it confusing and I recommend illustrating with in a figure.

on line 110, the authors need to explain the physical meaning of the dimensionless force and heat fluxes in a way that could eb understood by a non-specialist.

on line 138, I do not know what the sentence "Averaging over the year reduces the time-averaged lofting force by 3 %" means. 3% compared to what?

Referee #3

(Remarks to the Author)
I have read the authors' revised manuscript and their reply. The quality of the presentation increased and my questions/answers were answered satisfactorily. This work is a proof-of-concept study and definitely contains innovative elements and ideas. Although I'm not an expert in this field and can't judge the significance and the impact, I was very impressed by the results and the potential applications to space science. There are some issues that I believe should still be fixed or addressed:

(1) Line 32: The sentence ``We observed photophoretic levitation of a 1 cm-wide structure when illuminated by .." requires specification of the air pressure.

(2) Line 105: It is not clear at what distance from the membranes Tb and Tf are evaluated. According to Fig 2c it is ``far", but what does that mean? At infinite distance the two temperatures should be equal.

(3) Line 110: If something is dimensionless then it has been normalized with something. This something has to be defined in the main text. This normalization is defined by Eq.(7) in Methods, but needs to be mentioned in main text for sake of readability.

(4) Line 161: g/m2 is not a unit of force. Force has units of Newtons.

Responses to referees

We thank all reviewers for their constructive feedback and suggestions. Below, please find our responses in blue.

Referee #1 (Remarks to the Author):

This work studies photophoretic levitation on thin 1 cm plates with micrometer substructure and manufactured relevant physical properties of top and bottom sides. It combines models and measurements and shifts the possibilities somewhat. Future structures along these lines of optimising might be suitable small carriers for mesospheric probes. So this paper might be a step toward the next milestone of observing free flying plates in a solar like spectrum.
Here is a list of things that could be improved.

Radiation is not well defined throughout the paper. A light flux of the unit „sun“ is often used. It kind of reads as solar constant later in the paper (1367 W/m2). However, it is unclear to what wavelength range this refers as „sun“ implies some solar spectrum.

Response: We agree that the use of "sun" as a unit is inappropriate within this paper. Indeed, it was meant to represent the solar constant, but this was confusing in the context of monochromatic laser-based experiments. We replaced all uses of the quantity "sun" with the appropriate units of W/m^2, making sure to clarify the relevant light source where appropriate.

The measurements are carried out at one wavelength of 447 nm. This is not a solar spectrum. In fact, the manufactured structure has an interference layer specifically optimized for roughly this wavelength. Therefore, other parts of the solar spectrum are not absorbed as well by the plates. This implies that any levitation in the lab is much stronger than for solar insolation. It is not clear that this can easily be adapted by other surface layers for the whole spectrum. In fact, to show the applicability to the atmosphere a broader spectrum should be used.

Response: The absorptive bottom layer of the structure was not designed to optimize absorbance specifically at 447 nm, but rather the most intense part of the solar spectrum, 400 – 700 nm. To show this, we added analysis of the spectral sensitivity of the structural layers.

First, we added UV-visible-near-IR and Fourier transform IR spectroscopic data of the bottom layer to Extended Data Figure 2. The methods for analyzing this data are now described in section 6 of the SI. The spectroscopic data show that the structure is highly absorptive where the solar spectrum peaks, with only about 10 % within the range 400 - 700 nm, indicating a monochromatic source within this range is absorbed similarly to broadband sunlight.

Second, we added rigorous coupled-wave analysis to analyze any interference patterns within and between the films. These simulations are also discussed in section 4 of the SI. The results agree with the spectroscopic measurements and show no unexpected interference effects. The top alumina layer has a negligible effect on the structure's absorbance. Furthermore, analysis of the full structure shows no evidence of interference due to the spacing between the membranes.

Lastly, we performed additional flight demonstrations of freestanding structures using two broadband light sources: a halogen arc lamp and a white LED. Five new Supplementary Videos highlight the results of these demonstrations. Notably, broadband light produced flight at slightly lower light intensities than what was observed with a laser. Using just a white LED, we observed a structure fly at about 750 W/m^2 of illumination, roughly 55 % the equivalent intensity of sunlight.

Using a laser, not only specific interference layers on each side are important but the radiative interaction has ample opportunities for destructive and constructive interferences with the structures in general. It is not clear to what effects this amounts. Also here, an incoherent light source might be an alternative.

Response: Our response to the reviewer's previous comment also addresses these concerns.

A general note on units: these are given and used kind of sloppy throughout. Apart from „sun", which is not exactly an SI unit, torr is also not. Fig. 1d has „area force per light intensity" which gives area force as g/m2 if I take out „sun". This then comes with an „m" in italics and a bold „g", which implies g to be Earth's acceleration. This area force should be defined as this is not a usual quantity throughout physical sciences. It might also be confused with the surface density g/m2 both in bold in the text later.

Response: We replaced all non-SI units in our revision. Torr has been replaced with Pascals and "sun" has been replaced with W/m^2. In Fig. 1d, we simplified the units to a more meaningful quantity: lofted mass per unit light power (mg/W).

The lofting force per unit area on a structure has SI units of N/m^2. However, we find a more intuitive choice of units to be g/m^2, since this can be easily compared to the area density (also in g/m^2) of a device. To get from N/m^2 to g/m^2, we divide by the gravitational constant (9.8 m/s^2) and multiply by the conversion (1000 g = 1 kg). We have made this clear by adding a sentence in lines 116-118. To avoid confusion between grams and Earth's gravitational acceleration (which is referred to as the italicized quantity *g* in the SI), we only refer to Earth's gravitational acceleration explicitly in the main text. The italicized units in the axis labels were a typesetting error and have been corrected. Furthermore, force measurement data (e.g. Fig. 3f-h) is reported in units of mass (e.g. µg) to easily compare to the weight of the test structures and payloads. This was again calculated by dividing the measured forces by gravitational acceleration. We now state this in the caption of every figure that shows measured data.

In fig. 2 f,g,h and others: the unit on the y-axis is given as (m). But it says as text log10 of the radius. A log function does not have a unit so it should be log10 (radius / m).

Response: We changed the y-axis labels to "structure radius (m)."

The authors say that optimizing the optical properties is straightforward by adding various layers.

This stays nebulous until the experiment is described. In view of one specific interference layer given later, this also sounds a bit exaggerated.

Response: The optical layers referenced in the “device concepts” section are the lightest films that have been shown to have optimal solar-band absorbance and IR-emittance. References 35-39 describe the fabrication and characterization procedures of these films in detail. Incorporating the films into our devices and studying the resulting benefit to the lofting force is beyond the scope of this manuscript, but we aim to perform this work in subsequent experiments. However, we added a description of how such films could be integrated into the fabrication procedure of our existing devices in section 4 of the SI. Since the referenced films are near-uniform absorbers (across the visible spectrum) and emitters (across the mid-IR spectrum), we expect interference effects to be minimal over those spectrums, similar to what we observed with our current absorbing layer.

It is not clear why eps_IR are different for day and night and top and bottom. emissivity is usually just a physical property for a given wavelength (range). If it is already tied to Earth’s albedo or such, this would no longer be just an emissivity.

Response: We believe the reviewer is referencing lines 131-138 in the revised manuscript. Here, we discuss the optimal values of the visible absorptivity ϵ_{vis} and thermal emissivity ϵ_{IR} for both the top and bottom face sheets during the day and at night. In other words, these values maximize the lofting force at different times of day.

On a real device, the emissivities would, of course, not change depending on the time of day. Upon review, we realized that the absorptivity and emissivity variables were not clearly defined until the methods section. To avoid confusion, line 131 explains these quantities as soon as they are mentioned in the main text.

Line 176 mentions the dependence of the dimensionless force on the rarefaction factor. Again, it would be good to know if this is related to the pore radius or overall size.

Response: In the second paragraph of the main text, the rarefaction parameter δ is defined as the disk radius r divided by the mean free path λ of the surrounding gas. In lines 99-100, we state the hole radius is assumed to always be smaller than the mean free path. The definition of δ in Supplementary Information was updated for clarity.

The dimensionless force is known to people working on photophoresis but the term needs an immediate few word explanation when it first shows up for other readers.

Response: We added definitions of the dimensionless force and heat fluxes as soon as they are mentioned in the main text; see lines 111-114.

It is hard to see in Fig. 2 h,i what the important differences might be. On first sight, substructure might not be the game changer.

Response: To better show the difference between the plots in Fig. 2, we added plots showing the relative change in the area lofting force and payload capacity as a function of the structure porosity β. These plots better highlight the main effect of increasing β: that lofting is enhanced for larger structures at lower altitudes and is weakened for smaller structures at higher altitudes. We also added similar plots for the payload capacity, extending to very high β.

Line 265: The text says that the models agree well with the measurements below 1 Torr. Fig. 3f,g does not agree with this. They do only fit beyond 0.01 Torr. Somewhat below (line 295 ff) these data are again discussed, now agreeing with my observation that the fits are not so well. In fact, I only see the usual photophoretic peak at a rarefaction factor (or Knudsen number) of 1, which is not very model sensitive to my experience. If so, model and experiment do not really match or benefit from each other. The title says „model guided". What did the model guide to do?

Response: We have rewritten this section with the following changes.

- Similar paragraphs examining the agreement between the model and data were consolidated.
- The model is not statistically fit to the experimental data. Rather, we input the known parameters from the real samples into the model to observe how well the results predict our measurements. To prevent confusion, the plots in Fig. 3 now show only the experimental data, and comparison with the model is shown in Extended Data Fig. 4.
- The only structural parameters that are not known or measured are the accommodation coefficients α_n. While the models could hypothetically use the accommodation coefficients as a fit parameter for the experimental data, this would require collecting a large amount of numerical (DSMC) data for many discrete values of the NEAC and creating new interpolation functions among them. This was already done for varying δ and β, and such data collection for a third parameter would take many months and be computationally expensive. Instead, DSMC data for two new intermediate values of α_n (0.3 and 0.7) were collected and compared to the experimental data. These values provide better agreement with the experimental data in helium and air, respectively, than before.
- Further discrepancies between the model and experimental data, such as the effect of polyatomic gases, is expanded. Context for the choices of accommodation coefficients was added to the methods section.

In general, the model guided the choices of β, ϵ_{IR}, ϵ_{vis} and r for our fabricated samples. By using near-optimal values of these parameters as calculated by our models, we proposed a practical design in the Device Concepts section. As this may be unclear to the reader, the title of the manuscript has been revised to instead focus on the key goal of the work: to evaluate photophoretic flight of large structures in near-space conditions.

It is hard to connect fig. 1d and fig. 3h. Both are on experimental measured lifts. Fig 1. has more than 2 at maximum (black circles) with the given units (see above), I would estimate to be the lift over gravity ratio for 1 „sun". Fig. 3 h gives forces directly and gravity and at 1 „sun" has less than two.

Response: Fig. 1d only considers freestanding structures that have achieved flight, or rather, upward motion when illuminated. The black circles correspond to results from the flight demonstrations shown in Fig. 4 and the Supplementary Videos. Fig. 3h was not a flight test, but a measurement of the force as a function of illumination intensity. We clarified which structures are considered in Fig. 1d in the caption.

The lift of a structure at sub-solar (at 447nm) is mentioned in the abstract but not much of a focus in the main text. In fact Fig. 4b s hard to see. No note is given how this would translate to solar spectrum.

Response: We performed additional flight demonstrations using broadband light sources (a halogen arc lamp and white LED). We found that the broadband sources enabled flight at about 10 % less illumination than the 447 nm laser, indicating that wavelength-specific interference effects are small in the solar band. We added a sentence to the main text (lines 261-263) describing this difference and what it means for the solar spectrum. We replaced Fig. 4a with new images from a demonstration with a broadband source, which are much easier to see. Supplemental videos of four flight demonstrations are included with our revisions.

We also added spectroscopic and simulation results to show that the experimental structures exhibit strong and uniform absorption where the solar spectrum peaks, 400 – 700 nm. These results are shown in Extended Data Fig. 2 and are discussed in section 4 of the SI.

This work heavily seems to draw on references 5 and 9 from the same authors. It could be made clearer what the innovation in this work is.

Response: Kim *et al.* (Ref. 5) examines the effect of design parameters (e.g. the structure height H and face sheet thickness t_f) on the mechanical performance of structures that are entirely made of region I. Our current manuscript introduces region II as a way of increasing the photophoretic force. We added a sentence to the methods section explaining that our fabrication procedure is an adaptation of Kim *et al.* for photophoretically flying structures.

Sharipov and Schafer (Ref. 8) describes the numerical DSMC modeling of photophoretic forces on a thin perforated membrane. The methods and results of that paper are integrated into the larger model used to calculate dimensional photophoretic forces in our current manuscript. One may consider our current manuscript as an extension of the numerical model to real structures and flight environments. While the key elements of the numerical approach are described in the methods section, we added lines 399-401 to explicitly direct readers to this reference for full details.

Ref. 8 is a 20 year old, non-reviewed reference.

Response: This reference has been removed.

Referee #2 (Remarks to the Author):

The paper presents exciting theoretical and experimental findings related to photophoretic structures for near-space exploration. The novel contributions include:

1) Design of structures with minimal ligaments to maximize the temperature difference and, consequently, the lift force.
2) Development of a new experimental setup for measuring lift force.
3) Measurements of lift force on cm-scale structures in various gases.

Before recommending the paper for publication, I request that the authors address the following questions and concerns:

1) Line 222: The authors state, "to maximize payload capacity independent of altitude, a structure should have no perforations." This assertion appears to contradict Fig. 2e, where the lift force is maximized at a hole fraction of approximately 0.7 and a temperature difference of delta = 100. Additionally, the sentence could be rephrased to avoid a possible misinterpretation that a no-perforation structure maximizes force at *every* altitude.

Response: The reviewer raises an excellent point. We have rewritten our discussion of the payload capacities to explain the difference between Figs. 2f and 2k-n in our revision.

Fig. 2f assumes a constant temperature difference ΔT between the top and bottom of the structure, regardless of rarefaction parameter δ. In this scenario, adding perforations would maximize the attainable lofting force across all values of the rarefaction δ and structure porosity β, and this maximum occurs at large δ. However, in the atmospheric case, ΔT is not constant because the conduction between the face sheets depends on the atmospheric altitude. At low altitudes (i.e. large δ), conduction is stronger, so ΔT is smaller. This effect counteracts the benefit of adding perforations at large δ such that non-perforated structures attain the highest possible lofting forces across all altitudes and porosity β. Still, more porous (high β) structures can maximize the lofting force *at specific altitudes*, particularly at low altitudes. Lines 167-170 address these points.

2) Figure 3g: The experimental results for the photophoretic force in helium (He) and other gases are compared with theoretical fits, none of which match the data very well. Can the authors achieve a better fit by using alternative values for the NEAC and TMAC?

Response: We had previously only collected DSMC data for NEAC = 0.1 (a common value in Helium) and 1 (a common value in air and heavier gases). The reviewer correctly points out that these choices do not fit the data well.

We have since collected additional data for NEAC = 0.3 and 0.7, both of which have better agreement with the data collected in helium and air, respectively. They are also reasonable values given the somewhat large range of values reported in the literature. The data collected in SF_6 initially suggest that a low NEAC should be assumed, but previous studies have found NEAC very close to 1 for heavy gases. We believe this can be explained by our use of monatomic gas in our DSMC calculations. If polyatomic interactions were used, the model forces would be lower. We added an explanation of this to the main text.

To avoid cluttered plots and to let the experimental results stand on their own, we moved plots comparing the model and experimental results to Extended Data Fig. 4. In the caption for that figure, we stress that the choices of NEAC are not fits to the experimental data. While the models could hypothetically use the NEAC as a fit parameter, this would require collecting a large amount of numerical data for many discrete values of the NEAC and creating new interpolation functions among them. This was already done for varying δ and β, and such data collection for a third parameter would take many months and be computationally expensive.

Lastly, in the Methods section, we added a note that the models are insensitive to the TMAC.

3) Line 318: The authors write, "To our knowledge, this is the first reported photophoretic levitation of a macroscopic object under 1 sun of illumination and is a critical development in establishing the feasibility of sunlight-powered photophoretic aircraft." Although not yet published, a recent preprint (https://arxiv.org/abs/2312.15592) appears to report levitation at intensities below 1000 W/m².

Response: We have reviewed the above manuscript by Peng et al. and find it to be a novel use of graphene aerogel to create photophoretic structures capable of large photophoretic forces. However, we note a couple of issues with the manuscript's claim of levitation under 1 sun of illumination.

First, the manuscript's structures were levitated in a confined space, with the structures initially sitting on a transparent solid surface. Our Ref. 33 (Lu *et al.*) found that the ground effect on lofted photophoretic devices artificially increases the lofting forces when the chamber walls and base are within one structural width of the device. Due to this ground effect, we suspect that the lofting forces measured in the above manuscript may be larger than they would be if the structures were lofted in an open space.

Second, the manuscript's structures use an aluminum top surface and carbon aerogel bottom surface. The structure must be illuminated from below to reach the temperature needed to fly, whereas our structures can levitate when illuminated from above or below (see Supplementary Videos). Sunlight overhead would likely not levitate the structures of Peng et al., whereas our structures would levitate.

Lastly, Reviewer #1 rightly points out that referencing non-peer reviewed manuscripts should be avoided. Therefore, we will not reference this work in our manuscript.

4) Line 344: The authors propose using a "ground-based near-IR or microwave laser and small (3-10 cm) aperture telescope." How do the authors propose to track the position of the structures in the sky? What level of tracking accuracy is feasible, and what laser power would be required given the tracking accuracy? Additionally, the authors should address the challenges posed by the diffraction-limited angular divergence of microwave lasers/masers for such small apertures, which would require very high power. Finally, do the authors anticipate their structures will absorb microwave radiation effectively?

Response: In the SI, we expanded our analysis of communications to include remote tracking and power beaming. To answer the reviewer's questions:

- For communications only, we propose tracking the structures using a ground-based phased array of four or more 1 m-radius apertures spaced 100 m apart, which can track a 3 cm-radius device to within 1 km^2 in the mesosphere.
- The baseline of this array can be extended to ~ 1 km to track lofted devices with the accuracy needed for power beaming. However, it would be easier to use a laser to scan across solid angles and have the device communicate when hit by the laser. The most feasible power source for both tracking and power beaming is a kW-scale optical laser, though the wavelength should be chosen carefully to prevent atmospheric attenuation and eye safety concerns. We give a brief comparison to an RF array-based approach that requires more infrastructure but does not face could be scaled down with increased device size. Our calculations consider the diffraction-limited resolution of the optical laser and RF array.
- We reference Benford and Benford (Ref. 7), who proposed a framework for absorbing microwave radiation onboard photophoretically lofted devices.

Due to the word limit, we added the key results of this analysis to the main text.

5) Line 349: The authors calculate that "a device with r = 1 cm, β = 0.5, and H = 125 µm would generate 0.5 g/m² at 75 km at night," suggesting that sustained lofting is possible for lightweight devices. Could the authors propose specific dimensions for a structure capable of remaining aloft both day and night? Additionally, in Fig. 4e, they model nighttime descent. Can they design a structure that achieves continuous levitation, perhaps by ascending during the day and descending at night?

Response: We added a paragraph to the main text that examines the effect of overnight settling at two locations: the equator during the equinox and the poles during summer. Our expanded analysis shows a large difference in the device size and payloads that can sustainably fly in these two regions, highlighting the importance of continuous illumination. We added several subfigures in Fig. 4 to highlight this difference, moving those that were displaced to Extended Data Fig. 5. From these new subfigures, one can identify the specific dimensions for structures that can remain aloft overnight while staying upright. Lastly, we expanded our statements on nighttime-optimized structures and compared them to those that settle overnight.

6) Line 360: The authors suggest that "deployment might be achieved using high-altitude balloon technology that can lift kg-scale payloads to the 50 km stratopause." Can the authors be specific about the dimensions and payload capacities of a structure that is capable of generating sufficient lift at altitudes of 50 km (which is necessary for deployment from a balloon)?

Response: We added a few sentences that answer this question in lines 344-347. We also mention how the deployable size (and payload capacity) of a device can be increased by increasing the porosity β.

7) Figure 4g: The predicted horizontal airspeeds for tilted structures reach 350 m/s, which is likely supersonic at 75–80 km altitudes and therefore implausibly high. The authors may want to

revisit their drag assumptions in deriving these speeds. For instance, their use of subsonic, low-Reynolds-number drag in Eq. S25 may need reconsideration.

Response: We reviewed our approach to calculating horizontal airspeed and found that, indeed, the Reynolds numbers for cm to m-scale structures in the mesosphere were too high to use our prior Eq. S25. To remedy this, we adapted the current Eq. S12 to horizontal motion by incorporating the angle of attack into the spatial variables where appropriate. This is a crude approach since the drag coefficients for both low and high-Reynolds flows depend on the geometry of the object, though one study found that the angle of attack of a thin plate had a relatively small (~ 25 %) impact on the drag coefficient at Reynolds numbers as low as 1. Computational and experimental studies of thin, tilted structures at low pressures is certainly an area for further study, but such work is a large undertaking and beyond the scope of this manuscript.

Assuming some uncertainty surrounding the drag coefficients, we found tilted structures could attain horizontal speeds up to around 10 m/s in the mesosphere during the day. We updated our discussion and figure (now Extended Data Fig. 5b) in the main text with this corrected information.

8) The proposed payload capacities are relatively low, limiting practical utility. Perhaps the authors could provide more quantitative insights into how this technology could outperform existing mesospheric tools, such as remote satellite observation or ground-based radar and lidar measurements. Including a comparison with existing technologies for atmospheric sensing and Martian exploration would contextualize the benefits of this approach.

Response: Especially for a public-facing journal like Nature, we understand the importance of communicating the usefulness of this research. To better highlight the benefits that photophoretic fliers could bring to atmospheric sensing and telecommunications, we added two sentences to the introduction that compare it other high altitude platform systems (HAPS) and satellites. We also added a few more quantitative details and comparisons to the state-of-the-art in the practical device section. Our updated analysis of deployment, lofting during polar summer, and communications also provide quantitative context about the use cases of the fliers.

Further discussion of the context of this technology, such as a technoeconomic analysis of photophoretic flight or a cost comparison to current sensing tools, could themselves be additional journal articles. As research in this field progresses, we hope to contribute to such work.

9) Supplementary Material, Page 21: The authors write, "The antenna can be etched directly into the SS." It appears that "SS" refers to "silicon substrate," but this abbreviation is not defined in the text.

Response: The abbreviation "SS" was shorthand for the supporting structure from an earlier version of the manuscript. We have removed this notation.

Referee #3 (Remarks to the Author):

The authors present a feasibility study for a photophoretic aircraft operating in the mesosphere. They introduce a microstructured (perforated) photophoretic surface and show that it outperforms previous designs by a factor of two in terms of areal force. The authors performed a ground-based levitation experiment and demonstrated that with a 300mW blue laser they are able to lift a 1cm2 sample with a weight of about 1mg. Based on this demonstration they discuss the requirements and challenges for the realization of a photophoretic aircraft operating in the mesosphere.

This work presents a technological advance, that is, the enhancement of the photophoretic force by means of an engineered surface. To me, the manuscript reads like a proposal, i.e. based on a preliminary study it presents ideas that have yet to be realized. Based on simulations the authors provide estimates, but in some places these are quite speculative. I struggled with the general presentation of this work, i.e. many terms are undefined in the introduction and require to jump back and forth in the manuscript. All in all, I think this manuscript presents a noteworthy technological advance and a bunch of clever ideas. In the following I list some of my concerns and questions.

1) The meaning of the ``normal energy accommodation coefficient" should be explained in the introduction. What are the units? How is it measured?

Response: We added definitions of the accommodation coefficients as soon as they are mentioned. Because they are probabilities, they are unitless. In the experimental section, we mention that the coefficients are experimentally measured and added a reference to appropriate experiments.

2) Key parameters of the mesosphere should be mentioned (temperature, pressure, distance to earth).

Response: We added Fig. 1e to show these parameters, and the altitudes are now mentioned in the introduction.

3) A reader has to be familiar with Shapirov (2024) in order to understand the terms. What is a dimensionless heat flux, what is a dimensionless force?

Response: The dimensionless heat fluxes are measures of the average conductive heat transfer that incident gas particles add to (or remove from) the structure upon collision. The dimensionless force is a measure of the average force that incident gas particles impart to the structure. We added these definitions when the dimensionless quantities are first mentioned.

(34) 4) Many readers won't understand light intensities in units of ``suns". Also, this unit might be misleading, as it does not specify the wavelength. The authors used a blue laser for their experiment, which covers only a small fraction of the sun's spectrum. But photophoretic propulsion relies on absorption, which depends on wavelength, and might be strongest in the blue spectral range. The use of a blue laser might therefore not reflect the performance under solar radiation.

Response: These comments are similar to those of Reviewer #1. Our responses are copied here:

We agree that the use of "sun" as a unit is inappropriate within this paper. Indeed, it was meant to represent the solar constant (1366 W/m^2), but this was confusing in the context of monochromatic laser-based experiments. We replaced all uses of the quantity "sun" with the appropriate units of W/m^2, making sure to clarify the relevant light source where appropriate.

The absorptive bottom layer of the structure was not designed to optimize absorbance specifically at 447 nm, but rather the most intense part of the solar spectrum, 400 – 700 nm. To show this, we added analysis of the spectral sensitivity of the structural layers.

First, we added UV-visible-near-IR and Fourier transform IR spectroscopic data of the bottom layer to Extended Data Figure 1. The methods for analyzing this data are now described in section 6 of the SI. The spectroscopic data show that the structure is highly absorptive where the solar spectrum peaks, with only about 10 % within the range 400 - 700 nm, indicating a monochromatic source within this range is absorbed similarly to broadband sunlight.

Second, we added rigorous coupled-wave analysis to analyze any interference patterns within and between the films. These simulations are also discussed in section 4 of the SI. The results agree with the spectroscopic measurements and show no unexpected interference effects. The top alumina layer has a negligible effect on the structure's absorbance. Furthermore, analysis of the full structure shows no evidence of interference due to the spacing between the membranes.

Lastly, we performed additional flight demonstrations of freestanding structures using two broadband light sources: a halogen arc lamp and a white LED. Five new Supplementary Videos highlight the results of these demonstrations. Notably, broadband light produced flight at slightly lower light intensities than what was observed with a laser. Using just a white LED, we observed a structure fly at about 750 W/m^2 of illumination, roughly 55 % the equivalent intensity of sunlight.

5) It would help to compare the photophoretic force with the radiation pressure force. I estimate that it is 6 orders of magnitude stronger.

Response: We added a comparison to radiation pressure to the end of the first paragraph of the main text.

6) In Fig.3. the weight of the lofting surface has not been specified.

Response: We added the weight of the structure, 130 µg, to the caption of Fig. 3. It is also shown as a horizontal dashed line in Fig. 3h.

7) Propulsion with an earth-based laser during night times needs further discussion. I estimate

you require a one Watt laser expanded to over one meter and then focused on the spacecraft. How will you track the spacecraft so it remains in the focus? What about clouds?

Response: In the SI, we expanded our analysis of communications to include remote tracking and power beaming. The key results are:

- For communications only, we propose tracking the structures using a ground-based phased array of four or more 1 m-radius apertures spaced 100 m apart, which can track a 3 cm-radius device to within 1 km^2 in the mesosphere.
- The baseline of this array can be extended to ~ 1 km to track lofted devices with the accuracy needed for power beaming. However, it would be easier to use a laser to scan across solid angles and have the device communicate when hit by the laser. The most feasible power source for both tracking and power beaming is a kW-scale optical laser, though the wavelength should be chosen carefully to prevent atmospheric attenuation and eye safety concerns. We give a brief comparison to an RF array-based approach that requires more infrastructure but does not face could be scaled down with increased device size. Our calculations consider the diffraction-limited resolution of the optical laser and RF array.
- We include sentences on the atmospheric attenuation of both visible and RF signals.

Due to the word limit, we added the key results of this analysis to the main text.

Responses to referees (for the June 2025 revision)

We thank all reviewers for their additional feedback. Below, please find our responses in blue.

Referee #1 (Remarks to the Author):

All my concerns were answered appropriately, which is much appreciated.
I have no significant further comment and would recommend publication.

Gerhard Wurm

Referee #2 (Remarks to the Author):

The authors have adequately addressed my prior concerns and I am happy to recommend the paper for publication, assuming the can address the minor comments below. I particularly appreciate the the description of the conditions under which the proposed structures can levitate even at night using IR emission from the earth. This is a conceptually new idea that would be interest to a broader audience.

on line 83, the authors write "characteristic size is typically its smallest 83 dimension perpendicular to ΔT or $\Delta\alpha n$,". While I understand what this sentence means, I think many readers may find it confusing and I recommend illustrating with in a figure.

Response: We added Extended Data Fig. 4a, which shows the characteristic size of a few common structure geometries.

on line 110, the authors need to explain the physical meaning of the dimensionless force and heat fluxes in a way that could eb understood by a non-specialist.

Response: The sentence following the referenced line was previously introduced to address this point. However, we realize that the description of the dimensionless quantities may still be opaque to non-specialists. To address this comment and comment 3 from reviewer 3, we revised the entire paragraph. As recommended by reviewer 3, we now describe the normalization factors that convert the dimensionless quantities to the dimensional ones, and direct readers to their symbolic forms in the methods. We tried to keep this paragraph as clear and accessible as possible.

on line 138, I do not know what the sentence "Averaging over the year reduces the time-averaged lofting force by 3 %" means. 3% compared to what?

Response: We specified that the reduction of 3 % is relative to the lofting force during the equatorial equinox.

Referee #3 (Remarks to the Author):

I have read the authors' revised manuscript and their reply. The quality of the presentation increased and my questions/answers were answered satisfactorily. This work is a proof-of-concept study and definitely contains innovative elements and ideas. Although I'm not an expert in this field and can't judge the significance and the impact, I was very impressed by the results and the potential applications to space science. There are some issues that I believe should still be fixed or addressed:

(1) Line 32: The sentence ``We observed photophoretic levitation of a 1 cm-wide structure when illuminated by .." requires specification of the air pressure.

Response: We added the air pressure to this sentence.

(2) Line 105: It is not clear at what distance from the membranes Tb and Tf are evaluated. According to Fig 2c it is ``far", but what does that mean? At infinite distance the two temperatures should be equal.

Response: We are unsure about the "distance" to which the reviewer refers, since T_t and T_b are the membrane temperatures. If the reviewer refers to the domain size of the numerical model shown in Fig. 2c, the temperature of the domain's edge is fixed at the ambient temperature, and the membrane itself is infinitely thin with top and bottom surfaces fixed at T_t and T_b, respectively. The domain is several times larger than the width of the structure, which was shown to adequately model the temperature condition of a freestanding device surrounded solely by a gas in Ref. 8. If the reviewer refers to the distance between the membranes H, this quantity is introduced in the analytical model, but it is fixed at 125 μm in all results throughout the paper, since this value is the thickness of the experimental structures in Fig. 3. A discussion on H is given in lines 120-126. Without being sure about which distance the reviewer refers to, it is difficult to evaluate the reviewer's claim of T_t and T_b being equal.

(3) Line 110: If something is dimensionless then it has been normalized with something. This something has to be defined in the main text. This normalization is defined by Eq.(7) in Methods, but needs to be mentioned in main text for sake of readability.

Response: We rewrote this paragraph for clarity (see our response to the second comment from reviewer 2) . We included a qualitative description of the normalization factors and direct readers to their symbolic definitions in the Methods.

(4) Line 161: g/m2 is not a unit of force. Force has units of Newtons.

Response: We refer the reviewer to lines 96-98, where we describe why area forces are reported with units g/m^2 throughout the paper, and how these units are changed from Newtons. We added the word "area" to line 142 for clarity.