nature portfolio

# Peer Review File

# Detection of an atmosphere on a trans-Neptunian object beyond Pluto

Corresponding Author: Dr Ko Arimatsu

This manuscript has been previously reviewed at another journal. This document only contains information relating to versions considered at Nature Astronomy.

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

Version 0:

Reviewer comments:

Reviewer #1

(Comments for the Author)
This manuscript presents the surprising discovery of a possible atmosphere around the TNO 2002 XV93 from occultation light curves. The paper is generally very well written and presents a set of observations that are extremely compelling to publish. This work is certainly suitable for publication in Nature Astronomy — both in terms of quality and impact — and I recommend its publication after a few minor revisions responding to a few concerns I have.

My first concern is with the probability of observing such an atmosphere. Collisions in the TNO population are relatively rare, and with atmospheres lasting on the order of 10^2 years, they should have relatively low detectability. Although many TNO occultations are observed every year, is there reasonable chance that an impact-generated atmosphere could be detected? Said a different way, assuming this is an impact-generated atmosphere, does the required impactor size-distribution across all TNOs predicted a reasonable probability that a current impact-generated atmosphere is detectable?

Another reason that occultation light curves partially drop outside of the main body occultation is due to rings/disk structures. Is there any possibility that the detected dimming is instead due to a disk oriented such that it mimics an atmosphere? Alternatively a shell of material like that possibly around Chiron (Pereira et al. 2025) could imitate an atmosphere.

Throughout the manuscript, the authors assume a density of 1500 kg/m^3 for XV93. Although no measurements have been made of its true density, it is likely that the density is much lower, likely around 1000 kg/m^3 (Rommel et al. 2025). Do significant changes in density fundamentally alter the derived surface pressures, lifetimes, etc?

Although in a different regime than the atmosphere this manuscript focuses on, Makemake has recently had surrounding gas — possibly in a gravitationally bound atmosphere — detected via fluorescence of CH4 (Protopapa et al. 2025). It may be worth mentioning these results to give the reader greater context.

Why only fit CH4 and N2, but not CO? I understand the results would generally be similar, but is there any particular reason?

Overall, I applaud the authors for their fantastic discovery and manuscript.

Reviewer #2

(Comments for the Author)
This paper reports the detection of a tenuous atmosphere around the small TNO 2002 XV93. This discovery is novel, as previous searches for atmospheres around TNOs have suggested that only the largest ones are capable of sustaining them. The observational methodology and analysis are well-suited to the work at hand, and the results are robust and presented clearly and succintly. I recommend that this article be accepted for publication once a number of mostly minor points are addressed. These are detailed below.

General note: A recent paper (Protopapa et al. 2025) reported the detection of CH4 gas emission on Makemake. While it is not known whether the signature is indicative of a bound atmosphere or comet-like outgassing, it is worth mentioning this work in the main text (paragraph 5) and perhaps the abstract as well.

Paragraph 6: I recommend specifying which ices you consider “volatile”, as that definition depends on the temperature range being considered. For TNOs, the term “hypervolatile” is often used to refer to $CH_4/N_2/CO$, which have very low sublimation temperatures. Meanwhile, ice species such as $CO_2$ and $H_2O$, which were detected on 2002 XV93 with JWST are not expected to sublimate at TNO temperatures.

Paragraph 6: Although not a TNO per se, Triton should also be mentioned when discussing icy distant bodies with atmospheres. It is very analogous to Pluto.

Paragraph 7: I suggest also referring to recent geochemical modeling work on Eris (Glein et al. 2024), based on JWST observations (Grundy et al. 2024). That paper provides a good overview of the different internal processes that can drive surface-interior volatile exchange on mid-sized TNOs.

Paragraph 8: If possible, please provide an estimate of the lifetime of an atmosphere with the measured surface pressure level around 2002 XV93, assuming only Jeans escape and no replenishment. This is important for comparing with necessary timescales for sustaining such an atmosphere through cryovolcanic replenishment and the decay timescale for an impact-generated atmosphere.

Paragraph 8: Explicitly define what you consider to be “large” in the context of KBOs.

Paragraph 8: I suggest adding in discussion about the possibility of direct spectroscopic detection of volatile gases around TNOs, particularly with JWST. That is the most reliable way of determining the specific molecules that are present.

Table 1: Should the status for Fukushima be “positive?”, given that it observed a light curve signal? Also, I recommend adding in the exposure cadence, exposure time, and seeing (if measured) to the table for ease of viewing.

General figure comment: I strongly recommend increasing the axis label and panel label font sizes, if possible.

Data reduction section: “spectral energy distribution” -> do you mean “point spread function”?

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Data reduction section: How does the ephemeris offset compare to the JPL position uncertainties?

Version 1:

Reviewer comments:

Reviewer #1

(Comments for the Author)
I thank the authors for responding to my comments. I believe the manuscript is much improved and have no further comments. I recommend that the article be accepted for publication.

Reviewer #2

(Comments for the Author)
I appreciate the care with which the authors have responded to the comments and suggestions raised in my review. The changes have adequately addressed all of the points, and I therefore recommend the paper for publication.

Dear referees #1 and #2,

We are grateful to both referees for their thorough and helpful reviews. We have revised the manuscript in response to all comments and believe that the changes have improved clarity and presentation.

In addition to addressing the specific points raised, we standardized terminology and usage across the manuscript (for example, by unifying size notation to radius) while leaving the scientific content unchanged, and we made minor edits to the Abstract to comply with the journal's guidance without altering its meaning. We also added the "Correspondence and requests for materials" and "Competing interests" statements as requested by the journal, and corrected minor typos. The revised manuscript does not alter the results or discussion.

Below we provide a point-by-point response to each referee comment. Changes in the revised manuscript are marked in red.

Reviewer #1 (Comments for the Author):

This manuscript presents the surprising discovery of a possible atmosphere around the TNO 2002 XV93 from occultation light curves. The paper is generally very well written and presents a set of observations that are extremely compelling to publish. This work is certainly suitable for publication in Nature Astronomy — both in terms of quality and impact — and I recommend its publication after a few minor revisions responding to a few concerns I have.

My first concern is with the probability of observing such an atmosphere. Collisions in the TNO population are relatively rare, and with atmospheres lasting on the order of 10^2 years, they should have relatively low detectability. Although many TNO occultations are observed every year, is there reasonable chance that an impact-generated atmosphere could be detected? Said a different way, assuming this is an impact-generated atmosphere, does the required impactor size-distribution across all TNOs predicted a reasonable probability that a current impact-generated atmosphere is detectable?

Thank you for raising this point. We agree that, if the inferred gaseous envelope is impact-generated and short-lived, its present-day detectability depends critically on the product of the impact rate of small projectiles and the atmospheric survival time, and therefore on the faint-end Kuiper-belt size-frequency distribution (SFD), which remains uncertain.
To address this explicitly, we have added a discussion in the revised manuscript providing an order-of-magnitude occurrence estimate anchored to New Horizons crater-count constraints on Pluto, Charon, and Arrokoth (Morbidelli et al. 2021). Under the simplifying assumption that present-day impact rates scale from Pluto purely with geometric cross-section, we note that the cumulative probability for a TNO to be struck by a ~$10^2$ m–scale (or larger) projectile over ~$10^2$ yr is of order p ~$10^{-5}$, i.e., intrinsically low. We note that, however, this estimate remains uncertain by orders of magnitude, and that published occultation-monitoring results for sub-km to km scales allow the possibility of a higher small-body abundance than implied by the shallow crater-inferred branch, such that the true occurrence rate could plausibly be higher. Although the nominal expectation is low,

the rate is uncertain by several orders of magnitude. Therefore, given that the current sample includes nearly ~$10^2$ successful occultations of large TNO targets, we think that an impact-generated transient atmosphere cannot be ruled out.
We have added these discussions in the main text (revised Paragraph 9).

Another reason that occultation light curves partially drop outside of the main body occultation is due to rings/disk structures. Is there any possibility that the detected dimming is instead due to a disk oriented such that it mimics an atmosphere? Alternatively a shell of material like that possibly around Chiron (Pereira et al. 2025) could imitate an atmosphere.

We agree that rings or circum-object material can in principle generate flux drops outside the solid-body occultation. We have therefore added a short paragraph in the main text (revised Paragraph 4) to discuss these alternatives and to place them in the context of the rings/disk material reported around Centaurs and TNOs.

A ring/disk interpretation is not formally excluded by a single occultation event; however, reproducing the observed behaviour with extinction would require a rather extreme configuration. In our data, the dimming occurs immediately adjacent to the solid-body occultation at Kiso and is consistent with a shallower drop centred near the closest-approach time at Fukushima. Interpreting both chords with a ring/disk requires material confined within only a few tens of kilometres above the surface ($r \lesssim 1.1–1.2R$) and with an integrated opacity corresponding to an apparent equivalent width of ~ 11 km. This near-surface requirement is unusual compared with all ring systems currently known around Centaurs and TNOs. Confirmed (or strongly supported) ring systems orbit at several body radii. In addition, dynamical studies show that a non-axisymmetric gravity field can rapidly clear the innermost region around an aspherical small body (Giuliatti Winter et al. 2023), suggesting that a persistent ring extremely close to the surface would require exceptional conditions.

Regarding a Chiron-like “shell” of material, we note that activity-driven dust retained in bound orbits is expected to be favoured only when typical ejection speeds are comparable to the escape speed. The criterion discussed by Sicardy et al. (2025) gives a critical size of order R_crit ~ 130 km for representative jet speeds v_jet ~ 100 m $s^{-1}$, which is substantially smaller than 2002 XV93; in this regime, ejecta is expected to re-impact rather than remain in a shell/disk.

For these reasons, while we cannot completely rule out circum-object material from a single event, we consider an atmosphere to be the most self-consistent explanation for the similar ingress/egress behaviour at Kiso and the independent constraint from the Fukushima chord.

Throughout the manuscript, the authors assume a density of 1500 kg/m^3 for XV93. Although no measurements have been made of its true density, it is likely that the density is much lower, likely around 1000 kg/m^3 (Rommel et al. 2025). Do significant changes in density fundamentally alter the derived surface pressures, lifetimes, etc?

We agree that the bulk density of 2002 XV93 is currently unconstrained and that a lower value (e.g., ρ ≈ 1000 kg m^-3) may be possible. We have now quantified the sensitivity of

our results to this assumption by repeating the atmosphere refraction modeling with $\rho$ = 1000 kg $m^{-3}$.
For the refraction light-curve fitting, changing the assumed bulk density modifies the gravitational acceleration and thus rescales the mapping between pressure and altitude. However, for a given (p, T) structure, the refractive signature is primarily controlled by the vertical gradient of refractivity, which in our parameterization is dominated by the assumed temperature structure (including the inversion layer), rather than by moderate changes in g. Consistent with this expectation, the model light-curve profile at a fixed surface pressure changes only modestly when $\rho$ is reduced from 1500 to 1000 kg $m^{-3}$. We illustrate this comparison in a new Extended Data Fig. 3.
When the model is refit to the Kiso light curve with $\rho$ = 1000 kg $m^{-3}$, the best-fit surface pressures are slightly higher than, but statistically consistent with, our fiducial results for $\rho$ = 1500 kg $m^{-3}$ (see the revised Methods). Therefore, the inferred surface pressures and our primary conclusion of a ~$10^2$ nbar atmosphere are robust to plausible density variations.
In contrast, the atmospheric retention/escape timescale is more sensitive to $\rho$ because the Jeans parameter $\lambda$ scales linearly with bulk density (eq. 1), and (even if we only consider Jeans escape) the Jeans escape flux depends approximately exponentially on $\lambda$. Thus, adopting a lower density would further shorten the lifetime of any sublimation-supported atmosphere.
We have added a brief sensitivity discussion in the atmosphere refraction model fit section in the revised Methods and in the main-text discussion of escape/retention (revised Paragraph 7), and we include a new Extended Data Fig. 3 comparing model refraction profiles for $\rho$ = 1500 and 1000 kg $m^{-3}$.

Although in a different regime than the atmosphere this manuscript focuses on, Makemake has recently had surrounding gas — possibly in a gravitationally bound atmosphere — detected via fluorescence of $CH_4$ (Protopapa et al. 2025). It may be worth mentioning these results to give the reader greater context.

Thank you for highlighting the recent JWST study by Protopapa et al. (2025). We have now cited this work in the revised Paragraph 6 and 10, and briefly in the abstract.

Why only fit $CH_4$ and $N_2$, but not CO? I understand the results would generally be similar, but is there any particular reason?

In the original manuscript, we limited the refraction modeling to two end-member composition cases (pure $CH_4$ and a Pluto-like $N_2$-dominated atmosphere with trace $CH_4$ for upper-atmospheric heating) to keep the parameter exploration compact and directly comparable with previous occultation-based investigations.
Following this comment, we have now added an additional CO-dominant atmosphere case, recomputed the refractivity profiles and synthetic light curves, and repeated the $\chi^2$ minimization fits. You are correct that the inferred constraints are quite similar to those obtained for the other two molecular species considered here. We have revised the manuscript text and the relevant figures accordingly to present the CO-dominant fit results and to clarify this point.

Reviewer #2 (Comments for the Author):

This paper reports the detection of a tenuous atmosphere around the small TNO 2002 XV93. This discovery is novel, as previous searches for atmospheres around TNOs have suggested that only the largest ones are capable of sustaining them. The observational methodology and analysis are well-suited to the work at hand, and the results are robust and presented clearly and succintly. I recommend that this article be accepted for publication once a number of mostly minor points are addressed. These are detailed below.

・General note: A recent paper (Protopapa et al. 2025) reported the detection of $CH_4$ gas emission on Makemake. While it is not known whether the signature is indicative of a bound atmosphere or comet-like outgassing, it is worth mentioning this work in the main text (paragraph 5) and perhaps the abstract as well.

Thank you for highlighting the recent JWST study by Protopapa et al. (2025). We have now cited this work in the revised Paragraph 6 and briefly in the abstract.

Paragraph 6: I recommend specifying which ices you consider “volatile”, as that definition depends on the temperature range being considered. For TNOs, the term “hypervolatile” is often used to refer to $CH_4$/$N_2$/CO, which have very low sublimation temperatures. Meanwhile, ice species such as $CO_2$ and $H_2O$, which were detected on 2002 XV93 with JWST are not expected to sublimate at TNO temperatures.

Thank you for this suggestion. We agree that the term “volatile” is temperature-dependent and can be ambiguous without specifying the relevant thermal regime. We have therefore revised Paragraph 6 to clarify that, at typical TNO surface temperatures (T ~ 40–50 K), only the “hypervolatile” ices $CH_4$, $N_2$, and CO are expected to undergo active sublimation and thus plausibly supply an atmosphere. We also adjusted the wording in the manuscript accordingly.

Paragraph 6: Although not a TNO per se, Triton should also be mentioned when discussing icy distant bodies with atmospheres. It is very analogous to Pluto.

We have added sentences about Triton in the revised Paragraph 6 and appropriate references.

Paragraph 7: I suggest also referring to recent geochemical modeling work on Eris (Glein et al. 2024), based on JWST observations (Grundy et al. 2024). That paper provides a good overview of the different internal processes that can drive surface-interior volatile exchange on mid-sized TNOs.

We have now added the recommended context in the revised Paragraph 8 (originally Paragraph 7) and cited the recent studies by Grundy et al. (2024) and Glein et al. (2024).

・Paragraph 8: If possible, please provide an estimate of the lifetime of an atmosphere with the measured surface pressure level around 2002 XV93, assuming only Jeans escape and no replenishment. This is important for comparing with necessary timescales for sustaining such an atmosphere through cryovolcanic replenishment and the decay timescale for an impact-generated atmosphere.

Thank you for this request. Our current refraction-fit parameterization was designed to reproduce the observed light curve and adopts a hydrostatic temperature-inversion layer up to z~20 km, while the higher-altitude region is connected to a prescribed free-molecular density profile (Methods) as a toy model, because the inferred refractivity profile is largely insensitive to the specific choice of the high-altitude profile. In this framework, a physically motivated exobase is not explicitly determined, so a self-consistent Jeans-escape calculation is not possible.
To nevertheless provide the requested order-of-magnitude lifetime under "Jeans escape only" and "no replenishment," we assume an isothermal atmosphere with the measured surface pressure level *p* ~ 100–200 nbar and T ~ 40–50 K around a ~250-km-radius TNO (consistent with the assumptions already stated in the paragraph introducing the Jeans parameter). Using the classical Jeans escape flux formalism (Volkov et al. 2011) and neglecting any replenishment, we obtain an atmospheric decay timescale of order $10^2$–$10^3$ years for CH4, N2, and CO. This implies that sustaining the atmosphere by cryovolcanic replenishment would require activity on short timescales, whereas an impact-generated atmosphere would need to be very recent. We have added a brief order-of-magnitude statement to the manuscript at the end of the paragraph.

Paragraph 8: Explicitly define what you consider to be "large" in the context of KBOs.

We agree that the term 'large' can be ambiguous without an explicit size scale. We have revised the main text to define 'large' TNOs in this context as dwarf-planet-sized bodies ($R \gtrsim 500$ km), and adjusted the wording accordingly to avoid any ambiguity.

Paragraph 8: I suggest adding in discussion about the possibility of direct spectroscopic detection of volatile gases around TNOs, particularly with JWST. That is the most reliable way of determining the specific molecules that are present.

We agree that direct spectroscopy is the most reliable way to identify the specific volatile molecules. We have added a brief discussion in the revised Paragraph 10 noting that JWST has already demonstrated such capabilities, including the detection of multiple hydrocarbon gases in Pluto's atmosphere with MIRI-MRS and the detection of CH4 fluorescence from Makemake with NIRSpec. We further note that analogous high-resolution spectroscopic observations of 2002 XV93 would directly constrain the molecular species in its newly detected atmosphere. We also made minor revisions to the existing text to ensure consistency with the discussion above.

Table 1: Should the status for Fukushima be "positive?", given that it observed a light curve signal? Also, I recommend adding in the exposure cadence, exposure time, and seeing (if measured) to the table for ease of viewing.

We have revised Table 1 and the text accordingly: the Fukushima station is now listed as "Positive (gradual drop)" to reflect the detected light-curve signal. In addition, for ease of viewing we have added columns for the exposure cadence (frame rate), exposure time, and seeing (FWHM, where measured).

General figure comment: I strongly recommend increasing the axis label and panel label font sizes, if possible.

We have revised all figures by increasing the font sizes of the axis labels and panel labels throughout the manuscript to enhance clarity and readability.

Data reduction section: “spectral energy distribution” -> do you mean “point spread function”?

Here “spectral energy distribution” refers to the spectrum of the occulted star and is used to compute the wavelength-integrated Fresnel-diffraction light-curve model, not the point-spread function. In our fitting, the monochromatic knife-edge diffraction pattern is integrated continuously over wavelength with a weight given by the product of the stellar spectral energy distribution and the total system spectral response (throughput), which defines the effective bandpass of our unfiltered observations and is required to reproduce the diffraction-smoothed occultation profile. We have revised the Methods text accordingly to make this wavelength-weighting explicit and to avoid confusion with the PSF.

Data reduction section: It seems that the occultation chord lengths were determined when assuming a no-atmosphere model. This would yield large chi-squared values in the fit and likely bias the true chord length. If possible, the size of the solid body of 2002 XV93 should be determined through a simultaneous fit to the atmospheric refraction signal. Alternatively, you could consider fitting the light curve when excluding the points that contain the atmospheric signal near ingress and egress

Thank you for pointing out that deriving chord endpoints with a knife-edge (no-atmosphere) model could, in principle, bias the inferred chord length when a refractive dimming signature is present near ingress/egress.
In our analysis, the solid-body radius is primarily constrained by a self-consistent fit of an atmospheric refraction model to the high-S/N Kiso light curve, in which the chord length and surface pressure are optimized simultaneously (Methods; Extended Data Fig. 4). For the Kyoto data, however, the signal-to-noise ratio and 1-s integrations make a fully self-consistent simultaneous fit highly degenerate and therefore not robust.
To address this, we performed an additional “hybrid” fit for the Kyoto light curve by fixing p to the best-fit value obtained from the Kiso refraction fit (for each composition case) and allowing only the chord geometry (equivalently, the chord length/ingress–egress timing) to vary. The best-fit results remain consistent within uncertainties with the original no-atmosphere chord solution, and thus do not affect the manuscript’s conclusions based on the occultation geometry.
We have added a clarifying paragraph in the revised Data Reduction section of the Method describing this additional test and its outcome.
We also tested the alternative approach as suggested (excluding points near ingress/egress that may contain the refractive signal), but found that the inferred parameters and their uncertainties vary sensitively with the exact choice of excluded points. For this reason, we adopt the physically motivated refraction-model approach above.

Data reduction section: How does the ephemeris offset compare to the JPL position uncertainties?

We compared the shadow-center offset derived from the two-chord fit with the JPL24 ephemeris prediction. The best-fit offset is smaller than (and therefore consistent with) the 1σ JPL positional uncertainty at the epoch of the occultation (see also the revised Fig. 1).

We have added a sentence in the Data reduction section to explicitly state this comparison.

Sincerely,
Ko Arimatsu

Dear referees #1 and #2,

We thank the reviewers for their careful evaluation of our manuscript.

Reviewer #1:
Comments for the Author:
I thank the authors for responding to my comments. I believe the manuscript is much improved and have no further comments. I recommend that the article be accepted for publication.

We appreciate the reviewer's positive assessment and kind recommendation for publication. The reviewer raised no further comments.

Reviewer #2:
Comments for the Author:
I appreciate the care with which the authors have responded to the comments and suggestions raised in my review. The changes have adequately addressed all of the points, and I therefore recommend the paper for publication.

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Sincerely,
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