nature portfolio

Peer Review File

Isotopic Evidence for a Cold and Distant Origin of 3I/ATLAS

Corresponding Author: Dr Martin Cordiner

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

Version 1:

Reviewer comments:

Referee #1

(Remarks to the Author)
In the paper «Isotopic Evidence for a Cold and Distant Origin of the Interstellar Object 3I/ATLAS» by Cordiner et al. measurements of isotopic ratios for D/H in water and 12C/13C in CO and CO2 for comet 3I/Atlas are reported. These data are very precious as this is the first time that such ratios are presented for an interstellar object. The paper is very well written, the data are convincing and well presented, uncertainties are thoroughly discussed.
I have very little comments:
The discussion contains a lot of different options about the origin of 3I without a very clear conclusion, which is to be expected and which is fully ok. The two isotopic ratios are very important jigsaw pieces, but a final picture on when and where 3I was formed, if at all, can only be drawn once more data on 3I are available.
The discussion section about where 3I was most probably formed is not easy to read. I had to read it several times before fully understanding the different boundary conditions, for e.g. origin in the bulge vs origin in the dense disk vs. origin in the thin disk. Maybe this section could be somewhat better structured by introducing e.g. more paragraphs, shorter sentences? I leave this to the authors.

In the conclusions, the authors write “The abundance of C, H, O, N and S-bearing species in 3I/ATLAS demonstrates…..» . N- and S—bearing species are clearly only a very minor topic (some data for OCS and CN) of this paper. This sentence therefore doesn’t make sense as a somwhat important conclusion of this paper.
The citations are not according to the rules of “Nature” (Formatting guide | Nature). They should consist only of a number as superscript and ordered sequentially.

Referee #2

(Remarks to the Author)
Dear authors,

Congratulations on an impressive dataset and important work, which are of great interest.

The possibilities offered by interstellar comets to provide insights into the conditions in systems far from the Solar neighborhood are very promising, and the derived isotopic ratios indeed provide a powerful tool in this regard. While the underlying scientific principles are not themselves novel at this point, the quality of the data and analysis make this a paper worth considering for publication in Nature.

The draft, in its current form, is well formulated. However, I do have suggestions for additions to figures to improve the presentation, and to include a better overview of how the different characteristics of the 3I/Atlas observations relate to the proposed origin of the ISO.

I also have some objections pertaining to the claims made in the current paper. The impact of the proposed very long residence time should receive greater consideration to support the paper's conclusions.

My comments on the paper are listed below:

- A major comment concerns the conclusions drawn about the origin of 3I/ATLAS based on isotopic ratios. While the prospect of linking the observed D/H and 12C/13C ratios to the origin of 3I/ATLAS is of great interest, the potential impacts of the proposed ~10-12 Gyr transit through the Galaxy need more discussion. Given the highly unusual CO2/H2O, CO/H2O, and CH3OH/HCN ratios as well as the D/H and 12C/13C ratios observed, a more detailed explanation of the potential impact (or lack thereof) is needed. What are the probable effects of mass-selective outgassing, long-term exposure to the ISRF, and cosmic-ray processing (e.g., sputtering of ices, dissociation, see also Maggiolo et al. 2026) of the comet?
This needs to be addressed before the link back to the origin can be considered robust. This applies to both the isotopic ratios themselves and the ratios among CO2, CO, and water. As it stands, the paper makes a good argument for what the high D/H ratio might tell us, if the long residence time has little impact, but it needs to carefully support the argument that the long residence time hasn't obscured the initial D/H ratio from its formation.
Recent models, such as Fulle 2020 & 2021, suggest that a high D/H ratio and a high abundance of hypervolatiles correlate and contribute to water-poor pebbles in the nucleus. From the data presented in this work, 3I/Atlas may contain an unusually high fractionation of such pebbles, compared with Solay System comets. Could this be explained by the long residence time, or can this provide alternative links to the formation conditions (e.g., metallicity)?

- In the draft, I cannot see that the authors mention that if the ISO formed >10 Gyr ago, the D/H ratio at that time would have been close to the primordial ratio of ~2.5e-5. Direct comparison with existing astrochemical modeling should therefore be approached with some caution (many assume the local present-day ISM value of ~1.5-1.6e-5), and new models with this ratio would be of interest, even if the difference alone is not sufficient to directly explain the highly elevated D/H ratio. Moreover, this should also be mentioned when comparing to observations in star-forming regions (disks and protostars).

- The authors list a number of different scenarios for the origin of 3I/Atlas. To help the reader maintain an overview of the different scenarios and the characteristics that support or contradict them, either a table or a schematic figure would be useful.

Minor comments:
- For clarity, could the authors add the residuals in Fig. 2 for each panel?
- Can you provide more details on the remarkable transition from CO2 to CO-dominated coma? What mechanisms could drive this transition, and what can it reveal about the history of 3I/ATLAS?
- On the bottom of page 8, the sentence "Consequently, the 12C/13C ratio ...". I believe this statement is only valid for CO/CO2, not for other tracers of the ratio, such as HCN.
- For the Methods section, page 14: How does the derived T_rot compare to the ortho/para ratio of H2O? Please comment on this.

If the comments above are adequately addressed, I consider this paper a strong candidate for publication.

Referee #3

(Remarks to the Author)
This manuscript presents JWST/NIRSpec observations of the coma of the interstellar object 3I/ATLAS, measuring H2O, HDO, CO2, 13CO2, CO, and 13CO, together with ALMA/ACA observations used to constrain the gas outflow velocity. The authors report an exceptionally high D/H ratio in water and elevated 12C/13C ratios in both CO2 and CO, and interpret these results as evidence for formation in a very cold, relatively low-metallicity environment early in Galactic history. The topic is highly timely and potentially of broad interest. The attempt to combine JWST measurements of multiple major and minor isotopologues in an interstellar comet with a broader discussion of Galactic chemical evolution is ambitious, and, if the conclusions are robust, the study could represent a substantial advance. Overall, I find the dataset compelling and the manuscript potentially suitable for publication after substantial revision. My expertise is strongest in cometary spectroscopy and isotopic interpretation, and I am less well placed to judge in detail the full range of Galactic chemical evolution models discussed in the manuscript. At the same time, I still have several important concerns. In particular, the main issues are the robustness of the minor-isotopologue measurements, the degree to which the inferred isotopic ratios depend on continuum placement and modeling assumptions, and whether the strongest claims regarding age and formation environment go beyond what is directly supported by the observational evidence.

Major comments

1. The main conclusions of the paper rely heavily on HDO, 13CO2, and 13CO. At present, the overall conclusion that the isotopic ratios are elevated appears plausible, but it is not yet sufficiently clear how independent that conclusion is from reasonable choices made in the reduction and fitting procedure. The case for HDO appears relatively convincing, but for 13CO2 and 13CO, the manuscript itself acknowledges non-negligible issues related to baseline definition and line blending. In my view, the robustness of these measurements should be presented more clearly in the main text, rather than being left primarily to the Extended Data.
In particular, I would ask the authors to clarify the following points more explicitly. First, please quantify how much of the reported range in 12C/13C is driven by baseline choice and how much reflects statistical uncertainty. Second, please clarify whether the adopted ranges should be interpreted as systematic envelopes, confidence intervals, or simply the span of plausible fits.

2. The manuscript uses the measured isotopic ratios to infer that 3I/ATLAS accreted roughly 10–12 Gyr ago. This is a very interesting interpretation, but I feel that the present wording does not fully reflect the degree of model dependence involved. The manuscript itself acknowledges the uncertainties in Galactic chemical evolution models, as well as degeneracy with Galactocentric radius. For this reason, I think the language regarding age should be made somewhat more cautious, at least in the Abstract, Discussion, and Conclusion.
For example, it seems reasonable to state that the data are consistent with an old origin and suggest accretion early in Galactic history. However, in places the current wording reads as though the isotopic data alone provide a tight age determination, and I think this should be softened.

3. The strongest observational conclusion of the manuscript, in my view, is that 3I/ATLAS exhibits an isotopic composition clearly distinct from known Solar System material. That claim appears well supported. By contrast, the subsequent interpretation in terms of a cold, strongly irradiated, low-metallicity, carbon- and oxygen-rich natal environment connected to early massive star formation involves several layers of inference and is therefore more model-dependent.
I therefore encourage the authors to distinguish more clearly between what is directly measured, what can be inferred with moderate confidence from isotope chemistry, and what constitutes a plausible but non-unique formation scenario. Making these distinctions more explicit would help sharpen the logic of the paper and improve its clarity and overall persuasiveness.

4. The manuscript states that the results are consistent with submitted ALMA and VLT studies. Given that the isotopic anomalies are central to the paper's main message, I think this point deserves somewhat fuller treatment, where possible. For example, it would be useful to indicate more clearly the extent of the agreement, the differences in tracers or methodology, and what conclusions remain common across the different analyses. This would help demonstrate that the central result does not depend entirely on one instrument or one modeling framework.

Minor comments

1. It would strengthen the manuscript if the Introduction or Discussion included one or two sentences explaining more explicitly why this result changes the broader picture of interstellar-object science, rather than simply adding another compositional measurement of 3I/ATLAS.

2. Figure 3 is important but somewhat dense. Consider simplifying the labeling or moving some category labels to the caption for readability.

3. Please ensure that all quoted uncertainties, ranges, and error bars are explicitly defined at first use and in figure and table captions. For the isotopic-ratio ranges in particular, the reader needs a very clear statistical interpretation.

4. There are a few typographical and style issues that should be corrected.
- Table 1 heading (page 8): "Wavelegth" whould be "Wavelength".
- Table 1 note (page 8): "correspond to to the ensemble" should be "correspond to the ensemble".
- ALMA Observations section (page 17): "The observations were conduced" would be "conducted".

That's all.

Version 2:

Reviewer comments:

Referee #1

(Remarks to the Author)
The authors have taken into account all of my remarks. From my point of view, the manuscript is now ready for publication as I have no further concerns.

Referee #2

(Remarks to the Author)
The authors have adequately addressed all my comments and concerns, and I have nothing further to add at this stage. Great work.

Referee #3

(Remarks to the Author)
I thank the authors for their careful and thoughtful revisions. In my view, the revised manuscript, together with the Supplementary Information, has addressed my major concerns satisfactorily.

This study presents important isotopic constraints on the interstellar object 3I/ATLAS using JWST/NIRSpec observations of

H_2O, HDO, CO_2, $^{13}CO_2$, CO, and ^{13}CO, together with ALMA/ACA observations used to constrain the gas outflow velocity. The resulting evidence for unusually high D/H and elevated $^{12}C/^{13}C$ ratios remains highly interesting and potentially of broad significance. In particular, the manuscript now does a better job of conveying the importance of the work, including what appears to be the first constraint on the water D/H ratio in extrasolar cometary material.

The revised version has substantially improved the presentation of the data and methodology. The interpretation of the isotopic-ratio ranges is now much clearer, including the role of statistical uncertainties and the intended meaning of the reported ranges. I also appreciate the clearer distinction between what is directly measured, what can be inferred with moderate confidence from isotope chemistry, and what remains a more model-dependent formation scenario, especially with the aid of the Supplementary Discussion and Supplementary Table 1. The language surrounding the age inference has also been appropriately softened, which improves the robustness and balance of the conclusions.

The expanded comparison with the independent ALMA and VLT studies is also helpful and strengthens the case that the main isotopic conclusions do not depend on a single dataset or modeling framework. Overall, I find the conclusions to be appropriately more cautious and better supported in the revised manuscript.

At this stage, I do not think additional experiments or analyses are necessary for publication. I have only a few minor issues that should be corrected before publication:

In the Figure 3 caption, the phrase “r_G = 14 kpc CN data is from ?]” appears to be an unresolved reference or LaTeX formatting error.
I also noted two additional unresolved references in the main text: “[25, 30? , 31]” on p. 8 and “[25, 39?]” on p. 9.

Subject to these minor corrections, I believe the revised manuscript is suitable for publication in Nature.

We thank the referees for their thoughtful appraisal of the manuscript, and the comments raised, which we have addressed in the revised version. Responses to the individual referee queries are given below, and associated modifications to the manuscript are highlighted in bold. Some additional numerical and textual corrections have been implemented. As a result of the Editor's request to shorten the text, much of the discussion has now been moved (and expanded upon) in a new Supplementary Information document.

Referees' comments:

Referee #1 (Remarks to the Author):

In the paper Â«Isotopic Evidence for a Cold and Distant Origin of the Interstellar Object 3I/ATLASÂ» by Cordiner et al. measurements of isotopic ratios for D/H in water and 12C/13C in CO and CO2 for comet 3I/Atlas are reported. These data are very precious as this is the first time that such ratios are presented for an interstellar object. The paper is very well written, the data are convincing and well presented, uncertainties are thoroughly discussed.
I have very little comments:
The discussion contains a lot of different options about the origin of 3I without a very clear conclusion, which is to be expected and which is fully ok. The two isotopic ratios are very important jigsaw pieces, but a final picture on when and where 3I was formed, if at all, can only be drawn once more data on 3I are available.
The discussion section about where 3I was most probably formed is not easy to read. I had to read it several times before fully understanding the different boundary conditions, for e.g. origin in the bulge vs origin in the dense disk vs. origin in the thin disk. Maybe this section could be somewhat better structured by introducing e.g. more paragraphs, shorter sentences? I leave this to the authors.

> The discussion has been restructured in light of all three referees comments, and to reduce the number of words in the main text as requested by the editor. Most of the detailed discussion is now moved to the Supplementary Information, and is summarized in a Supplementary Table to clarify the various lines of evidence and their veracity.

In the conclusions, the authors write â€œThe abundance of C, H, O, N and S-bearing species in 3I/ATLAS demonstrates...
N- and Sâ€”bearing species are clearly only a very minor topic (some data for OCS and CN) of this paper. This sentence therefore doesnâ€™t make sense as a somwhat important conclusion of this paper.

> We agree that this conclusion is not the main focus of the paper, so have moved it to the supplementary discussion to save space.

The citations are not according to the rules of â€œNatureâ€ (Formatting guide | Nature). They should consist only of a number as superscript and ordered sequentially.

> Done.

Referee #2 (Remarks to the Author):

Dear authors,

Congratulations on an impressive dataset and important work, which are of great interest.

The possibilities offered by interstellar comets to provide insights into the conditions in systems far from the Solar neighborhood are very promising, and the derived isotopic ratios indeed provide a powerful tool in this regard. While the underlying scientific principles are not themselves novel at this point, the quality of the data and analysis make this a paper worth considering for publication in Nature.

The draft, in its current form, is well formulated. However, I do have suggestions for additions to figures to improve the presentation, and to include a better overview of how the different characteristics of the 3I/Atlas observations relate to the proposed origin of the ISO.

I also have some objections pertaining to the claims made in the current paper. The impact of the proposed very long residence time should receive greater consideration to support the paper's conclusions.

My comments on the paper are listed below:

- A major comment concerns the conclusions drawn about the origin of 3I/ATLAS based on isotopic ratios. While the prospect of linking the observed D/H and 12C/13C ratios to the origin of 3I/ATLAS is of great interest, the potential impacts of the proposed ~10-12 Gyr transit through the Galaxy need more discussion. Given the highly unusual CO2/H2O, CO/H2O, and CH3OH/HCN ratios as well as the D/H and 12C/13C ratios observed, a more detailed explanation of the potential impact (or lack thereof) is needed. What are the probable effects of mass-selective outgassing, long-term exposure to the ISRF, and cosmic-ray processing (e.g., sputtering of ices, dissociation, see also Maggiolo et al. 2026) of the comet?
This needs to be addressed before the link back to the origin can be considered robust. This applies to both the isotopic ratios themselves and the ratios among CO2, CO, and water. As it stands, the paper makes a good argument for what the high D/H ratio might tell us, if the long residence time has little impact, but it needs to carefully support the argument that the long residence time hasnâ€™t obscured the initial D/H ratio from its formation.
Recent models, such as Fulle 2020 & 2021, suggest that a high D/H ratio and a high abundance of hypervolatiles correlate and contribute to water-poor pebbles in the nucleus. From the data presented in this work, 3I/Atlas may contain an unusually high fractionation of such pebbles, compared with Solay System comets. Could this be explained by the long residence time, or can this provide alternative links to the formation conditions (e.g., metallicity)?

> The referee raises an interesting concern about mass-selective outgassing. When we consider outgassing of Solar System comets, the

pressure at the nucleus is sufficient to impart momentum to all the sublimated gases and drive them outward (in a ~free expansion). In this way, the sublimated gases are well mixed, and travel as a collisionally dominated fluid (eg. Combi et al. 1996, Rodgers et al. 2004, Zakharov et al. 2023). In such a fluid outflow, mass-selective effects are negligible. On the other hand, differences in volatility of different species can drive differential outgassing rates when the temperature is near the sublimation temperature. Therefore, close to the CO sublimation temperature, one expects the lighter isotopologue to sublimate more rapidly. If the nucleus spent a long time at 28 K, then the ice composition could become depleted in the lighter isotopologue. However, since this effect is so temperature-sensitive (confined to a range of a few Kelvin), the chances of isotope-selective outgassing occurring at a significant level are small. Considering the presence of hypervolatiles in 3I/ATLAS (CO and CH_4), we also consider it unlikely that the object has spent a significant amount of time close to the H_2O/HDO sublimation temperature, since that would have depleted the hypervolatiles. Therefore, isotope selective fractionation of H_2O also seems unlikely. With respect to mass-selective sputtering of the surface, the difference in isotopologue masses for the species of interest is negligible compared to the high momentum of cosmic rays, so we do not anticipate this would cause isotopic fractionation. Furthermore, any fractionation due to collisions with interstellar gas and dust would be confined to the outer surface of the nucleus. Due to its high gas production rate, we estimate that 3I/ATLAS lost 1-10 m of surface depth during the perihelion passage (depending on the size of the nucleus). Therefore, our post-perihelion JWST observations should have been probing material not subject to any such isotopic fractionation effects.

> The work of Fulle et al. (2021) reproduces a trend in D/H ratios as a function of nucleus "hyperactivity", that was found by Lis et al. (2019). It should first be noted that the Lis et al. (2019) trend is somewhat tentative due to noise and a lack of statistics; indeed, the low D/H found recently in comet 12P (Cordiner et al. 2025) goes against this trend, since this was not a hyperactive comet (its active fraction was < 1). Nevertheless, within the context of the Lis et al. trend, the high D/H ratio of 3I/ATLAS would correspond to an implausibly small H_2O active fraction. The fact that ALMA observations around perihelion (when the H_2O active fraction was >1) also showed very high D/H demonstrates that the Fulle et al. model is probably not the correct explanation for the D/H ratio in 3I/ATLAS. Furthermore, the Fulle et al. model does not touch on how a D/H ratio as high as 1% could be attained, which is more a question of the astrochemical origin of the ice, rather than a question related to the balance of water-rich and water-poor pebbles inside the nucleus. As mentioned above, a major reduction in the water-rich pebble content does not seem to be consistent with our understanding of how the interior of 3I/ATLAS may (or may not) have been processed during its interstellar passage.

- In the draft, I cannot see that the authors mention that if the ISO formed >10 Gyr ago, the D/H ratio at that time would have been close to the primordial ratio of ~2.5e-5. Direct comparison with existing astrochemical modeling should therefore be approached with some caution (many assume the local present-day ISM value of ~1.5-1.6e-5), and new models with this ratio would be of interest, even if the difference alone is not sufficient to directly explain the highly elevated D/H ratio. Moreover, this should also be mentioned when comparing to observations in star-forming regions (disks and protostars).

> This is a good point, that was neglected during our efforts to understand the D/H ratio at low metallicity. Preliminary modeling by co-author Furuya shows that the water ice D/H varies approximately linearly with the elemental D/H, and we now explain this in the Supplementary Discussion section.

- The authors list a number of different scenarios for the origin of 3I/Atlas. To help the reader maintain an overview of the different scenarios and the characteristics that support or contradict them, either a table or a schematic figure would be useful.

> We have added a new table to the new Supplementary Information section to summarize the evidence for a cold and distant origin.

Minor comments:
- For clarity, could the authors add the residuals in Fig. 2 for each panel?

> Done.

- Can you provide more details on the remarkable transition from CO_2 to CO-dominated coma? What mechanisms could drive this transition, and what can it reveal about the history of 3I/ATLAS?

> Although we agree that this is an interesting and notable feature of our data, unfortunately, discussion of the chemical evolution of the coma is beyond the scope of our Nature article, which by necessity, is tightly focused on the isotopic story. Loss of the CO-depleted surface layers during perihelion, exposing more deeply-buried CO ice, seems likely.

- On the bottom of page 8, the sentence "Consequently, the $^{12}C/^{13}C$ ratio ...". I believe this statement is only valid for CO/CO_2, not for other tracers of the ratio, such as HCN.

> We agree that this part of the discussion did not pay close attention to the variable isotopic ratios in other molecules, so this is now rectified.

- For the Methods section, page 14: How does the derived T_{rot} compare to the ortho/para ratio of H_2O? Please comment on this.

> It is interesting to note the high (near-equilibrium) ortho/para ratio for H_2O, while $T_{rot}(H_2O)$ remains low, which implies the OPR is not directly affected by the gas (rotational) temperature. We now comment on this finding in the Methods section, and reference Mumma et al. (1987) for further information.

If the comments above are adequately addressed, I consider this paper a strong candidate for publication.

> We thank the referee for their useful and insightful comments that have significantly improved the manuscript.

Referee #3 (Remarks to the Author):

This manuscript presents JWST/NIRSpec observations of the coma of the interstellar object 3I/ATLAS, measuring H2O, HDO, CO2, 13CO2, CO, and 13CO, together with ALMA/ACA observations used to constrain the gas outflow velocity. The authors report an exceptionally high D/H ratio in water and elevated 12C/13C ratios in both CO2 and CO, and interpret these results as evidence for formation in a very cold, relatively low-metallicity environment early in Galactic history. The topic is highly timely and potentially of broad interest. The attempt to combine JWST measurements of multiple major and minor isotopologues in an interstellar comet with a broader discussion of Galactic chemical evolution is ambitious, and, if the conclusions are robust, the study could represent a substantial advance. Overall, I find the dataset compelling and the manuscript potentially suitable for publication after substantial revision. My expertise is strongest in cometary spectroscopy and isotopic interpretation, and I am less well placed to judge in detail the full range of Galactic chemical evolution models discussed in the manuscript. At the same time, I still have several important concerns. In particular, the main issues are the robustness of the minor-isotopologue measurements, the degree to which the inferred isotopic ratios depend on continuum placement and modeling assumptions, and whether the strongest claims regarding age and formation environment go beyond what is directly supported by the observational evidence.

Major comments

1. The main conclusions of the paper rely heavily on HDO, 13CO2, and 13CO. At present, the overall conclusion that the isotopic ratios are elevated appears plausible, but it is not yet sufficiently clear how independent that conclusion is from reasonable choices made in the reduction and fitting procedure. The case for HDO appears relatively convincing, but for 13CO2 and 13CO, the manuscript itself acknowledges non-negligible issues related to baseline definition and line blending. In my view, the robustness of these measurements should be presented more clearly in the main text, rather than being left primarily to the Extended Data.
In particular, I would ask the authors to clarify the following points more explicitly. First, please quantify how much of the reported range in 12C/13C is driven by baseline choice and how much reflects statistical uncertainty. Second, please clarify whether the adopted ranges should be interpreted as systematic envelopes, confidence intervals, or simply the span of plausible fits.

> The text has been updated to indicate that the quoted ranges include statistical uncertainties, and those uncertainties are now given as a percentage. We now state that these ranges should be interpreted as the range of plausible fit parameters.

2. The manuscript uses the measured isotopic ratios to infer that 3I/ATLAS accreted roughly 10â€“12 Gyr ago. This is a very interesting

interpretation, but I feel that the present wording does not fully reflect the degree of model dependence involved. The manuscript itself acknowledges the uncertainties in Galactic chemical evolution models, as well as degeneracy with Galactocentric radius. For this reason, I think the language regarding age should be made somewhat more cautious, at least in the Abstract, Discussion, and Conclusion.
For example, it seems reasonable to state that the data are consistent with an old origin and suggest accretion early in Galactic history. However, in places the current wording reads as though the isotopic data alone provide a tight age determination, and I think this should be softened.

> Upon reappraisal of the uncertainties in the cited Galactic chemical evolution models, and the lack of broad consensus regarding stellar populations (and thus nucleosynthetic yields) as a function of time and Galactocentric radius, we agree that present models are not sufficiently accurate to define a firm quantitative age for 3I/ATLAS. Qualitatively, we believe the main conclusion regarding a likely old age for 3I/ATLAS is correct, so we have revised the manuscript text accordingly.

3. The strongest observational conclusion of the manuscript, in my view, is that 3I/ATLAS exhibits an isotopic composition clearly distinct from known Solar System material. That claim appears well supported. By contrast, the subsequent interpretation in terms of a cold, strongly irradiated, low-metallicity, carbon- and oxygen-rich natal environment connected to early massive star formation involves several layers of inference and is therefore more model-dependent.
I therefore encourage the authors to distinguish more clearly between what is directly measured, what can be inferred with moderate confidence from isotope chemistry, and what constitutes a plausible but non-unique formation scenario. Making these distinctions more explicit would help sharpen the logic of the paper and improve its clarity and overall persuasiveness.

> We concur with the referee that insufficient modeling has been performed to rigorously explore the parameter space for 3I/ATLAS's possible origin in space and time, so we have updated the text to be more cautious. As suggested by referee #2, a table is now included in the Supplementary Information summarizing the evidence for a cold and distant origin. This table classifies the strengths of the various different lines of evidence: (S)trong, (M)oderate and (C)ircumstantial.

4. The manuscript states that the results are consistent with submitted ALMA and VLT studies. Given that the isotopic anomalies are central to the paperâ€™s main message, I think this point deserves somewhat fuller treatment, where possible. For example, it would be useful to indicate more clearly the extent of the agreement, the differences in tracers or methodology, and what conclusions remain common across the different analyses. This would help demonstrate that the central result does not depend entirely on one instrument or one modeling framework.

> Additional discussion of the relationship between these observations and our own is now given at the start of the Supplementary Discussion.

Minor comments

1. It would strengthen the manuscript if the Introduction or Discussion included one or two sentences explaining more explicitly why this result changes the broader picture of interstellar-object science, rather than simply adding another compositional measurement of 3I/ATLAS.

> It is somewhat difficult to expound on the novel aspects of our work without falling foul of the strict Nature rules about not claiming precedence. Nevertheless, we have added a sentence to the end of the conclusion to reiterate the importance of 3I/ATLAS in the context of understanding the evolution of the Galaxy. If the referee has other ideas regarding the big-picture ISO science they had in mind, would they mind being more explicit?

2. Figure 3 is important but somewhat dense. Consider simplifying the labeling or moving some category labels to the caption for readability.

> It is difficult to update the labeling substantially, while retaining the required information. However, we found that we had misclassified HOPS370 as a MYSO, so this necessitated updating the labels in the protostars section of the D/H plot.

3. Please ensure that all quoted uncertainties, ranges, and error bars are explicitly defined at first use and in figure and table captions. For the isotopic-ratio ranges in particular, the reader needs a very clear statistical interpretation.

> Done.

4. There are a few typographical and style issues that should be corrected.
- Table 1 heading (page 8): â€œWavelegthâ€ whould be â€œWavelengthâ€.
- Table 1 note (page 8): â€œcorrespond to to the ensembleâ€ should be â€œcorrespond to the ensembleâ€.
- ALMA Observations section (page 17): â€œThe observations were conducedâ€ would be â€œconductedâ€.

> Fixed.

We thank the referees for taking the time to read and comment on the revised version of our manuscript.

Individual comments are addressed as follows:

In the Figure 3 caption, the phrase â€œr_G = 14 kpc CN data is from ?]â€ appears to be an unresolved reference or LaTeX formatting error.
I also noted two additional unresolved references in the main text: â€œ[25, 30? , 31]â€ on p. 8 and â€œ[25, 39?]â€ on p. 9.

> This was a mislabeled reference to Milam et al. (2005), due to not running Latex enough times! The erroneous reference has now been fixed. Many thanks.