nature portfolio

Peer Review File

Large Gas Inflow Driven by A Matured Galactic Bar in Early Universe

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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)
In this study, the authors present the findings of multi-wavelength observations of a disc galaxy, which is a typical dusty star formation galaxy that has a bar found earlier in time (at z~2.4). With the help of observations from Chandra, JWST, ALMA, and VLA, the authors were able to examine not just the stellar component but also the gas component's dynamics, star formation, and even the characteristics of the AGN at the centre.

This galaxy is extremely massive and contains a high gas mass content, as found by the authors. The gas presents non-circular motions in the centre, which is consistent with the presence of a bar and spiral arms. These stellar structures are found in the majority of massive disc galaxies today. The authors also found the properties of the bar, such as the bar strength and the bar pattern speed. This work can provide a general picture of the gas dynamics in a dusty star-forming galaxy at a high redshift.

That being said, it is not clear what the impact of their results will be on our understanding of how galaxies form and the role of the bars. The authors could show if their findings are expected in galaxy formation models such as state-of-the-art cosmological hydrodynamic simulations. Could these observations possibly provide some constraints on galaxy evolution models? There are also some concerns regarding the analysis presented here.

The main comments:

1) I would like to suggest that the authors include whether these results are consistent with galaxy formation models. For instance, cosmological hydrodynamical simulations predict stellar bars as early as z=4 (Rosas-Guevara et al. 2022, Bi et al. 2022b). I suggest the authors compare the gas fractions to the one observed at this given stellar mass. Also, I recommend that the authors see if this galaxy is an outlier according to simulations. This will clearly show the impact of the results, and this kind of study can give some understanding of how galaxies form and evolve, especially during earlier times.

2) In reference to my earlier comment, I would like to point out that the manuscript on page 3 mentions that ‘the observation indicates that a classical bar can grow and become mature in early gas-rich discs, as opposed to numerical simulations that suggest a high gas fraction makes bar difficult to form and survive because gas strongly responds to bar perturbation in a way counter to bar growth’. The numerical simulations referenced by the authors serve as control experiments to model a local spiral galaxy. However, as mentioned in comment 1, there are cosmological hydrodynamical simulations that predict the presence of bars in the high-redshift universe, where the gas content is generally high. It would be beneficial to compare the gas fractions obtained with those identified in the current state-of-the-art cosmological simulations.

3) I recommend that the authors include the dynamical mass, which could make clear that the mass in baryons is dominated in the central part to have a bar. I understand that the authors did not include this because the result was highly sensitive to inclination, but I believe it could be possible to cite a lower limit for the baryon fraction that could be compared with simulated galaxies.

4) The authors provide limited discussion on the scenario in which the non-circular motions might be attributed to a gas outflow potentially generated by the highly active supermassive black hole at the centre, as observed in the case of the

galaxy J0107a. The authors present the velocity gradient along the minor axis (Fig 3c), which might be interpreted as an outflow. However, they dismiss this hypothesis in the last paragraph of page 4, noting that it is detected on one side of the galaxy and exhibits low velocities. However, there are studies that have shown that low-velocity outflows are expected in a clumpy ISM (see Fig.6 in Ward et al. 2024) in the cold phase gas. I would kindly suggest that the authors provide a clearer explanation for their rejection of this scenario.

5) Furthermore, the authors noted that this galaxy is nearly face-on; however, they calculate the bar pattern speed using the Tremaine-Weinberg method, which is known to be influenced by the galaxy's inclination, particularly for those that are almost face-on (Roshan et al. 2021). The authors have not addressed this matter. They also did not specify which component, whether gas or stars, of the galaxy was used to trace the bar pattern speed. In the case of the gaseous component, it has been demonstrated that employing the Tremaine-Weinberg method may not accurately retrieve the true pattern speed and could lead to an overestimation when relying on CO tracers (Borodina et al. 2023). I recommend that the authors include these uncertainties in the estimate of the bar pattern speed.

Moderate and comments:

1) I would recommend that the authors include a table in the extended data that outlines the main properties of the galaxy and the bar, such as gas fraction, gas mass, stellar mass, bar strength, bar pattern speed, SFR, net gas inflow, Mdyn, and others. This would summarise the main results of the paper.

2) I would suggest the authors include the errors due to the uncertainty of the value of the inclination in the bar pattern speed in the section 'The pattern speed of J0107a;s bar'.

3) In the section titled 'Molecular gas mass', found on page 17. I kindly recommend that the authors provide justification for the value of the conversion factor between molecular gas and CO luminosity, as well as discuss how this might influence their results, given the wide range of values present in the literature.

4) I would like to suggest that the authors include information on how they determine the bar length and corotation radius, as this is not addressed in the extended data section.

Minor comments:

1) I would suggest the authors add a reference regarding the gas distribution of a local bar in the text on page 2: 'We observe the carbon monoxide and atomic carbon emission lines of the dusty star-forming galaxy J0107a and find the bar of J0107a has gas distribution and motion in a pattern identical to local bars'. This would make it easy to find the comparison with local bars.

2) There appears to be a typo on page 3, paragraph 1. In the text 'To unveil … using the ALMA' change to 'To unveil…using ALMA'

3) There appears to be a typo on page 3, paragraph 1 in the text '… as opposed to numerical simulations that suggest a high gas fraction makes bar difficult…', change to 'a bar'
I suggest that the authors include a vertical line in Extended Data Figure 5 to indicate the bar length. I would anticipate that the value of Qt is greater within the bar region.

4) I would like to suggest adding a vertical line to indicate the bar length in Figure 3(f), as this would help to show whether the non-circular motions peaked within the bar region.

5) I suggest that the authors include a reference to high redshift bars in galaxy formation models on page 2 in the text: 'They are believed to be long-lived structures3 and are now identified at redshift z > 2'.

Referee #2

(Remarks to the Author)
The role of bars in gas flows at z=2 has a renewed interest due to the observations of JWST that suggest they may be more common at high-z than we thought. The observation that both (1) bars can survive in very gas rich systems and (2) that they may be driving large inflows is indeed exciting science that may warrant Nature. I think the result is unique, and has the opportunity to make a significant impact.

My main issue is on the uncertainty in deriving the mass inflow rate. I realize that it is heavily model dependent, but it seems to me this could easily be very large and would impact the interpretation of the results. I would like to see more discussion on the uncertainty in the mass-flow rate of the bar driven inflow. Equation 5 in the extended material says that the inflow depends on deltaL/L, which I think is proportionate to 1/V(r)^2. How do uncertainties in the resolved velocity profile translate to changes the mass flow that is derived? I would recommend a monte carlo method to apply the uncertainty on V and propogate that through the mass flow calculation. I would also expect that the uncertainty in the inclination could affect this. The authors should discuss this as well. I assume that some amount of error could come from Sigma_gas. This is derived from the CI-to-gas conversion factor? This has some level of uncertainty, which should be considered for this calculation.

The mass flow rate is much higher that the SFR. This may be explained by uncertainty in mass-flow, but if it is not, then what happens to the rest of the gas? The authors suggest there may be evidence of an outflow, but argue it is not outflow. Does this create a problem for the mass build up in the center of the galaxy? Some discussion of this would be helpful.

This is a minor thing, but its not clear to me what the SFR is. In the 2nd paragraph the authors state that the total SFR of the galaxy is 500 Msun/yr. Then later the author's state that the SFR within the bar region is 500 Msun/yr. Is all star formation happening within the bar region? I suppose a very large fraction of the CO is in the bar, perhaps the SFR tracer is likewise constrained to that region.

If the bar is triggered by an instability, would this be reflected by low Toomre-Q? this seems straightforward to test with the data.

I was expecting more discussion of how the gas flow is impacting the galaxy morphology and substructure. If there is an inflow of 1000 Msun/yr to the center of the galaxy, and subsequent star formation, does this make a bulge? Can the authors speculate on the mass of the bulge this would form? I would expect the bulge that was formed with be very old at z=0, presumably of order 10 Gyr. The authors could estimate the velocity dispersion of the gas and use that to speculate on the dispersion of the stars. Typically in the local Universe if bars build up a "bulge" in their center this will have a relatively low velocity dispersion. Would this be different? I found this part of the discussion at the end a bit missing.

Referee #3

(Remarks to the Author)
In their manuscript "Gigantic Gas Inflow Driven by A Matured Galactic Bar in Early Universe", Huang S. and co-authors report a kinematic study that hints to the discovery of a galactic bar in the galaxy J0107a, at z=2.467.
Observations are mainly based on — though not limited to — carbon monoxide and atomic carbon emission lines. In this paper, they use a number of simple models, tools and equations commonly assumed to shed new light on the scenario of bar assembly and formation during galaxy evolution around z=2. This type of study is quite new thanks to the quality of observational data, and the framework in which it takes place does and provides important context to the field of study. In my opinion this is a valuable piece of work that warrants publication. However, there are quite a number of issues to be addressed. The most pressing of these are those related to the interpretation of a specific (only one) observation that is used to draw general conclusions on bar formation and galaxy evolution, along with issues dealing with the rather vague comparison with numerical simulations. In addition to these, there are numerous facets of the work which need to be described more clearly to be useful to the community.

1. — (page 1) - Can you infer the timescale of bar formation of your galaxy? This would be of paramount relevance to support your statement that "bars are believed to be long-lived structures". Do you have any evidence that suggests how long can the bar of this system survive in the future and maybe reach lower redshift?

2. — (page 2) - The authors claim "… at a strength unseen before in numerical experiments" in the abstract. This statement has to be heavily mitigated in the abstract, and the point clearly addressed in the main text (see my comment #6 here below).

3. — (page 2) - How generic can be the interpretation of results that you provide the community with? The bar that you discovered is located within the most barred spiral galaxy known at z>2, a system known to be even more massive than the heaviest discs in the Local Universe. How general can the picture that you draw be, in terms of bar and galaxy evolution? This point deserves an accurate discussion.

4. — (page 3) - "Considering the formation timescale of J0107a ~ M_star/SFR ~1 Gyr and the well-developed bar structure embedded in gas, the observation indicates that a classical bar can grow and become mature in early gas-rich disks, as opposed to..." —> This discrepancy between predictions and observations has to be deepened and explained in details. Which are the conditions that in simulations are analysed that cannot hold once this barred galaxy is considered?

5. — (page 3) - "… as opposed to numerical simulations that suggest a high gas fraction makes bar difficult to form and survive because gas strongly responses to bar perturbation in a way counter bar growth." —> This sentence is in clear opposition to the sentence "This is in agreement with numerical simulations that show a high baryon fraction promotes early bar formation". Possible reasons of discrepancies have to be clearly addressed.

6. — (page 3) - The authors often refer to numerical simulations, and I think there is some confusion in the comparison between observations and predictions from numerical simulations that has to be clearly addressed throughout the paper. When comparing with numerical experiments or simulations, relevant details have to be provided: are these works and ensuing conclusions based on theoretical or analytical models? Are results inferred from simulations with idealised set-ups? Are we considering findings obtained from full-physics cosmological, hydrodynamical simulations? Are the initial conditions of the cited simulations comparable with the observed system we are analysing?
While I would like to refrain to suggest papers to quote (e.g. Athanassoula et al., 1992; Wada, 1994; Kraljic et al., 2012; Goz et al., 2015; Sormani et al., 2015; Fragkoudi et al., 2017; Rosas-Guevara et al., 2024, just to name a few studies), I would strongly encourage the authors to devote a short section of their manuscript to the comparison with previous numerical works.

I would consider an asset if the authors may also want to consider to include a short paragraph about the comparison with previous observations of barred galaxies across redshift.

7. — (page 3) - You write (page 2) that the bar that you observe is “in the formation stage”. Can you be more precise and quantitative in suggesting supporting (observational/dynamical) evidence (besides the below-mentioned “remarkably strong bar perturbation” inferred from the ratio between maximum tangential and radial forces) of this statement?

8. — (page 4) - As for the gas inflow rate, can you provide details about the region within which it is computed? At page 6, you also mention the galaxy circum-galactic medium and the cosmic web: is there a way to quantify the gas inflow rate in different regions/at different distances from the galaxy centre?

9. — (page 4) - Please, provide some context to guide the reader on whether nuclear ring dimensions typically consistent with 0.8 kpc are common features for a system of this kind or not.

10. — (page 4) - "This result also indicates that the bar can tolerate high gas inflow rate, in contrast to some simulations which suggest it will dissolve under such conditions.” —> In my opinion, once more the author try to draw a general conclusion from one observation and to disprove results from simulations, without being able to explain possible motivations at the origin of the disagreement. This paragraph should be substantially re-phrased, and its content edited.

11. — (page 5 and Figure 3) - In Figure 3, errors on circular and non-circular velocities as a function of radius do not include uncertainties in inclination and position angle: please, add these uncertainties (in an additional panel if needed, for the sake of clarity).

12. — (page 6) - You write that “Besides the striking structural and dynamical similarities with local barred spirals, J0107a displays SFR and gas fraction more typical of a massive star-forming galaxy at z ~ 2.5, the peak
of cosmic star formation.”: In my opinion, this statement has to fit and be put in a clearer framework to interpret results. Do we have to think that observations in this work report an unprecedentedly seen barred galaxy, or do we rather have to imagine a massive disc galaxy at z>2 with a dynamical feature in the innermost region that resembles a low-z bar? The highlighted and discussed scenario should be as quantitative as possible.

13. — (page 10, Figure 3) - In several panels of Figure 3, units of measurement are not correctly reported: for instance, you write in the y-label of panel (f) “V / km s^{-1}”, while this should be “V (km s^{-1})”. Please, carefully check all the figure labels.

14. — (page 13) - You mention “partially failed observations”. Can you elaborate on them? How does including them in your analysis impact the quality of the results?

13. — (page 15) - “No companion galaxy is detected in the cube or continuum image” —> Please, state clearly (or repeat) the distance (in physical kpc, from the target centre) within which no galaxy companion is detected.

15. — (page 18) - As for the conversion factor between molecular mass and CO luminosity, while I am happy with the reference value [38] that you assume and I appreciate the substantial agreement with values inferred from dust continuum and another conversion factor [39], I think it would be beneficial to further discuss the inferred molecular gas mass, by also considering e.g. conversion functions (see e.g Tacconi et al., 2020) instead of conversion factors only (which can be more sensitive to the assumed galaxy physical parameters).

16. — (page 18) - Please, state the distance between J0107b and J0107a.

17. — (page 19) - Can you please elaborate more on the assumption that “the bar influence becomes smaller at large radii”, as for the derived parameters of the spatial configuration of the galaxy disc?

18. — (page 21, somewhat related to pt. 8.) - Is the net inflow rate of 1100 M_Sun/yr retrieved by assuming R=disc scale length? Please clarify.

19. — (page 23) - I think the paper would benefit from a critical discussion on the impact that the uncertainty on the galaxy disc inclination may have on all the results and conclusions.

20. — (page 23) - “This is in agreement with numerical simulations that show a high baryon fraction promotes early bar formation [46].” —> Please, also cite the recent paper by Verwilghen et al., 2024 (A&A, 687, A53).

Overall, I found the paper original, the data of very good quality and the methodology clearly explained; also, the manuscript is clearly written. More credit could be given to previous work, the validity of the interpretation of the results has to be strengthened, and the robustness of the conclusions has to be improved.

Version 1:

Reviewer comments:

Referee #1

(Remarks to the Author)
I appreciate the authors for addressing all my concerns and for incorporating the discussion of their findings with the expectations for galaxy formation models. This includes state-of-the-art cosmological hydrodynamic simulations and numerical simulations. The authors also provide constraints for the galaxy formation models. The clarity in the description of the results and the methodology, particularly regarding the errors, has been improved. I have no further concerns at this time, except that in the discussion of simulations on page 7, line 140, the authors cite Rosas-Guevara et al. 2020. However, it should be cited as Rosas-Guevara et al. 2022, as the authors mentioned using Figure 7 from Rosas-Guevara et al. 2022 in their response.

==== In response to follow-up email re: appropriateness of comparison to simulations ====

I understand your concern regarding the need for a proper comparison, and I believe the authors have addressed this issue. However, I respectfully hold a different view regarding the comparison being misleading. Even, the title of the manuscript refers to a massive galaxy, specifically focusing on the presence of a bar at high redshift, rather than a SMG spiral galaxy with a bar. I believe it is evident that the manuscript's relevance lies in the fact that a bar is contributing to the formation of this spiral massive galaxy by channelling gas towards the centre, and they offer a measurement of this infalling gas. The gas fraction is indeed high, which aligns with expectations for (SMG) galaxies at high redshift; however, it is too high to agree with the presence of a mature bar in that context. This is the intriguing aspect; they offer a measurement of a physical mechanism by which this galaxy is acquiring gas, despite the unlikelihood of having a mature bar.

The other point, as you mentioned regarding reference 5 and Fig 5, both papers are using the same predictions of TNG50 as you can see in reference 16 mentioned in the caption of Fig.5. However, in reference 5, the authors are comparing the observations of low-mass galaxies (stellar mass<10^10 Msun) with a catalogue that includes only massive galaxies (Mstar>10^10 Msun). I respectfully disagree with reference 5 regarding their statement that the simulation does not predict a bar and that it utilised a catalogue of massive galaxies. TNG50 suggests a low fraction of barred galaxies in the low mass galaxy range, as indicated in Zana et al. 2022 (link below) in Fig. 17.

Referee #2

(Remarks to the Author)
Well done. Thank you for thorough response to my questions. I'm happy to accept this article.

==== In response to follow-up email re: appropriateness of comparison to low-z galaxies ====

Personally, my understanding is that the reason to compare to low-z barred galaxies is because the vast majority of theory for barred galaxies is developed on low-z galaxies. And there are multiple theories that argue that if the gas fraction is above some value (I believe its around 25% but that could be wrong), then the disk cannot sustain a bar and there is no bar driven inflow. So the reason to compare to low-z galaxies would be to imply that this galaxy is vastly different than the what the theory is developed to match, and by caveat bar driven inflows may play a more important role in galaxy evolution at z=2. You're the editor so you may feel that this is not a strong enough case for the journal, or alternatively that the authors don't do a good enough job of making it.

Referee #3

(Remarks to the Author)
I thank the authors for having addressed the issues raised in my previous report and for agreeing to modifying the manuscript accordingly. Overall, I am happy with the clarity and content of the current, revised version of the manuscript.

As a moderate/minor remark, I would like to point out that the comparison with theoretical models and state-of-the-art simulations is somehow not as advanced as the remaining part of the paper, and appears to be little conclusive. The authors explicitly write that the complexity of the comparison "calls for theoretical follow-up": in my opinion, if some details of the considered simulation set-ups to compare with were analysed, along with limitations (of both numerical experiments and the current observational counterpart), the comparison would have been way more informative.

==== In response to follow-up email re: appropriateness of comparison to simulations ====
In particular, as for the comparison with simulations, what the authors were actually supposed to make is a fair and thorough comparison with other barred galaxies from simulations — TNG, but not only that suite! — at the same redshift of their observed galaxy.

Then, as an additional step, they could compare their observation to other barred galaxies at z=0 (or at lower z) to interpret peculiar features of their object and understand differences. However, in this latter step, highlighting the different boundary conditions (e.g., inflow rate, star formation history, role of the central AGN, …) would be crucial to properly place the new observation in a proper evolutionary scenario.

Such a detailed and accurate comparison with simulations is not in the paper (while I think it should).

Referee #1 (Remarks to the Author):
In this study, the authors present the findings of multi-wavelength observations of a disc galaxy, which is a typical dusty star formation galaxy that has a bar found earlier in time (at z~2.4). With the help of observations from Chandra, JWST, ALMA, and VLA, the authors were able to examine not just the stellar component but also the gas component's dynamics, star formation, and even the characteristics of the AGN at the centre.
This galaxy is extremely massive and contains a high gas mass content, as found by the authors. The gas presents non-circular motions in the centre, which is consistent with the presence of a bar and spiral arms. These stellar structures are found in the majority of massive disc galaxies today. The authors also found the properties of the bar, such as the bar strength and the bar pattern speed. This work can provide a general picture of the gas dynamics in a dusty star-forming galaxy at a high redshift.
That being said, it is not clear what the impact of their results will be on our understanding of how galaxies form and the role of the bars. The authors could show if their findings are expected in galaxy formation models such as state-of-the-art cosmological hydrodynamic simulations. Could these observations possibly provide some constraints on galaxy evolution models? There are also some concerns regarding the analysis presented here.

Response: we thank the referee for detailed suggestions on comparison with simulations. Our results show the galaxy has baryon fraction consistent with galaxy and bar formation models, but the gas fraction is unexpected. In the revised version, we have added more quantitative discussion about comparison with both cosmological and controlled simulation, where we also discuss possible origins of the discrepancy between our observation and galaxy formation models, with key quantities (bar strength, gas fraction) presented. Regarding the concerns about our analysis, some additional explanations are added to the revised manuscript. The details are presented in responses to main comments.

The main comments:
1) I would like to suggest that the authors include whether these results are consistent with galaxy formation models. For instance, cosmological hydrodynamical simulations predict stellar bars as early as z=4 (Rosas-Guevara et al. 2022, Bi et al. 2022b). I suggest the authors compare the gas fractions to the one observed at this given stellar mass. Also, I recommend that the authors see if this galaxy is an outlier according to simulations. This will clearly show the impact of the results, and this kind of study can give some understanding of how galaxies form and evolve, especially during earlier times.

Response: The comparison with simulations is separated into a new paragraph from line 134. The target is baryon-dominated, following simulation predictions, but its gas fraction M_gas/M_star is about 0.7, much higher than the simulation prediction of upper 80% bound < 0.1 at the same stellar mass and redshift (extrapolated from Figure 7 of Rosas-Guevara et al. (2022) as they do not have such a massive galaxy in the simulated box). Therefore, it is an outlier and inconsistent with simulations.

For comparison with cosmological simulations, we mainly use results from Rosas-Guevara et al. (2022) which is the most relevant work. Bi et al. 2022, ApJ, 934, 52 also presented the evolution of gas fractions, but they used only one mass value, so we cannot extrapolate their results to our target. This work is cited in line 103 in another way. We did not include Fragkoudi et al. (2024) arxiv:2406.09453 because they averaged all galaxies in their figures so we could not make a direct comparison. We did not include Ansar 2025, ApJ, 978, 37 because their simulated galaxies are all of the same masses, so we cannot extrapolate their conclusion to match the mass of our target. Some discussions about controlled experiments are also presented.

2) In reference to my earlier comment, I would like toy point out that the manuscript on page 3 mentions that ‘the observation indicates that a classical bar can grow and become mature in early gas-rich discs, as opposed to numerical simulations that suggest a high gas fraction makes bar difficult to form and survive because gas strongly responds to bar perturbation in a way counter to bar growth’. The numerical simulations referenced by the authors serve as control experiments to model a local spiral galaxy. However, as mentioned in comment 1, there are cosmological hydrodynamical simulations that predict the presence of bars in the high-redshift universe, where the gas content is generally high. It would be beneficial to compare the gas fractions obtained with those identified in the current state-of-the-art cosmological simulations.

Response: as we stated previously, the gas fraction M_mol/M_star of J0107a is about 0.7, inconsistent with cosmological simulation prediction at high redshift. Therefore, the lack of gas in simulated bars is a problem. The discussion is presented in line 134, where we also present a comparison with controlled experiments.

3) I recommend that the authors include the dynamical mass, which could make clear that the mass in baryons is dominated in the central part to have a bar. I understand that the authors did not include this because the result was highly sensitive to inclination, but I believe it could be possible to cite a lower limit for the baryon fraction that could be compared with simulated galaxies.

Response: we have presented the dynamical mass for inclination = 7 deg within a radius of 10 kpc (2.2 times effective radius) at line 363, and also in the summary table. We assume 1 deg standard deviation of inclination and propagate it into mass uncertainty, and find any inclination values that deviate significantly from 7 deg will lead to exotic results as discussed from line 344. A discussion on possible inclination range is provided in lines 341-357, and a comparison of baryon fraction with simulated galaxies is added to line 134. We find a baryon fraction of ~0.8 within r=10 kpc, which is consistent with trends in simulated galaxies.

4) The authors provide limited discussion on the scenario in which the non-circular motions might be attributed to a gas outflow potentially generated by the highly active supermassive black hole at the centre, as observed in the case of the galaxy J0107a. The authors present the velocity gradient along the minor axis (Fig 3c), which might be interpreted as an outflow. However, they dismiss this hypothesis in the last paragraph of page 4, noting that it is detected on one side of the galaxy and exhibits low velocities. However, there are studies that have shown that low-velocity outflows are expected in a clumpy ISM (see Fig.6 in Ward et al. 2024) in the cold phase gas. I would kindly suggest that the authors provide a clearer explanation for their rejection of this scenario.

Response: We acknowledge the possibility of a clumpy outflow, and such a possibility cannot be completely ruled out. However, for a large-scale biconical outflow to explain the S-shape twist in the moment 1 map, we expect that the bi-polar outflow adds a blueshifted component to the far side and a redshifted component to the near side. Since the galaxy rotates clockwise, the resulting S-shape will have a direction opposite to what has been observed. A misaligned outflow may give the observed sign, but to produce the symmetric S-shape twist, the outflow should also be symmetric, but this is not the case in the minor axis position-velocity diagram. Therefore, the observed non-circular motion is not caused by outflow. Combined with the presence of strong bar distortion in both light distribution and gravitational potential, we argue that bar-induced non-circular motion should also be the most likely explanation of the minor axis PV diagram. Correspondingly, some description is added to line 83.

5) Furthermore, the authors noted that this galaxy is nearly face-on; however, they calculate the bar pattern speed using the Tremaine-Weinberg method, which is known to be influenced by the galaxy's inclination, particularly for those that are almost face-on (Roshan et al. 2021). The authors have not addressed this matter. They also did not specify which component, whether gas or stars, of the galaxy was used to trace the bar pattern speed. In the case of the gaseous component, it has been demonstrated that employing the Tremaine-Weinberg method may not accurately retrieve the true pattern speed and could lead to an overestimation when relying on CO tracers (Borodina et al. 2023). I recommend that the authors include these uncertainties in the estimate of the bar pattern speed.

Response: The main issue with applying the TW method to very low-inclination galaxies is that the line-of-sight (LOS) velocity becomes too low compared to velocity errors (Cuomo et al. 2019, A&A, 632, 51). This is not the case for J0107a because it has rotation fast enough to produce LOS velocity in the order of several tens to ~ 100 km/s even at a very low inclination, and the typical error in line-of-sight velocity map is about 10 km/s so meaningful fit is possible. On the other hand, we acknowledge the uncertainty associated with position angle and the use of CO as a kinematic tracer. We have tested different position angle values (+/- 5deg from the current value) and found measured pattern speeds agree within error, albeit with larger scatter in measured <x> and <V> values. This is consistent with the larger tolerable position angle error at low inclination found by Borodina et al. (2023). Also, the result holds for [CI](1-0) but with larger uncertainty due to lower S/N.

The issue associated with CO is that it has a clumpy distribution that may not trace the bar pattern as well as stellar kinematics, as suggested by the comparison between pattern speeds derived from CO and stellar kinematics of PHANGS galaxies (Williams et al. 2021, AJ, 161, 185). However, measuring the stellar kinematics of a z=2.467 dusty star-forming galaxy is impractical even for JWST. We argue that CO may work better in J0107a than the tested cases (Williams et al. 2021, Borodina et al. 2023) because

(1) if all or at least a large portion of the 888 um continuum (~ rest-frame 250 um) traces star formation, the observed 888 um continuum emission, being confined to the bar region of J0107a (Huang et al. 2023, ApJL, 958, 26), implies the bar is also the site of active star formation. Then the z=2.467 gas bar seen in CO(4-3) is the progenitor of present-day stellar bars, so CO also closely traces the bar pattern. In this case, the stellar mass doubling time (~1Gyr) and gas depletion time (~600Myr) are much longer than the dynamical time (~100-200 Myr), and the requirements of the TW method are satisfied. We have added these justifications to line 414.

(2) J0107a is quite different from their test sample since (i) from PHANGS-ALMA CO data we can see the majority of regions detected in CO is not associated with their tested bars and the dynamical signature (i.e., S-shaped zero velocity line) in moment 1 map is not apparent. Conversely, J0107a’s CO intensity distribution is dominated by the bar and shows prominent non-circular motion features. (ii) J0107a has a molecular gas mass more than an order of magnitude larger within the same radius than the tested PHANGS galaxies, and the filling factor should be much higher. We see extended components underlying the 15 kpc-diameter gas bar in both CO(4-3) and [CI](1-0). This is qualitatively different from the blank regions in PHANGS galaxies tested by Williams et al. (2021), suggesting the gas distribution is not as clumpy as

local galaxies. Also, the gas in the stellar disk region is dominated by molecular phase at high surface density like J0107a (>1000 M_{sun}/pc^2 for both gas and star). The similarity between CO(4-3) and [CI](1-0) distributions suggests CO is a reasonable tracer of total gas content, and total gas distribution can be used to trace the bar pattern as pointed out by Borodina et al. (2023).

Moderate and comments:

1) I would recommend that the authors include a table in the extended data that outlines the main properties of the galaxy and the bar, such as gas fraction, gas mass, stellar mass, bar strength, bar pattern speed, SFR, net gas inflow, Mdyn, and others. This would summarise the main results of the paper.

Response: a table is added to the end of the method section.

2) I would suggest the authors include the errors due to the uncertainty of the value of the inclination in the bar pattern speed in the section 'The pattern speed of J0107a;s bar'.

Response: We now use an inclination of 7 -/+ 1deg in the calculation instead of a single value. However, the conclusion of J0107a hosting a fast bar is robust against inclination because 1/sin(i) enters equally into the calculation of rotation and pattern speeds.

3) In the section titled 'Molecular gas mass', found on page 17. I kindly recommend that the authors provide justification for the value of the conversion factor between molecular gas and CO luminosity, as well as discuss how this might influence their results, given the wide range of values present in the literature.

Response: we now consider conversion functions that take into account the scaling relations between stellar mass, metallicity and its redshift evolution (Tacconi et al. 2018, ApJ, 853, 179). The result remains unchanged, and we have included the scatter of scaling relations and uncertainty of stellar mass in error. In mass inflow rate calculation, we include the error of inclination and reduce the conversion factor if it is not allowed by the dynamical mass. The description is added to line 284.

4) I would like to suggest that the authors include information on how they determine the bar length and corotation radius, as this is not addressed in the extended data section.

Response: the bar length is measured by ellipse isophote fitting as the radius of maximum ellipticity, presented in our previous work (Huang et al., 2023, ApJL, 958, 26), and the corotation radius is calculated by using the rotation and pattern velocities from this work. We added additional descriptions at line 100.

Minor comments:

1) I would suggest the authors add a reference regarding the gas distribution of a local bar in the text on page 2: 'We observe the carbon monoxide and atomic carbon emission lines of the dusty star-forming galaxy J0107a and find the bar of J0107a has gas distribution and motion in a pattern identical to local bars'. This would make it easy to find the comparison with local bars.

Response: we have added references for local bars at line 26: Kuno et al. 2007, PASJ, 59, 117 shows gas distribution; Fathi et al., 2005, MNRAS, 364, 773 and Lopez-Coba et al. 2022, ApJ, 939, 40 show the S-shape zero velocity line caused by non-circular motions. Due to the limitation on the number of references, only a few examples are cited here.

2) There appears to be a typo on page 3, paragraph 1. In the text 'To unveil … using the ALMA' change to 'To unveil…using ALMA'

3) There appears to be a typo on page 3, paragraph 1 in the text '… as opposed to numerical simulations that suggest a high gas fraction makes bar difficult…', change to 'a bar'

I suggest that the authors include a vertical line in Extended Data Figure 5 to indicate the bar length. I would anticipate that the value of Qt is greater within the bar region.

Response: We thank the referee for carefully reading the manuscript. The typos are now corrected correspondingly. A vertical line marking the bar semi-major axis length is added to Extended Data Figure 5. The value of Qt does drop rapidly beyond this radius.

4) I would like to suggest adding a vertical line to indicate the bar length in Figure 3(f), as this would help to show whether the non-circular motions peaked within the bar region.

Response: a vertical line marking bar semi-major axis length is added to Figure 3(f). The non-circular motions roughly peak at the middle of the bar. At r=8 kpc, another component is present. This is associated with the spiral arm (also visible in Figure 3).

5) I suggest that the authors include a reference to high redshift bars in galaxy formation models on page 2 in the text: 'They are believed to be long-lived structures3 and are now identified at redshift z > 2'.

Response: we have cited Rosas-Guevara et al. 2020, MNRAS, 491, 2547 at line 20 as a reference to high redshift bars in galaxy formation theories.

Referee #2 (Remarks to the Author):

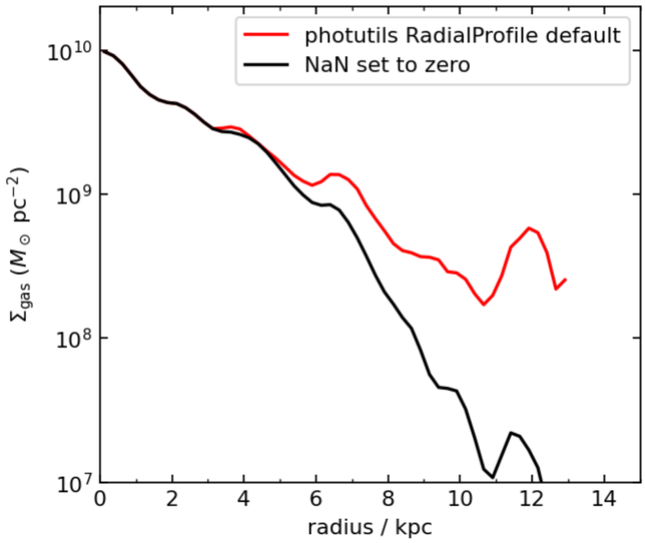
The role of bars in gas flows at z=2 has a renewed interest due to the observations of JWST that suggest they may be more common at high-z than we thought. The observation that both (1) bars can survive in very gas rich systems and (2) that they may be driving large inflows is indeed exciting science that may warrant Nature. I think the result is unique, and has the opportunity to make a significant impact.

My main issue is on the uncertainty in deriving the mass inflow rate. I realize that it is heavily model dependent, but it seems to me this could easily be very large and would impact the interpretation of the results. I would like to see more discussion on the uncertainty in the mass-flow rate of the bar driven inflow. Equation 5 in the extended material says that the inflow depends on deltaL/L, which I think is proportionate to 1/V(r)^2. How do uncertainties in the resolved velocity profile translate to changes the mass flow that is derived? I would recommend a monte carlo method to apply the uncertainty on V and propogate that through the mass flow calculation. I would also expect that the uncertainty in the inclination could affect this. The authors should discuss this as well. I assume that some amount of error could come from Sigma_gas. This is derived from the CI-to-gas conversion factor? This has some level of uncertainty, which should be considered for this calculation.

Response: We thank the referee for the suggestions on uncertainty analysis. In equation 5, there is another term 1/T(r) that also includes V(r), therefore the mass inflow rate is proportional to \Sigma_mol/V(r). The \Sigma_gas is derived by scaling total gas mass from CO(1-0) by [CI](1-0) flux map as both lines trace the total amount of cold molecular gas (e.g., Dunne et al. 2022, MNRAS, 517, 962, and the use of [CI](1-0) results in the same total gas mass). The error of mass inflow rate includes uncertainties in (1) V(r) measurement by fitting the resolved velocity map with limited S/N, including inclination, (2) CO(1-0) flux measurement and (3) the conversion from CO to total gas mass. We have added a description of error propagation at line 388.

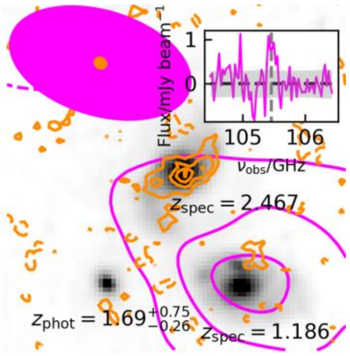
The mass flow rate is much higher that the SFR. This may be explained by uncertainty in mass-flow, but if it is not, then what happens to the rest of the gas? The authors suggest there may be evidence of an outflow, but argue it is not outflow. Does this create a problem for the mass build up in the center of the galaxy? Some discussion of this would be helpful.

Response: we have examined the calculation and found a mistake in the calculation: when calculating the radial profile of gas surface mass density from [CI](1-0) intensity map (this enters the \tau(r) term at line 381), regions without [CI](1-0) detection should be set to have zero mass, but by default, they are simply excluded by the astropy/photutils software, so the mass density was not averaged for full azimuthal range and overestimated, shown as the red line in the following figure. After fixing this error (the black line), the mass inflow rate agrees well with the SFR. For the blueshifted component seen in the minor axis PV diagram (now moved to the insert panel in Figure 3b), we argue it is caused by bar-induced non-circular motion instead of outflow. The description is refined at line 83.



This is a minor thing, but its not clear to me what the SFR is. In the 2nd paragraph the authors state that the total SFR of the galaxy is 500 Msun/yr. Then later the author's state that the SFR within the bar region is 500 Msun/yr. Is all star formation happening within the bar region? I suppose a very large fraction of the CO is in the bar, perhaps the SFR tracer is likewise constrained to that region.

Response: we added some additional description about SFR distribution at line 64. As described at line 33, the SFR is determined by multiwavelength SED fitting of galaxy-integrated photometry, where the main determinant of SFR is FIR dust continuum flux. The continuum emission at observed wavelength 888 um (orange contour in the following figure, Huang et al. 2023, ApJL, 958, 26) and 2.1 mm are only detected in the bar region (Figure 1b of this work) of this dusty star-forming galaxy. Therefore, most of the star formation is confined to the bar, but we do not know whether the star formation is totally in the nucleus, or also spreads over the bar because some dust emission may come from other heating sources such as old stars. The heating source problem also explains the relatively large uncertainty of the SFR as we have included dust emission from all types of heating mechanisms (star formation, old stars, AGN) in the model with their fractions varying freely from 0 to 1. Note that a high AGN fraction is ruled out by JWST/MIRI data, and contribution from old stars has constraint by the rest-frame NIR data from JWST/NIRCam, so we know most (if not all) of the dust emission is from star formation.



If the bar is triggered by an instability, would this be reflected by low Toomre-Q? this seems straightforward to test with the data.

Response: we calculated the Toomre-Q map using gas surface density and velocity dispersion probed by [CI](1-0) and rotation velocity from CO(4-3) but found the value is always higher than 1, so the bar instability is not reflected by Toomre-Q. On the other hand, the rotation velocity, gas disk size and mass satisfy the bar-instability criteria in Efstathiou et al. 1982, MNRAS, 199, 1069.

I was expecting more discussion of how the gas flow is impacting the galaxy morphology and substructure. If there is an inflow of 1000 Msun/yr to the center of the galaxy, and subsequent star formation, does this make a bulge? Can the authors speculate on the mass of the bulge this would form? I would expect the bulge that was formed with be very old at z=0, presumably of order 10 Gyr. The authors could estimate the velocity dispersion of the gas and use that to speculate on the dispersion of the stars. Typically in the local Universe if bars build up a “bulge” in their center this will have a relatively low velocity dispersion. Would this be different? I found this part of the discussion at the end a bit missing.

Response: Briefly, the answer is yes. The central part of J0107a is a pseudobulge with exponential radial profile, which is the product of bar-driven inflow and subsequent star formation. It also shows a lower velocity dispersion at the center than the surrounding regions in CO(4-3) and [CI](1-0) velocity dispersion maps. The feature persists among different masking methods when making moment 2 maps. Therefore, we may be witnessing rapid bar-driven inflow building the central disky bulge of a massive barred spiral during cosmic noon. Some discussion regarding this point is added on line 124. If 1/3 of the gas goes into the central starburst (Kormendy & Kennicutt 2004, ARA&A, 42, 603), the final pseudobulge mass will be added by ~1E11 Msun. Unfortunately, the S/N of the outer disk and spatial resolution in the inner region are not enough for reliable disk component decomposition, so the mass of the pseudobulge itself is unknown.

Referee #3 (Remarks to the Author):

In their manuscript “Gigantic Gas Inflow Driven by A Matured Galactic Bar in Early Universe”, Huang S. and co-authors report a kinematic study that hints to the discovery of a galactic bar in the galaxy J0107a, at z=2.467.
Observations are mainly based on — though not limited to — carbon monoxide and atomic carbon emission lines. In this paper, they use a number of simple models, tools and equations commonly assumed to shed new light on the scenario of bar assembly and formation during galaxy evolution around z=2. This type of study is quite new thanks to the quality of

observational data, and the framework in which it takes place does and provides important context to the field of study. In my opinion this is a valuable piece of work that warrants publication. However, there are quite a number of issues to be addressed. The most pressing of these are those related to the interpretation of a specific (only one) observation that is used to draw general conclusions on bar formation and galaxy evolution, along with issues dealing with the rather vague comparison with numerical simulations. In addition to these, there are numerous facets of the work which need to be described more clearly to be useful to the community.

Response: we thank the referee for the detailed suggestions for improving comparison with simulations. We have largely modified the part of comparison with simulations. This part is separated into a new paragraph, where we present the discussion in a way that focuses on specific physical processes and quantities related to bar formation instead of finding general conclusions. In the comparisons, we explicitly state what kind of simulation is to be compared and key quantities that have not been recovered by simulations (bar strength, gas fraction) and discuss possible origin of the discripancy. We also refined the description according to the comments.

1. — (page 1) - Can you infer the timescale of bar formation of your galaxy? This would be of paramount relevance to support your statement that “bars are believed to be long-lived structures”. Do you have any evidence that suggests how long can the bar of this system survive in the future and maybe reach lower redshift?

Response: to our knowledge, there is no method to directly measure the bar formation timescale from current data. A stellar age map may help, but spatially resolved spectroscopy of an SMG’s stellar population might be impractical even with JWST. Alternatively, we expect the formation of a large coherent structure extending the whole galaxy to take several dynamical times. Therefore, the formation timescale should be ~ several hundred Myrs - 1 Gyrs. The upper limit of bar formation timescale is 2.6 Gyr, set by the redshift z=2.467. Some description is added to line 50. For the connection with z=0 bars, there is some hint such as an exponential radial profile of the central stellar bulge and lower gas velocity dispersion in its center. If we have caught a typical local barred spiral in the stage of forming pseudobulge via rapid bar-driven inflow, then this is the expected behavior. The bar may survive to z=0 if not destroyed by a major merger. Some discussion is added to line 125.

2. — (page 2) - The authors claim “… at a strength unseen before in numerical experiments” in the abstract. This statement has to be heavily mitigated in the abstract, and the point clearly addressed in the main text (see my comment #6 here below).
Response: We have removed this statement from the abstract and added some description at line 134.

3. — (page 2) - How generic can be the interpretation of results that you provide the community with? The bar that you discovered is located within the most barred spiral galaxy known at z>2, a system known to be even more massive than the heaviest discs in the Local Universe. How general can the picture that you draw be, in terms of bar and galaxy evolution? This point deserves an accurate discussion.

Response: we acknowledge the fact that the target is a unique galaxy with extreme mass. To the best of our knowledge, it is the most massive barred spiral galaxy ever discovered. As such, any direct comparison that controls for mass is difficult. Therefore, we focus on specific processes related to bar formation and galaxy evolution. The main implications are (1) a bar can fastly grow and survive in a gas-rich disk, but both large-box cosmological and controlled simulations failed to reproduce it; (2) bars can be the engine of submillimeter galaxy. These two points are discussed in two paragraphs on lines 134 and 154.

4. — (page 3) - "Considering the formation timescale of J0107a ~ M_star/SFR ~1 Gyr and the well-developed bar structure embedded in gas, the observation indicates that a classical bar can grow and become mature in early gas-rich disks, as opposed to..." —> This discrepancy between predictions and observations has to be deepened and explained in details. Which are the conditions that in simulations are analysed that cannot hold once this barred galaxy is considered?

Response: The comparison and discussion are separated into a new paragraph starting from line 134. The discrepancy exists for both cosmological and idealized simulations as they cannot reproduce the bar strength and gas fraction at the same time. We argue that feedback and gas feeding may be responsible for the discrepancy, but exactly figuring out what is wrong in simulation is out of the scope of our paper.

5. — (page 3) - "… as opposed to numerical simulations that suggest a high gas fraction makes bar difficult to form and survive because gas strongly responses to bar perturbation in a way counter bar growth." —> This sentence is in clear opposition to the sentence "This is in agreement with numerical simulations that show a high baryon fraction promotes early bar formation". Possible reasons of discrepancies have to be clearly addressed.
Response: the reason was that numerical simulations included both idealized experiments that aim at reproducing features of local barred galaxies and cosmological simulations. The origin of this opposition is the different initial gas fractions in these simulations. In the new paragraph from line 134, we explicitly stated what kind of simulation is being compared.

6. — (page 3) - The authors often refer to numerical simulations, and I think there is some confusion in the comparison between observations and predictions from numerical simulations that has to be clearly addressed throughout the paper.
When comparing with numerical experiments or simulations, relevant details have to be provided: are these works and ensuing conclusions based on theoretical or analytical models? Are results inferred from simulations with idealised set-ups? Are we considering findings obtained from full-physics cosmological, hydrodynamical simulations? Are the initial conditions of the cited simulations comparable with the observed system we are analysing? While I would like to refrain to suggest papers to quote (e.g. Athanassoula et al., 1992; Wada, 1994; Kraljic et al., 2012; Goz et al., 2015; Sormani et al., 2015; Fragkoudi et al., 2017; Rosas-Guevara et al., 2024, just to name a few studies), I would strongly encourage the authors to devote a short section of their manuscript to the comparison with previous numerical works.
I would consider an asset if the authors may also want to consider to include a short paragraph about the comparison with previous observations of barred galaxies across redshift.

Response: We have separated the comparison with simulations into a new paragraph at line 134. For comparison with observations of barred galaxies across redshift, we added references to characteristics of z=0 bars at line 104. Unfortunately, to the best of our knowledge, similar studies do not exist at redshift z>>0.1. Such studies are too difficult to perform at higher redshift, as we stated in the second paragraph.

7. — (page 3) - You write (page 2) that the bar that you observe is "in the formation stage". Can

you be more precise and quantitative in suggesting supporting (observational/dynamical) evidence (besides the below-mentioned “remarkably strong bar perturbation” inferred from the ratio between maximum tangential and radial forces) of this statement?
Response: the evidence for ar strength is provided by Fourier analysis and the PV diagram. We can see that most of the gas follows bar-induced non-circular motion rather than rotation. We rearranged the text at lines 58, and 73. The argument of “in the formation stage” is discussed in line 122, mainly supported by a comparison of gas fraction with local bars.

8. — (page 4) - As for the gas inflow rate, can you provide details about the region within which it is computed? At page 6, you also mention the galaxy circum-galactic medium and the cosmic web: is there a way to quantify the gas inflow rate in different regions/at different distances from the galaxy centre?
Response: the gas inflow rate is computed from high-resolution data within a radial range of 0-13 kpc where [CI](1-0) is detected. The description is added to line 385. The cold gas surrounding the target is only visible in tapered maps, and the S/N is not enough to measure the velocity field. Therefore, we only consider bar-driven gas inflow toward the center inside the disk based on high-resolution data. For gas inflow on larger scales, we proposed ALMA band 4 follow-up observations to obtain velocity and see whether the extended CO(4-3)/[CI](1-0) emission originates from cosmic gas accretion, but the proposal was ranked a filler and not executed.

9. — (page 4) - Please, provide some context to guide the reader on whether nuclear ring dimensions typically consistent with 0.8 kpc are common features for a system of this kind or not.
Response: The typical size of a nuclear ring is <1 kpc in local galaxies. We have added a reference to Erwin, 2024, MNRAS, 528, 3613 at line 79 for information about local rings.

10. — (page 4) - "This result also indicates that the bar can tolerate high gas inflow rate, in contrast to some simulations which suggest it will dissolve under such conditions.” —> In my opinion, once more the author try to draw a general conclusion from one observation and to disprove results from simulations, without being able to explain possible motivations at the origin of the disagreement. This paragraph should be substantially re-phrased, and its content edited.
Response: To address this, we rearranged the text and separated simulation comparisons into a new paragraph at line 134. However, we removed the comparison of inflow rate because such controlled experiments do not match our target in terms of mass.

11. — (page 5 and Figure 3) - In Figure 3, errors on circular and non-circular velocities as a function of radius do not include uncertainties in inclination and position angle: please, add these uncertainties (in an additional panel if needed, for the sake of clarity).
Response: Errors due to inclination are now included, the position angle is still fixed to the best-effort value to avoid degeneracy between bar position angle and non-circular velocity as we believe disk position angle can be more precisely determined from low-resolution data.

12. — (page 6) - You write that “Besides the striking structural and dynamical similarities with local barred spirals, J0107a displays SFR and gas fraction more typical of a massive star-forming galaxy at z ∼ 2.5, the peak of cosmic star formation.”: In my opinion, this statement has to fit and be put in a clearer framework to interpret results. Do we have to think that

observations in this work report an unprecedentedly seen barred galaxy, or do we rather have to imagine a massive disc galaxy at z>2 with a dynamical feature in the innermost region that resembles a low-z bar? The highlighted and discussed scenario should be as quantitative as possible.
Response: the purpose of the sentence itself was to say the galaxy is unprecedentedly seen, but the paragraph is about the importance of bars in submillimeter galaxies. We think this makes the statement unclear and have moved the comparison with other barred galaxies to a new paragraph at line 104. We will focus on "a massive disc galaxy at z>2 with a dynamical feature in the innermost region that resembles a low-z bar with unprecedentedly seen gas inflow rate", also including some reference to local bars as suggested by comment #6.

13. — (page 10, Figure 3) - In several panels of Figure 3, units of measurement are not correctly reported: for instance, you write in the y-label of panel (f) "V / km s^{-1}", while this should be "V (km s^{-1})". Please, carefully check all the figure labels.
Response: the labels have been corrected now.

14. — (page 13) - You mention "partially failed observations". Can you elaborate on them? How does including them in your analysis impact the quality of the results?

Response: the situation was a bit complicated. We originally requested observations in C-7 and C-4 arrays, but the C-7 part was executed in C-8 due to the ALMA transporter issue in late 2023. More unfortunately, the C-8 data failed to pass ALMA Quality Assurance 2. The reason was not clear, but it could be due to our aggressive spectral setting that squeezed HNC(5-4), CO(4-3) and [CI](1-0) into a single correlator tuning, with new 4-bit correlator mode for the three spectral windows containing emission lines to reduce integration time and a conventional 2-bit mode spectral window to receive continuum. The result was that all the 4-bit mode spectral line data was corrupted due to correlator issues, but the observatory did not explain what had happened to the correlator. Then the target was re-observed in full 2-bit mode, and we combined re-observations with data from failed observation, discarding all corrupted 4-bit mode data. This is why the continuum map has only 1/4 of bandwidth compared to normal observations. We have confirmed that the dust lane feature remains consistent with or without the data combination.

13. — (page 15) - "No companion galaxy is detected in the cube or continuum image" —> Please, state clearly (or repeat) the distance (in physical kpc, from the target centre) within which no galaxy companion is detected.

Response: we have described the field of view in line 252. The diameter for 50% sensitivity is 330 physical kpc.

15. — (page 18) - As for the conversion factor between molecular mass and CO luminosity, while I am happy with the reference value [38] that you assume and I appreciate the substantial agreement with values inferred from dust continuum and another conversion factor [39], I think it would be beneficial to further discuss the inferred molecular gas mass, by also considering e.g. conversion functions (see e.g Tacconi et al., 2020) instead of conversion factors only (which can be more sensitive to the assumed galaxy physical parameters).
Response: We have replaced a single conversion factor with that calculated from scaling relations of general star-forming galaxies (Tacconi et al. 2018, ApJ, 853, 17) and propagated M_star uncertainty and scatter of the scaling relations into M_mol error (line 285). The resulting M_mol from CO(1-0) is slightly higher but still agrees within error. The value from CO(1-0) is

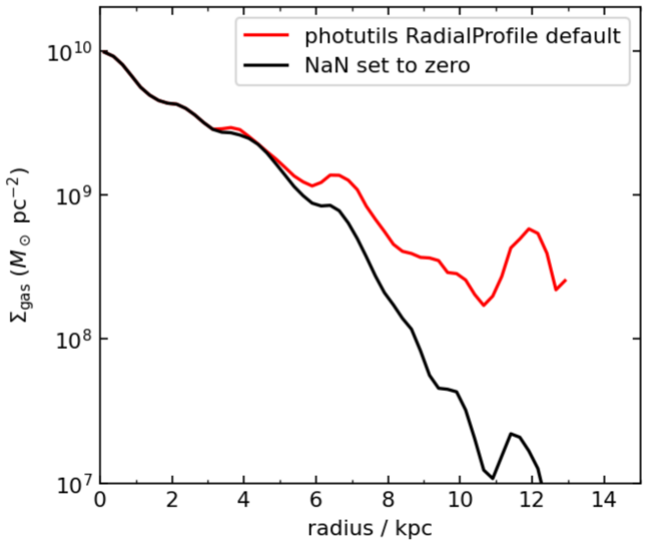
identical to that from [CI](1-0). This is not surprising because J0107a has CO(1-0)/[CI](1-0) ratio that happens to be the median of SMGs (Frias Castillo et al., 2024, arxiv: 2404.05596).

16. — (page 18) - Please, state the distance between J0107b and J0107a.
Response: the distance between J0107a and J0107b is 26 arcsec in the sky. We added this information to line 291.

17. — (page 19) - Can you please elaborate more on the assumption that "the bar influence becomes smaller at large radii", as for the derived parameters of the spatial configuration of the galaxy disc?
Response: we added a reference to Extended Data Figure 6 at line 318. The figure shows that the tangential force from the bar diminishes fast beyond the bar semi-major axis length.

18. — (page 21, somewhat related to pt. 8.) - Is the net inflow rate of 1100 M_Sun/yr retrieved by assuming R=disc scale length? Please clarify.
Response: we have added a description of the integral range at line 385. The gas inflow rate is calculated by assuming R=13 kpc, but there is little gas beyond the bar semi-major axis length, and 93% of the inflow rate is contributed by the inner 8 kpc. This is consistent with the argument that bar influence diminishes outside semi-major axis length.

19. — (page 23) - I think the paper would benefit from a critical discussion on the impact that the uncertainty on the galaxy disc inclination may have on all the results and conclusions.
Response: we have added a discussion on inclination uncertainty at line 341. We also examined the script and corrected an error in the gas mass surface density profile calculation (the following figure). When calculating the radial profile of mass surface density, regions without [CI](1-0) detection should be set to zero, but by default, they are neglected by astropy/photutils so not averaged for full azimuthal range, resulting in overestimated \Sigma_gas(r) (red line). Now this error is fixed (black line). The uncertainties in inclination, stellar mass, molecular gas mass and velocity measurements are propagated to the gas inflow rate using Monte Carlo simulation, described in line 386. The resulting gas inflow rate is smaller than the previous calculation, but still much higher (>10x) than local galaxies and agrees well with the SFR.



20. — (page 23) - "This is in agreement with numerical simulations that show a high baryon

fraction promotes early bar formation [46].” —> Please, also cite the recent paper by Verwilghen et al., 2024 (A&A, 687, A53).
Response: the paper is cited now, and some discussion about this is added to the main text at line 137.
Overall, I found the paper original, the data of very good quality and the methodology clearly explained; also, the manuscript is clearly written. More credit could be given to previous work, the validity of the interpretation of the results has to be strengthened, and the robustness of the conclusions has to be improved.

Response: changes in the above responses are highlighted with magenta color in the manuscript.

Referees' comments:

Referee #1 (Remarks to the Author):

I appreciate the authors for addressing all my concerns and for incorporating the discussion of their findings with the expectations for galaxy formation models. This includes state-of-the-art cosmological hydrodynamic simulations and numerical simulations. The authors also provide constraints for the galaxy formation models. The clarity in the description of the results and the methodology, particularly regarding the errors, has been improved. I have no further concerns at this time, except that in the discussion of simulations on page 7, line 140, the authors cite Rosas-Guevara et al. 2020. However, it should be cited as Rosas-Guevara et al. 2022, as the authors mentioned using Figure 7 from Rosas-Guevara et al. 2022 in their response.
Response: we thank the referee for the positive evaluation and constructive comments. We have corrected the erroneous citation.

Referee #2 (Remarks to the Author):

Well done. Thank you for thorough response to my questions. I'm happy to accept this article.
Response: we thank the referee for the positive evaluation.

Referee #3 (Remarks to the Author):

I thank the authors for having addressed the issues raised in my previous report and for agreeing to modifying the manuscript accordingly. Overall, I am happy with the clarity and content of the current, revised version of the manuscript.

As a moderate/minor remark, I would like to point out that the comparison with theoretical models and state-of-the-art simulations is somehow not as advanced as the remaining part of the paper, and appears to be little conclusive. The authors explicitly write that the complexity of the comparison "calls for theoretical follow-up": in my opinion, if some details of the considered simulation set-ups to compare with were analysed, along with limitations (of both numerical experiments and the current observational counterpart), the comparison would have been way more informative.
Response: we thank the referee for the positive evaluation and the comment on comparison with simulations. We have changed the last sentence of the comparison with simulations to explicitly inform theoretical models at high gas fraction limit.