Peer Review File

Manuscript Title: A very luminous jet from the disruption of a star by a massive black hole

Reviewer Comments & Author Rebuttals

Reviewer Reports on the Initial Version:

Referees' comments:

Referee #1 (Remarks to the Author):

This article presents a rich set of early-time, multi-wavelength observations of a newly-discovered optical transient, which the authors argue convincingly to arise from a relativistic jet launched from a tidal disruption event. This is a rare class of high-energy transients, with only four previous examples having been detected over the past decade. Only one of those had similarly comprehensive, early-time monitoring data, particularly at radio and millimeter frequencies.

With such well-sampled observational data, in both the time and frequency domains, this is an important observational data set that will have great value for the community. As stated in the manuscript, it is unclear why only a small fraction of tidal disruption events produce relativistic jets. Improving the sample of such events with high-quality observational data is an important step to addressing this question.

Nonetheless, while the observational data and the inferred physical parameters of the event are undisputedly important, it was not clear to me what key new insights had been gained from these data into the question of how and why relativistic jets are launched from tidal disruption events. The authors have performed a rate estimate from the optical band, which was in good agreement with the previously-estimated rates from gamma-ray and infrared-selected events. It is certainly reassuring to see that the rates agree, and this would argue against strong selection effects impacting certain bands. However, this did not seem to provide a significant step forward in our understanding.

In terms of novelty, the physical scenario put forward to explain the observations (lines 108-144) seems relatively consistent with what has previously been inferred for other tidal disruption events (at least as far as the accretion flow is concerned) or gamma-ray bursts (for the jet), as can be seen from the references. It did not seem to rely in any sense on the specific observations presented here. Indeed, I would have been interested to see the evidence for the early formation of an accretion disk, given that the X-ray emission was attributed to internal dissipation in the jets. The authors have derived a range of relevant physical parameters for the event (with details in the Methods section), but these were not compared in detail to those inferred for the other relativistic tidal disruption events (at least to events such as Swift J1644+57, for which similar data are available). That could have provided some helpful context, given insight into any novel features of this particular event, and identified key characteristics common to the observed relativistic tidal disruption events that might have helped to address the question of why these are so rare.

One novel aspect of this work is the detection of a relativistic tidal disruption event in the optical band, rather than in the gamma-ray or infrared bands. This is testament to the power of the current generation of optical surveys and the potential of the next generation (particularly the Vera Rubin Observatory). However, while impressive and exciting, this strikes me as more an instrumental and technical achievement than an advance in our scientific understanding of a particular class of astronomical objects. Unless the authors view this as the key scientific advance (when a stronger case would likely be required for why this is such an advance), I might suggest refocusing the start of Section 8 (in the Supplementary Information), or at least making it more concise and relevant to the main arguments put forward in the paper.

While I am convinced by the authors' arguments that this event is likely a tidal disruption event, I think it would be important to note the caveat that some of the comparison samples (particularly the fast blue optical transients [FBOTs] and the relativistic tidal disruption events) are relatively small (the main points of comparison being AT2018cow, and Sw J1644+57, respectively), and we may not have sampled the full region of parameter space available to each class. That said, the discrepancy in luminosity between AT2022cmc and the FBOTs is such that it seems unlikely that AT2022cmc was drawn from that class of objects, and I would agree that the tidal disruption event interpretation does seem most plausible.

As the authors have noted, one important piece of information that would lend further confidence to the tidal disruption event classification is the position of the transient. The authors provide both optical and radio positions (Sections 8.0.3 and 8.0.4), but these appear to be discrepant by 0.0166 arcsec in R.A. and 0.0085 arcsec in Dec. (5.5 and 2.8 sigma, respectively, given the stated VLA astrometric uncertainty of 0.003 arcsec - no optical positional uncertainty was given). I suspect that the radio uncertainty is limited not by the low statistical uncertainties quoted, but by the systematics. The NRAO webpages suggest that the typical positional accuracy of a VLA observation is limited to ~10% of a synthesized beamwidth (0.013 arcsec in the case of the A-configuration, Ku-band observations used here). Furthermore, given the phase decorrelation noted by the authors at higher frequencies, the observing conditions are likely to have been sufficiently far from ideal that even this level of absolute positional accuracy may not have been reached. The authors should consider this point, since it will be important should the future Hubble Space Telescope observations that they mention be able to detect the host galaxy.

A few more minor points that the authors might wish to address:
- The first paragraph seems to contain very few references. More would help to provide appropriate context for the work.
- In Figure 1 (and elsewhere), the authors mention that magnitudes have been corrected for Galactic extinction. They should note the extinction value (Av) that was used, to aid reproducibility.
- In Figure 3, the significance of the orange lines should be noted.
- In Figure 5, the significance of the grey shading should be noted.
- It seems odd to separate out the GMRT observations into Supplementary Information Table 2, whereas all other radio and millimeter observations and limits were reported in Supplementary Information Table 4. I would recommend concatenating these two tables.
- In Section 1 of the Methods, it would be good to provide a reference (or references) from which much of the standard physics was taken, since it did not seem that these equations had been

derived for the first time in this work.
- In Section 3 of the Methods, the authors suggest that this event could have occurred in a similar environment to that of a GRB. Since a TDE will occur at the center of its host galaxy, it was not clear to me that the environment in a galactic nucleus would be similar to that a few kpc from the center of a star-forming galaxy. The authors might wish to comment further on this point.
- In Section 5 of the Methods, uncertainties on the rates were not given. Such low numbers of events would be subject to Poissonian fluctuations, and it was not clear whether these had been taken into account in determining the likely rates.
- In Section 6 of the Methods, the quoted fade rates do not have units of 1/time, and I suggest that the authors address this.
- In Section 7 of the Methods, insufficient detail is given as to how the Bayesian modelling was conducted. Neither priors nor posteriors were provided, and the values and uncertainties were not fully characterised (are these medians and Bayesian credible intervals?). Furthermore, the authors should consider the appropriate number of significant figures to quote for their values and uncertainties.
- In Section 8.0.4, the authors should specify central frequencies as well as (or instead of) instrument-specific band names, to aid readability.
- In Section 8.0.4, when discussing phase decorrelation, are the authors able to quantify the magnitude of the decorrelation at the different frequencies (i.e. how much the measured flux density changed following the self-calibration)?
- In Section 8.0.15, the authors might wish to specify the wavelength ranges of the "orange" and "cyan" light curves from the ATLAS survey.
- The authors should conduct a thorough spelling and grammatical check; I found a number of typographical errors.

Referee #2 (Remarks to the Author):

Andreoni & Coughlin et al. present the discovery of a rare astrophysical transient, AT2022cmc. As outlined in the paper, multi-wavelength observations of AT2022cmc (in the radio, mm/sub-mm, infrared, optical, UV, and X-rays) are consistent with emission from a newly launched, extremely energetic relativistic jet interacting with a dense medium. They argue that the central engine powering this jet is most likely a supermassive black hole in a distant galaxy, being fed by the stellar debris from a star that was tidally disrupted. While a few TDEs powering relativistic jets have been previously observed, these events are extremely rare; there is only one other well-studied similar event (Sw J1644+57), and that event was discovered in 2011 (Bloom et al 2011; Burrows et al 2011; Levan et al 2011; Zauderer et al 2011). AT2022cmc is also the first relativistic jetted TDE to be discovered by an optical survey (Sw J1644+57 was initially identified based on its gamma-ray emission), enabling an independent estimate of the jetted TDE rate. Consistent with previous work, the authors find that jetted TDEs similar to AT2022cmc and Sw J1644+57 make up at most a few percent of the total TDE rate. The rarity of jets in TDEs remains an unexplained puzzle in the field, so the high-quality data presented here are likely to provide interesting constraints for models of TDE-induced accretion.

Apart from their rarity, jetted TDEs are of interest to multiple communities within astrophysics, as they may shed light on the poorly-understood physics of super-Eddington accretion onto supermassive black holes, and the general physics governing the production of relativistic jets (which appear in a range of astrophysical systems, from AGN to gamma-ray bursts to X-ray binaries). While this work includes a "broad-brush" description of a multi-component emission model capable of explaining the multi-wavelength properties of AT2022cmc (Figure 4), the authors do not comment in detail on the implications of their results for these big-picture questions. While the discovery method is novel and AT2022cmc's rapid discovery is a triumph of new automated techniques for identifying rare, interesting transients in real time in large datasets, it will likely still take years or decades to build a statistical sample of these rare events. In the absence of a large event sample, it is not clear how AT2022cmc itself extends our knowledge of jetted TDEs beyond what we had already learned from Sw J1644+57. If the paper is revised, the authors may wish to further emphasize what (if any) new conclusions we can already draw about the physics underlying this rare class of transient from the data presented here.

The paper provides a thorough overview of the observations that were conducted (including the process by which AT2022cmc was flagged as an interesting transient in the ZTF data stream), generally sufficient detail of the data processing to reproduce the authors' analysis if desired, and an adequate description of the rates calculations for similar events in mm and optical surveys. I do not have any major concerns with these portions of the paper, and the comprehensive multi-wavelength dataset presented here will undoubtably be of great interest to the community. I do however have some questions on the modeling, as well as some more minor comments, which I list below.

Line 78: Some more recent papers on FBOTs should probably also be cited here, particularly because the light curves for some events are currently included in Figure 2 without attribution.

Line 98: It would be helpful to explicitly state that Sw J1644+57 is the prototypical jetted TDE, and the only previous well-studied event of this class.

Line 115: This may refer to an earlier version of SI Figure 2, or to Figure 2a in the main text? The "high X-ray luminosity and flux variability" are not clear from the current SI Figure 2, which shows only the (consistent with constant) evolution of the power-law photon index of the XRT data. A citation is also needed for the quoted X-ray variability timescale of ~hr.

Line 156: Note that the jetted TDE rate quoted in reference 29 is based on only two of the 4 jetted TDEs mentioned here. The source for this rate is also inconsistently cited throughout the paper; in line 648-649 reference 1 is cited instead (the rate derived in reference 1 is very similar, but they use all 3 jetted Swift TDEs for their rate calculation).

Figure 2: The original sources for the data on other transients in these figures are not cited. The nomenclature is also inconsistent between panels (e.g. "AT2018cow-like versus FBOT) and some acronyms are not defined, which may be confusing for the non-expert (e.g. LLGRB = low-luminosity gamma-ray burst).

Figure 2 panel (b): Could the x-axis be updated to show rest-frame days rather than days elapsed in the observer frame, for consistency with panels (a) and (c)? The transients shown are at a very broad range of redshifts, so this is a more physically meaningful comparison. (It may also be useful to indicate the frequency of each light curve in the rest-frame.)

Methods Section 1 (see also lines 116 and 130 in the main text): Some of the calculations in this section are missing relevant correction factors for the redshift. As AT2022cmc is at z=1.19, these correction factors can become significant for some derived parameters (see e.g. Barniol Duran et al 2013). For example, as the variability timescale of 1 day appears to refer to a time difference measured in the observer frame, the calculation of the variability timescale in line 550 is missing a factor of (1+z).

Lines 526-527: This sentence is confusing; do the authors intend to imply that radio emission is expected from the self-crossing shock and the accretion disk as well as the jet?

As mentioned in Methods Section 1 (lines 555-557), the synchrotron models applied to the radio afterglow are subject to a number of parameter degeneracies. Although I understand that this is a discovery paper and the authors may wish to leave detailed modeling and a complete exploration of parameter space to future work, some more discussion of the allowed parameter ranges may be warranted. In the example model discussed here, several parameters are fixed to "fiducial" values with little to no justification. For example, in line 528 the authors state that an isotropic-equivalent jet energy of 1e53-1e54 erg is "implied by the X-ray emission", but no explicit derivation of the energy from the X-ray luminosity is included in the paper, so it is difficult to evaluate what assumptions underlie this statement. It is stated that parameters including epsilon_B are uncertain "to an order of magnitude", however this may underestimate the true uncertainties; for example, the range of epsilon_B values observed in relativistic transients spans at least 2e-3 to 0.33 (e.g. Laskar et al 2015; Hajela et al 2022).

Methods Section 4: I am curious how these constraints on the host galaxy compare to what is known about the host galaxies of other jetted TDEs, e.g. Sw J1644+57. Would the host galaxies of other known jetted TDEs also have evaded detection at z=1.19? Are archival upper limits at other wavelengths at all constraining? (For example, can archival radio and/or X-ray data place any limits on pre-existing AGN activity?)

Lines 635-636: Based on the recent slow evolution of the optical/UV light curves seen in Figure 1, it seems unlikely that the transient will have faded below HST's detection limit "in a few months" -- wouldn't observations at even later times be better?

Line 638: should read "rate of AT2022cmc-like transients"

Line 675: clarify that this is "optical light curve modeling" (or specify what range of wavelengths is fit to this model).

Line 896: The table referenced here is missing from the manuscript.

Referee #3 (Remarks to the Author):

In this work, the authors have observed this astronomical transient event at high redshift over the multiple wavelengths, compared several transient event candidates, and concluded that it is a tidal disruption event accompanied by an on-axis relativistic jet (i.e., Jetted TDEs). There have been three Jetted-TDE candidates so far. However, this is an extremely bright optical jetted-TDE that we have observed for the first time. The authors have also given an interesting interpretation of the lightcurve variation, which is composed of a peak and subsequent flat time evolution, from the observational points of view. The work contained in this paper is very interesting and impressive enough to attract the general audience, and the key results are well summarized. Thus, it is worth publishing this paper in Nature. However, the authors need to address a series of issues for further improvement of the manuscript and clarification of their results before I consider recommending it for publication.

1.
The authors gave an observational upper limit (~a few 10^8 solar masses) for the black hole mass by using the Eddington luminosity and M-sigma relation. On the other hand, there is a theoretical mass limit called Hill's mass for TDEs. This limit provides that only an SMBH with a mass smaller than 10^8 solar masses can cause TDEs for the non-rotating SMBH case (e.g., Kesden 2012). the black hole spin makes the SMBH mass more massive. I suggest the authors should add the discussion of the Hill's mass.

2.
Hill's mass argument tells us that the SMBH of this jetted TDE is likely to be spinning with a relatively high value in a prograde direction. The Jet's power inferred from the Blandford-Znajek mechanism is consistent with the X-ray observations? This could be related to the discussion about why the event rate of the Jetted TDEs that the authors have confirmed is so rare.

3.
It is interesting that the luminosity of IR radiation is as large as that of optical/UV and also the peak position is almost the same as seen in Figure 1. What is the origin of this IR emission? Is it re-emitted from dust by which the X-ray/UV radiation is absorbed? It would be better to explain the origin of IR emission.

4.
Whether this event occurred around the center of the nuclear region is important information for comparison purposes with other transient event candidates. It would be better to give a detailed description of the location information and future observational plans.

5.
The authors have stated that the AT2022cmc light curves are red at the early peak and become bluer as time goes by. By taking into account Figure 4, this means that photons from the matter moving at a relatively slow speed toward the earth were detected first, and then photons emitted from objects moving at a faster speed are detected at a later time. Is this interpretation correct? I am a little bit confused about it. In any case, the authors need to clarify why the early peaks are red and the later plateau becomes bluer, and show it in Figure 1 clearly.

6.
The authors mentioned (in lines 120-133) that the optical and mm radio origins are the same (i.e., from the reverse shock). Why is there a 5-day delay between these lightcurve peaks as seen in Figures 1 and 2(b)?

7.
If the outflow comes from a self-crossing shock, then the outflow velocity can be estimated by equating the gravitational energy and the kinetic energy. I suggest the authors should check whether such a theoretical value is consistent with the outflow velocity used here (10^9 cm/s).

8.
A relativistic jet would be emitted only after the disk is formed and the mass accretes to the SMBH. It takes time, i.e., circularization time, for the disk to form after the outflow is blown by the self-crossing shock. If the circularization time is longer than 10 days (it is theoretically considered to be much longer than 10 days, though [e.g., see Bonnerot et al. 2017]), the radiation from the photosphere should arrive earlier than the radiation from the Jet. This differs from the view that the authors proposed. How do you resolve this inconsistency? For TDEs (of course for this paper, too), the timescale of debris circularization (energy dissipation) is an important topic. I suggest the authors should discuss this in the main and/or supplementary text.

9.
I agree with the event rate estimation of on-axis Jetted TDEs. Do the authors have any suggestions (from their observation [I know they assumed the jet direction is along the line of sight in this paper, though]) about the rate estimation of off-axis jetted TDEs, e.g., whether the expected event rate of off-axis jetted TDEs could be comparable to that of non-Jetted TDEs? This argument is important for why only a very small fraction of TDEs launch relativistic jets.

Author Rebuttals to Initial Comments:

Referee 1

This article presents a rich set of early-time, multi-wavelength observations of a newly-discovered optical transient, which the authors argue convincingly to arise from a relativistic jet launched from a tidal disruption event. This is a rare class of high-energy transients, with only four previous examples having been detected over the past decade. Only one of those had similarly comprehensive, early-time monitoring data, particularly at radio and millimeter frequencies.

With such well-sampled observational data, in both the time and frequency domains, this is an important observational data set that will have great value for the community. As stated in the manuscript, it is unclear why only a small fraction of tidal disruption events produce relativistic jets. Improving the sample of such events with high-quality observational data is an important step to addressing this question.

We thank the referee for the kind comments.

Nonetheless, while the observational data and the inferred physical parameters of the event are undisputedly important, it was not clear to me what key new insights had been gained from these data into the question of how and why relativistic jets are launched from tidal disruption events. The authors have performed a rate estimate from the optical band, which was in good agreement with the previously-estimated rates from gamma-ray and infrared-selected events. It is certainly reassuring to see that the rates agree, and this would argue against strong selection effects impacting certain bands. However, this did not seem to provide a significant step forward in our understanding.

We have now changed the text to present more clearly the importance of this discovery.

The discovery of AT2022cmc presented us with a prototype for a new optical transient class. The (previously) best observed jetted TDE, Sw J1644+57, did not reveal an optically bright transient, probably due to extinction. Our rate analysis suggested that the majority of jetted TDEs may not be heavily affected by extinction. The discovery will prompt the search for a population of AT2022cmc-like transients in new survey data, as well as in existent data sets. In fact, bright optical transients such as AT2022cmc (and the class of luminous featureless TDEs, please see below) are much more accessible by current/near-future telescopes than the long-duration, faint gamma-ray transients (the cases of previous jetted TDEs) which barely met the triggering criteria of Swift.

Here we make a comparison with a couple of other well-known cases. Theoretical predictions and a few photometric datasets existed before the discovery of transients such as the GW170817 kilonova or AT2018cow, but their rich multi-wavelength observations have prompted dozens of projects searching for analogs in new and archival data. We expect the publication of AT2022cmc to have a similar impact, especially if the connection between TDEs with relativistic outflows and IceCube neutrinos is further established (see, e.g., Murase et al., 2021; Stein et al., 2021; VanVelzen et al., 2022). This is particularly important now that we are less than two years away from the start of Rubin Observatory operations and several time-domain surveys are in full swing, so that AT2022cmc-like transients can be actively searched for in real time and followed-up with other facilities.

During the paper revision process, another exciting result emerged. A class of luminous, featureless TDEs has recently been identified by Hammerstein et al. (2021). Unlike most TDEs, the featureless class do not show any broad lines in their optical spectra and are typically brighter in UV/optical/IR. There is currently no explanation on the origin of featureless TDEs. However, the blue & slow-evolving component of AT2022cmc, which dominates from ~4 days (rest frame) from discovery, shares remarkable similarities with luminous featureless TDEs. Thanks to our photometric and spectroscopic dataset, we can therefore argue, for the first time, that there is a connection between jetted TDEs and luminous featureless TDEs. This connection will provide a pathway to better understand the geometry and mechanisms of TDEs that are jetted and/or have mildly relativistic outflows.

In terms of novelty, the physical scenario put forward to explain the observations (lines 108-144) seems relatively consistent with what has previously been inferred for other tidal disruption events (at least as far as the accretion flow is concerned) or gamma-ray bursts (for the jet), as can be seen from the references. It did not seem to rely in any sense on the specific observations presented here. Indeed, I would have been interested to see the evidence for the early formation of an accretion disk, given that the X-ray emission was attributed to internal dissipation in the jets. The authors have derived a range of relevant physical parameters for the event (with details in the Methods section), but these were not compared in detail to those inferred for the other relativistic tidal disruption events (at least to events such as Swift J1644+57, for which similar data are available). That could have provided some helpful context, given insight into any novel features of this
particular event, and identified key characteristics common to the observed relativistic tidal disruption events that might have helped to address the question of why these are so rare.

Thank you for the feedback and for suggesting a path to better address it. A more direct comparison with Sw J1644+57 is now present in the text, especially in the form of new Table 1. The table presents observational properties as well as inferred quantities.

Indeed, the relative timing between the X-ray and optical lightcurves provides an important constraint on the disk formation time. Such a constraint is typically difficult to obtain in TDEs that only show optical emission (since the optical emission does not come directly from the accretion disk). From a theoretical point of view, there are large uncertainties in the current understanding of the hydrodynamics of the fallback stream and the disk formation process. In the revised paper, we have added a sentence in the Main Text: “Under the TDE interpretation, since the jet is already on-going when the blue UV component is observed, this suggests that the disk formation occurs on a timescale shorter than the evolutionary time of the blue UV component, which is of the order of weeks in the rest frame. This provides important constraints on the highly uncertain hydrodynamics of the disk formation process (Bonnerot et al., 2021)”

One novel aspect of this work is the detection of a relativistic tidal disruption event in the optical band, rather than in the gamma-ray or infrared bands. This is testament to the power of the current generation of optical surveys and the potential of the next generation (particularly the Vera Rubin Observatory). However, while impressive and exciting, this strikes me as more an instrumental and technical achievement than an advance in our scientific understanding of a particular class of astronomical objects. Unless the authors view this as the key scientific advance (when a stronger case would likely be required for why this is such an advance), I might suggest refocusing the start of Section 8 (in the Supplementary Information), or at least making it more concise and relevant to the main arguments put forward in the paper.

We thank the referee for appreciating the technical achievement of this project and modern optical surveys in general. Regarding the scientific relevance of the discovery, we ask the referee to please refer to our answer to the first comment of this report. In addition, we tightened up the beginning of Section 8 as the referee suggested.

While I am convinced by the authors' arguments that this event is likely a tidal disruption event, I think it would be important to note the caveat that some of the comparison samples (particularly the fast blue optical transients [FBOTs] and the relativistic tidal disruption events) are relatively small (the main points of comparison being AT2018cow, and Sw J1644+57, respectively), and we may not have sampled the full region of parameter space available to each class. That said, the discrepancy in luminosity between AT2022cmc and the FBOTs is such that it seems unlikely that AT2022cmc was drawn from that class of objects, and I would agree that the tidal disruption event interpretation does seem most plausible.

In the main text, we added the following sentence to reflect the referee’s suggestion: “Although astronomical surveys may not have sampled the full region of parameter space available to each class, we exclude an association between AT2022cmc and these transient classes.”

As the authors have noted, one important piece of information that would lend further confidence to the tidal disruption event classification is the position of the transient. The authors provide both optical and radio positions (Sections 8.0.3 and 8.0.4), but these appear to be discrepant by 0.0166 arcsec in R.A. and 0.0085 arcsec in Dec. (5.5 and 2.8 sigma, respectively, given the stated VLA astrometric uncertainty of 0.003 arcsec - no optical positional uncertainty was given). I suspect that the radio uncertainty is limited not by the low statistical uncertainties quoted, but by the systematics. The NRAO webpages suggest that the typical positional accuracy of a VLA observation is limited to ~10% of a synthesized beamwidth (0.013 arcsec in the case of the A-configuration, Ku-band observations used here). Furthermore, given the phase decorrelation noted by the authors at higher frequencies, the observing conditions are likely to have been sufficiently far from ideal that even this level of absolute positional accuracy may not have been reached. The authors should consider this point, since it will be important should the future Hubble Space Telescope observations that they mention be able to detect the host galaxy.

The uncertainty in the radio-optical comparison is dominated by the optical observations: the pixel scale of WFC3 is 0.04 arcsec and with only a few dithers available the positional accuracy is likely to only be a factor of a few better than this**.** To better assess the true astrometric accuracy of the VLA observations we have compared the position of the source across a range of different observations (frequencies, configurations, elevations, etc.) and found the errors as estimated from AIPS to be somewhat underestimated, but only by a factor of ~2 (as determined by a simple chi^2 calculation). We have conservatively doubled our quoted uncertainty to 0.006 arcsec to account for this. There will also be a systematic component associated with the uncertainty in the position of 3C286 itself, which should be subdominant in VLA observations.

A few more minor points that the authors might wish to address:

- The first paragraph seems to contain very few references. More would help to provide appropriate context for the work.

We added nine citations to the first paragraph (abstract).

- In Figure 1 (and elsewhere), the authors mention that magnitudes have been corrected for Galactic extinction. They should note the extinction value (Av) that was used, to aid reproducibility.

The value for the extinction is now reported.

- In Figure 3, the significance of the orange lines should be noted.

We measured two different sets of estimates. Could the referee please clarify his/her request, so that we can pick the appropriate answer to include in the manuscript?

We first compute the SNR for faint lines, as it is the case of AT2022cmc, using the general CCD formula and using the 2D count spectrum. The general CCD equation, which can be found in literature (Newberry (1991), Gullixson (1992)) gives the SNR as the ratio of the number of photons detected in the 2D region of the line, divided by a term that includes several noise sources (poissonian error on the detected photons, continuum, read-out noise and dark). We have not considered the last two terms given that these values are negligible for VLT and X-shooter. This method gives the SNR of the absorption lines, reported in the table below.

Second, we estimated how reliable is the detection wrt the spectral continuum, instead of the background continuum (e.g. the sky). Since the spectral continuum is brighter than the absorption line, we have estimated how much, in terms of sigma values, the absorption line fluxes deviates from the local spectral continuum. We have used for this estimate the value of the average and the standard deviation (that quantifies the level of fluctuations in the spectral continuum) of the very local continuum. The results are also reported in a table below.

Line (nm)	SNR	sigma
Fe II 258	9.1	2.50
Fe II 260	12.8	2.31
Mg II 279	17.8	3.64
Mg II 280	6.1	2.43
Ca II 393	18.1	3.47
Ca II 397	20.3	2.21

- In Figure 5, the significance of the grey shading should be noted.

We now explain in the caption: “The shaded features are those for which we cannot provide reliable measurements because they fall outside the spectral range of our data, or because they are in a region of the spectrum affected by a very low signal to noise ratio or by telluric features.”

- It seems odd to separate out the GMRT observations into Supplementary Information Table 2, whereas all other radio and millimeter observations and limits were reported in Supplementary Information Table 4. I would recommend concatenating these two tables.

The tables are now merged.

- In Section 1 of the Methods, it would be good to provide a reference (or references) from which much of the standard physics was taken, since it did not seem that these equations had been derived for the first time in this work.

At the beginning of Section 1 of Methods, a reference has now been added to Kumar & Zhang (2015), which provides a comprehensive review of the progress in the GRB field including extensive discussion on the afterglow modeling.

- In Section 3 of the Methods, the authors suggest that this event could have occurred in a similar environment to that of a GRB. Since a TDE will occur at the center of its host galaxy, it was not clear to me that the environment in a galactic nucleus would be similar to that a few kpc from the center of a star-forming galaxy. The authors might wish to comment further on this point.

We added the following text to Methods Sec. 3 to explain this point in a clearer way: "GRBs are typically found to be located well within star-forming host galaxies, and their spectra probe light paths from deep within their hosts. The spectral features imprinted in GRB afterglow spectra have been found to be at hundreds of parsecs or even kpcs from the GRB, so they are probing the overall material in the host galaxy and not necessarily their very local environment. This is similar to what one would expect from the path probed by a jet emitted from the core of an active and similarly star-forming galaxy, but is in contrast to what one sees in the spectra of damped Lyman alpha (DLA) absorbers in the line of sight of quasars, which probe the outskirts of intervening galaxies and show much weaker features."

In Section 5 of the Methods, uncertainties on the rates were not given. Such low numbers of events would be subject to Poissonian fluctuations, and it was not clear whether these had been taken into account in determining the likely rates.

This section has been updated to reflect this comment.

In Section 6 of the Methods, the quoted fade rates do not have units of 1/time, and I suggest that the authors address this.

This has been fixed.

In Section 7 of the Methods, insufficient detail is given as to how the Bayesian modelling was conducted. Neither priors nor posteriors were provided, and the values and uncertainties were not fully characterised (are these medians and Bayesian credible intervals?). Furthermore, the authors should consider the appropriate number of significant figures to quote for their values and uncertainties.

As requested, we now provide details about the prior and posteriors, including the requested figure.

In Section 8.0.4, the authors should specify central frequencies as well as (or instead of) instrument-specific band names, to aid readability.

We have given (in brackets) the receiver frequency ranges in the text. The ranges are more useful than the central frequencies since in almost all cases we have sufficiently high S/N to subdivide into multiple sub-bands.

In Section 8.0.4, when discussing phase decorrelation, are the authors able to quantify the magnitude of the decorrelation at the different frequencies (i.e. how much the measured flux density changed following the self-calibration)?

The change is typically ~25% at 35 GHz.

In Section 8.0.15, the authors might wish to specify the wavelength ranges of the "orange" and "cyan" light curves from the ATLAS survey.

The wavelength ranges are now specified and the sentence reads: “We obtained broad-band ``orange'' (5560A--8260A) and ``cyan'' (4120A--6570A) light curves from the ATLAS survey.”

- The authors should conduct a thorough spelling and grammatical check; I found a number of typographical errors.

A thorough spelling and grammatical check was performed by native-speaker co-authors. Such edits were not always highlighted in boldface in the attached PDF in favor of readability.

Referee 2

Andreoni & Coughlin et al. present the discovery of a rare astrophysical transient, AT2022cmc. As outlined in the paper, multi-wavelength observations of AT2022cmc (in the radio, mm/sub-mm, infrared, optical, UV, and X-rays) are consistent with emission from a newly launched, extremely energetic relativistic jet interacting with a dense medium. They argue that the central engine powering this jet is most likely a supermassive black hole in a distant galaxy, being fed by the stellar debris from a star that was tidally disrupted. While a few TDEs powering relativistic jets have been previously observed, these events are extremely rare; there is only one other well-studied similar event (Sw J1644+57), and that event was discovered in 2011 (Bloom et al 2011; Burrows et al 2011; Levan et al 2011; Zauderer et al 2011). AT2022cmc is also the first relativistic jetted TDE to be discovered by an optical survey (Sw J1644+57 was initially identified based on its gamma-ray emission), enabling an independent estimate of the jetted TDE rate. Consistent with previous work, the authors find that jetted TDEs similar to AT2022cmc and Sw J1644+57 make up at most a few percent of the total TDE rate. The rarity of jets in TDEs remains an unexplained puzzle in the field, so the high-quality data presented here are likely to provide interesting constraints for models of TDE-induced accretion.

Apart from their rarity, jetted TDEs are of interest to multiple communities within astrophysics, as they may shed light on the poorly-understood physics of super-Eddington accretion onto supermassive black holes, and the general physics governing the production of relativistic jets (which appear in a range of astrophysical systems, from AGN to gamma-ray bursts to X-ray binaries). While this work includes a "broad-brush" description of a multi-component emission model capable of explaining the multi-wavelength properties of AT2022cmc (Figure 4), the authors do not comment in detail on the implications of their results for these big-picture questions. While the discovery method is novel and AT2022cmc's rapid discovery is a triumph of new automated techniques for identifying rare, interesting transients in real time in large datasets, it will likely still take years or decades to build a statistical sample of these rare events. In the absence of a large event sample, it is not clear how AT2022cmc itself extends our knowledge of jetted TDEs beyond what we had already learned from Sw J1644+57. If the paper is revised, the authors may wish to further emphasize what (if any) new conclusions we can already draw about the physics underlying this rare class of transient from the data presented here.

We take this opportunity to stress a few novel and unique aspects of this discovery, in addition to the technical innovation needed to make it happen (we thank the referee for his/her kind comments in this regard). The likely most important aspect of this work is that a new optical transient class was revealed in the form of a fast “flash” followed by a plateau, slowly evolving component. Such a combination of observational signatures has not been observed before (J1644+57 had a fast component in the IR, but it was obscured at optical wavelengths) and will be of paramount importance to build a population. AT2022cmc will serve as a prototype in future real-time and archival searches. The multi-wavelength data were key to reveal the astrophysics of the source and confirm its nature. Thanks to experience with other types of energetic phenomena (e.g. GRBs), we managed to interpret the fast component as synchrotron radiation and build a coherent physical picture to explain the observations.

The discovery of Sw J1644+57 raised the puzzle that apparently only <~1% of TDEs launch relativistic jets. Before AT2022cmc, one of the possible solutions was that the jet fraction is perhaps much higher because the selection method based on gamma-ray triggers may be highly incomplete (this is indeed possible given that Swift BAT is not good at triggering on long-duration, faint events, see Levan et al. 2014, ApJ, 781, 13). Another possible solution was that the relativistic jets produce most of their emission in the soft-X-ray band without triggering our gamma-ray detectors. However, the discovery of AT2022cmc (selected as an optical transient) further pins down the physical rate of jetted TDEs to be <~1% of the total TDE rate — so the puzzle is real.

Regarding the big-picture implications on jet launching and super-Eddington accretion, we conclude from these jetted TDEs that super-Eddington accretion disk is capable of launching relativistic jets — the inverse-Compton drags from the intense radiation field and the gas surrounding the BH do not prevent the jets from escaping to large distances. However, since the peak fallback rate exceeds the Eddington limit for BHs less than 10^7 Msun, we conclude that super-Eddington accretion is not a sufficient condition for jet launching. With the current data, we are unable to provide a definite answer to why only a small fraction of TDEs launch jets. Previous works (Tchekhovskoy et al. 2014) have argued that the requirements of a high BH spin and high magnetic flux are not met for most TDEs. More theoretical works on the magnetic fields in TDE accretion disk around spinning BHs are needed.

We hope that the improved comparison with Sw J1644+57 helps highlight those aspects of AT2022cmc that are unique. AT2022cmc is the first jetted TDE found as an optical transient and without a GRB, it is the furthest jetted TDE ever found, it has a rich set of optical/IR spectra which show that it is featureless (unlike most TDEs), it represents the brightest transient source ever observed in the submillimeter ~days from its onset. Our work demonstrated that, even if the examples discovered by Mattila+18 and Sw J1644+57 were obscured in the optical, it may be common for jetted TDEs to be little affected by extinction.

Finally, we now suggest that a link exists between TDEs with jets and a specific class of luminous and featureless TDEs. As explained above, this idea can be tested in the near future and, if confirmed, it will be of great impact.

The paper provides a thorough overview of the observations that were conducted (including the process by which AT2022cmc was flagged as an interesting transient in the ZTF data stream), generally sufficient detail of the data processing to reproduce the authors' analysis if desired, and an adequate description of the rates calculations for similar events in mm and optical surveys. I do not have any major concerns with these portions of the paper, and the comprehensive multi-wavelength dataset presented here will undoubtably be of great interest to the community. I do however have some questions on the modeling, as well as some more minor comments, which I list below.

Line 78: Some more recent papers on FBOTs should probably also be cited here, particularly because the light curves for some events are currently included in Figure 2 without attribution.

Additional citations were included.

Line 98: It would be helpful to explicitly state that Sw J1644+57 is the prototypical jetted TDE, and the only previous well-studied event of this class.

We added to the text: “The best studied jetted TDE to date is Swift J1644+57, which was discovered in gamma-rays, but was too obscured by dust to be seen at optical wavelengths.”

Line 115: This may refer to an earlier version of SI Figure 2, or to Figure 2a in the main text? The "high X-ray luminosity and flux variability" are not clear from the current SI Figure 2, which shows only the (consistent with constant) evolution of the power-law photon index of the XRT data. A citation is also needed for the quoted X-ray variability timescale of ~hr.

The referenced figure is Fig. 2a in the main text: the reference to the figure has been corrected. Two references are now present for the hour timescale variability.

Line 156: Note that the jetted TDE rate quoted in reference 29 is based on only two of the 4 jetted TDEs mentioned here. The source for this rate is also inconsistently cited throughout the paper; in line 648-649 reference 1 is cited instead (the rate derived in reference 1 is very similar, but they use all 3 jetted Swift TDEs for their rate calculation).

We have gone through the text and modified the jetted TDE rate to be the Sun at al. (2015) result throughout.

Figure 2: The original sources for the data on other transients in these figures are not cited. The nomenclature is also inconsistent between panels (e.g. "AT2018cow-like versus FBOT) and some acronyms are not defined, which may be confusing for the non-expert (e.g. LLGRB = low-luminosity gamma-ray burst).

We added text to the caption including the original sources for the data and definitions for the acronyms. To be consistent and concise, we adopted the term "LFBOT" in both the text of the manuscript and in the figure to refer to events similar to AT2018cow.

Figure 2 panel (b): Could the x-axis be updated to show rest-frame days rather than days elapsed in the observer frame, for consistency with panels (a) and (c)? The transients shown are at a very broad range of redshifts, so this is a more physically meaningful comparison. (It may also be useful to indicate the frequency of each light curve in the rest-frame.)

We have updated this.

Methods Section 1 (see also lines 116 and 130 in the main text): Some of the calculations in this section are missing relevant correction factors for the redshift. As AT2022cmc is at z=1.19, these correction factors can become significant for some derived parameters (see e.g. Barniol Duran et al 2013). For example, as the variability timescale of 1 day appears to refer to a time difference measured in the observer frame, the calculation of the variability timescale in line 550 is missing a factor of (1+z).

The calculations were done in a self-consistent way in the host-galaxy rest frame taking into account relevant redshift factors. We have added the following two sentences to avoid confusion: “All quantities (time, frequency, energy, luminosity, etc) in this section are defined in the rest frame of the SMBH or the host galaxy. It is straightforward to convert these quantities to the observer's frame by multiplying the relevant cosmological factors for a given redshift.”

Lines 526-527: This sentence is confusing; do the authors intend to imply that radio emission is expected from the self-crossing shock and the accretion disk as well as the jet?

The radio emission comes from the jet afterglow instead of self-crossing shock or accretion disk. This sentence has been modified such that there is no longer confusion.

As mentioned in Methods Section 1 (lines 555-557), the synchrotron models applied to the radio afterglow are subject to a number of parameter degeneracies. Although I understand that this is a discovery paper and the authors may wish to leave detailed modeling and a complete exploration of parameter space to future work, some more discussion of the allowed parameter ranges may be warranted. In the example model discussed here, several parameters are fixed to "fiducial" values with little to no justification. For example, in line 528 the authors state that an isotropic-equivalent jet energy of 1e53-1e54 erg is "implied by the X-ray emission", but no explicit derivation of the energy from the X-ray luminosity is included in the paper, so it is difficult to evaluate what assumptions underlie this statement. It is stated that parameters including epsilon_B are uncertain "to an order of magnitude", however this may underestimate the true uncertainties; for example, the range of
epsilon_B values observed in relativistic transients spans at least 2e-3 to 0.33 (e.g. Laskar et al 2015; Hajela et al 2022).

The uncertainties/degeneracies in the adopted parameters (Eiso and epsilon_B) only weakly affect the shock radius “r” and Lorentz factor “\Gamma” — eqs. (8) and (9) show that r ~ (Eiso * epsilon_B)^{1/8} and \Gamma ~ (Eiso * epsilon_B)^{1/16}. These are the main quantities used in our arguments in the Main Text. In the paragraph in the Methods Section 1, we also commented on the inferred values of n_0 (pre-shock density of the surrounding medium) and B (the magnetic field strength in the shock-heated emitting plasma), which suffer larger uncertainties due to stronger dependence on Eiso and epsilon_B. In the revised paper, we have added the explicit scalings of n_0 ~ (Eiso/epsilon_B)^{1/2} and B ~ (Eiso * epsilon_B)^{5/16}. These two scalings show that if we allow epsilon_B to defer from our fiducial value of 10^-2 by two orders of magnitude, the resulting n_0 would change by a factor of 10 and B would change by a factor of ~4. We note that in the earlier version of the paper, it was confusing that the reader may misunderstand the text as “the value of epsilon_B is uncertain to an order of magnitude”. In the revised paragraph, we have made it more clear by saying that “the inferred values of n_0 and B are uncertain by about one order of magnitude”.

Regarding the fiducial value of Eiso = 10^53 erg, it is inferred from the observed time-integrated isotropic equivalent X-ray energy of ~1e52 erg (since the earliest X-ray detection) and taking an radiative efficiency of 0.1. It is possible that the true Eiso is higher than we have assumed by a factor of a few to 10 (in the case that our earliest X-ray observation was substantially delayed from the peak of the X-ray lightcurve), but the inferred values of “r” and “\Gamma” are hardly affected.

Methods Section 4: I am curious how these constraints on the host galaxy compare to what is known about the host galaxies of other jetted TDEs, e.g. Sw J1644+57. Would the host galaxies of other known jetted TDEs also have evaded detection at z=1.19? Are archival upper limits at other wavelengths at all constraining? (For example, can archival radio and/or X-ray data place any limits on pre-existing AGN activity?)

The host galaxies of Sw J1644, Sw J2058, and Sw J1112 would (likely) evade discovery
There are limited constraints put on the host galaxies of Sw J2058, and Sw J1112, and even Sw J1644. Upon discovery, the host of Sw J2058 (at a similar redshift to 22cmc) evaded detection, with SDSS being able to put 3-sigma upper limits of u' > 21.9, g' > 23.5, r' > 23.5, i' > 23.0, and z' > 21.7 mag, and the light being dominated by transient emission (Cenko+ 2012). This was not the case for Sw J1644, which was dominated by the host in the optical. Even so, the host was relatively small and faint with a host mass of log(M) = 9 - 10 at redshift 0.35 (Levan+ 2011). Based on the host magnitudes in Levan+ 2016, it is likely that this host would evade detection at z=1.19 for ~24 mag limit imaging surveys, as is the case for Sw J1112 based on the magnitudes in Brown+ 2016.

Lines 635-636: Based on the recent slow evolution of the optical/UV light curves seen in Figure 1, it seems unlikely that the transient will have faded below HST's detection limit "in a few months" -- wouldn't observations at even later times be better?

HST observations were requested promptly upon discovery of AT2022cmc to maximize the S/N of the transient detection in the first HST epoch. At the time, AT2022cmc was still evolving very rapidly in the optical/IR and its nature was still unclear. Hence, a few months' time gap seemed appropriate and conservative. New HST observations were scheduled and performed at the beginning of July 2022, sooner than we hoped after realizing that the transient changed its fading pace. A faint extended source is marginally detectable in F606W images where the transient, now at ~23.5 mag, is located. However, more data will be needed to confirm the detection and the position of AT2022cmc in the host confidently. Observations with JWST have now been proposed to complement HST imaging. A dedicated paper is planned to describe the analysis of space-based observations of the host, along with a paper that presents the late-time multi-wavelength light curve.

Line 638: should read "rate of AT2022cmc-like transients"

Fixed.

Line 675: clarify that this is "optical light curve modeling" (or specify what range of wavelengths is fit to this model).

Fixed.

Line 896: The table referenced here is missing from the manuscript.

Fixed.

Referee 3

In this work, the authors have observed this astronomical transient event at high redshift over the multiple wavelengths, compared several transient event candidates, and concluded that it is a tidal disruption event accompanied by an on-axis relativistic jet (i.e., Jetted TDEs). There have been three Jetted-TDE candidates so far. However, this is an extremely bright optical jetted-TDE that we have observed for the first time. The authors have also given an interesting interpretation of the lightcurve variation, which is composed of a peak and subsequent flat time evolution, from the observational points of view. The work contained in this paper is very interesting and impressive enough to attract the general audience, and the key results are well summarized. Thus, it is worth publishing this paper in Nature. However, the authors need to address a series of issues for further improvement of the manuscript and clarification of their results before I consider recommending it for
publication.

1. The authors gave an observational upper limit (~a few 10^8 solar masses) for the black hole mass by using the Eddington luminosity and M-sigma relation. On the other hand, there is a theoretical mass limit called Hill's mass for TDEs. This limit provides that only an SMBH with a mass smaller than 10^8 solar masses can cause TDEs for the non-rotating SMBH case (e.g., Kesden 2012). The black hole spin makes the SMBH mass more massive. I suggest the authors should add the discussion of the Hill's mass.

We thank the referee for bringing up the argument based on Hill's mass. A non-rotating SMBH of mass=10^8 Msun can disrupt a Sun-like star outside its horizon (Kesden 2012), whereas higher-mass stars can be disrupted by bigger SMBHs even in the non-rotating case. Since we do not know the mass of the disrupted star (which may be up to a few solar masses), a non-spinning BH of mass of a few times 10^8 Msun is allowed. For a rapidly spinning BH (which is likely the case), the upper limit for the BH mass would be close to 10^9 Msun according to Kesden (2012). This constraint based on the Hill's mass argument is similar to (perhaps slightly less constraining than) that from the host-galaxy scaling relations (M < 5x10^8 Msun).

We have added the constraint based on the Hill's mass argument in Methods Section 5 of the revised paper.

2. Hill's mass argument tells us that the SMBH of this jetted TDE is likely to be spinning with a relatively high value in a prograde direction. The Jet's power inferred from the Blandford-Znajek mechanism is consistent with the X-ray observations? This could be related to the discussion about why the event rate of the Jetted TDEs that the authors have confirmed is so rare.

The constraint on the spin of the SMBH based on the Hill's mass argument is rather weak (for instance, a non-spinning SMBH of e.g., 10^7 Msun can disrupt a main-sequence star outside the event horizon). When revising the paper, we instead applied the Blandford-Znajek model to the observed X-ray peak luminosity and inferred a lower limit on the SMBH spin to be a > 0.3. This is essentially because the Blandford-Znajek jet power roughly scales as the BH spin squared — the observed peak X-ray luminosity requires a minimum BH spin. We also apply the same argument to Sw J1644+57 (which has a higher peak X-ray luminosity by a factor of ~10) and obtain a >~ 0.7 for that case.

The end result is that the Blandford-Znajek mechanism is consistent with the X-ray observations provided that the BHs in jetted TDEs are spinning sufficiently rapidly.

This has been added at the end of Methods Section 2.

3. It is interesting that the luminosity of IR radiation is as large as that of optical/UV and also the peak position is almost the same as seen in Figure 1. What is the origin of this IR emission? Is it re-emitted from dust by which the X-ray/UV radiation is absorbed? It would be better to explain the origin of IR emission.

In our current model, the IR radiation in the first ~4 days is explained as the synchrotron emission from the jet afterglow (which spans from radio, mm, IR, optical and at higher frequencies, the synchrotron afterglow is overshone by other components). The referee raised an interesting question — can the IR emission originate from a dust echo?

The duration of the dust echo is given by the light-travel time across the dust sublimation radius projected on our line of sight. Theoretical considerations show that the dust sublimation radius is usually of the order 0.1 pc or larger. This means that the variability timescale of the dust echo is of the order a year (or longer). Thus, it is unlikely that the rapidly variable (t_var ~ 1 day) IR emission comes from the dust echo.

However, late-time IR observations may detect a bright dust echo from this event and this is certainly an important component to keep in mind (and it helps to "see" the source from other viewing angles). The current paper focuses on the early time emission and we do not discuss the prediction for possible dust echo.

4. Whether this event occurred around the center of the nuclear region is important information for comparison purposes with other transient event candidates. It would be better to give a detailed description of the location information and future observational plans.

A better assessment of the uncertainties are now implemented.

5. The authors have stated that the AT2022cmc light curves are red at the early peak and become bluer as time goes by. By taking into account Figure 4, this means that photons from the matter moving at a relatively slow speed toward the earth were detected first, and then photons emitted from objects moving at a faster speed are detected at a later time. Is this interpretation correct? I am a little bit confused about it. In any case, the authors need to clarify why the early peaks are red and the later plateau becomes bluer, and show it in Figure 1 clearly.

In our current interpretation, the color evolution is entirely due to the fact different emitting components dominate at different times — the (red-color) jet afterglow dominates the optical flux the first few days and the UV blackbody component dominates at later time. The speeds of photons at all relevant frequencies are essentially equal to the vacuum speed of light. We have emphasized the timing of the two components in the Main Text of the revised paper so as to avoid confusion.

Figure 1 was improved by:

- Removing filters with sparse data points
- Making the offsets proportional to each band's central wavelength. This makes it easier to infer the color and the color change.
- A gaussian process regression estimate was added, for the r-band data, to guide the eye.

6. The authors mentioned (in lines 120-133) that the optical and mm radio origins are the same (i.e., from the reverse shock). Why is there a 5-day delay between these lightcurve peaks as seen in Figures 1 and 2(b)?

The peak of the mm light curve in Fig. 2b likely occurred before our observations began. The apparent delay in the radio (Fig. 9) is due to the evolution of the synchrotron spectrum.

7. If the outflow comes from a self-crossing shock, then the outflow velocity can be estimated by equating the gravitational energy and the kinetic energy. I suggest the authors should check whether such a theoretical value is consistent with the outflow velocity used here (10^9 cm/s).

The velocity of the outflow from the self-crossing shock has been calculated by previous works (e.g., Jiang et al. 2016, Lu & Bonnerot 2020). The procedure is slightly more complicated than equating the gravitational energy and kinetic energy — because the stellar debris starts in bound orbits and one must subtract the gravitational binding energy in calculating the asymptotic speed of the outflow. The result can be seen from the bottom panel of Fig. 10 of Lu & Bonnerot (2020).

8. A relativistic jet would be emitted only after the disk is formed and the mass accretes to the SMBH. It takes time, i.e., circularization time, for the disk to form after the outflow is blown by the self-crossing shock. If the circularization time is longer than 10 days (it is theoretically considered to be much longer than 10 days, though [e.g., see Bonnerot et al. 2017]), the radiation from the photosphere should arrive earlier than the radiation from the Jet. This differs from the view that the authors proposed. How do you resolve this inconsistency? For TDEs (of course for this paper, too), the timescale of debris circularization (energy dissipation) is an important topic. I suggest the authors should discuss this in the main and/or supplementary text.

We, again, thank the referee for raising the interesting point of the disk circularization time. Since the debris circularization process is currently highly uncertain from a theoretical point of view, it is desirable to seek for observational constraints — and the best case would be these jetted TDEs where we know the jet must come from the disk.

The difficulty is that we do not know t_0 — the time when the disruption occurred. At such a large distance, the disruption itself does not provide any observable signal. According to our current interpretation, the jet has been launched before our earliest observations (of the fast optical/IR flare, X-ray and mm emission). Before the sharp rise in the optical/IR and the first X-ray detection, we do not know if the blue blackbody component was already on (since we only know the blue UV blackbody component since the first Swift UVOT observation at ~5 days after the discovery in the host rest frame). The best upper limits we have were obtained 9.5 days rest frame before the first ZTF discovery, but they are shallower (g > 19.7 mag) than the plateau (g~21.5 mag).

Therefore, it could in principle be the case that the blue blackbody component has been on before the jet launching — a delayed disk formation is allowed by the current data. However, since we know the blue blackbody component typically lasts for a timescale of the order ~1 month (the fallback time), it is unlikely that the disk formation is delayed by much longer than ~1 month.

We have added this point in the Main Text and also emphasized the implications on our theoretical understanding of the disk formation process.

9. I agree with the event rate estimation of on-axis Jetted TDEs. Do the authors have any suggestions (from their observation [I know they assumed the jet direction is along the line of sight in this paper, though]) about the rate estimation of off-axis jetted TDEs, e.g., whether the expected event rate of off-axis jetted TDEs could be comparable to that of non-Jetted TDEs? This argument is important for why only a very small fraction of TDEs launch relativistic jets.

As discussed in the final paragraph of the main text, the off-axis TDE rate is given by the on-axis rate multiplied by f_b^{-1}, where f_b is the beaming factor and is typically assumed to be of order 10^{-2}. Then, assuming a non-jetted TDE rate of 100-1000 Gpc^{-3} yr^{-1}, the fraction of TDEs with jets is $\sim 10^{-(2-3)}$. This calculation is included in the final paragraph of the main text.

Additional changes:

- Aaron Tohuvavohu was added to the author list for his contribution in the Swift analysis for the detectability of the GRB associated with Sw J1644+57 at z=1.2.
- Minor changes were made to improve readability without affecting the scientific content.
- A typo in the host galaxy upper limit was corrected from Mr > -22.4 mag to Mr > 21.4 mag.

Sincerely,
Igor Andreoni & Michael Coughlin

Reviewer Reports on the First Revision:

Referees' comments:

Referee #1 (Remarks to the Author):

I commend the authors for such a comprehensive response to my original comments on their work.

The authors have bolstered their arguments for the novelty of this result, and the advances that it enables. The possible connection to the class of optically featureless TDEs is interesting, and well worth having highlighted, and the potential of future optical surveys has been emphasised more strongly. I am particularly interested by the constraints placed on the disk formation timescale, and would have liked to have seen a little more discussion of the "important constraints" that this provides on the hydrodynamics (Section 2 of the Methods notes that they are not discussed here, but they are highlighted as important in the main text).

On balance, the importance of the discovery and the improved discussion on its implications lead me to support the publication of this article, albeit after addressing the few additional points raised in this report.

The only major issue that the authors have not satisfactorily addressed is the absolute positional accuracy of the VLA data, which the authors use to derive their best position for the transient event. They claim to have achieved a positional accuracy of 6mas at Ku-band, which would correspond to about 5% of the synthesized beam size. The VLA Observational Status Summary (https://science.nrao.edu/facilities/vla/docs/manuals/oss/performance/positional-accuracy) clearly states that 10% of a synthesized beam is a typical error floor for standard observing conditions, without special calibration. To achieve better accuracy (down to 1-2% of a synthesized beam), special calibration and ideal observing conditions are required. The astrometry is not discussed in detail in Section 11.0.4, although the atmospheric observing conditions are noted to have been relatively poor. The authors suggest that consistency between frequencies, array configurations and elevations imply an absolute positional accuracy of ~6mas, which seems surprisingly good, given the observing conditions and the array reconfiguration. If they are to claim a positional uncertainty so much lower than is expected for the VLA, a more detailed discussion of these tests would be warranted in Section 11.0.4. This is an important issue, since future host galaxy detections will enable a comparison of the host galaxy centroid with the radio position, ascertaining whether the transient is indeed consistent with the host nucleus. For this, an accurate absolute position is needed, not only relative positional consistency between measurements, and the uncertainty in the absolute position is important to quantify. Given the prominence of the quoted absolute position in the main text (occurring immediately after the main paragraph), I would hope to see a more detailed discussion in the Methods section.

A few other minor comments:
- Lines 36-37: The position is taken from the VLA observations, yet this sentence reads as though ZTF achieved this level of positional accuracy. I recommend rephrasing to make the source of the position clearer.

- Lines 150-151: I assume that this discussion of the detection rates of Sw J1644+57 if placed at higher redshift refers to Section 6 of the Methods. However, line 727 makes a comparison to z=1.9325, and the relevance of that redshift is unclear, since AT2022cmc was at z~1.2 (as noted in the title of the subsection).

- Figure 5: I would suggest changing the label in the figure from "ZTF afterglows" to "GRB afterglows", to be consistent with the caption, and to make it clear which physical phenomenon was being referred to.

Referee #2 (Remarks to the Author):

I thank the authors for their thoughtful revisions to the paper in response to my comments and those of the other referees. The manuscript is much improved, and I think it may soon be acceptable for publication.

My main remaining concern relates to the novelty and broad impact of the results. The authors have made a good effort to reframe the text of the main paper to emphasize the uniqueness of AT2022cmc and to further contextualize it within the broader populations of known astrophysical transients. The technical achievement of its discovery remains extremely impressive, but I am not certain that a new observational technique for discovering jetted TDEs is enough to merit publication in Nature, no matter how promising. The connection to the new class of "featureless TDEs" is interesting, although still preliminary. If the paper is accepted to Nature, I would recommend emphasizing even further a couple of unique aspects of AT2022cmc:

-- I would argue that a for a broader audience, AT2022cmc's unique optical light curve is more interesting for the physics it potentially helps to constrain than for its usefulness in identifying jetted TDEs in survey data. (The latter is of course also important, but is mostly of interest to optical transient astronomers.) The new details in lines 134-146 help with this, but the authors should also explicitly mention the uniqueness of the optical light curve in the abstract.

-- AT2022cmc is the furthest jetted TDE yet discovered, but it might be possible to further emphasize why this is interesting (for example, TDEs uniquely probe quiescent SMBHs and jetted TDEs may be the only way to probe this population at high redshifts in detail).

-- Optical surveys are not just a new technique for finding jetted TDEs (lines 176-177), they may be the *best* or *only* technique for doing so at the highest redshifts (following the argument in Sections 6 and 10 of the Methods). This point could be further emphasized in the main text.

-- Lines 107-108 state that the only two well-studied jetted TDEs both occurred on black holes with non-zero spin -- how robust are these limits? And how unusual is this compared to what we know about the distribution of spins in the SMBH population overall? Does this hint that high spin is also needed to launch a relativistic jet? If so, that could be an important result of potentially broad interest.

Overall, while the authors have argued that the data presented here (in combination with other ongoing data collection) will ultimately provide a rare opportunity to probe poorly-understood physics, this paper in itself still does not seem to contain a major step forward in our physical understanding of jetted TDEs. I believe the paper could be published as-is in Nature Astronomy, and it will unquestionably provide an extremely important dataset to be incorporated into future work. It remains an extremely important work for time domain astronomers, particularly those studying TDEs and relativistic transients. However, it may require substantial additional work on AT2022cmc -- such as the authors' follow up papers "in prep" referenced here, or additional work in progress by other members of the community, to ultimately achieve the deeper physical insight that would further increase the impact of this paper beyond the subfield of time domain astronomy.

Referee #3 (Remarks to the Author):

The authors have addressed most of the comments that I gave. However, I yet have a reservation about the publication of the paper in Nature because I am still wondering about some of the authors' claims and responses.

[Previous point 6] 1.
The outflow velocity surely depends on black hole mass, stellar mass, and stellar radius (Jiang et al. 2016). Figure 10 of Lu & Bonnerot 2020 suggests that for v/c~0.033 (i.e., v=10^9 cm/s), the black hole mass will be a few times 10^6 solar masses or less for less massive stars than one solar masses. This would give a constraint on the black hole mass. Including the Margorrian relation and Hill's mass arguments, the black hole mass and stellar mass/radius should be discussed in the paper.

[Previous point 8] 2.
How to circularize stellar debris is theoretically uncertain, as the authors' said, but there is a standard scenario that debris is circularized by debris self-crossings due to relativistic apsidal precession (e.g., Rees 1988, Hayasaki et al. 2013, 2016, Bonnerot et al. 2016, Jiang et al. 2016). The authors' model that the UV component is emitted at the photosphere due to shock heating by stream-stream collisions, and the soft-X-ray photons are emitted from the inner region of the disk after the debris circularization seems to be made based on the standard scenario. However, the author's scenario is not consistent with the multi-wavelength observations, and therefore what possible scenarios for debris circularization from observations should be discussed.

The authors pointed out the zero-point uncertainty of the observations and claim that the peak of the thermal UV component could come before the soft-X-ray component. However, this is unlikely to happen since the luminosity peak would be beyond the Eddington luminosity if the true peak of UV emissions were at an earlier time. The observational fact that the UV peak and the soft-X-ray peak are almost the same suggests that they both can be of disk origin, unlike the authors' claim. Alternatively, there could be a scenario in which the disk and subsequently jet form until the UV photons are produced and then reach the photosphere. The key questions, in this case, are the time between UV photon production and emission and the energy loss time in units of the most tightly bound debris orbital period (both timescales can depend on the black hole mass and stellar mass, and radius).

[Previous point 9] 3.
The authors' estimate of the off-axis Jetted TDE rate is based on the assumption that all the non-jetted TDEs do not have a relativistic jet. Is this justified?
Relativistic jets may be emitted from an opening angle larger than the relativistic beaming angle (~1/Gamma), although they are too dark to be seen directly in this case. If TDEs that are currently regarded as non-jetted TDEs would have such jets, the event rate of off-axis jetted TDEs would be significantly larger.

4.
Regarding line 167 (and in the response from referee 1), the three TDEs currently associated with the IceCube neutrinos show no strong evidence of a relativistic jet. Without mentioning the discussion, it is not appropriate to mention the association between the Jetted TDE and neutrino emission.

Author Rebuttals to First Revision:

NAT #2022-04-05292B

In this document, we address the referees' feedback after a second review of the manuscript.

I commend the authors for such a comprehensive response to my original comments on their work.

We thank the referee for the kind feedback.

The authors have bolstered their arguments for the novelty of this result, and the advances that it enables. The possible connection to the class of optically featureless TDEs is interesting, and well worth having highlighted, and the potential of future optical surveys has been emphasised more strongly. I am particularly interested by the constraints placed on the disk formation timescale, and would have liked to have seen a little more discussion of the "important constraints" that this provides on the hydrodynamics (Section 2 of the Methods notes that they are not discussed here, but they are highlighted as important in the main text).

On balance, the importance of the discovery and the improved discussion on its implications lead me to support the publication of this article, albeit after addressing the few additional points raised in this report.

The only major issue that the authors have not satisfactorily addressed is the absolute positional accuracy of the VLA data, which the authors use to derive their best position for the transient event. They claim to have achieved a positional accuracy of 6mas at Ku-band, which would correspond to about 5% of the synthesized beam size. The VLA Observational Status Summary (https://science.nrao.edu/facilities/vla/docs/manuals/oss/performance/positional-accuracy) clearly states that 10% of a synthesized beam is a typical error floor for standard observing conditions, without special calibration. To achieve better accuracy (down to 1-2% of a synthesized beam), special calibration and ideal observing conditions are required. The astrometry is not discussed in detail in Section 11.0.4, although the atmospheric observing conditions are noted to have been relatively poor. The authors suggest that consistency between frequencies, array configurations and elevations imply an absolute

positional accuracy of ~6mas, which seems surprisingly good, given the observing conditions and the array reconfiguration. If they are to claim a positional uncertainty so much lower than is expected for the VLA, a more detailed discussion of these tests would be warranted in Section 11.0.4. This is an important issue, since future host galaxy detections will enable a comparison of the host galaxy centroid with the radio position, ascertaining whether the transient is indeed consistent with the host nucleus. For this, an accurate absolute position is needed, not only relative positional consistency between measurements, and the uncertainty in the absolute position is important to quantify. Given the prominence of the quoted absolute position in the main text (occurring immediately after the main paragraph), I would hope to see a more detailed discussion in the Methods section.

A well-determined astrometric accuracy is certainly important for the broader understanding of this system. We do wish to emphasize that members of this project have recently obtained VLBA observations that will provide a much superior position than what is possible with the VLA, and it is this (plus our existing HST imaging) which will be used to establish the absolute position in future work once late-time HST imaging of the host is available. We have nevertheless carefully examined the error budget of the VLA astrometry and incorporated a conservative accounting of potential sources of systematic error, and revised the uncertainty to 10 mas (up from 6 mas). A breakdown of this calculation is provided below.

The astrometry has a random component (associated with noise statistics and phase errors) and a systematic component (associated with the reference system).

The random component was determined directly by comparing the position of the source during observations taken under good conditions (atmospheric conditions varied over the course of our campaign and were occasionally poor, but in many cases the conditions were excellent). The positional scatter of these measurements at Ku-band is 6 mas, which is what we quoted previously.

The systematic component originates from the phase calibrator, which fortuitously is an extremely well-determined and well-measured source (3C286). The systematic uncertainty has two components in its own. The first component is the absolute astrometric uncertainty in the position of 3C286. This is derived from VLBA observations and is negligible: less than 1 mas. The second component is associated with uncertainties in the structure model of this source used by the VLA. To quantify this, we examined the VLA model and determined the relative flux and average offset associated with clean components originating from the side of the 3C286 nucleus opposite the jet, where VLBA observations show no emission. These components constitute 5% of the total flux and are offset up to 50mas from the nucleus or up to 100mas from the expected location of components from the nuclear region. The product of these figures provides an approximate accounting of the possible overall astrometric offset originating from the VLA model: 2.5mas as a reasonable estimate, or 5mas as an upper bound.

Using the latter and combining in quadrature with the statistical uncertainty we obtain an improved astrometric uncertainty of 8mas, which we further round up to 10mas to be maximally conservative. This is still slightly better than the rule-of-thumb in the VLA handbook, which is justified because we have extensive repeated observations of this source and are able to phase-calibrate directly against 3C286.

A few other minor comments:

- Lines 36-37: The position is taken from the VLA observations, yet this sentence reads as though ZTF achieved this level of positional accuracy. I recommend rephrasing to make the source of the position clearer.

We now clarify in the text that the position was “obtained via radio data analysis”.

- Lines 150-151: I assume that this discussion of the detection rates of Sw J1644+57 if placed at higher redshift refers to Section 6 of the Methods. However, line 727 makes a comparison to z=1.9325, and the relevance of that redshift is unclear, since AT2022cmc was at z~1.2 (as noted in the title of the subsection).

We thank the referee for catching this inconsistency. Fortunately, this was just a typo: z=1.9325 should have been z=1.19325, thus z~1.2. This has been fixed.

- Figure 5: I would suggest changing the label in the figure from "ZTF afterglows" to "GRB afterglows", to be consistent with the caption, and to make it clear which physical phenomenon was being referred to.

We have changed the label.

Referee #2 (Remarks to the Author):

I thank the authors for their thoughtful revisions to the paper in response to my comments and those of the other referees. The manuscript is much improved, and I think it may soon be acceptable for publication.

My main remaining concern relates to the novelty and broad impact of the results. The authors have made a good effort to reframe the text of the main paper to emphasize the uniqueness of AT2022cmc and to further contextualize it within the broader populations of known astrophysical transients. The technical achievement of its discovery remains extremely impressive, but I am not certain that a new observational technique for discovering jetted TDEs is enough to merit publication in Nature, no matter how promising. The connection to the new class of "featureless TDEs" is interesting, although still preliminary. If the paper is accepted to Nature, I would recommend emphasizing even further a couple of unique aspects of AT2022cmc:

-- I would argue that a for a broader audience, AT2022cmc's unique optical light curve is more interesting for the physics it potentially helps to constrain than for its usefulness in identifying jetted TDEs in survey data. (The latter is of course also important, but is mostly of interest to optical transient astronomers.) The new details in lines 134-146 help with this, but the authors should also explicitly mention the uniqueness of the optical light curve in the abstract.

We now say in the abstract that the optical light curve is “unique”.

-- AT2022cmc is the furthest jetted TDE yet discovered, but it might be possible to further emphasize why this is interesting (for example, TDEs uniquely probe quiescent SMBHs and jetted TDEs may be the only way to probe this population at high redshifts in detail).

In the concluding paragraph, we now state that the discovery of jetted TDEs “uniquely enables the study of distant quiescent SMBHs.”

-- Optical surveys are not just a new technique for finding jetted TDEs (lines 176-177), they may be the *best* or *only* technique for doing so at the highest redshifts (following the argument in Sections 6 and 10 of the Methods). This point could be further emphasized in the main text.

We modified the concluding paragraph to accommodate the referee’s suggestion. Instead of stressing the technical characteristics of the required optical observations, now the text says: “Here we demonstrated that the discovery of such energetic phenomena not only has become accessible to the optical community, but optical may also be the best technique for discovery at the highest redshifts, which uniquely enables the study of distant quiescent SMBHs.”

-- Lines 107-108 state that the only two well-studied jetted TDEs both occurred on black holes with non-zero spin -- how robust are these limits? And how unusual is this compared to what we know about the distribution of spins in the SMBH population overall? Does this hint that high spin is also needed to launch a relativistic jet? If so, that could be an important result of potentially broad interest.

We believe these results are robust — the only assumption here is that the jet is powered by the Blandford-Znajek mechanism (which is widely accepted to be what is powering jets in radio loud AGNs). The other competing power source — the Blandford-Payne wind — does not drive highly relativistic jets due to heavy baryon loading in disk wind. Another mechanism that is a competing model for highly relativistic jets in GRBs is neutrino annihilation, but this is irrelevant for the TDE case since the disk temperature is not high enough to produce a high neutrino density. Our lower limits on the BH spins in these two jetted TDE cases are not unusual as compared to what has been inferred from jetted (radio-loud) AGNs. However, the new aspect here is that we are constraining spins for dormant SMBHs, which are otherwise impossible to know without the TDE.

We have added the following sentence to the revised paper:

"We conclude that a high spin is likely required to launch a relativistic jet."

Overall, while the authors have argued that the data presented here (in combination with other ongoing data collection) will ultimately provide a rare opportunity to probe poorly-understood physics, this paper in itself still does not seem to contain a major step forward in our physical understanding of jetted TDEs. I believe the paper could be published as-is in Nature Astronomy, and it will unquestionably provide an extremely important dataset to be incorporated into future work. It remains an extremely important work for time domain astronomers, particularly those studying TDEs and relativistic transients. However, it may require substantial additional work on AT2022cmc -- such as the authors' follow up papers "in prep" referenced here, or additional work in progress by other members of the community, to ultimately achieve the deeper physical insight that would further increase the impact of this paper beyond the subfield of time domain astronomy.

Referee #3 (Remarks to the Author):

The authors have addressed most of the comments that I gave. However, I yet have a reservation about the publication of the paper in Nature because I am still wondering about some of the authors' claims and responses.

[Previous point 6] 1.

The outflow velocity surely depends on black hole mass, stellar mass, and stellar radius (Jiang et al. 2016). Figure 10 of Lu & Bonnerot 2020 suggests that for v/c~0.033 (i.e., v=10^9 cm/s), the black hole mass will be a few times 10^6 solar masses or less for less massive stars than one solar masses. This would give a constraint on the black hole mass. Including the Margorrian relation and Hill's mass arguments, the black hole mass and stellar mass/radius should be discussed in the paper.

What the referee quoted (v ~ 10^9 cm/s) is for the case with penetration factor rT/rp = 1 (tidal disruption radius divided by pericenter radius of the original star). The penetration factor is a free parameter here because it can be larger than one. Deeper penetration (smaller rp) means stronger GR apsidal precession and hence faster outflow speed, as can be seen from the dashed lines in Fig. 10 of Lu & Bonnerot 2020. Thus, it is not possible to uniquely infer the BH mass based on the outflow speed alone, as it is degenerate with the penetration factor as well as the stellar mass. Another complication is that Fig. 10 in Lu & Bonnerot 2020 only shows the mean speed of the unbound portion, whereas in reality there is a continuous distribution in the energies of the different fluid elements from stream-collision outflow from negative (bound) to positive (unbound). This makes it even harder to infer the BH mass. We did not include this part in the revised paper because it is model dependent and suffers significant uncertainties.

[Previous point 8] 2.

How to circularize stellar debris is theoretically uncertain, as the authors' said, but there is a standard scenario that debris is circularized by debris self-crossings due to relativistic apsidal precession (e.g., Rees 1988, Hayasaki et al. 2013, 2016, Bonnerot et al. 2016, Jiang et al. 2016). The authors' model that the UV component is emitted at the photosphere due to shock heating by stream-stream collisions, and the soft-X-ray photons are emitted from the inner region of the disk after the debris circularization seems to be made based on the standard scenario. However, the author's scenario is not consistent with the multi-wavelength observations, and therefore what possible scenarios for debris circularization from observations should be discussed.

It is not clear what aspect of observations the referee had in mind when saying “the author’s scenario is not consistent with multi-wavelength observations”. Based on AT2022cmc alone, the observations so far do not put a strong constraint on the processes that lead to the circularization of the bound debris. This is because the thermal optical component seen after the first few days in the host rest frame is not very different from other TDEs with thermal optical emission, although AT2022cmc is on the most luminous end of the luminosity distribution (but this may not be surprising given the high redshift), as shown in Fig. 8.

The authors pointed out the zero-point uncertainty of the observations and claim that the peak of the thermal UV component could come before the soft-X-ray component. However, this is unlikely to happen since the luminosity peak would be beyond the Eddington luminosity if the true peak of UV emissions were at an earlier time. The observational fact that the UV peak and the soft-X-ray peak are almost the same suggests that they both can be of disk origin, unlike the authors' claim. Alternatively, there could be a scenario in which the disk and subsequently jet form until the UV photons are produced and then reach the photosphere. The key questions, in this case, are the time between UV photon production and emission and the energy loss time in units of the most tightly bound debris orbital period (both timescales can depend on the black hole mass and stellar mass, and radius).

The UV emission coming before the X-ray component is allowed by observations for the following reason. Looking at panel (a) in Fig. 1, if the jet afterglow (which has a red color and dominates the optical bands in the first ~3 rest-frame days) is removed, then one sees that the blue or UV-dominated component evolves rather slowly with time on a timescale of 10 days to a month. If one extrapolates the emission in the UV bands backwards in time (for e.g., two weeks in the rest frame of the host), the UV luminosity will not be significantly higher given the slow evolution. The problem here is that our observations do not strongly constrain this component before the discovery of this event, with the latest upper limit having been measured 21d before the transient detection (which is 9.6d in the transient rest frame).

The referee mentioned that, in the case that the UV peak and X-ray peak occur around the same time, this suggests that these two components both come from the accretion disk. We do not agree with this argument. The X-rays (which are spectroscopically hard at super-Eddington luminosities) must be from a relativistic jet instead of the surface of the disk. The blackbody radius inferred from the blue component is about 2x10^15 cm (see Fig. 7), which is much larger than the theoretically expected accretion disk radius (of the order 10^13cm). Indeed, the Keplerian orbital period at a distance of 2x10^15 cm from a 10^6 Msun BH would be 1.5 yr, which means that it is unlikely that the system forms an accretion disk at this large radius. The more likely scenario is an outflow.

We have added the following sentence in Section 10 of the revised paper: (right after we infer the blackbody radius to be 2x10^15cm)

“This large radius suggests that the emission comes from an outflow instead of the surface of an accretion disk --- the Keplerian orbital period at a distance of 2x10^{15}cm would be 1.5yr (M/10^6Msun)^{-1/2} which is too long for any plausible black hole mass M.”

[Previous point 9] 3.

The authors' estimate of the off-axis Jetted TDE rate is based on the assumption that all the non-jetted TDEs do not have a relativistic jet. Is this justified?

At this point, we do not know whether non-jetted TDEs have very weak jets — they are in principle allowed by our radio surveys (which can only put strong constraints on jets with energies of the order 1e52 erg or above). Here, our estimate of the off-axis jetted TDE rate is only based on powerful on-axis jets like AT2022cmc/SwiftJ1644. Thus, our inferred off-axis jetted TDE rate should be considered only as a lower limit — weaker jets from normal TDEs would increase the rate.

Relativistic jets may be emitted from an opening angle larger than the relativistic beaming angle (~1/Gamma), although they are too dark to be seen directly in this case. If TDEs that are currently regarded as non-jetted TDEs would have such jets, the event rate of off-axis jetted TDEs would be significantly larger.

The off-axis jetted TDE rate reported in this paper should be interpreted as the rate of jetted TDEs analogous to the one detected in this work. It is plausible that some of the "non-jetted" TDEs have weak, or otherwise unusual, jets, but the launching of such jets during TDEs are relatively unconstrained and is outside the scope of this paper. We have updated the text to clarify that the rates discussed are for jets with the similar properties to AT2022cmc.

4.

Regarding line 167 (and in the response from referee 1), the three TDEs currently associated with the IceCube neutrinos show no strong evidence of a relativistic jet. Without mentioning the discussion, it is not appropriate to mention the association between the Jetted TDE and neutrino emission.

We removed the mention of jetted TDEs being a possible source of neutrinos.