Dear editor, dear referees,

thank you very much for your points. We incorporated most of them in our 2nd revision (please, see boldface text). I'm again sorry for the delay. I also prepared some materials in case they are needed as "nice illustrations":

http://sirrah.troja.mff.cuni.cz/~mira/fargo_terrestrial/illustrations/

--

We address editors comments from the PDF file first:

> "Massive cores of the giant planets..." -- Is this sentence really needed? As
> the focus is on the formation of the inner terrestrial planets, mentioning
> the giant planets seems unnecessary and the abstract can start with the next
> sentence.

We think it is needed, because it provides a broader context, explains some mechanisms (aerodynamic drag, pebble accretion) related to both giant and terrestrial planets and this similarity seems to be logical.

> "... terrestrial planet system..." -- as you focus on the Solar System, I'm
> wondering if you have to specify it here ("the terrestrial planets of the Solar
> System formed later..."), to be clear it's not about exoplanets (mostly)

We had exactly this discussion in our team. Previously, we decided to prefer "terrestrial planet system" -- because some of the planets may be young (cf. Dauphas & Pourmand 2011) and the implications of the model are more general -- but you are right that the phrase "of the Solar System" is somewhat missing in the abstract. We thus included it in the next sentence in the revised manuscript.

> "(related ref. 4 invoked a different process to concentrate planetesimals)."
> -- this is a bit clunky to include in an abstract -maybe it's material for
> just the main text (appropriately expanded)? after all an abstract is mostly
> the place to describe what you did and what your main result is.

Yes, it was a reference to an alternative model, but it can be possibly dropped (as it is cited in the 2nd paragraph).

> and determine the thermal structure of the gas and pebble disks in the
> terrestrial planet zone, we... [then of course it must be removed below]

Corrected.

> I am wondering if you can specify the timing ("protoplanets grow in the first
> ... Myrs") -not mandatory, it's just nice to have some quantitative result
> considering that you give the timing of the formation in the "classical model"
> at the beginning of the abstract

Yes, we added 10 Myrs.

> inner Solar System?

Yes, it is better.

> ", next to the evaporation lines of iron and silicates," -- maybe it can be
> cut if the abstract is too long

We prefer to retain it, because "highly-reducing environment" sounds a bit too abstract.

> what is the connection with the presented model? (which, by the description,
> seems about the early evolutionary stage)

We included a modified sentence, where we mention volatile elements
(in general), because it is rather counter-intuitive that early-on
the gas disk is hot (and temperatures are much higher than the equilibrium
temperature T_eq =~ 253 K at 1 au, in a gas-free environment), but at the end
of the gas phase, it is just the opposite, T_gas < T_eq. Consequently,
the position of the snow line was not always at 2-3 au and, in principle,
a delivery of volatiles is possible.

> in a Nature Astronomy Letter, no further introductory material should be put
> after the first paragraph in bold (if unavoidable, it should be restricted to
> a couple of sentences).
> Please remove and restructure (it is ok to start directly with your model; I
> think that several of the material presented here as introductory can be
> shifted, with modifications, to later in the paper, for example when you
> discuss the results of your model with respect to the present models)
> In this case, the first paragraph and part of the second are quite
> introductory in tone

I see; we modified the 2nd paragraph accordingly. It is now much
shorter -- it briefly explains the key observational constraint --
and it was straightforwardly merged with the 3rd paragraph; with all
refs. retained (or shifted elsewhere).

--

Next we address the respective checklist:

In Editor's Summary, I would change "not several million years"
to "not tens of million years".

Figure captions were clarified as suggested.

--

> From luca.maltagliati@nature.com Fri Feb 19 18:55:16 2021
> Date: Fri, 19 Feb 2021 12:55:05 -0500
> From: luca.maltagliati@nature.com
> To: mira@sirrah.troja.mff.cuni.cz
> Cc: natureastro@nature.com
> Subject: Your manuscript, NATASTRON-19092655B
>
> Our ref: NATASTRON-19092655B
>
> 19th February 2021
>
> Dear Dr. Brož,
>
> Thank you for your patience as we've prepared the guidelines for final
> submission of your Nature Astronomy manuscript, "Terrestrial planet formation by
> torque-driven convergent migration of planetary embryos" (NATASTRON-19092655B).
> Please carefully follow the step-by-step instructions provided in the
> personalised checklist attached, to ensure that your revised manuscript can be
> swiftly handed over to our production team.
> I also attached my comments directly in the attached PDF of the paper.

Yes, please, see above.

> We hope to hear from you within not more than two weeks -one week would be
> better, as owing to strict production deadlines a longer period could result in
> a delay in formal acceptance and publication-; please let us know if the

> revision process is likely to take longer.
>
> When you upload your final materials, please include a point-by-point response
> to any remaining reviewer comments.
>
> If you have not done so already, please alert us to any related manuscripts from
> your group that are under consideration or in press at other journals, or are
> being written up for submission to other journals (see:https://www.nature.com/nature-research/editorial-policies/plagi
> duplicate-publication for details).
>
> In recognition of the time and expertise our reviewers provide to Nature
> Astronomy's editorial process, we would like to formally acknowledge their
> contribution to the external peer review of your manuscript entitled
> "Terrestrial planet formation by torque-driven convergent migration of planetary
> embryos". For those reviewers who give their assent, we will be publishing their
> names alongside the published article.

Yes, of course.

> Cover suggestions
>
> As you prepare your final files we encourage you to consider whether you have
> any images or illustrations that may be appropriate for use on the cover of
> Nature Astronomy.

Well, please see the URL above. If there is a need for a resolution higher
than 4K or a specific aspect ratio, please let me know and I can easily render
the 3D scene again, using a suitable setup.

> Covers should be both aesthetically appealing and scientifically relevant, and
> should be supplied at the best quality available. Due to the prominence of these
> images, we do not generally select images featuring faces, children, text,
> graphs, schematic drawings, or collages on our covers.
>
> We accept TIFF, JPEG, PNG or PSD file formats (a layered PSD file would be
> ideal), and the image should be at least 300ppi resolution (preferably 600-1200
> ppi), in CMYK colour mode.
>
> If your image is selected, we may also use it on the journal website as a banner
> image, and may need to make artistic alterations to fit our journal style.
>
> Please submit your suggestions, clearly labeled, along with your final files.
> We'll be in touch if more information is needed.
>
>
> Nature Astronomy has now transitioned to a unified Rights Collection system
> which will allow our Author Services team to quickly and easily collect the
> rights and permissions required to publish your work. Approximately 10 days
> after your paper is formally accepted, you will receive an email in providing
> you with a link to complete the grant of rights. If your paper is eligible for
> Open Access, our Author Services team will also be in touch regarding any
> additional information that may be required to arrange payment for your article.
>
> Please note that you will not receive your proofs until the publishing agreement
> has been received through our system.
>
> For information regarding our different publishing models please see our
> Transformative Journals page. If you have any questions about costs, Open Access
> requirements, or our legal forms, please contact ASJournals@springernature.com.
>
>
>
> Please use the following link for uploading these materials:
> https://mts-natastron.nature.com/cgi-bin/main.plex?el=A4Cl2CVF6A2BcJ5J3A9ftdQDY

> ElgkkYHR9Y06wOTJYJgZ
>
> If you have any further questions, please feel free to contact me.
>
> Best regards,
>
> Luca Maltagliati
> Editor
> Nature Astronomy

> Reviewer #1:
> Comments for the Author:
> I have read with interest the new version of the paper.
> I thanks the authors for their detailed revision taking into
> account all the requirements. The additional runs are clearly shown and added to
> the SI when necessary.
>
> The paper as well as the SI give now a clear idea of the validity of the model
> and of the range of parameters in which the scenario of terrestrial planet
> formation by convergent migration of planetary embryos is valid. This range of
> parameters looks wide enough to infer that results are robust.
> Precisely, the parameters are well within the nowadays knowledge (or the
> nowadays uncertainty) that we have about protoplanetary disks.
>
> I certainly recommend now the paper for publication on Nature Astronomy.
>
> As a very small remaining correction I have noticed that in top panels of Fig.3
> as well as in Fig.10 of the SI the selected best fit simulation is represented
> by color which nicely correspond to the outcome of the bottom panels. However,
> the label "best fit" is associated to a purple filled circle in Fig.3 and to a
> yellow one in Fig.10 which is a bit misleading. If possible I suggest to change
> it: best fit "color" could be a possible choice as it is written in the caption.

Oh, it was chosen automatically by Gnuplot. If it is not 'critical',
we would prefer to retain it.

> Reviewer #2:
> Comments for the Author:
> Second Review of \Terrestrial planet formation by torque-driven convergent
> migration of planetary embryos" by Broz et al
>
> I appreciate the authors' having addressed the points brought up in my previous
> report and in those of the two other referees. They clearly did a lot of work,
> and are very nicely done on the whole.
>
> Overall I think the paper is in quite good shape. My only outstanding criticism
> of consequence is that I don't see how their idea for pebble-driven water
> delivery in a late cold disk can make sense. I recommend removing that part of
> the paper and simply invoking a different water delivery mechanism (as explained
> below).
>
> After the authors address that issue and a few other small comments I am happy
> for the paper to be published in Nature Astronomy.
>
> Detailed comments:
>
> Lyra et al (2010) needs to be cited as the first demonstration of migration
> convergence zones, as well as the fact that they shift radially due to disk
> evolution.

Yes, we added this reference (in Paragraph 7).

> Am I understanding right that the disk did not evolve but was maintained for 10
> Myr? That seems excessively long compared with the generally-accepted few Myr
> observed via hot dust around other stars (Haisch et al 2001, Mamajek 2009, many
> others) and the 4-5 Myr lifetime inferred from the age distribution of the
> youngest chondrites. Obviously the simulations are already run, but would
> anything change if the disk dispersed after 3-5 Myr instead?

Yes, our nominal disk is up to 10 Myr old. We also performed tests
with exponentially decaying disks, the time scale 5 Myr, and somewhat
higher initial Sigma(r) -- and the answer is: "Not much". Our point here
is that we simply need enough gas to migrate/detach/damp/excite orbits
of terrestrial protoplanets.

Regarding Haisch etal. (2001), or Fedele etal. (2010), which was
mentioned in our previous response, we think that a non-negligible
fraction of observed disks must be older than 10 Myr, because
the plots only show a fraction of disks vs time, not the ages
of ALL disks, and there are also 'old' star clusters with disks
(eta Cha, TW Hya, NGC 1960).

> Page 6, \The hot-trail effect can explain the current orbital eccentricities of
> Venus and Earth (proper e = 0.02 and 0.01, respectively), which was never
> suggested before".
> While interesting, this ignores the later phases of Solar System evolution (e.g.
> the giant planet instability) and is not terribly relevant (unless put in
> context citing Brasser et al 2010 for example).

Well, we acknowledge later phases in the next sentence. We added a ref.
to Brasser etal. (2009), which is specifically devoted to terrestrial
planets. Our point here is that when non-zero e's are used as an
argument that, e.g. the giant-planet instability, has occurred, one
should be careful, because e's > 0 could have been created even earlier.

> Page 8: I am highly skeptical of the late pebble-driven water delivery in a cold
> disk. I think the easiest solution is to remove the idea of late pebbles and
> instead just invoke planetesimals scattered inward, an inescapable byproduct of
> Jupiter and Saturn's growth (Raymond & Izidoro 2017). The authors already cite
> the Grand Tack as an alternative, but if there was a Grand Tack then convergent
> migration would not be needed to explain the terrestrial planets' orbital
> distribution.

Unfortunately, the Grand Tack scenario (or generally any scenarios
which invoke a truncation of the planetesimal disk) cannot simply
explain Venus--Earth separation as low as 0.3 au (Deienno etal. 2019).

> -- \water delivery to the Earth if the flux of icy/hydrated pebbles from >3 au
> remained sufficiently high for sufficiently long time." Isn't this idea directly
> contradicted by Brasser & Mojzsis (2020)? Is there a realm of parameter space in
> which enough mass in carbonaceous pebbles can enter the inner Solar System
> (regardless of the unclear delivery mechanism) without messing up the isotopic
> constraints?

We agree that this is potentially problematic point. We think that
isotopic constraints must be fulfilled for both mechanisms,
planetesimal and/or pebble delivery. According to very recent works,
see Johansen etal. (2021; https://arxiv.org/abs/2102.08611), please,
it seems that pebble accretion alone is capable to retain at least
some of the anomalies. In the revised manuscript, we thus kept
a restructured discussion of water delivery, where we mention
planetesimals more explicitly. It is in line with the modified
abstract.

> -- assuming pure ice impactors (f=1) is absurd. Anything above f=0.1 (typical
> value for the wettest carbonaceous chondrites) needs justification.

We modified the value to f = 0.1 and prolonged the time scale to 10^5 yr.

> Reviewer #3:
> Comments for the Author:
> I have read the revised version the manuscript. I believe the authors have
> addressed my comments on the earlier version. The revised version is now
> suitable for publication.

Thank you once again.

With kind regards, Miroslav Broz

>
>
>   [Part 2, Application/PDF (Name:]
>   ["19092655BZ_Broz_-_LM_1613757304_1.pdf") 228 KB.]
>   [Unable to print this part.]
>
>
>   [Part 3, Application/PDF (Name:]
>   ["NATASTRON-19092655B_Broz_AIP_-_Author_guidance_1613757304_2.pdf")]
>   [187 KB.]
>   [Unable to print this part.]