August 10, 2022

Responses to Journal Reviews

"Investigation of Impurity Transport in DIII-D Diverted Negative Triangularity Plasmas" authored by Sciortino *et al.* [PPCF-103801]

We thank Referee #1 for the positive review and for recommending the paper for publication. We also appreciate the comments by Referee #2 and #3 and hope to satisfy their requests in the latest version of the paper.

In what follows, we address comments and questions by Referee #2 and Referee #3, leaving **the Referees' comments in black** and adding our comments/responses in green.

Referee #2:

The requested major revision has not been performed. Instead, only minor edits have been made to the manuscript. As an example, no changes have been made to the abstract. As pointed out before, claims made in the abstract are not backed up by the findings presented in this paper. The conclusion that reduced impurity confinement is due to a missing main-ion pedestal is a "hypothesis,…, since the present investigation does not have sufficient experimental constraints" (as noted by the author). Moreover, two out of three experiments discussed in the paper do not even allow studies for rho>0.85. Hence, this statement in the abstract should be removed.

We have now removed any reference to our conjecture regarding main ion pedestals from the abstract.

Moreover, the discussion of "impurity seeding" in the divertor is still not addressed in detail in this paper and is certainly not the focus of this work.

We have now removed the reference to "divertor impurity seeding" from the abstract, as requested by the Referee.

Moreover, the first sections of the draft do still not have a clear direction and wander from an overview of energy confinement times in negative triangularity plasmas to a detailed discussion why H-mode is formed in one of the experiments.

We have re-organized section 2 with several subsections, which improve clarity and readability:

- 2.1 Energy Confinement

- 2.2 Setup of Impurity Transport Studies
- 2.3 Impurity Confinement Times

We have also eliminated Fig.3, which previously showed H89 vs. radiated power fraction, to make the manuscript shorter and easier to follow.

Then the manuscript spends time explaining generalities of impurity transport and modeling using the aurora package. Most of this discussion is not very helpful and makes the paper difficult to read. Considering the title and abstract, the paper should focus on the study of impurity transport in negative triangularity plasmas. For the first part of the paper, I therefore still ask to remove the part about general impurity transport and to revise the work with a focus on the discussion of plasma conditions that will be evaluated based on the LBO study discussed later.

To address the Referee's concern, we have reviewed each of these sections and eliminated some more sentences.

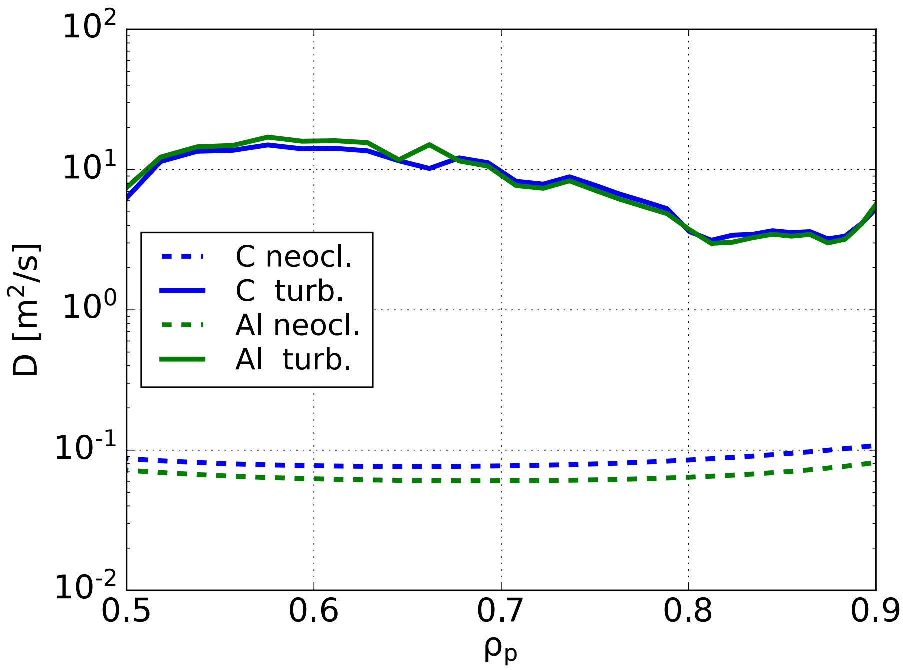
Now, the section on "Forward Modeling of Impurity Transport" is just over half a page; the "Bayesian inference" section is also half a page. They mostly contain critical information on choices that were made with Aurora and with Bayesian inferences that are different from previous work; i.e. they do not really contain redundant material.

The changes described above, separating Section 2 into separate subsections, have added greater clarity regarding the examined plasma conditions, and we hope that this will satisfy the Referee.

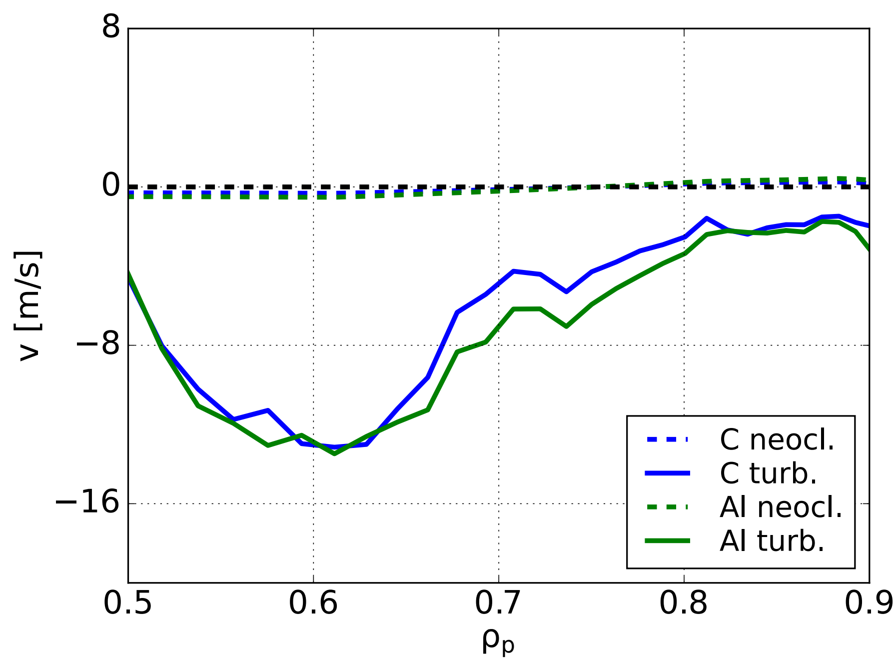
In addition, the new "multiply impurity CER" analysis method needs to be validated using the forward modeling tools at hand. It is not acceptable to refer to other publications given that the author has all tools at hand to perform this study. Moreover, previous findings have not considered the very same plasma conditions (e.g. negative triangularity plasmas) during which conditions might be different. The detailed comparisons of the neoclassical and turbulent diffusion and convection velocities of carbon and F and Al is therefore necessary. The author claims that this "adds extra computational complexity". However, this additional complexity is needed to motivate the new approach presented here.

The figures below show D and V profiles for one of our diverted negative triangularity cases (180526, t=2.75 s), obtained for C and for Al. Neoclassical components were computed via NEO, turbulent ones via TGLF SAT-2.

As expected, the differences between the two species are negligible for the purposes of experimental transport model validation.



10^2
10^1
10^0
10^{-1}
10^{-2}
D [m^2/s]
0.5
0.6
0.7
0.8
0.9
ρ_p
C neocl.
C turb.
Al neocl.
Al turb.



8
0
−8
−16
v [m/s]
0.5
0.6
0.7
0.8
0.9
ρ_p
C neocl.
C turb.
Al neocl.
Al turb.

In these figures, we are only showing the radial extent where TGLF provides reliable results, i.e. where growth rates are sufficiently high to avoid numerical issues. Further into the core, results are scattered, but the conclusion that D and V are very similar generally holds. Further into the pedestal, TGLF results are not applicable or not yet validated, and impurity transport is likely to be dominated by neoclassical transport.

We note that both near the magnetic axis and in the near-edge/pedestal region our Bayesian priors are much weaker, in the sense that we do not force different species to have identical transport coefficients. Hence, the figures above motivate and support our combined use of experimental data from multiple impurity species in Bayesian inferences of impurity transport coefficients.

The figures above are now presented and discussed in a new appendix - Appendix B: "Weak Variation of Turbulent Transport Coefficients with Z".

In the following, additional comments, not addressed in the new manuscript are reiterated.

Page 2: Please provide a reference for the statement that the experiments have dominant ITG modes (over TEM). The author should e.g. state that this finding is based on "own simulations, described in Section 5".

We have decided to eliminate the specification of ITG vs. TEM mode dominance on page 2, since this is an anticipation of what is discussed later in the paper.

Figure 4: There are no dashed lines visible when printing the document. The dashed lines are only there when viewing the document with Adobe reader.

This is clearly an issue with a printer, not an issue with the PDF that we provided. Nonetheless, we have now replaced the figure with a new version with increased transparency of the uncertainty bands and 50% thicker dashed lines in the bottom-right panel.

Figure 8: Plots for Al, F and C should be provided. Just because the author's previous work was focused on Argon impurity does not justify selecting a random impurity for this plot. This paper should focus on the study of C, F and possibly Al impurities.

We have now repeated the simulations for Al and substituted the plots in the paper. The text has been adjusted accordingly.

Page 7: Please provide a reference explaining why strong rotation provides a hollow Carbon impurity profile. Making this statement without a citation is not good scientific practice.

We have now addressed this point in some depth, adding the following to the manuscript:

"Comparing to the time traces in Fig. 1, we note that different NBI power is applied in each of these 3 time slices and this causes Te (top-left panel) and Ti (bottom-left) to vary significantly. Electron density (top-center) is highest in the discharge that entered H-mode, #180520, due to the formation of a density pedestal, whereas core electron density gradients are very similar between the three cases. The core Ti is much higher in the high-NBI case (#180530). Since the corresponding rise in Ti gradients is not balanced by an analogous change in main ion density gradients (closely related to the ne gradients, given the low Z_eff), the neoclassical temperature screening coefficient [27, 28] must also vary. On the other hand, the high rotation frequency (ω_φ, top-right) does not significantly contribute via neoclassical transport to the modified C density profile shape in #180530, since the "effective" Mach number of impurities (M_∗b in Ref. [29]), which determines the strength of centrifugal effects, is small for low-Z fully-stripped ions. Turbulence contributions to the impurity density profile shapes are discussed in Sections 4 and 5."

Page 16: The fact that sawtooth crashes affect the measurements should be better highlighted and addressed. What are the assumptions made in Aurora to model sawteeth? Has this model been benchmarked and what are the uncertainties that come with it? Please provide the AURORA-predicted evolution of the C+6 density during the sawtooth crashes.

The impact of sawteeth on measurements and modeling is very small. This can be best visualized in Fig.6, where the temporal and radial evolution of F9+ and C6+ densities (from both experimental data and Aurora modeling) is shown for one of the discharges that have a single sawtooth during the signal decay phase. The presence of such sawteeth cannot be distinguished by eye.

For completeness, we remark that we are using the standard Aurora model for sawteeth, described in [Sciortino et al 2021 Plasma Phys. Control. Fusion 63 112001]. The model is essentially the same used in STRAHL and fundamentally derives from experimental observations of impurity behavior in [Seguin et al 1983 PRL 51 6]: every time a sawtooth crash occurs, the density profiles of all charge states are flattened inside the sawtooth mixing radius (of course, with the total number of particles being conserved). In this work, the sawtooth mixing radius has been estimated from experimental (ECE) data, with a correction factor (deriving from finite spatial resolution of ECE) included in the inference as a free parameter. In the inferences discussed in this paper, this correction is well determined from the CER time-dependent intensity profiles and does not significantly affect results.

To address this point, we have now added the following sentence to the text, near the end of Section 5

"We note that, within our inferences, the presence (and modeling) of a single sawtooth in #180520 and #180526 does not add significant uncertainty to the inferred D and v profiles."

Page 14: Provide figures with the neutral density profiles considered for the analysis. In their response, the author shows figures from their Ph.D. thesis which obviously refers to a different plasma. The detailed neutral density profile for the conditions considered in this work are of interest and should be displayed in the paper.

There appears to have been a misunderstanding: the figure that we reported in our previous response is indeed for one of the diverted negative triangularity plasmas discussed in this paper.

Nonetheless, we decided to add more information about the neutral density profiles in a new appendix — Appendix D: "Radial Profiles of Edge Deuterium Atomic Neutral Density", where we provide exhaustive information on this matter.

Referee #3

This paper introduces the method of Bayesian inferences to process the impurity diagnostic data during negative triangularity discharges on the DIII-D device, and then studies the impurity transport in the negative triangular configuration. The paper uses a large amount of space to introduce the process of Bayesian inferences processing impurity diagnostic data in great detail. The application of this new method and tool has a very important reference value for researchers studying impurity transport.

The transport results from the Bayesian inferences were also compared with the results of several simulation codes. And in this way the authors tried to find the dominant physical mechanisms that affect the transport of impurities in the core of the negative triangular configuration. Such an attempt is necessary and useful. However, the current limited negative-triangular configuration discharge experiments of DIII-D cannot actually satisfy the adequate study of these complex physical problem. Therefore, in this paper, the problem of impurity transport is limited to the description of the apparent phenomenon and the extremely limited analysis of the physical mechanism, lacking sufficient experimental data and analysis.

Nonetheless, this article is still very valuable and worthy of publication in PPCF. But I suggest that the authors focus this paper on the case of Bayesian inferences dealing with discharge states of many different negative triangular configurations, and obtaining the basic characteristics of impurity transport under negative triangular discharges, and unhighlight the discussion on the transport mechanisms. After all, the authors do not have enough experimental data to support it, nor can they draw very robust conclusions. Preliminary studies of these physical mechanisms can be included as part of the Discussion.

We thank the Referee for these suggestions. Unfortunately, it is not possible for us to examine "many different negative triangularity configurations", because we do not have so much experimental data. In subsection 2.2, we now have the following sentences:

"Although our investigation would have benefited from injections into each step of the NBI power ramps, this was in practice not possible during this experimental campaign. Among the LBO injections that were executed, we consider here only those with high data quality, aiming for an effective comparison between transport theory and experiments in a variety of plasma conditions"

We note that older DIII-D inner-wall-limited negative triangularity discharges had a much higher Zeff, which makes it difficult to rigorously examine trace impurity transport at the level of detail that we delve into for this paper. In other words, we cannot really examine a wider dataset at the moment, although this will certainly be attempted during a future DIII-D negative triangularity campaign.

Given the data available, we have decided to first focus on a qualitative overview of impurity transport (observation of flat/hollow carbon profiles; short impurity confinement times), and then to present an in-depth comparison of our experimentally-inferred D and V radial profiles with predictions of theoretical models (NEO, TGLF, CGYRO). We consider the 3 examined cases to be case studies for the validation of theoretical transport models in a scenario where they have been hardly ever experimentally tested. The results of this work suggest that these models are generally adequate to describe transport in the DIII-D diverted negative triangularity discharges, but our focus here has not been to discuss the fundamental transport mechanisms at play - e.g. whether changes in trapped particle orbits affect impurity transport. We agree with the Referee that this would be excessive for this experimental/modeling paper.

The discussion on transport theory that we currently have in the paper is aimed at offering a valuable comparison to the results of our Bayesian inferences, which are the key analysis tool developed for this study. We hope that this clarifies the reasons for the structure/scope of the present paper.

At present, the paper is very complete, but before the paper is accepted, I have the following suggestions.

(1) The title of the paper is too broad, and the conclusion of the paper cannot support such a title. It is recommended to change it to "Primary investigation of impurity transport in DIII-D diverted negative triangularity plasmas via Bayesian inferences" or other suitable titles.

We appreciate that the previous title was too broad. On the other hand, it is quite appropriate to describe this work as "an investigation". Qualifying this as a "primary" (i.e. preliminary) study seems quite reductive of our work. In the new version, we have added the specification that this is an investigation of *core* impurity transport, e.g. not a discussion of divertor impurity seeding in negative triangularity discharges. We hope that this will satisfy the Referee's request to be more specific and limit the scope of the paper title.

(2) In the abstract, "Lack of central impurity accumulation and short impurity confinement times (compared to energy confinement times) are conjectured to be due to lack of a main-ion density pedestal." is just a conjecture, and the paper does not fully demonstrate at this point. This does not fit in the abstract.

We have now eliminated the conjecture from the abstract and modified the text appropriately.

(3) In section 2 - Description of Experiments, there are many experimental results in this section which looks like an experiment report rather than a scientific paper. It would be better to added several subtitles and reorganize the sequence of the figures to make it easier to read.

We have now made a number of modifications to address this issue. First, we have re-organized section 2 with several subsections:

- 2.1 Energy Confinement
- 2.2 Setup of Impurity Transport Studies
- 2.3 Impurity Confinement Times

This re-organization is definitely helpful to improve clarity and readability.

Secondly, we have eliminated what used to be Fig. 3, showing H89 vs. Prad/Pin, since the content of this figure could adequately be described in the text (now amended).