In agreement with Roberto Sommariva, I have severe concerns regarding the publication of this manuscript under given circumstances.

License:
I agree with Roberto that a GPL license might be more appropriate. In addition, the authors of this manuscript want to make code publicly available which has however (at least partly) not been written by anyone of them.

Author contribution:
As the authors themselves emphasise that "the paper develops the branch of MISTRA based on von Glasow (2000)", it should be pointed out clearly and unmistakably (in the manuscript text itself as well as the author contribution section) that development and application of MISTRA took place under the lead of Roland von Glasow.

Overall, this manuscript appears to me to be half-hearty written. I explain in the following why I come to this conclusion:

Just listing a number of publications written based on simulations using MISTRA (p.2, l.54 - p.3 l.67): (i) is from my point of view not sufficient for publishing the code as the variety of scientific applications is only vaguely mentioned (ii) does not value the work of those people who worked intensely with MISTRA (in Roland von Glasow's working group, first at the university of Heidelberg, later at UEA in Norwich) and all of them contributed to the continuous development and improvement of MISTRA.

My own work is cited wrongly (p.3, l.63): One of my publications with Roland deals with organic surface coatings on sea salt aerosols (this is not mentioned at all) but in the manuscript both citations (Smoydzin and von Glasow, 2007 and 2009) refer to chemistry over the Dead Sea. In addition, I did not implement an "ocean model" into MISTRA; I wrote a code making it possible to calculate chemistry in a liquid medium (i.e. the Dead Sea) and to calculate air-sea exchange of gas phase species explicitly!

Calling Susanne Pechtl's work "an improvement of iodine chemistry" (p.7, l. 167) is - politely spoken - an understatement.
She developed a completely new aqueous phase iodine chemistry mechanism coupled to the gas phase (and the existing chemistry scheme) which was unique and new in atmospheric chemistry research - and which is an essential and outstanding part of MISTRA. In addition, she was the first who investigated the nucleation potential of iodine species and wrote this part of the code in MISTRA which the authors now want to make public.

Roberto Sommariva wrote a comment himself. He added a substantial part of code to MISTRA and must be offered co-authorship of the manuscript.

Matthias Piot significantly changed the MISTRA code to apply the model at Arctic conditions. This part of the model development deserves from my point of view more credit than just a citation without any further explanation of this work (neither a co-authorship).

I am sure, a careful review of the work done by Roland and his co-workers would reveal other code contributors.

A few comments following the review by Rolf Sander:

Rolf suggests to call MISTRA a "tropospheric model" instead of "atmospheric model". Though, this expression is even more wrong. MISTRA (the 1d column version) is a model of the atmospheric boundary layer. It cannot be used for studying chemistry in a column reaching from the surface into the free troposphere or even up to the tropopause as the upper boundary conditions are not suitable for such an application (from a physical/numerical point of view).

This fact answers also one of his questions:"... does it mean that there are not aerosols above clouds?" If (boundary layer!) clouds are simulated, they usually reach to the top of the boundary layer, thus to the top/upper boundary of the column model.

Rolf further suggests to compare MISTRA with 0D box models: At first, it should be clearly pointed out, that MISTRA can be used both, as a box model (a comparison with CAABA/MECCA might be obvious) and a column model (which is rather unique).

As mentioned above, a more detailed description of the possible scientific applications of MISTRA (marine halogen chemistry in coastal regions, volcanoes, Arctic applications) would also be desirable as well a description of typical model setups (e.g. using the column or box in a pseudo-Lagrangian way as done in many studies discussed in the MISTRA publications).

A minor comment:
There is a typo in the reference of Joyce et. al (p.31, l.510)