R1:

First, we would like to express our sincere gratitude for this detailed comprehensive review. This will surely help improving our work.

We argue that the majority of these remarks are on rather simpler issues, and these are really easy to fix, and this goes without saying: we will not only exhaustively address them, but also scan through the whole paper from top to bottom to guarantee its quality.

Yet, we should kindly state that among these complaints, we could not find any substantial technical justification (e.g., scientific incorrectness of results) for offering the rejection of the paper. In the following, we will share our view on these major criticisms, and elaborate in this limited-space:

-The Reviewer argues that "The paper is not self-contained" and mentions "the difficulty of understanding 'resolution + transformation rules' we refer from [Zhang et al., 2014]"

We are unable to see the point of re-writing all these technical machinery in a conference article (rather than citing them) while they are published in a large journal article which is already publicly available.
We understand the natural difficulty of grasping when reading all these technical elements fragmented in various papers, however going through all of them in a conf-paper, let alone self containment for this work (preliminary + technical contribution) is really not in the region of feasible. We sincerely share that we did not have this option here.

-On criticism "The practical use.. its complexity theoretic consequences remain hazy..."

In many resolution calculus studies in the literature, potential exponential blow-up is expected (and assessed in its own vein). By [Zhang et al., 2014] we already know that the resolution process will terminate for any CTL formula. Practically, depending on the resolution process for a particular setup, many clauses would be deleted, and IF many atoms are aimed to be forgotten, THEN surely it will become more manageable in most cases.
Indeed, we share the enthusiasm of the reviewer in providing a bound, which would be more elegant. As always, there is a room for improvement. A great avenue for the future work.

Concerning some technicalities:

-On semantics of SNF_CTL^g, "..unclear..."

We weren't expecting any confusion here, since we point the semantics of CTL is contained in SNF_CTL^g semantic [Zhang et al., 2014].

-On "Example_4, p.6: give reasons.."

In our example, we point that there are two clauses "\top->\neg y\vee q" and "y->AX(q\vee f\vee m)", then we know that "y->q" and "y->AX(q\vee f\vee m)", which means "y->q\wedge AX(q\vee f \vee m)".

-About the function "simp": this function is to simplify CTL formulae. It is said that throughout the transformation the formulae are kept in NNF. In such a case, it won't be needed.

On b)
By [Zhang et al.2014], for each CTL A, there is always a sequence Se of new atoms introduced by the transformation process for each sub-formula in A. After the removing_atoms process, we obtain a set B of clauses. Given a model M of B, when constructing a model M' of T_A from M we can always find some atoms that do not be instantiated to add or delete to satisfy the deleted clause along the Se. We will mention this intuition.

R2:
Thanks for the kind comments and the appreciation.

R3:
Thank you for your encouragement and seeing a value in our work. We agree that the empirical evaluation would be highly relevant and in our agenda as well. At this very moment, we should share that the empirical evaluation is not ready for presentation. Even if not in this single step, we surely will investigate it in a next step throughout our journey.