Comments to author (Associate Editor)

The paper deals with the feasibility in externally switched systems. Instead of considering all possible switching signals that leads to the curse of dimensionality, the switching signal is constrained by an automaton. One iterative algorithm is provided to compute the time-varying control invariant sets. Three reviews have been obtained. The reviews mention interesting results, however the readability should be significantly improved to present correctly the contribution. It includes notation and developments of the paper. The reviewers propose a list of constructive suggestions and also of concerns that should be taken into account.

Reviewer 1

1. In section II-B the definition of the constrained switching signals is redundant with the section II-A.

   Removed the description in section II-B and improved readability in both sections mentioned.

2. Notation |\mathcal G| is undefined. Does it denote the cardinality of a finite set. In that case, I am not sure how I should understand |\mathcal M|

   Originally, this was meant to define the number of nodes in the graph while |\mathcal M| defined the number of modes in the system. This notation has been replaced, however, with G and M (2nd paragraph of Section II.A, 1st paragraph of Section II.C).

3. In section II-A, the notation \overline \mathcal G is used. However, I have no clue what it stands for.

   This was a typo. It has been removed.

4. After definition 2, you set that the definition holds regardless of the preset operator. This simply untrue. Because it should be:
   1. nominal: \forall x, \exists u, f(x,u,0) \in S
   2. robust: \forall x, \forall w, \exists u, f(x,u,w) \in S
   3. previewed: \forall x, \exists u, \forall w, f(x,u,w) \in S

   The comment was supposed to only relate to the pre-set condition of CI sets. Placing right under the definition, however, was confusing. Since it is not relevant to the results, it has been deleted in this version of the paper.

5. This is just what concerns page 2 of the paper. However, there are also problems on other pages. The authors should carefully proofread their paper to improve the writing.

   We appreciate the reviewers comments on page 2 and have combed through the remainder of the paper for additional errors in the paper's grammar and notation.

6. The authors seem to ignore the abundant literature on switched systems with graph constrained switching signals, see e.g.
   1. Athanasopoulos, Nikolaos, Konstantinos Smpoukis, and Raphaël M. Jungers. "Invariant sets analysis for constrained switching systems." IEEE Control Systems Letters 1.2 (2017): 256-261.
   2. Philippe, Matthew, et al. "Stability of discrete-time switching systems with constrained switching sequences." Automatica 72 (2016): 242-250.
   3. Fiacchini, Mirko, Marc Jungers, and Antoine Girard. "Stabilization and control Lyapunov functions for language constrained discrete-time switched linear systems." Automatica 93 (2018): 64-74.

4. Athanasopoulos, Nikolaos, and Mircea Lazar. "Stability analysis of switched linear systems defined by graphs." 53rd IEEE Conference on Decision and Control. IEEE, 2014.
5. Lee, Ji-Woong, and Geir E. Dullerud. "Uniformly stabilizing sets of switching sequences for switched linear systems." IEEE Transactions on Automatic Control 52.5 (2007): 868-874.

   Some of these works are, indeed relevant to the submitted paper. I had incorrectly thought that the graph based constraints used here were somewhat uncommon in switched literature. They are now referenced in the introduction when graph based constraints are introduced. However, the objectives and systems in these papers where different enough that we do not feel like they warranted in depth discussion.

Reviewer 2

We appreciate reviewer 2's remarks. However, they did not contain any critical feedback for us to address.

Reviewer 3

1. First of all, I find your work and referring to the existing results incomplete:
   1. I would recommend adding the "motivation applications" part to the introduction.

      A motivating application would be helpful to contextualize this work. However, page limits prevent us from adding one.
   2. Please provide more details on how your paper distinguishes from [6,7,8] and improves [3,5].

      Note that the indicated references now correspond to references [12,13,14] and [8,11]. The approaches used in [12,13,14] compared to the one suggested in this paper are quite different in varying ways but the most important is the lack of parallelization in their results. This leads to either conservatism or numerical intractability. This is noted in a sentence added to the introduction that reads "Furthermore, these previous works did not provide a framework to parallelize their results, leading to either increased conservatism or increased numerical complexity."

      This submission and reference [5] (now [11]) are approaching the same problem from different directions. Therefore, this submission is not meant to improve [11]. This is now called out after [11] is first referenced.

      Of all the cited works, [3] (now [8]) and related ones most closely resemble this submission. However, there are key differences. Primarily, these previous works took a centralized approach and can not be called to system's in higher dimensions. Furthermore, they considered a single switching signal only. The literature review has been improved to clarify these points.
   3. Please consider comparing your results with the results of the assume-guarantee contracts community [see the works of Adnane Saoud, for example]

      Thank you for drawing our attention to this body of work which I was not familiar with. In a longer work, these should be included in a literature review. However, the similarities compared to the submission did not seem to be strong enough to warrant mentioning them here where space is at a premium.

    4. There are several statements about existing results with no references at all. For example,
        1. "A further shortcoming of the current literature is that only a single switching signal is explicitly considered." in the introduction.
        2. "Using directed graphs to constrain the switching signal generalizes dwell time and successor constraints from previous literature, representing a richer set of constraints" Section II subsection B.
        3. "This structure is motivated by distributed systems with coupling in the system's states." Section III.

        We added references to points 1 and 2 to make the connection to previous works explicit. However, point 3 is more an "internal reference". That is, we were motivated to study problems in this form by related projects we were looking at. Therefore, no references were added here.
    5. I believe you also should pay more attention to distributed system community in the introduction.

        This paper certainly takes some queues from the distributed system community. Once again, however, the allotted space precluded a more in depth discussion in the relationship between the two. The paragraph starting with "A better solution for a system..." in the introduction (with its associated changes in response to feedback 1.2) must suffice.
2. Section II subsection B. In my opinion, your usage of a directed switching graph to generalize the dwell time is incorrect. At least if you work with a continuous and not discrete system. Indeed, continuous systems allow switchings instantaneously. It is unclear what you mean under "odd dwell time or minimum dwell time of 2". Please mention this directly if you consider a time-sampled system. And I recommend you prohibit the system switch "too often" to avoid the Zeno effect.

    The entire paper is meant to be viewed in the discrete context. This is mentioned in the first sentence of section II.B. To address the confusion related to "odd dwell time or minimum dwell time of 2", the caption has been reworded and a comparison is made to [8].
3. Please could you introduce the model in Section III properly? It is not clear what a state is or what an agent is? How (3) is related to the definition of $\mathcal{M}_{\mu}$ you provide in Subsection C of Section II. I am guessing you are splitting the system into N_{alpha} subsystems and suppose that every subsystem is aware of its proper switching signal. It would help if you also emphasized that you only consider linear systems.

    The beginning of Section III has been heavily rewritten to ensure the model is introduced correctly. After an opening paragraph, the model is introduced as "a linear system partitioned into N_a agents" and it goes on to define their state spaces. After that, full, centralized description of the system is given. This should greatly improve the section's clarity and readability.
4. In (4) it is not clear what you denote as $E^{\alpha}_{\sigma_{\alpha}^\mathcal{L}(t)}$ and what you denote as w(t). Please, consider two provide more information about the size of these matrices/vectors. Please illustrate Algorithm 2 with a figure. And also comment on it line by line. It is hard to get. I was one step from rejecting your paper because of that!

    I separated equation 5 to hopefully make the definition of the E matrix more clear. Furthermore, a figure was added demonstrating one iteration of Algorithm 2 to provide intuition about what is going on.
5. Please write down the equations at least for one node in Section V. It also makes sense to comment again on your choice of "block-row" structure for an interconnected system.

Sorry, but I believe that a significant change in the mass of an agent will affect the dynamic of other agents. However, if I understand correctly, the latter can not be modeled within your framework. Could you comment on this limitation?

A single equation has been provided (symbolically since the numerical values are unimportant). The forces applied by the springs and the dampeners are generated by the difference between the position and velocities of neighboring agents. This can be viewed as a negative force based on local perturbations from equilibrium, and a positive force based on the neighboring perturbations from equilibrium. The neighboring perturbations are considered as previewed disturbances. Therefore, changing the mass of a neighbor will change how the local previewed disturbances evolve but not their effect on the local agent.

6. Please consider providing a run-time comparison of your approach with the existing results. You use a parallelized approach, and I believe as many agents you have as faster your computations are. However, on the other hand, since agents are not aware of switching signals of one another, it increases the conservatism of your approach. Could you comment on this too?

    As mentioned in the numerical example, the system under examination is too large to compare with similar works such as [8] and standard, desktop computers. This point is further emphasized with the inclusion of the comment "Furthermore, to combine all the switching signals would require a constraint graph with almost 40 billion nodes". The conservatism related to ignoring the neighboring switching signals is addressed in the final paragraph of Section III where motivation for the chose solution is provided.

7. The paragraph with the definition of a directed graph and switching signal mapping this graph at the beginning of Subsection B of preliminaries almost repeats one from the "Notations" section. I believe one time (in section B) is enough.

    This was certainly redundant. The portion in II.B has been removed.

8. In Defenition 2, I recommend keeping only the last sentence since you should provide different quantifiers for disturbance depending on the type of a pre-operator.

    I rewrote the definition and eliminated a troublesome comment following it. It should be correct. We chose to include more details than suggested here to draw a more direct comparison between nominal and preview-robust CI sets.

9. What is \overline{G} in Section II subsection A and B?

    That was a typo and has been removed.

10. Algorithm 2 line 3, you repeated \alpha two times.

    This typo has been addressed.