**Editor**

I hope this message finds you well. I am now enclosing the comments that the 3 original reviewers have made on your revised manuscript. I apologise again for the extended time taken to obtain these.

As you will see, referees #1 and #3 and now more positive, while reviewer #2 remains sceptical that your analysis meets Joule'sdemanding requirements.

After discussion in the Joule editor team, and mindful of the topical nature of this work, we are willing to somewhat overrule the negative reviewer and we would be happy to consider a revised version of the manuscript provided that you quickly make a good-faith effort to address the remaining concerns of the reviewers. We feel much of this can be done with textual changes highlighting the limitations of your analysis. We understand that some revisions take time, but I should mention that we take into account the published literature available on the day we make our final decision. We suggest a one week time frame here. If you think you would need more time, please let me know. We understand that the global COVID-19 situation may well be causing disruption for you and your colleagues. If that is the case for you and it has an impact on your ability to make revisions to address the concerns that came up in the review process, please let us know so we can discuss with you about what types of revisions you would be able to do and work with you on a plan for moving your paper forward.

**Reviewers' Comments:**

Reviewer #1:  
  
I thank the authors who have responded to most of my comments by either extending their analysis or explicitly stating in the report where certain assumptions have/have not been made. The emphasis that early adoption of low carbon technologies (i.e. not using gas as a "transition" fuel and risk stranding assets) can lead to lower cost power system (and energy system?) costs is a really significant and important result.  
  
I am still sceptical about not having any constraints in the modelling framework for the growth of new technologies and the decline of incumbents (i.e. to reflect system inertia). I understand and largely agree with the authors point that the rate of deployment of low carbon technologies will in all likelihood have to be beyond any historical transition if 1.5 degrees (in particular) is to be met, and agree that the deployment of these technologies is highly uncertain in terms of the S-curve diffusion pathways. However I would like to have seen at least one sensitivity where some of these constraints are implemented. In my opinion, a range of constraints (either implemented via different S-curves or singular growth rates) via different sensitivities provides a more feasible set of results than no constraints at all (e.g. the deployment trajectories for wind and solar techs shown in Figure S3 are hugely volatile).

Comment:

It is a deliberate choice we have made to not constrain renewable build-out rates. We fully agree that a constraint on build-out rates has a huge impact on both the model results and the practical implementation. Since the growth and installation rate of renewables is highly uncertain we have chosen not to implement a constraint. This allows us to study the cost-optimal build-out rates which we believe is valuable, although perhaps too optimistic. Choosing not to constrain build-out rates of course means that we don’t get to study the “best compromise” solutions required when renewable build-out is constrained. To elaborate on these limitations of our study section 2.6 of the manuscript has been expanded. See line 301 to 336 in the manuscript.

On Figure S3 I also do not understand the negative values for annual installation rate - is this a net retirement? However I do understand that this is a report and not a full length article and therefore this could be work for a future full length paper and further exploration of this issue.

Comment:

The figure legend has been changed to indicate that net build-out rates are shown. The legend now reads:

*“Net build-out rates of renewable and heat-pump capacity. Heat-pump capacity is measured in terms of electrical energy. Technologies are decommissioned at the end of their lifetime resulting in net negative build-out rates in some years”*

Due to a restructuring of the supplementary material the Figure is now named D13.

Reviewer #2:  
  
The author present an improved version of their analysis of the mid-term and long-term impact of the reduced gas imports on the decarbonization of the European energy system. Compared to the previous version the authors submitted, the manuscript has improved. However, the manuscript, in my opinion, still reads as a preliminary, partial analysis of this issue at hand. It may be suitable in the present form for a publication in another outlet, but not in a high impact, general interest journal as Joule. I have the following comments:  
  
1/ As the authors state themselves in the response to one of the reviewers' comments, the "the scientific motivation and novelty of the paper is to study how the decarbonization of the European energy system is affected if natural gas is limited in its ability to play a key role as a transitional fossil fuel". To rigorously answer that question, the presented analysis does not suffice. A key drawback in this regard would be the unlimited build-out of renewables, which, given the resolution of the model, serve as a substitute for natural gas. The robustness of your findings w.r.t. these build-out rates is, in my opinion, critical. Similarly, the relation between the carbon budget, renewable build-out constraints and gas availability is not really explored, yet, this could yield interesting insights.

Comment:

It is a deliberate choice we have made to not constrain renewable build-out rates. We fully agree that a constraint on build-out rates has a huge impact on both the model results and the practical implementation. Since the growth and installation rate of renewables is highly uncertain we have chosen not to implement a constraint. This allows us to study the cost-optimal build-out rates which we believe is valuable, although perhaps too optimistic. Choosing not to constrain build-out rates of course means that we don’t get to study the “best compromise” solutions required when renewable build-out is constrained. To elaborate on these limitations of our study section 2.6 of the manuscript has been expanded. See line 301 to 336 in the manuscript.

2/ I'm missing a discussion of the limitations of the modeling approach. I'm a bit skeptical w.r.t. whether the employed model is a suitable tool to analyze the impact of dunkelflautes (Fig. 5).

Comment:

Our employed model (PyPSA-Eur-Sec) represents state-of-the-art modelling and includes uninterrupted hourly simulation for the full year. We consider this is the best approach to properly model dark doldrums.

3/ The discussion of the methodology ("Experimental procedures") mentions myopic optimization, but at the same time "long-term market equilibrium, perfect foresight and perfect competition". If the optimization is "myopic", can we then meaningfully compare results? The results no longer reflect the "true" optimum, hence, difference may be driven by a different impact of myopia with or without the gas supply constraint.

Comment:

Although using a myopic approach in every modelling horizon throughout the transition is less optimal than assuming perfect foresight, the former captures better the short-shighted behaviour of policymakers and companies in the energy sector. Hence, we believe it is a good approach to model the transition in Europe.

Experimental procedures text on energy system model has been updated. The following has been added (lines 459 to 465):

*“PyPSA-Eur-Sec is a least-cost linear optimization model representing the energy supply of 33 countries within the ENTSO-E. It performs myopic optimization in 5 year time-steps over the period from 2020 to 2050, by solving a single year to optimality for every 5 year period. For each 1-year simulation the model assumes long-term market equilibrium, perfect competition and perfect foresight.*   
  
  
Reviewer #3:  
  
The authors have thoroughly revised the manuscript and have made major changes, which improved the article significantly. The novelty is much clearer stated now and also emphasized in the results. I understand that the report format has length restrictions, so I think, given the urgency of the issue, publication of the piece is warranted. However, there are still some issues that should be addressed:  
  
First, and this is the most technical comment, I only understood now that the model does not explicitly model the gas flows in Europe. This may have fundamental impacts on results. Transmission capacities in Europe are very limited, and e.g. Germany or Austria may suffer much more seriously from a supply shortage than the Western countries, which partly have access to LNG terminals. Unfortunatley, I cannot derive from Fig. 4 where the remaining gas is used, as only additional annual costs are shown. These can stem from larger investments into capacity or from higher gas prices. I'd actually like to see a figure (perhaps in the appendix) that shows gas consumption over time for the most important countries. Furthermore, it should be discussed if this pattern is supported by current gas infrastructure in Europe. If not, gas consumption in some regions should be capped more than in others, if the implementation of at least a simple gas transportation model is infeasible.

Comment:

National gas consumption for the most important countries from 2025 – 2040 are now added as supplemental figures D24-D27. We have commented on the results and compared it with current European infrastructure. Furthermore, we have mentioned the copperplate transport model as a limitation to the study under “Experimental procedures”.

Second, it seems that you are using only one meteorological year to run your model (2013, if I read the manuscript correctly). Is there a particular reason for chosing 2013? I think testing, in a sensitivity analysis, how the model performs in a - maybe artificially - low wind/PV supply scenario would be a good assessment of the robustness of your results`ñ against annual variations in the availability of variable renewable energies.

Comment:

To clarify, text have been added to experimental procedures section on model (lines 474 to 475):

*“A single weather year has been used to generate the renewable capacity factors. Here 2013 was chosen as it represents a year with relative low availability of wind and solar [7].”*

Third, Austria and Belgium closed their last coal power plants recently. Where does the additional coal use come from in your model? Are new coal power plants built? I would deem this infeasible.

Comment:

You are right that Supplemental Fig D.15 (previously named Fig.S5) shows a higher coal capacity for the 2C-constrained scenario than for the case with an unconstrained gas supply. Model-technical wise, old capacity is decomissioned while new capacity is installed. However, this should not reflect newly built coal power plants, but instead a prolonged lifetime of an existing coal power plant. A measure (to have planned-decomissioned plants to stay standby) that is already taken in some countries to ensure energy security in case of a gas shortage

(<https://247newsagency.com/top-news/143230.html>, <https://www.nytimes.com/2022/05/25/business/germany-plans-to-keep-coal-fired-plants-ready-in-case-russian-gas-is-cut.html>, <https://www.bbc.com/news/business-61256615>).  
  
Fourth, a statement in terms of the framing of the paper. You write: "Conversely, for the 2ºC pathway, the future system cost is less certain, as it depends on exterior political conditions, i.e., whether sufficient gas can be imported."  
This somehow gives the impression that the 1.5°C is the scenario of choice, but actually it comes at substantial cost and high uncertainty in terms of feasibility due to high build-out rates. I think in the abstract, the main conclusions could be presented in a more balanced way. Perhaps put a focus on electricity and gas prices, which seem to be pretty strongly affected in the 2°C scenario, much stronger than total system cost.

Comment: The main conclusions in the summary have been modified to include discussion on build-out rates, as well as a greater focus on electricity and gas prices in the 2°C scenario:

*“We find that the 1.5 ◦C pathway quickly relieves Europe’s dependency on imported gas, but relies on high build-out rates and has high investment costs. In, the 2 ◦C pathway gas and electricity prices are more affected by the uncertainty in gas supply.”*

Fifth, you write "In both the 1.5C and 2C scenarios, this results in lower annual build-out rates as the transition is performed more gradually."  
Yes, maximum growth rates are lower, but growth happens earlier, so ramping up has to be done quicker. This should be mentioned.

Comment: We clarify the need for faster initial ramp-up, and add more discussion on the growth of build-out rates required.

Sixth, why are deployment rates of most low-carbon technologies in Fig. S3 in the 1.5D scenario lower when less gas is available? I would have expected it the other way round.

Comment:

When less gas is available in the 1.5d scenario, the build-out of renewables becomes more gradual. Although deployment is lower in 2030, it is higher in 2025, and 2035. Thus, similar amounts of low-carbon technologies are installed, but in the gas constrained scenario the transition is more gradual.