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Review of: The impact of shale gas on the cost and feasibility of meeting climate targets - a global energy system model analysis and an exploration of uncertainties

[Note: responses to this review have been added in red underneath each comment]

Overall remarks:

This report is a timely and well-thought research into an important topic that has not been thoroughly researched in the literature. What is the role of shale gas in the deep decarbonization world? Does low cost shale gas help us reach the decarbonization goal? This is a variation of another topic that has been thoroughly researched: the role of shale gas in the world without decarbonization policy. In that line of research, literature has found that the abundance of shale gas alone will not help reduce GHG emissions. It is natural to further inquire whether the shale gas will help reach a climate target provided that there are globally concerted efforts to reduce climate change. This report provides one crucial building block for better understanding the intersection between shale gas and climate change.

The key findings of the research is as follows:

1. Natural gas can be a key bridge-fuel in the near-term, but it must be phased out of the economy in order to meet deep decarbonization target.
2. The role of shale gas under deep decarbonization is highly uncertain, but it is unlikely to significantly reduce the cost of deep decarbonization.
3. In “worst case scenarios”, in which either shale gas has high methane leakage or hinder capital investment in renewable energy, high utilization of shale gas could indeed make deep decarbonization more difficult.

In the hindsight, these are very obvious findings that could be simply deduced from a thought experiment. However, demonstrating these effects using a formal model is critical in advancing the scientific understanding of the issue. This report makes a critical contribution in that front.

This report contains two parts: literature review of shale gas and modeling of shale gas futures. Literature review is comprehensive and thorough ranging from government reports to peer-reviewed academic articles. Modeling analysis is well designed and executed. The results from the modeling analysis are clearly presented in-depth. There is abundance of sensitivity analysis to assess the robustness of the findings. The conclusion section draws important findings from the analyses and places them into a broader context.

There are two critical weaknesses of the report. First, the study can definitely use a wider variation in natural gas supply curves. It’s important to remember why this line of research came about. Even as late as 1990’s, majority of researchers and stakeholders did not believe shale gas could be a major contributor to the world’s energy supply. But then, everyone was caught off-guard when the shale gas revolution unfolded in the U.S. Acknowledging that the energy research community as a whole inadequately understood the shale gas potential, I believe it makes sense to seriously consider the future possibilities far outside the comfort zone. The report as it stands now has well-designed sensitivity analysis on shale gas supply curve, but the range of variation in high vs. low gas supply is quite narrow. This is evidenced by the small changed observed in the gas consumption output. What if the shale gas revolution were to go through another successful phase, this time globally, and as a result what if the global gas consumption were to double or triple? Would the report's findings be robust under such extreme circumstances? For a report focusing on the effect of shale gas, looking into such far end of the possibility frontier would add much value to an already solid research.

Second, The results section is very numerics heavy, but it's quite short on decomposing the mechanism behind the numerical results. The report would greatly benefit from adding in more plain-word reasoning behind some of the interesting dynamics observed in the numerical results. For instance, how does low cost shale gas differentially affect different sectors such as electricity and transportation? Why is the mitigation cost non-monotonic function of gas supply? If there is an optimal level of gas supply that minimizes the mitigation cost, what do we know about its level and characteristics? Why does shale gas displacing coal not reduce the cost of mitigation? Or conversely, if shale gas must be phased out in the long run, why isn't low cost shale gas making it more significantly expensive to drive them out of the system? Simulation model is simply a tool to better explain the dynamics under which the energy system operates. There are numerous questions that this research can shed lights on that goes beyond the simple numerical output. It would be great to focus a bit more on the explanation and a bit lighter on the detailed numerical results (which could go into supplementary material or appendix).

Exact numerical values were explicitly requested in feedback on a previous draft of this study. However, we have added a more explicit caveat at the start of the "Results" section to indicate that these are the results of one modelling study, and that more value should be attributed to directions and orders of magnitude of impacts than exact numerical values.

Other suggestions are addressed in comments throughout.

I believe improving upon these two weaknesses of the research would make the current report a much stronger product. Below I provide some detailed suggestion for improving the current paper.

1. The literature review is reasonably up to date, although the understanding on shale gas extraction is changing rapidly. There is some room for incorporating more recent understanding.

We have made use of the most up to date literature which we were able to find publicly available. Having made some further checks, there is to our knowledge no additional comprehensive global supply curve data for shale gas (at least not on the extraction cost basis that our energy system model uses) that is available to add to our analysis. We understand the degree of uncertainty in the analysis and have chosen a reasonable illustrative range of supply curves, which do cover the possibility that – in the cheapest cost bands – shale gas is in general less expensive to extract than conventional gas. This allows us to understand the implications of this less expensive shale gas being extracted.

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2. There is large uncertainty in methane leakage. I do think the report is not completely neutral on this issue. The authors focus heavily on the possibility of high leakage more than the low leakage. However, this bias in approach is sufficiently caveated when describing the findings from uncertainty analysis.

Methane leakage is an area in which there are significant disagreements and inconsistencies within peer-reviewed literature, and we feel we have adequately represented the range in the literature. For our model runs, we have illustrated a range of scenarios for which the highest leakage rate is well below the highest rates suggested in this literature. We now acknowledge more explicitly that the highest leakage rate scenarios may not occur, should sufficient mitigation measures be put in place (see Section 3: Methods). There is not yet sufficient detail available in plans for emission reductions (particularly surrounding well-monitoring when using large numbers of wells, as typically required for shale gas extraction), so whilst our higher leakage rate scenario is not a forecast of the probable future, it serves as a useful illustration of the degree to which inadequate control or mitigation of fugitive methane is important to any large-scale extraction of shale gas. We have further emphasized this point in the updated paper (Section 3: Methods and Relevant results sub-section: 4.8).

3. Integrated assessment modeling is primarily used as an analytical approach. Considering the integrated nature of the topic: energy sector impact on greenhouse gas emissions, this is an appropriate analytical tool for assessing the issue.

4. The choices of scenarios used in the analysis are quite comprehensive. There are more than 20 scenarios analyzed. My main concern here is that the variation across scenarios are quite small. It could be worthwhile to explore the far limits of the possible shale gas supply, and see if the findings are robust.

As per our response to point 1. above, our scenarios are based upon the range of possible levels and costs of shale gas supply presented in the literature. We acknowledge that much more abundant and inexpensive shale gas than our lowest cost estimate scenario could have a significant impact on the energy system. However, our analysis (which covers a reasonably wide range of shale gas supply curves, as shown in Figure 4) suggests there is relatively little sensitivity to overall energy system costs to shale gas, in a 2^{o}C scenario where gas use is ultimately decreasing in importance. More importantly, we don't have robust estimates at a global level to support such further scenarios at this time.

5. Some sensitivity analyses are not designed to be neutral. For instance, the capital financial sensitivity is primarily targeted against renewable energy, when it may be equally plausible to target fossil energy. However, this serves the purpose of the sensitivity analysis quite well considering that the goal is to assess the "worst case scenario" possibility of increased cost of mitigation. And the authors rightly caveat that the results from this sensitivity analysis are for illustrative purposes only and the choice of capital financing rate is arbitrary.

This scenario is based on policy and mitigation effort being directed towards the support of shale gas rather than low-carbon generation in a scenario that has been specifically designed to understand the direction and possible magnitude of the occurrence that shale gas crowds out investment funds for other low-carbon sources. We agree it's possible that shale gas investment could crowd out other energy asset/fuel investments, but since the scenarios are 2°C scenarios, in which we expect a high degree of low-carbon energy assets to be deployed, the scenario serves to illustrate this specific effect. We are pleased to see that the reviewer believes the scenario has been sufficiently explained and caveated. We have further de-emphasised the results in the executive summary.

6. Most critical assumptions of the modeling shale gas are clearly explained. These include shale gas supply, methane leakage, and competition with other energy sources. However, the report could use a more in-depth description of the demand side of shale gas. What sectors consume the largest amount of shale gas? What energy sources are primarily being displaced by shale gas? What assumptions are used in modeling the potential for using low cost shale gas in passenger and freight transportation? A broader description of the model used in the study would benefit the strength of the report.

A more detailed overview of the demand side is now provided in the newly added Annex D, and a brief description of demand side impacts added in the body of the report. In 2030 (peak gas use), comparing HCNS and HCLS, the majority of additional gas displaces coal in industry, but there is also a small shift from coal to gas in electricity generation, oil to gas in road transport, and electricity to gas in the residential sector.

7. Furthermore, I'd encourage the authors to describe in more detail about how other greenhouse gases are treated. The report goes in depth into the methane leakage rates of shale gas. It would be good to know what assumptions are being used for methane leakage rate for coal mines. Also, previous research have shown that changes in SOx and other aerosols from shifting use of coal to gas is also an important contributing factor on changes in radiative forcing. What assumptions are being used for aerosol emissions of coal vs. gas?

Our analysis suggests that shale gas predominantly displaces other sources of natural gas rather than coal in a climate-constrained energy system. The methods used to account for aerosols are as per the other analysis in the AVOIDing dangerous climate change programme, whereby aerosol emissions from fossil fuels are accounted for in the assumptions made by Met Office Hadley Centre, with full details provided in AVOID 2 reports WPC2a (http://www.avoid.uk.net/2015/11/assessing-the-challenges-of-global-long-term-mitigation-scenarios-c2a/) and WPC2b (http://www.avoid.uk.net/2015/11/the-contribution-of-non-co2-greenhouse-gas-mitigation-to-achieving-long-term-temperature-goals-c2b/).

8. The results appear to follow logically from assumptions, scenarios and modeling. There are no surprises in the results. The direction of the results are as expected. However, the magnitude of impact (being small) is an important finding that should be clearly communicated to the stakeholders and other researchers.

We feel the discussion, conclusions and executive summary are now clear on the magnitude of the findings.

9. The conclusions place the research in a broader context, and they are well supported by the results of the modeling. While there are not many studies dealing with this issue (hence the research “gap”), the conclusions are mostly inline with the few available research.

Detailed section remarks:

Non-technical summary

1. “Shale gas cost range is combined with the higher end of unconventional” >>> conventional?

Corrected, thanks for spotting.

2. “The incremental benefit of adding shale gas to the global energy mix is relatively small” >>> I think the authors mean the “reduction in mitigation cost is small”. Surely, the economic benefit of shale gas through lower cost of energy is large.

We find relatively small benefit with respect to energy system costs, as elucidated in the subsequent clause, “Comparing a scenario with the highest cost in the range for conventional gas and no shale gas, with a scenario in which shale gas is then made available at the lowest costs in its range, the saving in global energy system costs to 2100 is about 0.4%;”. Shale gas chiefly displaces the more expensive sources of conventional gas, coal bed methane, and tight gas, whose cost is not significantly higher than shale gas.

3. The authors use “energy system costs” and “mitigation costs” interchangeably. I’m quite certain that they are conceptually quite different in standard economics. For instance, reduction in overall energy consumption would reduce the system cost, but it would contribute to the mitigation cost through reduction in consumer surplus. It would be beneficial to clearly define the terms and use consistent wording.

We do not use these terms interchangeably. “Mitigation cost” refers to the difference in cost between an energy system meeting long term climate targets, and one failing to meet meeting climate targets, whilst “energy system cost” refers to the cost of the energy system as a whole.

1 Introduction

1. 2-degree target is described as 50% chance in 2100. This is probably an acceptable variation. But it would be good to mention that in IPCC context, it’s typically 66% chance in any year. I believe this is much more stringent than the one being used here.

Thanks, footnote added to this effect.

2. Reference 14. It would be best to credit this reference to Obama or his speechwriter.

Adjusted accordingly.

3. The authors repeatedly claim that there isn’t any authoritative consistent assessment of shale gas. This may well be true. However, Rogner’s work has consistently included low cost shale gas both in 1997 and 2012. Whether you agree with his assessment or not, it would be good to give him credit for acknowledging the shale gas potential from early on, and discuss why you don’t consider his work to be authoritative and/or consistent.

We do acknowledge Rogner for this (see 2nd paragraph of section 2.1). However, Rogner himself states “in many cases [these estimates] are highly speculative”. Later multi-region assessments state similar. We therefore feel that there is sufficient evidence, supported by Rogner himself, that there is a lack of authoritative data on shale gas supply.

4. “if leakage rates are higher than around 3%, then the 100-year climate benefits of using gas rather than coal for electrical power are eradicated” >>> Please elaborate or provide supporting reference for this statement. 3% sounds like a very low break-even point. What are you assuming for methane leakage in coal mines? What heat rate are you using for power plants?

This was based on Howarth 2011, but now updated based on more recent reference to 4% and 20-years.

5. “and strong local opposition in some communities” >>> I’m sure this has happened, but citation needed.

Citation added.

2 Literature on shale gas resource availability, extraction costs, and fugitive emissions

1. “shale gas supply and cost scenarios used in global assessment models could be significantly over-optimistic” >>> In the interest of fairness, it should be noted that the models have been extremely over-pessimistic about the shale gas potential in the U.S. it’s important to remember that this line of research is happening precisely because the modelers and forecasters did not see the shale gas revolution coming. IT would be good to open up possibilities of bias in both directions.

There is, to our knowledge, no clear evidence of over-pessimism surrounding shale gas potential in the literature. Resource assessments are largely similar to early resource assessments by Rogner. It is true that these were not necessarily considered economic before innovations in horizontal drilling and hydraulic fracturing in the past decade or so, but nor were extraction costs estimated before these developments. Having made some further checks, there are to our knowledge no additional sources which indicate that extraction cost estimates are overly pessimistic.

2. “Methane has a significantly higher global warming potential than CO_2 (as 86 times higher over a 20 year timeframe, and 34 times higher over a 100 year time frame” >>> I believe the best practice is to mention both GWPs with and without CC feedback.

In this context the standard IPCC fifth assessment report method is appropriate. Difference without CC feedback is not significant (86 -> 84, 34 -> 28).

3. UT vs. Howard debate is described at length. I’m not sure if this is necessary. Without consensus resolution, a simple “there exists debate” should suffice.

We have adjusted for conciseness.

4. “This suggests that other geological factors such as field size and clay content, regional expertise, and regulatory frameworks are not taken into account.” >>> is this

true? I've read his work, and it's hard to imagine he'd ignore some of the more obvious factors such as field size and clay content. It would be good to verify than to speculate.

We have been unable to get in touch with Medlock to confirm details of this study despite repeated attempts. We have amended this sentence, which now reads "it is not clear whether other geological factors such as field size and clay content, regional expertise, and regulatory frameworks are not taken into account."

5. "more central estimates are not necessarily better than more extreme estimates." >>> History suggests that this is absolutely true. Just two decades ago, the forecaster making the most extreme estimates for U.S. shale gas would have been the one closet to the truth.

See response to 2, part 1.

6. Figure 3: it would be great to include the central scenario of "this study". So the readers can compare them side-by-side.

Cost scenarios in this study are included in Figure 4 (but labelled by source, rather than by our scenario label). We have added cross references in Table 3 to clarify.

7. "there is around 5,000 EJ of shale gas available at a cost of below approximately $2/GJ. Comparing this with the bottom panel of figure 4 suggests this is below the cheapest shale gas extraction cost, which is just above the $2/GJ level." >>> this is a simplistic view that contradicts history. These numbers simply measure the technical difficulty of extraction. They don't include local regulations, land ownership structure, ease of access, cost of trade, etc. The empirical evidence suggests that, at least in U.S. there exists shale gas that would be profitably extracted before conventional gas. The notion that shale gas would only be extracted after all low-cost conventional gas is extracted is in my view too simplistic and not reflective of the reality.

In practice, choices may be made which are economically sup-optimal in a global context. However, this falls outside the scope agreed for this report, which is based upon a cost-optimising model using publicly available estimates on shale gas extraction costs.

3 Methods

1. Box 1: please describe natural gas consumption sectors in more detail.

This detail has been added to Annex D, which focuses on natural gas consumption by demand sectors (industry, buildings, transport, power).

2. "meeting of the weak end of Cancun pledges to 2020" >>> it would be good to mention whether the pathway undershoots or overshoots the Paris pledges. There are several literature on this topic already.

To note that the 2C mitigation pathway significantly undershoots the INDC pledge pathway, which has a broadly flat emissions path between 2020 and 2030. A comment to this effect has been added at the beginning of Section 3: Methods.

3. "Shale cost curves from ETSAP-TIAM 2012 model version, based upon McGlade [38]." >>> this strikes me as possibly too old for this purpose. 2012 is indeed after the shale gas revolution started, but it can take several years for the modelers to

implement this in the model. Considering the magnitude of the change induced by the shale gas revolution, it would be worth considering the sensitivities far beyond what has been considered the "lowest cost" by the 2012 modelers.

These reflect the most up to date publicly available data we were able to obtain.

4. "the capital financing rates for low carbon electricity technologies (here taken to include solar, wind, tidal, hydro, geothermal, nuclear power, and biomass for electricity generation) are increased" >>> please explain the logic behind excluding high carbon fossil electricity technologies. Surely they'd also be subject to capital constraints?

See comment on point 5 in overall remarks.

5. "this leakage is not mitigated through specific methane mitigation measures." >>> this is adequate for modelling "worst case scenario". But this is highly unlikely to be true considering the fact that the leaked gas means less revenue for the gas producers. It would be good to explicitly caveat this. Alternatively, a 1% leakage rate could be considered as 80% mitigation of otherwise 5% leakage rate.

See response to point 2 on leakage in overall remarks.

4 Results

1. Table 4 would be easier to read with full scenario names spelled out instead of acronyms.

Amended in tables 4-6.

2. This section is very numerics heavy. It would be good to add in more logical explanations. For instance, I'm comparing LC-MC-HC while keeping NS constant. I see that the mitigation cost is non-monotonic! It's quite rare observing non-monotonicity in energy models. What logical mechanism would explain the reason behind this non-monotonicity? Same applies for holding MC constant and varying LS-MS-HS.

Thanks for the insightful observation. Both the reference scenario and 2C energy system costs do increase monotonically with cost of conventional gas, as expected. It is entirely possible that the mitigation cost (the difference between the reference and 2C scenario energy system cost) can be non-monotonic, depending on the relative selection of gas against more carbon-intensive sources of energy such as coal and oil, and against less carbon-intensive sources such as renewables.

3. "which turns out to be uneconomic to meet a 2°C target up to 2100." >>> is "uneconomic" the correct term here? Do you mean "suboptimal'?

Thanks, amended.

4. "whilst even in the HC_LS scenario, lowest shale gas costs are similar to conventional gas costs, and the vast majority of gas is extracted from conventional sources up to 2100." >>> this is where I think the analysis could benefit from a larger variation in the supply curve. If everywhere in the world experienced the same level

of impact as the shale gas revolution in the U.S., and the cost of shale dipped below much of the conventional gas, would the results be the same?

See responses to section 2, part 1 and section 3, point 3.

5. Figure 5: it would be great to include a "no policy" reference line to demonstrate the severity of the decarbonization.

We have added a similar figure for reference scenario in a new annex (Annex C).

6. "a theoretical backstop technology which mitigates CO_2 at a cost of 2005US\$10,000/tCO_2." >>> surely, there exists a combination of, say, solar and compressed air that can deliver this at \$10000/tCO2. This need not be theoretical.

There probably are cheaper options, but understanding the potential of negative emissions technologies largely falls outside of the scope of this study. This technology is really a dummy variable which indicates the infeasibility of this scenario and the fallback to meet it through increasing the carbon price to very high levels to stifle energy demand.

7. Table 5: why does reference cost does not change w.r.t. capital financing rates? If the financing gets expensive, it would increase the cost in the reference case as well.

The reference in Table 5 uses standard 10% capital financing rates, on the basis that neither shale gas nor low carbon electricity technologies are favoured for mitigation, and in order to compare to a common reference wrt Tables 4 and 6.

8. Table 5: It would be good to provide some clarity on the meaning of mitigation cost w.r.t. capital financing rates. Is this a true welfare loss due to shortage of capital, or is this a perceived financial cost that is in fact simply a transfer between two entities. I suspect it's a combination of both. I'd conjecture that the "transfer" portion is larger. In this case, it would be inaccurate to state that the mitigation costs are 50% higher, when it may be the case that half of the increase is simply a transfer from one entity to another.

Mitigation costs here are based on change in whole system cost with and without mitigation policy (see response to comment on non-technical summary point 3), upon which transfer between entities would have no influence.

9. Figure 7: not sure where the double hump is coming from. It would be good to add an absolute value chart in addition to the % chart in the appendix.

The double hump is the result of the forced extraction of shale gas in this scenario, which pushes levels of gas supply higher than the model would optimally choose early on. The exact profile of shale gas extraction in this period is somewhat arbitrary. As shale gas becomes depleted (2040-2060), the model makes use of some lower cost conventional gas which it did not use in favour of shale gas earlier in the model run. Absolute value charts added to appendix C for key scenarios.

10. Table 6: increased mitigation cost. This could be easily decomposed into the cost attributable simply to the "dash" vs. the true additional cost of inconsistency between dash and deep decarbonization. The difference between the cost in reference scenario with and without "dash" is not necessarily a part of "mitigation cost". This is simply a cost of government policy to "dash". The true additional mitigation cost is the difference between "dash reference" and "dash policy". I suspect majority of cost additions are simply attributable to "dash" itself. It would also be good to elaborate

why a government would implement such a policy (e.g. energy security, regulatory capture, etc.).

We think this is a good point. A dash for shale gas while there are less expensive conventional gas resources available would likely incur significant additional system cost even in the absence of climate policy. Our scenario examines the question of a dash for shale gas perceived as a mitigation policy (eg. in order to get away from coal), and is compared to a standard reference for this reason, and for consistency with reference in other scenarios.

A government might indeed implement such a policy for the reasons you outline, or perhaps under the impression that resources might be less expensive than they turn out to be. More detail added in section 2.5.

11. "at around 5% per year in the decade 2020-2030, are unprecedented at a global level, and therefore all likely to be challenging to achieve in practice." >>> I think this is too pessimistic. Indeed, it has never been achieved at a global level. And it would be challenging to achieve this. However, there are individual country precedents where the countries have achieved this level of decarbonization. See supplementary materials for: Fawcett, Allen A., et al. "Can Paris pledges avert severe climate change?." *Science* 350.6265 (2015): 1168-1169.

Thanks for the useful reference. We have added a footnote along the lines of what you state here.

12. "where fugitive methane is largely eliminated by increased flaring, well inspections and other mitigation measures." >>> I understand the logic. And I think it's valuable to test this extreme assumption for bounding exercise. But it seems dangerous to assume 0% fugitive methane as a standard assumption for conventional gas. I don't think anyone's realistically expecting to see 0% fugitive rate to happen.

Thanks for the comment. In each scenario, standard emission rates associated with conventional natural gas are assumed to be mitigated based on a set of non-CO2 marginal abatement cost curves, with full details of the methods taken into account for non-CO2 mitigation described in AVOID 2 reports WPC2a (http://www.avoid.uk.net/2015/11/assessing-the-challenges-of-global-long-term-mitigation-scenarios-c2a/) and WPC2b (http://www.avoid.uk.net/2015/11/the-contribution-of-non-co2-greenhouse-gas-mitigation-to-achieving-long-term-temperature-goals-c2b/). These scenarios indicate the impact of additional unexpected emissions over those specified in these industries. We have added a footnote to this effect and clarified in the body of section 3: Methods and Sub-section 4.8.

13. Figure 9: I'm a bit confused on methodology here. My understanding was that all 2-degree scenarios would be, indeed, 2 degrees. But it looks like you're fixing all other gases at 2 degrees while letting methane exceed 2 degrees. I think this should be explained for clarity. Also, please explain how the median value is selected from the temperature ranges (e.g. Monte Carlo MAGICC, Roe and Baker, etc.).
    In this section, we consider the implications of a scenario in which policy has been enacted with a two degrees temperature rise target, under the assumption that leakage rates are identical to that of conventional gas. We then consider the temperature implications if methane leakage rates turn out to be higher (as a result of

ineffective regulation and policy, or unexpected challenges in monitoring and emissions reduction). We have added an introductory paragraph to clarify.

The full methods to determine a 2°C-consistent CO_2 budget, and to calculate the resulting temperature change, are described in AVOID 2 reports WPC2a (http://www.avoid.uk.net/2015/11/assessing-the-challenges-of-global-long-term-mitigation-scenarios-c2a/) and WPC2b (http://www.avoid.uk.net/2015/11/the-contribution-of-non-co2-greenhouse-gas-mitigation-to-achieving-long-term-temperature-goals-c2b/).

14. "a leakage rate of 1% would increase global average temperature change from 1.80°C to 1.84°C (with 50% likelihood) by 2045" >>> 2045 is an odd year to focus on.

On reflection this point is not particularly useful, so removed.

15. "at the current time the measures to accurately monitor and mitigate fugitive methane emissions are not in place" >>> however, it is well-known fact that flaring is an effective measure to mitigate fugitive emissions.

This sentence is to indicate that it is challenging to direct mitigation effort in other areas to compensate for unexpectedly high methane emissions, rather than that there exist no measures to reduce methane emissions (discussed in detail in section 2.3). We think it's better to keep discussion of this to section 2.3.

5 Discussion and conclusions

1. "If there is a concerted effort to exploit global shale gas reserves in spite of these resources not necessarily being the least-cost gas resources " >>> again, some context on why a government may implement such suboptimal policy would be helpful here.

See response to section 4, point 10.

2. "0.08°C additional global warming" >>> I'd strongly suggest that this be expressed in terms of "additional risk of exceeding 2 degrees". I do not believe climate modelling has the specificity to make forecasts at 0.08 degree precision.

This is probably true, but specificity in model results was explicitly requested, and we have made sure to explicitly caveat that these are results from one model alone (see response to first point in "Overall remarks").

3. "if the average fugitive methane leakage from shale gas is 5% of the total volume of shale gas extracted – a value which is deemed plausible given the wide range of the current evidence" >>> "plausible" may be too strong a word here, considering that there is a whole camp of researchers reporting 1-2% range.

It's true that there are researchers reporting this range, but satellite and atmospheric measurements certainly indicate emissions have been higher. See response to point 2 in "Overall Remarks"

4. More of a stylistic comment. I'd recommend not ending the report with "the key caveat…" you could state the caveat before hand, and ending with a strong summary to wrap up on a positive note.

Agreed. We have deleted this paragraph, as I think these caveats are already sufficiently discussed at the end of the "Methods" section.

5. "the results presented are those from one energy systems model, and hence depend on the structure, technology cost, performance and availability assumptions and solution objectives of that model." >>> I cannot agree with this more strongly. We work our hardest to build the best model, but each model has its own idiosyncrasy. There is strong value in multi-model exercise to demonstrate consensus (or lack there of). On the other hand, multi-model exercise always ends up with a least common denominator. The value of in-depth single model study such as this is the liberty to dig deep into the model mechanism to decompose the results. On that note, I'd like to urge the authors once again to focus less on the numerical results and expand on decomposition of the inner working of the model.

Agreed. See first response at start of document on numerical results.