Peer Review File

Manuscript Title: Unextractable fossil fuels in a 1.5°C world

Editorial Notes:

Reviewer Comments & Author Rebuttals

Reviewer Reports on the Initial Version:

Referee #1 (Remarks to the Author):

This paper presents an important update from McGlade and Ekins 2015 paper and the 1.5° results are policy relevant to the current international climate governance. Below are a few comments and questions for the authors to consider:
Conceptually, this paper follows what proposed in McGlade and Ekins 2015, however, presented two main updates: reserve uncertainties and 1.5° carbon constraint. The methodological contribution of the paper is marginal, but policy contribution could be useful. The updated fossil fuel supply curves are a great resource for the community.
My first question: the paper did mention "unburnable fossil fuel are estimated as the percentage of the reserve base that is not extracted, to achieve a particular climate target" , however, are not clearly defined. If it is a %, then what are the nominator/denominator, is it related to a reference year, or dynamic year? What fuel, or fossil as a whole? What particular climate target? Those will help clarify the concept.
My main question: unburnable fossil fuel is good to know, but essentially for climate policy it is how much extra fossil fuel the world can still burn within the climate constraint, and the regional disparity considering the share of carbon budget. That said, the paper needs to highlight how the results of the paper "unburnable fossil fuel" could inform the current climate policy debate, and the policy implications to major energy/climate stakeholders.
As an update, it might offer insight on what are the update: the paper did say "This is a large increase in the unburnable estimates for a 2oC carbon budget previously published, particularly for oil." But how large? How reserve numbers change, how production numbers change, and how results change?
For future scenario projection, the unburnable fossil fuel will heavily rely on modeling assumptions of CCS and other carbon removal technologies, however, are not well presented or overly simplified in the paper. For example, the paper assumes a 5% of CCS deployment rate starting from 2030? It would be more appropriate to let the model decide optimized approaches to supply energy at given climate constraints. The cost assumption of renewables vs CCS also needs some clarifications.
Some other comments and questions:
Ln67-68 "We estimate this to be 58% for oil, 59% for methane gas, and 89% for coal in 2050."
Are those percentages against today's economic reserve? Or 2050's economic reserve?
The readers will benefit from a full list of regional codes in the manuscript.
Ln169-170 "Over the last decade the capacity grew by 400GW at an annual average growth rate of 36%, well above all three scenarios (Figure 1)." The high growth rate at an earlier stage could not sustain, these assumptions are not well-grounded.
The paper reports results in each fuel type and its common unit which is fine. The readers might also benefit from a combined result of all fossil fuel using energy contents. Which gives the scale of all fossil fuels.
The paper also mingling around fossil fuel supply and demand and creating confusion, using "fossil fuel consumption" might help to address this confusion.
Supplementary Table 3. Percentage of total CO2 residual emissions by key sectors across our 1.5oC consistent scenarios and cumulative CO2 capture from BECCS. Are the percentages in

transport, industry, and power sectors should add up to 100%? Which is not the case in its current form? Or any sector missing in the table?
Supplementary Table 15. “The maximum build rate of new solar PV and wind capacity each year is set at 30% of existing capacity in line with recent solar PV build rates”. Is it should be the modeling results of TIMES? Same question for the 5% CCS growth. There is a range in the hydro-dam costs, what costs, in the end, are used in the model? The cost of onshore wind might be an overestimate for 2040/2050. The CAPEX costs are key assumptions and the paper did show sources of certain assumptions, however, are there any rationales or scenarios why those costs are picked? For example, low renewable costs scenarios, low CCS costs scenarios? Are sensitivity scenarios only include demand assumptions?
Can you clarify “The code underlying the TIAM-UCL model is available at this link https://github.com/etsap-TIMES/TIMES_model”. Is TIAM-UCL TIMES model? If they are the same, what’s the added value of TIAM-UCL?

Referee #2 (Remarks to the Author):

Comments on the manuscript “Unburnable fossil fuels in a 1.5°C world” submitted for publication to Nature by Dan Welsby, James Price, Steve Pye and Paul Ekins.

Welsby et al. set out the limits to fossil fuel extraction under stringent climate targets of limiting average warming to 1.5°C. This is based on global energy system modelling that captures primary energy sources. Unburnable fossil fuel reserves are estimated as the percentage of the reserve base that is not extracted. The study estimate this to be over 50 % oil and natural gas reserves, and 89% for coal reserves in 2050.
The main difference with the McGlade and Ekins (2015)1 study is the much smaller carbon budget of 580 Gt CO2 from 2018 onwards, which is consistent with a 50% probability of limiting average global warming to 1.5°C, whereas McGlade and Ekins had an implicit budget of well over 1 Tt CO2. Unsurprisingly, this results in a much higher share for fossil reserves that must remain unburned. However, the study goes into detail. For me, the most interesting results of the study are the regional variations of unburnable shares and the production profiles for major oil and natural gas producers in the next decades. Overall, this study has the potential to interest not only many researchers across disciplines and around the globe but also political decision-makers and possibly investment decision makers. To achieve this, I think the authors should first and foremost still substantiate the data base (see below). Once this is done and the uncertainties of the input values are clear, I would say that this would be a very significant work on fossil fuels in the transition of the global energy system.

In the following, I will not comment on the TIMES Integrated Assessment Model as this is not my expertise. The same is true for investment and operational costs. Besides some general comments on the text, I will focus on the database that is the input for this study. This is accompanied by suggestions that may help to further improve the study.
Abbreviations: ms – manuscript; O&G – crude oil and natural gas; bbl - barrel

General comments:

The authors introduce the term “methane gas” as synonym for natural gas (I guess – there is no explanation). Since this can lead to unnecessary confusion, I recommend sticking with the more widely used term “natural gas”. In most illustrations, it is referred to as “gas” anyway. When “methane gas liquids” (or “associated methane gas”) are introduced, it becomes obvious that this term is inaccurate – natural gas liquids are not primarily composed of methane (like natural gas) but of propane, butane, pentane, and hexane, and heavier straight-chain alkanes. In the following, I use the term “natural gas” instead of “methane gas”.

My first critical comment relates to the consistency of this study. For a meaningful database, all data should be selected according to identical criteria (e.g. same classification and the same base year). If for some reason this is not possible, this should be explained and the effects determined. Yet, in this ms, a 1P basis (proved) is used for natural gas reserve estimates, while 2P reserves (proved and probable) are considered for crude oil. This is surprising since, e.g. McGlade and Ekins2 wrote “We consider 1P reserves to be significantly less useful than 2P reserves for a variety of reasons …”. The authors now state that the choice of 1P or 2P is based on data availability at the time the studies were conducted (the studies? The authors mean this study or are they referring to McGlade29 for oil and Welsby38 for natural gas?). It is worth noting that McGlade and Ekins2 used 2P reserves for both oil and natural gas. Further, the base year of this study is not entirely clear to me. As I understand it, there are different base years for each fossil fuel? This may be problematic as reserves are not a constant but a variable, which change due to changes in technology, infrastructure and economics. Thus, the most recent data should be used rather than trying to adjust outdated (10 years old, or more) data, as, I understand was done for crude oil.

It also remains unclear why the authors propose a new classification for petroleum and natural gas that is contrary to the usual usage: “conventional and unconventional oil refers to the density of the liquid found in the oil reservoir, whereas conventional and unconventional methane gas refers to the geological structure of the reservoir”. Classification of conventional and unconventional O&G can be based on monetary considerations, the fuel itself, or geologic/technical reasons, among others. However, mixing of these classifications should be avoided. There are several types of unconventional hydrocarbon deposits, but heavy oil, shale oil, and oil sands, for example are all very widely classified as unconventional. If shale oil and/or light tight oil is classified as conventional, as the authors propose, why than is shale gas and tight gas, with the same composition as conventional natural gas, classified as unconventional?

My second concern is the fossil fuel database. I have not been able to verify the values of the fossil fuel reserves and resources input data because the numbers are neither provided in the ms nor in accompanying documents. I am sure that these data are extensively discussed in the thesis of McGlade and the (forthcoming) thesis of Welsby, however, the input data for modelling should come along with this ms. Thus, I recommend to present the data and to compare the reserve database (perhaps in the supplements) with other publicly available global fossil fuel reserve data. There are certainly significant uncertainties within regional and global estimates of fossil fuel reserves. This is even more true for resources estimates. Such uncertainties need to be addressed and taken into account when modeling is performed. To my surprise, the figures presented in this ms do not have any (probability) ranges indicated, so there are no uncertainties?

Important emission sources missing in this study are methane emissions from the supply chain of hard coal and crude oil, while methane emissions from the natural gas supply chain are included. Methane emissions from lignite may indeed be negligible, but those from coal and crude oil (e.g.3 4) are not. According to the IEA5, about 40 % of the O&G supply chain methane emissions are attributable to the oil sector. This number may not be definitive; nevertheless, this aspect needs to be considered. As methane is a much more potent greenhouse gas, the exclusion of these emissions is expected to skew the results.

Another issue is that a minor share of fossil fuels is used non-energetically, i.e. is not burned but used for the production of plastic, medicals, fertilizers, etc. As far as I understand, this is only taken into account for years 2050 onwards. Thus, I recommend explaining this (if considered before 2050 as well) or including the non-combusted feedstocks in the calculations for the years up to 2050 as well, since this production will definitely have an impact on the reserves and resources base. While the non-combusted feedstock share of crude oil post-2050 appears quite high, the non-combusted share of natural gas may be underestimated in this study (Fig. S6). According to IEA8, some 205 bcm natural gas is used globally for hydrogen production, mainly for fertilizer. This alone represents 6 % of global natural gas consumption.

What I do miss in this ms is a discussion of the results with respect to other estimates of changes in future oil and natural demand, supply and net trade position, as e.g. with the three IEA scenarios. Are there differences and if yes, what is the origin for such differences?

One criticism of the cost-optimal approach used here might be that it does not take into account the political economy of fossil fuel production and use, including carbon prizing. So, as a final general comment, I would have liked to see more discussion of the use of coal versus O&G, especially natural gas. What effect do carbon pricing and regional coal phase-out decisions have on natural gas use? If not decarbonized, natural gas will have to be phased out as well, by 2050 at the latest; the interesting question is the prediction of the next decades. I encourage the authors to explore and discuss this question a bit further. At the same time, the a bit unfocused discussion on fossil fuel phase-out strategies could be shortened (Discussion section).

Other comments:

Abstract
• The findings of this study will not only have "significant implications for oil and methane gas producers" but also for O&G consumers whose consumption will need to decline rapidly to ensure climate goals are met. Why exclusively the producers are addressed in the Abstract is not clear to me.

Main text
• Production decline of major producers – "The outlook is one of decline, with 2020 marking both global peak oil and methane gas production" – Well, for natural gas, the cited reference for the oil demand peak, BP 2020, says something different. Even for the "Net Zero" scenario, peaking natural gas consumption is proposed to occur not before the mid-2020s while the other scenarios propose an increase in natural gas consumption.
• I did not understand why the "..scenarios for the global fossil fuel industry (result) very likely (in) an underestimate…" I do agree that there are high "risks underestimating the required rate of emissions reduction" but a "risk for an underestimate" is different to "very likely an underestimate"?
• Tables: abbreviations must be explained. Oil volumes are presented in Gb – I guess this means Giga barrels. If so, I suggest changing to SI units (e.g. gram, or Joule). Further, there is a mixture between tcm and Tcm in Table 1. What is $M (Table S9)?
• An explanation which of the available definitions for "Arctic" is used in this ms is missing (latitude, temperature, etc.). If e.g. the Arctic Circle is used, Southern Greenland, with its climate conditions and ice cover being similar to the high Arctic and thus similar vulnerable would not be part of the Arctic.

Methods section
• What do the authors mean with "Some volumes of unconventional oil and gas are also categorised as reserves, …". Sure there are unconventional oil and gas reserves, production is ongoing since decades– but why are only some reserve volumes categorized?
• I suggest being very careful about using directional drilling as an indication of unconventional exploration. Today, there may not be many non-directional wells in any O&G fields.
• Reference 37 (2016) is outdated. This report is published annually and the comparison should be done with the most recent version.
• It is not recommended to cite publications that are not available, as e.g. a forthcoming publication (38.Welsby, D. Modelling uncertainty in global gas resources and markets (forthcoming). (University College London, 2021)).
• Why are the global supply cost curves (e.g. Fig. 3) on a $2005 basis?

Supplementary information
• The TIMES model "represents the countries of the world as 16 regions (Supplementary Table

20)”. However, there is an inconsistency between the countries of the FSU (former Soviet Union; main text) and the Commonwealth of Independent States (CIS) as listed in table S20. FSU is missing in this table. Is Eastern Europe (Bosnia and Herzegovina, Bulgaria, Croatia, Czech Republic, ...) part of EUR or FSU (e.g. in Fig. 1)?
• The supplementary information terminates with the sentence “A number of these constraints are outlined in the next section”. At least the most important constraints for this ms should be listed here.
• In Supplementary Figure 6 the y-axis is incorrect (going two times to 100%).
• Please update: “Power of Siberia pipeline, which is due to come online in 2020”
• There is a confusing mixture of endnotes numbers at the bottom of the pages and references numbers at the bottom of the manuscript.
• As stated previously, I recommend comparing the reserve database with other publicly available global reserve data for fossil fuels. Such fossil fuel inventories are provided by BP, BGR, OPEC, EIA, WEC, IEA to name just a few publicly available at no cost. It is perfectly clear to me that many of these inventories are subject to criticism; some of it may be justified. However, McGlade29 stated that “A database of reserve estimates has therefore been generated by choosing the most robust existing estimates for each country from the sources listed in Table 3.1. By carefully choosing and restricting the choice of studies, this approach helps mitigate or reduce many of the problems and uncertainties identified”. This could be read as a relatively subjective selection and why these inventories have been selected has not been explained. Coal data was obviously compared in this ms with the BGR inventory (without giving details), it would be interesting to compare O&G reserves and resources as well. A rough overview of the McGlade and Ekins1 values (basically the selection of McGlade?), which somehow form the input for the crude oil estimates in this ms, shows differences with several of these inventories (Table 1). This obviously needs to be addressed by the authors.

Table 1: Sample crude oil reserves inventories (EIA, BP, OPEC, BGR) for comparison with some of the McGlade & Ekins estimates. To allow comparison with the McGlade & Ekins data, the 2010 values from the other inventories are presented. However, these are available on an annual basis. Please note considerable variations in inventory estimates, often resulting from different conventional/unconventional classifications (e.g. Canada). Overall, the inventories appear to be fairly consistent. Gbbl – Giga barrel.

Crude Oil Reserves (Gbbl) Base Year 2010

	EIA	BP	OPEC	BGR	McGlade & Ekins
Africa	123,6	125,0	126,0	126,9	111,0
Canada	175,2	174,8	4,1	4,1	53,0
China+ India	26,0	29,0	29,2	29,0	38,0
Middle East	752,9	766,0	794,6	790,7	689,0
USA	25,2	35,0	23,3	30,9	50,0

I hope the authors find these comments as helpful and supportive as they are meant to be.

References
1 McGlade, C. & Ekins, P. The geographical distribution of fossil fuels unused when limiting global warming to 2 °C. Nature 517, 187-190, doi:10.1038/nature14016 (2015).
2 McGlade, C. & Ekins, P. Un-burnable oil: An examination of oil resource utilisation in a decarbonised energy system. Energy Policy 64, 102-112, doi:https://doi.org/10.1016/j.enpol.2013.09.042 (2014).
3 Kholod, N. et al. Global methane emissions from coal mining to continue growing even with

declining coal production. Journal of Cleaner Production 256, 120489, doi:https://doi.org/10.1016/j.jclepro.2020.120489 (2020).
4 Höglund-Isaksson, L. Global anthropogenic methane emissions 2005-2030: technical mitigation potentials and costs. Atmos. Chem. Phys. 12, 9079-9096, doi:10.5194/acp-12-9079-2012 (2012).
5 International Energy Agency. World Energy Outlook (WEO), Chapter 10: The environmental case for natural gas. 399-436 (2017).
6 BGR. BGR Energy Study 2019 - Data and Developments Concerning German and Global Energy Supplies. 200 (2020).
7 IEA. Resources to Reserves 2013. (2013).
8 IEA. The Future of Hydrogen. (International Energy Agency, Paris, 2019).

29. McGlade, C. Uncertainties in the outlook for oil and gas. Doctoral thesis, UCL (University College London). (2013).
38. Welsby, D. Modelling uncertainty in global gas resources and markets (forthcoming). (University College London, 2021).

Referee #3 (Remarks to the Author):

Summary of the key results
This paper provides an update of the widely cited Nature publication, McGlade and Ekins (2015), and its seminal analysis of the extent and regional distribution of fossil fuel reserves and resources that would not be extracted under a low-carbon future. It uses the same model (TIAM-UCL) and general methodology and considers the implications of a much more limited carbon budget (580 GtCO2) associated with a 50% probability of keeping warming within 1.5 degree as compared with the carbon budget used in M&E 2015, which was associated with a 2-degree temperature limit and earlier start date (2011 vs 2018 here) and sooner end date (2050 vs 2100 here). This tighter budget results in an increase in the fraction of reserves that cannot be extracted, with much of the reduction coming from oil to a lesser but significant extent from gas. The tighter budget has little implication for coal, which is already heavily constrained at 2 degrees.
The relative regional implications appear to be quite similar to those in M&E 2015, for example, in the limited space for developing unconventional resources in North America, or the regions bearing the brunt of reserves unusable.

Originality and significance:
The model and data are quite similar to those used in M&E 2015 but with some notable updates and improvements, e.g. in capturing upstream energy use and emissions. (Note that it would be helpful to draw out the key enhancements and their significance in a paragraph in the main paper, as it required a review of supplementary information to appreciate these changes.)
As noted, the results are quite similar as well, especially in terms of relative implications by fuel and region, though it is interesting to see that the tighter budget has a significantly greater impact on reducing oil as compared with gas extraction. The reasons for that difference, since it is a notable finding, could benefit from brief reflection and discussion: to what extent is it explained by lower relative mitigation costs for oil vs. gas in specific sectors or technologies?
More broadly, since this analysis is an update and change in constraint relative to M&E, and it would be useful for the reader to better understand to what extent the change in results is due merely to the tighter constraint (580 vs 1100 Gt CO2) – i.e. as compared with changes in model parameters and assumptions (reserve estimates, supply cost curves, mitigation potentials, and so forth) and the change in start date. For example, it would appear that Canada's unusable oil reserves do not much decrease while those of other regions do: why?
While the paper is thus not necessarily original or novel, it is significant in that a) M&E 2015 has served an important touchstone for many policymakers and observers; b) the findings provide an update that can inform national and international policy discussions around net zero and other targets that are associated with the more ambitious 1.5-degree temperature limit, especially in the run up to COP26.

The significance and the relationship of this analysis to M&E 2015 suggest, in this reviewer's opinion, that it warrants publication in Nature. The relative lack of novelty and limited extent of differences in findings, also suggest publication could take a reduced form, as briefer update rather than full length manuscript, highlighting the handful of more consequential findings and their implications – akin to those in M&E but starker, with greater risks for oil in particular -- with much of the material, especially on the regional distribution, moved to supplementary information.
For example, I would suggest replacing the section "production decline of major producers" altogether and present the key points in a single paragraph to sharpen the text and avoid excessive precision (see below). All oil producing regions show similar enough paces of decline and the expectation that US continues to lead increases in the near term may be undone already by recent market changes (See IEA's recent Oil market report https://www.iea.org/reports/oil-market-report-march-2021). What is particularly interesting here (to this reviewer) is the more nuanced picture for gas, which could be explained more briefly without reference to individual projects or individual regional decline rates at two significant digits.

Data & methodology: validity of approach, quality of data, quality of presentation
The overall approach appears robust and valid much as M&E 2015 was.
Given large uncertainties related to reserve and resources, especially by 2050, as extraction, as well as CCS and DAC and other technologies evolve, it is unclear whether extending the analysis beyond 2050 to 2100 adds significant insight or value to that overall analysis.
The use of the term "unburnable estimates" is confusing and somewhat misleading. A more accurate term would be "unextractable" since what is extracted is not necessarily burned – it includes fossil fuels that are used for petrochemicals not just energy use.

Conclusions: robustness, validity, reliability
The regional results are driven by least cost optimization. However, global and regional fossil fuel markets do not necessarily operate only in this fashion; they can be influenced by government production subsidies, cartel behavior, political sanctions, preferential trade agreements, and so forth. While on the whole, cost considerations dominate, these other factors could be more explicitly acknowledged as potential influences on the regional distribution of future production.
Given these and other underlying uncertainties and model limitations, which are acknowledged in the paper, results are presented with excessive precision. Compare for example, with the abstract from M&E: "Our results suggest that globally a third of oil reserves, half of gas reserves and over 80% of coal"… should remain unused". Here, the second sentence states "58% of oil, 59% of methane gas... must remain unburden". Arguably no more precision than "three-fifths of oil and gas, and nearly 90% of coal" is needed or warranted here, and as noted above, "should remain unused" would be more accurate than "unburned".
As noted above, I would suggest the use of the term "unusable" rather than "unburnable", especially if the results are extended beyond 2050 when so much of the fossil fuel extracted and used are not in fact burned.

Author Rebuttals to Initial Comments:

Referees' comments are in black text, and author responses are in red text.

Response to reviewer #1

This paper presents an important update from McGlade and Ekins 2015 paper and the 1.5° results are policy relevant to the current international climate governance. Below are a few comments and questions for the authors to consider:
Conceptually, this paper follows what proposed in McGlade and Ekins 2015, however, presented two main updates: reserve uncertainties and 1.5° carbon constraint. The

methodological contribution of the paper is marginal, but policy contribution could be useful. The updated fossil fuel supply curves are a great resource for the community.

We thank the reviewer for highlighting the policy relevance of this paper, and its usefulness as an update to earlier work by McGlade and Ekins. We would also like to reiterate the novel contribution of this paper, which we have tried to draw out more explicitly in the revised version of the manuscript.

Firstly, unlike McGlade and Ekins (2015), we have bridged the gap between the concept of unburnable carbon and the analysis of regional production dynamics required to achieve this. In short, we believe our work combines the aggregated findings of the Production Gap Report (2020) with the higher-level concept of unburnable fossil fuel reserves. For example, we provide time-series production profiles (2018-2050) for the five largest producing regions of oil and gas, and have (albeit briefly) tried to reconcile these with current production levels and the direction of investments. These regional level insights into the actual application of a managed production decline were not included in the 2015 paper, and provide new insights for policy implications under a 1.5°C target.

Secondly, and whilst we agree that the general method of the paper is similar to the McGlade and Ekins (2015) paper, we have used a significantly reduced carbon budget and provided context to the post-2050 time period, extending the analysis out to 2100. This is important, as the unburnable estimates in 2100 provide insights on continued use of fossil fuels post-2050, with implications for broader system such as the need for carbon dioxide removal (CDR) options if the temperature target is to be met. For example, our entire 580 Gt CO_2 budget is used between 2018-2050 (with cumulative emissions between 2018 and 2050 reaching 631 Gt CO_2) and therefore any additional consumption of fossil fuels (and residual CO_2 left from the pre-2050 period) must be sequestered directly or compensated for by CDR, while the system must also redress the carbon budget exceedance pre-2050 via global net negative CO_2 emissions post 2050.

These aspects of research novelty are now laid out more explicitly from Lines 29-33 of the main manuscript.

My first question: the paper did mention "unburnable fossil fuel are estimated as the percentage of the reserve base that is not extracted, to achieve a particular climate target", however, are not clearly defined. If it is a %, then what are the nominator/denominator, is it related to a reference year, or dynamic year? What fuel, or fossil as a whole? What particular climate target? Those will help clarify the concept.

We hope we have made this much clearer in our revised manuscript. In Lines 67-68 we have explicitly stated these are 2018 reserves, and changed the sentence to: "Unburnable oil, gas and coal reserves are estimated as the percentage of the 2018 reserve base that is not extracted, to achieve a 50% probability of keeping global temperature increase to 1.5°C". For additional clarity, the unburnable estimates are for each fossil fuel. For a given fossil fuel, the numerator is the reserves not extracted (over a given time period) while the denominator is the total reserves in 2018.

My main question: unburnable fossil fuel is good to know, but essentially for climate policy it is how much extra fossil fuel the world can still burn within the climate constraint, and the regional disparity considering the share of carbon budget. That said, the paper needs to highlight how the results of the paper "unburnable fossil fuel" could inform the current climate policy debate, and the policy implications to major energy/climate stakeholders.

We agree with this comment, as to how we relate the concept of unburnable fossil fuels to the climate policy debate. This was why we thought it was important to include the section 'Production decline of major producers' in the main manuscript. This section aims to highlight the implications for major producers, if the world is stay at or below 1.5°C by 2100. We believe that this makes the unburnable numbers much more useful in the debate. In the discussion section, within word limit constraints, we also highlight what the policy implications are for different stakeholders, including the implication of declining production for major producers, highlighting the case of Middle Eastern oil peaking now, and therefore fiscal revenues declining, particularly in the case of low oil prices from demand destruction (Lines 191-195 in the main manuscript).

As an update, it might offer insight on what are the update: the paper did say "This is a large increase in the unburnable estimates for a 2oC carbon budget previously published, particularly for oil." But how large? How reserve numbers change, how production numbers change, and how results change?

Given space limitations in the main manuscript, we have provided a full comparison to the 2015 McGlade and Ekins paper in SI section 2. As per the reviewer's recommendation this covers the difference in reserves between the two studies, the difference in cumulative production, and finally the difference in unburnable reserves (shown in terms of relative percentages and absolute physical units). We have recalibrated the McGlade and Ekins cumulative production so production between 2018 and 2050 can be directly compared between the two studies. We have also extended this to compare unburnable resources directly between the two studies.

For future scenario projection, the unburnable fossil fuel will heavily rely on modelling assumptions of CCS and other carbon removal technologies, however, are not well presented or overly simplified in the paper. For example, the paper assumes a 5% of CCS deployment rate starting from 2030? It would be more appropriate to let the model decide optimized approaches to supply energy at given climate constraints. The cost assumption of renewables vs CCS also needs some clarifications.

Thank you for highlighting this point, which we have clarified in the text. We have made it clearer in the SI, under Supplementary Table 21 (SI section 6), that 5% capacity growth constitutes the *upper growth rate* at which CCS technologies can deploy. However, it is up to the model whether CCS deploys at that rate or at lower levels. In short, the 5% rate is not exogenously forced on the model but instead the model can choose to build CCS capacity and other carbon removal technologies *up to a maximum growth of 5% per year*. The chosen start date of 2030 for commercial scale CCS is typical of other estimates in the literature.

In terms of cost assumptions, we would direct the reviewer to the SI Section 6, where Supp Table 21 and 22 provide CAPEX and OPEX assumptions for renewable generation technologies, fossil generation with CCS, and BECCS technologies (including power generation, heat, hydrogen and Fischer Tropsch fuels). Additionally, Supp Table 23 provides CAPEX and fixed operational and maintenance (FIXOM)cost assumptions for the direct air capture technology in TIAM-UCL.

Some other comments and questions:
Ln67-68 "We estimate this to be 58% for oil, 59% for methane gas, and 89% for coal in 2050." Are those percentages against today's economic reserve? Or 2050's economic reserve?

We have added "2018" to the first sentence of this paragraph, as we agree with the reviewer that this needs to be stated upfront for clarity.

The readers will benefit from a full list of regional codes in the manuscript.

In the main manuscript we do not use regional codes extensively, and when we do, they are spelled out. However, in the SI Supp Table 26 provides the regions with their full name, code, and the countries included in each region.

Ln169-170 "Over the last decade the capacity grew by 400GW at an annual average growth rate of 36%, well above all three scenarios (Figure 1)." The high growth rate at an earlier stage could not sustain, these assumptions are not well-grounded.

It is not clear what this comment is referring to as we do not recognise the quoted sentence provided by the reviewer.

The paper reports results in each fuel type and its common unit which is fine. The readers might also benefit from a combined result of all fossil fuel using energy contents. Which gives the scale of all fossil fuels.

Whilst not exactly the same, we think that the inclusion of the "Embodied carbon in resource estimates" goes some way towards covering the reviewer's suggestion on this point. We show in Supp Figure 2 the embodied CO_2 within fossil fuel reserves and resources that allows for an aggregate view. We have also included a line in the SI (72-73) stating the combined energy contents of all fossil fuels, and the cumulative production of these (in energy content measured in zetajoules (ZJ)) for 2018-2050 and 2018-2100. In the main paper we have reported in units we believe are most commonly used for reporting each fossil fuel (billion barrels for oil, trillion cubic meters for gas, billion tons for coal), in order to make the messaging as effective as possible for the audience (e.g. we believe most audiences would be more familiar with billion barrels, trillion cubic meters etc. than joules), however we appreciate the reviewer's comment that a combined metric is also helpful in order to provide context to the total fossil resource base.

The paper also mingling around fossil fuel supply and demand and creating confusion, using "fossil fuel consumption" might help to address this confusion.

It would be useful for the reviewer to highlight the specific terminology in the manuscript that is causing confusion. We use supply and demand at different points in the manuscript, often to differentiate between issues around production (supply) and consumption (demand). Whilst we appreciate the paper focuses on the supply side (fossil fuel production), the co-dependence between supply and demand means we feel discussing both is of fundamental importance.

Supplementary Table 3. Percentage of total CO2 residual emissions by key sectors across our 1.5oC consistent scenarios and cumulative CO2 capture from BECCS. Are the percentages in transport, industry, and power sectors should add up to 100%? Which is not the case in its current form? Or any sector missing in the table?

We thank the reviewer for bringing this to our attention. Our initial intention was to highlight emissions in key sectors and therefore the numbers were not supposed to add up to 100%, however we have added another column ('Other' with these sectors defined below Supp Table 4) to avoid any confusion and the percentages now add up to 100.

Supplementary Table 15. "The maximum build rate of new solar PV and wind capacity each year is set at 30% of existing capacity in line with recent solar PV build rates". Is it should be the modelling results of TIMES? Same question for the 5% CCS growth. There is a range in the hydro-dam costs, what costs, in the end, are used in the model? The cost of onshore wind might be an overestimate for 2040/2050. The CAPEX costs are key assumptions and the paper did show sources of certain assumptions, however, are there any rationales or scenarios why those costs are picked? For example, low renewable costs scenarios, low CCS costs scenarios? Are sensitivity scenarios only include demand assumptions?

The TIMES model formulation allows constraints to be set which may or may not be binding. For example, the 5% (30%) growth rate on CCS (solar PV) technologies allows a *maximum* 5% (30%) annual growth of existing capacity. However, the model solution decides the optimal deployment of these technologies and therefore the 5% (30%) may or may not be binding, with the model free to choose growth rates up to a maximum of 5% (30%). For a more definitive exploration of the functional form of these user constraints we would direct the reviewer to the Supplementary Information (SI) Section 7 and the second paragraph of SI Section 3.

Regarding the CAPEX costs, we thank the reviewer for making these points. For hydro-dam, the cost range reflects the cost variation for the different tranches of this technology, of which there are 5 within the model, with 1 cheapest and 5 most expensive. For onshore wind, the CAPEX is an unweighted average across the 16 regions based on data from BNEF. Here, Japan, ODA and MEA push the average up with regions such as China seeing costs of 711 £2005/kW.

The CAPEX assumptions are derived from a variety of sources and we make every effort to keep them up-to-date based on best available data. Our focus here is to do our best to mitigate the critique that IAM cost assumptions are often out of date, particularly those of variable renewables which are rapidly evolving. We therefore draw on recent data from BNEF and other sources.

Our sensitivity scenarios cover various key dimensions of the model that are particularly relevant to the analysis at hand including the size of the biomass resource available (crucial because this shapes the role of BECCS), the speed with which CCS can deploy (the upper limit, again critical for the role of fossil fuels) and the scale of certain important energy service demands (i.e. those which the model has either no or limited ability to abate directly thus relying on negative emissions). In response to the reviewer's suggestion, we did run a low-cost CCS scenario, but found this did not alter our unburnable reserve numbers. This is largely because TIAM is being pushed to the very limit of its feasibility with the carbon budget we used and therefore there is no more 'room' within the budget for any more fossil fuels. However, we would direct the reviewer to the sensitivities we conducted in the Supplementary Information Section 3 on CCS deployment rates which have a larger bearing on the modelling results (e.g. in the low CCS deployment scenario, with an upper annual growth rate of 2.5%, the model cannot satisfy energy service demands without the use of a 'backstop' technology which removes CO2 (~ 1.5% of the total budget is removed from this backstop technology in the low CCS deployment sensitivity)).

Regarding other potential scenario dimensions, an important point here is that TIAM-UCL is already at its feasibility limit when modelling an energy future that stays within a 1.5°C-50% probability carbon budget. In doing so the model moves as fast as possible to decarbonise the energy system, pushing deployment rate constraints for key low-carbon technologies to their limit. As such, we have found CAPEX considerations to be a secondary factor, i.e. it is more about how fast the transition can proceed than the precise details of technology costs.

Furthermore, during our initial testing of our scenarios we found that lowering our CCS cost assumptions had little impact on our results and so opted to focus on build rate limits. For renewables, and particularly variable renewables which are expected to become a key foundation of the energy system, we note that our base scenario already sees a rapid deployment of wind and solar PV with a 75% and 85% (both global averages) share of annual electricity generation from these technologies in 2050 and 2100 respectively. This is combined with a drive to utilise this low-carbon electricity by increasing the electrification of the energy system. Therefore, we do not believe that lower renewable costs would alter our results substantially.

Can you clarify "The code underlying the TIAM-UCL model is available at this link https://github.com/etsap-TIMES/TIMES_model”. Is TIAM-UCL TIMES model? If they are the same, what's the added value of TIAM-UCL?

Yes, TIAM-UCL is a model that uses the TIMES modelling framework. The code underlying this framework, as provided at the github link, provides the set of mathematical equations which represent the 'rules' of the energy system (as described in SI section 7). SI section 7 also outlines the other components of a TIMES model, like TIAM-UCL, in addition to the code. This includes the structure of the system (or reference energy system) and the range of different assumptions used to parameterise the different parts of the system. These elements are unique to TIAM-UCL compared to other TIMES models, based on many years of development of model structure and input assumptions.

Response to reviewer #2

The authors introduce the term "methane gas" as synonym for natural gas (I guess – there is no explanation). Since this can lead to unnecessary confusion, I recommend sticking with the more widely used term "natural gas". In most illustrations, it is referred to as "gas" anyway. When "methane gas liquids" (or "associated methane gas") are introduced, it becomes obvious that this term is inaccurate – natural gas liquids are not primarily composed of methane (like natural gas) but of propane, butane, pentane, and hexane, and heavier straight-chain alkanes. In the following, I use the term "natural gas" instead of "methane gas".

We completely agree with the reviewers highlighting of the use of "methane gas liquids" as an error given their chemical composition, which has been corrected to "gas liquids" in the main manuscript. We would suggest associated methane gas is a correct term given that this is methane contained in oil reservoirs (either as a gas cap or dissolved in the oil stream). We are aware that there are also potentially gas liquids in these reservoirs, however our reserve numbers (and therefore unburnable reserves) for methane gas refer only to non-associated and associated methane gas.

We have used the term 'fossil methane gas' because we believe it is the correct scientific name. We have altered our terminology to "fossil methane gas" to distinguish between methane produced from oil and gas reservoirs and bio-methane from renewable sources. We also suggest that the term 'natural' gas has more positive connotations which need to be contested in the narrative of unburnable reserves under a 1.5°C target. We have added the link below as a reference point and hope the reviewer finds it equally of interest.

https://climatecommunication.yale.edu/publications/should-it-be-called-natural-gas-or-methane/

My first critical comment relates to the consistency of this study. For a meaningful database, all data should be selected according to identical criteria (e.g. same classification and the same base year). If for some reason this is not possible, this should be explained and the effects determined. Yet, in this ms, a 1P basis (proved) is used for natural gas reserve estimates, while 2P reserves (proved and probable) are considered for crude oil. This is surprising since, e.g. McGlade and Ekins wrote "We consider 1P reserves to be significantly less useful than 2P reserves for a variety of reasons ...". The authors now state that the choice of 1P or 2P is based on data availability at the time the studies were conducted (the studies? The authors mean this study or are they referring to McGlade29 for oil and Welsby38 for natural gas?). It is worth noting that McGlade and Ekins2 used 2P reserves for both oil and natural gas. Further, the base year of this study is not entirely clear to me. As I understand it, there are different base years for each fossil fuel? This may be problematic as reserves are not a constant but a variable, which change due to changes in technology, infrastructure and economics. Thus, the most recent data should be used rather than trying to adjust outdated (10 years old, or more) data, as, I understand was done for crude oil.

We completely understand the reviewer's concerns and have made significant edits to both the main manuscript and SI to account for these. We think this was partly a communication issue on our part where we were not completely clear about how fossil reserve numbers were developed.

Our approach has been to derive estimates for 2018 (referred to in the manuscript as 2018 reserves) employing the robust exploration of volumetric uncertainty conducted by both McGlade and Welsby. In both cases, we use the central estimates derived from this uncertainty analysis in TIAM-UCL. We still believe the basic analysis undertaken by McGlade is robust (and help ensure comparability with McGlade and Ekins 2015), but this has been updated to 2018 to account for movements between different resource classifications, as well as cumulative production. Welsby's analysis follows a similar approach to reserve definition as used by McGlade, and in the paper both are now referred to as 1P. Whilst McGlade was originally estimated on a 2P basis, we state the following approach in the Methods section of the manuscript – 'For oil, we have updated and recalibrated McGlade's study using 1P estimates from public sources given these are the most up to date available. This allows for us to account for reserves of light tight oil in the United States[41], whilst maintaining the robust assessment of uncertainty conducted by McGlade[30].'

Further information on the uncertainty approach is provide in the section *Fossil resource extraction* of SI section 5.

A key addition in the revised paper has been to put our estimates of reserves and resource in the context of the wider literature, both to highlight some of the challenges with determining 1P versus 2P, and the uncertainty across the literature. To this end, we have significantly updated the section *Reserve estimates for oil and fossil methane gas* in Methods in the manuscript plus added a new 4 page section 'Reserve and resource estimates' at the start of SI section 5 that provides additional detail on the reserve and resource estimates used in the analysis, and in the wider literature.

In the SI, we have compared publicly reported 1P oil and gas reserves across a range of sources and have included a detailed discussion at the start of SI Section 5 of why these cannot be relied on solely (i.e. the key areas of uncertainty). Supplementary Tables 5-10 now provide both comparisons of our reserve and resource estimates with public sources, as well as the regional breakdown of reserves and resources across all fossil fuels for each region and split between different geological categories and techno-economic classifications.

The reviewer is correct in quoting McGlade and Ekins (2015) suggesting that 1P reserves are too restrictive. However, this statement is based on a robust application of the definition of 1P, which for many of the publicly available reserve-reporting sources is not always the case. Key examples highlighted in the main manuscript and SI include Venezuelan reserves for oil and Russian reserves for gas (taken from a section of Welsby's thesis discussing the reporting of 1P Russian reserves which appear to use the Russian reporting system rather than SPE rules, including fields such as Shtokman which has consistently exhibited uneconomic operating conditions in their reserve estimates). For example, BP estimates Russian reserves at 38 tcm in 2018, whereas Cedigaz and the OGJ estimates are significantly higher at 48-50 tcm. We have noted these uncertainties briefly in the Methods section of the main manuscript (Line 383-400), and covered them in detail in the SI Section 5 ("Reserve and resource estimates").

Therefore, we suggest there is a trade-off between a strict application of 1P, which we agree would be restrictive (e.g. Rystad estimate that global 1P oil reserves are 386 Gb, estimated from their field-level database and having a strict application of "conservative" estimates in "existing" fields), and the 1P estimates available for *both oil and gas* in publicly available literature. Therefore, given up to date publicly available estimates for oil and gas are only available at a quoted 1P level, we suggest that these should be used, though with significant caution, and employing the robust exploration of volumetric uncertainty conducted by both McGlade and Welsby. Additionally, McGlade (2013) and McGlade and Ekins (2015) had a 'starting' reserve/resource base of 2010, which was sufficiently close to Campbell and Heaps 2009 "Atlas of Oil and Gas Depletion" study which provided country-level oil and gas reserves at a 2P level. As the reviewer noted, ideally the same reporting would be used for both oil and gas, and therefore because no equivalent study on 2P reserves has been conducted in recent years, we decided to focus on more recent publicly available 1P data, but taking into account the inherent uncertainties in these reported volumes.

We also explain in the SI why the publicly available reserve numbers were not used directly, given the huge range of uncertainties. This also explains why we have used some of the guiding principles for assessing fossil fuel resource availability sensitivities from McGlade (2013) and McGlade and Ekins (2015), as well as the analysis for gas conducted by Welsby (2021).

It also remains unclear why the authors propose a new classification for petroleum and natural gas that is contrary to the usual usage: "conventional and unconventional oil refers to the density of the liquid found in the oil reservoir, whereas conventional and unconventional methane gas refers to the geological structure of the reservoir". Classification of conventional and unconventional O&G can be based on monetary considerations, the fuel itself, or geologic/technical reasons, among others. However, mixing of these classifications should be avoided. There are several types of unconventional hydrocarbon deposits, but heavy oil, shale oil, and oil sands, for example are all very widely classified as unconventional. If shale oil and/or light tight oil is classified as conventional, as the authors propose, why than is shale gas and tight gas, with the same composition as conventional natural gas, classified as unconventional?

We would like to highlight that we distinguish between light tight oil and shale oil (i.e. kerogen). We define light tight oil as conventional because it more closely resembles the production of oil liquids from conventional reservoirs than “unconventional oil” which requires additional upgrading/processing before it can be compared to a representative barrel of crude oil. In TIAM-UCL, the commodity output from the light tight oil mining processes outputs a barrel of crude oil, whereas the mining processes for unconventional oils output bitumen, ultra-heavy oil or kerogen, which require additional processes to upgrade into crude oil (the term ‘synthetic crude oil’ (SCO) has been used widely). Shale oil (kerogen oil) is defined as unconventional given its density. For example, using the BGR’s “classification of crude oil according to its density” figure (BGR, 2019, p. 195), kerogen oil from shale formations, bitumen from oil sands, and ultra-heavy oil (e.g. Venezuelan Orinoco Belt) would all fall in our definition within the “non-conventional” (< 10 API) category, whilst conventional oil, light tight oil, and condensates are considered “conventional” based on their API.

We appreciate that production technologies for light tight oil could fall under the umbrella of “unconventional” oil. However if it is classed as unconventional then a corresponding critique of this is the aforementioned density of the oil in the reservoir (i.e. that light tight oil more closely resembles the crude oil produced in conventional reservoirs). This was discussed in McGlade and Ekins (2015) and McGlade (2013) where light tight oil was considered conventional to differentiate from heavier unconventional oils.

We would also argue that as long as these definitions are applied consistently, then potential limitations of including light tight oil as “conventional” can be overcome. We understand there are many possible conditions for oil to be considered conventional or unconventional as identified by the reviewer: “monetary considerations, the fuel itself, or geologic/technical reasons, among others”. However, we have chosen the density of oil °API as a singular determinant and have applied this consistently. Any cateogorisation of fossil fuels is open to a large degree of subjectivity, with the IEA (World Energy Outlook, 2019) stating “what is unconventional today may be considered conventional tomorrow”. The IEA themselves categorise tight oil separately from conventional, however in order to keep the analysis to two overarching categories (conventional vs. unconventional), we have included tight oil in the former, given it is far more closely aligned with the chemical formation of conventional oil than with bitumen, kerogen or ultra-heavy oil.

As far as methane gas is concerned we again use the same definition provided by McGlade and Ekins (2015) where tight gas, shale gas and coalbed methane are classed as unconventional. We have amended the main manuscript on the reviewer's suggestion removing the term directional drilling as a precondition for unconventional; we were highlighting the heavier reliance on horizontal drilling in unconventional reservoirs to yield commercial flow rates but have removed "directional" to avoid any confusion. The categorisation of light tight oil as conventional and tight gas as unconventional to us is not a contradiction given they are two different commodities, and the reservoir conditions of tight gas (permeability and porosity) fall within our definition of unconventional for gas. We are aware that some institutions, including the BGR, now define tight gas as conventional; however we again propose that as long as these definitions are applied consistently, they do not result in a fundamental flaw in this categorisation. Additionally, the categorisation does not in practice have direct bearing on the derivation of unburnable fossil fuels in our work (i.e. the model makes an endogenous decision of where oil and gas get produced based on the relative cost of each production category and constraints on production growth/decline). Finally, there is no difference between conventional and unconventional gas in a chemical sense (i.e. they are both methane), and therefore the categorisation of gas differs to some extent to oil where the chemical composition of the output commodity from oil mining processes do differ. We hope this short description covers the concerns of the reviewer.

My second concern is the fossil fuel database. I have not been able to verify the values of the fossil fuel reserves and resources input data because the numbers are neither provided in the ms nor in accompanying documents. I am sure that these data are extensively discussed in the thesis of McGlade and the (forthcoming) thesis of Welsby, however, the input data for modelling should come along with this ms. Thus, I recommend to present the data and to compare the reserve database (perhaps in the supplements) with other publicly available global fossil fuel reserve data. There are certainly significant uncertainties within regional and global estimates of fossil fuel reserves. This is even more true for resources estimates. Such uncertainties need to be addressed and taken into account when modelling is performed. To my surprise, the figures presented in this ms do not have any (probability) ranges indicated, so there are no uncertainties?

We hope the discussion in the SI and amendments in the main manuscript (as per the previous response) alleviate any concerns the reviewer may have. We would also highlight the representation of uncertainty has been covered in part in the SI already e.g.

an example of the uncertainty ranges for technically recoverable shale gas resources in Central and South America is shown in SI Figure 12. These were constructed from bottom-up estimates of play level technically recoverable resource ranges.

The data inputs for oil and gas are therefore the central (median) assumptions for different geological categories. We completely appreciate the reviewer's comment on uncertainty analysis and all of the inputs are the result of widespread literature reviews and/or probabilistic analysis of reserve/resource uncertainty. We have only run the scenarios in this work with our central (median) reserve/resource assumptions for two main reasons: 1) We simply do not have the scope (word count) to undertake a systematic sensitivity analysis of all three fossil fuels, which could be a very interesting paper in its own right, 2) TIAM-UCL is pushed to the brink of feasibility in the central 1.5^{o}C scenario, by which we mean that any shifting of oil/gas supply cost curves would have negligible impacts. This was explored by Welsby (forthcoming) (albeit only for gas), where shifting availability of gas was found to have significant impacts on cumulative production in an "NDC" case (similar to the IEA's STEPS), but < 1% difference between central resource assumptions and higher availabilities of gas in scenarios limiting temperature increases to 1.5 degrees.

Important emission sources missing in this study are methane emissions from the supply chain of hard coal and crude oil, while methane emissions from the natural gas supply chain are included. Methane emissions from lignite may indeed be negligible, but those from coal and crude oil (e.g.3 4) are not. According to the IEA5, about 40 % of the O&G supply chain methane emissions are attributable to the oil sector. This number may not be definitive; nevertheless, this aspect needs to be considered. As methane is a much more potent greenhouse gas, the exclusion of these emissions is expected to skew the results.

Methane emissions from the oil and coal supply chains are covered by TIAM-UCL, however these were only very briefly mentioned in the original manuscript (Line 257-258). We have now briefly covered how methane emissions from the oil supply chain are represented in TIAM-UCL in SI Section 5 (the section has been retitled "Supply chain emissions from upstream fossil fuel activity", Line 924 onwards). We would also like to draw the attention of the reviewer to the section on associated gas (just above Supplementary Table 13). Methane emissions from the oil supply chain are often due to leakage of associated gas during oil production. TIAM-UCL has options to build infrastructure in order to minimise these leakages by utilising the methane, as well as a dummy option which allows the associated methane to be flared instead of vented (i.e. emitted as CO_2 instead of CH_4). This is also the case of methane produced as a by-product of coal mining (aside from designated coal bed methane mining), where there is a dummy option within the model to utilise the methane (i.e. consume it in an end-use or secondary transformation sector) or flare it emitting CO_2 rather than venting into the atmosphere as methane.

Another issue is that a minor share of fossil fuels is used non-energetically, i.e. is not burned but used for the production of plastic, medicals, fertilizers, etc. As far as I understand, this is only taken into account for years 2050 onwards. Thus, I recommend explaining this (if considered before 2050 as well) or including the non-combusted feedstocks in the calculations for the years up to 2050 as well, since this production will definitely have an impact on the reserves and resources base. While the non-combusted feedstock share of crude oil post-2050 appears quite high, the non-combusted share of natural gas may be underestimated in this study (Fig. S6). According to IEA8, some 205 bcm natural gas is used globally for hydrogen production, mainly for fertilizer. This alone represents 6 % of global natural gas consumption.

Oil and gas used as feedstocks are included across the modelling analysis time horizon i.e. from 2010. We have made this clearer (particularly in the SI), that feedstock oil and gas are included in our estimates of "burned" fossil fuels throughout the whole time horizon (i.e. 2018-2100). In respect of Supp Figure 6, we are only showing feedstock shares from 2050 because they become an increasingly important part of the mix in the second half of the century. This is part of the reason that we argued that extending our analysis beyond 2050 was important, to highlight these changing shares. We would also like to add that in 2020 total feedstock gas consumption across different sectors in TIAM-UCL is 324 bcm (or 8% of 2018 total consumption). Because of the sectoral aggregation in TIAM, fertiliser production is not explicitly modelled as a standalone technology, but is instead incorporated into the wider service demand for chemicals. Within this, separate fuel technologies can provide feedstocks to non-energy petrochemicals (which would include fertiliser production). From 2030, the share of gas in the petrochemical subsector drops rapidly to 2055, before picking up again. This is for a range of reasons but most notably there is a temporary replacement of gas for longer chain hydrocarbons in this sector, in large part due to a lower emissions intensity. Additionally, the bulk of hydrogen production in TIAM-UCL is from electrolysis by 2050 (98%). We have added a brief discussion on this switch from gas to lower emissions intensity feedstocks between 2020 and 2050, and subsequent increase of feedstock gas consumption post 2050 from lines 224-229 in the SI.

What I do miss in this ms is a discussion of the results with respect to other estimates of changes in future oil and natural demand, supply and net trade position, as e.g. with the three IEA scenarios. Are there differences and if yes, what is the origin for such differences?

We thank the reviewer for this important point on comparing our results to other production pathways. We note that we do compare our production trajectories for all 3 fossil fuels to results from the IPCC database (see SI Section 4). There is also a reporting issue comparing our production pathways to the IEA, given they only report to 2040, and our unburnable calculation incorporates 2018-2050. We would also suggest that comparing our 1.5 scenarios to some of the IEA scenarios ("Stated Policies" and "Current Policies") would not provide any notable insights given the hugely divergent climate targets in each. We have added a brief comparison in SI Section 4 (Line 276-279) between our cumulative production values and those of the IEA's Sustainable Development Scenario between 2018 and 2040.

We are also constrained in terms of what we can put in the main manuscript and decided to focus on the main narrative of our paper (i.e. unburnable fossil fuels and the production pathways required to meet a 1.5 target). In particular, if we were to dive into why different scenarios generate different results this opens up a wider discussion around data input assumptions, modelling methods (e.g. optimisation vs. simulation etc.), user constraints etc., which is outside the scope of this work, but would nevertheless be a very interesting exercise.

One criticism of the cost-optimal approach used here might be that it does not take into account the political economy of fossil fuel production and use, including carbon prizing. So, as a final general comment, I would have liked to see more discussion of the use of coal versus O&G, especially natural gas. What effect do carbon pricing and regional coal phase-out decisions have on natural gas use? If not decarbonized, natural gas will have to be phased out as well, by 2050 at the latest; the interesting question is the prediction of the next decades. I encourage the authors to explore and discuss this question a bit further. At the same time, the a bit unfocused discussion on fossil fuel phase-out strategies could be shortened (Discussion section).

We have provided more transparency on the coal to gas switching question, for large coal-consuming regions (notably China). However, because the focus of this paper is supply side (granted supply and demand in global gas markets are inextricably linked) and due to space limitations, we have kept this relatively brief. From Line 182-184 in the main ms we have provided peak Chinese gas demand (and the level of imports required), and the huge transition risk of switching to gas as an intermediate fuel given post 2035 decline of gas consumption. Welsby (2021) explored the issue of coal to gas switching in these economies in more detail, however for this paper we focus on the production decline of major producers.

Other comments:

Abstract

• The findings of this study will not only have "significant implications for oil and methane gas producers" but also for O&G consumers whose consumption will need to decline rapidly to ensure climate goals are met. Why exclusively the producers are addressed in the Abstract is not clear to me.

We have slightly altered this to "fossil fuel producers", given a reversal in current coal production is also required. We mention producers here because the main focus of the paper is on the supply side; i.e. unburnable fossil fuels and therefore the decline of fossil fuel production required. We appreciate that the demand side is also incredibly important (including demand side interventions to shift fossil based consumption to low carbon alternatives) and supply and demand are interdependent, however we focus our attention on the supply side and the scale of production decline required to meet our carbon budget.

Main text

• Production decline of major producers – "The outlook is one of decline, with 2020 marking both global peak oil and methane gas production" – Well, for natural gas, the cited reference for the oil demand peak, BP 2020, says something different. Even for the "Net Zero" scenario, peaking natural gas consumption is proposed to occur not before the mid-2020s while the other scenarios propose an increase in natural gas consumption.

The "Net Zero" scenario from BP suggests peak gas consumption in the mid-2020s, which is the most closely aligned of the three BP scenarios to those conducted in this work. Therefore, we would argue this is a sign even from an oil and gas major that gas consumption compatible with below 2°C needs to peak in the next five years (in BP net zero scenario), whereas we suggest gas consumption needs to peak now. For oil, the "Rapid" and "Net Zero" indicate that "oil demand never fully recovers from the Covid 19 pandemic", suggesting peak oil demand is now (or even passed), as proposed in this manuscript

• I did not understand why the "scenarios for the global fossil fuel industry (result) very likely (in) an underestimate…" I do agree that there are high "risks underestimating the required rate of emissions reduction" but a "risk for an underestimate" is different to "very likely an underestimate"?

The main reason why we suggest this is "very likely an underestimate" of what is required is because the carbon budget we have used is of a 50% chance of meeting 1.5°C.

Therefore, if we were to constrain the budget even further to a 66% probability of meeting 1.5°C then the unburnable numbers would increase.

• Tables: abbreviations must be explained. Oil volumes are presented in Gb – I guess this means Giga barrels. If so, I suggest changing to SI units (e.g. gram, or Joule). Further, there is a mixture between tcm and Tcm in Table 1. What is $M (Table S9)?

We hope the alterations in the SI have made these clearer (we have added definitions under Table S1 for Gb, Tcm and Gt, and stated that $M is million dollars under Table S15 where it first appears) and thank the reviewer for bringing this to our attention as we completely agree clarity in what units mean is crucial.

• An explanation which of the available definitions for "Arctic" is used in this ms is missing (latitude, temperature, etc.). If e.g. the Arctic Circle is used, Southern Greenland, with its climate conditions and ice cover being similar to the high Arctic and thus similar vulnerable would not be part of the Arctic.

We have stated that the geographical extent of our estimates for undiscovered Arctic oil and gas is derived from the USGS CARA study. We understand that this means that volumes of oil and gas which are in climatic regions which could justifiably be defined as "Arctic" are excluded from our "Arctic" category, however we hope we have been clearer in the volumes of oil and gas our Arctic category includes (see Lines 357-361 in the main ms where we highlight the USGS CARA study).

Methods section

• What do the authors mean with "Some volumes of unconventional oil and gas are also categorised as reserves, …". Sure there are unconventional oil and gas reserves, production is ongoing since decades– but why are only some reserve volumes categorized?

We have removed text about unconventional reserves from the section describing categories of conventional oil and gas as we agree with the reviewer this was confusing.

We have added a separate sentence below stating "Whilst unconventional oil and gas do not have the same disaggregation in terms of mining technologies (i.e. there is no distinct "proved reserves" mining technology for unconventional oil and gas as with conventional reserves, but instead three different cost steps for the overall resource base), we have identified volumes of unconventional oil and gas which we categorise as reserves, with the relevant cumulative production from these mining technologies counted in the overall picture of unburnable fossil fuel reserves."

We hope this makes the representation of conventional and unconventional reserves clearer in terms of the representation of individual mining technologies in TIAM-UCL.

• I suggest being very careful about using directional drilling as an indication of unconventional exploration. Today, there may not be many non-directional wells in any O&G fields.

We agree with this suggestion and have removed the word "directional" in the main manuscript, instead focusing the definition of unconventional on geological conditions (e.g. permeability).

• Reference 37 (2016) is outdated. This report is published annually and the comparison should be done with the most recent version.

We have updated this reference to BGR (2019) and compared our coal reserves (including at a regional level) specifically to the most up to date BGR report.

• It is not recommended to cite publications that are not available, as e.g. a forthcoming publication (38.Welsby, D. Modelling uncertainty in global gas resources and markets (forthcoming). (University College London, 2021)).

We hope that the additional data and discussion provided in the main manuscript and the supplementary information alleviates any concern of using an unpublished thesis. Much of the SI discussion on the derivation of supply cost and resource estimates for gas is based on this thesis, and we have now provided data and references for all Figures/Tables. The thesis in question was submitted in March 2021, and we suggest it needs to be referenced in some way given much of the underlying representation of gas was taken from this work. We would also direct the reviewer to Pye et. al (2020) article linked below where examples of outputs from Welsby (2021) were published in the Supplementary Information of that work (and provided much of the representation of gas for the analysis on "An equitable redistribution of unburnable carbon").

https://www.nature.com/articles/s41467-020-17679-3

• Why are the global supply cost curves (e.g. Fig. 3) on a $2005 basis?
This is the base year of TIAM-UCL and all cost assumptions are in this unit.

Supplementary information

• The TIMES model "represents the countries of the world as 16 regions (Supplementary Table 20)". However, there is an inconsistency between the countries of the FSU (former Soviet Union; main text) and the Commonwealth of Independent States (CIS) as listed in table S20. FSU is missing in this table. Is Eastern Europe (Bosnia and Herzegovina, Bulgaria, Croatia, Czech Republic, …) part of EUR or FSU (e.g. in Fig. 1)?

Many thanks for bringing this to our attention. This is simply due to an oversight and CIS has been changed to FSU. Eastern Europe is part of EUR in our work (i.e. EUR aggregates the following regions in TIAM-UCL: Eastern Europe, UK, and Western Europe).

• The supplementary information terminates with the sentence "A number of these constraints are outlined in the next section". At least the most important constraints for this ms should be listed here.

This has been changed to: "For the purpose of modelling fossil fuels in a 1.5°C world, key constraints used to represent production growth and decline were outlined in Section 5 ("Key Upstream Constraints")". Section 5 provides a detailed analysis of both the assumed growth and decline rates, and the mathematical formulation of these asymmetric constraints (these can be found starting in Line 856 in the SI)

• In Supplementary Figure 6 the y-axis is incorrect (going two times to 100%).

Axis has been corrected.

• Please update: "Power of Siberia pipeline, which is due to come online in 2020"

This has been updated to "are based on the Power of Siberia pipeline, which came online at the end of 2019".

• There is a confusing mixture of endnotes numbers at the bottom of the pages and references numbers at the bottom of the manuscript.

The footnotes have either been deleted or incorporated into the main text in order to avoid confusion with the reference numbers.

• As stated previously, I recommend comparing the reserve database with other publicly available global reserve data for fossil fuels. Such fossil fuel inventories are provided by BP, BGR, OPEC, EIA, WEC, IEA to name just a few publicly available at no cost. It is perfectly clear to me that many of these inventories are subject to criticism; some of it may be justified. However, McGlade29 stated that "A database of reserve estimates has therefore been generated by choosing the most robust existing estimates for each country from the sources listed in Table 3.1. By carefully choosing and restricting the choice of studies, this approach helps mitigate or reduce many of the problems and uncertainties identified". This could be read as a relatively subjective selection and why these inventories have been selected has not been explained. Coal data was obviously compared in this ms with the BGR inventory (without giving details), it would be interesting to compare O&G reserves and resources as well. A rough overview of the McGlade and Ekins1 values (basically the selection of McGlade?), which somehow form the input for the crude oil estimates in this ms, shows differences with several of these inventories (Table 1). This obviously needs to be addressed by the authors.

Table 1: Sample crude oil reserves inventories (EIA, BP, OPEC, BGR) for comparison with some of the McGlade & Ekins estimates. To allow comparison with the McGlade & Ekins data, the 2010 values from the other inventories are presented. However, these are available on an annual basis. Please note considerable variations in inventory estimates, often resulting from different conventional/unconventional classifications (e.g. Canada). Overall, the inventories appear to be fairly consistent. Gbbl – Giga barrel.

Crude Oil Reserves (Gbbl) Base Year 2010					
	EIA	BP	OPEC	BGR	McGlade & Ekins
Africa	123,6	125,0	126,0	126,9	111,0
Canada	175,2	174,8	4,1	4,1	53,0
China+ India	26,0	29,0	29,2	29,0	38,0
Middle East	752,9	766,0	794,6	790,7	689,0
USA	25,2	35,0	23,3	30,9	50,0

As discussed above, we thank the reviewer for bringing this critically important part of this paper to our attention and hope the amendments we have made (including laying out both our reserve and resource estimates, and those of publicly available data) cover any concerns. The above comments have been covered in detail, particularly in SI Section 5 "Reserve and resource estimates" where both reserve/resource ranges and the sources used to derive these numbers have been laid out in more detail.

We would like to thank the reviewer for these incredibly useful and detailed comments, particularly on the representation and categorisation of fossil fuels. We would also like to thank them for the supportive message at the end which we very much appreciate! We hope the responses and amendments made sufficiently answer any remaining concerns.

Response to Reviewer #3

Originality and significance:
The model and data are quite similar to those used in M&E 2015 but with some notable updates and improvements, e.g. in capturing upstream energy use and emissions. (Note that it would be helpful to draw out the key enhancements and their significance in a paragraph in the main paper, as it required a review of supplementary information to appreciate these changes.)

We thank the reviewer for this comment and in particular for appreciating the extensive improvements made to TIAM-UCL since the 2015 McGlade and Ekins paper. However, given our space limitations within the main paper itself, we have decided to keep the bulk of the improvement discussion in the supplementary information. However, we have more clearly signposted to these updates in the main paper itself, whilst keeping the manuscript focused on the implications of unburnable fossil fuel reserves in a 1.5°C world.

We would also highlight that, particularly for oil, there are significant regional changes from the McGlade and Ekins paper in 2015. For example for oil, unburnable reserves in the Middle East increase from 38% in the 2015 paper, to 62% in this work.

As noted, the results are quite similar as well, especially in terms of relative implications by fuel and region, though it is interesting to see that the tighter budget has a significantly greater impact on reducing oil as compared with gas extraction. The reasons for that difference, since it is a notable finding, could benefit from brief reflection and discussion: to what extent is it explained by lower relative mitigation costs for oil vs. gas in specific sectors or technologies?

Linked to the point above, we have tried to provide more detail in the SI on the improvements made since McGlade and Ekins. One thing in particular which facilitates the increase in unburnable oil reserves is the updated representation of renewable generation technology costs and deployment rates, as well as the costs of electric vehicles in TIAM-UCL. We have provided data for the generation costs from solar and wind in SI Section 6 under Supp Table 21 (taken from Bloomberg New Energy Finance). In addition to this, the technical potential of these technologies (i.e. in terms of potential installed capacity for each region) has also been updated and is significantly larger than the assumptions made in McGlade and Ekins (based largely on NREL). This means there is far more potential low-carbon electricity to feed into the electric vehicle market, particularly for road transportation. Additionally, cost reductions in electric vehicles, including technology vintaging (i.e. cost reductions through time) is also included in TIAM-UCL, and these have been updated to reflect ongoing and future projections of cost reductions. We would also highlight the deployment rates of these technologies: for example, solar PV installed capacity has been growing at an annual growth rate of 36% between 2010 and 2019. Therefore, the upper growth rates in TIAM-UCL have been amended to reflect these rapid deployment potentials (for example, the 30% upper annual growth rate of solar PV capacity means the model can build solar PV at a maximum rate of 30%). This is also true for electric vehicles, where growth in battery electric vehicles has exhibited annual growth rates of 84% between 2010 and 2019 according to the IEA.

More broadly, since this analysis is an update and change in constraint relative to M&E, and it would be useful for the reader to better understand to what extent the change in results is due merely to the tighter constraint (580 vs 1100 Gt CO2) – i.e. as compared with changes in model parameters and assumptions (reserve estimates, supply cost curves, mitigation potentials, and so forth) and the change in start date. For example, it would appear that Canada's unusable oil reserves do not much decrease while those of other regions do: why?

We have provided a direct comparison between our cumulative production and reserve assumption figures and those of McGlade and Ekins in the SI Section 2. Given TIAM-UCL is under constant development, a range of factors, in conjunction with the tighter carbon budget, have led to the different results. The discussion above highlighted several of these key updates including the cost and technical potentials (i.e. maximum capacity potential in each region) of solar and wind power generation technologies and the costs of electric vehicles, as well as the deployment rates (i.e. speed at which these technologies can grow) of both.

We would also add that our intention was not to simply replicate the McGlade and Ekins study like-for-like, but to update under a 1.5°C carbon budget (50% probability) and then provide new insights (e.g. putting unburnable reserves within the context of the required managed production decline).

On the question of why Canadian unburnable reserves (83% in our work vs. 74% in McGlade and Ekins) do not shift as much as other regions, several dynamics are at play:

1. The tighter budget decreases the amount of oil which can be produced, therefore large producing regions (even regions with low-cost reserves) have to forgo production (e.g. Middle Eastern unburnable reserves increase from 38% in McGlade and Ekins to 62% in this work by 2050)
2. Given that relatively high cost, and high carbon intensity, oil sands form the bulk of Canada's reserves, the share of unburnable reserves is relatively high in both McGlade and Ekins and our work. In short, the model decides to produce elsewhere in both studies.

We hope the brief discussion above alleviates any remaining questions the reviewer has about why some results from this study differ from those of McGlade and Ekins.

The significance and the relationship of this analysis to M&E 2015 suggest, in this reviewer's opinion, that it warrants publication in Nature. The relative lack of novelty and limited extent of differences in findings, also suggest publication could take a reduced form, as briefer update rather than full length manuscript, highlighting the handful of more consequential findings and their implications – akin to those in M&E but starker, with greater risks for oil in particular -- with much of the material, especially on the regional distribution, moved to supplementary information.
For example, I would suggest replacing the section "production decline of major producers" altogether and present the key points in a single paragraph to sharpen the text and avoid excessive precision (see below). All oil producing regions show similar enough paces of decline and the expectation that US continues to lead increases in the near term may be undone already by recent market changes (See IEA's recent Oil market report https://www.iea.org/reports/oil-market-report-march-2021). What is particularly interesting here (to this reviewer) is the more nuanced picture for gas, which could be explained more briefly without reference to individual projects or individual regional decline rates at two significant digits.

We appreciate that the reviewer sees the benefit in publication of the paper. However, we would also like to reiterate the novel contribution of this paper, which we have tried to draw out more explicitly in the revised version of the manuscript (e.g. Line 29-32 introducing managed fossil decline insights at regional level).

Firstly, unlike McGlade and Ekins (2015), we have bridged the gap between the concept of unburnable carbon and the analysis of regional production dynamics required to achieve this. In short, we believe our work combines the aggregated findings of the Production Gap Report (2020) with the higher level concept of unburnable fossil fuel reserves. For example, we provide time-series production profiles (2018-2050) for the five largest producing regions of oil and gas, and have (albeit briefly) tried to reconcile these with current production levels and the direction of investments. These regional level insights into the actual application of a managed production decline were not included in the 2015 paper, and provide new insights for policy implications under a 1.5°C target.

Secondly, and whilst we agree that the general method of the paper is similar to the McGlade and Ekins (2015) paper, we have used a significantly reduced carbon budget and provided context to the post-2050 time period, extending the analysis out to 2100. This is important, as the unburnable estimates in 2100 provide insights on continued use of fossil fuels post-2050, with implications for the broader system such as the need for carbon dioxide removal options. For example, our entire 580 Gt CO_2 budget is used between 2018-2050 (with cumulative emissions between 2018 and 2050 reaching 631 Gt CO_2) and therefore any additional consumption of fossil fuels must be sequestered directly or compensated for by CDR while the system must also redress the carbon budget exceedance pre 2050 via global net negative CO2 emissions post 2050.

We also suggest that the detailed regional picture is crucial when it comes to practical application of the required production decline, and reconciling these with current production plans (i.e. putting the unburnable reserves into an operational context). As mentioned above, this is a crucial novel contribution of this work in that it bridges the gap between suggesting volumes of unburnable reserves/resources and how this actually materialises in terms of required production declines at a regional level. We would therefore like to retain this section of the paper, which is also flagged as a key element of the paper by Reviewer 2.

Data & methodology: validity of approach, quality of data, quality of presentation
The overall approach appears robust and valid much as M&E 2015 was.
Given large uncertainties related to reserve and resources, especially by 2050, as extraction, as well as CCS and DAC and other technologies evolve, it is unclear whether extending the analysis beyond 2050 to 2100 adds significant insight or value to that overall analysis.

We would argue extending to 2100 is critical given the crucial role of CCS and negative emissions technologies (NETs such as BECCS and DAC) which the reviewer identified, notably post 2050. In particular, we explore sensitivities in the SI by varying the growth rates of CCS and NETs, finding that reducing these upper growth rates to 2.5% (or half of our central assumption of 5% upper AGR) leads to modelling infeasibility (i.e. demand cannot be met whilst keeping within the climate and carbon budget constraints). Additionally, we also identify in the main paper that our estimates are likely an underestimate of what might be required given we use a carbon budget associated with 50% probability of reaching 1.5 degrees and therefore either more reserves/resources need to remain in the ground or the deployment of negative emissions needs to be far greater. We would also argue that extending the analysis to 2100 shows where these residual emissions could be, and therefore where additional mitigation focus needs to be directed by policy makers. We also highlight the switch in gas consumption from predominantly a combustion fuel to largely as a feedstock input.

The use of the term "unburnable estimates" is confusing and somewhat misleading. A more accurate term would be "unextractable" since what is extracted is not necessarily burned – it includes fossil fuels that are used for petrochemicals not just energy use.

This is a good point and one that we have debated internally. Whilst we recognise that 'unburnable' is not a good descriptor as some of that unburnable reserve could be used as non-energy fossil fuels, we made the decision that it was important to be consistent with M&E. We have also used the term unburnable because combustion produces significantly more emissions than feedstock use, so while not perfect, it focuses on the key thing we shouldn't be doing with the fossil fuel reserves.

Conclusions: robustness, validity, reliability

The regional results are driven by least cost optimization. However, global and regional fossil fuel markets do not necessarily operate only in this fashion; they can be influenced by government production subsidies, cartel behavior, political sanctions, preferential trade agreements, and so forth. While on the whole, cost considerations dominate, these other factors could be more explicitly acknowledged as potential influences on the regional distribution of future production.

We thank the reviewer for this comment and completely agree with its sentiment. Fossil fuel markets exhibit numerous imperfections, however the modelling of these requires a more granular approach than TIAM-UCL is capable of representing in an effective manner. This was one of the main areas of research for the thesis of McGlade (oil) and Welsby (gas) where field-level bottom-up models were constructed to explore some of the very uncertainties the reviewer identified (fiscal regimes, subsidies, trade agreements under different indexation formula and contract durations). In general a systematic representation of the impact of the carbon budget we used on detailed elements of fossil fuel markets would require combining TIAM-UCL with these detailed bottom-up market models (as per the analysis conducted by Welsby). The focus of this study has been to suggest the cost-optimal allocation of production in the remaining 1.5°C (50% probability) carbon budget.

We provide some caveats for the use of TIAM-UCL below which we hope will address the comments by the reviewer:

1. TIAM-UCL is a global energy system optimisation model, and therefore as mentioned previously the more intricate, granular elements of fossil fuel markets cannot be captured. However, the model has a detailed bottom-up representation of fossil fuel resources and costs, as well as being able to effectively model whole energy system transitions including emissions across the energy system
2. The model has a large range of constraints which represent production dynamics of oil and gas fields, i.e. asymmetric constraints on oil and gas production growth and decline. These ensure that the ramp-up/down of oil and gas production reflect geological dynamics (albeit aggregated into the regions of the model).

> Additionally, the representation of trade flows has been updated (at least for natural gas), reflecting some of the dynamics around bilateral contracts and constraints on the construction of new pipeline/LNG facilities). A detailed description of these constraints (and their formulation) can be found in SI Section 5 "Key upstream constraints".

Additionally, in the second paragraph of the "Description of TIAM-UCL" section (in Methods), we state: "Regional coal, oil and methane gas prices are generated within the model. These incorporate the marginal cost of production, scarcity rents (e.g. the benefit foregone by using a resource now as opposed to in the future, assuming discount rates), rents arising from other imposed constraints (e.g. depletion rates), and transportation costs but not fiscal regimes. This means full price formation, which includes taxes and subsidies, is not captured in TIAM-UCL, and remains a contested limitation of this type of model". We hope this, along with the correspondence above is sufficient to address the concern of the reviewer.

Given these and other underlying uncertainties and model limitations, which are acknowledged in the paper, results are presented with excessive precision. Compare for example, with the abstract from M&E: "Our results suggest that globally a third of oil reserves, half of gas reserves and over 80% of coal"… should remain unused". Here, the second sentence states "58% of oil, 59% of methane gas… must remain unburden". Arguably no more precision than "three-fifths of oil and gas, and nearly 90% of coal" is needed or warranted here, and as noted above, "should remain unused" would be more accurate than "unburned".

We accept this comment and have altered percentages accordingly.

As noted above, I would suggest the use of the term "unusable" rather than "unburnable", especially if the results are extended beyond 2050 when so much of the fossil fuel extracted and used are not in fact burned.

This comment makes a very valid point which we identify in the main paper in the second paragraph of the "Unburnable fossil fuel reserves under a 1.5°C target" section and in SI Section 3 "Continued use of fossil fuels post-2050"; i.e. that some fossil fuels are not combusted but instead used as feedstocks.

We have also taken on board the reviewers correct insight that not all fossil fuels are combusted, with some instead used as feedstocks, with Reviewer 3 suggesting the use of another term such as "unusable". We have had extensive discussions within the authoring team about whether to switch to another term (unusable/unextractable were both suggested by the reviewers), however we decided to remain with the term "unburnable" albeit with an explicit reflection on the use of feedstocks in the main manuscript. This was for two main reasons:

1. The term 'unburnable' is very well know based on the 2015 McGlade and Ekins paper and we wanted to ensure that continuity of terminology, which we believe is important for communicating the key insights from this paper.
2. The term 'unburnable' is also important as the vast majority of fossil fuels that are extracted are also combusted, producing GHG emissions. The focus of the paper is on remaining within climate limits, and therefore restricting the extraction of reserves that are primarily used for fuel combustion.

Reviewer Reports on the First Revision:

Referee #1 (Remarks to the Author):

Thank you for a detailed response and revision. I will post my comments below your response where I have any remain questions, in blue text.

Referee #2 (Remarks to the Author):

Comments on the revised version of the manuscript "Unburnable fossil fuels in a 1.5°C world" submitted for publication to Nature by Dan Welsby, James Price, Steve Pye and Paul Ekins.

In my opinion, the manuscript by Welsby and others on the limits of fossil fuel extraction under stringent climate targets has greatly improved, and I enjoyed reading it. A lot of effort has gone into the new version. There is now perfect consistency in the data base for this study, and the classification of fossil fuels is clear. A comparison of the production trajectories for the different fossil fuels with the results from the IPCC database is given. Overall, the discussion section is much better to the point and concise.
For me, there are few points left that could easily be considered by the authors.

The discussion of reserve and resource estimates (SI Section 5) is particularly appreciated. I must say that I do not fully agree with the fossil fuel reserve and resource figures presented (logical, we publish different figures) and the basic explanations for the differences (methods: reserve estimates for oil and fossil methane gas and SI) are not really convincing in my opinion. My sense is that much of the uncertainty in petroleum reserves is caused by non-transparent reporting in many Middle Eastern countries while the challenges addressed in the SI are well known. However, I agree that a full discussion of this issue and the uncertainties involved is beyond the scope of the article. Therefore, I raise no further objections, especially since these differences are described and explained in the SI. However, I suggest in any case that the differences be mentioned in the main text, since they have implications for the percentages of noncombustible fossil fuel reserves reported. The estimates of global reserves for all fossil fuels, oil, gas, and coal in this study are systematically and significantly lower than most, if not all, other publicly available estimates. Perhaps the differences lie in the strict application of 1P reserves?

To avoid unnecessary confusion, perhaps especially, or only for non-native English speakers (like me), I still suggest using the term "natural gas" instead of "fossil methane gas". I see the authors' point that the terms "methane" and "methane gas" evoke many more negative feelings than "natural gas." Nevertheless, my advice would be to present scientific results in a factual and neutral manner and as simple as possible.

In the chapter "Production decline of major producers", I am wondering if the term "producer" should be changed to "producing country" or "region". Sometimes, indeed, state-owned enterprises are major producers, but often these are independent enterprises and what is meant here is the country/region.

In Figure 1, I suggest to additionally indicate the fossil fuel reserves for the regions shown on the map. This would help the readership to classify the percentages of unburnable reserves given. I think it would be easier for the readers if the countries and regions were consistent between figure 1 and table 1. The abbreviations (FSU, CHI, AUS, ...) can then be added to table 1 in the first column.

Congratulations to the authors for this impressive work.

Referee #3 (Remarks to the Author):

The authors are to be commended for their thorough consideration and response to reviewer comments. They have made a several valuable improvements and have stood their ground in response to other suggestions. While overall I would find the paper somewhat more convincing and easier to follow were they to have made a number of further changes, most of these are editorial and more subjective, and I trust the authors and Nature editors to sort them if and as appropriate. Overall, I find the submission, to be sufficiently valuable, robust, and timely to recommend publication, and offer the following further comments with those point in mind.

• Terminology. I find the authors' rationale for retaining "unburnable" not terribly compelling. Times have changes since the term was coined. It is now much more widely accepted that most fossil resources cannot be "burned" under a 1.5/2 deg world. Many producers are now pursuing strategies that would continue extraction without burning – from plastics to pyrolysis for grey or blue hydrogen, and might read into these findings that there is more resource to extract than what is "unburnable". And yet this analysis is suggests that is not the case and there is no additional to extract; as the unburned uses are already taken into account in the "burnable" fraction. I do wonder, therefore, whether it is more important to avoid this misinterpretation of results rather than retain a term that may be outliving its usefulness. That said, I understand the other perspective, and thus leave it to the authors and editors to make any further changes at their discretion. (Also why not simply say "fossil gas" or "gas"? The methane term seems unnecessary.)

• Comparison with M&E 2015. Thanks for the various clarifications, which make clear just how many differences there are in approach and assumptions. Consequently, it is difficult to fully decompose them, and I would thus agree with their point that this paper should not be framed as an update or direct comparison. Questions may continue to arise however, and thus I would recommend that the authors have a pithier explanation at the ready for the key factors explaining the difference. It need not however be included here, which could slow the publication process.

• General editing. One more pass to sharpen the prose would be valuable. For example, for abstract, the phrase "where a rapid decline is required" could be read as conditional, e.g. for some producers it is not required. Instead "as" or "given" for "where" would remove that question. The following could be rephrased -- "Globally, we find production needs to decline annually at 3% for oil and fossil methane gas respectively, requiring a reversal of the current direction of operational and planned fossil fuel projects." – so that it doesn't mix an overall trend (global decrease) with the (unintended?) suggestion that all projects must change course. Arguably it's the producers in aggregate that must. And the since probabilities aren't mentioned prior, the last sentence should note "higher probably than [criterion here]"... given the "[particularly large] uncertainties", since uncertainties abound for many assumptions.

Author Rebuttals to First Revision:

Referees' comments are in black text, and author responses are in red text.

Response to Reviewer #1 (N.B. We have included the previous comments/questions from Reviewer #1 in order to provide context to the dialogue provided in this response document)

As an update, it might offer insight on what are the update: the paper did say "This is a large increase in the unburnable estimates for a 2oC carbon budget previously published, particularly for oil." But how large? How reserve numbers change, how production numbers change, and how results change?

Given space limitations in the main manuscript, we have provided a full comparison to the 2015 McGlade and Ekins paper in SI section 2. As per the reviewer's recommendation this covers the difference in reserves between the two studies, the difference in cumulative production, and finally the difference in unextractable reserves (shown in terms of relative percentages and absolute physical units). We have recalibrated the McGlade and Ekins cumulative production so that production between 2018 and 2050 can be directly compared between the two studies. We have also extended this to compare unextractable resources directly between the two studies.

Can you clarify why oil numbers changes so much and that of coal and methane numbers have much smaller changes?

The change in oil numbers (i.e. % unburned) is largely driven by the significantly reduced carbon budget. The optimisation within the model decarbonises the cheapest sectors first (often called "low hanging fruit"), usually starting with the power sector and moving onto buildings/some industrial processes. However, the additional room in the 2°C budget used by McGlade and Ekins allowed significantly more oil to remain in the transportation (and to a lesser extent industrial) sector, given transport is one of the most costly sectors to decarbonise. In our work with the 1.5°C budget, this space in the budget is removed and therefore oil is increasingly phased out of the transport sector as well. On a related point, TIAM-UCL has seen significant development since the McGlade and Ekins paper in 2015, particularly surrounding the cost and technical potential of renewable generation technologies, as well as the costs of zero emission vehicles. This means that, in combination with the significantly reduced carbon budget, there is more electrification potential from zero carbon generation, at lower costs. These two factors combine to significantly increase the presence of zero emission vehicles in the transport fleet, and significantly reduce the consumption of oil.

SI Ln 142-143, typos on "2oC scenarios" and "1.5°oC scenarios".

Thank you for spotting this! These have been corrected.

For future scenario projection, the unburnable fossil fuel will heavily rely on modelling assumptions of CCS and other carbon removal technologies, however, are not well presented or overly simplified in the paper. For example, the paper assumes a 5% of CCS deployment rate starting from 2030? It would be more appropriate to let the model decide optimized approaches to supply energy at given climate constraints. The cost assumption of renewables vs CCS also needs some clarifications.

Thank you for highlighting this point, which we have clarified in the text. We have made it clearer in the SI, under Supplementary Table 21 (SI section 6), that 5% capacity growth constitutes the upper growth rate at which CCS technologies can deploy. However, it is up to the model whether CCS deploys at that rate or at lower levels. In short, the 5% rate is not exogenously forced on the model but instead the model can choose to build CCS capacity and other carbon removal technologies up to a maximum growth of 5% per year. The chosen start date of 2030 for commercial scale CCS is typical of other estimates in the literature. In terms of cost assumptions, we would direct the reviewer to the SI Section 6, where Supp Table 21 and 22 provide CAPEX and OPEX assumptions for renewable generation technologies, fossil generation with CCS, and BECCS technologies (including power generation, heat, hydrogen and Fischer Tropsch fuels). Additionally, Supp Table 23 provides CAPEX and fixed operational and maintenance (FIXOM)cost assumptions for the direct air capture technology in TIAM-UCL.

This is helpful. Please correct me if I get the impression wrong: the paper updated the carbon budget and reserves numbers, but the technology cost assumptions are kept the same as the 2015 paper? While the paper have regional representativeness, the model does not differentiate regional cost disparity?

The cost of technologies have also been updated since the 2015 paper, particularly for solar and wind generation technologies as well as electric vehicles (given the unprecedented cost reductions of these techs in particular). In SI Section 2 "Comparison to McGlade and Ekins (2015) paper", we have laid out where the main updates in the model have occurred since 2015 (including updating the technical potential and costs of renewable generation technologies, EVs etc).

Additionally, there are regional variations in terms of costs for these technologies, however in Table 21 we presented the global averages. We have added an additional caveat under Table 21 which now reads "All costs presented above are averages across the 16 regions in TIAM-UCL, however in the model costs are differentiated across the different regions". We hope this alteration removes any lingering concerns over the representation of regionalised costs in the model. We have also provided the full model (including databases) in a Zenodo repository, the doi of which has been provided at the end of our responses in this document.

The paper also mingling around fossil fuel supply and demand and creating confusion, using "fossil fuel consumption" might help to address this confusion.

It would be useful for the reviewer to highlight the specific terminology in the manuscript that is causing confusion. We use supply and demand at different points in the manuscript, often to differentiate between issues around production (supply) and consumption (demand). Whilst we appreciate the paper focuses on the supply side (fossil fuel production), the co-dependence between supply and demand means we feel discussing both is of fundamental importance.

I understand the paper uses demand and supply where it means supply and demand, what confuse me does supply always equals demand in this paper, the paper needs to clarify the relations better.

We thank the reviewer for clarifying where the confusion arises from the use of both demand and supply in our manuscript. In TIAM-UCL, we have exogenous projections of individual energy service demands based on different socioeconomic drivers. The model then chooses (within the large range of constraints) the optimal energy mix to satisfy that demand (i.e. it brings supply and demand into equilibrium). We have made some minor amendments to the methods section where we describe (Lines 281-7 onwards) how TIAM-UCL brings supply and demand into equilibrium on the reviewers suggestion. There is an additional elastic demand function which we also introduce briefly which lowers energy service demands as the cost of satisfying each increases (therefore service demands with limited and/or very expensive alternatives to fossil energy, would be expected to be more elastic and see a larger demand response).

Supplementary Table 15. "The maximum build rate of new solar PV and wind capacity each year is set at 30% of existing capacity in line with recent solar PV build rates". Is it should be the modelling results of TIMES? Same question for the 5% CCS growth. There is a range in the hydro-dam costs, what costs, in the end, are used in the model? The cost of onshore wind might be an overestimate for 2040/2050. The CAPEX costs are key assumptions and the paper did show sources of certain assumptions, however, are there any rationales or scenarios why those costs are picked? For example, low renewable costs scenarios, low CCS costs scenarios? Are sensitivity scenarios only include demand assumptions?

The TIMES model formulation allows constraints to be set which may or may not be binding. For example, the 5% (30%) growth rate on CCS (solar PV) technologies allows a maximum 5% (30%) annual growth of existing capacity. However, the model solution decides the optimal deployment of these technologies and therefore the 5% (30%) may or may not be binding, with the model free to choose growth rates up to a maximum of 5% (30%). For a more definitive exploration of the functional form of these user constraints we would direct the reviewer to the Supplementary Information (SI) Section 7 and the second paragraph of SI Section 3.

Regarding the CAPEX costs, we thank the reviewer for making these points. For dam-based hydro, the cost range reflects the cost variation for the different tranches of this technology, of which there are 5 within the model, with 1 cheapest and 5 most expensive. For onshore wind, the CAPEX is an unweighted average across the 16 regions based on data from BNEF. Here, Japan, ODA and MEA push the average up with regions such as China seeing costs of 711 £2005/kW.

The CAPEX assumptions are derived from a variety of sources and we make every effort to keep them up-to-date based on best available data. Our focus here is to do our best to mitigate the critique that IAM cost assumptions are often out of date, particularly those of variable renewables which are rapidly evolving. We therefore draw on recent data from BNEF and other sources.

Our sensitivity scenarios cover various key dimensions of the model that are particularly relevant to the analysis at hand including the size of the biomass resource available (crucial because this shapes the role of BECCS), the speed with which CCS can deploy (the upper limit, again critical for the role of fossil fuels) and the scale of certain important energy service demands (i.e. those which the model has either no or limited ability to abate directly thus relying on negative emissions). In response to the reviewer's suggestion, we did run a low-cost CCS scenario, but found this did not alter our unburnable reserve numbers. This is largely because TIAM is being pushed to the very limit of its feasibility with the carbon budget we used and therefore there is no more 'room' within the budget for any more fossil fuels. However, we would direct the reviewer to the sensitivities we conducted in the Supplementary Information Section 3 on CCS deployment rates which have a larger bearing on the modelling results (e.g. in the low CCS deployment scenario, with an upper annual growth rate of 2.5%, the model cannot satisfy energy service demands without the use of a 'backstop' technology which removes CO2 (~ 1.5% of the total budget is removed from this backstop technology in the low CCS deployment sensitivity)).

Regarding other potential scenario dimensions, an important point here is that TIAMUCL is already at its feasibility limit when modelling an energy future that stays within a 1.5o C-50% probability carbon budget. In doing so the model moves as fast as possible to decarbonise the energy system, pushing deployment rate constraints for key low-carbon technologies to their limit. As such, we have found CAPEX considerations to be a secondary factor, i.e. it is more about how fast the transition can proceed than the precise details of technology costs.

Furthermore, during our initial testing of our scenarios we found that lowering our CCS cost assumptions had little impact on our results and so opted to focus on build rate limits. For renewables, and particularly variable renewables which are expected to become a key foundation of the energy system, we note that our base scenario already sees a rapid deployment of wind and solar PV with a 75% and 85% (both global averages) share of annual electricity generation from these technologies in 2050 and 2100 respectively. This is combined with a drive to utilise this low-carbon electricity by increasing the electrification of the energy system. Therefore, we do not believe that lower renewable costs would alter our results substantially.

I agree the modelling structure and relations are more important than specific technology and costs assumptions. My comments here are most related rationales/justification of your assumptions, for example 5% CCS rate which doesn't have a historical reference yet. Sensitivity is an helpful way to put those assumptions in context.

We completely agree with the reviewer on both points (i.e. 5% being relatively arbitrary given no historical precedent) and the therefore the importance of conducting sensitivities around this upper annual growth rate. Uncertainty around CCS deployment rates is the primary reason why we ran two additional scenarios (discussed in detail in SI Section 3) which halved ("1.5D-Low CCS", with a 2.5% upper annual growth rate) and doubled ("1.5D-HighCCS", with a 10% upper annual growth rate) the rate at which CCS capacity could be built.

Can you clarify "The code underlying the TIAM-UCL model is available at this link https://github.com/etsap-TIMES/TIMES_model". Is TIAM-UCL TIMES model? If they are the same, what's the added value of TIAM-UCL?

Yes, TIAM-UCL is a model that uses the TIMES modelling framework. The code underlying this framework, as provided at the github link, provides the set of mathematical equations which represent the 'rules' of the energy system (as described in

SI section 7). SI section 7 also outlines the other components of a TIMES model, like TIAM-UCL, in addition to the code. This includes the structure of the system (or reference energy system) and the range of different assumptions used to parameterise the different parts of the system. These elements are unique to TIAM-UCL compared to other TIMES models, based on many years of development of model structure and input assumptions.

I appreciate the efforts the team has put on it, then the paper should share the TIAM-UCL modelling codes and data if published, not the TIMES model link.

We thank the reviewer for all their comments throughout this review process. We believe our manuscript has been significantly improved because of the suggestions/questions raised by the reviewer. Finally, we have uploaded the entire model onto the Zenodo repository (link provided below) and provided the doi in the main manuscript in addition to the TIMES source code hosted by ETSAP and hope this will help any remaining issues on data transparency etc.

https://zenodo.org/record/4725672#.YLYsxKhKhaR

Response to Reviewer #2

Comments on the revised version of the manuscript "Unburnable fossil fuels in a 1.5°C world" submitted for publication to Nature by Dan Welsby, James Price, Steve Pye and Paul Ekins.

In my opinion, the manuscript by Welsby and others on the limits of fossil fuel extraction under stringent climate targets has greatly improved, and I enjoyed reading it. A lot of effort has gone into the new version. There is now perfect consistency in the data base for this study, and the classification of fossil fuels is clear. A comparison of the production trajectories for the different fossil fuels with the results from the IPCC database is given. Overall, the discussion section is much better to the point and concise. For me, there are few points left that could easily be considered by the authors.

We would like to thank the reviewer for the incredibly detailed comments/suggestions from our initial submission on our fossil fuel reserve and resource databases. We believe, using the detailed feedback the reviewer provided, that our paper (and the supporting supplementary information document) is now significantly improved because of this. The encouragement across the review process has also been greatly appreciated.

The discussion of reserve and resource estimates (SI Section 5) is particularly appreciated. I must say that I do not fully agree with the fossil fuel reserve and resource figures presented (logical, we publish different figures) and the basic explanations for the differences (methods: reserve estimates for oil and fossil methane gas and SI) are not really convincing in my opinion. My sense is that much of the uncertainty in petroleum reserves is caused by non-transparent reporting in many Middle Eastern countries while the challenges addressed in the SI are well known. However, I agree that a full discussion of this issue and the uncertainties involved is beyond the scope of the article. Therefore, I raise no further objections, especially since these differences are described and explained in the SI. However, I suggest in any case that the differences be mentioned in the main text, since they have implications for the percentages of noncombustible fossil fuel reserves reported.

The estimates of global reserves for all fossil fuels, oil, gas, and coal in this study are systematically and significantly lower than most, if not all, other publicly available estimates. Perhaps the differences lie in the strict application of 1P reserves?

We agree that transparency is a significant issue, particularly in large oil (and gas) reserve holders in the Middle East which we did not identify in our last manuscript, and have therefore added the Middle East as a key region where our reserve numbers differ significantly. The key issue of "political reserves" was raised by McGlade [1] based on the discussion by Laherrere [2] for countries with large oil resources, where governments report inflated reserve volumes (e.g. by including sub-optimal or undiscovered volumes of oil/gas) to generate political leverage etc, and we now explicitly identify this in the main manuscript. We raised this issue in our discussion in SI Section 5, highlighting the cases of Russia (for gas) and Venezuela (for oil) where the reporting of 1P either lacks transparency and/or seems to be using an alternative definition of 'proved reserves'. Whilst we did not directly allude to the term "political reserves" in our previous manuscript, where countries report volumes of oil or gas as proved reserves but knowingly include volumes which are sub-economic/undiscovered etc., this is a significant reason for our lower reserve numbers as identified by the reviewer. We have also highlighted that our estimates are systematically lower than public sources in the main manuscript and identified the methodological reasons for these differences (bottom up derivation of 1P using field-data and probability distributions) on the reviewers recommendation (Lines 410-413).

The reviewer is correct in noting that our 1P numbers are systematically lower than other publicly available reporting sources. They are also correct in suggesting that the difference largely lies in a stricter and more systematic application of the definition of 1P using field-level data and uncertainty distributions where possible, and stated this in the manuscript. Additionally, as far as coal is concerned, we would like to reiterate that some of the remaining discrepancy between our reserves numbers and the BGR estimates comes from energy conversions from joules into tons coal equivalent. Additionally, we note in SI Section 5 (just above Supp Table 9) that our combined hard coal reserves for India and China (the two largest coal consumers) are almost exactly the same as those of BGR.

References

[1]. McGlade, C. Uncertainties in the outlook for oil and gas.

https://discovery.ucl.ac.uk/id/eprint/1418473/2/131106%20Christophe%20McGlade_PhD%20Thesis.pdf

[2]. Laherrere, J. Future of oil supplies.

https://journals.sagepub.com/doi/abs/10.1260/014459803769520061

To avoid unnecessary confusion, perhaps especially, or only for non-native English speakers (like me), I still suggest using the term "natural gas" instead of "fossil methane gas". I see the authors' point that the terms "methane" and "methane gas" evoke many more negative feelings than "natural gas." Nevertheless, my advice would be to present scientific results in a factual and neutral manner and as simple as possible.

We thank the reviewer for this comment and completely understand why they have suggested using the term ‘natural gas’. We can assure them we had lengthy discussions around this topic, however we feel that on balance because the chemical composition of gas is (by the time it is transported downstream, almost entirely) methane, the term ‘fossil methane gas’ provides both scientific robustness and brings this terminology to a wide audience. We would also add that we are not necessarily trying to sway the debate by using a “negative” term, as we believe our empirical analysis achieves that end, but rather shift the terminology around methane gas from that used by the fossil fuel industry which draws far more positive connotations.

In the chapter "Production decline of major producers", I am wondering if the term "producer" should be changed to "producing country" or "region". Sometimes, indeed, state-owned enterprises are major producers, but often these are independent enterprises and what is meant here is the country/region.

We agree with the reviewers sentiments and realise this is an important element we overlooked in terms of terminology and reconciling an aggregated regional model vs. actual producing agents (whether International Oil Company, National Oil Company, or independent small scale producers). We have altered the title of the section to "Production decline of major producing regions" and altered the second sentence to "Figure 2 shows the outlook to 2050 for the five largest oil and fossil methane gas producing regions". We hope this will suffice to frame the section in terms of regional production, rather than individual producing entities.

In Figure 1, I suggest to additionally indicate the fossil fuel reserves for the regions shown on the map. This would help the readership to classify the percentages of unburnable reserves given. I think it would be easier for the readers if the countries and regions were consistent between figure 1 and table 1. The abbreviations (FSU, CHI, AUS, ...) can then be added to table 1 in the first column.

We thank the reviewer for spotting the inconsistency between Figure 1 and Table 1 and completely agree that consistency of region definition should be applied. We have therefore altered Figure 1 so it matches the regional aggregation in Table 1. On providing fossil fuel reserves on the map, we have not included this due to presentation issues. However we believe that combining Figure 1 and Table 1 (unextractable percentages and absolute volumes of unextractable fossil fuels) allows the reader to derive fossil fuel reserves and understand the geographical variation.

Reviewer #3

The authors are to be commended for their thorough consideration and response to reviewer comments. They have made a several valuable improvements and have stood their ground in response to other suggestions. While overall I would find the paper somewhat more convincing and easier to follow were they to have made a number of further changes, most of these are editorial and more subjective, and I trust the authors and Nature editors to sort them if and as appropriate. Overall, I find the submission, to be sufficiently valuable, robust, and timely to recommend publication, and offer the following further comments with those point in mind.

We sincerely thank the reviewer for their valuable feedback on the last manuscript, and we are pleased they find the revisions/responses we have made as an authoring team to be helpful and valuable in improving the paper. As an authoring team, we would like to thank the reviewer for all comments, suggestions and questions (particularly the thought provoking discussion around unburnable and unextractable!), and we believe our manuscript (and supplementary information) have been significantly improved over the review process.

• Terminology. I find the authors' rationale for retaining "unburnable" not terribly compelling. Times have changes since the term was coined. It is now much more widely accepted that most fossil resources cannot be "burned" under a 1.5/2 deg world. Many producers are now pursuing strategies that would continue extraction without burning – from plastics to pyrolysis for grey or blue hydrogen, and might read into these findings that there is more resource to extract than what is "unburnable". And yet this analysis is suggests that is not the case and there is no additional to extract; as the unburned uses are already taken into account in the "burnable" fraction. I do wonder, therefore, whether it is more important to avoid this misinterpretation of results rather than retain a term that may be outliving its usefulness. That said, I understand the other perspective, and thus leave it to the authors and editors to make any further changes at their discretion. (Also why not simply say "fossil gas" or "gas"? The methane term seems unnecessary.)

We thank the reviewer for the initial thought provoking question around our use of the term unburnable. We have had significant internal discussions as an authoring team and have agreed to accept the term "unextractable" as suggested by the reviewer in the first iteration of the review process. We have defined "unextractable" in the main manuscript, Line 34-36) to be: "We define unextractable fossil fuels to be the volumes which need to stay in the ground, regardless of end-use (i.e. combusted or non-combusted), to keep within our 1.5°C carbon budget."

Whilst we maintain the term "unburnable" provides continuity with the 2015 paper, as well as provides context to the absolutely crucial aspect of our paper (i.e. the non-combustion) of fossil fuels, we do agree that this term is not all encompassing (i.e. it does not cover those fossil fuels which need to be left in the ground even if they are unburned post extraction (e.g. as feedstocks)). We believe the term "unextractable" encompasses all forms of fossil fuel consumption (i.e. combusted and non-combusted), and therefore our estimates of "unextractable" fossil fuels covers all forms of consumption regardless of the end use. We would also highlight that, given our demand projections in fossil-intensive sectors, oil and gas continues to be extracted post-2050, with the vast majority used as feedstocks (e.g. petrochemicals (including plastics), and hydrogen).

On the question of why we use fossil methane gas, rather than simply "fossil gas", we believe the term methane is critical given this is the chemical composition of fossil gas (small amounts of hydrogen sulphide etc. may be present but these are all but removed before transporting gas downstream). We also wanted to be clear that we are not including biomethane in this analysis, or other gases associated with the breakdown of organic matter. Therefore, we hope fossil methane gas provides a consistent and clear indication of methane gas which has formed in different geological formations due to the breakdown of organic matter (i.e. fossilised organisms) over millennia.

• Comparison with M&E 2015. Thanks for the various clarifications, which make clear just how many differences there are in approach and assumptions. Consequently, it is difficult to fully decompose them, and I would thus agree with their point that this paper should not be framed as an update or direct comparison. Questions may continue to arise however, and thus I would recommend that the authors have a pithier explanation at the ready for the key factors explaining the difference. It need not however be included here, which could slow the publication process.

We thank the reviewer for their feedback on comparing our work to the McGlade and Ekins paper, and we are appreciative of the positive response to our addition to SI Section 2 "Comparison to McGlade and Ekins (2015) paper". We have provided the full model database on the Zenodo repository (link provided below) which we hope will provide the required transparency for all modelling assumptions.

• General editing. One more pass to sharpen the prose would be valuable. For example, for abstract, the phrase "where a rapid decline is required" could be read as conditional, e.g. for some producers it is not required. Instead "as" or "given" for "where" would remove that question. The following could be rephrased -- "Globally, we find production needs to decline annually at 3% for oil and fossil methane gas respectively, requiring a reversal of the current direction of operational and planned fossil fuel projects." – so that it doesn't mix an overall trend (global decrease) with the (unintended?) suggestion that all projects must change course. Arguably it's the producers in aggregate that must. And the since probabilities aren't mentioned prior, the last sentence should note "higher probably than [criterion here]"… given the "[particularly large] uncertainties", since uncertainties abound for many assumptions.

We thank the reviewer for their suggestions on making the language sharper, particularly in the abstract. We hope we have improved the wording in the abstract, taking into account the suggestion of the reviewer along with suggestions of the editor. The sentence "Globally, we find production needs to decline annually at 3% for oil and fossil methane gas respectively, requiring a reversal of the current direction of operational and planned fossil fuel projects" has been changed to "Furthermore, we estimate that, globally, oil and gas production must decline by 3% annually until 2050. This implies that many regions face peak production now or during the next decade, making many operational and planned fossil fuel projects unviable". The reviewers suggestion that our decline rates cannot be used to infer a reversal of all *individual projects* is a valid point, and therefore we have made this less prescribed across every project.