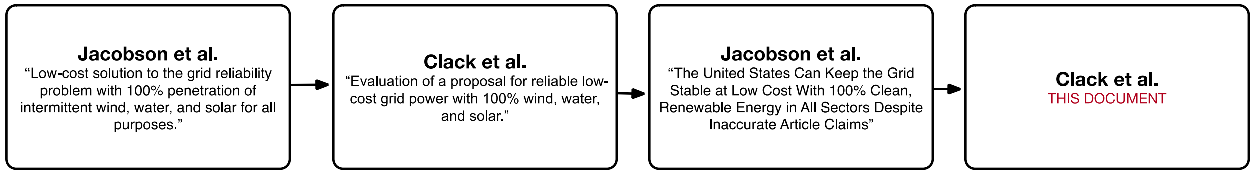
# Response to Jacobson et al. (June 2017)

## Introduction

This document contains point-by-point responses to the reply by Jacobson et al. to the article "*Evaluation of a proposal for reliable low-cost grid power with 100% wind, water, and solar*".

Previous analyses have found that the most feasible route to a low-carbon energy future is one that adopts a diverse portfolio of technologies. In contrast, Jacobson et al. have suggested that future primary energy sources for the United States should be narrowed to almost exclusively wind, solar, and hydroelectric power and that this can be done at "low-cost" in a way that supplies all power with a probability of loss of load "that exceeds electric-utility-industry standards for reliability". We have found and published that their analysis involves errors, inappropriate methods, and implausible assumptions. Jacobson et al. have, in their reply, tried to challenge some of the errors we have identified, and this is our response to that reply.

## Publications timeline



## Download links for reference:

1. Jacobson et al. original article
2. Clack et al. critique
3. Jacobson et al. reply
4. Clack et al response to reply (this document)

## Contact information

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## Summary of Jacobson et al. reply critique

Answers to nearly all of the criticism by Jacobson et al. presented here can be found by reading the original Clack et al. paper and, crucially, its supporting information document. The Jacobson et al. critique to which we respond here comes in three general forms:

1. Attempted references (including several self-references) to studies that have made the same mistakes as Jacobson et al. and/or have used the same or similar assumptions or have reached similar conclusions. In some cases, this referencing effort includes misrepresenting statements from the IPCC and others.
2. Defense by critique of other studies, most notably one paper co-authored by Christopher Clack[1]. Again, this is irrelevant for the matter at hand here.
3. Purposefully refusing to acknowledge clear mistakes. This is most clearly seen in this exchange from the discussion of installed capacity of hydropower in the Jacobson et al. models.

## Guide to this document

In this document, Jacobson reply-claims are stated in yellow boxes, our responses are in the attached grey boxes. In order to keep the claims in their exact original form, we refer the reader to the reference list of Jacobsons reply document for references written with square brackets: []. Most importantly, reference [1] refers to the Clack et al. critique of the Jacobson et al. paper, which is reference [2].

We have tried to keep answers here short, so for further information we refer the reader to the Supporting Information of the Clack et al. document, where most of these topics are expanded upon in greater detail.

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[1] An interesting side-note is that Mark Jacobson, in stark contrast to these most recent comments, quite recently wrote favourably of this article "*the study pushes the envelope to show that intermittent renewables plus transmission can eliminate most fossil fuel electricity while matching power demand at lower cost than a fossil-fuel-based grid, even before storage is considered.*", until finding out that one of its co-authors was involved in pointing out the fundamental errors of his own studies (Jacobson 2016).

**#1 Jacobson et. al claim**
*First*, [1] implies [2] is an outlier for excluding nuclear and CCS. To the contrary, Jacobson et al. are in the mainstream, as grid stability studies finding low-cost up-to-100% clean, renewable solutions without nuclear or CCS are the majority [3-16].

**#1 Response**
The point made by Clack et. al is that the a priori exclusion of potentially major contributors to a low carbon energy system is likely to lead to a sub-optimal solution. For instance, nuclear power produces over 60% of total US low-carbon electricity today, but in Jacobson et al. they are omitted entirely from consideration (even the use of existing, profitable, operational plants).

Contrary to Jacobson et al's assertion that successful renewables-only pathways emerge from the "majority" of studies, in a large recent comprehensive review of decarbonization studies, the only studies that did not include a significant contribution from nuclear, biomass, hydropower, and/or carbon capture and storage are those that exclude these resources from consideration to begin with (Jenkins and Thernstrom 2017). The studies that Jacobson et. al. reference illustrates the point. Not one of the studies cited include nuclear or CCS as options for the electricity mix, making the statement that these studies "find" solutions without these components rather obvious (once they are excluded, nothing else is possible).

Reference [4] is a *self-reference* (co-authored by Jacobson), references [5-9] and [10-11] are all produced by the same authors. Excluding the self-reference, the cited studies are produced by a total of 5 different author groups, thus doing nothing to validate the scientifically irrelevant claim that the Jacobson et. al type of study design is "*mainstream*" or in the "*majority*". None of the studies referred to make the claims that Jacobson et al. have made, and are thus not applicable.[2]

For a more detailed discussion of the references, please download the following document: "RESPONSE TO JACOBSON ET AL. CLAIM THAT THERE ARE MANY 100% RE STUDIES THAT BACK UP THEIR CLAIM TO RELY ALMOST ENTIRELY ON WIND, SOLAR, AND HYDRO"

**#2 Jacobson et. al claim**
*Second, IPCC [17] contradicts [1]'s claim that including nuclear or CCS reduces costs (7.6.1.1): "...high shares of variable RE power...may not be ideally complemented by nuclear, CCS,..." and*

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[2] For example, ref. [5] states more modestly: "*Although the results illustrate a potential 100% renewable energy-system for Ireland, they have been obtained based on numerous assumptions. Therefore, these will need to be improved in the future before a serious roadmap can be defined for Ireland's renewable energy transition*."

Ref. [12] states: "*The power capacities of the storage and balancing facilities are not determined; this would require a more complex modeling with explicit inclusion of power transmission. We focus on wind and solar power and assume no bottlenecks in the power grid, employ an optimal storage dispatch strategy* ***and ignore*** *storage charge and discharge capacities and* ***economic aspects***."

*(7.8.2) "Without support from governments, investments in new nuclear power plants are currently generally not economically attractive within liberalized markets,..."*

**#2 Response**
The above is a misrepresentation of the IPCC text, which clearly does not include any contradiction to anything stated in the Clack et al. analysis. IPCCs statement that "High shares of renewables ***may*** not be ***ideally complemented*** by nuclear and CCS" (emphasis added) is hardly motivation to not include either option in any analysis aimed at identifying optimal low-carbon energy systems.

**#3 Jacobson et. al claim**
Similarly, [18] state, *"...there is virtually no history of nuclear construction under the economic and institutional circumstances that prevail throughout much of Europe and the United States,"* and [19], who compared decarbonization scenarios, concluded*, "Neither fossil fuels with CCS or nuclear power enters the least-cost, low-carbon portfolio."*

Third, unlike Jacobson et al., IPCC, NOAA, NREL, or IEA has never performed or reviewed a cost analysis of grid stability under deep decarbonization. For example, [20]'s grid-stability analysis considered only electricity, which is only ~20% of total energy, thus far from deep decarbonization. Further, deep-decarbonization studies cited by [1] have never analyzed grid stability. [2] obtained grid stability for 100% WWS across all energy sectors, thus simulated complete energy decarbonization.

Fourth, [*1*]'s objectives, scope, and evaluation criteria are narrower than [*2*]'s, allowing [*1*] to include nuclear, CCS, and biofuels without accounting for their true costs or risks. [*2, 21*] sought to reduce health, climate, and energy reliability costs; catastrophic risk; and land requirements while increasing jobs. [*1*] focuses only on carbon. By ignoring air pollution, they ignored bioenergy, CCS, and even nuclear health costs [*22*]; by ignoring land use they ignored bioenergy feasibility; by ignoring risk and delays, they ignored nuclear feasibility, biasing their conclusions.

Fifth, [*1*] contends that [*2*] "places constraints" on technology options. To the contrary, Jacobson et al. include many technologies and processes not in Clack et al's models. For example, [*2*] includes but [*20*] excludes CSP, tidal, wave, geothermal, solar heat, any storage (CSP, pumped-hydro, hydropower, water, ice, rocks, hydrogen), demand-response, competition among wind turbines for kinetic energy, electrification of all energy sectors, calculations of load decrease upon electrification, etc. Model time steps in [*20*] are also 120- times longer than in [*2*],

**#3 Response**
While the co-authors of study [20] probably appreciate discussions on potential limitations of their model, all of the comparative statements above are entirely irrelevant to the matter at hand here.

**#4 Jacobson et. al claim**
[Clack] claims wrongly that [*MZJ*] assumes a maximum hydropower output of 145.26 GW even though [*2*] Table S.2 shows 87.48 GW. [*Clack*] then claims incorrectly that the 1,300 GW drawn in [*MZJ*] Fig. 4(b) is wrong because it exceeds 87.48 GW, not recognizing 1,300 GW is instantaneous and 87.48 GW, a maximum possible annual average (Table S.2, Footnote 4 and the available LOADMATCH code).

**#4 Response**
As is clearly stated in Clack et. al, 145.26 GW was the most generous interpretation that could be made (summing pumped hydro storage and hydropower outputs), somewhat reducing the massive hydropower modelling error in Jacobson et. al. This statement confirms that the error is actually more severe than this.

In addition, there is no basis or supporting analysis for the assumption that 87.48 GW could be an annual average hydropower output, since this would correspond to almost 3 times the average annual hydropower production in the US over the last three decades (US EIA 2017).

**#5 Jacobson et. al claim**
1,300 GW is correct, because turbines were assumed added to existing reservoirs to increase their peak instantaneous discharge rate without increasing their annual energy consumption, a solution not previously considered. Increasing peak instantaneous discharge rate was not a "modeling mistake" but an assumption consistent with [*2*]'s Table S.2, Footnote 4 and LOADMATCH, and written to Clack Feb. 29, 2016.

**#5 Response**
Nowhere in the 28 pages of main and supplemental material of the Jacobson et al. paper is there any mention or analysis of an expansion of hydropower. As confirmed above, the installed capacity of the hydroelectric system is stated as 87.48 GW.

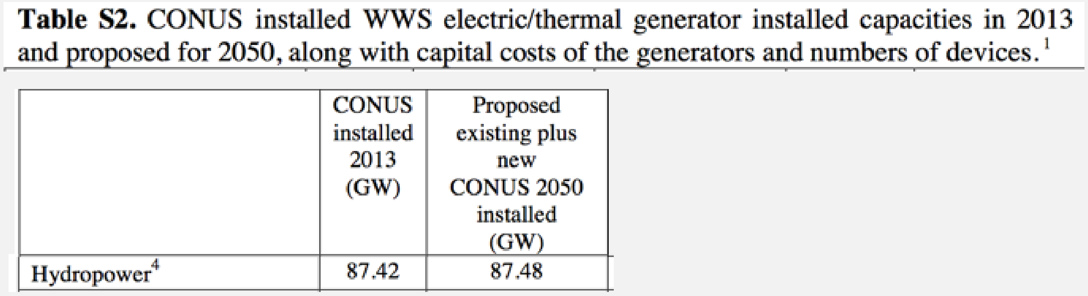

**Table S2.** CONUS installed WWS electric/thermal generator installed capacities in 2013 and proposed for 2050, along with capital costs of the generators and numbers of devices.[1]

| | CONUS installed 2013 (GW) | Proposed existing plus new CONUS 2050 installed (GW) |
|---|---|---|
| Hydropower[4] | 87.42 | 87.48 |

*Table S2, of the supporting information document of Jacobson et. al (2015)*

The scale of this error is staggering. The maximum instantaneous electricity generation capacity of *all* electricity sources in the United States today is 1170 GW (U.S. Energy Information Administration 2017). ***Jacobson et al. neglects to mention an assumed 1500% expansion in generation capacity of hydropower, leading to this system being capable of producing more power than all sources combined in the US today.***

One should note that the 1300 GW number is only what we have been able to infer from Figure 4 in the Jacobson et. al paper – it does not appear that any upper limit has been imposed at all on this value in the model. The capacity factor of wind power during the night of simulation day 1475 (in which 1300+ GW of hydropower is shown to be used) is around 24%. Since this is far above the likely minimum combined capacity factor of wind power seen during a night in a 5-year period[3], the actual installed hydroelectric capacity used in the model is actually far higher than 1300 GW. Perhaps even more alarmingly, had Jacobson et al. selected a time period for Figure 4 that did not happen to include high hydropower output, this error may never have come to light.

For the benefit of the reader, the footnote on the fourteenth page of the supporting information of the Jacobson et al. paper (Table S.2. Footnote 4) does nothing to change this error. It states, in full: "*Hydropower use varies during the year but is limited by its annual power supply. When hydropower storage increases beyond a limit due to non-use, hydropower is then used for peaking before other storage is used.*"

**#6 Jacobson et. al claim**

[*2*] only neglects the cost of additional turbines, generators, and transformers needed to increase the maximum discharge rate. Such estimated cost for a 1000-MW plant [*23*] plus wider penstocks is ~$385 (325-450)/kW, or ~14% of hydropower capital cost. When multiplied by the additional turbines and hydropower's fraction of total energy, the additional infrastructure costs ~3% of the entire WWS system and thus doesn't impact [*2*]'s conclusions. Increasing CSP's, instead of hydropower's, peak discharge rate also works.

[1] (Fig. 3) then claims mistakenly that [2]'s annual hydropower energy output is 402 TWh/yr and too high, when it is 372 TWh/yr because they missed transmission and distribution losses. This is less than half the possible U.S. hydropower output today, well within reason.

[*1*] next claims wrongly that [*2*] Table 1 loads are "maximum possible" loads even though the text clearly indicates they are annual-average loads. The word "maximum" is never used. They compound this misrepresentation to claim flexible loads in [*2*]'s time figures are twice "maximum possible" loads even though [*2*] P.15,061 clearly states that the annual loads are distributed in time.

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[3] For comparison, the minimum 1-hour combined capacity factor of all renewable energy sources in the EU (including wind power data from 12 countries and solar PV data from 5 countries) was 3.39%, 2.64% and 2.75% in 2012, 2013 and 2014 respectively.

**#6 Response**

In addition to not adding any costs at all to this, the Jacobson et al. study also neglects that additional turbines need extra water and therefore penstocks, tunnels, and space. Even disregarding all hydrological and legal constraints, one cannot simply assume that one can fit at least 15x more turbines in same space. A radically increased instantaneous flow rate would have a number of downstream impacts, such as: impact on other downstream (and upstream) hydro power plants, fisheries and ecosystem destruction, flooding of towns, illegal breach of water rights of downstream farmers and cities, loss of recreation and endangered species impacts.

For an output of 372 TWh/y, as stated above, the actual hydropower capacity factor of the WWS-system is at or below 3.26%. However, Jacobson et al. also states "*the annual average capacity factor of hydropower as used in LOADMATCH was given in Footnote d of Table 2 as 52.5% (before T&D losses)*". This is an assumed value based on a fictitious installed capacity of 87.48 GW and is therefore entirely nonsensical.

To illustrate one of the many problems that the omission of analysis regarding this capacity expansion entails, the Hoover Dam has been used as an example in Clack et. al supporting information section S.2.5.

Here are a couple more examples:

If the capacity at all major hydropower facilities are assumed to expand by the same relative amount, the Grand Coulee Dam would have a new peak power rating of 101 GW – more than all hydropower in the US combined today, and 4.5 times larger than the largest power plant of any kind ever constructed (the Three Gorges Dam). The required flow rate through the upgraded Grand Coulee Dam at full power would regularly need to be 5.5 times higher than the largest flow rate of its part of the river ever recorded in history, which occurred on June 12, 1948, during an historic Columbia River flood period (US Bureau of Reclamations 2017). This flow rate corresponds to 13 times the average discharge rate of the entire Columbia river system, 9 times higher than the peak discharge rate ever in January (when the Jacobson et. al. system assumes 1300 GW of total output), and 3.5 times the maximum spillway capacity of the Grand Coulee dam. One can only imagine the environmental impacts of the massive flooding of lands, towns and cities downstream of such reservoirs once water is released so rapidly.

The Robert Moses dam at the Niagara river (the 4th largest US hydro plant), once it is "upgraded", would then be relied upon to occasionally deliver up to 36.43 GW (by then also far larger than the world's largest-capacity power plant today). This would require a flow 6.3 times higher than the highest ever recorded flow rate of the entire Niagara river (recorded in May 1929), and about 18 times higher than its average total flow rate. To put it mildly, this project is hardly likely to be popular either with tourists, downstream and upstream residents or with the Canadians power plant operators drawing water from the same river.

The same type of examples as those above can be made for essentially all other major hydropower facilities in the US. As has been shown, the hydropower capacity error is one of many in the Jacobson et al. study, but it is so large (and so obvious) that it by itself invalidates the entire effort.

**#7 Jacobson et. al claim**

[*1*] asserts that UTES can't be expanded nationally, but we disagree. UTES is a form of district heating, which is already used worldwide (e.g., 60% of Denmark), UTES is technologically mature and inexpensive; moreover, hot water storage or heat pumps can substitute for UTES. Similarly, molten salt can substitute for PCM in CSP storage.

**#7 Response**

Clack et al. has not at all asserted that UTES (specifically BTES) *cannot* be expanded, but rather that the expansion suggested in Jacobson et al. is on an unrealistic scale and not appropriately costed. As stated, both UTES and PCM are promising resources, but neither technology has reached the level of technological maturity to be confidently used as the main underpinning technology in a study aiming to show the technical reliability and feasibility of an energy system.

The Jacobson et al. UTES analysis does not include an accounting of the costs of the physical infrastructure (pipes and distribution lines) to support these systems. The reference used by Jacobson et al. for costing of the UTES system is a 10-page conference publication, which includes a "financial model" spanning roughly half of one page, and a capital costs section of two paragraphs. The capital costs section actually ends midsentence (exact quote) (Gaine and Duffy 2010):

*"The total of these add up to give the capital costs associated with the BTES system. Industry quotations, rates and estimates were obtained and were applied to each system analysed. Piping from the borehole headers to the energy centre have been accounted for based on an average pipe length of 75 meters. Labor rates used are based on industry quotations and"*

It is not clear whether this information can or should be relied upon to accurately cost the main underpinning technology of the entire future US energy system.

Solar district heating (SDH) with UTES on large scales and at high rates of deployment is rare outside of Denmark. Countries that have seen significant usage of SDH, most notably Denmark, are outliers, and can do so specifically because of the high penetration of legacy district heating systems that pre-date the solar components of the system. Capital costs for SDH systems (including the majority, which do not have UTES) ranged from around 400-800 \$/m$^2$ of installed collector area in Europe, energy costs ranged from 5 cents / kWh in Denmark to 11 cents / kWh in Austria (Dalenbäck och Werner, Market for Solar District Heating (pg. 16) 2012) (Dalenbäck 2010). The cost of the Drake Landing system in Canada (the example UTES is modelled by in Jacobson et al.) is far higher. Capital costs for Drake Landing were over 1145 \$/m$^2$ of collector area (far higher than the most expensive SDH systems in Europe), due to the need to install brand new storage and distribution infrastructure (Sibbitt, The performance of a high solar fraction seasonal storage district heating system – five years of operation. 2012) (Sibbitt 2015). The capital costs for Drake Landing suggest a UTES installation cost of at least $1.8 trillion for the Jacobson et al. system, nearly four times the mean-estimate used by Jacobson et al.

The argument that hot water storage could substitute for UTES is nonsensical. Hot water storage is not a cost-effective seasonal energy storage strategy, as a hot water tank does not have the same heat flow properties as soil or concrete. In addition, moving heating sectors toward heat pumps will further increase electricity loads which would need to be included in the modelling.

**#8 Jacobson et. al claim**
[*1*] further criticizes [*2*]'s hydrogen scale-up, but this is easier than [*1*]'s proposed nuclear or CCS scale-up. [*1*] also questions whether aviation can adopt hydrogen, but a 1500-km- range, 4-seat hydrogen fuel cell plane already exists, several companies are now designing electric-only planes for up to 1500 km and [*21*] proposes aircraft conversion only by 2035- 2040.

**#8 Response**
This again is diversion from the matter at hand, which are the errors and implausible assumptions in the work of *Jacobson et. al*. The total worldwide production of hydrogen from electrolysis is approx. 2.6m tons/year, corresponding to an average electrolysis power consumption of ~16 MW (International Energy Agency 2012). The US electrolysis scale-up envisioned by Jacobson et al. is thus at least a factor *100,000x* increase over total world electrolysis capacity today.

In contrast, both Sweden and France decarbonized their electricity grids in less than two decades by expanding nuclear power. As has been shown (Qvist and Brook 2015), continued nuclear power plant construction at the relative (GDP-normalized) rates that France or Sweden have already achieved, could theoretically decarbonize global electricity production in three decades. While carbon capture and storage facilites of commercial scale have been in operation for nearly 50 years, CCS indeed remains to be implemented at commercial scale at power plants.

**#9 Jacobson et. al claim**
[*1*] questions whether industrial demand is flexible, yet the National Research Council review it cites ("Real Prospects for Energy Efficiency in The U.S". P. 251) states, "Demand response can be a lucrative enterprise for industrial customers."

**#9 Response**
It remains to be explained how it could be "lucrative" for an "industrial customer" to suffer frequent multi-hour blackouts at their production facilities without being paid any compensation. One of the many criticisms of the Jacobson et al. treatment of "flexible load" is that it has no associated costs. Indeed, the very statement by NRC that demand response can be lucrative suggests that grid operators need to offer substantial returns to industrial customers to induce them to shed load. That makes industrial load shedding less economically feasible, not more. Demand response in the sense that one can tailor electricity consumption to lower the average electricity costs, which indeed could be lucrative, is a voluntary exercise on the part of the "industrial customer" and an entirely different phenomena than what is imposed by the system in Jacobson et al.

**#10 Jacobson et. al claim**
[*1*] criticizes [*2*]'s use of a 1.5%-4.5% discount rate even though that figure is a well- referenced social discount rate for a social cost analysis of an intergenerational project [*21*, Supp. Info. P. 44].

**#10 Response**
Ref [21] is a self-reference to another Jacobson et al. publication, adding nothing to defend these numbers. An earlier version of the Jacobson et al. response included the statement "*The only relevant studies are those that are recent and among those, Lazard (10.0) is the most detailed and relied upon by the energy industry, and capital costs are consistent with that study and other contemporary studies"*. The 3.0% (span of 1.5-4%) discount rate used by Jacobson et al. is less than half of the 8.0% used by the source that Jacobson et al. previously cited as support for their value (Lazard 2016), which may explain why it is no longer referred to and has been replaced by a self-reference.

Using realistic discount rates instead of those used by Jacobson et al. would alone *double* the estimated levelized cost of electricity.

**#11 Jacobson et. al claim**
[*1*] states misleadingly that [*2*]'s storage capacity is twice U.S. electricity capacity, failing to acknowledge [*2*] treats all energy, which is 5 times electricity, not just electricity, and [*2*] storage is only 2/5 all energy. Further, [*2*] storage is mostly heat.

**#12 Response**
The Clack et al. claim is correct: the total combined nameplate capacity (maximum theoretical output) of all electricity generators in the US today is estimated at 1.17 TW (U.S. Energy Information Administration, 2017), and is meant to give the reader a relatable sense of scale.

Furthermore, Jacobson et al. are wrong in their numbers even in this response. The fraction of electricity in US energy consumption is not 20% but rather 39% (US Energy Information Administration 2017), and this share is not likely to decrease with an electrification of additional sectors as proposed in the plans of Jacobson et al.

**#13 Jacobson et. al claim**
[*1*] claims the average installed wind density is 3 W/m$^2$, but fails to admit this includes land for future project expansion and double counts land where projects overlap. Also, real data from 12 European farms give 9.4 W/m$^2$ (P. Enevoldsen, pers. comm.)

**#13 Response**
Clack et al. accurately reports the value from the NREL study on the subject (Denholm 2009), as wrongly referenced by Jacobson et al. (2015). The NREL study conclusion is: "*Excluding the outliers, the reported data represents a capacity density range of 1.0 to 11.2 MW/km$^2$ and an overall average capacity density of 3.0 ± 1.7 MW/km$^2$.*"

A personal communciation reference added to a reply document 2 years after publication, the data and real source of which cannot be verified, does nothing to correct the erroneous reporting of data from the NREL report that was referenced in Jacobson et al. study. In addition, this new number is different from that used in the Jacobson et al. paper, so even if verified and shown to be applicable, this does not remove the error.

**#14 Jacobson et. al claim**
[1] claims [22] didn't rely on consensus data for CO2 lifecycle estimates although [22]'s nuclear estimate was 9-70 g-CO2/kWh, within IPCC's [17] range, 4-110 g-CO2/kWh. [1] claims falsely that [22] didn't include a planning-to-operation time for offshore wind, even though P. 156 states 2-5 yr.

**#14 Response**
As stated in the Clack et al. article: "*The life-cycle GHG emissions for nuclear power generation in [ref. 22] include the emissions of the background fossil-based power system during an assumed planning and construction period for up to* ***19 y per nuclear plant****. Added to these emissions,* ***the effects of a nuclear war, which is assumed to periodically reoccur on a 30-y cycle, are included in the analysis of emissions and mortality of civilian nuclear power.*****"** (Emphasis added). In the almost 60 years of civilian nuclear power (two of the assumed war cycles), there have been no nuclear exchanges. The existence of nuclear weapons does not depend on civil power production from uranium.

Whether the values cited happen to fall within the range of IPCC or not is in this case irrelevant, since nuclear and other potentially contributing sources to the system were excluded from consideration, based on what can only be described as a highly "selective assessment" of its merits.

No opportunity costs related to planning and construction time of offshore wind farms were included in the [22] study. The only operational US offshore wind farm (the 30 MW Block Island Wind Farm) had a planning, permitting & construction period well above the upper limit of Jacobson et al. values (7+ years). The largest proposed off-shore wind farm (468 MW Cape Wind) is now in its 16th year of planning and permitting – it is not yet operational.

**#15 Jacobson et. al claim**
Clack et al. criticize [22] for considering weapons proliferation and other nuclear risks, although IPCC [17] agrees (Executive Summary): “Barriers to and risks associated with an increasing use of nuclear energy include operational risks and the associated safety concerns, uranium mining risks, financial and regulatory risks, unresolved waste management issues, nuclear weapons proliferation concerns,...(robust evidence, high agreement).”

**#15 Response**
Jacobson’s publication (ref. [22]) suggests that any use of civilian nuclear power will lead to nuclear wars to periodically reoccur on a 30-year cycle. This is quite far away from the IPCC statement, listing one of the barriers to the expanded use of nuclear energy as concerns regarding potential nuclear weapons proliferation.

**#16 Jacobson et. al claim**
Clack et al. claim falsely that GATOR-GCMOM “has never been adequately evaluated,” despite it taking part in 11 published multi-model inter-comparisons and 20 published evaluations against wind, solar, and other data; [24]’s evaluation that GATOR-GCMOM is “the first fully-coupled online model in the history that accounts for all major feedbacks among major atmospheric processes based on first principles;” and hundreds of processes in it still not in any other model [25].

[*1*] contends LOADMATCH is not transparent even though LOADMATCH has been publicly available since [*2*]’s publication.

**#16 Response**
We refer the reader to Section S4.1: Inadequate Evaluation of Climate Model Results of Clack et al. for a detailed discussion on this.

We suggest that Jacobson et al. makes the full timeline of simulation output data available publicly, in addition to the four days of simulation that one can see in the paper itself.

**#17 Jacobson et. al claim**
[1] criticizes LOADMATCH for not treating power flows, and claims [2]’s transmission costs are “rough.” Yet [1] doesn’t show such costs are unreasonable or acknowledge [2]’s HVDC cost per km [21] are far more rigorous than [20]’s.

**#17 Response**
Remarkably, Jacobson et al. do not model the electricity grid at all in a paper claiming to solve the "grid reliability problem", making its title severely misleading. They neglect to show how much or where transmission would need to be built to get energy from sources to users. Comparisons to [21], which in stark constrast to Jacobson et al. does in fact model the electricity grid, are a diversion and not the subject of the Clack et al. critique or this response.

**#18 Jacobson et. al claim**
Finally, [1] falsely claims LOADMATCH has perfect foresight, thus is deterministic. However, LOADMATCH has zero foresight, knowing nothing about load or supply the next time step. It is prognostic, requiring trial and error, not an optimization model.

**#18 Response**
The actual claim by Clack et al. is: "It should be noted that LOADMATCH models generation from wind and solar a priori and then aggregates them together. It does not determine the capacity of generation endogenously. The model is essentially one-dimensional; all loads, generation and storage are considered in a single place though time. Thus, the sensitivity analysis performed ultimately relies only on changes in storage and demand response (and erroneous hydropower capacity) on a trial and error basis."

The LOADMATCH model also fails to account for a range of realistic power system operation constraints, including the need for various categories of operating reserves necessary to ensure demand and supply can remain balanced following errors in renewable energy forecasts, demand forecasts, or unanticipated failures of power plants or transmission lines. The model also does not account for typical constraints on thermal generators, including geothermal and concentrating solar thermal units (such as ramp rate constraints and minimum up and down times related to thermal stress on steam systems and minimum stable output levels for online units). Taking these factors into account is critical for appropriately evaluating the reliability of power systems with high shares of variable renewable resources (Palmintier och Webster 2016). As a deterministic model with perfect foresight, LOADMATCH does not perform a stochastic optimization that would endogenously account for uncertainty in renewable energy or load forecasts or power plant or transmission contingencies (Zheng, Wang och Liu 2015), nor does it deterministically model reserve requirements based on offline studies of forecast errors or plant/line failure probabilities as is best practice in modeling reliable power system operations (de Sisternes 2013). This can result in significant errors due to abstraction of relevant power system details and the failure to account for the full variability of renewable resources, demand, and contingencies. Claims that the model demonstrates the reliability of power systems with 100% WWS are therefore suspect.

**#19 Jacobson et. al claim**

In sum, [1]'s analysis is riddled with errors and has no impact on [2]'s conclusions.

**#19 Response**

The Jacobson et al. work has been show very clearly to contain a large number of fundamental errors, each on their own invalidating the results of the studies (many of which are not at all brought up by this response).

## References (specific to this document)

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