LETTER

The United States can keep the grid stable at low cost with 100% clean, renewable energy in all sectors despite inaccurate claims

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The premise and all error claims by Clack et al. (1) in PNAS, about Jacobson et al.'s (2) report, are demonstrably false. We reaffirm Jacobson et al.'s conclusions.

False Premise

Clack et al.'s (1) premise that deep decarbonization studies conclude that using nuclear, carbon capture and storage (CCS), and bioenergy reduces costs relative to "other pathways," such as Jacobson et al.'s (2) 100% pathway, is false.

First Clack et al. (1) imply that Jacobson et al.'s (2) report 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).

Second, the Intergovernmental Panel on Climate Change (IPCC) (17) contradicts Clack et al.'s (1) claim that including nuclear or CCS reduces costs (7.6.1.1): "…high shares of variable RE [renewable energy] power…may not be ideally complemented by nuclear, CCS,…" and (7.8.2) "Without support from governments, investments in new nuclear power plants are currently generally not economically attractive within liberalized markets,…" Similarly, Freed et al. (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 Cooper (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. (2), the IPCC, National Oceanic and Atmospheric Administration, National Renewable Energy Laboratory, and International Energy Agency have never performed or reviewed a cost analysis of grid stability under deep decarbonization. For example, MacDonald et al.'s (20) grid-stability analysis considered only electricity, which is only ~20% of total energy, thus far from deep decarbonization. Furthermore, deep-decarbonization studies cited by Clack et al. (1) have never analyzed grid stability. Jacobson et al. (2) obtained grid stability for 100% wind, water, and solar power across all energy sectors, and thus simulated complete energy decarbonization.

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

Fifth, Clack et al. (1) contend that Jacobson et al. (2) place "constraints" on technology options. In contrast, Jacobson et al. include many technologies and processes not in Clack et al.'s (1) models. For example, Jacobson et al. (2) include, but MacDonald et al. (20) exclude, concentrated solar power (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, and so forth. Model time steps in MacDonald et al. (20) are also 120-times longer than in Jacobson et al. (2).

False Error Claims

Clack et al. (1) claim wrongly that Jacobson et al. (2) assume a maximum hydropower output of 145.26 GW, even though table S2 in Jacobson et al. shows 87.48 GW. Clack et al. (1) then claim incorrectly that the 1,300 GW drawn in figure 4B of Jacobson et al. (2) is wrong because it exceeds 87.48 GW, not recognizing that 1,300 GW is instantaneous and 87.48 GW, a maximum possible annual average [table S2, footnote 4 in Jacobson et al. (2) and the available LOADMATCH code]. The value of 1,300 GW is correct, because turbines were

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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 Jacobson et al.'s (2) table S2, footnote 4, and LOADMATCH, and written to Clack on February 29, 2016.

Jacobson et al. (2) only neglect 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 wind, water, and solar power system and thus doesn't impact Jacobson et al.'s (2) conclusions. Increasing CSP's—instead of hydropower's—peak discharge rate also works.

In their figure 3, Clack et al. (1) then claim mistakenly that Jacobson et al.'s (2) annual hydropower energy output is 402 TWh/yr and too high, when it is actually 372 TWh/yr because they missed transmission and distribution losses. This is less than half the possible United States hydropower output today and well within reason.

Clack et al. (1) next claim wrongly that in Jacobson et al.'s (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. Clack et al. (1) compound this misrepresentation by claiming flexible loads in Jacobson et al.'s (2) time figures are twice "maximum possible" loads, even though Jacobson et al. clearly state that the annual loads are distributed in time.

Unsubstantiated Claims About Assumptions. Clack et al. (1) assert that underground thermal energy storage (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 phase change materials in CSP storage.

Clack et al. (1) further criticize Jacobson et al.'s (2) hydrogen scale-up, but this is easier than Clack et al.'s (1) proposed nuclear or CCS scale-up. Clack et al. (1) also question whether aviation can adopt hydrogen, but a 1,500-km range, four-seat hydrogen fuel cell plane already exists, several companies are now designing electric-only planes for up to 1,500 km, and Jacobson et al. (21) propose aircraft conversion only by 2035–2040.

Clack et al. (1) question whether industrial demand is flexible, yet the National Academy of Sciences (24) review they cite states, "Demand response can be a lucrative enterprise for industrial customers."

Clack et al. (1) criticize Jacobson et al.'s (2) 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).

Clack et al. (1) state misleadingly that Jacobson et al.'s (2) storage capacity is twice United States electricity capacity, failing to acknowledge that Jacobson et al.'s (2) report treats all energy, which is five times electricity, not just electricity, and in Jacobson et al. (2), storage is only two-fifth of all energy. Furthermore, in Jacobson et al.'s (2) report, storage is mostly heat.

Clack et al. (1) claim the average installed wind density is 3 W/m^2, but fail to admit this includes land for future project expansion and double counts land where projects overlap. Furthermore, real data from 12 European and Australian farms give 9.4 W/m^2.

Clack et al. (1) claim that Jacobson (22) didn't rely on consensus data for CO_2 lifecycle estimates, although Jacobson's nuclear estimate was 9–70 g-CO_2/kWh, within the IPCC's (17) range, 4–110 g-CO_2/kWh.

Clack et al. (1) claim falsely that Jacobson (22) didn't include a planning-to-operation time for offshore wind, even though ref. 22 states 2–5 y.

Clack et al. (1) criticize Jacobson (22) for considering weapons proliferation and other nuclear risks, although the 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)."

False Model Claims. Clack et al. (1) claim falsely that the gas, aerosol, transport, radiation-general circulation mesoscale, and ocean model (GATOR-GCMOM) "has never been adequately evaluated," despite it taking part in 11 published multimodel intercomparisons and 20 published evaluations against wind, solar, and other data; despite Zhang's (25) 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 despite hundreds of processes in it still not in any other model (26).

Clack et al. (1) contend that LOADMATCH is not transparent, even though LOADMATCH has been publicly available since Jacobson et al.'s (2) publication.

Clack et al. (1) criticize LOADMATCH for not treating power flows, and claim that Jacobson et al.'s (2) transmission costs are "rough." However Clack et al. (1) do not show such costs are unreasonable or acknowledge Jacobson et al.'s (2) high-voltage direct current cost per kilometer (21) are far more rigorous than MacDonald et al.'s (20).

Finally, Clack et al. (1) falsely claim that 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.

In sum, Clack et al.'s (1) analysis is riddled with errors and has no impact on Jacobson et al.'s (2) conclusions.

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