Response to "J.P."'s column "New numbers, same conclusion"
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Dr. Charles R. Frank, Jr. published in May 2014 a Brookings Institution Working Paper, "The Net Benefits of Low and No-Carbon Electricity Technologies," which *The Economist* featured online in late July. I posted on 1 August a documented 12-page technical critique, noted in an *Economist* letter and *Forbes* and GTM blogs. It found that updating nine of Dr. Frank's outdated numerical assumptions would reverse his conclusions, without correcting his methodological errors. On 22 August, "J.P.", apparently acting as an anonymous surrogate for Dr. Frank, posted a short response in the same *Economist* online section that had posted the feature a month earlier.

J.P.'s response—wrongly, as I'll show—denies that *two* of those nine outdated numbers, solar and windpower capital costs, are unrealistic as my critique showed. But I didn't assert that correcting only those two numbers would reverse Dr. Frank's conclusions; rather, they'd do so when seven *other* wrong numbers were also fixed:

- solar and windpower capacity factors (which J.P. says make little difference, apparently *by themselves*, but doesn't quantify), including spurious adverse "adjustments" that J.P. ignores;
- solar power's *onpeak* capacity factor in particular;
- gas-combined-cycle plants' capacity factor and methane leakage; and
- nuclear power's operating cost and new-build construction time.

Summarizing an uncited and apparently unposted recalculation by Dr. Frank, J.P. says that "nothing in these new calculations seems to provide a strong reason for changing [his]…main conclusion…." That's because, as nearly as one can tell from J.P.'s sketchy description, the "new calculations" are very like Dr. Frank's first one. It seems he's slightly updated his solar and windpower capital costs to others still far above modern competitive practice; changed modestly, if at all, his very low solar and wind capacity factors; and most importantly, changed none of his *other* 5–7 key variables. If that's actually what he did, then of course it would yield a similar result —but it would neither rebut my critique's results-reversing claim nor address its detailed refutation of his analysis. Which "actual, rather than estimated, numbers" did Dr. Frank use? We can't tell, so the results J.P. mentions are meaningless.

On the narrow issue J.P. addresses—solar and windpower capital costs—my analysis was correct and J.P.'s reply is not, as I'll next show on pp. 2–6.

Here's what page 3 of my critique said about solar and windpower capital costs:

Based on an EIA forecast published in 2013, performed in 2012, using still older data—well-known among technology experts to be a poor basis for assessing plummeting renewable costs—Dr. Frank assumes overnight capital costs of $2,213/kW for a new wind plant and $3,873/kW for a new PV plant. Adding cost of capital during construction yields installed costs (which I assume are actually installed *prices*) of $2,351/kW for wind and $4,115 for PV. Yet these figures are badly outdated.

For U.S. windpower, the actual capacity-weighted average 2012-$ installed price in 2012 was $1,940/kW and falling, just $1,760/kW in the windy interior region, and for the lowest-cost projects, only about $1,400/kW—two-fifths below Dr. Frank's $2,351/kW assumption. Dramatic further drops were reported in 2013–14—apparently the average fell by ~$300/kW in 2013, with the official data due for release shortly in Lawrence Berkeley National Laboratory's annual digest. Dr. Frank further disadvantages windpower by assuming each project lasts only 20 years rather than the modern industry-standard 25, thus increasing its annual capital charge by one-fourth. Some operators even say their expert monitoring and maintenance should enable their turbines to keep running far beyond 25 years.

For U.S. utility-scale PV projects added in 2012, the actual average installed price was about $3,300/kW for crystalline or $3,200/kW for thin-film fixed-tilt systems. Nearly all projects >20 MW cost well below Dr. Frank's $4,115/kW assumption (that is, his supposed average was actually near the upper bound), many were priced below $3,000/kW, and the cheapest projects had installed prices around $2,300/kW, or about $2,500/kW at utility scale. The latest official data for the first half of 2013 suggest even faster drops in system prices than in 2012, largely because—unexpectedly for Dr. Frank (cf. p. 12 of his paper)—balance-of-system (non-module) prices are falling rapidly too as U.S. installers catch up with the streamlined techniques that enabled German residential PV installers to achieve half the U.S. installed price even though they all buy the same equipment. That's why, according to early-2014 public statements by CEO Lyndon Rive, SolarCity's installed residential system costs fell 30% in 2013 even though his module prices rose 3%. Consistent with these trends, Lazard's 2013 analysis uses a utility-scale PV capital cost of $2,000/kW tracking or $1,750/kW fixed-tilt—the latter falling to $1,500/kW in 2015.

Let's now unpack eleven errors in J.P.'s contentions about these capital costs.

Solar capital costs

J.P.'s attempt to validate Dr. Frank's solar cost assumptions combines five errors:

1. He uses EIA's "overnight" capital cost of $3,873/kW rather than Dr. Frank's Table 7's $4,115/kW, which includes the cost of financing during 1.5 years of construction—a cost included in my citations' installed system prices. (I assume that as LBL-6408e pp. 3–4 calls for, Dr. Frank's kW denominator is measured in alternating not direct current, though neither EIA nor Dr. Frank says so. These two metrics differ by up to a third.) I'll return to cost shortly.
2. Dr. Frank's economic calculations converted capital costs into charges per kWh by applying a capacity factor—the percentage of full-time, full-power output that a plant actually sends out during a year. His Table 6A assumed an unrealistically low annual-average utility-scale photovoltaic-system capacity factor of 15.5%. My critique documented an actual 2008–13 average of 20.4% for all U.S. PV projects, many in relatively cloudy places. That average in turn spans a twofold range. The *cumulative* average of recently installed utility-scale projects (thus diluting learning effects by early experience and

startup phase) covers a range of ~15–27% for fixed-tilt and ~20–33% for tracking arrays. The higher values reflect the sunnier locations where developers of utility-scale projects naturally tend to build. Thus Dr. Frank assumes that PV projects are roughly one-third to one-half less productive than they actually are, making them correspondingly costlier per kWh. It's unclear if Dr. Frank's recalculation corrects this; J.P. mentions a modest effect, but not whether capital cost *and* capacity factors were corrected nor by how much.

3. J.P. doesn't mention that Dr. Frank actually assumed (Table 6A) not a 15.5% but implicitly a *12.4%* solar annual capacity factor (calculated from 15.9% onpeak and 12.0% offpeak) after making a spurious "adjustment" based on an invalid methodology meant to reflect inter-year reliability. That 12.4% is less than half what high-quality modern plants achieve, and only three-eighths of the best performance observed through 2013. Dr. Frank's rerun of his model presumably retained this adverse "adjustment."
4. J. P. further ignores Dr. Frank's important assumption that solar production in the 800 peak-load hours of the year (mostly on hot summer afternoons) has only a 20.0% capacity factor, which step #3 then "adjusted" back to 15.9%. My critique documented on p. 5 that even in the cloudy northeast, utilities found that PVs' contribution to system peak loads in New England in summer 2012 averaged 40% of their rated capacity; in New York State (with southwest orientation), 70–90% despite annual-average capacity factors in the upper teens. Thus solar power's onpeak capacity factor, a key to Dr. Frank's economic conclusions, was apparently understated by severalfold. My critique's results-reversing sensitivity test assumed an onpeak solar capacity factor of 45%. That's 2.8 times Dr. Frank's assumption of 15.9%, but based on the Northeastern utilities' measurements, it appears conservative as a national average, especially since most utility-scale solar PV is in the sunnier West. (Interestingly, Dr. Frank assumed an "adjusted" peak-to-offpeak PV capacity-factor ratio of 1.3, while EIA reported a 2012 July/January capacity-factor ratio—not at all the same thing, but indicative—of 15.9.)
5. Finally, J.P. misrepresents Dr. Frank's $3,873/kW base capital cost—understated in #1 and undercosted in #2–4—as "Dr Lovins's preferred figure," ignoring the lower figures right after it. As explained next, it's neither preferred nor representative. I used it in the nine-variable sensitivity test that reversed Dr. Frank's conclusions, not because it realistically reflects modern industry achievements, but because it's conservatively high.

J.P. says my DOE "figures for the capital cost of solar" (DOE's Lawrence Berkeley National Laboratory's LBNL-6408e, Sep 2013, linked to "cost" in paragraph 3, line 3, of the quotation on p. 2 above) show that "the weighted average capital cost of 113 utility-scale solar projects completed in 2012 was $3,900 per kilowatt", matching Dr. Frank's EIA figure of $3,873 ($3.873/W). Yes, LBNL did find a 2012 average of $3.9 per AC watt ($W_{AC}$). The same lab two months earlier, in LBNL-6350, p. 4, also found the $3.2–3.3/$W_{AC}$ system prices in the previous two lines of my critique, and on p. 39, noted that utility-scale systems installed through 2012 are weighted by "small or otherwise atypical" projects with costs "likely to be higher than that of prototypical large-scale utility PV projects." J.P. ignores all that. What does it mean?

Dr. Frank's PV price matches the 2012 average, but that doesn't validate it as an accurate representation of what a capable developer would pay in 2012, let alone later (PV electricity prices halved during 2011–13). That's because this single average 2012 price *conceals a nearly threefold range* of \$2.8–8.0/W_{AC}, and that range has a far from normal distribution. Instead, it averages many projects clustered around the lowest costs (as I said) with fewer outliers scattered more widely near the high end. Averaging them all together is as fallacious as calculating typical U.S. income by dividing national income by the number of households. Because there are far more low- and middle-income households (the 98–99%) than very rich ones (the 1–2%), 2004 U.S. income was 40% higher in mean than in median—the value that half the households are above and half below. The distribution of PV-system prices also shows a strong asymmetry. (By using capacity-weighted average prices, the LBNL reports I relied upon help to correct averaging distortions caused by unit size, but not by other factors they describe.) That's why my critique's explanation of installed prices was about range, lower values, and rapid continuing drops—not a single, mythical, static average. Dr. Frank's use of average costs as representative is like the proverbial statistician who drowned in a river averaging six inches deep.

LBNL-6408e notes the PV price spread at the bottom of p. 4, graphs its skewed distribution in Fig. 1, and elaborates its causes on pp. 5–7. In 2012, projects in the cloudy Northeast averaged about 19% costlier per watt than those in the sunny West. Nearly all the \$5–8/W_{AC} projects that boosted Dr. Frank's average cost were in costly regions and sites, and/or in sizes now scarcely considered utility-scale (<15 MW and usually just 2–5 MW_{AC}—LBNL-6408e, p. 6). LBNL-6350e, at p. 41, discusses the resulting price distortions, exacerbated by the use of mean not median costs. The data I cited clearly show that Dr. Frank's adopted average price is ~30% above many 2012 projects and ~56% higher (3.9/2.5) than the best—inflated by many small projects, those in especially difficult sites, and those in the costliest regions.

My analysis quoted above correctly describes these important distinctions and properly differentiates anomalous designs, sizes, sites, and other cost-exaggerating factors from the many market-dominating low-cost projects representative of modern competitive practice. Dr. Frank' single average price not only conceals these factors, but also ignores interactions between capital cost and performance. For example, modern tracking arrays cost little more (~\$0.2) per watt than fixed-tilt arrays, but produce far more output (LBNL-6408e, pp. 12–13).

J.P. also ignores my reference to accelerating price drops in the first half of 2013. LBNL is about to publish the 2013 data, which should show even wider price scatter but lower best prices. For example, Public Service Company of New Mexico is now building a \$2.03/W_{AC} (2014 \$) PV plant and requesting one at ~\$1.98/W_{AC}. That matches the \$2/W_{AC} Lazard tracking-system cost I cited—but *it's also about half Dr.*

Frank's cost estimate, and half the $4.0/W_{AC}$ PNM paid in 2011, thanks to the plummeting prices that I described and J.P. ignores. Dr. Frank's costs are badly outdated.

Those rapid price drops have made the contract price for new PV plants' electricity (LBNL-6408e, pp. 19–27) fall by more than half in 2006–11, then more than halve again in 2011–13. Dr. Frank doesn't appear to understand (see p. 12 of his Working Paper) that a major cause of this rapid price drop in the past few years, accelerating in 2013, has been balance-of-system cost reductions, discussed in the last half of paragraph 3 quoted above in a residential context but also important in larger projects. As LBNL-6350e notes at p. 39, "It is worth repeating again that focusing on upfront installed price trends ignores performance-related differences and other factors influencing the levelized cost of electricity…, which is ultimately the more meaningful metric…" —and the metric that drives Dr. Frank's conclusions.

Windpower capital costs

J.P.'s errors about windpower are analogous to those just shown for photovoltaics:

1. J.P. uses EIA's $2,214/kW "overnight" capital cost rather than Dr. Frank's financed installed cost of $2,351/kW.
2. J.P. adopts Dr. Frank's assumed 25.5% annual-average windpower capacity factor. My critique at p. 2 documented an actual 2008–13 U.S. average of 31.0%, or in 2006–12, 32.1%, both net of several percentage points' reductions due to forced or economic curtailment. Yet averages again mislead. In the midwestern windbelt where the most capacity and projects get built, the average generation-weighted capacity factor in 2013 (a nearly normal wind year, and net of curtailment) was 38%, with a range of 18–53%. Quite a few recent windpower contracts, as my citation from Mr. Goggin shows, are based on ≥50% capacity factors. Thus Dr. Frank substantially understated windpower's practical productivity, overstating the price of its electricity. As a simple check, his assumed prices and capacity factors cannot explain actually existing windpower market prices—by fourfold. Without subsidies (said Bloomberg New Energy Finance in April 2014) a realistic U.S. onshore-windpower levelized price is $37/MWh (consistent with 2013 levelized prices down to ~$18/MWh and *averaging* $25/MWh, plus a ~$18 levelized federal tax credit for construction begun by 2013). That's *less than one-fourth* of Dr. Frank's implicit $157/MWh. His equally implausible apparent implicit levelized prices, likewise using his "adjusted" capacity factors, were $327/ MWh for PV power, nearly five times its unsubsidized 2012–13 sunbelt market price, and $90 for new-build nuclear, about half the subsidized U.K. market price. Had his paper included such levelized-cost-of-energy figures, which we've back-calculated from his data, more readers may have smelled a rat.
3. J.P. ignores Dr. Frank's "adjustment" of his already-too-low annual-average wind capacity factor down to a calculated 20.4%—*roughly half* the actual midwest-region 2012 average capacity factor without deducting curtailment.

4. J.P. further overlooks Dr. Frank's "estimate" that his 25.5% average windpower capacity factor is 26.1% offpeak and 20.0% onpeak—or, after "adjustment" (#3), 20.4% offpeak and 15.7% onpeak. My critique didn't address this assumption because I could find no data fitting Dr. Frank's onpeak definition (800 unspecified hours, which will differ for each system and each year), but it's unclear how he "estimated" this important number. It couldn't have come from the ten years' EIA data mentioned in his note 4, which, if validly usable (see p. 7 below), would produce *annual* capacity factors but couldn't distinguish offpeak from onpeak periods. He may simply have assumed that one specific California site whose topography favors night over daytime winds is typical nationwide. I know of no reason to suppose this is true. On the contrary, some regions' windpower correlates rather well with peak loads, often because their wind is driven by the sun's daily heating of the landmass. This is often even more true offshore, *e.g.* in the Northeast.
5. Just as Dr. Frank's average PV installed price matches a national average but doesn't represent good modern practice by solar-industry leaders, the same approach in windfarm prices is misleading for the same reason. The 2012 data show that, as my critique summarized (also quoted on p. 2 above),

For U.S. windpower, the actual capacity-weighted average 2012-$ installed price in 2012 was $1,940/kW and falling, just $1,760/kW in the windy interior region, and for the lowest-cost projects, only about $1,400/kW—two-fifths below Dr. Frank's $2,351/kW assumption. Dramatic further drops were reported in 2013–14—apparently the average fell by ~$300/kW in 2013, with the official data due for release shortly in Lawrence Berkeley National Laboratory's annual digest.

J.P. doesn't and can't challenge those documented data; he simply doesn't mention any but the national average. He also ignores the cited ~16% cost drop in 2013 reported from preliminary DOE data by the American Wind Energy Association; it was actually 17%, to a 2013 average of $1,608/kW (2012 $), or 32% below Dr. Frank's figure. And as with PVs, the range of windfarm prices—3.3-fold in 2012, ~3.1-fold in 2013—reflects important regional differences. Most of the price scatter and costlier projects are in the Northeast and West (especially California) regions. Prices there are up to 3+ times times those of the best Interior (chiefly High Plains) projects, whose capacity-weighted average price in 2012–2013 was 9% below the national average, while the Interior's best projects cost another ~18% less. Finally, J.P. overlooks a sixth difference documented in my critique:

6. Dr. Frank assumed wind turbines last only 20 years rather than the modern industry standard of ≥25, increasing their capital burden by one-fourth.

The origin of Dr. Frank's capacity factors

Finally, J.P. packs an impressive six misrepresentations into his seventh paragraph:

- He ironically agrees "Dr. Frank's numbers are rather old. They come from the EIA's Annual Power Report, published in December 2013." The previous two paragraphs are all about capital costs, so one might infer that EIA's *Electric*

Power Annual (its correct title) is where Dr. Frank got them. But actually, only his capacity factors, not his "numbers" generally, came from that report.

- J.P. continues: "This report did not give figures for capacity factors for wind and solar power....It merely estimated them." Yet EIA's online December 2013 *Electric Power Annual* seems to contain no renewable capacity-factor estimates. (As pp. 1–2 of my critique explain, the Table of Contents lists EIA's capacity-factor *data* in a new Table 4.8.B scheduled for posting in January 2014 but still not posted, so Dr. Frank couldn't have used those data either.)
- However, the volume does report annual data on wind and PV net generation (Table 3.1.B) and year-end capacity (Table 1.2 or 4.2.B), from which Dr. Frank's Table 2A says he calculated his capacity factors—using no estimates. As my critique's pp. 1–2 explain, quoting the volume's methodological Appendix—but J.P. doesn't mention—*that calculation is wrong*: PV and windpower are growing rapidly, so much of the capacity installed as of year-end wasn't there all year. That's mainly why those *Electric Power Annual* data yield a 12.4% "PV" capacity factor for 2012. That's about half the actual utility-scale cumulative average computed by LBNL, which correctly combines all the project-specific capacity factors based on actual dates of entering service. It's even two-fifths below the lower 20.3% average posted by EIA, which reportedly tries to do the same thing. (The *Electric Power Annual* solar generation and capacity data Dr. Frank used are also unsuitable because the PV data are not just utility-scale, the capacity data combine PV with solar-thermal-electric, EIA relies on federal reports that few small producers file, and any production for onsite use, displacing grid supply, is often not counted. It's unclear how far other EIA data sets avoid these issues. Of course, national-average historic capacity factors, like prices, also don't reflect performance in the more favorable areas where new capacity tends to be built.)
- J.P. continues: "Dr. Frank used these estimates [that J.P. claims are in the December 2013 *Electric Power Annual* but don't seem to be]. Since then the EIA has published actual capacity factors for wind and solar...." This falsely implies that the actual EIA capacity-factor data were not available when Dr. Frank did his analysis. Actually, EIA's *Electric Power Monthly* has posted them monthly (back to 1990) since 1996 for both windpower and PVs; my critique merely cited the latest posting of Table 6.7.B. Dr. Frank therefore could and should have used the actual data, which reportedly handle the timing issue and separate PVs (albeit of all reporting sizes) from solar-thermal-electric.
- J.P. concludes: "...and these do indeed, [*sic*] make wind and solar look better, as Dr Lovins claims. But not all that much." How much? Based on what inputs? Alone or together with other sensitivity tests? We can only guess. The fundamental issue with Dr. Frank's paper, apart from its methodological flaws, is that its results flowed from nine wrong numbers none of which got proper sensitivity-testing—a basic requirement for credible models—so its results were flipped by modest updates to all nine together (a "multivariate sensitivity test"). Dr. Frank's paper reports sensitivity tests only for carbon price, gas price, and a package of two solar and wind changes—one-third-higher capacity factors plus lower capital costs. (Those are cited to an

unlisted May 2013 reference—perhaps this slideshow, which doesn't specify those costs and is set in very different Italian and German conditions.) Rarely have such sweeping conclusions been declared "very robust" (Working Paper, p. 17) without sensitivity-testing most of the "key assumptions" that propel its logic—not collectively, seldom individually, and never credibly.

- J.P.'s claim that "Dr Frank's numbers…come from the EIA's Annual Power Report, published in December 2013" might lead unwary readers to suppose his data were fresh. Actually, as Dr. Frank's Working Paper specifies in Table 2A, he calculated capacity factors (invalidly, as noted above) from the December 2013 volume's annual average data *for 2002–2012*. My critique notes that U.S. wind and solar generation respectively rose by about 12- and 100-fold over that period, with huge gains in efficiency and productivity. Yet Dr. Frank weighted each year equally—a method akin to estimating the typical parameters of a modern car by averaging its characteristics in 1910, 1920, and so on to 2010. Renewables have evolved so quickly that most of the data behind his wind and solar capacity factors are about as outdated as antique cars.

Summary

J.P. claims that of Dr. Frank's *nine* incorrect key assumptions whose updating I showed would reverse his conclusions, *two* numbers are sound because they roughly match 2012 average capital costs given in sources I cited. But those averages heavily dilute widespread competitive practice with anomalously costly projects, and don't represent what capable developers routinely achieve: solar and windpower costing, without subsidy, respectively four and five times less than Dr. Frank claims. J.P. also ignores five if not all seven of Dr. Frank's *other* seven key assumptions. He summarizes the results, but not the inputs, of an apparently unavailable new calculation by Dr. Frank that, from J.P.'s description, changes in some unstated degree two or perhaps four of the nine numbers but ignores most other issues. And J.P. introduces at least 17 new errors and misrepresentations that obscure the original discussion. What readers may well find "baffling" is not (as J.P. asserts) the "conflicting numbers, which also change all the time," but J.P.'s, and so far Dr. Frank's, failure to address the fatal analytic defects—in source, methodology, and interpretation—that my 1 August critique systematically set out.

I therefore hope Dr. Frank will publish not only his new calculations but also a clear point-by-point reply to that critique, so readers can judge who is right. J.P.'s sketchy comment does not begin to fulfill that scholarly duty, but does misleadingly imply that Dr. Frank's broad conclusions have broadly survived my critique and that less unrealistic assumptions (about capital cost and perhaps capacity factor) do "not alter the ranking," leaving a "fairly large" gap between modern renewables and their supposedly cheaper competitors. On the contrary, J.P.'s essay must make careful readers wonder whether my inconvenient critique remains unrefuted, and very largely unaddressed, not because it is baffling but because it is irrefutable.