Comments on Skrable et al. (2022)

Dear Editors:

Health Physics ventured outside its area of expertise when it accepted the paper by Ken Skrable, George Chabot, and Clayton French on atmospheric ^{14}C for the February 2022 issue of Health Physics (Skrable et al. 2022). Broadening the scope of a journal to help its readers understand related topics is a laudable goal, and radiocarbon studies of the carbon cycle should certainly interest readers of this journal. But *Health Physics* editors dropped the ball here. They should have included a qualified reviewer from the radiocarbon or atmospheric science community. They clearly did not because any reviewer with previous knowledge of atmospheric ^{14}C would have found the fatal errors cited below. An extensive peer-reviewed literature exists on studying the carbon cycle with ^{14}C, but next to none of it is cited by the authors. The average *Health Physics* reader will therefore not have the tools to critically evaluate the conclusions of the paper. We will let readers of this letter decide if Skrable et al.'s conclusions should be described as "controversial" or just plain wrong. Unfortunately, unless withdrawn this paper now becomes part of the peer-reviewed literature. That is not something *Health Physics* should be proud of.

One would expect an article promising new insights into a critical current issue would at least start with the best data available. But this paper builds its case on inaccurate data, some of which it simply invents. Skrable et al.'s "educated guess" of 16.33 dpm $(gC)^{-1}$ for the specific activity of ^{14}C in the 1750 atmosphere is not bad for a guess, but it is 20% too high. It corresponds to a $\Delta^{14}C$ of 200 ‰ (parts per thousand, see below). Except during the era of atmospheric nuclear testing, such a value has not been seen for over 10,000 years (Cheng et al. 2018). A 20% specific activity error in ^{14}C converts to a dating error of over 1,800 years. Carbon-14 (^{14}C) dating is much better than that because the atmosphere's specific activity during historical times is accurately known. It is meticulously calibrated from materials of known age such as tree rings. There was no need to guess its starting point in 1750 or its trajectory since. There was no need for the authors to limit themselves to only the recent data from Niwot Ridge. They could have looked up the specific activity, for example in Graven et al. (2017), and they would have found that it looked like the "data" curve in Fig. 1, rather than like their guessed "model" curve, also shown in Fig. 1.

The literature expresses specific activity not in dpm $(gC)^{-1}$, but with the dimensionless variable "$\Delta^{14}C$." $\Delta^{14}C$ is the fractional deviation of the specific activity S from a standard in parts per thousand:

$$\Delta^{14}C = 1000\left[\frac{S_{sample}}{S_{standard}} - 1\right].$$

The appropriate $S_{standard}$ to convert published $\Delta^{14}C$ to Skrable et al.'s preferred units is 13.56 dpm $(gC)^{-1}$. This "modern pre-industrial" standard, chosen to make $\Delta^{14}C$ close to 0 around 1750, is shown as a dashed line in Fig. 1. The curve called "data" in Fig. 1 was calculated from tabulated $\Delta^{14}C$ data (taking an average of northern hemisphere, southern hemisphere, and tropical data from Graven et al. 2017) and using this formula rearranged: $S(t) = 13.56*[1+ .001*\Delta^{14}C(t)]$. Skrable et al. use a slightly different and inappropriate formula to convert from $D^{14}C$ to S(t), a formula which includes fractionation and decay corrections. These are necessary when computing $\Delta^{14}C$ from raw data, for example for finding the specific activity of the atmosphere in 1750 from measurements in 2022 on tree ring material from 1750. But once incorporated into the tabulated atmospheric $\Delta^{14}C$ values, these corrections should not be used again when simply converting between units. This conversion error accounts for the offset between "data" and "model" in the early 21st century apparent in Fig. 1. The Niwot Ridge measurements of $\Delta^{14}C$ overlap with other data sets in their limited range.

The slight decline of the specific activity between 1850 and 1950 apparent in Fig. 1 is the "Suess effect" (Suess 1955), the dilution of atmospheric ^{14}C by carbon from fossil fuels emissions devoid of ^{14}C. This is the signal that Skrable et al. try to extract and interpret with their analysis of (wrongly guessed) data from 1750 through 2018, despite the complication of the dramatic "bomb pulse" from atmospheric nuclear weapons testing in the late 1950s and early 1960s.

Aware that their 1750 value of ^{14}C specific activity is high, the authors argue that a lower 1750 specific activity would lead to a still lower estimate of the anthropogenic contribution to CO_2 increases because it would imply a still smaller Suess effect. The trouble with that argument is that the true 1750 value [13. 56 dpm $(gC)^{-1}$)] is *less than* the early 21st century values Skrable et al. show–around 14.5 dpm $(gC)^{-1}$. Because the authors are making inferences based

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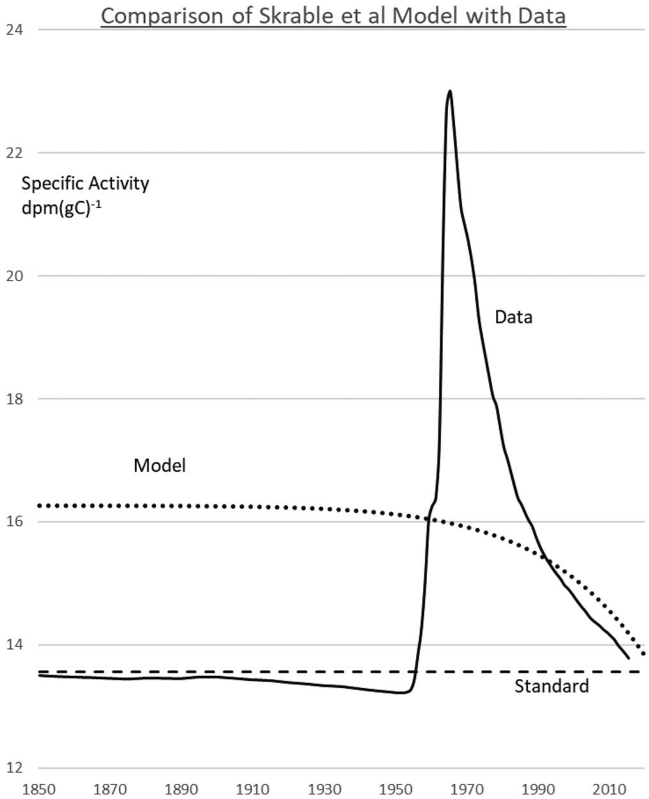

Fig. 1. Comparison of Skrable et al. (2022) model with data.

on how much they think the specific activity *went down* between 1750 and 2018, and in fact the net specific activity *went up* after 1950, clearly they missed something. What they missed is apparent from Fig. 1. Notwithstanding quotes the authors have taken from the internet suggesting that the bomb pulse no longer mattered after 2005, the bomb pulse still matters in 2022. In an earlier version of this paper available on the internet (Skrable et al. 2020), no mention whatsoever is made of the bomb carbon. Although this paper does contain a short discussion, that is only for the purpose of justifying the authors' flawed decision to ignore the bomb carbon. Their present model is unchanged from one they built in apparent ignorance of it.

It is data on the *specific activity* of atmospheric ^{14}C that is displayed in Fig. 1. Being in units of *decay rate[^{14}C] per gram of carbon* the specific activity measures the $^{14}C/C_{total}$ abundance ratio. As shown in Fig. 1, this ratio has largely recovered to its 1950 value since the nuclear testing ended. That observation is probably the basis for the unfortunate and misleading internet comments quoted (e.g., "By the 1980s, most of the 'bomb' ^{14}C had been absorbed in the oceans and land biota"). The curve in Fig. 2 tells a different story. Shown is the total atmospheric content of ^{14}C expressed as the total atmospheric activity in Becquerels (Bq). In Skrable's notation this is the product $S(t) \times C(t)$ (specific activity $\times$ atmospheric CO_2 abundance), with an appropriate normalization factor. To produce Fig. 2 the same specific activity data as in Fig. 1 was used, expressed in $dpm(gC)^{-1}$. The CO_2 concentration in ppm from 1850–2015 was taken from (EIA US Energy Information System 2022). Since 1 dpm corresponds to .0167 Bq, and 1 ppm corresponds to 2.124 GT of carbon in the whole atmosphere, 1 $dpm(gC)^{-1}ppm$ = .0354 PBq. It is easy to understand why the curves are different. Since 1950, the total carbon abundance has gone up more than 30%. Since the net change in $^{14}C/C_{total}$ is now small, the ^{14}C total activity must also be well above its 1950 value. Displays of the "bomb pulse" in plots of specific activity such as in Fig. 1 [generally using $\Delta^{14}C$ instead of $dpm(gC)^{-1}$] are common in the literature and on the internet, but displays such as in Fig. 2 of total content or total activity are only occasionally found, as in Caldeira et al. (1998). Skrable et al. are not the first to have been tripped up by this (Andrews 2020). Skrable et al. (2022) quote Wikipedia as saying the total atmospheric content of ^{14}C is 140 PBq. Looking at Fig. 2, that would have been a good number before the bomb testing. Perhaps the Wikipedia contributor (a 2009 book is referenced) also mistakenly thought the total activity, not just the specific activity, is now back near its 1950 value.

Not all the increase in atmospheric ^{14}C since 1950 evident in Fig. 2 is from residual bomb carbon. The nuclear testing resulted in high $^{14}C/C_{total}$ ratios in the atmosphere compared to the land and sea sinks, causing the ratio to fall

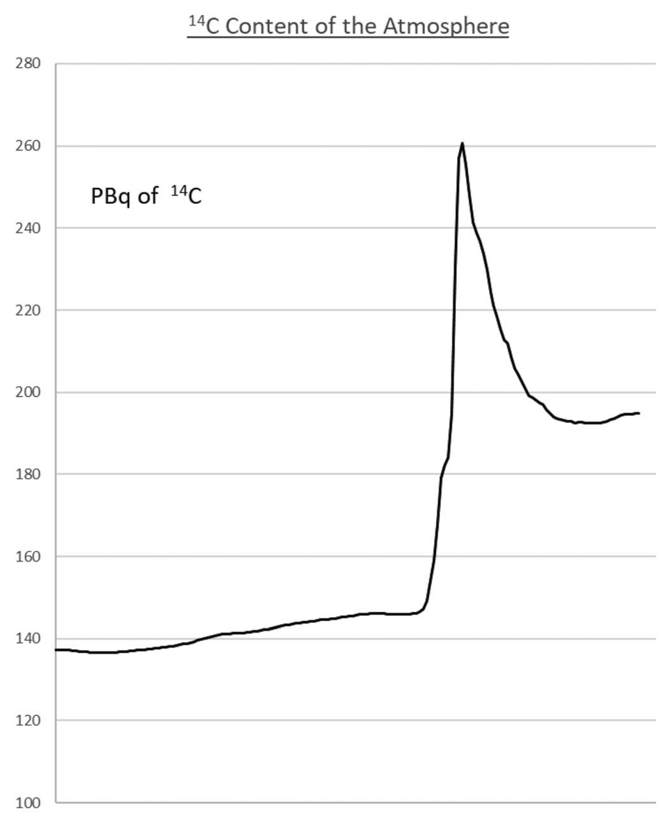

Fig. 2. ^{14}C content of the atmosphere.

in the atmosphere and rise elsewhere as the inventories mixed. The Suess effect now reverses the isotope ratio gradient, and a net efflux of ^{14}C from land and sea sinks to the atmosphere results (Caldeira et al. 1998). But one can hardly look at Fig. 2 and argue that ^{14}C produced in nuclear testing can be ignored in understanding ^{14}C levels in 2004–2012, the only period from which Skrable et al. (2022) took specific activity data. Net ^{14}C dilution is suppressed, not because anthropogenic carbon in the atmosphere is lower than conventional models suggest as Skrable at al. argue, but because substantial ^{14}C from nuclear testing remains circulating in the fast carbon cycle. Perhaps a health physicist reading this should worry about dosages from the extra ^{14}C in the air we breathe now compared with 1950.

Health Physics readers may have had their appetites whetted to study this topic further. While this is not the place for a thorough literature review, Caldeira et al. (1998) anticipated details of Fig. 2, and Turnbull et al. (2009) analyzed the effects of ^{14}C dilution with anthropogenic carbon, but with full appreciation of the role and complications of bomb carbon. A good place for the interested reader to start would be the recent review by Graven et al. (2020) and references therein.

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