
From: **entropy** <entropy@mdpi.com>
Date: Mon, Oct 21, 2024 at 2:53 AM
To: David Burton <ncdave4life@gmail.com>

Dear Dave,

Professor Stallinga just provided an informal reply regarding your comment via email. Please kindly check it.

Kind regards,
Vincent

(attachment)

From: **David Burton** <ncdave4life@gmail.com>
Date: Mon, Oct 21, 2024 at 10:49 AM (as corrected Mon, Oct 21, 2024 at 12:21 PM)
To: entropy <entropy@mdpi.com>

Dear Vincent & Rita,

Thank you very much for forwarding Dr, Stallinga's response to me. It resembles my previous, private attempts to discuss this with him directly, in which he consistently ignored the points I made, while asserting that I am confused.

Dr. Stallinga wrote:

> *This subject remains confusing. And the comment on my paper once again shows that. Mr. Burton is confused, possibly by the definitions of the terms residence time and adjustment time. With the residence time the time molecules spend on average in a box, and the adjustment time defined as the exponential decay time to a new equilibrium after an abrupt stop of excitation signal (delta-Dirac, pulse, or Heaviside stimulus), the adjustment time is always shorter than*

> *the residence time in either of the two boxes, as can easily be shown mathematically by solving the ordinary differential equation.*

The actual residence time under discussion is not "the time [CO2] molecules spend on average in a [theoretical] box," Rather, it is the time they spend in the air, before being removed, either temporarily or permanently, or being exchanged with CO2 molecules from other reservoirs. It is generally (but roughly) estimated to be about 3-5 years. The IPCC calls this the "turnover time."

As I wrote in my comment, the adjustment time is "the duration of the effect on CO2 levels from adding CO2 to the atmosphere." As I explained, "It is easily shown that the adjustment time is much longer than the residence time, because some of the processes which reduce the residence time do not reduce the adjustment time,"

Dr. Stallinga simply ignored that.

As I explained in my Comment:

1. Processes which **permanently remove CO2** from the air equally shorten both the residence time **and** the adjustment time.
2. Processes which only **temporarily remove CO2** from the air, before soon returning it to the air, shorten the residence time, **but not** the adjustment time.
3. Processes which **exchange CO2** in the air with CO2 from another reservoir, shorten the residence time, **but not** the adjustment time.

You don't need to solve differential equations to understand that when three processes shorten the residence time, and only one of the three shortens the adjustment time, it means that the residence time must be shorter than the adjustment time.

As I wrote in my Comment, "The adjustment time can be determined from measurements, and it is approximately fifty years." I also explained in detail how the adjustment time is determined from measurements, and I also cited an excellent paper reporting that result.

Dr, Stallinga ignored that, too.

Dr. Stallinga wrote:

> *As was done in the paper: The adjustment time τ is given by $1/\tau = 1/\tau_{r1} + 1/\tau_{r2}$, and is always shorter than residence time τ_{r1}, as well as τ_{r2}. It is like two parallel resistances, R1 and R2, which have a total resistance smaller than each of the individual resistances. $1/R = 1/R1 + 1/R2$.*
>
> *There is no way avoiding this.*

Dr. Stallinga is apparently attempting to model the carbon cycle with a simple box model, with two unidirectional flows of CO_2 out of the air. He mistakenly used the residence time (turnover time) as the first of those two unidirectional flows, with time constant τ_{r1}, (and the second, with time constant τ_{r2}, is not defined).

But that's not what the residence time represents. Most of the contribution to the short residence time is from factors other than the unidirectional (permanent) flow of CO_2 out of the atmosphere. The movement of CO_2 permanently from the atmosphere to other reservoirs is a contributor to the short residence time, but the major contributors are merely **temporary removals**, and **exchanges** of CO_2 with other reservoirs.

If his second flux (τ_{r2}) is supposed to represent CO_2 flowing ***into*** the atmosphere, such as the release of CO_2 by rotting grasses and leaves (carbon which had been briefly sequestered when the grasses and leaves were growing), then Dr. Stallinga has gotten the sign wrong, because those processes increase the adjustment time, rather than reducing it.

Dr. Stallinga wrote:

> *All the rest of the comment of Mr. Burton then becomes irrelevant, as in not pertaining to my publication in Entropy. I will thus not further comment on it. For example, "long tails". If functions have long tails, they are not exponential but more power-law (read the books of Nassim Taleb, Black Swan and Antifragile where he describes such functions, as well as my own comment on scalable functions in DOI: 10.9734/BJMCS/2016/28107) and such functions do not have an adjustment time.*
>
> *Therefore such an analysis, while possibly correct, is not covered by my publication. I will not comment on the correctness of the ideas of Mr. Burton,*

> *apart from pointing out his misunderstanding of the concepts of 'residence time' and 'adjustment time'.*

As I wrote in my Comment, it is true that the theoretical decay curve of the CO2 level in the atmosphere is not precisely exponential. The "exponential" decay curve is merely an approximation. (It is a pretty good approximation for at least the first half-life.)

Dr. Stallinga wrote:

> *The onus is on Mr. Burton and others to explain how simple diffusion processes can have long tails, apart from coming in handy to explain away obfuscated models.*

We're not talking about "simple diffusion processes," we're also talking about biology, as I explained in my Comment:

"If CO_2 levels were falling, “browning” would replace “greening,”[Zhu 2016] and the terrestrial biosphere would presumably become a source of CO_2, rather than a sink. So models of the theoretical decay curve for atmospheric CO_2 in the hypothetical event that emissions suddenly cease typically show a "long tail."[Archer 2008] That is, the decay curve would gradually diverge from a simple exponential decline, in that the second CO_2 half-life would be longer than the first, the third would be longer than the second, etc."

Dr, Stallinga simply ignored that, too.

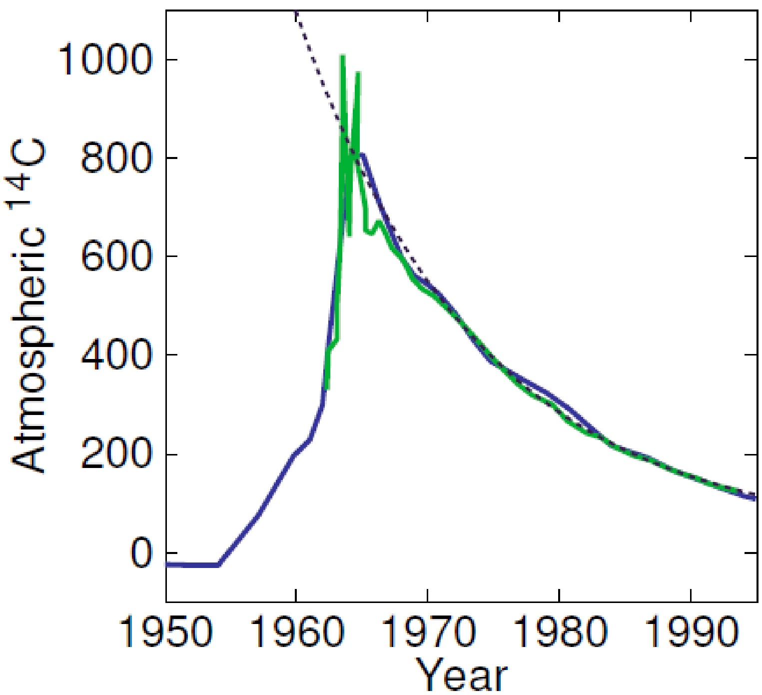
Dr. Stallinga wrote:

> *As to the mistake of reporting the decay time of 14 years: That was my own personal fit to some specific data, and not a "mistake". There is a wide range of values, and the value given by Burton (20 years) is not an odd one out and within the range of reported values, surely not absurd compared to 14 years. All based on modeling and thus can be discussed. My own value is not a "mistake", but a simple model (the simplest one can imagine, and thus pedagogically correct) to highlight how adjustment times in principle can be determined. My publication*

was not about the exact value of the residence times or adjustment times but rather twofold:

The specific data which he "fit to" was Δ14C, which is defined as the fraction of atmospheric carbon which is (or was) in the form of the 14C isotope, expressed in parts-per-thousand (‰), relative to a standard "Modern" value for that fraction. So, for example, if 14C were double the standard "Modern" fraction it would be expressed as +1000‰, or if it were half the standard "Modern" fraction it would be expressed as "-500‰". Using Δ14C for that purpose was a mistake because Δ14C is reduced by "Suess effect" dilution, which does not remove CO2 from the air. Here's his Figure 4:

https://sealevel.info/Stallinga2023_Fig4.png



The green trace is from Nydal 1999, On p.26 of that paper, Nydal wrote that, **"$\delta^{14}C$ values (the deviation in the $^{14}C/^{12}C$ ratio relative to a standard) were calculated as per mille excess above the normal ^{14}C level defined by the US NIST oxalic acid standard."**

The blue trace is from Perruchoud 1999. On page 7 of that paper, Perruchoud wrote that, **"all ^{14}C data are expressed in ‰$\Delta^{14}C$."**

In other words, the vertical axis which Dr. Stallinga labeled "Atmospheric 14C" is not the amount of 14C in the atmosphere. Rather, it is the fraction (per mille) of carbon in the atmosphere which is in the form of 14C, minus a baseline standard fraction. (There's a slight ambiguity w/r/t whether 13C is included in the denominator.)

Since the denominator (the amount of 12C or 13C+13C carbon in the atmosphere) was increasing, due to the addition of 14C-depleted fossil carbon, Dr. Stallinga's Fig. 4 shows a decay time constant which is too short (about 14 years). The actual average atmospheric lifetime of "bomb spike" 14C was about 20 years.

Note that the approximately 20 year bomb spike lifetime is not primarily "based on modeling," it is based mainly on measurements. The ony use of modeling in that calculation is to account for Suess effect dilution. Since Suess effect dilution only modestly reduced Δ14C over the first half-life of the bomb spike decay, errors in modeling that effect could only slightly affect the calculated 14C lifetime of 20 years.

Dr. Stallinga wrote:

> *- The adjustment time is shorter than the residence time, as can be mathematically shown, and then*

As I showed, that is wrong, because both exchanges of CO2 with other reservoirs and temporary removals of CO2 from the air contribute to the short residence time, without significantly reducing the adjustment time.

Dr. Stallinga wrote:

> *- Using the residence time supplied by the IPCC (4 years, which seems rather short, so 5 years is used to be safe), make a convolution of input signal ('emissions') and find the resulting signals, with conclusions.*

Again, Dr. Stallinga has confused the short residence (turnover time) time with a unidirectional flow of CO2 out of the atmosphere. That's not what it is.

Dr. Stallinga wrote:

> *Nothing else. And there are no "mistakes" made anywhere. The discussion of the determination of the values of the residence times is not the aim of the publication and is left to others. If Mr. Burton does not agree with the above two items, it means he rejects the core ideas of the report of the IPCC. Which is an important observation in itself.*

I have a number of disagreements with the IPCC (some of which I enumerated in many comments on their AR5 and AR6 Reports, in my role as an Expert Reviewer). But I do not disagree with their definition of what they call the "turnover time," which Dr. Stallinga calls the "residence time." IPCC AR6 WG1, Annex VII Glossary, p.2237 says:

> *"...In simple cases, where the global removal of the compound is directly proportional to the total mass of the reservoir, the adjustment time equals the turnover time: T = Ta. An example is CFC-11, which is removed from the atmosphere only by photochemical processes in the stratosphere. In more complicated cases, where several reservoirs are involved or where the removal is not proportional to the total mass, the equality T = Ta no longer holds.*
>
> *"Carbon dioxide (CO2) is an extreme example. Its turnover time is only about 4 years because of the rapid exchange between the atmosphere and the ocean and terrestrial biota. However, a large part of that CO2 is returned to the atmosphere within a few years..."*

The CFC-11 example is wrong, because CFC-11 is not "removed from the atmosphere only by photochemical processes in the stratosphere." It dissolves in water, and some of the removal of CFC-11 from the air is due to that (Wang et al 2021). But the IPCC's error on that point is irrelevant to our discussion. Their main point is correct: the "rapid exchange" of CO2 between the air and other carbon reservoirs, and merely temporary removals of CO2 which is quickly returned to the air, contribute greatly to the very short "turnover time" (residence time), but they do not significantly shorten the adjustment time.

I am grateful to you for forwarding Dr. Stallinga's response to me. I will also be grateful if you publish my Comment, with or without his response to it.

If you think that any of the points which I made in this email need to be added to my comment, or if you require any other changes, then I'll be happy to make those changes.

The attached version of my Comment is identical to the version which I sent on October 4th, except that I've "accepted changes" in Microsoft Word (so that the edits which I made in the final three paragraphs aren't shown with red insertions and strike-throughs when viewed in GMail or Google Docs).

Do you need an Abstract for my Comment? If so, we can just use the first five paragraphs as the Abstract.

Warmest regards,
Dave

(attachment)

From: **entropy** <entropy@mdpi.com>
Date: Wed, Nov 13, 2024 at 3:13 AM
To: David Burton <ncdave4life@gmail.com>

Dear Mr. Burton,

We are still checking with our academic editor for this. If there is any further update, we will let you know.

Kind regards,
Vincent Shang
Managing Editor

From: **entropy** <entropy@mdpi.com>
Date: Wed, Nov 20, 2024 at 12:58 AM
To: David Burton <ncdave4life@gmail.com>

Dear Mr. Burton,

I trust this email finds you well. We have received suggestions from our academic editor, which are posted below. And it is a pity to let you know that we can not further process this comment.

Burton's comment on Stallinga's article, sent by email rather than the usual submission procedure, criticizes the author's interpretation of CO_2 residence time adjustment time, claiming that Stallinga misrepresents these concepts. Burton asserts that the adjustment time is longer than the residence time, not shorter, and provides different atmospheric lifetimes of CO_2 to support his argument. Burton's comment is detailed, including calculations, models, and quotes from various studies, to correct what he perceives to be significant interpretation errors in Stallinga's work.
Burton's comment is exceptionally extensive, covering multiple sections and discussions of carbon models. This level of detail goes beyond the scope of a typical comment and makes the response difficult for readers to follow. The comment assumes a high level of knowledge on the part of readers.
The comment is excessively detailed and includes tangential arguments. For these reasons, I recommend declining to publish this comment.

Kind regards,
Vincent Shang
Managing Editor