15 October 2025 ·

Qeios

Peer Review

Review of: "The Role of Carbon Dioxide in the Evolution of Intelligence: Solving the Dinosauroid Question"

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This article by Ugo Bardi is clearly written and thought-provoking but falls into the category of pushing an idea based on a correlation, rather than building a plausible case based on a reasonable analysis of relevant mechanisms that would establish possible biological causality. The presentation is highly speculative and treats the relative literature superficially, relying on a limited number of sources, often in the secondary literature. The tone of the argument can be annoying in that the disparate evidence is selectively used, often without proper context and without sufficient attention to statistical variation and uncertainty in the data. As with the dinosaurid idea of Dale Russell, it borders on science fiction, but to its credit, it raises ideas not widely considered and so can be considered informed science fiction that stimulates the imagination and could inspire the necessary analyses to move the ideas beyond speculation to supported hypotheses. The proposed linkages are suspect and poorly supported, as outlined below, but do have enough credibility that they justify the necessary work that would demonstrate why they are false, should this indeed be the case. Enjoy the arguments, as they are clear, but be skeptical as the speculation (upon speculation) is a long way from being accepted.

Weaknesses: The author proposes that an increase in the $O2/CO2$ ratio drives an increase in intelligence, via a higher encephalization quotient (EQ). The supposed mechanism is disrupted physiology of large brains due to physiological imbalances in the blood that reduce effective $O2$ levels, such that larger brains would not be possible at low $O2/CO2$ ratios. Some weaknesses in this argument are as follows:

1. There have been, globally in the past century, in indoor environments for millennia, and experimentally, variations in $O2/CO2$ of the magnitude the author proposes would lead to the difficulties that cause reduced function of large brains. This variation could be considered to be 300 to 1000 ppm. However, there is little evidence that 300 to 1000 ppm is causing reduced mental

 doi.org/10.32388/6ORKYU

performance, or put differently, increased mental performance in past atmospheres of reduced CO_2. 300 to 1000 ppm are common indoor air levels regarded as safe, while 1000 to 2000 ppm are considered to be a sign of poor air quality that can promote drowsiness. Only above 2000 ppm are acute problems such as headaches noted. In discussing the problems occurring at elevated CO_2 (low O_2/CO_2 ratios), the author is unclear on exactly what levels of O_2/CO_2 are considered problematic, in that reduced mental or physical performance would create sufficient variation that natural selection could act upon. The author implies levels twice the current CO_2 levels and raises alarms for expected global increases in atmospheric CO_2. Problems often restricted to greater than 2000 ppm are made to sound common below 1000 ppm. This matters because, against a backdrop of 21% O_2 in the late Cenozoic, variation in CO_2 has been largely below 800 ppm for the past 30 million years, and below 500 to 600 ppm for the past 5 million years. As Fig. 5 indicates, the majority of the increase in EQ in hominins has occurred in the past 5 million years, when CO_2 levels were largely below 500 to 600 ppm, based on ice core records or biogeochemical models. Given the absence of clear effects on mental performance in people in indoor environments where CO_2 is less than 600 ppm, it is difficult to see how these post-Miocene CO_2 levels would have created variation in performance that would have triggered pronounced selection for higher EQ.

2. If there was indeed strong selection for higher EQ, then one would expect, by the same physiological mechanism, that there would be parallel selection for higher EQ in many other animal lineages, including disparate mammalian, bird, and reptile clades. The author could produce parallel EQ graphs for these other clades as an independent and more convincing test. Similarly, if one argues the rise in EQ is restricted to apes and monkeys, then there are distinct old and new world monkey clades that should have shown similar rises in EQ. The methods of comparative evolutionary biology allow for testing of evolutionary hypotheses such as these where one cannot rerun the evolutionary clock or experimentally run evolutionary experiments. The author should consider these methodologies to provide some deeper analysis of the ideas presented.
3. The author focuses on the O_2/CO_2 argument to the exclusion of many other possibilities, including leading possibilities proposed by those who have carefully studied the issues. For example, it has been proposed that hominin movement into the expanding savannas of Eastern and Southern Africa is associated with selection for larger brains due to habitat challenges of living out in the open grasslands (the Savannaization hypothesis). This hypothesis would have a link to O_2/CO_2 and climate change because the grasslands driving savannaization are dominated by C4 grasses, which experience increased fitness at higher O_2/CO_2, that is, reduced CO_2 at 21%.

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4. The author selectively focuses on aspects of the data that appear favorable. For example, in Fig. 5, a trend line is presented between EQ values and estimated O_2/CO_2 as evidence in favor of causality. However, at a critical time in hominin evolution, at 1.18 O_2/CO_2 (PAL), there is a large spread in EQ values, as there is at 1.32 O_2/CO_2 for late H. erectus to H. sapiens. Such spread in the data can indicate other drivers of selection. If O_2/CO_2 was a strong driver, as suggested, then how did the variation in EQ at 1.18 O_2/CO_2 arise in the first place?
5. There is a general lack of statistical consideration and the inherent uncertainty in the data. No variation about EQ points is presented, and with one exception, the uncertainty in CO_2 predictions is not considered. Absolute O_2 and CO_2 data are not shown, but relativized data to current values exclusively are, which masks appreciation of actual gas concentrations that would have a physiological effect. Except for the ice core records extending back 800,000 years, the CO_2 and O_2 concentrations are modelled based on geological proxies and have large uncertainties. Similarly, the variation in EQ for species is treated as a narrow value when there is likely some degree of variation, which can be documented by evaluating extant species.
6. As numerous authors have pointed out, and Bardi notes in passing, it is a hazardous exercise to consider brain size, EQ, and other morphological characters as measures of intelligence and mental performance. The Nazis tried this, and the approach has been thoroughly debunked.
7. Intelligence should be dealt with carefully and not equated with mental function, which is really what the O_2/CO_2 argument is addressing. The mental function of many animals is sophisticated and sharp, enhancing the fitness of the respective animal in its environment. A tiger cannot do computational physics, nor track the stars, but it is keen in its ability to track prey and be aware of danger. These other mental faculties would also presumably be compromised by low O_2/CO_2 and lead to greater development in the respective neural capacity of the brain for the aspects that improve fitness. What makes humans fit is not necessarily larger brains, but the development of specific areas of the brain that facilitate abstract thought, communication, and social networks. Do these need more O_2 than other mental sectors, such as vision, smell, or situational awareness, which can be acute in other animals?
8. Finally, the superficial treatment of the vascular physiology and brain responses to O_2/CO_2 is disappointing given these areas are well studied in the biomedical literature, both theoretically and experimentally. I would have liked to have seen case studies demonstrating exact conditions when people and other animals might experience mental degradation due to low O_2/CO_2, and whether this threshold would be amenable to acclimation, exercise, or adaptation. When would the effects be

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large enough to trigger variation that natural selection could work on? Marmots, for example, routinely live in very low O2/CO2 in their burrows – how do they do this without neural degradation? Experimentally, there is a wealth of studies on people (e.g., those who work in artificial atmospheres in airplanes, spaceships, and submarines, and their experimental analogues) to provide exact specifics the author could use to build mechanistic plausibility for their argument. This should have been done.

In sum, the author proposes the first semblance of an idea but does not build a credible case based on the biological information and theory that is available. The annoying aspect is that the author attempts to convince the reader that the hypothesis is correct, using many of the flawed approaches one commonly experiences in places such as beerhalls where speculative ideas are put forth and argued for superficial, incomplete, or selectively picked treatments of the relevant literature. The ideas could just as easily have been presented in ¼ of the space as a correlation that leads to an interesting possibility, which then warrants careful study involving particular aspects of the literature and experimentation. In a typical peer-reviewed journal, this work would be rejected and the author encouraged to follow it up with meaningful analysis that moves beyond superficial treatments and speculation, or pared back to a two- to three-page letter that presents the relationship and suggests meaningful analysis that could follow to show causality.

Declarations

Potential competing interests: No potential competing interests to declare.