Table 1. Criticisms in Sweatman et al. (2024) and Our Replies[1,2]

1. Introduction

Comment "…[A]**ny study that** [Holliday et al. (2023)] **cite that does not relate the 'archaeological, paleontological, and paleoenvironmental records' to the Younger Dryas boundary (YDB), which is assumed to be synchronous wherever it occurs, can automatically be questioned.**"

Reply This is an absurd admonition representing blatant disregard for the scientific method.

Comment "**The environmental upheaval from** [the YDIH impact] ***may have contributed*** **to the extinction of megafauna and significant human cultural shifts and population declines during the late Pleistocene,** ***but whether they did is a separate question*** [emphasis added]**.**"

Reply Their response to our criticisms that there was not a catastrophic decline of the Clovis population, the megafauna extinctions occurred over an extended period of time rather than simultaneous, and neither were correlated to a singular date, appears to be a retraction, of sorts, of that claimed YDIH evidence.

Sweatman et al. (2024) and other YDIH proponent publications are rife with inconsistent and confusing logic. Sweatman et al. (2024) cannot argue they are a separate question to the YDIH, but then argue they are evidence of the YDIH. Yet, they continue to offer them as evidence. In Section 3.1 they quote Haynes (2008), **" 'The megafaunal extinction and the Clovis-Folsom transition appear to have occurred in <100 years, perhaps much less.' As Haynes sums up, '*A remarkable major perturbation occurred at [the YDB] that needs to be explained.*'** [emphasis added]**"** Also in Section 3.1 they write, "**It is our position that changes in Clovis cultural objects are consistent with a catastrophic event.**" In Section 3.2 they continue to argue, **"…many megafaunal extinctions or extirpations correlate strongly with the onset of the YD….**" In Section 6 they quote Haynes (2008), "'**Stratigraphically and chronologically the** [megafauna] **extinction appears to have been catastrophic, seemingly too sudden and extensive for either human predation or climate change to have been the primary cause.'** "

2. Defining the "Younger Dryas"

Comment "**…**[Holliday et al. (2023)] **describe different usages for the term 'Younger Dryas.' Nothing in this Section can refute the YDIH.**"

Reply The term "Younger Dryas" (YD) is frequently used in an ambiguous and imprecise way (sometimes as noun, sometimes as a phrasal adjective), particularly by proponents of the YDIH. Precision in scientific writing ensures clarity, reproducibility, and credibility by allowing accurate interpretation and replication of research, avoiding misunderstandings, and upholding ethical standards. It is essential for effective communication, peer review, and maintaining the integrity of scientific work. The term "Younger Dryas Boundary" (YDB) seems to have been coined by Firestone et al. (2007), to refer to stratigraphic units

[1] Quotations appear in bold face text.

[2] To avoid potential confusion, citations that appear within quoted text are modified within brackets to match our reference list when they differ in date enumeration or reference style.

just above the Allerød/Younger Dryas boundary, as opposed to the conventional use of the term for the transition itself. Both YD and YDB have been used as a temporal term, a climate term, and as a stratigraphic term (e.g., represented by the "Black Mat") that can and usually does have boundaries that vary in time from site to site and even within a site.

3.1. Misunderstood Decline of Clovis Paleoindians

Comment "**Haynes (2008) writes 'that no post-Clovis Paleoindian artifacts have ever been found in situ stratigraphically below [the YD black mat].' "**

Reply That is because they are post-Clovis and thus occur in younger YD or post-YD strata. But black mats that started forming before the YD and continued to develop afterwards are well documented by Holliday et al. (2023) and Pigati et al. (2012). See also reply in Section 6.

Comment "**The lower boundary of this black mat often marks the position of the YDB.**"

Reply It *often* does in Southeast Arizona and a few other areas, but clearly doesn't everywhere (e.g., Holliday 1995, 1997; Mandel 2008; Meltzer and Holliday, 2010; Holliday and Miller 2013). Sweatman et al.'s (2024) claim implies the proposed impact debris layer is at the base of the black mat. However, this contradicts claims by other YDIH proponents that the impact debris layer is the black mat. Often YDIH proponents contradict themselves on this very point (Holliday et al., 2023).

Comment Quotes Haynes (2008), "**The YD black mat covers the Clovis age landscape….**"

Reply Black mats are recorded through Quaternary and older terrestrial stratigraphic records but make up a small percentage of the sedimentary record. They bury a very small percentage of "Clovis landscapes" (e.g. Holliday and Miller, 2013).

Comment "**It is our position that the changes in Clovis cultural objects are consistent with a catastrophic event at the YDB and, therefore, do not contradict the YDIH. This debate continues below in Section 5.7** [of Sweatman et al. (2024)]."

Reply This is mere conjecture. No evidence is provided here or in Sweatman et al. (2024, Section 5.7). The burden of proof is on them to demonstrate that the end of Clovis artifact manufacture was catastrophic. Paleoindian archaeological research is a continuing endeavor like most scientific research. The age of the Clovis and especially subsequent Folsom technologies are better dated now than they were in 2008.

3.2. Misunderstood Megafauna Extinctions

Comment "**Haynes (2008) also notes 'No skeletal remains of horse, camel, mammoth, mastodon, dire wolf, American lion, short-faced bear, sloth, tapir, etc., or Clovis artifacts have ever been found in situ within the YD age black mat.' "**

Reply He *does not say* that remains are directly below the black mat nor that those extinctions and the end of Clovis are all synchronous. The extinctions are well documented to vary by species over space and time, clearly contradicting the YDIH. Clovis archaeology is now known to persist into the YD chronozone (YDC), see Section 5.7. Their claim is also complicated by Sheriden Cave, which produced extinct flat headed peccary and giant beaver remains stratigraphically *above* Clovis artifacts and a purported YDB impact proxy layer (Redmond and Tankersley, 2005).

Comment “…[R]**ecent reports … show that many megafaunal extinctions or extirpations correlate strongly with the onset of the YD (O’Keefe et al., 2023; Stewart et al., 2021)**”.

Reply “**Temporal staggering is a defining feature of the late-Quaternary megafauna extinctions, … with extinctions concentrated in different time windows in different areas, extending from ~50,000 years ago until well into the Holocene, and often spread across thousands of years even within a given region**” (Svenning et al., 2024, p 5). Thirty-eight megafauna species became extinct in North America. As synthesized by Holliday et al. (2023), Grayson (2016, table 4.2) shows that current dating indicates that only about half of the megafauna survived to 13,000–10,000 ^{14}C yr BP (~15.6 k to ~11.5 cal ka BP) in North America, none of the avian extinctions are dated. In South America, extinctions continued until as late as 7‑5 kya (Prado et al., 2015).

O’Keefe et al. and Stewart et al. clearly show a decline in species and staggered extinctions of most species before the YDB. Sweatman et al. (2024) are apparently unaware of these interpretations by Stewart et al. (2021) who stated the following:

p 5 “**…final declines in horse and saber-tooth cat population densities significantly pre-dated those of mammoths and mastodons.**”

p 6 “**…the mammoth and mastodon data suggest that this occurred not with the arrival of Clovis-point wielding people, but much later during the YD.**”

p 7-8 “**…final mammoth population declines appear to have occurred later in the YD, and final horse population declines appear to have occurred during the terminal** [Bølling-Allerød]**.**”

3.3. Problematic chronologic and paleoclimatic assumptions

Comment “**Holliday et al. (2023) cite the speleothem evidence reported by Cheng et al. (2020) and Nakagawa et al. (2021). However, speleothem resolution is typically > 10 years and the GICC05 chronology has a maximum counting error of 140 years, while the postulated impact winter is suggested to last much less than 1 year.**”

Reply Cheng et al. (2020, p 23409) note the “**…Seso δ^{18}O record confirms a robust correlation with NGRIP within ±20 to 40 y, suggesting that the NGRIP chronology (on the GICC05 time scale) around the YD is more precise than the quoted absolute error of ±100 to 140 y….**” Indeed, the Maximum Counting Error has long been regarded as a conservative (2-sigma) error estimate. Recent synchronization of the GICC05 and U-Th (speleothem) chronologies by Muschitiello and Aquino-Lopez (2024) produces an age offset at 12,950 [b2k] of ~ 5 yrs.

Nuclear winter scenarios typically show multi-year to decadal-long global effects, and it’s difficult to imagine that the “Impact Winter” scenario of Wolbach et al. (2018a, p 179) (where ~10% of the world’s biomass burned, 6 weeks of smoke blocking all sunlight, etc.) would have an impact that lasted “much less than one year.” Indeed Mahaney and Somelar (2024, p 24) note: “**…the nuclear winter event as postulated by the YDIH would last 1.4 kyr.**”

Comment “[Holliday et al. (2023)] **also claim that the similarity of the YD to other Pleistocene Dansgaard-Oeschger (D-O) periods of abrupt climate change indicates that the YD cooling needs no special explanation. However, it is simply** [their] **opinion….**”

Reply Hodel et al. (2023) describe hundreds of instances AMOC/ACC-related millennial climatic variations over the past 1.5 Myr recorded in a single North Atlantic sediment core. The YDIH proponents must demonstrate that the YDC is somehow unique rather than simply assert it.

4. Flawed Sampling

Comment "…[Holliday et al. (2023)] **claim that YDIH proponents have not provided an adequate sampling of time intervals across the YDB to be sufficiently confident that the YDB is unique… the YDIH does not claim the YDB is unique….**"

Reply Sweatman et al. (2024) dodges the issues of inadequate sampling by confusingly and misleadingly stating the YDB is not unique.

Firestone et al. (2007, p 16021) wrote, "**The *unique*, carbon-rich, YDB layer, coupled with a distinct assemblage of impact tracers, implies isochroneity of the YDB datum layer** [emphasis added]**….**" Kennett et al. (2008, p 2531) wrote, "**…distinctive YD black layer … serves as a stratigraphic marker horizon … where Clovis artifacts and select Rancholabrean fauna occur below, but never within or above this *unique* bed** [emphasis added]**….**" Firestone (2009, Section 6) wrote, "**Unique impact markers found in the YDB layer….**" Kennett et al. (2015, p E6723) state, "**…**[N]**o interval other than the YDB layer in 23 widely separated stratigraphic profiles, spanning up to 50,000 y, contains the same broad assemblage of proxies….**" But no sites with long (thousands of years), continuous records of sedimentation have been subjected to close-interval, continuous sampling to show that claimed impact proxies are unique to ~12.8 ky.

4.1. Arlington Canyon Confusion

Comment "**…**[M]**ap coordinates provided by Scott et al. (2010) for the SRI-09 samples show definitively that they did not come from the Arlington Canyon site** [AC003] **sampled by Kennett et al.** [(2009b)]**… and do not bear on the YDIH.**"

Reply Scott et al. (2017, p 44-45) state "[w]**hile Kennett et al.** (2008, **2009b) gave UTM coordinates without specifying which datum or map projection was used, we were able to navigate to their published location using the North American Datum 1983 (NAD83) and found there the largest, best exposed, and most accessible outcrop in Arlington Canyon. Later we surmised that Kennett et al.** (2008, **2009b) had used NAD27 (confirmed in Wittke et al., 2013).**" The Kennett et al. section Scott et al. located was measured, sampled, and dated by Scott et al. (2017), a paper ignored by YDIH proponents. Comparisons of photographs in Scott et al. (2017, Fig. S2 supplemental) taken at our location and those of AC003 in Wittke et al. (2013, supplemental) unquestionably demonstrate they are the same site. Our coordinates for that location are 762436,3764546 NAD83 zone 10. Those reported for AC003 are 762524,3764532 NAD27 zone 10 (Wittke et al., 2013) and must be in slight error.

Scott et al. (2017, p 45) "**…continue to be puzzled why YDIH proponents have focused extraordinary attention on one single age horizon in one <5 m section, when such a broad range of deposits and ages are represented in the surrounding area (see Hardiman et al., 2016).**" If the YDIH impact event occurred, geophysical markers must be present in all YDB-aged sediments on the Channel Islands, not only at the AC003 site as confusingly implied by Sweatman et al. (2024). In contrast to Kennett et al., we conducted a comprehensive study where additional sites were examined and their coordinates are reported in Scott et al. (2010, supplemental). Stratigraphy in Arlington and in other canyons on Santa Rosa Island including material ranging in age from ~29,000 cal a BP to ~5,000 a BP were also described, analyzed, and sampled (Scott et al., 2010; Pinter et al., 2011; Hardiman et al., 2016). Scott et al. (2017, p 45) wrote, "[w]**e show from our lithological logging and analysis that there was not an 'impact horizon' as claimed.**"

Carbonaceous materials from Arlington Canyon do not require extraterrestrial (ET) input or ignition, or in some cases preclude such an event. Details of all the arguments concerning the carbonaceous material are discussed in detail in Scott et al. (2017). None of the YDIH proponents (including Sweatman, Kennett or West) have considered any of our data or evidence and themselves are not experts on modern wildfires and appear to have limited understanding of fire in the fossil record.

4.2. Selective Sampling at Abu Hureyra

Comment **"…**[Holliday et al. (2023)] **dispute the origin of scoria-like objects, i.e., meltglass, at Abu Hureyra. They argue that because meltglass occurs at other levels at Abu Hureyra and other archaeological sites in the Levant with similar dates, the scoria-like objects at Abu Hureyra are unlikely to have been produced by an ET impact. … discussed by Sweatman (2021), the key point is … Abu Hureyra meltglass was shown to form at temperatures above 2000°C, consistent with an ET event …."**

Reply There was selective sampling by YDIH proponents who only examined melt glass from the purported YDB, precluding comparison to other melt glass at the sites. Nevertheless, the key point of Sweatman et al. (2024) is the melt glass at the purported YDB is assumed to differ from the otherwise ubiquitous melt glass at other levels at Abu Hureyra and at other sites due to the high melting temperature inferred by YDIH proponents.

Thy et al. (2015, p 205) challenge those melt temperature estimates. They wrote, "**…point analyses obtained in this study with the** [electron microprobe] **represent the melt (now quenched as a glass) present in the droplets. The alternative would be to analyze a larger area … to obtain … bulk compositions of the droplets. The former approach would ideally allow us to trace the initial melting process and to estimate the melting temperature. The latter approach would ideally allow an estimate of the final fictitious equilibrium temperature when all minerals have melted and only melt remains. This latter was apparently chosen by Bunch et al. (2012) and Wittke et al. (2013), although the descriptions of their analytical methods are not conclusive. For materials that represent non-equilibrium and incomplete melting, as argued here, this approach may be fatal for obtaining reliable temperature estimates."** They also wrote, **"…melts in the Syrian droplets projects around binary melting relations approaching eutectic melting in the** [CaO-Al_2O_3-SiO_2] **ternary system indicating temperatures of 1170 C, or above …. The results by Bunch et al. (2012) and Wittke et al. (2013) varies from the same eutectic towards the SiO_2 apex, suggesting an increasing component of unmelted quartz incorporated into the analyses. These analyses thus do not necessary imply increased melting temperature to 1500 C, or above …."** They then discuss additional evidence that the melt temperature estimates by Bunch et al. (2012) and Wittke et al. (2013) are unreliable.

5.1. Befuddled dating the beginning of the YDC

Comment "**…**[Holliday et al. (2023)] **claim that the platinum abundance in the GISP2 ice core at 12,825 ± 10 cal BP follows by around 25 years the occurrence of a large fluctuation in the deuterium excess (d)** [sic] **in the NGRIP ice core which they use to define the onset of the YD period (Steffensen et al., 2008). Thus,** [Holliday et al. (2023)] **claim that any event associated with the GISP2 platinum signal cannot be responsible for the triggering the YD period.**" Sweatman et al. (2024) question the accuracy of the deuterium climate proxy to date the YD onset and *guess* that the onset date is actually synchronous with the Pt signal. They then discuss other climate proxies but ignore the results of Cheng et al. (2020).

Reply Effects necessary follow causes in time. Why are "signals" of putatively ET origin that *postdate* the YD/GS-1 transition by decades somehow responsible for the *cause* of the transition?

Cheng et al. (2020, p 23415) wrote, "**A possible extraterrestrial impact event at ~12,820 B.P. inferred by Pt-anomaly in the GISP2 ice core appears to lag the initial onset of the YD by ~50 y without apparent disruption on the hydroclimate trend…. These observations are thus inconsistent with the hypothesis that the extraterrestrial event triggered the YD unless the extraterrestrial event did not leave any imprints in the Greenland ice core, which would be also inconceivable.**"

There is thus no evidence whatsoever that the Pt signal is synchronous with the YD onset. Cheng et al. (2020, p 23414) examined additional climate proxies and wrote, "**…immediate hydroclimatic impact** [of Pt deposition event]**, if any, was likely minor as inferred from GISP2 $\delta^{18}O$ record …. In the same ice core, the Pt-anomaly occurred at the middle of a gradual increase in Ca^{2+} (dust proxy) … without disrupting the course …. Provided that the GISP2 and NGRIP records were synchronized precisely … the Pt-anomaly did not disrupt NGRIP and AM $\delta^{18}O$ records …. Additionally, there is no clear evidence that the YD-onset excursion has been interrupted substantially around the time of the Pt-anomaly, either in the South American Monsoon or in tropical records…. These observations are thus inconsistent with the hypothesis that the extraterrestrial event triggered the YD unless the extraterrestrial event did not leave any imprints in the Greenland ice core, which would be also inconceivable.**"

If the source of the Pt is from an impact, then other platinum group elements (PGE) should have also been deposited in YDB sediments. However, purports of elevated Ir (Firestone et al., 2007) at the YDB could not be reproduced (Paquay et al., 2009). Nakagawa et al. (2021, p 12) wrote, "**Because Lake Suigetsu was a stable sedimentary basin and the annually laminated layers were not subject to vertical mixing, the iridium-rich dust (if it did shower Japan) should be preserved and show a sharp iridium peak in the sediment …. There is no visible peak in iridium across the *ca*. 250-year long period that certainly encompasses the onset of the Younger Dryas.**" Furthermore, Paquay et al. (2009), Wu et al. (2009, 2011, 2013), and Sun et al. (2020, 2021) showed measured Os isotopic ratios in YDB sediments do not support a meteoritic source. Impact proponents wrote, "**Our analyses … do not support an extraterrestrial origin of the platinum** [group] **metals in YD horizons from North America and Europe**", Wu et al. (2009). Sweatman et al. (2024) admit, "**…analysis of the *relative PGE abundances in the YDB does not clearly indicate their origin*** [emphasis added]." In the wishful flawed logic of Sweatman et al. (2024), Pt of unknown terrestrial origin (see Section 11) that is deposited near the onset of the YD/GS-1 is evidence that the accepted dating of the onset of the YD/GS-1 is incorrect.

5.2. Pseudoarchaeological Divined Date of the Impact Event

Comment "[Holliday et al. 2023]**'s counter-argument hinges on quotes from Notroff et al. (2017), which simply express the latter's opinion… their view was … rebutted by Sweatman and Tsikritsis (2017b), an article** [they] **fail to mention or cite."**

Reply We cited Sweatman and Tsikritsis (2017b), but it fails to rebut criticisms. **"The established chronology** [of Göbekli Tepe] **is strictly based on archaeological dating (i.e. typological comparison) of material culture as well radiocarbon dates produced by organic matter in direct relation to the architecture in this specific context. … can hardly count as 'opinion', in particular since even the oldest yet obtained dates from this context are still considerably younger than the dates Sweatman and Tsikritsis suggested … based on a single (and highly selective) iconographic analysis. They argue that the exact date of the construction of Pillar 43 is not known, but … suggest other than, in what appears circular reasoning, pointing to the (still only assumed) Younger Dryas Impact which they say is depicted on the pillar. Yet this again leaves out the problem of their selective iconographic approach as discussed in detail in our original rebuttal"** (Notroff, 2024).

Sweatman and Tsikritsis (2017a) interpret the imagery on Pillar 43 to specify the date at which the sun was relative to certain constellations, at one of the equinoxes or solstices. Banning (2023, p 18) wrote, "**…with so many alignments … so many celestial bodies and potential celestial events, and so many ways to define an alignment, it would be surprising if there were no astronomical alignments at all, just by accident, especially considering the chronological uncertainties. … so much iconography, providing so many images from which to choose, that one might argue for correspondence with almost any constellation. We as yet find no consistent, repeated, or internally coherent pattern of plausible astronomical indicators that would strengthen a hypothesis for astronomical significance.**" Sweatman (2024, p 27-28) admits their interpretation of "**…animal symbols on Pillar 43 with Greek constellations … is based on a subjective evaluation…**" (i.e., their opinion), despite their dubious claims of "**…compelling statistical analysis….**"

The interpretation of prehistoric imagery can only be conjecture, as we have no written sources explaining the depictions. Dietrich (2024, p 32, 40-41) provides an interpretation for Göbekli Tepe that does not require an unproven and extraordinary impact event and, instead, is based on archaeologically derived cultural and social context of the makers and their worldview.

5.3. Deficient Dating of YDIH Sites

Comment "**…**[Holliday et al. (2023)] **must provide dispositive evidence that the ages of many YDB sites are clearly inconsistent with the age range of the YD impact (12,835-12,735 cal BP) reported by Kennett et al. (2015).**"

Reply Standard deviations of hundreds of years and more (many based on problematic samples or interpretations), as noted by Meltzer et al. (2014) and Holliday et al. (2023), clearly offer no proof that all samples date to 12,800 cal yrs.

Several serious problems with the statistical analysis by Kennett et al. (2015) were recently noted: pubpeer.com/publications/1F7147A644242D2914CF890FA5F7E0

Comment "…**Meltzer et al. (2014) … ignore the uncertainty in their linear age-depth models for each YDB site….**"

Reply As noted by Holliday et al. (2023), this statement is not true. Sweatman et al. (2024) did not look at the 68-page Supplemental Data, which details error bars and confidence intervals.

5.4. Poorly Dated Platinum Anomalies

Comment Holliday et al. (2023) criticized the inadequate age control in many sites purported to have Pt anomalies. Sweatman et al. (2024) counters, "**…the Pt spike has been found at well-dated sites at White Pond, SC; Wakulla Springs, FL; Parsons Island, MD; Newtonville, NJ; and Flamingo Bay, SC….**"

Reply The dating (and YDB sampling) problems for White Pond, Newtonville, Flamingo Bay (as well as other YDIH sites) were outlined in Table 4 of Holliday et al. (2023), however Sweatman et al. (2024) fails to address any of these issues. Without presenting any contrary dating/sampling evidence, they claim those are "**well-dated sites**".

The Wakulla Springs sediments were purported to have a Pt spike with a date that lies somewhere between 10.3ka - ~20.6ka (PU1) and 11.3ka – 12.5 ka (PU7) as determined by optically stimulated luminescence (Moore et al., 2023a). The studied samples from Parsons Island are directly below an "Ap" (i.e., a plowed zone) (Moore et al 2024, Fig, 9) and according to the text (p, 5) rest on an unconformity. Thus, as noted also for Newtonville, they could be the result of downward mixing from the Ap horizon. Furthermore, the source of the Pt remains largely undetermined (see Section 5.1).

5.5. Inconsistent Dating of Nanodiamond Zones

Comment "**To refute … the YDIH,** [Holliday et al., 2023] **must provide dispositive evidence that the nanodiamond evidence in these two sources is inconsistent …. However, Bement et al.'s (2014) text makes it clear that there is no real inconsistency with Kennett et al.** [(2009a)]."

Reply Following publication of Bement et al. (2014), the Bement group undertook a more detailed study of the purported YDB nanodiamonds. They reexamined the *same processed specimens* from Bement et al. (2014) purported to contain nanodiamonds and *no nanodiamonds were found* (Sexton, 2016). Sexton (2016, abstract) wrote "**…**[an electron microscopy] **grid was prepared from a sediment digest solution shown by Bement et al. (2014) to have a peak abundance of nanodiamonds. No nanodiamonds were observed …. Prepared samples of sediment solution previously confirmed to have nanodiamonds showed no Raman peaks associated with diamonds …**" and "**I examined 12 additional samples collected at the same time as those reported by Bement et al. (2014) but not analyzed for nanodiamond content …** [including those across] **the Younger Dryas Boundary …. No nanodiamonds were found ….**" Further studies of Bull Creek sediments by the Bement group also appear not to have found any nanodiamonds (Sluder, 2023; Sluder et al., 2024).

The results of Bement et al. (2014) are irreproducible by the same group using the same samples, same techniques, and additional techniques. However, YDIH proponents continue to offer the results of Bement et al. (2014) as reliable YDIH evidence (see Section 13.5).

Kennett et al. (2009a) first purported YDB nanodiamonds at Bull Creek and Bement was a coauthor. Bement et al. (2014) was a more detailed follow up study of Bull Creek. Thus, the results of Kennett et al. (2009a) are also placed into serious question. The discrepancies in the purported nanodiamond containing sediment layers between the two studies further reinforce the unreliability of those studies.

Comment In regard to the discrepancies in purported nanodiamond containing sediment layers, Sweatman et al. (2024) argue, "**Possibly,** [Holliday et al. (2023)] **are confused by typographical errors in Table 1 of Bement et al. (2014) that caused** [Holliday et al. (2023)] **to inappropriately shift a column in Table 5 of** [Holliday et al. (2023)] **upwards by one cell….**"

Reply This is blatantly untrue. Lead author Bement is unaware of a typographical error (Bement, 2024).

5.6. Logical Lapses in Dating and Interpreting Usselo and Finow Soils

Comment "**To refute this aspect of the YDIH,** [Holliday et al. (2023)] **must provide dispositive evidence that the dating of the Usselo or Finow charcoal-rich boundary horizons is inconsistent with the age of the YD impact suggested by Kennett et al. (2015); i.e., they must be well outside of error bounds. This they fail to do.**"

Reply Dispositive evidence was provided long before Kennett et al. (2015), which considered only a small subset of dated Usselo horizons. Kaiser et al. (2009, p 602) present direct radiocarbon dates on charcoal from 18 Usselo/Finow soils. Nine of these have calibrated 95.4% age intervals that precede the YDC and another two postdate the YDC. Less than half of the direct dates plausibly overlap Kennett et al.'s (2015) modeled YDB age.

Kaiser et al. (2009, p 606) (experienced Quaternary soil-stratigraphers) argued that the dating revealed "**…a range of ages from the Allerød (predominant) to the Younger Dryas, with some outliers dating into the Preboreal….**" Their claim is correct and would be an obvious conclusion by any experienced Quaternary stratigrapher. The burden of proof is on YDIH proponents to disprove Kaiser et al. and demonstrate that their dating is proof of synchroneity.

Comment "[Holliday et al.'s (2023)] **misunderstanding can be traced to their incorrect analysis of the uncertainty in radiocarbon data for many Usselo/Finow sites in Kaiser et al. (2009). In particular, they fail to acknowledge that the intrinsic radiocarbon measurement uncertainty is not a good estimator for the true sample age uncertainty. In fact, as already discussed by Sweatman (2021) and shown by** [Sweatman et al.'s unpublished manuscript] **in detail, the radiocarbon data in Kaiser et al. (2009) is consistent with a synchronous event, contrary to** [Holliday et al.'s (2023)] **claims.**"

Reply Sweatman (2021) and Sweatman et al. (2024) provide no clear definition of what they mean by error and uncertainty. Statistical uncertainty in the radiocarbon age of a sample is a product of laboratory measurement error and error calculation procedures, the radiocarbon calibration curve, and decisions made in subsequent age-depth models of the type employed by Kennett et al. (2015) (although see Table 2 regarding problems with this paper). While there is a true age for a sample that is being estimated through radiocarbon dating, there is no true uncertainty. Different uncertainties will emerge from different age-depth models. These models will depend on researcher choices, assumptions, the density of sample dates in a stratigraphic sequence, and the selection of chronological modeling software, of which there are numerous options with different model structures. There is no reason to reject the wide scatter of precise radiocarbon ages presented by Kaiser et al. (2009). If they believe that additional samples and age-depth models with reasonable assumptions would remodel the Usselo/Finow charcoal ages such that they are consistent with Kennett et al.'s YDB age, this must be demonstrated rather than presumed true. We note that this would require dramatic remodeling on the order of centuries or even millennia for many sample ages.

5.7. Improved Dating of Clovis Sites and Clovis Archaeology

Comment "**To refute this aspect of the YDIH,** [Holliday et al. (2023)] **must provide dispositive evidence that Clovis artifacts are routinely found above the YDB.**"

Reply No, Sweatman et al. (2024) must deal with 1) the interpretation of the end of Clovis technology as post-YDB as presented by Waters et al. (2020, fig. 2) and 2) the work of summed probability analysis of radiocarbon dates from across North America by Buchanan et al. (2022) demonstrating an overlap of the two artifact traditions by as much as 200 years, discrediting the notion of an abrupt cultural termination at the YDB.

Further, a consensus shows that "Northeast fluted artifacts," very similar in morphology to Clovis and likely derived from it, arrived in the recently deglaciated northeastern U.S. at about the YDB and spread out through the YDC (Miller and Gingerich, 2013; Miller et al., 2013; Lothrop et al. 2016; Ellis and Lothrop, 2019). Sweatman et al (2024) must demonstrate rather than merely assert that: 1) the end of Clovis artifact manufacture was due to a catastrophe and 2) the work of the other researchers is faulty.

5.8. Radiocarbon Simulations of the YDB

Comment "**…**[Holliday et al. (2023)] **rely on Jorgeson et al. (2020), who, based on a Monte Carlo simulation, claim that the YDB is likely not synchronous. However, Sweatman (2021) already described many reasons why Jorgeson et al.'s models are inadequate, and** [Sweatman et al.'s unpublished manuscript] **show that** [Holliday et al. (2023)] **do not adequately address these reasons. Indeed, through line-by-line rebuttal of this section,** [Sweatman et al.'s unpublished manuscript] **argue that Jorgeson et al.'s (2020) research program is not even sensible.**"

Reply Both Holliday et al. (2023) and Jorgeson et al. (2022) provided extensive point-by-point accounts of logical lapses and misunderstandings that characterize Sweatman's (2021) arguments against the simulations in Jorgeson et al. As these lapses and misunderstandings are not addressed by Sweatman et al. (2024), we refer readers back to those publications.

6. Misinterpreted Black Mats

Comment "[Holliday et al. (2023)] criticize **… contradictory statements by YDIH proponents regarding the YD black mat … the YD black mat is generally a good guide to the position of the YDB where it exists.**"

Reply Rather than addressing the contradictory claims that the impact debris layer is the black mat (e.g., Mahaney et al., 2013, 2017, 2022, 2024; Israde-Alcántara et al., 2018; Wolbach et al., 2018b; Firestone 2020) or at its base (e.g., Firestone et al., 2007, 2010; Israde-Alcántara et al., 2012; Bunch et al., 2012; Moore et al., 2017; Pino et al., 2019), they focuses on its base and make a vague claim that it is "**… a good guide to the position of the YDB….**" However, it is not (see Section 3.1).

Nevertheless, when dating is unavailable or not well constrained and the black mat is present, impact proponents have assumed the black mat or its base is the YDB, see Holliday et al. (2023, Table 4).

Comment "**…**[T]**ext in Haynes (2008) make it clear that he considers it likely that the base of the YD black mat is synchronous at many locations and consistent with the suggested date of the YD impact. [Holliday et al. (2023)] cite Mandel (2008) and Quade et al. (1998) to refute this view, but few radiocarbon samples in those works correspond to the base of the YD black mat where the YDB is often found….**"

Reply Their comment mischaracterizes what Holliday et al. (2023) state and what Quade et al (1998) and Mandel (2008) (along with Holliday, 1995 and Pigati et al., 2012) document. There are black mats of multiple age ranges and black mats dating to the YD are highly localized. Organic-rich strata (as described by Haynes, 2008) are well documented to be of YD age, YD and older, YD and younger, spanning pre- to post-YD or, more commonly, not documented at all for the YD. Whether or not Mandel (2008) and Quade et al. (1998) (or the other investigators) dated the base of their black mats, their dating clearly shows that the ages of those deposits vary significantly.

7. Multifarious YDB Impact Scenarios

Comment "**YDIH opponents have … criticized the hypothesis because the model … has changed as new evidence has come to light. But such changes are a mark of good science.**"

Reply As outlined in Holliday et al. (2023) the model/scenario proposed for the YDIH constantly evolves or differs from one YDIH publication to another. Each new version is claimed supported by the same set of evidence and some times additional evidence is purported. However, that growing list of purported evidence is disjointed and contradictory with regard to any single impact model/scenario. There is no YDIH impact model/scenario that is consistent with all the evidence claimed to support it (see Sections 8.1 and 13.7).

Comment "[Holliday et al. 2023] **speculate that the Cape York meteorite is likely to be the source of the GISP2 platinum abundance despite there being no evidence of any crater.**"

Reply That is a rather strange criticism for Sweatman et al. (2024) to make given there is no evidence of a YDB crater (see Section 8.1) associated with the proposed impactor. An impactor that has no credible evidence of existing as opposed to the Cape York meteorite where least 58 tons of specimens were collected (Buchwald, 1975).

8.1. Proposed YDIH Craters

Comment "**…**[A]**bsence of the Chicxulub crater during the 1980s did not discourage scientist from pursuing … evidence of a K-Pg impact….**"

Reply A 66 million year crater, even a huge (≈180 km) one, would be difficult to locate due to significant erosion and geological resurfacing. However, a large 12 ka crater should be relatively pristine today and far easier to discover. While Sweatman et al. (2024) attempts to brush aside the crater issue in writing, **"…absence of a crater of YD age in no way invalidates the YDIH…"**, the absence of a YDB-aged crater is a serious problem for purported YDIH evidence that require a surface impact to explain. For example, microspherules are currently used as major evidence for the YDIH. Sweatman (2021, p1) wrote, "**Elemental analysis shows most microspherules are consistent with a terrestrial source….**", i.e. a surface impact.

Sweatman et al. (2024) emphatically wrote in defending the lack of a crater, **"YDIH proposes widespread airburst that do not require a crater…."** However, this is in contradiction to Sweatman's (2021, p2) review of the YDIH literature that specifically clarified, **"The YDIH *explicitly* claims the impact event was caused by one or more low density *ET objects falling onto* the Laurentide Ice sheet** [emphasis added].**"** Both statements are inconsistent with Sweatman (2021) and Sweatman et al. (2024) claims that microspherules (of terrestrial source) are evidence for the YDIH (see Section 13.7).

Comment "**…**[T]**he absence of a crater of YD age in no way invalidates the YDIH….**"

Reply Throughout the YDIH literature the proponents offer evidence for various particulate "impact indicators" across multiple continents (e.g., microspherules, melt glass, claimed shock quartz, PGEs, fullerenes with ET He, etc.). However, to have a multi-continent "event bed" (resulting from any impact) the requires an impactor likely larger than 5 km in diameter and a surface impact that would result in a crater on the order of 80 km in diameter (see also Section 13.7). Such a dramatic event at ~12,850 cal years old should be well preserved and obvious in an array of geomorphic and biologic systems but are not. Smaller impacts or airbursts do not create marker beds across the planet.

Comment "[Holliday et al., 2023] **refer to the discovery of the Hiawatha crater … thought** [by YDIH proponents] **to be of possible YD age…**" and that it's actual 58 million year age **"…leaves the YDIH in the same position as the Alvarez Theory… during the 1980s…."**

Reply This is not correct. YDIH proponents widely speculated with firm conviction that the Hiawatha crater was of YDB age while its age was clearly not yet established (see Voosen, 2018). Powell (2020, p 109-117) has a chapter of his YDIH book on the Hiawatha crater and criticizes the peer-review of Kjær et al. (2018) as the reason those authors did not publish claims the YDB impact formed Hiawatha. Powell (2022, p 33) wrote, "**Hiawatha Crater in Greenland *could be of YD* age *but has not yet been directly dated*** [emphasis added]**.**" Sweatman (2021, p 18) wrote, "**Clearly, this crater is a candidate YD-age impact structure.**" Sweatman then wildly speculates a second not-yet-confirmed crater (MacGregor et al., 2019) is a "**twin of Hiawatha**" (contrary to conclusions of MacGregor et al.) and formed by the YDB impact. Even after the Hiawatha Crater was dated to ~58 million years old (Kenny et al., 2022), West wildly speculated "**While these dating studies per se offer little support for a YDB age, certain aspects–for example, warmth of crater and stratified ice over chaotic ice – *keep open the possibility of it being so.* The features indicating a young age for the Hiawatha Crater, seemingly incompatible with a Paleocene age, render the postglacial conformable ice column (11.7 ka) over disturbed ice, and other characteristics described above, *prime evidence for the elusive YDB impact crater*** [emphasis added]" (Mahaney et al., 2024, p 4).

Alvarez et al. did not make any speculative claims of possible K-Pg craters. This distinctly sets apart the two research groups, and is highly relevant to the credibility of their respective results.

9.1. Misperception of global charcoal as evidence of impact

Comment "**This section of [**Holliday et al. (2023)**] focuses on the interpretation of wildfire evidence in Marlon et al. (2009)….**"

Reply This section also focuses on Sweatman's (2021) complete misapprehension of the data analyses in Power et al. (2008).

Comment "…[W]**ildfire evidence in Marlon et al. (2009), especially Figure 1C, … shows a peak in charcoal abundance centered on the YD onset based on 35 North American lacustrine charcoal records. A similar peak in Figure 3D of Wolbach et al. (2018a) based on 65 such records is centered at 12,900 cal BP, which is close to the YD onset considering typical radiocarbon uncertainty ranges for this time period.**"

Reply We stand by our correction of Sweatman's (2021) misquotation of Marlon et al. (2009) and reassert that the analyses in Marlon et al. (2009) and Wolbach et al. (2018a) are consistent and show the response of biomass burning to warming at the end of the "Inter-Allerød Cold Period" (IACP, now referred to as GI-1b), i.e. before the beginning of the YD/GS-1.

9.2. Misinterpretation of the NGRIP ammonium-ion record

Comment "[Holliday et al. (2023)] **claim that significant peaks in the background ammonium ion concentration at the YD onset evident in the NGRIP ice core record (see Figures 4b and 4c of Fischer et al. (2015)) contradict the YDIH: 'If anything, the NGRIP NH4+ record offers strong evidence against a biomass-burning peak at the beginning of the YD/GS-1.' But their argument defies logic. To reach their conclusion,** [Holliday et al. (2023)] **prefer to focus on frequency data for local wildfires.**"

Reply The logic that is defied here is not stated and so remains unknown. The authors continue the tradition among YDIH proponents of obstinately misinterpreting the ice-core ammonium record. As is implicit in the title of Fischer et al. (2015) "**Millennial changes in North American wildfire and soil activity over the last glacial cycle**" there are two components in the ammonium record: 1) a slowly varying "background" component contributed by soil and biomass emissions, and a "peak" component contributed by North American wildfires. This decomposition is supported by shallow (post 1960 CE) firn cores (Kjær et al., 2022), and the overall structure of the NGRIP ammonium record has been replicated for the NEEM ice core by Erhardt et al. (2022), see also Schüpbach et al. (2018). The peak frequency of North American fires clearly *decreases* at the beginning of the YDC.

The authors evidently think that the fire-event frequency data from the ice cores represent "local" wildfires. This misapprehension probably arises because high-resolution charcoal records from lakes can typically be decomposed into a background component representing regional fires, and a local component representing fires in the watershed of the lake, see Marlon et al. (2009) for examples.

9.3. Miscellaneous Wildfire Misapprehensions

Comment "**To refute the YDIH,** [Holliday et al., 2023] **must provide dispositive evidence that indicators of extensive wildfires near the YD onset are either (i) outside reasonable** [dating] **error bounds, or (ii) not caused by an ET impact….**"

Reply This is an example of the spurious logic that underlies a fundamental failure of the YDIH proponents reasoning. Wildfires are common and evidence of them is ubiquitous in sediments. To prove the YDIH it is necessary to provide wildfire evidence that *both* dates to the YDB and *was* created by an ET impact. The burden of proof of the YDIH is on the proponents. Holliday et al. (2023) demonstrate the YDIH proponents do not provide sufficient proof of these.

10. Purported YDIH Evidence of Impact: Spherules/Microspherules

Comment "**To refute … the YDIH,** [Holliday et al. 2023] **must provide dispositive evidence that the YDB microspherules are not the result of an ET impact.**"

Reply This is another example of spurious logic. Microspherules are ubiquitous in sediments. To prove the YDIH it is necessary to provide evidence that the purported YDB microspherules *both* date to the YDB *were* created by an impact. Holliday et al. (2023) show that the YDIH proponents do not provide sufficient proof of these.

Comment Complain that we fail **"…to note that** [microspherules] **have chemistry and surface textures that are evidence of an ET origin, as reported in numerous** [YDIH proponent] **articles….**"

Reply We fail to note surface textures and nonchrondritic chemistry as evidence of impact because they cannot be used as diagnostic indicators of ET impactors (see French and Koeberl, 2010).

Comment Criticize French and Koeberl's (2010) assertion, **"…microspherules alone cannot be used to confirm an ET impact…"** and point out that they state, "[t]**here are rare exceptions: inclusions of lechatelierite … establish an impact origin…**" and claim, **"Lechatelierite has been found at several YDB sites…."**

Reply Sweatman et al. (2024) ignore our point that there is mounting evidence that the use of lechatelierite as an impact indicator is problematic. Lechatelierite is present in anthropogenic spherules (Marini and Raukas, 2009), in non-impact frictionites/pseudotachylytes (Masch et al., 1985; Lin 1994; Sanders et al., 2020; Tropper et al., 2021) and can form by lightning strikes. Through lightning discharges, lechatelierite could also be in volcanic spherules (e.g. see, Genareau et al., 2015, 2019; Wadsworth et al., 2017; Kletetschka et al., 2017, 2018), contrary to Bunch et al. (2012, p E1904). Various materials can be misidentified as lechatelierite if insufficient microanalysis is performed, as is commonly the case in YDIH papers (see our criticisms of their characterization of carbon spherules and purported nanodiamonds).

11. Purported YDIH Evidence of Impact: Platinum Group Elements

Comment "…[A]**nalysis of the *relative PGE abundances in the YDB does not clearly indicate their origin*** [emphasis added]**. Instead, the fact that the platinum abundance in the YDB is accompanied by other impact proxies, such as meltglass, nanodiamonds, and microspherules with clear impact characteristics, shows it almost certainly has an ET origin."**

Reply The elevated levels of Pt in purported YDB sediments are relatively small. Any contribution from an ET impactor, if present, would be subject to extreme dilution by terrestrial sediments and extremely difficult to identify. Furthermore, measurements of accompanying Os isotopic ratios in YDB sediments do not support a meteoritic source

(Paquay et al. 2009; Wu et al., 2009, 2011, 2013), and instead suggest a volcanic source at several sites (Sun et al., 2020, 2021). We agree that the source of the PGE abundances purported in YDB sediments is largely undetermined.

Also undetermined is the source of ubiquitous magnetic microspherules. Fungi are the source of most of the ubiquitous carbon spherules. The source of any non-sclerotia carbon spherules, if they exist, remains undetermined. The nanodiamonds are misidentified ubiquitous minerals. Finding sediments with Pt (or PGEs) of undetermined source along with ubiquitous microspherules, ubiquitous sclerotia, and ubiquitous minerals is not at all diagnostic of a possible impact. The source of the so-called melt glass is undetermined.

Comment **"…Petaev et al. (2013) interpret the GISP2 Pt peak as evidence of an ET impact."**

Reply That is correct, but not the YDIH impact. First, the Pt peak in the Greenland ice is not synchronous with the YD/GS-1 onset (see, Section 5.1). Second, Petaev et al. (2013) measured extremely high Pt/Ir and Pt/Al ratios that suggest a very specific ET source like a highly differentiated iron meteorite. For it to distribute PGEs across the hemisphere (as claimed by YDIH proponents) would require a large mass and "**…complete disintegration of such a large iron meteorite during its atmospheric passage seems unlikely, the event is expected to form a crater of a few kilometers in diameter. No such crater at YDB has been found so far**" (Petaev et al., 2013, p 121918). Petaev now attributes the Pt peak to a small local event, probably the one associated with the large shower of Greenland's Cape York meteorites (Buchwald, 1975).

12. Purported YDIH Evidence of Impact: Nanodiamonds

Comment "**…**[T]**o refute evidence of nanodiamonds at the YDB,** [Holliday et al., 2023] **would have to present a plausible alternative origin for them."**

Reply This is another example of spurious and confused logic. This argument assumes the nanoparticles are nanodiamonds and thus to refute their presence (we assume they actually mean their impact origin), one must explain a non-impact origin for the assumed nanodiamonds.

12.1. Cubic Nanodiamonds

Comment **"Cubic nanodiamonds occur in YDB at a multitude of sites across four continents, far too many to be put down to misidentification."**

Reply Rather than addressing the numerous issues described in detail by Holliday et al. (2023) and Daulton et al. (2017a) on the lack of proper nano/micro-characterization of the structure and chemistry of the nanoparticles in question and rather than presenting credible evidence identifying those nanoparticles as diamond, they instead simply argue that since YDIH proponents purport them across four continents, they must be cubic diamonds.

12.2. Hexagonal Nanodiamonds (Lonsdaleite)

Comment "**…**[Holliday et al. (2023)] **dispute the identification of lonsdaleite …. However, Kinzie et al. (2014) already stated that 'we consider the identification of lonsdaleite to be provisional pending further work.' "**

Reply Despite evidence of misidentification of lonsdaleite (Daulton et al., 2010, 2017a, 2017b), despite confirmation of graphene/graphane aggregates that resemble lonsdaleite are present in purported YDB sediments (Madden et al., 2012; van Hoesel et al., 2012; Kinzie et al., 2014; Bement et al., 2014; van Hoesel, 2014), and despite Kinzie et al. (2014, p 491) admitting they lack conclusive evidence for their identification of lonsdaleite, Sweatman (2021) clearly promote the nonprovisional and definitive identification. Sweatman (2021) wrote (p 9), **"[**Israde-Alcántara et al. (2012)**]** [Israde-Alcántara is a coauthor of Kinzie et al. (2014) where lonsdaleite at Lake Cuitzeo was also purported] **also found abundant nanodiamonds at the YDB at Lake Cuitzeo, Mexico. … Crystal structure, bonding and elemental analysis performed with HRTEM, STEM, FFT, EDS, SAD, and EELS indicate the presence of abundant n-diamond with smaller amounts of i-diamond and Lonsdaleite"**; (p 11), **"…it is highly likely that at least some of the nanodiamonds presented by Kinzie et al. (2014) are Lonsdaleite or Lonsdaleite-like."** Kinzie et al.'s (2014) inconclusive identification of lonsdaleite also includes particles first purported as lonsdaleite by Kennett et al. (2009b). However Powell (2022, p 20) wrote, "**Kennett et al.** [(2009b)] **reported, 'shock-synthesized hexagonal nanodiamonds (lonsdaleite) in YDB sediments'**" and clearly does not state that this identification is provisional "**pending further work**."

Holliday et al. (2023) discuss the evidence that the nanoparticles purported as lonsdaleite by Kinzie et al. (2014) and others are graphene/graphane aggregates. Sweatman et al. (2024) presents no new contrary evidence on our identification and those offered by Sweatman (2021) are refuted by Holliday et al. (2023).

The current use of the term "lonsdaleite-like" rather that lonsdaleite by Kinzie et al. (2014), Kennett et al. (2015, supplemental), LeCompte et al. (2018), Sweatman (2021), and Powell (2022) is effectively a retraction of sorts and admission that these purported YDB grains are graphene/graphane aggregates since those aggregates are "lonsdaleite-like".

Comment "**Kinzie et al. (2014) show clearly that the supposed lonsdaleite is unlikely to be graphene or graphane,** [Holliday et al., 2023)] **argue that it 'could still be a mix of graphane and graphane….'** "

Reply Sweatman et al. (2024) misquotes Holliday et al. (2023); we made no such statement. Our conclusions are not qualified by the word "could". Further, Sweatman (2021) egregiously misstates the diffraction data and analysis of Daulton et al. (2010, 2017a). Daulton et al. (2010) decisively wrote, "**We demonstrate that previous studies misidentified graphene/graphane-oxide aggregates as hexagonal diamond and likely misidentified graphene as cubic diamond.**" Holliday et al. (2023, p 48) wrote, "**…Daulton et al. (2017a, p 15)** [stated], **'there are many missing lonsdaleite reflections.' The set of missing reflections indicate the grain cannot be lonsdaleite unless a highly fortuitous and improbable texturing geometry is present**" and "[d]**espite calibrating the pattern with the initial assumption that the diffraction lines were from lonsdaleite, the diffraction lines more closely matched that of graphene/graphane.**" Kinzie et al. (2014) present no convincing data demonstrating those grains in question cannot be graphene/graphane and their identification as lonsdaleite is inconsistent or inconclusive.

12.3. Controversial 'n-Diamond' and 'i-Carbon'

Comment Claim 'n-diamond' and 'i-carbon' were not misidentified.

Reply Holliday et al. (2023) and Daulton et al. (2017a) present strong evidence that copper and copper oxides in sediments (and carbon spherules) were misidentified as 'n-diamond' and 'i-carbon". Furthermore, in processing sediment samples at Bull Creek, Sluder (2023, abstract) reported, "**…copper nanoparticles persisted throughout soil digestion methods….**" Sweatman et al. (2024) presents no contrary and conclusive evidence that YDB nanoparticles are not ubiquitous copper and copper oxides or other minerals, but are instead diamond, 'n-diamond', or 'i-carbon'.

12.4. Nanodiamond Host Grains – Carbon Spherules

Comment **"Kimbel et al. (2008), Kinzie et al. (2014), and Wolbach et al. (2018b) mention lab experiments using carbon spherules created from tree sap in the lab. Nanodiamonds were produced in these carbon spherules through a high-temperature, low-oxygen process 'identical to the commercial process for producing activated charcoal.' "**

Reply They are referring to dubious experiments originally mentioned in a short abstract (Kimbel et al., 2008) and described in an associated, highly questionable and abandoned patent application (West and Kennett, 2011).

Holliday et al. (2023) and many others (see references in Holliday et al., 2023) demonstrate sclerotia are ubiquitous in sediments, and further, are often at elevated concentrations in YD-aged sediments due to their concentration in those sediments arising from YD environmental changes. Curiously, and very revealing is that in all the YDIH papers that discuss finding carbon spherules, never do they also mention finding any of the ubiquitous sclerotia and how they distinguished the claimed impact-induced wildfire formed carbon spherules from the morphologically identical (based on their descriptions) sclerotia.

To claim any carbon spherule is of impact-induced wildfire origin, requires concrete micro-characterization evidence, which Sweatman et al. (2024) and others do not provide. The claim that carbon spherules contain nanodiamonds, and thus of impact-induced wildfire origin, is without merit as detailed by Holliday et al. (2023) and Daulton et al. (2017a) by the numerous issues with the identification of the nanoparticles within the carbon spherules. Further, Holliday et al. (2023) describes in detail the problems with the YDIH proponents' carbon dating of the spherules.

Comment **"There is no evidence that fungi or any other terrestrial mechanism can produce nanodiamonds in sclerotia…."**

Reply This is undoubtedly because there is no evidence of nanodiamonds in sclerotia.

12.5. 'Nanodiamond' Misidentifications

Comment Claim nanodiamonds were not misidentified.

Reply Holliday et al. (2023) and Daulton et al. (2017a) detail numerous issues with the identification and quantification of the nanoparticles. Sweatman et al. (2024) presents no specific microcharacterization evidence that would definitively support their phase identifications. The only specific evidence discussed by Sweatman et al. (2024) is their statement, **"TEM-EDS analyses confirm that these crystals are > 99 wt% carbon…."** However, they present no conclusive data in support of this claim. Even if such data is presented, that show some nanoparticles are carbon, this is not sufficient to identify them as diamond.

12.6. 'Nanodiamond' Concentration Spike at YDB

Comment Claim our statement, **"published nanodiamond concentrations from purported YDB sediments are completely unreliable and scientifically meaningless"** is **"spurious."**

Reply Crushed carbon spherules and acid-dissolution residues of sediments are typically not pure isolates and contain a multitude of different minerals. Kinzie et al. (2014, p 480) state that most nanoparticles in any of their residues are not diamond, thus most of their nanoparticles must have yielded some microscopy data, like diffraction, that was clearly inconsistent with diamond.

The greatest limitation in quantification of nanodiamond in non-pure isolates from sediments and carbon spherules using electron microscopy is that *detailed laborious measurements* must be performed on *each individual* nanoparticle in order to correctly identify whether it is diamond or not. For example, single diffraction patterns, single high-resolution images, and elemental composition alone (or at times in combination) cannot uniquely identify the phase. A complete array of nanoanalysis techniques performed on any given nanoparticle must yield results all consistent with diamond in order to identify it as diamond.

The critical failure of Kinzie et al. (2014) is they did not perform a comprehensive nanoanalysis to identify any given nanoparticle for quantification. Instead, Kinzie et al. (2014) describe in their supplemental materials (p 9), **"… for the purpose of estimating abundances, *we assumed that all rounded particles were NDs* [*nanodiamonds*]. *… This estimation procedure focused solely on the presence or absence of rounded particles*** [emphasis added]**.**" Kinzie et al. (2014) measured projected areal densities of "rounded particles," not necessarily nanodiamonds, and they certainly did not measure modal mass abundances. Furthermore, purported nanodiamond concentrations in other YDIH papers published by coauthors of Kinzie et al. (2014) that used similar techniques are drawn into serious question. See also Section 12.4.

Of all YDB sites, three should potentially offer the most compelling concentration profile measurements: two with the highest purported nanodiamond concentrations (Bull Creek, Oklahoma and Lubbock Lake, Texas), and one with the most detailed concentration measurements (Arlington Canyon, California). Instead, the results illustrate that those measurements are unreliable (Holliday et al. 2023). There is also indication that the purported presence of nanodiamonds in YDB-aged Greenland ice (Kurbatov et al., 2010) could not be replicated by that group (Section 13.4).

12.7. Redefinitions and 'Nanodiamond'-Related Markers

Comment "[Holliday et al., 2023] **challenge the definition, or redefinition of carbon spherules and carbon elongates containing nanodiamonds. However, they do not dispute the presence of nanodiamond per se in these materials, and therefore, this does not refute the presence of nanodiamonds in the YDB…."**

Reply Regardless of their original classification and subsequent reclassification, Sweatman et al. (2024) ignores, and fails to address, that carbon forms with different morphological shapes have distinctly different purported concentration profiles in the sediments and this is inconsistent with the claim they were both formed by impact-generated wildfires (see Holliday et al., 2023, p 55). Holliday et al. (2023) in great detail disputes the identification of nanodiamond by YDIH proponents (and thus their presence in YDB sediments, carbon spherules, and carbon elongates).

12.8. Diamondoids

Comment **"This section does not concern the identification of nanodiamonds in the YDB, so it is irrelevant and does not refute the YDIH."**

Reply Kinzie et al. (2014, p 487) wrote, **"…the residue between NDs appears to consist of diamond-like nanocrystals …. It is possible that these are diamondoids…"** and argued **"**[b]**ecause both n-diamonds and diamondoids have been found in petroleum deposits related to the K-Pg, one might speculate that something similar happened during the YDB impact…."** Bunch and other coauthors of Kinzie et al. (2014) wrote, "[Kinzie et al. (2014)] **concluded that impact-related nanodiamonds and diamond-like carbon (DLC or diamonoids** [sic]**) are produced from … carbon sources … that were pyrolyzed during high-temperature, high-pressure airburst/impact events"** (Bunch et al., 2021, p 11).

Thus, our discussion of purported diamondoids in YDB sediments is relevant and places into serious question a line of evidence purported to support the YDIH. It demonstrates one of many instances where highly speculative and unsupported claims are offered as evidence of the YDIH.

13.1. Carolina Bays and High Plains Playas

Comment "[Holliday et al. (2023)] **spuriously claim that YDIH proponents are somehow committing "scientific malfeasance" because** [Firestone et al. (2006) and Firestone (2009)] **merely cite** [Holliday's] **and other people's work…."**

Reply Firestone et al. (2006, p 216) claim that Holliday et al. (1996) recovered dates of 16,000 to 20,000 years ago from beneath playa basins and so they must be younger, but what the paper says is that most of the basin fill spans the past 15,000 years or more and thus most of the basins are older. They have no link to a purported YDB impact.

13.2. Fullerenes with ET Helium

Comment **"…**[Fullerenes with ET helium] **were reported in the YD deposits by Firestone et al. (2007), but evidently, no other researchers have attempted to replicate the finding, for which there can be many reasons, including that the original finding, being peer-reviewed, is accepted."**

Reply Another reason, given by I. Gilmour who was quoted in an article by Kerr (2003, p 1316), "[t]**here's is not a great incentive for people to chase things and not find them.**"

Purports of YDB fullerenes with ET helium were almost immediately questioned and soon after rejected. These measurements were undoubtedly performed by Firestone et al. (2007) coauthors L. Becker, T.E. Bunch, and R. Poreda. Becker's group had also purported fullerenes in Allende and Murchison chondrites (Becker et al., 1994, 1995; 1999, 2000; Becker and Bunch, 1997). They also purported fullerenes containing ET helium in those chondrites, the KT boundary (Becker et al., 2000), the Sudbury impact structure (Becker et al., 1996) and in the Permian-Triassic Boundary (Becker et al., 2001; Poreda and Becker, 2003). But, those results, which drew more attention than those of the YDIH, all came under question as they could not be replicated (e.g., Heymann, 1995, 1997). Kerr (2003, p 1316) wrote, "**No one but Becker and Poreda has identified fullerenes in meteorites, despite considerable effort, most of it unpublished.**"

Heymann (1997, p L114) wrote, "**Several firm conclusions are now possible … Allende studied in this work did not contain any extractable C_{60}, C_{70}, epoxides of C_{60}, or $C_{60}H_2$. … The different results of the searches for C_{60} in the Allende meteorite (**[Tingle et al., 1991]**; De Vries et al. 1993; Becker et al. 1994; Becker et al. 1995; this work) have therefore not been resolved ….**" Latter, Sabbah et al. (2022, p 2) wrote that the Becker group **"…combined chemical extraction to increase the concentration of fullerenes with one-step laser desorption ionization (LDI) mass spectrometry. … However, the level of detection was found to be highly variable between samples of the same meteorite** [(Becker and Bunch, 1997)]**. Moreover, their detection could not be confirmed by other groups …** [Hammond and Zare (2008)] **demonstrated that the detected fullerenes were not intrinsic to the samples but generated by the one-step LDI process used to analyze the samples. A similar conclusion was reached in a recent study of insoluble organic matter in the Paris meteorite by Danger et al. (2020).**"

Gilmour (1998, p 207) wrote, "**Becker et al. (1996) measured a $^3He/^4He$ ratio … in two fullerene-rich residues from Sudbury Onaping formation …** [and] **suggested that the high $^3He/^4He$ is indicative of a presolar source … Heymann was unable to confirm the presence of fullerenes in Onaping formation samples, though this may merely reflect the heterogeneity of this very large rock unit …. The apparent survivability of extraterrestrial fullerenes is also at odds with the lack of extraterrestrial signatures in more refractory phases present in both meteorites and impact-produced rocks, which would presumably be more likely to survive an impact. Studies of several K-T boundary acid-residues have shown no evidence of a presolar noble gas, carbon or nitrogen isotopic signatures….**" With regard to the Permian-Triassic Boundary (PTB) at Graphite Peak Antarctica purported to contain fullerenes with ET He by Poreda and Becker (2003), Farley et al. (2005) wrote **"…there is strong evidence for heating to temperatures far above those required for total ^3He loss, so the presence of extraterrestrial helium there is not easily rationalized."** Within the PTB at Meishan, China purported to contain fullerenes with ET He by Becker et al. (2001), Farley and Mukhopadhyay (2001) were unable to detect ET helium (nor at an equivalent bed at Shangsi, China) and Carrasquillo et al. (2016) were unable to detect fullerenes. Isozaki (2001) questioned the sampling at the Sasayama, Japan site studied by Becker et al. (2001), arguing the PTB horizon is missing and ET He was purported at a layer at least 0.8 m below the PTB. Continuing their search of the PTB, Farley et al. (2005) were unable to detect ET ^3He containing fullerenes at Opal Creek, Canada. Kerr (2008, p 1332), wrote, "**Throughout a half-dozen years of effort, no one** [other than Becker] **has replicated the isolation of fullerenes with helium**."

13.3. More Pseudoscience (Fringe) Evidence and Conjecture

Comment "…[T]**his section … relates to … work published before Firestone et al. (2007) and is … irrelevant to … the YDIH**."

Reply Firestone adopts and merges together most of his YDIH ideas from earlier YDIH publications (Donnelly, 1883; Melton and Schriever, 1933; Sass, 1944; Brakenridge, 1981) making those sources relevant for a comprehensive review that recognizes all the contributions to the topic.

The YDIH of Firestone et al. (2007) is strikingly similar to the YDIH of Donnelly (1883). The speculative book by Donnelly (1883) (p 95-96) claims a comet hit the Earth and created the Great Lakes; (p 94, 97) claims an impact debris layer was deposited across a hemisphere; (p 106) describes a comet airburst; (p 108) describes impact-driven continent-wide wildfires, "**The world is on fire!**"; (p 108) describes catastrophic human population declines, "**…smitten by mighty rocks, they perish by the million…**"; (p 49-50) describes "**…remains of elephant and mastodon found below and in the drift** [the impact debris layer] **in America … These animals were slaughtered outright, and so suddenly that few escaped… the Drift came suddenly upon the world, slaughtering the animals…**"; (p 112) claims a "**…visitation of a comet would, therefore, necessarily eventuate in a glacial age….**" These are essentially the major claims of Firestone et al. (2007).

13.4. Mislaid Greenland Ice Expedition

Comment **"Authors are not required to replicate their own results…."**

Reply This is ridiculous. Reported scientific results *must* be accurate, reliable, and reproducible to be meaningful. Authors *should* be able to replicate their results. There are many indications that authors from Kurbatov et al. (2010) attempted to reproduce the nanodiamond results from Greenland ice with no success. Furthermore, Sexton (2016) reports the failure of the Bement group to find nanodiamonds in the *exact same processed specimens* earlier reported to contain nanodiamonds (Bement et al., 2014), see also Sluder (2023) and Sluder et al., (2024). The non-reproducible Greenland and Bull Creek results emphasize the multitude of problems we describe in the methodologies used by YDIH proponents for the identification and quantification of the purported nanodiamonds.

13.5. Mislaid Contrary Evidence

Comment "[Holliday et al., 2023] **write, 'Proponents of the YDIH fail to report negative or conflicting results…. More troublesome is that YDIH proponents continue to report original results as valid even after failed attempts to reproduce those results.' These are spurious accusations of scientific misconduct with no evidence at all to back them up."**

Reply One egregious example is that of the irreproducible Bull Creek nanodiamond results of Bement et al. (2014), which are presented as YDIH evidence by Carlson and Bement (2017), LeCompte et al. (2018), Wolbach et al. (2018a, 2018b), Napier (2019), West et al. (2020), Wolbach et al. (2020), Sweatman (2021), Powell (2020, 2022a), Bement et al. (2022), Moore et al. (2023b), Silvia et al. (2024), and Sweatman et al. (2024). More YDIH proponent papers can be added to the above list if we include Bement's original Bull Creek results in Kennett et al. (2009a).

Powell (2022, p 21) wrote, "**Bement et al.** [(2014)] **independently replicated the earlier finding of nanodiamonds at the Bull Creek site.**" However, those results were not reproducible (Sexton, 2016), see Sections 5.5 and 13.4. Powell should have been well aware of this point because the results of Sexton (2016) are discussed in the short abstract Daulton et al. (2017b) that Powell (2022) cites. Sexton's results are further discussed in Holliday et al. (2023, p 54). In Sweatman et al.'s (2024) criticism of Holliday et al. (2023), they repeatedly ignore Sexton (2016) and use the discredited results of Kennett et al. (2009a) (Section 5.5), Madden et al. (2012) (see Section 12.5), and Bement et al. (2014) (see Sections 5.5, 12.5, and 12.6) as evidence of the YDIH. They also blame discrepancies in sediment layers purported to contain nanodiamonds at Bull Creek to non-existent typographical errors in Bement et al. (2014) (see Section 5.5), while claiming there is no evidence of "**scientific misconduct**" by YDIH proponents.

Further examples include the purports by Sweatman (2021) and Powell (2022) of lonsdaleite present at the YDB when in fact that identification is inconclusive **"pending further work"** (Kinzie et al., 2014, p 491)**.** Sweatman and Powell are fully aware of this point as demonstrated in Section 12.2 of Sweatman et al. (2024). Further examples are given in Holliday et al. (2023), and Greenland nanodiamonds might represent another important example (Section 13.4).

13.6. Lack of Transparency in YDIH Evidence

Comment "**If** [Holliday et al., 2023] **have convincing evidence to back up these repeated accusations of scientific misconduct, they should pursue them in appropriate academic and government settings…. Every research university has a process for investigating claims of misconduct, as do granting agencies …. Unless and until** [they] **make use of these processes, their scurrilous claims of misconduct have no standing."**

Reply The words "**scientific misconduct**" do not appear in Holliday et al. (2023). We have discovered many data inconsistencies and other contradictions in papers by many YDIH proponents and are professionally obligated to report these discrepancies. We discussed many in Holliday et al. (2023) and continue to document them on PubPeer (see Table 2).

In one case, Kinzie et al. (2014) purported elevated concentrations of nanodiamonds at the YDB and their title stated, "**Nanodiamond-Rich Layer across Three Continents Consistent with Major Cosmic Impact at 12,800 Cal BP**." Their concentration profiles were displayed in Figure 2, whose caption states, "**Abundances of nanodiamonds (NDs; ppb) for 22 Younger Dryas Boundary (YDB) stratigraphic sections….**" There is a major problem in that those are *not* measurements of nanodiamonds (see Section 12.6). Despite all the discussion of elevated abundances nanodiamonds at the YDB in their main text, in their less accessible and less read supplemental materials they state (p 9), **"… for the purpose of estimating abundances, we *assumed* that all rounded particles were NDs. … *This estimation procedure focused solely on the presence or absence of rounded particles*."** The processed samples they studied were not purified diamond isolates and contained a multitude of minerals, of those minerals, they counted the rounded ones. In response, we published Daulton et al. (2017a, 2017b).

In another case, a number of YDIH proponents published work claiming an airburst destroyed Tall el-Hammam (Bunch et al., 2021). It was shown by image-forensics investigators that a number of images in figures were inappropriately manipulated by the authors. We reported this and other issues to the journal editor, and the authors published several corrections (Bunch et al., 2022, 2023). This data manipulation rotated the apparent orientation of images to be consistent with the group's hypothesis. We clearly stated that this apparent rotation could have an innocent explanation, but it is a fact that the images were inappropriately modified. We reported this and other discrepancies to the journal editors, who subsequently published an editorial expression of concern.

13.7. Conspicuously Missing Impact Evidence

Comment "[Holliday et al., 2023] **state, 'Several … specimens that YDIH impact proponents argue are impact markers require for their explanation YDIH impact scenarios where an ET body physically impacts the Earth's surface (Section 7).' If YDIH proponents had found YDB-age diagnostic evidence for a ground impact, then a YD-age impact would naturally be confirmed. As this is not yet the case, then it means only that a ground impact is not yet confirmed…." Then they argue that a bolide airburst, not a surface impact occurred, hence explaining the missing impact evidence in question.**

Reply Microspherules are frequently purported as major evidence for the YDIH. Sweatman (2024) wrote, "**…microspherule evidence alone strongly suggest a widespread cosmic impact event near the Younger Dryas onset.**" In the largest study of microspherules by YDIH proponents, Wittke et al. (2013, p E2094) wrote, **"…we compared spherule compositions with those of >100,000 samples of terrestrial sediments and minerals from across North America, including sedimentary, igneous, and metamorphic rocks … YDB spherules are compositionally similar to surficial sediments and metamorphic rocks, e.g., mudstone, shale, gneiss, schist, and amphibolite … which suggests that *YDB objects formed by the melting of heterogeneous surficial sediments comprised of weathered metamorphic and other similar rocks*** [emphasis added]**."**

They are explicitly purporting evidence of a surface impact. Furthermore, what is the source of the purported "**…10 million tonnes of impact spherules…**" (Wittke et al., 2013, title) of terrestrial composition if not from the rock ejecta of a large crater? However, conspicuously missing from the evidence are any identified impact structures (craters) dating to the YD/GS-1 as well as well-recognized and established impact markers in YDB sediments such as shatter cones, tektites, shocked minerals, and meteoritic fragments of an impactor.

Comment "**The resulting impacts are proposed mainly to have been airbursts… Airbursts do not produce all of the signals of a ground impact, but they nevertheless do produce meltglass, which has been reported in the YDB … and shock-fractured quartz, which has been reported by** [Moore et al. (2023c, 2024)]**….**"

Reply First there is the problem of a Tunguska-type airburst generating the necessary temperatures and pressures to melt rock and shock rock strata. As described in the main text (Section 3.4), the air behind the blast wave only increases by tens of degrees, and is not 'superheated' as claimed by YDIH proponents. High temperatures are only reached at the surface when the hot jet of meteoritic vapor reaches the surface, but the pressures are still too low to generate shock features. Additionally, Lussier et al. (2017, abstract) report that the Trinity nuclear airburst had at the ground surface "**…maximal temperatures in excess of 1500 °C and pressures of <10 GPa, the latter being considerably less than for any natural impact event**."

Their termed "meltglass" is not a diagnostic impact indicator, see Section 4.2. The questionably named "shock-fractured quartz" are quartz grains with microscopic non-planar fractures reported to contain amorphized material. These grains are not dominated by parallel open planar fractures indicative of shock effects. Furthermore, those known from impact structures would be filled with secondary (alteration) material, not any impact-related glass (see French and Koeberl, 2010). On the other hand, conchoidal fractures (that do not follow cleavage planes and can contain amorphous and cryptocrystalline material) are caused by any form of mechanical stresses. For the grains described by Moore et al. (2023c, 2024) it is unclear what process caused the mechanical stresses and the resultant conchoidal fractures shown. They purport the lack of these grains in sediments above and below the YDB. This is similar to purports on the nanodiamonds at the YDB whose measurements were shown to be completely unreliable (Section 12.6). Hence, concentration spikes in both these materials are likely attributed to observation bias as demonstrated in the case of early reports of microspherule spikes at the YDB, where a subsequent blind study produced concentration spikes outside the dated YDB (Holliday et al., 2016).

14. Same Specimens and Specimen Splits Studied by Different Groups

Comment “**This section focuses on the only result yet published that appears to contradict the YDIH** (Holliday et al., 2016).”

Reply This is incorrect. Our section focused on eight studies (Surovell et al., 2009; Haynes et al., 2010; Paquay et al., 2009; Scott et al., 2010, 2017; Daulton et al., 2010, 2017a; Holliday et al., 2016) in which different groups examined the same YDIH proponent’s specimens or splits of YDIH proponent’s specimens and obtained contradictory results. Further, the YDIH proponent group of Bement reexamined their own specimens and obtained contradictory results (Bement et al., 2014; Sexton, 2016, see also Sluder, 2023; Sluder et al., 2024).

15. Unparalleled Promotion of the YDIH Outside of Scientific Literature

Comment Misconstrues our point to be that **“…scientists who have published articles on a topic should not also publish a popular book on the same topic.”**

Reply Many respected scientists have written popular books. We take issue with only those books that have misstatements of facts, selective omission of facts, questionable purported evidence and wildly unsupported speculation that blurs peer-reviewed scientific literature together with imaginative speculation, as is the case with all of the YDIH books (e.g., Firestone et al., 2006, Sweatman, 2019, Powell, 2020).

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