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Comprehensive refutation of the Younger Dryas Impact Hypothesis (YDIH)

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ABSTRACT

A series of publications purport to provide evidence that the Earth was subjected to an extraterrestrial event or events at ~12.9 ka creating an environmental cataclysm and the onset of the Younger Dryas stadial. The varied and sometime conflicting speculations in those publications have become known collectively as the "Younger Dryas Impact Hypothesis" (YDIH). As the YDIH has evolved, it has yet to converge into a hypothesis with a self-consistent scenario involving orbital dynamics, impact physics, geology, geochemistry, paleobotany, paleoclimatology, and anthropology. The YDIH invokes a cosmic event at a moment in time to explain complex processes that varied in space and time around the globe. No craters have been identified that date to the onset of the Younger Dryas. The physical evidence offered in support of an impact is nano to microscopic in scale, e.g., charcoal, carbon spherules, magnetic grains/spherules, nanodiamonds, and Pt minerals to name a few. However, many have critical issues with their identification, measurement, and interpretation. Furthermore, most are associated with terrestrial processes not uniquely associated with impacts or periods of abrupt climate change. Very few sites with high levels of any of the purported indicators have accurate and high-precision dating to 12.9 ka. The identification and quantification of several purported impact indicators is also questionable. The claim that a suite of supposed indicators is unique to that moment is not substantiated with data. There is no obvious evidence of environmental cataclysm at that time in the vast published geomorphic or paleobotanical records. There is no support for the basic premise of the YDIH that human populations were diminished, and individual species of late Pleistocene megafauna became extinct or were diminished due to catastrophe. Evidence and arguments purported to support the YDIH involve flawed methodologies, inappropriate assumptions, questionable conclusions, misstatements of fact, misleading information, unsupported claims, irreproducible observations, logical fallacies, and selected omission of contrary information. In this comprehensive review of the available evidence, we address and draw attention to these critical failings. We demonstrate that research in numerous fields has shown the YDIH should be rejected.

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1. Introduction

The Younger Dryas impact hypothesis (YDIH) is a collection of ideas proposed to explain terminal Pleistocene environmental change across North America and other continents at the onset of the Younger Dryas (YD) stadial and the beginning of the YD Chronozone (YDC) (Section 2). While the specific details of the YDIH vary from publication to publication, the general premise is that at ~12.9 ka[1] North America and other continents were subjected to some sort of extraterrestrial 'event' (either supernova shockwave; meteoritic, cometary, or very low-density object - impact(s); bolide airburst(s); or some combination thereof). The term 'impact' in "YDIH" represents all these possible cosmic events. That event supposedly caused climate changes that define the onset of the YD stadial (see Firestone and Topping, 2001; Firestone et al., 2006, 2007; Kennett et al., 2008a, 2009a; Bunch et al., 2012; Israde-Alcántara et al., 2012; LeCompte et al., 2012; Wittke et al., 2013a; Moore et al., 2017; Kennett et al., 2018; LeCompte et al., 2018; Sweatman, 2021; Powell, 2020, 2022). More significantly, YDIH proponents claim that the proposed impact at the beginning of the Younger Dryas (i.e., the *lower* **"Younger Dryas Boundary** [YDB]") **"triggered an 'impact winter' and the subsequent Younger Dryas (YD) climate episode, biomass burning, late Pleistocene megafaunal extinctions, and human cultural shifts and population declines"** (Wolbach et al., 2018a, abstract), among other claims. A comprehensive and self-consistent statement that describes the YDIH, clarifies confusing/contradictory data, arguments, and interpretations, does not exist.

This paper is an in-depth critical review of the data and interpretations used to both promote the YDIH and counter critics of the YDIH, including recent summary reviews of the hypothesis (Sweatman, 2021; Powell, 2020, 2022). In the following discussion we make liberal use of direct quotes to clarify communication disconnects that seem to characterize the debate and to better make our points.[2] We repeat some of the critiques from previous papers. The reason is obvious, as is apparent throughout this paper. The vast majority of critiques and contradictory data have never been directly addressed by YDIH proponents. Critiques of the YDIH were published by researchers in a broad array of fields regarding reproducibility of results, extinctions, Clovis archaeology, stratigraphy, dating methods, YDC climate change, mineralogy, geochemistry, statistical probability, and impact physics, among other topics. Proponents of the YDIH have argued that such critiques have been addressed, but either provide no citations or when provided, those citations do not adequately address the critiques (see Table 1). For example, Kennett et al. (2015b, p E6723) assert that criticisms that purported YDIH **"impact proxies"** also occur in multiple horizons outside the YDB were **"refuted in detail"** (citing Kennett et al., 2015a; Bunch et al., 2012; LeCompte et al., 2012; Wittke et al., 2013b). Similarly, Sweatman (2021, p 14) falsely asserts that rebuttals to Wolbach et al. (2018a, 2018b) **"were already addressed"** but provides no references regarding those claims. Holliday et al. (2020, table 2) list eleven major claims based around the YDIH that are either partially or completely unaddressed in the YDIH literature. Table 1 summarizes the

Table 1
Papers said to rebut critics of the YDIH according to Sweatman (2021) (following Holliday et al., 2020, table 2).

"Rebuttal" Paper	Substance of "rebuttal"
Bunch et al., 2012	Offers brief comments on the work of Pinter et al. (2011) and Pigati et al. (2012).
LeCompte et al., 2012	Refer to remarks of Bunch et al. (2012) and then devote 4 pages of their paper to a rebuttal of Surovell et al. (2009), focusing only on select aspects of the methods used in that paper (ignoring recovery of microspheres by Surovell et al. and input from A. West). See Section 10.
Wittke et al., 2013a	Briefly dismiss Surovell et al. (2009), Pigati et al. (2012), and Pinter et al. (2011) citing LeCompte et al. (2012) and Bunch et al. (2012).
Kennett et al., 2015a	Devote one sentence in the main text to **"inherent uncertainties"** in the dating along with several critiques of some statistical methods used by Meltzer et al. (2014) but do not address fundamental issues of stratigraphic context and the original dating. Otherwise, their response consists of a few comments in the Supplemental Information (SI) on methods used by Meltzer et al. (for the Arlington Canyon and Murray Springs dating) and, in response to **"questions raised"** by Meltzer et al., provide additional data on the stratigraphy at Sheriden Cave.
Sweatman, 2021, p 15	**"In their criticism of paper 2 by Wolbach et al. (2018b), Holliday et al. (2020) begin by suggesting that large cosmic impacts are not known to generate extensive wildfires and that, in any case, evidence for such wildfires cannot be sought in the charcoal record. These views are self-evidently incorrect and rebutted by Wolbach et al. (2020) in their counter-response."**
comment	Holliday et al. (2020, p 69) write **"The presence of charcoal at the beginning of the YDC** [YDB] **fails to unambiguously support the hypothesis of impact-related fires because there is also a large peak at the end of the YDC."** See Section 9.1.
Wolbach et al. (2020) response to Holliday et al. (2020)	
Wildfires	Holliday et al. (2020, p 69) state **"The presence of charcoal at the … [YDB] fails to unambiguously support the hypothesis of impact-related fires because there is also a large peak at the end of the YDC."**
Wolbach et al., p 96	**"We agree that the presence of charcoal in the YDB sediments is not sufficient by itself to show causation. Nevertheless, the charcoal evidence presented in Wolbach et al. (2018a, 2018b) indicates a significant peak at the Younger Dryas (YD) onset, when many rapid changes occurred simultaneously across a large geographic area, coeval with deposition of impact-related proxies."**
comment	Unambiguous impact markers (craters, shatter cones, planar deformation features, meteorite fragments) are unknown at the YDB. There are otherwise no unambiguous impact proxies (Sections 9, 10, 11, 12, and 13). Wolbach et al. (2020) fail to note that Holliday et al. (2020, p 90) also commented that **"The landscape of North America from the post-LGM terminal Pleistocene into the early Holocene underwent rapid reorganization…"** See also Section 13.7. Dramatic climate changes can produce much fuel for burning (e.g., Usselo soil). There is no reason to invoke an ET explanation for one particular charcoal peak.
Wolbach et al., p 96	**"Holliday et al. (2020) also claimed in their introduction that 'the exceptionalism claimed for many of the key data points is the result of exaggerations.' This is a puzzling claim because we report peaks in charcoal and biomass-burning proxies that are in the 99th percentile by size. Surely that percentile is exceptional enough."**
comment	As we discuss in Section 9.2, in the case of the NGRIP ammonium record (Fischer et al., 2015), there are 950 such values above the 95-th percentile, which would

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[1] Conventions used in this paper for numerical expressions of geologic time: Ages: e.g., the beginning of the Younger Dryas Chronozone 12.9 ka (kilo annum) or 12,846 yr BP; Durations: 1200 yr or 1.2 kyr; ^{14}C ages: 11,200 ^{14}C yr BP or 12.9 cal ka BP; Ice-core ages: 12,896 yr [b2k, GICC05], 12,846 yr [BP 1950, GICC05]. Where further clarification is needed, we use square brackets in the editorial-comment sense, to indicate the chronology name and reference date. All age references in quoted material are verbatim.

[2] To avoid potential confusion, citations that appear within quoted text, which are also cited in this review, are modified within brackets to match our reference list when they differ in date enumeration or reference style. Also references to our tables and figures are capitalized while cited ones are not.

Table 1 (*continued*)

"Rebuttal" Paper	Substance of "rebuttal"
	yield an average rate of occurrence of about one every 1000 yrs.
Dating	Wolbach et al. (2020, p 97) claimed **"Impact-related proxies have been found in sediment samples from 23 Bayesian-dated sites in nine countries on three continents, as reported by Kennett et al. [2015a]; table 1)."** (apparently meaning fig. 2; there are no tables in this paper)
comment	Besides the issues with **"impact proxies,"** this statement ignores the wide array of problems with most of the dating (Meltzer et al., 2014) and the broad standard deviations. See Section 5.
Wolbach et al., p 97	**"Kennett et al. [2015a] simply used marker layers for stratigraphic correlation and never claimed that these definitely are YDB sites. Instead, they proposed that the observed impact-related proxies may date to the YD onset."**
comment	Kennett et al. (2015a, p E4351) state **"Nine other proxy rich sites currently lack sufficient dating for robust Bayesian analysis. *Even so*, the stratigraphic context of a proxy-rich layer or samples at these *sites supports a YDB age*"** (emphasis added) and in the Supporting Information (p 34) wrote that even though **"dating is insufficient for robust Bayesian analysis, a wide range of evidence indicates that all nine are YDB sites"** and **"because these nine sites contain the same abundance peaks in proxies that are found at well-dated YDB sites, we have proposed that they are of YDB age."** Issues of circular reasoning and related issues in dating and other aspects of the YDIH are documented in Table 2 and Sections 5 and 6.
Sampling	Holliday et al. (2020, p 75) note **"A significant issue here, as it is with most of the YDIH literature, is that most sampling and data are only from within and around the presumed YDB zone."** Wolbach et al. (2020, p 97) respond **"This is inaccurate. Impact-related proxies have been found in sediment samples from 23 Bayesian-dated sites in nine countries on three continents…"**
comment	Ignoring the fact that Bayesian statistics don't date anything, this comment completely misses the point about sampling. To date, no section with thousands of years of continuous deposition has been subjected to close interval sampling to determine if suites of purported impact proxies are unique to the YDB. See Section 4, Table 3, Endnote 4.
Nanodiamonds	Holliday et al. (2020, p 75) remark **"Data from several studies show that claimed impact indicators are found in deposits of a wide age range… For example, nanodiamonds were misidentified and/or the data are not reproducible (Daulton et al., 2017a) or there are multiple nanodiamond peaks over the past 13,000 y."**
Wolbach et al., p 97	**"This is inaccurate".**
Comment	See Section 4.1.
Wolbach et al., p 98	**"Holliday et al misrepresent the results of Bement et al. (2014)"** at the Bull Creek site.
comment	The record at that site is exactly as described by Holliday et al. (2020). Moreover, the results of the nanodiamond analyses at that site could not be reproduced. See Sections 5.5 and 12.6, Endnote 9.
Greenland ice-cores Wolbach et al., p 98	Referring to Holocene-age peaks in charcoal **"Holliday et al. (2020) claim these peaks are not due "to 'anthropogenic burning' (which none of these articles claim)." This claim is inaccurate, even though two coauthors of Holliday et al. (2020) are also coauthors of Power et al. (2008), who referred to end-Pleistocene/Holocene anthropogenic influences on wildfires multiple times."** Wolbach et al. (2020, p 98) also state **"Daniau et al. (2010), who also share a coauthor with Holliday et al., wrote, 'The interactions between climate, vegetation and fire regimes are complex, and can be *difficult to disentangle*** [emphasis added] **under modern conditions when fire regimes are influenced by human activities' (p. 2918)."**
comment	Power et al. (2008, p 902) clearly were discussing anthropogenic fires as a late Holocene phenomenon: **"We have focused predominately on the role of climate rather than human intervention in modulating past fire activity, although studies of individual regions suggest that humans may have played a role, especially during the latter part of the Holocene".** Daniau et al. (2010), in motivating their study on fire regimes during the Last Glacial were clearly talking about the present day. Daniau et al. (2012, p GB4007) provide this summary of the role of anthropogenic burning that is still relevant 10 years later: **"There has been a great deal of speculation about the supposedly pre-eminent role of ancient human populations in determining paleo-fire regimes…. However, regional scale analyses have consistently failed to show an association between human presence or activities and the amount of biomass burning as shown by charcoal records [Daniau et al., 2010; Mooney et al., 2011; Marlon et al., 2012; Power et al., [2013]**]." Holliday et al. (2020, p 84) wrote: **"Wolbach et al. (2018a, p. 170) assert that Fischer and colleagues 'identified a single large NH$_4$ peak that begins at the YD onset, reflecting the largest biomass-burning episode from North American sources in the entire record.' Fischer et al. (2015) make no such claim."**
Wolbach et al., p 98	**"This statement is false. In figs. 2c, 3c, and 4c of Fischer et al. (2015), those authors plotted running averages of the concentrations of NH$_4$ in two ice cores, NGRIP and GRIP. Their fig. 4c displays very large peaks in NH$_4$ concentrations at the YD onset for both cores within the interval of the past 10,000 to 20,000 y. Compared with the running averages for NH$_4$ in figs. 2c and 3c of Fischer et al., the peaks at the YD onset are more than twice as large as any other peak within the past 10,000 to 100,000 y."**
comment	As can be seen in the data themselves, Fischer et al. indeed made no such claim. Wolbach et al. are misinterpreting the running-averaged background curves in Fischer et al. (2015), which depict the emissions of ammonium from soils, not wildfires. See Section 9.2.
Wollbach et al., p 98	**"Fischer et al. (2015) also discuss peak fire frequency (peaks per 201 y) and show that fire frequency was low at the YD onset. Holliday et al. use that to claim that no unusual impact fires occurred. However, that logic is seriously flawed. Any 201-y interval may contain hundreds of fires or only a few. Either way, the number of nonimpact fires is irrelevant to the question of whether nearly simultaneous impact fires also occurred in that interval."**
comment	Wolbach et al., are apparently claiming that all impact fires could be registered in a single peak. The plot in figure 4c of Fischer et al. is a running total of corrected fire peaks in a 201-yr wide window, every 50 years. Using the data in the supplemental materials of Fischer et al., it is possible to plot those cumulative values annually along with the individual fire peaks, which, if Wollbach et al.'s contention is correct, should show a peak from **"nearly simultaneous impact fires"** at the beginning of the YD/GS-1. In the first 100 yrs. after the beginning of the YD/GS-1 (12,846 y[BP1950, GICC05]) there are only two peaks, the first occurring 30 yrs. after the onset of the YDC. Fire frequency was indeed low at the beginning of the YD/GS-1.
Biomass burning	Holliday et al. (2020, p 84) state "[T]**he evidence for a link between extraterrestrial impacts and wildfires is weak… The idea of a global fire following the K/Pg impact is widespread in the literature and is used as corroborative evidence for a YD impact…."**

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Table 1 (*continued*)

"Rebuttal" Paper	Substance of "rebuttal"
Wolbach et al., p 98	**"This position is not widely accepted, as Holliday et al. (2020) acknowledge."**
comment	Many of the claims regarding the use of supposed carbon markers to indicate not only extensive biomass burning but also the type and intensity of fires have never been addressed or are simply dismissed. See Section 9.
Tunguska	Holliday et al. (2020, p 85) comment **"the Tunguska impact is cited as having started a fire…, but an investigation based on contemporary reports, research articles, and websites provided no firm evidence of major wildfires… Of particular significance, contemporary photos show downed trees but no charring."** Wolbach et al. (2020, p 99) respond **"This statement is highly inaccurate. Intense fires caused by the Tunguska impact** [sic] **event were described by eyewitnesses."**
comment	Jones (2002, p 407) write that the Tunguska event **"happened in 1908, and the eyewitness reports were published 19 and 59 years later… and catalogued 73 years later."** Florenskiy (1963, p 5) wrote, **"The presence of live trees at the center of the catastrophe… bears witness to the comparatively low level of any possible flash burning."** See Section 9.3.
Carbon spheres	Holliday et al. (2020, p 85) comment, **"Firestone et al. (2007, p. 16,018) state that 'we recovered them from one of four modern fires… confirming that they can be produced by intense heat in high-stand wildfires.' However, no hypothesized process or evidence is provided showing how they may be formed."** Wolbach et al. (2020, p 99) refer to, but do not cite, a dubious abstract by Kimbel et al. (2008) as some sort of proof that **"carbon spherules containing NDs** [nanodiamonds] **have been demonstrated to form from tree sap under laboratory conditions that duplicate the temperature, pressure, and redox values within an impact fireball."**
comment	Scott et al. (2010, 2017) demonstrate most carbon spherules are sclerotia. See Section 12.4.
Usselo soil	Holliday et al. (2020, p 87) state **"Wolbach et al. (2018b, p. 190) argue that the charcoal in the Usselo soil, a widespread stratigraphic marker in northwest Europe, is evidence of biomass burning…. Based on the dating and the evidence for pedogenesis, van Hoesel et al. (2012, p. 7651), van der Hammen and van Geel (2008, p. 360) and Kaiser et al. (2009) all reject the claim that the Usselo soil is a rapidly deposited YDB 'event' layer."**
Wolbach et al., p 99	**"This statement is a misrepresentation."**
comment	Wolbach et al. (2018b, p 190) clearly argue that **"Kaiser et al. (2009) sampled across the YD-age Usselo Horizon… At approximately half the sites, they found 'conspicuous amounts of … charcoal' (p. 601) near or in the YDB layer, some of which they noted were associated with possible impact proxies."** Kaiser et al. (2009, p 601) state **"About half of the buried soil horizons have conspicuous amounts of macroscopic charcoal…** ***dispersed in the soil matrix…*"** (emphasis added). On p 606, they unequivocally state **"With respect to the conspicuous charcoal content in the palaeosols, a conceivable correlation to both the ET impact and the terrestrial impact of the eruption of the Laacher See volcano… seems implausible, considering the broad range of radiocarbon ages and fundamental doubts… Thus, the claim that the Usselo soil is a rapidly deposited ET event layer is rejected."** Wolbach et al. (2020, p 99) do agree **"geochemical impact tracers"** as defined by Firestone et al. (2007) are present at one site (Lommel), uncritically accepting the geochemical interpretations of Firestone et al. The data from the Usselo shows charcoal throughout the soil dating both before and after the YDB. See Section 5.6.
Extinctions	Wolbach et al. (2020, p 99) claim that **"YDB publications have never argued that the impact was the sole cause of the extinctions and, instead, claim it was one major factor in a complex extinction event."**
comment	YDIH papers clearly invoke a YDB impact as a key component of the extinction. See Section 3.2.
Wolbach et al., p 100	**"Considerable evidence supports the hypothesis that the large-scale extinction of some megafaunal genera and species occurred at or close to the YD onset within the uncertainties of radiocarbon dating…"**
comment	About half of the megafauna survived to ~15.6 k to ~11.5 cal ka BP (i.e., sometime within a span of over ~4000 years, from ~2.8 k before to ~1.3 k years after the YDB) in North America. see Section 3.2.
Clovis archaeology	Holliday et al. (2020, p 89) note that **"No stratigraphic or chronologic data exist to indicate a post-Clovis population decline."**
Wolbach et al., p 99	**"This is a false claim"**
comment	But they ignore the comment by Holliday et al. in their previous paragraph on p 89 **"No data based on dated regional records of in situ archaeological materials have been offered as evidence of a population decline."** That remains the case. They also neglect to note the summed probability analysis of radiocarbon dates from across North America by Buchanan et al. (2008), one of the first critiques of the YDIH. They conclude (p 11651) **"The results of the analyses were not consistent with the predictions of extraterrestrial impact hypothesis."** See Section 3.1 and Endnote 1.
Geomorphic and biological changes	Holliday et al. (2020, p 90) argue **"The instant and cataclysmic alterations of climate, flora, and fauna that are hypothesized should manifest themselves in paleobiological, geomorphologic, and stratigraphic records; however, they do not."**
Wolbach et al., p 100	Wolbach et al. respond **"On the contrary, it is well known that widespread major changes occurred at the YD onset."**
comment	They fail to note the rest of the discussion by Holliday et al. See Section 13.7.
Microspherules	Wolbach et al. (2020, p 103) note the comparative study by Holliday et al. (2016), but not data in that study on microspherules generated by J Kennett (YDIH proponent) which are largely similar to the data reported by Surovell et al. (2009).
Wolbach et al., p 103	**"West did not 'confirm' any microsphere IDs, as Holliday et al. (2020) claim, but rather confirmed candidate particles for SEM analyses."**
comment	West wrote, **"Your dusting of the material looks perfect for viewing the spherules, and in my opinion, you have definitely discovered some"** (email A West to T Surovell, July 15, 2008).
Wolbach et al., p 103	**"eight independent groups followed the prescribed protocol and identified YDB spherules, as reported by Pino et al. (2019)."**
comment	The sites reported by Pino et al. have serious flaws in their dating (Meltzer et al., 2014) and the interpretations by Pino et al. are seriously flawed. See Endnote 16.

limited rebuttals to critics of the YDIH. Wolbach et al. (2020) provide the only lengthy attempt to rebut criticisms, but most of those rebuttals either repeat claims regarding the YDIH previously dismissed or miss the key points raised by critics (Table 1). This review demonstrates that the YDIH is untenable in the light of research since its initial conception.

The YDIH has a long, checkered history that is not rooted in science (see Daulton et al., 2017a, p 7). One of the earliest versions of the hypothesis is the speculative book by Donnelly (1883), which claims a comet struck North America forming the Great Lakes. As the story goes, the aftermath devastated human (in particular) and other faunal populations and plunged the climate into a period of extreme cold (or a return to glacial conditions). This idea was resurrected by R. Firestone in a series of popular magazine comments and a popular-press book. Firestone and Topping (2001) embraced and combined earlier, long-rejected ideas predating modern understanding of impact craters to argue that the Carolina Bays are Late Pleistocene impact structures (Melton and Schriever, 1933; Sass, 1944; Eyton and Parkhurst, 1975) and that a supernova irradiated the Earth in the Late Quaternary (Brakenridge, 1981). Subsequently, Firestone's focus shifted from a supernova (Firestone and Topping, 2001; Firestone, 2002) to Donnelly's (1883) comet that created the Great Lakes. This shifted focus is described in the book *The Cycle of Cosmic Catastrophes: How a Stone-Age Comet Changed the Course of World Culture* (Firestone et al., 2006)[3] and a journal article (Firestone et al., 2007) (see also Section 7). As for the supernova, it was then claimed to have perturbed the orbit of a solar comet (Firestone et al., 2006) or ejected an exosolar comet (Firestone et al., 2006; Firestone, 2009a, 2009b) that struck the Earth. Firestone et al. (2006, 2007) were the first publications to gain wide attention, in part due to an AGU symposium in 2007 that drew considerable attention from the news media. The book is based on fanciful speculation and demonstrates a remarkable lack of understanding of the archaeological and stratigraphic data discussed. It contains many examples of misleading or blatantly untrue statements (noted throughout this review) and was described by Morrison (2010) as "pseudoscience."

The 2006 book and the other papers were not an auspicious prelude to the 2007 paper by Firestone et al. Further, that 2007 paper has problems including: a) poor-to-nonexistent numerical age control for most sites (see Section 5.3); b) no data on identification of nanodiamonds, polycyclic aromatic hydrocarbon (PAH) molecules (see Section 9.3), and fullerenes with extraterrestrial (ET) helium (see Section 13.2); c) highly speculative interpretations of the origins of magnetic spherules (see Section 10) and carbon spherules (see Section 12.4); and d) failure to publish a table of the measured concentrations of their proposed markers that they used to generate ambiguous graphs (see Section 13.6). That publication, as well as many subsequent papers by the YDIH proponents, contains many significant and obvious misstatements of fact, circular reasoning, and problematic age control, all reviewed here. Misunderstanding or misstating stratigraphic and archaeological records is a common theme in support of the YDIH, as documented in this review and elsewhere. Claiming evidence where none exists and providing misleading citations may be accidental, but when conducted repeatedly, it becomes negligent and undermines scientific advancement as well as the credibility of science itself. Also culpable is the failure of the peer review process to prevent such errors of fact from entering the literature. The *Proceedings of the National Academy of Sciences* "contributed review" system for National Academy members (e.g., Aldous, 2014), as in the case of Firestone et al. (2007) and Kennett et al. (2009a), is at least partially responsible. The "pal reviews" (as some refer to them) were significantly curtailed in 2010, in part due to the YDIH controversy.

We begin our review of the YDIH by examining its foundation; specifically, we probe the enigmatic questions the YDIH attempts to answer, and the assumptions behind those questions. To place the archeological, paleontological, and paleoclimatic questions the YDIH attempts to answer into proper context the "Younger Dryas" is defined in Section 2. In Section 3, the assumptions that underpin the foundational questions of the YDIH are examined in detail, showing that several are flawed or fundamentally false, any one of which would reject the overall hypothesis.

The underlying assumptions and propositions of the YDIH include:

a) *The environmental changes at the beginning of the YDC are synchronous around the world.* This assumption is probably true, and is supported by high-resolution, independently dated speleothem and lake records (Section 3.3), but synchroneity is not unique to the YDIH.

b) *The direct effects of the hypothesized impact were synchronous around the globe and date precisely to the YDB.* This is clearly contradicted in archaeological, paleontological, and paleoenvironmental records (Sections 3, 5, 13.1 and 13.7).

c) *The direct and indirect effects of the hypothesized impact were consistent in sign, pattern and magnitude with the "Impact Winter" scenario (or with nuclear winter or exceptional volcanism scenarios).* This is contradicted by the spatial pattern of YD climates (Section 3.3).

d) *The YD (and its accompanying climate reversals) was a unique episode during the Quaternary and requires a special explanation.* This is contradicted by numerous long terrestrial, marine and ice-core records, which demonstrate that hundreds of such episodes occurred during the Quaternary (Section 3.3).

e) *Clovis Paleoindians disappeared immediately after the impact.* The 'disappearance' of Clovis was no more than an instance of cultural change, technological change and/or a change in settlement strategy (Section 3.1).

f) *Megafauna extinctions began immediately following the impact* (although extinctions are also claimed by some YDIH proponents to have occurred from multiple impacts over tens-of-thousands of years). Many genera have last appearance ages that predate the YDC by millennia, and others survived to the end of the YDC or into the Holocene (Section 3.2).

g) *The demise of Clovis technology, and megafauna extinctions were unique, discretely dated events and require special explanation.* These are baseless interpretations or assertions that contradict extensive data sets (Sections 2, 3, 5, 13.1 and 13.7).

We then examine in detail the evidence purported by YDIH proponents to support an extraterrestrial impact. We first examine the provenance of the evidence and demonstrate it is problematic. Section 4 describes the flawed sampling in the collection of the evidence and Section 5 describes the problems in the dating and stratigraphic context of the collection sites. Proponents of the YDIH make several claims with regard to sampling:

h) *The sampling for data from sections spanning only hundreds or a few thousand years is sufficient to categorize an event as unique and unprecedented within many millennia.* Long, well-dated sections with records of uninterrupted deposition must be subjected to discrete, continuous sampling and analysis to demonstrate the uniqueness of any claimed event of suite of purported impact indicators (Section 4). No such sections and data sets have been reported.

i) *The beginning of the YDC must be determined using terrestrial age control.* The YDC is defined as a component of the geologic time scale and its lower and upper boundaries are defined by Greenland ice-cores, supplemented by speleothem and other annual-resolution records (Section 5.1).

j) *Numerical age control is accurate and precise at most sites with impact indicators and statistically conforms to a singular geologic event.* Most sites lack directly dated samples from within their purported YDB layers and on adjacent layers, and even among those that have such samples, their dates vary between sites and many dates lack precision (Section 5). Age-depth models provide only an estimated age, typically with large statistical errors.

k) *So-called "black mat" deposits and the Usselo/Finow soils are unique, date to the YDB (or YDC, depending on the version of the YDIH), and are a consequence of the impact.* These organic-rich soils and sediments comprise a major source of confusion and contradictions surrounding YDIH. They are not linked to the YDB, and few examples are unique to the YDC (Sections 5.6 and 6).

l) *There is a simple YDB impact scenario consistent with known physics and all the purported evidence.* Various (often conflicting and disjointed) impact scenarios have been proposed and are necessary to explain the wide range of physical sediment constituents offered in support of an "impact event", i.e., supernova event, surface impact(s), and/or aerial bolide(s) (Section 7). The YDIH is a collection of different variant

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[3] The paperback version was originally published as *The Cycle of Cosmic Catastrophes: Flood, Fire, and Famine in the History of Civilization*. The two versions differ slightly.

hypotheses (and impact scenarios) that attempt to use the same purported set of evidence with unavoidable conflicts and contradictions.

A broad array of physical evidence is claimed by YDIH proponents to support the various impact scenarios. Proponents of the YDIH make a number of assumptions in their interpretation of the physical evidence and these include:

m) *Craters that date to the YDB may or may not exist regardless of the purported evidence* (to the contrary see Sections 7, 8 and 13.1). Craters provide the strongest evidence of an impact and those dating to ~12.9 ka should be well preserved, but none are known (Section 8).

n) *The charcoal record of fire has been interpreted correctly and shows* ***"the entire continent was on fire"*** (J. Kennett in Pringle, 2007). The data on wildfires cannot be used to unambiguously indicate the extent, type, intensity or temperatures of fire (Section 9). The global charcoal record has been subject to various misapprehensions and misinterpretations (Section 9.1) and when reanalyzed by YIDH proponents yields results similar to that in the literature. Multiple peaks in charcoal abundance are documented through late Quaternary sections, but none have been shown to be uniquely associated with an impact.

o) *The ice-core record of fire was interpreted correctly and shows a big peak in fire at the YDB.* YDIH proponents have badly misinterpreted the ice-core record (Section 9.2). The ice-core and charcoal records are in agreement that the YDC (and the YDB in particular) was a time of low incidence of fire (Section 9).

p) *Spherules and microspherules are unambiguous indicators of an extraterrestrial impact and/or impact-generated wildfire.* Microspherules can have various origins other than impact and cannot be used as impact indicators unless they are shown to be of meteoritic origin, which is not the case for most purported YDB microspherules (Section 10). The YDB carbon spherules are not impact-generated wildfire products but rather are fungal sclerotia that are ubiquitous in sediments (Sections 9.3 and 12.4).

q) *Platinum-group element measurements of YDB sediments and ice provide support for an impact* (to the contrary see Section 11). Platinum anomalies can arise from terrestrial sources and those reported by YDIH proponents are not uniquely associated with the YDB (Section 11).

r) *Techniques and methods used to measure nanodiamond abundances are correct and accurate, nanodiamond identification is also correct, and nanodiamonds are reliable impact indicators.* In most cases nanodiamond identification is suspect and in any case, they are not reliable impact indicators. All measurements of nanodiamond concentration in sediments/ice is scientifically meaningless, and in several cases irreproducible by YDIH proponents (Section 12).

In evaluating any evidence, the reliability and credibility of the source of the evidence are critically important. In Section 13, we discuss the most wildly speculative evidence backed with no viable support that has been offered then abandoned but in certain instances continue to be offered in support of the YDIH. In Section 13.6 we discuss the lack of transparency of YDIH evidence. In Section 13.7 we discuss the critical evidence that is missing that should be observable if the YDIH is correct. In Section 14 we test and show the failure of the reproducibility of YDIH results by examining specific studies where the same specimens or specimen splits were analyzed by different research groups.

2. Defining the "Younger Dryas"

Any discussion of events or their causes during the Younger Dryas must grapple with defining the interval itself, which is not straightforward. **"The term 'Younger Dryas' is frequently used inconsistently in the literature, while lacking precise definition or recognition of its roots. It is most commonly used to denote a specific time interval (the 'YD chronozone'** [12.9 k to 11.7 cal ka BP during the Last Glacial-Interglacial Transition (LGIT; Fig. 1)]**) or a climatic event (the 'YD stadial'), but also refer to particular environmental response to climatic forcing, for example local glacier or ice sheet expansion ('YD glaciation' or 'YD readvance'). These labels are not, however, universally applicable or interchangeable"** (Lowe et al., 2019, p 171). This climate episode was originally described as a cold interval or stadial in Scandinavia and adjacent North Atlantic and European regions (see Mangerud, 2021, for the history of the "discovery" of the Younger Dryas). It has also been known by other names: e.g., "Loch Lomond Stadial", "Nahanagan Stadial", "La Chonta Stadial", and "El Abra Stadial" with regard to other regional records: Great Britain, Ireland, Costa Rica, and Colombia, respectively (see Islebe et al., 1995; van der Hammen and Hooghiemstra, 1995; van 't Veer et al., 2000; Lowe et al., 2019). The climate episode is clearly global in nature, and as we discuss below, appears to begin synchronously around the globe, but varies in magnitude and sign (Kaplan et al., 2010; Carlson, 2013; Cheng et al., 2020). It is the last of 26 such major climate reversals that occurred over the past ~120 kyr, known as Dansgaard-Oeschger (D-O) "events" and is characterized by rather abrupt beginnings and endings (on the order of decades to centuries) (Dansgaard et al., 1982; Labeyrie et al., 2013; Lynch-Stieglitz, 2017). As the most recent such episode, the "Younger Dryas" has become the exemplar of abrupt climate change, both at its beginning (the YDB) and end.

In practice, the term "Younger Dryas" itself is ambiguous. In one sense of its usage, "Younger Dryas" is the name of a climostratigraphic unit (i.e., a cold interval or stadial), but there is no implicit assumption about global or regional spatial synchroneity of either the beginning or the end of the interval. Indeed, since the "discovery" of the "Younger Dryas" (see Björck, 2013; Mangerud, 2021) the question of whether the climatic changes marking the beginning and end of the unit are synchronous as opposed to diachronous has been repeatedly asked. The alternative use of the term is as a chronostratigraphic unit, i.e., a fixed dated interval (12.9 to 11.7 ka), that begins and ends everywhere at the same time. The term "Younger Dryas" is used both as a noun and an adjectival modifier in both contexts, as in "the Younger Dryas climatic reversal" (in the climostratigraphic sense), or "Younger Dryas Chronozone" (in the chronostratigraphic sense). The distinction is important because individual authors may use the term in one sense or the other (or both), which can add considerable ambiguity to discussions, particularly when the term is used as a noun ("the Younger Dryas").

The development of a common chronology for the Greenland ice cores ("GICC05" Rasmussen et al., 2006) introduced another name for the cold interval, GS-1 (Greenland Stadial 1, corresponding to the chronostratigraphic interval 12,896 ± 4 to 11,703 ± 4 yr [b2k, GICC05]).[4] Rasmussen et al. (2014, p 25) pointed out that the "Younger Dryas" (and the other episodes during the LGIT) were originally defined as periods **"of biostratigraphic change reflected in terrestrial records in Denmark (Iversen, 1954), but subsequently they are widely used in other geological contexts, and in areas for which they were never initially intended. Strictly speaking, this terminology should be restricted to Scandinavian terrestrial records"**. They caution that the term "Younger Dryas" **"can be used as a non-archive-specific synonym for the stadial period between Bølling-Allerød and the Holocene (approx. 12.9-11.7 ka b2k), but not as a synonym for GS-1"**.

Thus, the "Younger Dryas" and GS-1 refer to climostratigraphic units defined by different sets of climate proxies for Denmark and Greenland, respectively, and defined this way, the climostratigraphic units are not necessarily synchronous. But, the units are essentially synchronous (Section 3.3) and so, to provide consistency with discussions of the YDIH and other literature, we will refer to the "Younger Dryas" and GS-1 as YD/GS-1. This also applies to the stratigraphic units that precede (or are below) the YD/GS-1, i.e., the Bølling-Allerød (14.7 to 12.9 ka, B-A/GI-1), or follow (or are above) it, i.e., the Holocene (post-11.7 ka). The term "lower boundary of the YD" (YDB) has been used to describe both the onset date of the YD/GS-1 and the physical sediment layer deposited at

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[4] For ice-core ages we add "[b2k, GICC05]" to explicitly refer to a reference date, "b2k" (i.e. 2000 CE) and chronology, "GICC05".

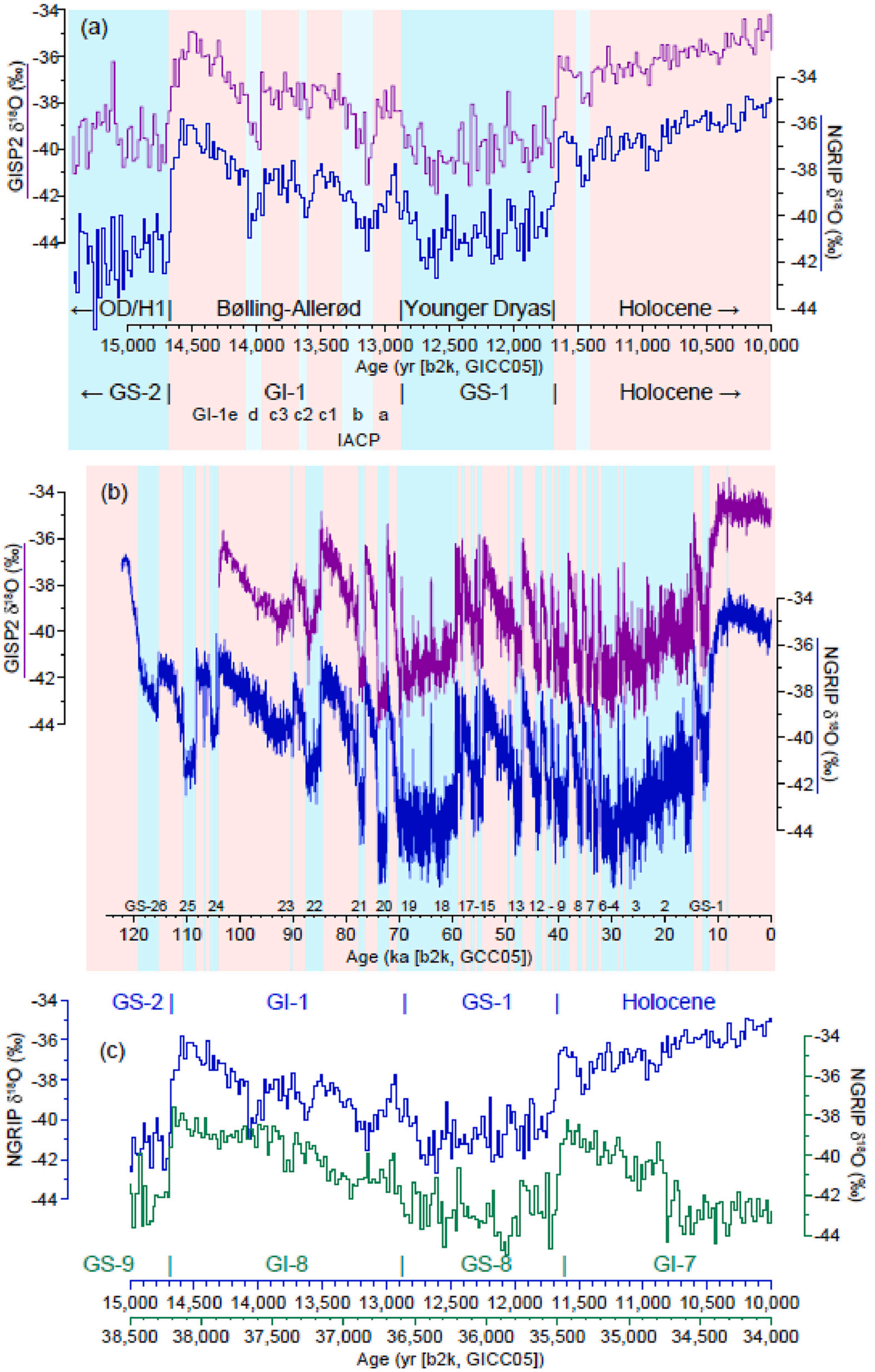

Fig. 1. The Last Glacial/Interglacial Transition and the last glacial/interglacial cycle in Greenland ice cores. The data plotted are 20-year averages of δ^{18}O from the GISP2 and NGRIP ice cores (from Rasmussen et al., 2014), plotted on the GICC05 time scale (b2k). Relatively warm episodes are shaded in pink, and relatively cool episodes in blue, with warm intervals during stadials in light blue. a) Data for the interval 15,000 to 10,000 yr b2k. The traditional or common chronostratigraphic names for the episodes appear above the age axis, and the INTIMATE event-stratigraphy names appear below. Arrows indicate that the episodes extend beyond the age-axis limits. "OD/H1" means "Oldest Dryas/Heinrich Event 1," and "IACP" stands for "Inter-Allerød Cold Period." The unlabeled cool episode between 11,500 and 11,400 yr b2k is the "11.4 ka event" in the INTIMATE event stratigraphy. b) Data for the last glacial/interglacial cycle. The 26 cold or stadial intervals are labeled above the x-axis. c) comparison of NGRIP data plotted over the interval 15,000 to 10,000 yr b2k, including the warm interstadial B-A/GI-1, the cold stadial YD/GS-1 and Holocene (blue), as well as over the interval 38,500 to 34,000 yr b2k, including the warm interstadial GI-8, the cold stadial GS-8, and the warm interstadial GI-7 (see Mangerud, 2021 fig. 3, for a similar plot).

that time. This imprecision of language has propagated into the term "YDIH" itself, which hypothesizes an impact event at the YDB. As Mangerud (2021) notes, **"…interchanging climatostratigraphy with geochronological and chronostratigraphic units is common to many parts of Quaternary stratigraphy…".**

The overall structure of the LGIT can be visualized using 20-y average values of δ^{18}O from the GISP2 and NGRIP ice cores (from Rasmussen et al., 2014) (Fig. 1). The traditional names of the chronostratigraphic (and climostratigraphic in Scandinavia) units are shown above the age axis, and the "INTIMATE" (Rasmussen et al., 2014; Seierstad et al., 2014) event-stratigraphy labels are shown below. The general variations from the cold GS-2 (also known traditionally as the "Oldest Dryas"), to the warm GI-1 (Greenland Interstadial 1, Bølling-Allerød), to the cold YD/GS-1 and finally to the warm Holocene are readily apparent, as is considerable variability within units, as during the B-A/GI-1. Note that, these paleoclimatic records are specific to Greenland, and Greenland is not necessarily the "thermometer of the world." However, as discussed in Section 5.1, the Greenland stratigraphy is globally applicable for organizing the climatic variations within the LGIT.

The working hypothesis for the cause of these events involves the Atlantic Meridional Overturning Circulation (AMOC, Lynch-Stieglitz, 2017). Specifically, the hypothesis proposes that weakening of the AMOC, in particular by fluxes of freshwater to the North Atlantic, results in cooling of the North Atlantic, which is transmitted globally through atmospheric circulation and changes in the global ocean thermohaline circulation or conveyor belt. In the case of the YD/GS-1, the freshwater flux was apparently related to the rapid draining of Lake Agassiz toward the North Atlantic at the beginning of the YD/GS-1 (Carlson et al., 2007), reinforced by later drainage of the Baltic Ice Lake (Stroeven et al., 2016; see Björck, 2013 and Carlson, 2013 for reviews).

Over the last glacial cycle (and before that as well, see below), there is abundant evidence for frequent freshwater fluxes and contemporaneous AMOC changes (Lynch-Stieglitz, 2017; Menviel et al., 2020; Capron et al., 2021). As noted by each of these authors, there is variability among the different events, and the potential for more complicated sources of AMOC variability than simple meltwater forcing. For example, Menviel et al. (2020) review additional potential controls on AMOC variations, including internal oscillations and variations in the carbon cycle, and Capron et al. (2021) describe climate model simulations that effectively capture the differences among D-O events owing to differing background states (see also Vettoretti et al., 2022; Obase et al., 2021). There is little reason to suspect, however, that the basic AMOC/abrupt climate-change (AMOC/ACC) hypothesis will be rejected, based on the trajectory of recent work on this subject. Significantly, the AMOC hypothesis attempts to explain *all* of the major abrupt climate changes of the last glacial cycle (see Section 3.3), of which the YD/GS-1 is only one.

3. Flawed fundamental assumptions in the YDIH

Blatant misstatements along with misunderstandings about the archaeological, paleontological, and paleoclimatological records of the termination of the Clovis archaeological tradition, timing and geography of Late Pleistocene extinctions, as well as the uniqueness of YD-age environmental change provide the foundational rationale for the YDIH (e.g., Firestone et al., 2006, 2007; Kennett et al., 2018; LeCompte et al., 2018; Wolbach et al., 2018a, 2018b; Powell, 2020) (Sections 3.2 and 3.3). Powell (2022, p 4) asserts **"One reason scientists have had difficulty settling on the cause or causes of the extinction and the disappearance of the Clovis toolkit is that they have been unable to reach consensus on the cause of the YD cooling itself."** No citations for this claim are provided. He assumes that these three issues (extinction, human behavioral change, and climate change) must be linked but offers no tenable evidence nor specific linking mechanism to explain all three. The following discussion summarizes the abundant, widely distributed and easily available literature that contradicts these assumptions.

3.1. *Misunderstood decline of Clovis Paleoindians*

Firestone and Topping (2001, p 14) claimed, **"The enormous energy released by the catastrophe at 12,500 yr B.P. could have heated the atmosphere to over 1000°C… Radiation effects on plants and animals exposed to the cosmic rays would have been lethal."** Firestone et al. (2006, p 2) proclaimed evidence for **"a violent calamity that doomed millions of animals to extinction…** [and] **decimated the human race…"** and asked, "**What caused the sudden annihilation of Ice Age mammoths, mastodons, saber-tooth tigers and … other animals along with much of the human race?"** Subsequently, the title of Firestone (2009a) refers to **"Mammoth, Megafauna, and Clovis Extinction, 12,900 Years Ago"** and the abstract states, **"The onset of >1000 years of Younger Dryas cooling, broad-scale extinctions, and the disappearance of the Clovis culture in North America simultaneously occurred 12,900 years ago followed immediately by the appearance of a carbon-rich black layer at many locations."** The abstract of Firestone et al. (2010a) claims, **"At many locations the impact layer is directly below a black mat marking the sudden disappearance of the megafauna and Clovis people"** and write, **"The impact event followed by extensive fires and sudden climate change likely contributed together to the *rapid*** [emphasis added] **extinction of the megafauna and many other animals"** (p 57). Kennett et al. (2008a, p 2531) make similar claims, **"The rapid extinction of many Rancholabrean animals is closely timed to the Clovis cultural assemblage that abruptly appeared and disappeared across North America between 13.1 and 12.9 ka"** and Kennett et al. (2018, p 182) assert **"the termination of Clovis is well indicated at ~12.8kya."** LeCompte et al. (2012, p 2961) similarly allude to archaeological **"dormancy"** at the Topper site as a function of a **"YDB event."** Wolbach et al. (2018b, p 196) surmise that the black mat **"represents both the extinction boundary and the termination of the Clovis culture. This termination is coeval with the decline in other paleohuman populations across the Northern Hemisphere (Anderson et al., 2011)."**

Anderson et al. (2011) was cited by Wolbach et al. (2018b) and again by Powell (2022, p 4) to support such assertions. However, Anderson et al. (2011) acknowledged that many of their study sites have poor to nonexistent age control or stratigraphic context for artifact assemblages, particularly in eastern North America, along with a variety of problems in tying frequencies of radiocarbon dates to population sizes (e.g., Section 5.1 and ENDNOTE **1**). Mahaney et al. (2022, p 18) claim that Meltzer et al. (2014), Holliday et al. (2020), and others **"dispute the catastrophic connotation and disjunct age relationship of 12.8 ka to the megafaunal extinction and disappearance of the Clovis culture. A measured response to Holliday et al.** [2020, not 2009] **by Wolbach et al.** [2020, not 2019] **stressed the isochronous C14 for black mat sites, thus refuting their inaccurate age claims**." Mahaney et al., Wolbach et al., and other YDIH proponents fail to deal with many problems in dating the YDIH (Section 5.3) as well as the voluminous data on extinctions and Clovis archaeology (Sections 3.1, 3.2 and 5.7).

Nevertheless, Sweatman (2021, p 2) insists "**The impact theory also has far-reaching consequences for…cultural transitions, such as the end of the Clovis culture in North America"**, and **"The sudden ending of the Clovis culture in North America is one of the claims of the impact theory"** (p 15). Insistence is not a substitute for data. When Firestone and Topping (2001) was published (and still in 2023), there is no dated, in situ archaeological evidence nor other data to support claims that Clovis populations experienced a catastrophe.

All of the quoted statements regarding Clovis archaeology derive from the false assertions about it at the Blackwater Draw site (aka the Clovis type site). Firestone et al. (2006, p 73) assert that humans abandoned the site for 1000 years after the YDB. Other YDIH proponents mention a similar post-YDB hiatus at the site (Kennett and West, 2008, p

E110; LeCompte et al., 2012, p 2967). But YDIH proponents never acknowledge the voluminous archeological, stratigraphic, and chronological data for the site that clearly contradict these claims. Indeed, the recognition of the now classic Clovis-Folsom archaeological sequence was first noted at Blackwater Draw (Cotter, 1938; Sellards, 1952). There is no evidence for a **"culturally dead zone"** as described by LeCompte et al. (2012, p E2967). Such statements demonstrate either a selective choice of data or a fundamental misunderstanding of the geoarchaeological record of North America and indeed much of the world. Hunter-gatherer sites of any period rarely display evidence of long, continuous occupation for a simple reason: hunter-gatherers typically spend much of their time moving from site-to-site hunting and gathering (Kelly, 2013, p 77-1113). ENDNOTE **2.**

Firestone et al. (2007, abstract) claimed **"Causes for the… termination of Clovis culture have long been controversial"** and 13 years later Powell (2020) proclaimed without evidence that **"scientists have struggled to explain"** the disappearance of Clovis archaeology. No, they have not. The disappearance of the Clovis tool kit (which shares many similarities with Folsom and other successors) is no more difficult to explain than the disappearance of the thousands if not hundreds of thousands of hunter-gather toolkits identified in the global archaeological record spanning the Pleistocene and a multitude of environmental changes. This non-issue was dismissed over a decade ago (Hamilton and Buchanan, 2009; Holliday and Meltzer, 2010; Meltzer and Holliday, 2010). The YDIH is an ET explanation for an archaeological problem that does not now and has not existed in the past. Dating of Clovis archaeology is further discussed in Section 5.7.

3.2. Misunderstood megafauna extinctions

In their opening paragraph, Firestone et al. (2007, p 16016) mischaracterize megafaunal extinctions occurring **"abruptly and perhaps catastrophically at the onset of the YD… in North America where at least 35 mammal genera disappeared…, including mammoths, mastodons, ground sloths, horses, and camels, along with birds and smaller mammals."** Kennett et al. (2009a, p 12627) then claimed "**The vast majority of the North American megafaunal taxa abruptly vanished from the North American continent at the onset of the YD at 12.9 ± 0.1 ka…"** Sweatman (2021, p 2–3) claimed, **"many extinct megafaunal species are found below the black mat, but not within or above, including horse, camel, mastodon, direwolf, American lion, short-faced bear, sloth and tapir.**" Finding extinct species stratigraphically below the "black mat" (a purported YDC marker bed, fully discussed in Section 6) has little significance. All manner of extinct species, including dinosaurs, are "below" YDC deposits.

Abundant evidence contradicts these claims. Kennett et al. (2018, p 187) correctly state that the YDIH predicts that **"The primary extinction horizon should be synchronous, as determined using highly accurate, precise, and well-calibrated radiocarbon dating at high statistical probability."** The YDIH fails to satisfy this prediction on all points. As described by Grayson (2016, table 4.2) and Holliday et al. (2014, p 525) current dating suggests 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, and none of the avian extinctions are dated. A wide variety of fauna (mega and otherwise) apparently disappeared well before ~12.9 cal ka BP (e.g., Barnosky et al., 2004; Cooper et al., 2015; Grayson and Meltzer, 2015; Grayson, 2016; Koch and Barnosky, 2006; Meltzer, 2015; Stuart, 2015), but some also persisted later (e.g., Cooper et al., 2015; Grayson and Meltzer, 2015). Mastodon survived until the end of the YDC in the Great Lakes region. Redmond and Tankersley (2005) report that extinct giant beaver and flat-headed peccary occur stratigraphically above the purported YDB at Sheriden Cave, Ohio, a key YDIH site (e.g., Tables 3, 4). In South America, a variety of megafauna survived into the YDC and early Holocene (Bampi et al., 2022). Further the complexity of extinction is demonstrated by data from Rancho La Brea, California (O'Keefe et al., 2023) where seven species were extirpated before the onset of the YDC.

Firestone et al. (2007, p 16016) argue **"the end- Pleistocene extinction event is unique within the late Quaternary and is unlikely to have resulted only from climatic cooling and human overkill. The extinctions were too broad and ecologically deep to support those hypotheses."** Kennett et al. (2009a, p 12623) further argue that **"More sophisticated models combining environmental and human induced causes… are potentially viable for explaining singular mammal extinctions (e.g., Mammuthus), but fall short of explaining the full taxonomic depth and ecological breadth of the latest Pleistocene extinctions."** They are clearly offering the YDIH as an explanation, but their argument about a misunderstood extinction event they perceive to be unlikely more appropriately applies to the YDIH, which has been quantitatively shown to be exceedingly unlikely based on known impact statistics (Boslough et al., 2012). ENDNOTE **3.**

In arguing that impacts are not a low-probability rare event and that the YDIH is a viable hypothesis, (Sweatman, 2021, p 18) cites Hagstrum et al. (2017) as support of coherent catastrophism (see Section 7). However, Hagstrum et al. (2017) has marginal relevance to most versions of the YDIH, which propose the impact event occurred precisely at the YD/GS-1 onset and caused the megafauna decline to extinction. Hagstrum et al. (2017, p 2) claimed **"impact-related microspherules and elevated platinum concentrations in fine-grained sediments retained within seven radiocarbon-dated Late Pleistocene bison and mammoth skull fragments from Alaska and the Yukon Territory."** They concluded that **"repeated airbursts, including ground/ice impacts, and their associated blast winds"** (p 2) occurred **"between ~46 and 24 ka B.P… and again… between ~15 and 11 ka B.P."** (p 12). Hagstrum et al. (2017) use their fig. 9 (adapted from fig. 1 of Cooper et al., 2015) to suggest that these airburst/impacts were responsible for extinctions of various megafauna species over that time. However, Hagstrum et al. (2017, p 13) state, **"The radiocarbon dates determined for the skull fragments of this study are too few to support any robust correlation with the Late Pleistocene extinction events."** Cooper et al. (2015) show the extinctions began well before the YD/GS-1 onset and continued 2000 years after the YD/GS-1 onset, contrary to the "mainstream" YDIH. Hagstrum et al. (2017) allude to a very different version of the YDIH where impacts repeatedly occurred from ~50 to ~10 ka causing the megafauna extinctions over that time span. Moreover, Hagstrum et al. (2017) fail to present a single example of evidence that is widely accepted as diagnostic of an impact event (e.g., shatter cones, shocked mineral phases), have not identified any candidate impact craters, and do not explain the physics of how a swarm of cometary fragments would have a size distribution capable of generating blast waves over such a large area without leaving a single impact crater in the geological record.

None of these papers advocating for the YDIH by bringing in the extinction issue confront the complex issues of variation in timing of extinction by species and region (e.g., Barnosky et al., 2004; Koch and Barnosky, 2006; Faith, 2014; Cooper et al., 2015; Stuart, 2015). As described by Holliday et al. (2020, p 88), the late Pleistocene extinction is **"A complex issue that is well documented in the extinction literature… Late Pleistocene vertebrate extinctions took place across the Americas, Europe, and Asia and were complex, time-transgressive, and geographically far-reaching… YDIH proponents have invoked a YDB-age cataclysm to explain the extinctions but have never attempted a coherent argument outlining how an impact at ~12,900 cal BP could explain what is known about the extinctions in time and space."**

3.3. Problematic chronologic and paleoclimatic assumptions

Four assumptions are implicit in the YDIH: 1) that the climatological and environmental changes at the beginning of the YD/GS-1 (i.e., YDB) are globally synchronous, 2) that the ages of the direct "impact indicators" are identical to that of the hypothetical impact; 3) that the indicators are consistent in sign and magnitude with the "Impact Winter" scenario proposed by Wolbach et al. (2018a, 2018b); and 4) that all of those environmental changes are unique or so exceptional in a longer-term context that a singular, as opposed to general, explanation for them is required. If any supposition were shown to be false, that would discredit the YDIH.

The first three assumptions jointly propose that the climatological effects generated by a purported impact produced synchronous changes in other environmental systems. Furthermore, these changes occurred in a direction consistent with that predicted by the YDIH, i.e., the "Impact Winter" i.e., "**a sustained decrease in near-global temperatures**" that occurred at the beginning of the YD/GS-1 (Wolbach et al., 2018b, p 200). However, this did not occur (see also Section 13.7). The notion of synchroneity is central to the YDIH: "**If an ET event caused the YD, then within the limits of dating precision, the YDB will have the same age everywhere. If on the contrary, different YDB sites have different ages and especially if those ages spread over a significant amount of time, that would falsify the claim of an instantaneous event**" (Powell, 2022, p 27). However, it is important to note that while an ET impact might be presumed to produce globally synchronous environmental changes, it is not the only mechanism that could do so. In particular, the AMOC/ACC hypothesis described in Section 2 also predicts that the climate changes in the North Atlantic region will be registered globally, without an appreciable lag.

The "Impact Winter" scenario holds that at the YDB "**The radiant and thermal energy from multiple explosions triggered wildfires that burned ~10% of the planet's biomass, producing charcoal peaks in lake/marine cores that are among the highest in 368,000 y**" (Wolbach et al., 2018a, p 179). (Wolbach et al., 2018b, p 200) propose that the smoke from those fires "**might have persisted for 6 wk or more at the YD onset, blocking all sunlight and causing rapid cooling.**" The reduced insolation "**would have had widespread and catastrophic biotic effects, including insufficient light for plant photosynthesis and growth**" (Wolbach et al., 2018b, p 200). They also propose that "**The impact event destabilized the ice-sheet margins, causing extensive iceberg calving into the Arctic and North Atlantic Oceans**" which "**collapsed multiple ice dams of proglacial lakes along the ice-sheet margins, producing extensive meltwater flooding into the Arctic and North Atlantic Oceans**" (Wolbach et al., 2018a, p 179). Note that except for the "impact event" feature, this latter proposal is consistent with elements of the AMOC/ACC hypothesis; see Section 2). By using analogies with nuclear war and other ET impact scenarios, Wolbach et al. (2018a, 2018b) clearly propose that the impact and its immediate effects occurred on timescales too short to resolve using standard radiocarbon dating. An additional complication in assessing synchroneity of environmental changes at the YDB using radiocarbon dating arises from the presence of an "age plateau" in radiocarbon calibration curves at the YDB (see Section 5.1). This has the effect of compressing a range of calendar ages into a shorter span of radiocarbon ages, which can contribute to a greater sense of synchroneity among radiocarbon ages than is real.

On the global scale, there is also substantial evidence for synchroneity of the YDB, but with some relatively short (decadal) lags in the expression of the accompanying climatic changes between the Greenland ice-core records and high-temporal-resolution records elsewhere (Cheng et al., 2020; Nakagawa et al., 2021). For example, recent comparisons of the Greenland ice-core records with a global network of U-Th dated speleothem records (Cheng et al., 2020), and the annually laminated sedimentary record of Lake Suigetsu, Japan (Nakagawa et al., 2021), strongly support the notion of global synchroneity of the onset of the YD/GS-1. None of the records examined by Cheng et al., or Nakagawa et al. depend on conventional radiocarbon dating, and both incidentally also support the use of the Greenland ice-core chronology, i.e., GICC05, as the "master" one. Both studies admit the possibility of regional lags in the full expression (as opposed to the initiation) of the climatic response to North Atlantic-focused abrupt climate changes and relate those to large-scale atmospheric and oceanic circulation mechanisms. Both studies also propose and test hypotheses about those linkages that invoke elements of the AMOC/ACC hypothesis (Cheng et al., 2020, p 23414; Nakagawa et al., 2021, their section 5.4). In general, paleoclimatic and paleoenvironmental records do support the notion of a globally synchronous beginning of the YD/GS-1, but as is the case of the latter two examples, an impact is neither required nor invoked, and Cheng et al. (2020) explicitly consider and reject it.

Larsson et al. (2022), by supplementing the usual dating methods applied to European paleoenvironmental records with tephrochronology (to chronologically align records), were unable to support a long-standing hypothesis that the YD/GS-1 climate reversal was asynchronous across Europe on a regional scale. Similarly, Reinig et al. (2021, p 68), while focusing on the determining the age of the Laacher See eruption, noted that "**Our study demonstrates that the Greenland GI-1–GS-1 transition coincided with the European AL/YD cooling. The temporal match between Greenland ice core and central European climate proxies suggests that the last major Northern Hemisphere cooling interval before the Holocene was initiated and steered by an abrupt climate system change that instantly affected the whole North Atlantic region.**" Likewise, Engels et al. (2022, abstract), examined the vegetation change across Europe at the YDB, and concluded that it "**shows instant and, within decadal scale dating uncertainty, synchronous response of the terrestrial plant community to Late-Glacial climate change across northwest Europe.**" Consequently, the first assumption of global synchrony in the beginning of the YD/GS-1 is probably true.

As to the second assumption, synchroneity in the response of impact indictors, the Impact Winter scenario places strong demands on the quality of local chronologies for establishing synchroneity among purported YDIH indicators (especially the direct impact indicators), and with global records. An environmental or biotic change that is simply in the chronological neighborhood of the beginning of the YD/GS-1 does not automatically imply that the environmental change supports the YDIH. It must be exactly synchronous with or slightly post-date the YDB, but not occur before. This situation also implies that where available, absolute chronologies produced by annual layer-counting or radiometric dating methods not subject to calibration errors should be preferred when discussing questions of timing or synchroneity. The second assumption, synchroneity of impact indicators, is unlikely to be supported in all cases (see Section 5 for a fuller discussion of dating issues among purported YDIH impact indicators). The third assumption is that the climate changes at the beginning of the YD/GS-1 were consistent in sign, pattern, and magnitude with the "Impact Winder scenario". Climate-model simulations of nuclear war scenarios offer insight into the spatial and temporal expression of an impact winter. One of the more robust features of such simulations is the nearly global uniformity in the sign of the climate change: decreases in surface air temperature nearly everywhere and in all seasons (except some scattered high-latitude regions, related to stratospheric warming), persisting for several years (Robock et al., 2007a, 2007b). The hypothesized YDB impact should inject significant particulate in the atmosphere, and simulations of the response to the widespread injections of aerosols by volcanism or geoengineering solutions (Zhao et al., 2021) also show uniformly negative changes in surface air temperature.

Fastovich et al. (2020, abstract) used a variety of paleovegetation indicators to show that "**YD cooling was pronounced in the northeastern United States and muted in the north central United States. Florida sites warmed during the YD, while other southeastern sites maintained a relatively stable climate.**" Further illustrating the complexities of climate during and before the YDC, Griggs et al. (2022) show that climate in the eastern Great Lakes during LGIT was antiphased to the stereotypical warmer B-A/GI-1 and colder YD/GS-1 (owing to the effects of the Great Lakes and other periglacial lakes on regional climate, see also Hostetler et al., 2000). In a similar fashion, Schenk et al. (2018) synthesized multi-proxy biotic evidence from Europe and showed that summer temperatures remained high over the YD/GS-1, a result consistent with high-resolution climate simulations they describe that employed an experimental design consistent with the AMOC/ACC hypothesis.

Other lines of evidence are inconsistent with the "Impact Winter" scenario. Extinction of megafauna is the clearest example, a claim discussed and dismissed in Section 3.2. The occurrence of catastrophic wildfires at the YDB is another well-known claim. Evidence for fires is ubiquitous in lake and other stratigraphic records (Marlon et al., 2013) and provides no evidence for a unique burning event at the YDB, further discussed in Section 9. Other paleobotanical records from across North America show a wide variety of changes before, during, and after the YDC but nothing unique at the YDB (e.g., Meltzer and Holliday, 2010; Straus and Goebel, 2011; Eren, 2012). Similarly, geomorphic and stratigraphic records provide no evidence for a cataclysm at the YDB (Meltzer and Holliday, 2010; Holliday and Miller, 2013; papers in Gillespie et al., 2004; Straus and Goebel, 2011; Eren, 2012) (Sections 5.6, 6 and 13.7). Thus, the third assumption, that the sign, pattern, and magnitude of the climatic changes at the beginning of the YD/GS-1 are consistent with the "Impact Winter" scenario, is false, i.e., not supported by the paleoclimatic evidence.

The fourth major assumption that underlies and motivates the YDIH is that the YD/GS-1 (and especially its beginning) is a unique event, featuring the occurrence of specific controls or exceptional or unusual levels of individual paleoenvironmental indicators not found at other times in those records. If so, that would imply that the YD/GS-1 must require a singular explanation (such as a comet impact, supernova, or exceptional volcanism), as opposed to a more generic explanation with roots internal to the Earth's climate system, such as that provided by the AMOC/ACC hypothesis. The variability recorded by the Greenland ice cores during the LGIT (Fig. 1), and throughout the last glacial cycle—the Dansgaard–Oeschger (D-O) events (Lynch-Stieglitz, 2017), immediately contradicts this supposition. Within the LGIT, for example, the change in δ^{18}O values from the warm GI-1c1 into the cooler GI-1b interval (formerly called the Inter-Allerød Cold Period (IACP)) is almost as great as that at the beginning of the YD/GS-1, and simple inspection of the ice-core δ^{18}O data shows how similar in shape, duration, and amplitude the variations of individual D-O "cycles" (e.g. GS-9 through GS-8 and into GI-7, Fig. 1c) are to those during the LGIT. Simple inspection of the data also contradicts the claims of the occurrence of exceptional values of various paleoenvironmental indicators at the beginning and during the YD/GS-1 (Holliday et al., 2020, table 8).

If the (global) climatic changes during the LGIT were unique in character, they would stand out as an outlier if statistically (as opposed to simply visually) compared with those of the other D-O events. Nye and Condron (2021) applied a multivariate outlier-detection approach (Filzmoser et al., 2008) to test this hypothesis using four records that depict the abrupt (and global) climate variations over the last glacial/interglacial cycle: the Greenland NGRIP δ^{18}O and CH_4 records (Rasmussen et al., 2014; Baumgartner et al., 2014), as well as δ^{18}O and CO_2 records from Antarctic ice-cores (Barbante et al., 2006; Bereiter et al., 2015). For each record and D-O event, they characterized the shape of the variations by the magnitude of change from interstadial to stadial, the slope (rate and direction of the change), and the median value (overall level) of the records. They concluded that the variations during the LGIT (from B-A/GI-1 through the YD/GS-1) were "**not unique compared to those of the other D–O events recorded in the Greenland ice core record, other than the fact that the median δ^{18}O levels are higher due to proximity to deglacial warming into the Holocene. The higher median δ^{18}O is also not unique to the BA/YD, as D–O events 2, 20, and 23 exhibit a similar phenomenon, which we attribute to their occurrence proximal to long-term global climate fluctuations**" (Nye and Condron, 2021, p 1419). This observation discredits the notion that the world was already in an interglacial mode but returned to a glacial one from the consequences of an impact. Nye and Condron (2021, p 1419) conclude "**The non-uniqueness of the BA/YD's shape is clearly indicated by the statistical indistinguishability of the changes in the Greenland ice core record with the other D–O events, especially in terms of its δ^{18}O variability.**"

Contemporaneously with the Firestone et al. (2006, 2007) publications, the AMOC/ACC hypothesis, involving catastrophic drainage of Lake Agassiz (Broecker et al., 1989; Broecker, 2006) was also being discussed, and there was at the time the perception that an abrupt climate reversal at the close of a glacial period (a termination) *was* unique to the last one, or at least not common during terminations (Carlson, 2008; see also Cheng et al., 2009). However, subsequent work shows that abrupt climate reversals during terminations, and D-O-type variations during glacial periods, were pervasive throughout the latter half of the Quaternary (and probably longer). Abrupt climate changes that accompany meltwater events have been identified at the end of the penultimate glaciation (MIS 6, 191–132 ka; Capron et al., 2014; see also Menviel et al., 2019), and the one before that (MIS 8, 300–243 ka; Pérez-Mejías et al., 2017). In fact, D-O-type events over the past two glacial cycles (i.e., since MIS 7, 243–191 ka) have been identified in loess sequences (Rousseau et al., 2020), and in marine records with terrestrial indicators (e.g. Margari et al., 2010; Sierro and Andersen, 2022), at least as far back as MIS 11 (424–374 ka; Martrat et al., 2007, 2014; Oliveira et al., 2016), and also in composite speleothem records spanning the past 0.5 My (Cheng et al., 2016). Relative sea level can be used as a rough index of ice sheet configurations that would produce proglacial lakes that might rapidly drain to the North Atlantic and produce abrupt YD-style climate reversals. The relative sea-level (see Spratt and Lisiecki, 2016) that prevailed between 13 ka and 11 ka (i.e., from the B-A/GI-1 through the early Holocene, contemporaneous with the existence of Lake Agassiz) can be seen via inspection to occur 26% of the time over the past 800 kyr.

The ubiquity throughout the paleoclimatic record of D-O-type events consistent with the AMOC/ACC hypothesis suggests that this hypothesis is quite robust, and that the abrupt climate changes themselves are recurrent features of climate variability, and do not require a special explanation. Smaller amplitude, shorter-term variations are also apparent throughout the record, like those during B-A/GI-1 (e.g., the IACP or GI-1b, Fig. 1), as well as during the Holocene. The "8.2 ka event" is one such variation (Barber et al., 1999; Thomas et al., 2007), apparently triggered by the final drainage of glacial lakes Agassiz and Ojibway into Hudson Bay, and thus to the North Atlantic. Like the YD/GS-1 (and other D-O events), abrupt climate change in the North Atlantic region is registered globally (e.g., Morrill et al., 2013), and also like the YD/GS-1, with no discernable lag (Parker and Harrison, 2022). Because there are no others of comparable magnitude during the Holocene, the "8.2 ka event" might be regarded as unique or a "one-off" occurrence, but there is evidence for a similar "interglacial" event during the previous interglacial, at 125 ka, the "LILO" ("Last Interglacial Laurentide outburst") event (Zhou and McManus, 2022). Both the "8.2 ka" and "LILO" events are consistent with the AMOC/ACC hypothesis, and so neither requires a special explanation.

Consequently, the primary attribute that distinguishes the YD/GS-1 is that it is simply the most recent D-O-type event (and well within the range of annual layer-counting chronologies or calibration of radiocarbon ages, and so comparatively well studied). It is consistent with the AMOC/ACC hypothesis, and there is no need for a singular or exceptional explanation for the YD/GS-1.

Surprisingly little discussion within the YDIH literature deals with the other 25 abrupt climate changes within the last glacial cycle (e.g., the D-O events) with the exception of Wolbach et al.'s (2018a, 2018b) and Sweatman's (2021) misunderstanding of paleofire records over the last glacial cycle (Sections 9.1 and 9.2). This is in keeping, however, with a methodological approach of looking only where one expects to find something, and not elsewhere (see also Section 4). For example, Powell (2022, p 3) cites as an example of exceptional occurrences and observations at the YDB by quoting Kennett's (2019, abstract) observations that the drainage of Lake Agassiz and the Baltic Ice Lake, as well as changes in the margins of the ice shelves at 12.9 ka, cannot be explained "**by invoking conventional climatic and/or paleoceanographic processes. Instead, this broad range of evidence is more readily explained by catastrophic processes triggered by a cosmic impact with Earth; the YDB cosmic impact theory.**" But the ice-core and other paleoenvironmental records of the D-O events tell us that conditions sufficient to significantly weaken the AMOC as well as to produce a globally registered abrupt climate change happened 26 times over the past 120 kyr, and hundreds of times over the Quaternary. The YDIH evidently was conceived to solve another problem that does not exist.

To summarize, the YD/GS-1, and the YDB in particular, and the accompanying environmental changes appear to be globally synchronous, which is consistent with the first assumption of the YDIH, but note those changes are also consistent with the AMOC/ACC hypothesis. The second assumption, synchroneity of the impact indicators is indeed problematical, is deeply discussed in Section 5. The heterogeneous spatial pattern of the environmental changes is not consistent with the "Impact Winter" scenario, and hence does not support the third assumption, and the observation that abrupt climate changes appear frequently throughout the Quaternary, provides no support for the notion that YD/GS-1 is exceptional or special, the fourth assumption.

4. Flawed sampling

Sampling is a critical issue in the YDIH debate. It is at the heart of many of the controversies and problems associated with the YDIH but receives relatively little attention in the literature. When individual purported impact "indicators" were shown to have other explanations or at least ambiguous origins, unsubstantiated arguments emerged claiming that *assemblages* of the "indicators" found in concert were unique to an impact. For example, Sweatman (2021, p 2) notes that the various purported indicators **"have only ever been found together in the context of a cosmic impact."** This is a circular argument (Table 2). The purported "indicators" have only been found together by YDIH impact proponents, and they are the ones that presume an impact context. Furthermore, the same impact scenario cannot explain all the indicators. Conflicting impact scenarios have been proposed for a number of indicators (see Section 7). Some scenarios include the infeasible speculation of involvement of a supernova to explain radioactive sediments (Firestone et al., 2006) and young ages of carbon spherules (Firestone, 2009a). Bunch et al. (2012, p E1903), Kinzie et al. (2014, p 499), and Moore et al. (2020, p 1) also allude to the uniqueness of peak abundances of impact/wildfire proxy assemblages. However, uniqueness has never been established because most sampling focuses on the YDB, the purported YDB, and/or zones just above and below, an issue of confirmation bias. This is well illustrated by Firestone et al. (2007), Israde-Alcántara et al. (2012, 2018); Wittke et al. (2013a), Kinzie et al. (2014), Moore et al. (2017) and Pino et al. (2019), among others and documented by (Holliday et al., 2020, table 4) (Table 3).

Kennett et al. (2015b, p E6723) also argue that **"No 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."** In contradiction, as stated by Holliday et al. (2020, p 75) and documented in their table 4: **"Few reported sites have long (thousands of years), continuous records of sedimentation subjected to close-interval, continuous sampling to show that claimed impact proxies are unique to ~12.8 ky. Essentially no data to date show that increased levels of proxy impact indicators at the YDB are unique to, for instance, the post–Last Glacial Maximum (LGM) Quaternary record or, for that matter, the last glacial-interglacial cycle. At a minimum, several thousand years before and after the YDB (including the end of the YDC) must be analyzed for unusual changes in selected indicators to be claimed. Very few studies do so."** No site in the Americas, Europe, Africa, Asia or Greenland with 50,000 years of continuous sedimentation preserved, much less full

Table 2
Issues of confirmation bias, presumption, and consistence.

Reference(s)	Assertions[1] of YDIH Proponents and Critical Responses
Firestone et al., 2006, p 127	**"All of the evidence fits our theory that the rims and bays formed all at the same instant."**
Response	Meltzer et al. (2014, SI p 28) "**Firestone et al. [(2007, p 16019)] subsequently realized the ages of the Carolina Bays vary, and sought to assert that the supposed YDB layer found in 15 of the bays dated to 12,900 years BP. They based this assertion on the fact that the markers found therein were identical to those found elsewhere dated to 12,900 years BP… That circular argument cannot be used as chronological evidence, as it assumes what it ought to demonstrate.**" See Section 13.1.
Bunch et al., 2012, p E1903; Kinzie et al., 2014, p 499; Moore et al., 2020, p 1	Allude to the uniqueness of peak abundances of claimed proxy assemblages (with no substantive basis); these suites of claimed indicators are interpreted (with no support) as evidence for an impact and then argued that these suites of "indicators" are only found in the context of an impact.
Sweatman, 2021, p 5	"**Petaev et al. (2013a) also measured the iridium content of the Greenland ice, finding only a very weak but extended iridium signal coeval with the platinum signal, which is difficult to interpret in terms of known meteorite types. But this combination might be explained by a cometary source.**"
Response	*Difficult to interpret* but somehow *might be explained* by a comet.
Sweatman, 2021, p 5	Wonderkrater site **"Although radiocarbon dating of this site is highly uncertain, it is consistent with a YDB age, and pollen based measurements indicate it likely corresponds to the onset of the YD period."**
Response	*Highly uncertain* date, but somehow *consistent* with the YDB.
Sweatman, 2021, p 12	Results of Gill et al. (2009) **"suggests this charcoal layer is not inconsistent with a Younger Dryas age."**
Response	Somehow *suggests* it is *not inconsistent.*
Sweatman, 2021, p 15	Usselo Soil: **"Proper age-depth models that intersect the boundary were not generated for any of them, leaving open the possibility that the sites are synchronous and the dispersion in dates they found was due to natural processes."**
Response	They *might be synchronous*; thus, they might be YDB-age, therefore they must be YDB-age.
Sweatman, 2021, p 16	**"The eight high-quality sites span N. America, western Europe and south west Asia, and each is consistent with a synchronous event, which suggests all YDB sites are likely synchronous."**
Response	Because 8 sites are statistically consistent, they might be YDB-aged, therefore somehow the other 15 must be consistent with the YDB.
Sweatman, 2021, p 16	In a critique of Holliday and Meltzer (2010, figure 3), **"the Wilcox** [sic] **Playa site, Arizona,…datapoint… at 10,000 ± 700 cal BP (1 sd) corresponds to a minimum age for the black mat, and not the age of its base where, presumably, the geochemical signals of the impact are likely to be found."**
Response	Assumes the undated base of the layer must be the YDB, and that impact signals are present.
Sweatman, 2021, p 17, 19	**"No YDB site has yet been found to be obviously inconsistent with a synchronous event."**

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Table 2 (*continued*)

Reference(s)	Assertions[1] of YDIH Proponents and Critical Responses
Sweatman, 2021, p 16	**"…we should conclude that the data presented by Kennett et al. (2015a) are not obviously inconsistent with a synchronous event… Given the apparent synchronicity of the high-quality sites that span three continents, it would be surprising if the others were not all eventually found to also be consistent."**
Response	*Not obviously inconsistent*, thus *must be consistent*.
Sweatman, 2021, p 19	Samples from Lubbock Lake: **"the nanodiamond and magnetic microspherule evidence from** [J. Kennett's] **lab is new**… [and is] **inconsistent with the impact hypothesis. Considering the uniqueness of these results, this work should be repeated."** All other sites: **"given the strength of the impact hypothesis, we can expect it to be consistent with other YDB sites."**
Response	The low precision and/or lack of direct dating of the purported YDB impact proxies at most claimed YDIH sites suggests that the Lubbock Lake record is not unique. The perception that the YDIH is correct results in the expectation bias that all other sites, besides perhaps Lubbock Lake, are consistent with the YDIH.
LeCompte et al., 2018, p 165	Nanodiamonds in sediments **"range from nonexistent to extraordinarily rare, being found in high abundances only in known or proposed impact-related sedimentary layers."**
Sweatman, 2021, p 10	Bement et al. (2014) found an **"abundance in surface soils, potentially indicating an airburst in more recent times near this location."**
Response	A purported high abundance of nanodiamonds from an unconfirmed impact implies that all nanodiamonds in sediments must be from an impact. An unambiguous link between nanodiamonds and an impact has yet to be confirmed. See Sections 12.1 and 12.2.
Bunch et al., 2012, SI p 2	**"All such YD-aged pit-houses at Abu Hureyra and their immediate environs contained a dark charcoal-rich layer indicating extensive burning that the excavators previously attributed to residue from cooking fires…, but which is also consistent with broader-scale biomass burning at 12.9 ka."**
Response	Pit houses at an archaeological site have YD-age charcoal that *could* be from *common* cooking fires or *rare* broad-scale burning, therefore it must represent a YDB impact.
Kennett et al., 2015a, p E4351	**"Nine other proxy rich sites currently lack sufficient dating for robust Bayesian analysis. Even so, *the stratigraphic context* of a proxy-rich layer or samples at these sites supports a YDB age"** (emphasis added).[2]
Kennett et al., 2015a, SI p 34	Even though **"dating is insufficient for robust Bayesian analysis, a wide range of** [unspecified] **evidence indicates that all nine are YDB sites"** and **"because these nine sites contain the same abundance peaks in proxies that are found at well-dated YDB sites, we have proposed that they are of YDB age."**
Response	Questionable impact proxies (see Sections 9, 10, 11, 12) and poor dating at most sites (Meltzer et al., 2014; see also Section 5) provide firm evidence for a YDB age.[3]
Kennett et al., 2015a, p E4352	Bayesian analyses: **"354 dates at 23 sites in 12 countries across four continents demonstrate that modeled YDB ages are consistent with the previously published range of 12,950–12,650 Cal B.P."**
Response	23 sites are statistically *consistent*; thus they *could be* YDB-aged, therefore must be YDB-aged.[3]
Kennett et al., 2015a, p E4352	"[T]**he 23 YDB age estimates appear isochronous within the limits of chronological resolution (~100 y) and could have been deposited during a single event."**
Response	23 sites *appear isochronous* and thus *could* represent a single event and therefore must support the YDIH.
Kinzie et al., 2014, title	**"Nanodiamond-rich layer across three continents consistent with major cosmic impact at 12,800 Cal BP."**
Response	24 sites presented (just one in South America) though most have problematic age control,[3] questionable nanodiamond identification, and erroneous nanodiamond quantification, yet somehow support the YDIH.
Kinzie et al., 2014, p 481	**"No matter the cause, the ages of these two sites remain poorly constrained. Nevertheless, 18 of the 24 sites with the same YDB markers are well dated, suggesting that the YDB layer is correctly identified at Gainey and Chobot."**[4]
Response	They *could be* YDB, therefore they must be YDB.
Mahaney et al., 2022, p 3	**"The upper Guil catchment is the type locality for the cosmic airburst, presumed to be the black mat, formed at or close to the Younger Dryas boundary (YDB) in the European Alps at 12.8 ka."**
Response	*Presumed* black mat from a speculated cosmic airburst "at or close to" the YDB (with no numerical age control) must be the YDB.
Wu et al., 2013	Assume that spherules and magnetic grains are impact indicators, and therefore the presumed impact indicators are stratigraphic markers that define the YDB with no numerical age control.
Moore et al., 2017	Use "indicators" to identify where the YDB is located and then argue that they have proof of an impact at the YDB based on those indicators.
Chobot site Wittke et al., 2013b, p E3900	**"Chobot may seem unpersuasive as a single site**… [it] **is highly consistent with the multicontinental YDB record. Similar coeval marker peaks occur at ~30 dated YDB sites in 10 countries on four continents. Thus, the best explanation is that Chobot contains the YDB layer where indicated."**
Response Meltzer et al., 2014, SI p 29	The site produced no numerical age control nor Clovis points in place (only non-Clovis artifacts), but because it is somehow *highly consistent* with the YDB record it is likely YDB age.[5]
Melrose site Bunch et al., 2012, SI p 5, 6	**"Major abundance peaks in SLOs… and spherules. occurred in an 8-cm-thick interval at 19 cmbs** [(cm below the surface and)]… **about 15 cm above the till, consistent with emplacement after 18 ka when the ice sheet retreated."** **"We collected a sample for OSL [Optically Stimulated Luminescence] dating at 28 cmbs, 5 cm below the layer containing peaks in SLOs and spherules (see approaches described earlier for the Blackville site). The sample yielded an OSL date of 16.4 ± 1.6 ka (SI table 2), and assuming a modern age for the surface layer, then linear interpolation dates the proxy-rich YDB layer centered at a depth of**

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Table 2 (*continued*)

Reference(s)	Assertions[1] of YDIH Proponents and Critical Responses
	19 cmbs to 12.9 ± 1.6 ka. This date is supported by its location relative to the glacial till known to date to <18 ka (Colgan et al., 2003) and is consistent with a YDB age."
Response	Based on one date with a large standard deviation and an unsubstantiated assumption about the age of the surface (Meltzer et al., 2014), the dating is somehow *consistent* with the YDB, and therefore it must be YDB.
Wittke et al., 2013a, p E2090	**"Other criteria helped confirm the identification of the YDB layer, including … the presence at 12 sites of darker lithologic units, e.g. the 'black mat' layer."**
Response	If it is a black layer, it must be the YDB black mat.
Wittke et al., 2013a, SI figure 12	Ommen site (Usselo soil): **"A single ^{14}C date of 13.33 ± 0.06 cal ka… is somewhat older than typical charcoal in the YDB, perhaps because of bioturbation, redeposition, or the 'old wood' effect."**
Response	Soil and charcoal are assumed to be YDB-aged, therefore any problematic dates must be wrong. The most obvious explanation of the dates is that it is from the Usselo soil, which spans much of the Bølling-Allerød and YDC.
Powell, 2020, prologue	Assumptions (presented as facts): **"some 13,000 years ago…"** [the post LGM] **"warming trend reversed… temperatures plunged…** [causing a] **return of glacial conditions", "some 35 of the big mammals…went extinct", "the YD… fundamentally changed (1) the evolutionary development and history of terrestrial animals on Earth; (2) human cultural evolution and history; and (3) climate and ocean history."**
Powell, 2022, p 34	The flawed assumptions (above) are presented as **"findings"** (Powell 2022, p 34) that support the YDIH.
Response	The assumptions about events at ~13,000 years ago became data that support the YDIH.
Powell, 2022, p 24	**"Moore et al. [2017] expanded their search for the platinum peak to seven other YDB sites… These are poorly or not directly dated and lack the black mat, but do provide a coherent Clovis archaeological record."** They found **"a Pt peak coincided with the YD onset based on archaeostratigraphy and chronometric dates…. consistent with the YDIH."**[3]
Response	Poor to no age control and mixed archaeostratigraphy (Section 5.3) somehow provide a precise date for the YDB and therefore the Pt anomaly is consistent with the YDIH.

[1] Italics added for emphasis.

[2] These sites are Chobot, Alberta, Canada; Gainey, MI; Kangerlussuaq, Greenland; Kimbel Bay, NC; Morley, Alberta, Canada; Mt. Viso, France/Italy; Newtonville, NJ; Paw Paw Cove, MD; and Watcombe Bottom, United Kingdom.

[3] See discussions of problematic dating by Meltzer et al. (2014), Holliday et al. (2014, 2020) and Section 5 in this review.

[4] The Gainey and Chobot sites are devoid of reliable age control as well as clear archaeological and stratigraphic context, as documented by the primary investigators (Meltzer et al., 2014; Holliday et al., 2014).

[5] Meltzer et al., (2014, SI p 29) state, **"In that same response Wittke et al., [2013b] assert there is a nonalgal black mat present at the Folsom (NM) site. That is neither correct** [Meltzer, 2006]**, nor relevant."**

sampling of such a record with accurate, high-resolution dating, is reported in the YDIH literature (Tables 3 and 4) (ENDNOTE **4**). As discussed in Section 5.1, Greenland ice-core records *do* span the last glacial cycle, but analyses of isotopic and geochemical indicators that span the entire record do not show anything exceptional about the last cold phase, except that it was the last (Holliday et al., 2020, table 8). Analyses of particular elements in the ice-core records that have been claimed as support for the YDIH (e.g., Pt, in Petaev et al., 2013a) are confined to only a short interval around the beginning of the YDC (in addition to other issues regarding the Pt record; Sections 5.4 and 11), and therefore their exceptionalism over the whole record cannot be judged. Importantly, many so-called wildfire and impact proxies used by YDIH

Table 3

Sampling information from YDB sites (modified from Holliday et al., 2020, table 4).

Site	Total samples collected & analyzed	Samples at or near claimed YDC zone	References
Aalsterhut[1]	3?	?	van Hoesel et al., 2012
Geldrop-Aalsterhut	3	1	Andronikov et al., 2016a
Abu Hureya	6	1	Bunch et al., 2012
	7	1	Wittke et al., 2013a
	1	1	Moore et al., 2020
Arlington Canyon	27	27[2]	Kennett et al., 2008a Kinzie et al., 2014
Arlington Canyon	17	9	Moore et al., 2017
Barber Creek	3	3	Wittke et al., 2013a
Barber Creek	22	3	Moore et al., 2017
Big Eddy	5	1	Wittke et al., 2013a
Blackville[3]	11	1	Bunch et al., 2012 Wittke et al., 2013a
Blackwater Draw	15	1	Firestone et al., 2007
	8	1	Wittke et al., 2013a
	9	3	Moore et al., 2017
Bull Creek	8	4	Kennett et al., 2009a
	34	1[6]	Bement et al., 2014
Chobot	12	1	Firestone et al., 2007
Daisy Cave	4	2	Firestone et al., 2007
Flamingo Bay	20	3	Moore et al., 2017
Gainey	7	1	Firestone et al., 2007
	11	1	Wittke et al., 2013a
Hiscock[4]	19	?	Laub, 2010
Indian Creek[5]	?	?	Baker et al., 2008
Johns Bay	9	2	Moore et al., 2017
Kolb	20	3	Moore et al., 2017
Kimbel Bay	8	1	Wittke et al., 2013a
Lake Acambay[6]	?	1?	Israde-Alcántara et al., 2018[7]
Lake Chapala[8]	?	1?	Israde-Alcántara et al., 2018[7]
Lake Cuitzeo	16	5	Israde-Alcántara et al., 2012
	10	1	Wittke et al., 2013a
"Cuitzeo"[9]	?	1?	Israde-Alcántara et al., 2018[7]
Lake Hind	8	1	Firestone et al., 2007
	32	1	Teller et al., 2020
Lindenmeier[3]	4	1	Kinzie et al., 2014
Lingen	10	1	Wittke et al., 2013a
Lommel	9	2	Firestone et al., 2007
Lommel Maatheide	13	5	Andronikov et al., 2016a
Lommel Molse-Nete	10	5	Andronikov et al., 2016a
Lutterzand	9	5	Andronikov et al., 2016a
Melrose	5	2	Bunch et al., 2012 Wittke et al., 2013a
	?	1	Wu et al., 2013
Morley	5	1	Firestone et al., 2007
Murray Springs[3]	12	1	Firestone et al., 2007
Newtonville	3	1	Wu et al., 2013
	2	1	Kinzie et al., 2014
Ommen	7	1	Wittke et al., 2013a Kinzie et al., 2014
Paw Paw Cove	1	1	LeCompte et al., 2012
Pen Point	20	3	Moore et al., 2017
Pilauco	16	1	Pino et al., 2019

(continued on next page)

Table 3 (*continued*)

Site	Total samples collected & analyzed	Samples at or near claimed YDC zone	References
Santa Maria	2	1	Kinzie et al., 2014
Sheriden Cave	9	1	Wittke et al., 2013a
	8	1	Moore et al., 2017
Squires Ridge	44	3	Moore et al., 2017
Stara Jimka[10]	?	?	Kletetschka et al., 2018
Talega	9	3	Wittke et al., 2013a
Tocuila[11]	?	1?	Israde-Alcántara et al., 2018[7]
Topper	11	1	Firestone et al., 2007 corrected
	20	2	Moore et al., 2017
Wally's Beach[12]	?	?	Firestone et al., 2007
Watcombe Bottom	7	2	Kinzie et al., 2014
White Pond	33	6	Moore et al., 2019
Wonderkrater[13]	16	1	Thackeray et al., 2019

[1] Number of samples from van Hoesel et al. (2012) for possible impact indicators is not clear. According to van Hoesel et al. (2012, p 7652), 14 radiocarbon samples were collected along with 3 samples for SEM and TEM analysis. They further state **"Samples of 200–800 g of sediment were taken… from three selected locations in two pits at several intervals within and above the charcoal-rich part of the Usselo horizon….One bulk sample of the charcoal layer at the top of the Usselo horizon at Aalsterhut, AH-4, was sampled… during an earlier field visit."**

[2] Twenty seven samples were collected from a zone ~4 m thick (Kennett et al., 2008a). That zone yielded 15 radiocarbon dates accepted by the authors (their fig. 5 and table 4). They are out of stratigraphic order but span 13,470 to 12,830 cal years BP. The nine basal samples yielded carbon spherules and that zone was identified as the YDB, based on accepted dates ranging from 13,090 to 12,780 cal years BP. Four other older dates are rejected due to the **"old wood effect"** without explanation (Kennett et al., 2008a, p 2538). The sediments above are also dated **"to between 13 and 12.9 ka"** (Kennett et al., 2008a, p 2539) but contained no spherules. No samples were collected from below the level identified as the YDB.

[3] Samples from YDB are on a disconformity: At Blackville lower 5 samples immediately below claimed YDB level are from Miocene deposits; at Lindenmeier the dark YDC soil is on erosion slope; at Murray Springs the samples from YDC "black mat" rest on disconformity.

[4] Hiscock number of samples from claimed YDB not indicated.

[5] Indian Creek. Nanodiamonds **"reported independently in conference presentations"** (Kinzie et al., 2014, p 478).

[6] Acambay has the same levels of microspheres at the **"event"** level and **"above the event"** (Israde-Alcántara et al., 2018, table 3) (black mat dated <14.2–13.9 and > 9.5 cal ka BP; Table 4).

[7] Northern Mexico: sample interval of 20 cm but total number of samples collected and analyzed not indicated (Israde-Alcántara et al., 2018, p 62).

[8] Chapala black mat reported with high level of spherules 20 cm **"below the event"** and at the **"event"** but no data **"above the event"** (Israde-Alcántara et al., 2018, table 3) (Black mat <15.0 cal ka BP; Table 4).

[9] Cuitzeo black mat reported with high levels of spherules at the **"event"** and lower levels 20 cm **"below the event"** as well as 5, 10, 15, 25, and 45 cm **"above the event"** (Israde-Alcántara et al., 2018, table 3) (Black mat dated to ~ < 12.6 cal ka BP; Table 4).

[10] Kletetschka et al. (2018) report a total of 32 microspherules distributed over about a little more than 1 cm of core, over slightly more than 10 cm of core they examined, which according to their estimated accumulation rate corresponds to 360 years, and when adjusted to conform to their sediment interpretations, spans the YDB. They sliced the core into 2 mm layers and associated the lowest slice with the YDB. Analyses for microspherules from samples in two cores at 445.0–498.2 and 373.2–422.0 cm depth.

[11] Tocuila black mat reported with high level of microspheres at the **"event"** and lower level 20 cm **"above the event"** (Israde-Alcántara et al., 2018, table 3) but no data for other levels (black mat dated ~12.7 cal ka BP; but see Table 4).

[12] Wally's Beach number of samples collected and analyzed not indicated by Firestone et al. (2007).

[13] YDB zone proposed based on modelling and presence of purported impact indicators (Thackeray et al., 2019).

proponents are not generally accepted as such by experts in these fields (see Section 8). Therefore, it is critical for YDIH proponents to prove the uniqueness of these materials at the YDB for them to be relevant to the YDIH debate. Further, the methodologies used by YDIH proponents to identify and quantify abundances of some claimed wildfire and impact proxies are seriously flawed (charcoal, Section 4.1; carbon spherules and glassy carbon, Section 5.8; nanodiamonds, Section 12.6), ambiguous/meandering (magnetic and carbon spherules, Section 10) or undisclosed precluding critical assessment (fullerenes, Section 13.2).

4.1. *Arlington Canyon confusion*

Wolbach et al. (2020, p 97) falsely allege, "[Daulton et al., 2017a] **claim to have analyzed samples** [for nanodiamonds and found none] **from the YDB layer provided by Pinter et al. (2011). However, figures 3 and 4 of Pinter et al. reveal that not a single sample was acquired from 12,800-y-old strata and instead, samples were acquired up to thousands of years younger and older, completely missing the YDB-age layer.**" Pinter et al. (2011) show sediment logs of Sauces Canyon and Verde Canyon in their figs. 3 and 4, respectively, to demonstrate magnetic grains and spherules are present throughout those sequences rather than only at the YDB. Those sediments were not examined for nanodiamonds. They clearly state on p 258 that microcharcoal from Murray Springs and carbon spherules from Arlington Canyon (both dated to the YDB) were examined for nanodiamonds.

Wittke et al. (2013a, SI) incorrectly assert that Scott et al. (2010, 2017) and Daulton et al. (2010, 2017a, 2017b) sampled the wrong localities at Arlington Canyon. This flawed assertion was echoed by Kinzie et al. (2014), Sweatman (2021), and Powell (2020, 2022). Kinzie et al. (2014, p 477) wrote, **"Their incorrect stratigraphic locations apply to all those investigations, explaining their inability to detect YDB NDs** [nanodiamonds]**, cosmic- impact spherules, and ND-rich carbon spherules at Arlington Canyon."** In Sweatman (2021) much of the 'discussion' of the non-reproducibility of the nanodiamonds focuses on this incorrectly perceived misidentification of sampling localities rather than addressing any of the substantive criticism (similar to the approach taken in dismissing the many critical problems of dating documented by Meltzer et al., 2014; Section 5.3). Impact proponents make much of the spatial coordinate problem that was in fact due to a failure by Kennett et al. (2009b) to state the associated Datum. For example, Sweatman (2021, p 9) not only reproduces criticisms that had already been addressed but failed to understand what was said. **"Wittke et al. [2013a] and Kinzie et al. (2014) show… that Daulton et al. (2010) did not, in fact, sample the same site as Kennett et al. (2009b) at Arlington Canyon – instead their samples with labels SRI-09 were obtained from several different locations separated by up to 7000 m from the site sampled by Kennett et al. (2009b). Scott et al. (2017), with Daulton as co-author, later refuse to admit this error, pointing to photographs that show that they did indeed sample the same sediment bank as Kennett et al. (2009b)."**

Scott et al. (2017, p 44–45) clearly explained the situation, which we further clarify for emphasis.

> **"Arlington Canyon has featured centrally in results suggesting a global-scale impact drove broad changes at the onset of the Younger Dryas (the YDIH). Wittke et al.** [2013a] **assert that we did not study the same section as theirs (AC003). This is not true. While Kennett et al. ([2008a], 2009b) gave UTM coordinates without specifying the associated datum or map projection, we were able to navigate to** [the general area which we searched and found] **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. ([2008a] , 2009b) had used NAD27 (confirmed in Wittke et al., 2013). We subsequently**

Table 4
Dating of YDIH sites and regions.

Site or **Study Area**	**Dating & issues**	**References**++
	Firestone et al., 2007	
Chobot, Alberta	No Clovis material in place; reported Clovis points were found on the surface; zone with carbon spheres radiocarbon dated to late Holocene.	Firestone et al., 2007† Ives & Froese, 2013†† Kinzie et al., 2014† Meltzer et al., 2014††
	“[W]ell dated” and of **“long-established archaeological and paleontological significance”** (Firestone et al., 2007) but those claims are not supported.	
Comment	No numerical age control for this site.	
Daisey Cave, CA	Radiocarbon age approximates YDB, but only one sample collected for posited impact indicators.	Firestone et al., 2007† Kinzie et al., 2014† Meltzer et al., 2014††
Comment	**“[W]ell dated”** according to Firestone et al. (2007).	
Gainey, MI	Shallowly buried site with mixed Clovis context; Thermoluminescence [TL] date is low precision and highly questionable.	Simons et al., 1984††† Firestone et al., 2006, 2007† Firestone, 2009a† Boslough et al., 2012†† Holliday et al., 2014†† Kinzie et al., 2014† Meltzer et al., 2014††
	“[W]ell dated” according to Firestone et al. (2007).	
Comment	Carbon spherules representing the “YDB” directly dated to late Holocene and modern.	
Lake Hind, Manitoba	Age of zone with purported impact indicators <YDB. **“[W]ell dated”** according to Firestone et al. (2007).	Boyd et al., 2003††† Firestone et al., 2007† Kinzie et al., 2014†
	Age of zone with purported impact indicators dated 12.7 k cal yrs. BP. Numerous reversals in the section (>YDB above indicators and < YDB below indicators). Confused modelling used.	Teller et al., 2020† Breslawski et al., 2020†† West et al., 2020b†††
Comment	Direct date on the purported YDB with “impact markers.”	
Morley, Alberta	Section is in a drumlin created by the Cordilleran ice sheet, but no stratigraphic context was provided for samples of purported indicators; correlation with Lake Ontario drumlins ~2600 km to the southwest (undated but estimated to be >13,000 ^{14}C years BP) created by a different glacial system. Dating of both drumlin systems is uncertain.	Boyce & Eyles, 1991††† Karrow, 1984††† Firestone et al., 2007† Holliday et al., 2014†† Meltzer et al., 2014††
	“[W]ell dated” and of **“long-established archaeological and paleontological significance”** (Firestone et al., 2007) but those claims are not supported.	
Comment	No numerical age control for this site.	
Myrtle Beach, SC (M33)	The YDB layer of this Carolina Bay was purported to contain glass-like carbon with both “**ET helium ratio that is 84 times that of air**” (Firestone et al., 2007, p 16018) and nanodiamonds (Kinzie et al., 2014, p 489). See Carolina Bays below.	Firestone et al., 2007† Kinzie et al., 2014†
Wally’s Beach, Alberta	Context of purported impact markers with fauna and dating is confused. Timing of deposition of sediment and proposed impact markers within the bone could be older than the bone.	Firestone et al., 2007† Kooyman et al., 2001††† Meltzer et al., 2014††
	“[W]ell dated” according to Firestone et al. (2007). 27 ^{14}C dates on horse and camel bones show that the site dates to ~13.3 k cal yrs. BP.	Waters et al., 2015†
Comment	No direct dating of the purported YDB with “impact markers.”	
	Firestone et al., 2007; Moore et al., 2017	
Blackwater Draw, NM (aka the Clovis site, Blackwater Draw Locality 1)	Spherule spike on the South Bank is at the base of the “black mat” (Unit D). YDB date in Firestone et al. (2007, fig. 1) is from the North Bank, ~360 m north of the sampling section. The “Clovis surface” on the South Bank is at the Unit B/C contact, not at the C/D contact.	Haynes, 1995††† Firestone et al., 2006, 2007† Andronikov et al. (2016b)† Haynes & Warnica, 2012††† Holliday et al., 2014††
	Clovis point illustrated by Wittke et al. (2013a, SI fig. 4C) and by Moore et al. (2017, SI fig. 6d) and claimed to be from top of Stratum C not documented by those authors; no Clovis points are reported from this sampling area. The illustrated artifact is from the North Bank excavations.	Wittke et al., 2013a† Meltzer et al., 2014†† Moore et al., 2017†
	Age model based on spatially scattered ages, varying in absolute elevation and distance from one another, were onto a common absolute vertical scale. The manner in which the integration was done is not specified, rendering the scale of these interpolations entirely arbitrary and with it their statistical results.	
	Statistically and chronologically flawed age-depth interpolations.	
	“[W]ell dated” according to Firestone et al. (2007) and Moore et al. (2017).	
Comment	No direct dating of the purported YDB with “impact markers.”	

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Table 4 (*continued*)

Site or **Study Area**	**Dating & issues**	**References++**
Murray Springs, AZ	(Firestone et al., 2007, table 2) cite a date of 10,890 ± 50 ^{14}C yrs. BP (12,916 ± 25 cal yrs. BP) for the YDB based on an average of 8 unspecified samples in Taylor et al. (1996). YDB sampling section exposes a “black mat” (Stratum F2) resting unconformably on undated Stratum D in a section a minimum of 100 m from archaeological excavations and radiocarbon samples.	Taylor et al., 1996††† Firestone et al., 2006, 2007† Haynes & Huckell, 2007††† Kinzie et al., 2014† Meltzer et al., 2014††
	Wittke et al. (2013a) provide a “date” based on Interpolation by 2nd order polynomial regression. 7 out of 28 radiocarbon ages are used, but with no explanation of the choices. Age model based on spatially scattered ages, varying in absolute elevation and distance from one another, were onto a common absolute vertical scale. The method of integration is not specified, rendering the scale of these interpolations entirely arbitrary and with it their statistical results.	Wittke et al., 2013a† Meltzer et al., 2014†† Moore et al., 2017†
	Statistically and chronologically flawed age-depth interpolations. “[W]**ell dated”** according to Firestone et al. (2007) and Moore et al. (2017).	
Comment	No direct dating of the purported YDB with “impact markers.”	
Topper, SC	YDB dating and relationship of Clovis artifacts on slope with hypothesized impact markers not clear. Strata at and above Clovis zone mixed.	Miller, 2010††† Firestone et al., 2007† LeCompte et al., 2012† Kinzie et al., 2014†
	Modelled “date” based on Interpolation by 2nd order polynomial regression using 4 OSL dates and 1 ^{14}C age out of 7 OSL dates available with no explanation.	Wittke et al., 2013a† Meltzer et al., 2014††
	Spatially scattered ages, varying in absolute elevation and distance from one another, integrated onto a common absolute vertical scale, but the manner in which the integration was done is not specified. This renders the scale of these interpolations entirely arbitrary and with it their statistical results. Statistically and chronologically flawed age-depth interpolations.	
	The area that produced that single radiocarbon date is an excavation block ~70 m north of and further upslope from the sampling area reported by LeCompte et al. (2012), and ~ 120 m north of and upslope from the area investigated by Firestone et al. (2007).	
	Pt spikes within “Clovis floor” and just above in “YDB?”	Moore et al., 2017†
	Pt from **“Paleoindian and Early Archaic components within the same stratigraphic zone or with very little separation”** (Moore et al., 2017, SI Info p 5).	
	“[W]**ell dated”** according to Firestone et al. (2007) and Moore et al. (2017).	
Comment	No direct dating of the purported YDB with “impact markers.”	
	Moore et al., 2017	
Arlington Canyon, CA	More than 20 “black mats” with statistically identical dates at the YDB through a 4 m section. Only multiple basal layers yielded purported carbon spherules. Multiple zones through the section yielded Pt.	Kennett et al., 2008a† Kinzie et al., 2014† Meltzer et al., 2014†† Moore et al., 2017†
	“[W]**ell dated”** according to Moore et al. (2017).	
Comment	Most YDB layers devoid of “impact indicators.”	
Barber Creek, NC	Modeled the YDB at 110 cm based on three OSL ages with 10% error (or standard deviation of 12.7 ± 0.7 k yr from one date), omitting 4 other OSL dates (and published radiocarbon chronology) with no explanation. Three samples (97.5, 100, 105 cm) produced a spike in microspherules at 100 cm.	Wittke et al., 2013a† Meltzer et al., 2014††
	Confused regression analyses.	
	Pt spike below Dalton artifacts and OSL date (12.8–11.4 k yr BP), at base of 10 cm thick “YDB?” zone (12.9–12.3 k cal yr BP).	Moore et al., 2017†
Comment	No direct dating of the purported YDB with “impact markers.”	
Flamingo Bay, SC	Artifact correlation based on **“generalized archaeo-stratigraphy for the downslope portion of the main excavation block”** (Moore et al., 2017, SI fig. 12 caption). Pt from **“Paleoindian and Early Archaic components within the same stratigraphic zone or with very little separation”** (Moore et al., 2017, SI p 5) **“The large Pt and smaller Pt/Pd anomaly at Flamingo Bay is located at the same depth or just below Clovis artifacts”** (Moore et al., 2017, SI p 7).	Moore et al., 2017†
	“[G]**ood chronostratigraphic and/or archaeostratigraphic control”** according to Moore et al. (2017, p 3).	
Comment	No numerical age control for this site.	
Johns Bay, SC	Pt spike 17 cm below OSL dates of 10.5–8.1 k yrs. BP, 3 cm above date 18.7–14.7 k yrs. BP.	Moore et al., 2017†
	“[G]**ood chronostratigraphic and/or archaeostratigraphic control”** according to Moore et al. (2017, p 3).	
Comment	No direct dating of the purported YDB with “impact markers.”	
Kolb, SC	Pt spike 6 cm above OSL dates of 17.5–14.3 k yrs. BP and 9 cm below OSL 10.6–8.6 k yrs. BP.	Moore et al., 2017†

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Table 4 (*continued*)

Site or **Study Area**	**Dating & issues**	**References++**
	Pt from **“Paleoindian and Early Archaic components within the same stratigraphic zone or with very little separation”** (Moore et al., 2017, SI p 5), i.e., associated with mixed archaeological assemblage.	
Comment	“[G]**ood chronostratigraphic and/or archaeostratigraphic control”** according to Moore et al. (2017, p 3). No direct dating of the purported YDB with “impact markers.”	
Pen Point, SC	Pt spike below Dalton artifacts.	Moore et al., 2017†
Comment	“[G]**ood chronostratigraphic and/or archaeostratigraphic control”** according to Moore et al. (2017, p 3). No numerical age control for this site.	
Sheriden Cave, OH	A charcoal lens (“black mat”) in stratum 5a produced 2 YDB ages and spikes in carbon spheres and Pt, but the charcoal dates are anomalously young among a statistical population of 4 dates 11,550 ± 30 yrs. BP below 2 dates in stratum 5b averaging 11,100 ± 40 yrs. BP. Wittke et al. (2013a) state that a Clovis point was found within the 5a charcoal layer, but it was in overlying 5b layer along with samples producing a typical Clovis date. Multiple lenses are of YDB age (Meltzer et al., 2014; Kennett et al., 2015a) yet only one, clearly below a Clovis point, yielded purported impact indicators.	Wittke et al., 2013a† Kinzie et al., 2014† Moore et al., 2017†
Comment	“[G]**ood chronostratigraphic and/or archaeostratigraphic control”** according to Moore et al. (2017, p 3). Complex microstratigraphy and confused chronology.	
Squires Ridge, NC	2 Pt spikes, ~5 & ~10 cm above “YDB?” zone between OSL dates 10.3–8.9 k yrs. BP & 13.6–11.2 k yrs. BP. Early Archaic artifacts **“are found only within and above the deepest Pt anomaly”** (Moore et al., 2017, SI p 5), i.e., Early Archaic artifacts <Clovis age.	Moore et al., 2017†
Comment	**“[G] ood chronostratigraphic and/or archaeostratigraphic control”** according to Moore et al. (2017, p 3). No direct dating of the purported YDB with “impact markers.”	
	Firestone et al., 2007; **Usselo soils**	
Aalsterhut, Geldrop-Aalsterhut, Netherlands	Nanodiamonds across the Allerød-YD/GS1 boundary associated with charcoal documenting plant growth and burning 12.99–12.65 k yr BP. Bayesian analysis shows that the age falls within 1 standard deviation of the YDB layer at 12,800 ± 150 cal yr BP. Examined trace-elements across the Allerød-YD/GS-1 boundary. “**The volcanic component may be related to the Laacher See volcano eruption, whereas the cause of the extensive biomass burning remains unclear**” (Andronikov et al., 2016a, abstract).	van Hoesel et al., 2012†† Kinzie et al., 2014† Andronikov et al., 2016a†
Comment	Dating of “impact markers” unclear.	
Lingen, Germany	Single date on charcoal 9 cm below spherule zone in surface of Usselo soil dated 11.31 ± 0.06 k ^{14}C yrs. BP (13.20 ± 0.08 k cal yrs. BP).	Wittke et al., 2013a† Kinzie et al., 2014†
Comment	No direct numerical age control for YDB with “impact markers.”	Holliday et al., 2014††
Lommel, Belgium	Date of ~12.8 k cal yrs. BP cited by Firestone et al. (2007) as coming from Lommel is from type Usselo section ~160 km away in the Netherlands. “[W]**ell dated”** and of “**long-established archaeological and paleontological significance”** (Firestone et al., 2007) but those claims are not supported.	van Geel et al., 1989††† Firestone et al., 2007† Wittke et al., 2013a† Kinzie et al., 2014† Holliday et al., 2014††
Comment	No direct numerical age control for YDB with “impact markers.”	
Lommel Maatheide, Belgium	Examined trace-elements across the Allerød-YD/GS-1 boundary. Charcoal dated 13.44–13.25 k yr BP. **“The volcanic component may be related to the Laacher See volcano eruption, whereas the cause of the extensive biomass burning remains unclear”** (Andronikov et al., 2016a, abstract).	Derese et al., 2012††† Wittke et al., 2013a† Andronikov et al., 2016a†
Comment	No direct numerical age control for YDB with “impact markers.”	
Ommen, Netherlands	Age of spherule zone dated by a single sample to 13.33 ± 0.06 cal ka **“somewhat older than expected”** (Wittke et al., 2013a, SI fig. 12 caption).	Wittke et al., 2013a† Kinzie et al., 2014†
Comment	Purported impact markers dated to ~ 13 k cal yrs BP.	Holliday et al., 2014††
Lommel Molse-Nete, Belgium	Examined trace-elements across the Allerød-YD/GS-1 boundary. Charcoal dated 13.50–13.17 k yr BP. **“The volcanic component may be related to the Laacher See volcano eruption, whereas the cause of the extensive biomass burning remains unclear”** (Andronikov et al., 2016a, abstract).	van Hoesel et al., 2014†† Andronikov et al., 2016a†
Comment	Purported impact markers dated > YDB.	
Lutterzand, Netherlands	Examined trace-elements across the Allerød-YD/GS-1 boundary. Charcoal dated 12.56–12.24 k yr BP. **“The volcanic component may be related to the Laacher See volcano eruption, whereas the cause of the extensive biomass burning remains unclear”** (Andronikov et al., 2016a, abstract).	Vandenberghe et al., 2013††† Andronikov et al., 2016a†
Comment	Purported impact markers dated < YDB.	

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Table 4 (*continued*)

Site or **Study Area**	**Dating & issues**	**References++**
	Northern Mexico[1]	
Lake Acambay	Black mat dated to $<$14.2–13.9 k yr BP and $>$ 9.5 k yr BP.	Israde-Alcántara et al., 2018†
Comment	Age of black mat and purported impact indicators is uncertain.	
Cedral	Black mats dated to ∼12.2 k, ∼10.6 k, ∼9.5 k yr BP	Israde-Alcántara et al., 2018†
Comment	No purported impact indicators.	
Lake Chapala[2]	Black mat dated to $<$15.0 k yr BP.[1]	Israde-Alcántara et al., 2018†
Comment	No direct dating of the purported YDB with "impact markers."	
Lake Cuitzeo, Michoacán[3]	Six dates that bracket or are directly on purported YD/GS-1 zone and YDB are in correct stratigraphic order but rejected because they are **"older than the** [age/depth] **interpolation predict"** (Israde-Alcántara et al., 2012, p E739). Age of zone with purported impact indicators unknown but possibly ∼ 30,000 ^{14}C yrs BP. Black mat dated to ∼12.6 k yr BP.	Israde-Alcántara et al., 2012† Wittke et al., 2013a† Blaauw et al., 2012†† Meltzer et al., 2014†† Kinzie et al., 2014† Israde-Alcántara et al., 2018†
Comment	Age of black mat and purported impact indicators $<$ YDB.[3]	
Tocuila	Black mat dated to ∼12.7 k y BP but bone in inset channel fill is dated to∼13.2–12.8 k yrs. BP.	Israde-Alcántara et al., 2018†
Comment	Age of black mat and purported impact indicators is uncertain.	
	Other Investigations	
Abu Hureyra, Syria	Wittke et al. (2013a) and Bunch et al. (2012) differ in their identification of the YDB layer. 3 of 16 ages not used in regression for unspecified reasons (corrected from Holliday et al., 2014) in statistical analyses of Bunch et al. (2012) and Wittke et al. (2013a). Spatially scattered ages, varying in absolute elevation and distance from one another, were integrated by Wittke et al. onto a common absolute vertical scale. Method of integration is not specified nor is an explanation why 4 ^{14}C dates were omitted, rendering the scale of these interpolations entirely arbitrary and with it their statistical results. Statistically interpolated depths for the YDB are not reproducible.	Bunch et al., 2012† Wittke et al., 2013a† Kinzie et al., 2014† Meltzer et al., 2014††
Comment	No direct dating of the purported YDB with "impact markers."	
Big Eddy, MO	30 Accelerator Mass Spectrometry [AMS] dates available but only 7 "key dates" with no depth information provided by Wittke et al. (2013a). Meltzer et al. (2014, SI table S6) show reversals among these 7 dates (not addressed by Wittke et al., 2013a). Claimed YDB zone 327-335 cm (Wittke et al., 2013a) but mixing of charcoal suggested, leading to problems in **"accurately dating charcoal layers"** (Wittke et al., 2013a, SI p 5); 5 ^{14}C ages from 315 to 347 cm overlap with YDB at 1 standard deviation (sd). 2 of 3 ^{14}C ages from 327 to 335 cm do not overlap YDB at 1 sd. Purported YDB impact proxies are from a zone 8 cm thick in soil Bt horizon; pedogenic processes likely mixed particulate material, negating dating precision. Statistically and chronologically flawed age-depth interpolations (see Meltzer et al., 2014); recalculation shows that all layers 320–348 cm have median ages that fall within the span of 12,900 ± 100 cal yr BP.	Hajic et al., 2007††† Lopinot et al., 1998, 2000††† Wittke et al., 2013a† Meltzer et al., 2014††
	Pt spike at 110 cm from zone identified as "YDB?" below Dalton artifacts, OSL date OSL 12.8–11.4 k yrs. BP, and ^{14}C date of 12.9–12.3 k cal yrs. BP. **"**[G]**ood chronostratigraphic and/or archaeostratigraphic control"** according to (Moore et al., 2017, p 3).	Moore et al., 2017†
Comment	No direct dating of the purported YDB with "impact markers."	
Bull Creek, OK	Organic matter from buried soil; no data on sampling or stratigraphy. Sampling thickness from (Bement et al., 2007, table 1). Very high concentration (190 ppm) of nanodiamonds in undated layer ∼10 cm below layer with ^{14}C date of 11,070 ± 60 yrs. BP (∼13.0 k cal yrs. BP) and in layer $<$3000 yrs. BP, however neither concentration peak can be reproduced.	Kennett et al., 2009a† Kinzie et al., 2014† Bement et al., 2014† Sexton, 2016††
Comment	Irreproducible nanodiamond peaks accepted as correct and they do not occur in the layer dated to the YDB.	
Hall's Cave, TX	Stafford et al. (2009) is an AGU conference abstract that purports at the YDB **"an abundance of nanodiamonds (5 different allotropes), aciniform soot at 2400 ppm, magnetic spherules, and carbon spherules".** Kinzie et al. (2014) conspicuously do not mention Hall's Cave although five out of six of the coauthors of Stafford et al. (2009) were coauthors of Kinzie et al. (2014). Wittke et al. (2013a) make no mention of Hall's Cave, although three out of six of the coauthors of Stafford et al. (2009) were coauthors of Wittke et al. (2013a). Wolbach et al. (2018a) claim spikes at the YDB in aciniform carbon and Pt, but did not identify the Pt source. Waters et al. (2021) provide a comprehensive summary of the geology and archaeology of the site with Stafford as co-author but make no mention of the YDIH other than noting the AGU abstract. Thus, most of the results of Stafford et al. (2009) appear abandoned or irreproducible. Sun et al. (2020, 2021) showed the highly siderophile element abundances, including Pt, within the YDB at Hall's Cave are consistent with volcanic signatures and not an impact.	Stich et al., 2008† Stafford et al., 2009† Wolbach et al., 2018b† Sun et al., 2020, 2021†† Waters et al., 2021††

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Table 4 (*continued*)

Site or **Study Area**	**Dating & issues**	**References++**
Comment	Well-dated site with an uniquely well-resolved stratigraphy, however most results purported in support of the YDIH are presumably abandoned (or not reproducible) and inconsistent with YDIH.	
Hiscock site, NY	Iron-rich spherules at two levels in **“the Pleistocene horizon”** and **“late-Holocene levels”** (Laub, 2010, p 168) but links no radiocarbon dates to those samples and further discusses the poor numerical age control at the site (p 169). Laub (2010, p 168) also notes that none of the spherules **“have the surface smoothness of those figured by Firestone and his colleagues”** and that the older ones **“may have originated geochemically.”** He concludes (p 169) **“it is surprising that evidence of the putative catastrophe is not more obvious here.”**	Laub, 2010††† Pino et al., 2019, SI†
Comment	No direct dating of the purported YDB with “impact markers.”	
Indian Creek, MT	An unnamed mammoth site (Indian Creek?) with a “black mat” with claimed impact indicators dated to **“11.5 ka (C14) before present”** and at the Indian Creek site itself, **“below the cultural layers and below a 11.2 ka (C14) volcanic ash layer”** were more alleged impact indicators (Baker et al., 2008, abstract).	Baker et al., 2008† Kinzie et al., 2014† Pino et al., 2019, SI†
Comment	One or two localities with claimed impact markers older than the YDB. This study was only reported in an abstract with no subsequent publication of data and discussion.	
Kangerlussuaq, Greenland	Kurbatov et al. (2010, p 750) described, **“Our sampling** [of the surface exposed ice section] **was guided by the presence of clear, dust-poor ice of assumed early Holocene age, stratigraphically higher than a sharp visual change into dusty ice of inferred YD age.”** Identification was based on expectation and dating was based on assumption. Furthermore, layers were subjected to deformation and shearing. Results of subsequent field sampling never published.	Kurbatov et al., 2010† Kinzie et al., 2014†
Comment	No numerical age control for this site.	
Lindenmeier, CO	Nanodiamonds from base of “black mat” (stratum D) at contact with loess (stratum C). Contact is a disconformity represented by a soil in C. At the sampling section with “YDB” the contact is also on the slope of the paleo-valley and is erosional. The contact could represent up to 1000 ^{14}C years.	Kinzie et al., 2014† Holliday, 2016††† Holliday et al., 2020††
Comment	No direct dating of the purported YDB with “impact markers.”	
Melrose, PA	Bunch et al. (2012) recovered an OSL date (16.4 ± 1.6 ka) from beneath the purported impact layer, assumed **“a modern age for the surface layer”** (p E1905) or **“0 cal ka BP”** (fig. SI S5) and used a “linear interpolation” to date the alleged YDB zone. Such a model must assume continuous deposition, which can’t be known given that the surface could have been stable or eroded. Based on one date with a large standard deviation and an unsubstantiated assumption about the age of the surface, the resulting “date” cannot be accepted as meaningful.	Bunch et al., 2012† Wu et al., 2013† Kinzie et al., 2014† Meltzer et al., 2014††
	No direct dating of the purported YDB with “impact markers.” OSL date associated with nanodiamonds 11,701 ± 1846 ka.	Kinzie et al., 2014†
Comment	Very low precision dating of the purported YDB with “impact markers.”	
Mt Viso, Italy	Till considered to be of late glacial age with soil (and “black mat”) buried by till assumed to be YD/GS-1 age. Black mat interpreted as evidence for YDIH.	Mahaney et al., 2022†
Comment	No numerical age control for this site or the regional glacial record.	
MUM7B, Venezuela	Claimed YDB layer “black mat” is iron-manganese concretion. Peat 21 cm below black layers date 13.8–13.4 k, 14.1–13.3 k and 13.5–13.1 k cal yrs. BP.	Mahaney et al., 2008, 2010a†
Comment	No numerical age control for purported YDB.	
Newtonville, NJ	Magnetic microspherules found in two layers: the upper purported YDB zone is undated; the lower zone is dated by OSL at 16.8 ± 1.7 k yrs. BP.	Wu et al., 2013† Kinzie et al., 2014†
Comment	No numerical age control for purported YDB with “impact markers.”	
Paw Cove, MD	Claimed YDB zone and Clovis artifacts are on eroded surface. Artifacts represent a lag deposit (Lowery, 2009, p 56). YDB level estimated based on correlation with “nearby” Clovis artifacts.	Lowery, 1989† Lowery et al., 2010† LeCompte et al., 2012†
Comment	No numerical age control for this site.	
Pilauco, Chile	Purported impact indicators at contact between organic-rich mudstone with thin, muddy laminae buried by woody peat.	Pino et al., 2019†
Comment	Bracketing numerical dates and modelling places the contact and the “indicators” at the YDB.	
Santa Maira Cave, Spain	Carbon spherules and nanodiamonds from layer “inferred” to be YDB because it is at base of “darker” zone and bracketed by ^{14}C ages of 11.9 k and 14.5 k cal yr BP. “Abundant charcoal” is reported from purported YDB but no dates. Previous research produced no YDC dates due to “erosive hiatuses.”	Kinzie et al., 2014†
Comment	No numerical age control for purported YDB with “impact markers.”	
Stara Jimka, Czech Republic	^{14}C dates of 11.4, 12.7, 12.9, 13.1 cal ka BP (top to bottom; CALIB rev8.2); “YD” plotted <12.7 ka (Kletetschka et al., 2018, table 2).	Kletetschka et al., 2018†

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Table 4 (*continued*)

Site or **Study Area**	**Dating & issues**	**References**++
Comment	No clear link between "impact markers" (at very low levels) and ^{14} C dates. No numerical age control for purported YDB.	
Talega, CA	Stratigraphic context and discussion of radiocarbon dates are confused and contradictory. 3 dates listed but at least 7 alluded to without explanation. Purported YDB layer dated to 13.02 ± 0.15 ka (i.e., >YDB) with no clear explanation of dating. The layer located 3.1 m above is older than or at YDB age with no explanation and no claimed impact indicators. Statistically and chronologically flawed age-depth interpolations.	Wittke et al., 2013a† Meltzer et al., 2014††
Comment	No numerical age control for purported YDB with "impact markers."	
Watcombe Bottom, UK	Nanodiamonds from top of "fossil Rendzina soil" (thus a soil and not a geologic deposit) is YDB (Kinzie et al., 2014, SI p 13). ^{14}C date of 11,690 ± 120 yr BP from lower soil. The soil formed in poorly stratified chalk muds and rubble produced by frost-shattering and solifluction (gradual mass wasting). At other localities Preece (1994) interpret the soil as forming quickly and incorporating charcoal spanning pedogenesis, yielding dates of 10,900 ± 120, 11,100 ± 110, 11,220 ± 110 and 11,240 ± 110 ^{14}C yr BP with reversals. Preece (1994) illustrate the upper Rendzina in the region as affected by erosion and/or solifluction.	Preece, 1994††† Kinzie et al., 2014†
Comment	No numerical age control for purported YDB with "impact markers."	
White Pond, SC	Moore et al. present a YBD zone 224-213 cm with depositional hiatus until the early Holocene based on Bayesian modelling although there are reversals among the ^{14}C date sequence in this zone. Krause et al. show no depositional hiatus. Pt spike between ^{14}C dates of 10,640 and 10,920 yrs. BP.	Krause et al., 2018††† Moore et al., 2019†
Comment	Confusing context of purported impact indicators, field context, and modeled dating.	
Wonderkrater, South Africa[4]	Platinum spike claimed to be of YDB age. Statistical age modelling includes multiple models applied. Varying and unclear reasons why some dates are rejected, and others accepted. The reason why one model is accepted, and others rejected is not clear. The model is based on dating in core B3 from the site. In examining Scott (2016), understanding where the B3 core age model comes from is difficult. But if the age model selected by Thackeray et al. (2019) is correct, the Pt spike is after the YD/GS-1 onset.	Scott, 2016††† Scott et al., 2003††† Thackeray et al., 2019†
Comment	No numerical age control for purported YDB with "impact markers."	
Carolina Bays	Firestone et al. (2006) claims repeatedly that Carolina Bays are impact craters and purports the **"topsoil"** (p 346) of a Carolina Bay rim in Bladen County (B14) has peaks in glasslike carbon. Firestone et al. (2006, p 352) claim, **"in the T13 Carolina Bay, the high levels of Ir appeared throughout most of the 10 feet (3 meters) of the rim, making a strong case that some bays formed 13,000 years ago."** In (Firestone et al., 2007, SI table 4), a total of 15 Carolina Bays were purported to have a YDB layer with **"impact markers"**: Blackville, SC (T13); Howard Bay, NC (HB); Lake Mattamuskeet, NC (LM); Lake Phelps, NC (LP); Lumberton, NC (L28, L31, L32, L33); Moore City, NC (MC1); Myrtle Beach, SC (M24, M31, M32, M33); Salters, Lake, NC (B14); Sewell, NC (FS3). Firestone et al. (2006) misunderstand the difference between OSL and radiocarbon dates. They cite dates with low precisions of 10–50% and some dates are >12.k cal yr BP, while others are older. Firestone et al. (2007, p 16019) subsequently realized the ages of the Carolina Bays vary, and sought to assert that the supposed YDB layer found in 15 of the bays dated to 12,900 years BP. They based this assertion on the fact that the markers found therein were identical to those found elsewhere dated to 12,900 years BP but produced no stratigraphic or geochronologic data.	Ivester et al., 2003††† Firestone et al., 2006, 2007† Brooks et al., 2010††† Meltzer et al., 2014††
Comment	OSL dating shows that Bays formed 140-120 ka and ~ 12-50 ka and all Bays are on LGM and older age landscapes. Bay rims have a wide range of ages.	
Blackville, SC	OSL date of 12,960 ± 1190 yr BP is from spherule zone in sand rim along Bay on erosional disconformity with potential mixing of dated sediments. Dates have low OSL precision and OSL age reversal among the three published dates, but this is not discussed or addressed.	Bunch et al., 2012† Wittke et al., 2013a† Kinzie et al., 2014† Meltzer et al., 2014††
Comment	No numerical age control for purported YDB with "impact markers."	
Kimbel Bay, NC	Spherule zone 10 cm above the youngest OSL age of 25.5 ka in sand rim along Bay. No indication of depositional or weathering hiatus, nor erosion. The time gap not considered in age estimate nor discussed or otherwise considered. Age reversal not discussed.	Wittke et al., 2013a† Kinzie et al., 2014† Meltzer et al., 2014††
Comment	No numerical age control for purported YDB with "impact markers."	
Flamingo Bay, SC Johns Bay, SC White, Pond, SC	See Moore et al. (2017), and other investigations above.	
High Plains Playas	Holliday et al. (1996) report dates of 16,000 to 20,000 years ago underlying "salina" (playa) basins in the Panhandle of Texas. Firestone et al. (2006) claims those basins may have blown out of the soft earth by flying debris from the extraterrestrial explosions to the northeast.	Firestone et al., 2006, p 216–217†

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Table 4 (*continued*)

Site or **Study Area**	**Dating & issues**	**References**++
Comment	Data from 23 playa basins show that **"Lacustrine mud accumulated in at least some basins at some time throughout the past 15,000 yr, and locally much earlier"** (Holliday et al., 1996, p 963).	Holliday et al., 1996, p 963††† Holliday et al., 2014†† Meltzer et al., 2014††

++ Key to references: †Key YDIH studies; ††Key Critiques; †††Other related studies Kinzie et al. (2014) report on claimed nanodiamond impact indicators from **"22 sites… and independent researchers conducted six studies, for a total of 24 sites"** (p 480) and note (their fig. 3 caption) **"Most of the six independent studies did not quantify NDs** [nanodiamonds] **at or near the YDB."** But they do not clearly indicate whether the data among the 22 sites is entirely theirs or determined by other investigators they reference. We attribute data to their work only where we are confident that they generated the data.

[1] Northern Mexico: No information was provided by Israde-Alcántara et al. (2018) on the thickness or depths of the sample zones for either the radiocarbon dates or the microspherules. Thus, the radiocarbon dates could not be linked to microspherule spikes.

[2] Lake Chapala, radiocarbon date of ~10,550 ^{14}C yr BP for this section in fig. 2 of Israde-Alcántara et al. (2018) is apparently a mistake based on their table 1 and text.

[3] Lake Cuitzeo: Kinzie et al. (2014, p 481) report **"To further test the age model, we acquired a new accelerator mass spectrometry ^{14}C date (NOSAMS-71325: 10,550 ± 35 RCYBP, 12,897 ± 187 cal BP) on organic sedimentary carbon collected above the YDB layer in a nearby exposed shoreline sediment sequence, lithologically correlated with the lake core."** But three problems arise. 1) In addition to the sample coming from a **"nearby** [exposure]" rather than the core, Israde-Alcántara et al. (2018, fig. 2) plot the sample at the base of the black mat. 2) The calibration of "**10,550 ± 35 RCYBP**" is presented as **"12,580 ± 55 cal BP"** in appendix b of Kinzie et al. (2014) on p 4 and in fig. B2. Further, 3) Both calibrations are presented by Israde-Alcántara et al. (2018); the younger in their table 1 and the older in their text (p 68).

[4] See also Endnote 6.

> **measured, sampled, and dated the small section at that location."**

Further, Scott et al. (2017, p 37) explained:

> **"Wittke et al. [2013a] claim that 'coordinates, photographs, stratigraphic descriptions, and radiocarbon ages presented in their papers… conclusively demonstrate that none of their samples collected were taken from the same stratigraphic section studied by Kennett et al. [2008a].' On the contrary, our Locality III is identical to their locality AC003…. Furthermore, material from AC 003 was sent to the senior author in March 2007 by G. James West (via John Johnson) with a request to report on the charcoal. Lithological logs of other Arlington sections and radiocarbon data are given in Hardiman et al. (2016)."**

Sweatman may not be familiar with sedimentary logs nor their interpretation in a geologic context. (Sweatman, 2021, p 9) misconceived argument is that explanation **"is misleading, as the site and samples depicted in these photos are all labelled SRI-10 to SRI-13, whereas the relevant samples in Daulton et al. (2010) all have labels SRI-09. So it is quite clear the nanodiamond samples in Daulton et al… did not, in fact, come from the same sediment bank sampled by Kennett et al. (2009b)."** These comments show a misunderstanding of sample numbering. ENDNOTE **5.**

Sweatman (2021, p 20) also asserts, **"when attempting to reproduce purported evidence for a cosmic impact, it is important that similar samples from *exactly the same stratum at the same site* are taken. Daulton et al.'s (2010) search for nanodiamonds appears to be hamstrung by this issue, an error these researchers seem determined not to admit…"** (emphasis added). Scott et al. (2017, SI fig. S1) unambiguously establish that Scott et al. (2010, 2017) and Daulton et al. (2010, 2017a, 2017b) sampled precisely the same section as field site AC003 of Kennett et al. (2009b). But even if it was not **"exactly the same stratum"**, Sweatman's (2021) assertion is ridiculous because impact proponents claim that at the YD/GS-1 onset a layer of impact markers was deposited across North America to Europe. *If* this were the case, then certainly an YDB-dated layer containing those markers would have covered the entire island of Santa Rosa.

Sampling methods used by YDIH advocates at the Arlington Canyon section are also problematic. Clearly the proponents are unfamiliar with sampling in fluvial sediments or the potential pitfalls in the interpretation of the data obtained from such sediments. All the sections studied by the YDIH proponents are represented by a single set of vertical samples. There are no lateral duplicates. In lacustrine sections this may be satisfactory as sediments are deposited in horizontal layers. However, several observations may be made concerning the sampling at Arlington Canyon in particular. 1) The section shows considerable facies variation, both vertically and laterally (see Scott et al., 2017, fig. 2). 2) Samples only 1 m lateral to those sampled would have given quite different results. This is clearly shown by the lateral duplicate sampling performed by Scott et al. (2017, SI fig. S4). 3) Obtaining quantitative data on the carbonaceous material by quoting particles per unit weight is meaningless as the facies range from pebble conglomerates to silty sands. In addition, charcoal particles break up during processing making number-based quantification meaningless (Scott et al., 2017, fig. S6). 4) No samples were obtained below the so-called YDB layer so the position of its base cannot be fully determined. In addition, the "YDB layer" was not clearly documented to be a layer as no lateral samples were collected nor was the layer unique in the section. 5) Some of the organic material was concentrated by fluvial processes (see fig. S4 of Scott et al., 2017). 6) There are many sections within Arlington Canyon that could have been studied (see Hardiman et al., 2016) besides the one by Kennett et al. (2009b), but none of the other sections were examined by the YDIH proponents. None of the features mentioned, therefore can be demonstrated to be unique to the proposed "horizon".

4.2. Selective sampling at Abu Hureyra

Very selective sampling clearly skews interpretations. This is exemplified at the archaeological site of Abu Hureyra, Syria. It produced what has been described as **"scoria," "meltglass"** or **"AH** [Abu Hureyra] **glass"** (i.e., scoria-like objects, SLOs) coating occupation layers and dated to about the time of the YDB (Bunch et al., 2012, abstract; Moore et al., 2020, abstract). Bunch et al. (2012) suggested that the material was the result of a cosmic impact or airburst. Thy et al. (2015) followed up with investigation of "scoria" from multiple levels at Au Hureyra and three other archaeological sites in the region. Sweatman (2021, p 8) states "**Thy et al. (2015) selected a few SLO particles from…Abu Hureyra site, although only one… was from the burned 'Level II' layer examined by Bunch et al. (2012) thought to represent the YDB. On the basis of this single particle they estimated a melting temperature close to 1200°C, consistent with the lower end of the temperature range in Bunch et al….. *Moreover, they found similar particles at other levels at Abu Hureyra and at other archaeological sites across Syria with similar radiocarbon ages…* Moore et al. (2020)**

re-examined SLOs from around the burned layer, Level II, at Abu Hureyra, confirming Bunch et al.'s... original findings. They document a wide range of silica-rich particles... showing signs of bubbling indicating temperatures in excess of 2200°C... This effectively confirms a cosmic impact at Abu Hureyra, recorded in the level II burned layer" (emphasis added). This statement is misleading in terms of both sampling and fact. The problem here is noted in the quote above from Sweatman. There are younger layers of the scoria at Abu Hureyra (dating 12.3 k-11.5 cal ka BP) (Moore et al., 2000) and at other archaeological sites in the region: Qaramel (12.3 k-11.5 cal ka BP); Jerf el Ahmar (11.2 k-10.8 cal ka BP); and Murebet (11.3 k-10.8 cal ka BP) (Thy et al., 2015). Thus, scoria is relatively common and does not require an extraordinary source such as an impact. The failure of Moore et al. (2020) is that they did not perform a comparative study of the younger scoria, only a study of alleged YDB samples (Table 4).

5. Inadequate dating and stratigraphic context

As suggested above, a variety of unsupported or misleading claims about dating of sites critical to the YDIH permeate the early publications supporting the hypothesis (Firestone and Topping, 2001; Firestone et al., 2006, 2007). This sort of dating misinformation carries through many subsequent papers supporting the YDIH (Table 4). The two most critical issues of chronology are the age of the start of the YDC and the claim that purported indicators are well dated to the lower boundary of the YDC (i.e., the YDB) (see also Section 2).

5.1. Befuddled dating the beginning of the YDC

A theme that runs through the YDIH literature is the effort to determine, using terrestrial radiocarbon dates, the precise age of the beginning of the YDC. This is an unnecessary effort to solve another nonexistent problem. With the development of the Greenland Ice Core Chronology 2005 (GICC05) that placed the GRIP, NGRIP, and GISP2 ice cores on a common time scale (Rasmussen et al., 2006, 2014), the age of the onset of the last stadial (Fig. 1), termed the Greenland Stadial 1 (GS-1), was established as 12,846 ± 4 yr [BP 1950, GICC05 or 12,896 ± 4 yr [b2k, GICC05]. This is an age very close to that given by Mayewski et al. (1993) for the GISP2 record that played a role in the initial discussions of the YDIH (e.g., 12,859 ± 250 yr [BP 1950, Meese/Sowers]).

One of the general issues that arise when establishing correlations or tie-points among multiple records is the variations in age controls and chronologies of the records. This can be seen in the simple task of establishing the calendar age of the onset of YD/GS-1. The most precise chronology is that provided by the Greenland ice-core records, based on annual-layer counting and measurement, and which does not require "calibration" or conversion to an absolute age scale (as would radiocarbon ages). Rasmussen et al. (2014) give an age of 12,896 ±4 yr [b2k, GICC05] or 12,846 [BP 1950, GICC05], a refinement of the earlier estimate of Steffensen et al. (2008) of 12,900 yr [b2k, GICC05]. This age was determined by a statistical change-point analysis of δ^{18}O as well as other constituents of the NGRIP core and placed on the GICC05 chronology (Rasmussen et al., 2006), as described by Rasmussen et al. (2014) and Seierstad et al. (2014). The latter two references also describe the transfer of the GICC05 chronology to the older "Meese/Sowers" chronology of the GISP2 core (see Holliday et al., 2020, table 7 for chronology sources).

Cheng et al. (2020) analyzed a large suite of U-Th dated speleothems, and again using change-point analysis, determined an age of 12,870 ± 30 yr [BP 1950, U-Th] (or 12,920 yr [b2k, U-Th]) (see also Section 11). Consequently, even in relatively well-dated ice-core and speleothem records not dependent on calibration, there is a range of about 25 years (or more when including uncertainties) in the estimated age of the onset of the YD/GS-1 using sources other than conventionally calibrated ^{14}C dates. Recently, Reinig et al. (2021) determined the age of the Laacher See eruption using multiparameter radiocarbon age calibration (i.e., wiggle-matching to the Swiss Late Glacial Master Radiocarbon [SWILM-14C] datasets) to 13,006 ± 9 cal yr BP [1950] and placed the onset of the YD/GS-1 at 12,801 ± 12 cal yr BP [1950], or 12,851 cal yr [b2k]. The existence of multiple generations of chronologies for the ice cores, and more than one reference age (i.e., 1950 CE vs. 2000 CE "b2k") creates amplitude for making mistakes. See, for example, the plotting anomalies in Wolbach et al. (2018a) first noted by Holliday et al. (2020), and which were not fully addressed by Wolbach et al. (2020), or Sweatman's (2021, p 3) statement that the beginning of the YD/GS-1 in the GISP2 core was at **"10,890 BP."** This latter instance is likely a typo, i.e., it probably should have read "12,890 yr BP" (presumably yr BP 1950), but if the typo was in the calendar-age designation, i.e., if it should have read "10,890 BCE", then that would give a plausible age of 12,839 yr [BP 1950, Meese/Sowers]. Later, Sweatman (2021, p 19) states, **"No YDB site has yet been found to be obviously inconsistent with a synchronous event circa 10,785 ± 50 cal BP (2 sd)."** Again, this is problematical: the suffix **"cal BP"** implies a calibrated radiocarbon age, in which case **"10,785 ± 50 cal BP"** should probably be read as "12,785 cal BP", but it could also be the case that what intended was "10,785 ± 50 BCE", which gives a plausible age of 12,734 yr BP.

This situation can also lead to the adoption of overly casual approaches for aligning chronologies. For example, Sweatman (2021, p 5) notes that to compare platinum anomalies from near and far with those in the GISP2 core (i.e., Petaev et al., 2013a) (Fig. 2), **... the GISP2 ice core chronology must first be converted into a radiocarbon timescale. This is achieved by the GICC05 chronology. Essentially, according to the radiocarbon-aligned GICC05 chronology we should subtract around 80 years from GISP2 dates in the vicinity of the YD cooling (Svensson et al., 2008)"**. Leaving aside the question of why the calendric chronology of the ice cores should necessarily be converted to a radiocarbon one (i.e., **"inverse calibration"** which can create artifacts in the "uncalibrated" ages; Bartlein et al., 1995), there are three things that are wrong with this idea.

First, the GICC05 chronology is not **"radiocarbon-aligned"** (Sweatman, 2021, p 5), but is based on annual layer-counting and electrical conductivity as well as continuous-flow measurements of impurities in the GRIP and NGRIP ice cores (Rasmussen et al., 2006); *no radiocarbon dating was involved.* Second, the paper by Svensson et al. (2008) focuses on the extension of the GICC05 timescale from 42 ka to 60 ka and offers no simple prescription for adjusting from one chronology to another nor does it prescribe the 80-year offset as quoted by Sweatman (2021). Sweatman (2021) is unclear on the source of this offset value, perhaps from Southon (2002). The chronologies of the GRIP and GISP2 ice cores exhibit discrepancies first thought to arise from gradual accumulation of errors during counting of annual layers. Southon (2002) found most of the offset centered on two periods, an 80-year discrepancy near 3300–3400 yr BP and a second 100-year discrepancy near the onset of the YD/GS-1. Third, the difference in ages between the GICC05 chronology assigned to the GISP2 core, and the "original" (Mayewski et al., 1993) or Meese/Sowers chronology is 65 yr at the beginning of GS-1, and decreases to near zero at the end, making any simple prescription for converting from one ice-core chronology to another unsuitable in the first place (Fig. 3).

Similarly, Wolbach et al. (2020), while attempting to explain the discrepancy between the published GISP2 data and the plot in their fig. 3C of Wolbach et al. (2018a) argued that **"Mayewski et al. reported their data on a pre-GICC05 age scale that cannot be directly converted to GICC05 because the ice layers were subsequently recounted. Instead, the original GISP2 age scale must be interpolated to the GICC05 scale using the ages of depths that are common to both scales, yielding an average difference of 10–15 ice-layer years."** Fig. 3 demonstrates that this value, like Sweatman's 80-year offset, is not appropriate. We also note that two different age scales are used in fig. 3C of Wolbach et al. (2018a) to plot data from the same ice core. They are aligned at **"12600 Calendar yrs B.P."** (on their plot), but differ by about 20 yrs. at **"13000 Calendar yrs B.P.".**

The specific chronology assigned to a particular core does influence comparisons among cores but does not alter the relative position of samples within a core. Thus, the "Platinum spike" samples of Petaev et al. (2013a), at 1712.125 to 1712.250 m (82.2 ppt) and 1712.250 to 1712.375 m (27.6 ppt) in the GISP2 core, when placed on the GICC05 timescale still lie the better part of a meter above the level in the core dated to the onset of the YD/GS-1 (12,896 yr [b2k, GICC05]; 1713.00 to 1713.20 m in the GISP2 core), and therefore must post-date it (Fig. 2). If we take the midpoint depths of these samples, 1712.3125 m and 1712.1875 m, respectively, then these samples date to 12,874.6 yr [b2k, GICC05] and 12,871.3 yr [b2k, GICC05], 20 years after the YD/GS-1 beginning. Cheng et al. (2020, SI figs. S3, S8) further demonstrate that the Petaev et al. (2013a) "Pt-spike" occurs after the onset of the YD/GS-1 (but note that they present ages relative to 1950 CE).

A further complication in dating samples at the YDB (and throughout the LGIT) arises from the presence of "age plateaus" in the radiocarbon calibration curve (Bradley, 2015; Sarnthein et al., 2020). The age plateaus mark intervals when atmospheric ^{14}C temporarily increased, which could be related to increased production, but around the time of the YDC, is likely due to changes in atmosphere-ocean ^{14}C exchange (ocean ventilation) and in oceanic and atmospheric circulation (Stuiver et al., 1991). The age plateau in the latter half of the YD/GS-1 is comparatively well known, in which 1000 years of calendar time (12.4–11.4 ka) is compressed into 400 years of radiocarbon time (10.4–10.0 ka ^{14}C yr BP). The recent IntCal20 curve (Reimer et al., 2020) provides details on a second plateau at the transition between the B-A/GI-1 and YD/GS-1, in which 350 years of calendar time (13.10–12.75 ka) is compressed into 200 years of radiocarbon time (11.1 – 10.9 ^{14}C yr BP). The age compression has the effect of making a range of calendar ages appear to be more tightly clustered in radiocarbon time than they really are, thereby contributing to a false sense of synchroneity.

5.2. Pseudoarchaeological divined date of the impact event

Sweatman and Tsikritsis (2017a, p 233) ask in their abstract, **"Is Göbekli Tepe the 'smoking gun' for the Younger-Dryas cometary encounter, and hence for coherent catastrophism?"** Sweatman in his 2019 book *Prehistory Decoded* (see also Sweatman and Tsikritsis, 2017a, 2017b; Sweatman, 2017; Sweatman and Coombs, 2018; Sweatman, 2020) claims the date of the impact event is actually recorded on a carved stone pillar at the archaeological site of Göbekli Tepe, Turkey, **"Given what is now known about the Younger Dryas impact event, summarised in Chapters 3 to 5, dated by the platinum spike in the GISP2 ice core to 10,940 BC (using the ice core chronology), the most obvious possibility is that Pillar 43 records the date of this event"** (p 154). Pillar 43, known as the Vulture Stone, is part of a stone wall of Enclosure D and is described **"as one of the most artistically decorated pillars"** and **"most important artefacts in the world"** Sweatman (2019, p 31). Sweatman (2019, p 154) wrote, **"This idea is supported by the little headless man with an erection at the bottom of the Vulture Stone, who, presumably, indicates the date is associated with death"**. Sweatman (2019, p 154) further wrote, **"Pillar 43, likely represents the date 10,950 BC to within a few hundred years. This date is written using a symbolic representation of the position of the sun relative to some constellations on the summer solstice, where the constellations are represented as animal symbols in various poses."** These ideas appear to originate in the fanciful books by Andrew Collins (2014) *Göbekli Tepe: Genesis of the Gods: The Temple of the Watchers and the Discovery of Eden* and by Graham Hancock (2015) *Magicians of the Gods*, in which the later wrote, **"it seems reasonable to accept the summer solstice sunset, north of west, in the epoch of 9600 BC as a candidate for the scene depicted on Pillar 43."** The premise of Graham Hancock's book is that a highly advanced "lost civilization" was destroyed by an impact at the onset of the YD/GS-1, mirroring ideas first speculated upon by Donnelly (1883).

Archaeologists studying Göbekli Tepe have challenged these interpretations of Pillar 43. Notroff et al. (2017, p 60) wrote, **"The chronological frame Sweatman and Tsikritsis [2017a, p] (233, 246) suggest for Pillar 43 (10950 BC +/- 250 years) is still 700-1000 years older than the oldest radiocarbon date so far available for Enclosure D (which stems from organic material retrieved from a wall plaster matrix, ... While there is evidence for later re-use of pillars (see above), assuming such a long tradition of knowledge relating to an unconfirmed (ancient) cosmic event appears extremely far-fetched. So far, earliest radiocarbon dates from Göbekli Tepe coincide with the end of the Younger Dryas and not its onset."** Notroff et al. (2017, p 60–61) further wrote, **"Sweatman's and Tsikritsis' contribution appears incredibly arbitrary, considering images adorning just a few selected pillars"** and **"it is extremely problematic to pick out any one pillar and draw far-reaching but isolated interpretations while leaving out its context. A purely substitutional interpretation ignores these subtler but significant details. Details like the headless man on the shaft of Pillar 43, interpreted as a symbol of death, catastrophe and extinction…, silently omits the clearly emphasised phallus which must contradict the lifeless notion; rather, this image implies a more versatile narrative behind these depictions."**

5.3. Deficient dating of YDIH sites

Dating fundamentals in the context of the YDIH debate are summarized by Holliday et al. (2014, p 519) but bear repeating here from Holliday et al. (2020, p 70) given how crucial the issue is on both sides of the debate: **"Reliable and precise numerical age control for stratigraphic sections and associated samples is a key component of the YDIH debate. Proponents recommend 'very high chronological resolution to test the hypothesis'** [(Kennett et al., 2008a, p 2531)]. **Furthermore, they argue that 'only' radiocarbon dates with precisions of '<100 years, and preferably <60 years'** [apparently meaning ^{14}C years] **should be used for dating the YDB layer and complain that many dates employed by others have 'precisions from 200 years to >2,000 years'** [Kennett et al., 2008b, p E107]. **They also propose that the only valid dates are those processed with 'modern techniques [e.g., XAD … or ultrafiltration].' Given that the debate is about whether some sort of extraterrestrial event created an environmental catastrophe at a precise moment in geologic time, we agree that accurate and high-precision dating is essential for testing the hypothesis."** Kennett et al. (2015a, p 4344) wrote, **"In a test of synchroneity, it is ideal to have numerous, highly accurate, and precise dates to develop robust chronological models."** Samples for dating should also be from secure, unambiguous stratigraphic context. Unfortunately, no dates used to support the YDIH meet these requirements, and very few sections or samples are so accurately or precisely dated (Table 4). LeCompte et al. (2018, p 156) complain that YDIH critics **"do not use rigorous dating methods…"** A more critical issue is that the YDIH proponents do not meet this standard. Indeed, **"precisions from 200 years to >2,000 years"** (Kennett et al., 2008b, p E107) characterize the results presented by Kennett et al. (2015a).

Besides misunderstanding and mischaracterizing Clovis archaeology and extinctions (Sections 1, 3.1, 3.2, and 5.7), the basic dating for the hypothesis proposed by Firestone et al. (2007) was fundamentally flawed at the outset. More broadly, radiocarbon dating has been a long-standing conundrum for the YDIH (see Firestone and Topping, 2001; Southon and Taylor, 2002; Firestone, 2009a, 2009b, 2014; Gillespie, 2009; Melott et al., 2015). Firestone et al. (2007, p 16017) state that **"Ten Clovis and equivalent-age sites were selected because of their long-established archeological and paleontological significance, and, hence, most are well documented and dated by previous researchers."** This is not the case, as thoroughly discussed and documented by Meltzer et al. (2014) and summarized in Table 4. At best, only three of the sites (Blackwater Draw, Murray Springs, and possibly Daisey Cave) have reasonable age control and four have very poor to no age control (Chobot, Gainey, Morley, and Wally's Beach). Firestone et al. (2007) also allude to stratigraphic correlation with and sampling of their purported YDB in 15 Carolina Bays but provide no stratigraphic nor geochronologic data (Table 4).

Subsequent investigations of the YDIH produced additional attempts at age control based on field samples or models (e.g., Firestone et al., 2007, 2010a; Bunch et al., 2012; Israde-Alcántara et al., 2012; Kennett et al., 2009a, 2009b; LeCompte et al., 2012; Wittke et al., 2013a; Wu et al., 2013). From among these publications, the dating of 29 sites was evaluated by Meltzer et al. (2014). As summarized in their abstract (p E2162) **"Several of the sites lack any age control, others have radiometric ages that are chronologically irrelevant, nearly a dozen have ages inferred by statistically and chronologically flawed age–depth interpolations, and in several the ages directly on the supposed impact layer are older or younger than ~12,800 calendar years ago. Only 3 of the 29 sites fall within the temporal window of the YD onset as defined by YDIH proponents."** Further, Meltzer et al. (2014, p E2169) note **"We even relaxed one of their criteria, namely that 'only ^{14}C dates with measurement precisions <100 years, and preferably <60 years, should be used' in assessing the supposed impact chronology and its potential effects** [Kennett et al., 2008b]. **Had we applied it, we would have had to discard all luminescence ages and almost 60% of all radiocarbon ages used by YDIH proponents. Doing so would have instantly removed all radiometric age control from 11 sites and left 8 more with only a single age that in no case dates to the YD onset, meaning that 19 of their 26 sites with radiometric ages (group 1b) would become essentially free floating chronologically."** ENDNOTE 6.

The approach taken by Meltzer et al. (2014) was criticized by Sweatman (2021, p 15–16, 20). He wrote **"*no standard errors were provided for their calculations.* It is therefore not possible to determine if any of these age differences are significant. In a technical sense, therefore, their data is meaningless and their conclusions cannot be supported"** (emphasis added). This comment misses several key points and is factually untrue. The text (Meltzer et al., 2014, p E2167-E2168) includes discussion of error and uncertainty and the Supplemental Data clearly includes the standard errors in their calculations for all 29 YDIH sites reviewed. The ages just had to be fully outside the range of the YD/GS-1 onset age (~12.9 cal ka BP) to show that the markers of a YDB impact did not occur in the profile when/where they were supposed to occur. The issue is not whether their results and the cases they re-analyzed were significantly different. Further, in the main text and the Supporting Data, Meltzer et al. (2014) provide ample discussion of their methods and their statistical significance. Like the alleged issue of incorrect sampling at Arlington Canyon (Section 4.1), Sweatman (2021) and other YDIH proponents never address the

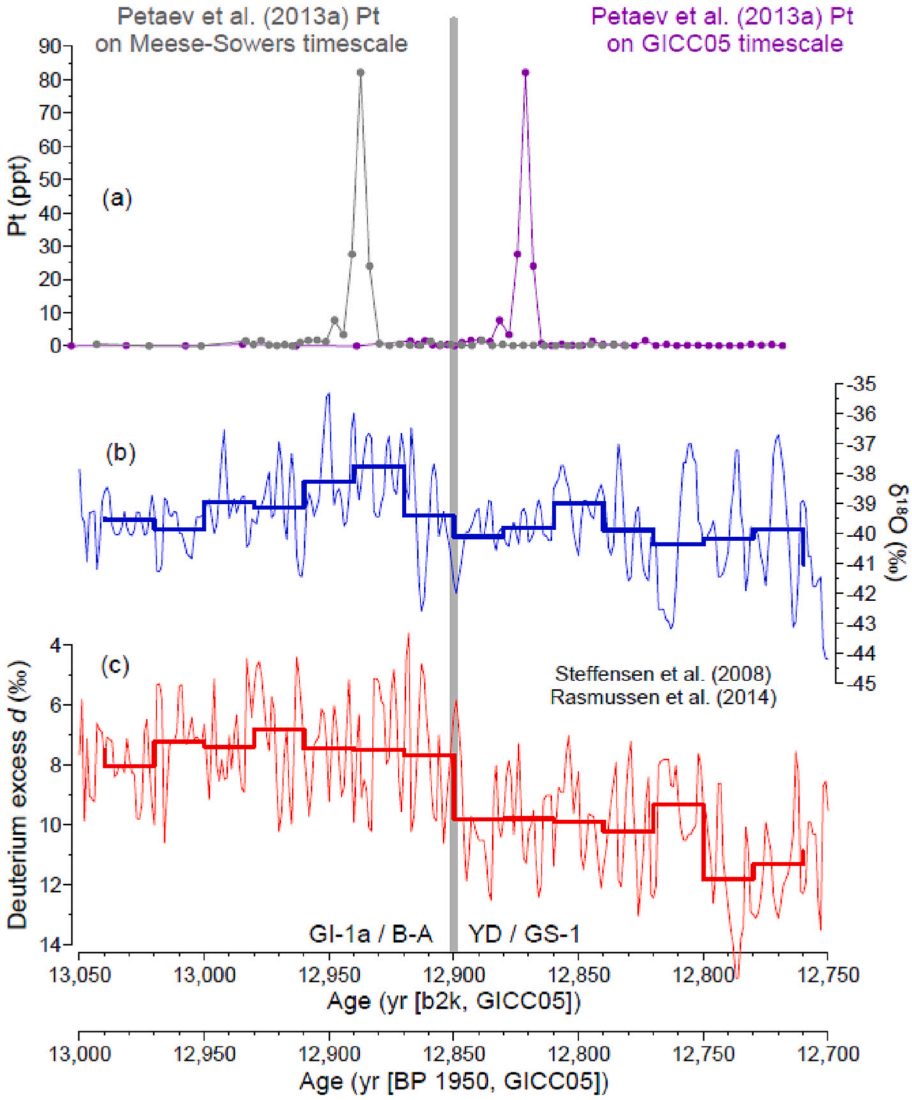

Fig. 2. Effect of chronology choice on the timing of the occurrence of the "Pt-spike" relative to the beginning of the YDC. (a) Petaev et al. (2013a) Pt data plotted using the original Meese/Sowers chronology (gray), and using the GICC05 chronology (purple). (b and c) Annual and bidecadal (20-yr average) δ^{18}O and deuterium-excess data (on the GICC05 time scale) used by Steffensen et al. (2008) and Rasmussen et al. (2014) to objectively define the transition between GI-1a and GS-1 (the beginning of the YDC). When plotted using the original Meese/Sowers chronology, the Pt-spike appears to precede the onset of the YDC, but when plotted on the common GICC05 chronology, the spike follows the onset by several decades. The data in panel (a) were assembled by combining data from Sheet 2 (Match points NGRIP-GRIP-GISP2) in the file 1-s2.0-S027737911400434X-mmc2.xlsx (Rasmussen et al., 2014; Seierstad et al., 2014), and data from the Supporting Information file of Petaev et al., 2013a). The data in panels (b) and (c) were obtained from the Supporting Online Material file of Steffensen et al. (2008), Sheet 3 (δ^{18}O and Ca 20 yrs. mean, NGRIP2) in the file 1-s2.0-S027737911400434X-mmc2.xlsx (Rasmussen et al., 2014; Seierstad et al., 2014), and the file https://www.iceandclimate.nbi.ku.dk/data/GICC05modelext_GRIP_and_GISP2_and_resampled_data_series_Seierstad_et_al._2014_version_10Dec2014-2.xlsx. See table 7 of Holliday et al. (2020) for other source-file URLs.

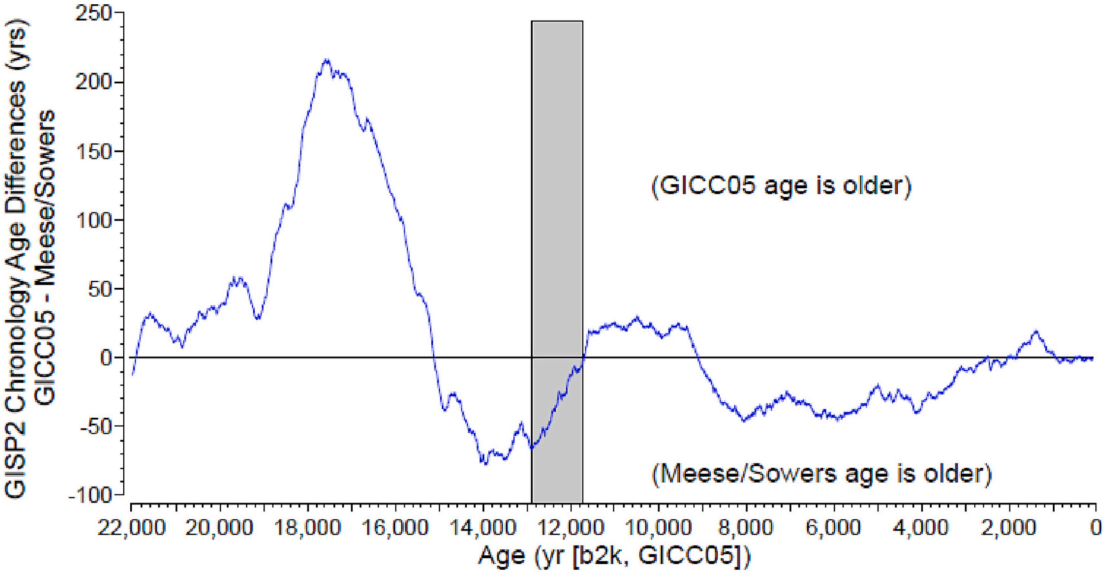

Fig. 3. Difference in assigned ages for samples in the GISP2 ice core using the GICC05 and Meese/Sowers chronologies. Values that plot below the horizontal line indicate that the assigned age in the original Meese/Sowers chronology are older than the assigned age in the GICC05 chronology, and values that plot above indicate that the assigned age in the GICC05 chronology is older than the assigned age in the Meese/Sowers chronology. For samples near the beginning of the YDC (gray-shaded area), the assigned ages in the original Meese/Sowers chronology are about 70 yrs. older than the assigned age in the GICC05 chronology, and the age differences decrease to about 0 yrs. by the end of the YDC, indicating there is no universal adjustment (i.e. scalar offset) value that could be used to reassign the ages of samples using the original Meese/Sowers chronology. The figure was constructed by combining data from Sheet 6 (Ages for GISP2 ion data) in the file 1-s2.0-S027737911400434X-mmc2.xlsx (Rasmussen et al., 2014; Seierstad et al., 2014), and data from the file gisp2_iond.txt (Mayewski et al., 1997). See table 7 of Holliday et al. (2020) for source-file URLs.

problematic nature of many of the samples, sample contexts, and resulting dates. In a technical sense, therefore, Sweatman (2021) simply dismisses a carefully laid out analysis using an irrelevant technicality.

Sweatman (2021) similarly criticizes a box plot (from Holliday and Meltzer, 2010, fig. 3; Holliday et al., 2014, fig. 2) of radiocarbon dates on the **"black mat"** (Section 6). Sweatman (2021, p 16) comments that "**much of the data in this plot is considered unreliable or is unpublished.**" ENDNOTE **7**. All data in the figure are directly from the *published* citations in Holliday and Meltzer (2010, fig. 3 caption). The integrity of those dates can be evaluated from the information within those cited sources. However, Sweatman (2021) does not elaborate on his perceived unreliability of **"much of the data."** Only two examples of problematic dates are offered, Naco and Willcox. The Naco date was included inadvertently in Holliday et al. (2014) and should be discarded. Sweatman's discussion of the dating of the Willcox section provides further circular reasoning (Table 2). Besides, Kinzie et al. (2014, p 478) cite unpublished data from three sites with nanodiamond horizons purported to support the YDIH and employ circular reasoning (Table 2).

Shortly after the publication of the dating critique by Meltzer et al. (2014), Kennett et al. (2015a) published a paper on a Bayesian chronological approach for estimating the ages of claimed YDB zones from many of the sites examined by Meltzer et al. (2014). Thirty-two sites are listed and discussed. The dating at nine is of such poor quality that Kennett et al. (2015a) could not include their results, but they were still claimed to be YDB via circular reasoning (Table 2). In a brief response to a critique of their Bayesian modeling, Kennett et al. (2015b, p E6723) dismiss the criticisms by stating that these "**same claims previously were presented in Meltzer et al. [2014] and were discussed and refuted in Kennett et al. [2015a] …**" But in fact, few of the criticisms enumerated by Meltzer et al. were even addressed by Kennett et al. (Table 1).

YDIH proponents (e.g., Wolbach et al., 2018a, 2018b; Sweatman, 2021; Mahaney et al., 2022; Powell, 2022) largely accept the work of Kennett et al. (2015a), suggesting that the YDB is synchronous across four continents, and thereby assert that the impact indicators were deposited synchronously over four continents. Mahaney et al. (2022, p 17) states that the conclusions of Meltzer et al. (2014) were **"refuted using Bayesian statistics by Kennett et al. [2015a]."** But like Kennett et al. (2015a), those proponents fail to recognize or refute the identification of many problems with the original site contexts of the dating discussed in detail by Meltzer et al. (2014; with over 60 pages of text and tables) and by Holliday et al. (2014, 2020). Sweatman (2021, p 16) focuses on eight **"high-quality sites"** (using the classification of Kennett et al., 2015a for ranking the chronologies at the 23 sites included in the age estimate). But several of these high-quality sites are problematic. Six were claimed to produce **"radiocarbon dates from directly within the YDB layer"** (Kennett et al., 2015a, table 1). However, one date is from the Bull Creek site, where the radiocarbon date is stratigraphically above a claimed "impact indicator" spike (Section 5.5, Table 5) and, ironically, YDIH proponents often use such spikes to identify the YDB (Table 2). Two dates (from Barber Creek and Blackville) have standard deviations ≥700 years (i.e., very poor precision) and thus no evidence whatsoever that the claimed YDB zone is of YDB age. Two other dates (from Aalsterhut, Lingen) are for the Usselo soil in northern Europe and of YDB age but selected from among scores of dates for dozens of sites falling far outside of the YDB (e.g., Hoek, 1997; Kaiser et al., 2009) (further discussed in Section 5.6). Picking out dates that are conveniently YDB age has no relevance to the YDIH debate and, moreover, is scientifically unsound. These examples of just the so-called "high-quality sites" well demonstrate that statistical analyses, Bayesian or otherwise, cannot overcome poor sample context, selection or precision, previously published flawed age–depth interpolations, or unexplained and inappropriate rejection of published dates. This is a classic example of the use of poor data resulting in the production of poor statistical results. ENDNOTE **8.**

Sweatman (2021, p 16) goes on to discuss standard deviations of the modelling results of Kennett et al. (2015a). He seems satisfied with modeled results at 2 or 3 standard deviations (sd) confirming a YDB age for a sample zone. But such statistical confirmation has nothing to do with stratigraphic or chronologic reality. An age model uncertainty of 100 years (1 sd) means that the age of a sample at 2 sd would be within a range of 400 years (or 800 years at 3 sd). Such broad age ranges cannot confirm the identification of a moment in time in the stratigraphic record. The ages of the authors of this commentary could be modelled to statistically date to the signing of the Declaration of Independence (1776 CE). The modelling could be statistically correct, but obviously meaningless.

Further problems with the results of the Bayesian age estimation are enumerated by Holliday et al. (2020, p 70–71, 75). **"Modeled age ranges with standard deviations of >300 years up to 2405 years are presented for layers of claimed impact indicators at nine sites of 'low quality' in terms of dating (their description in Kennett et al., 2015a, table 2). These layers are argued to represent the YDB based solely on the premise that if they could be YDB, they must be the YDB"** (Holliday et al., 2020, p 70-71, table 5) (Table 2). The conclusions of Kennett et al. (2015a) also suggests that the modelled dating of 12,255 ± 2405 cal yr BP for the sample zone at Melrose is somehow proof of a YDB age, which is of course preposterous. There is also a continuing and troublesome problem of omitting radiocarbon ages without explanation or analyses, which is a problem in much of the YDIH literature (enumerated by Meltzer et al., 2014; Boslough et al., 2015; Holliday et al., 2020) (Table 4).

Sweatman (2021) offers several contradictory and inconsistent concluding statements of sorts regarding the dating of the YDB. "**No YDB site has yet been found to be obviously inconsistent with a synchronous event circa 10,785 ± 50 cal BP (2 sd)."** (p 19; see also p 17). Besides the problematic date notation and apparent typographical errors (Section 5.1), this statement is a non sequitur. The YDB of the YDIH, especially when it is claimed to contain "impact markers," must (by definition) represent a synchronous moment in time, dated to ~10,785 ^{14}C yr BP. To be a "YDB site" the site must clearly contain a zone accurately dated to the YDB with high precision. As pointed out above and in Sections 5.4 to 5.7, and Table 4, many claimed YDB zones and evidence for a synchronous "event" are not so or are not clearly shown to be so. He also comments that dating at eight **"high-quality sites"** among the 23 dated "**is consistent with a synchronous event, which suggests all YDB sites are likely synchronous"** and **"it would be surprising if the others were not all eventually found to also be consistent"** (p 16). These are baseless assertions that defy fundamental principles of objective science. Taken to its logical conclusion, this assertion argues that there is no longer a need to date archaeological sites or geologic sections. We can just assume high-precision dating by correlation. Given that the burden of proof is on the YDIH proponents, dating results from one site or a group of sites does not confirm the dating at others. It only provides testable hypotheses, of the kind evaluated by Jorgeson et al. (2020) (Section 5.8).

5.4. Poorly dated platinum anomalies

Platinum (Pt) anomalies are used by YDIH proponents to unquestioningly support the YDIH (see Section 11). The Pt anomaly in the GISP2 ice core (Petaev et al., 2013a), which is proclaimed by Sweatman (2021, p 20) to be **"probably the most significant** [YDIH] **evidence so far"** post-dates the onset of the YD/GS-1 (see Section 5.1). Moore et al. (2017) report a widespread Pt anomaly at the YDB in 11 sites across North America. Powell (2022, p 24) and Sweatman (2021, p 17) uncritically accept those interpretations, the latter going so far as to conclude that the Pt zone can be used to "**unambiguously identify the Younger Dryas boundary at many locations around the globe."** Unfortunately, the dating reported by Moore et al. (2017) suffers from many of the same problems of chronology that plague the original YDB sites in Firestone et al. (2007) (Section 5.3) (Table 4). As is obvious from Moore et al. (2017, SI), only three sites seem to have direct dating for the Pt anomaly, but only near the YDB (Arlington Canyon; Murray Springs, Blackwater Draw). However, Arlington Canyon shows considerable vertical and lateral facies variation (Section 4.1). As for the other sites, five are indirectly inferred dates based on archaeology (two of which are based on optically stimulated luminescence (OSL) with large standard deviations), two have no numerical age control whatsoever, and one (Sheriden Cave) has a very confusing geochronological context (Table 4).

Repeating Moore et al.'s (2017) claim, Powell (2022, p 24) states that the Pt anomaly at three sites (Murray Springs, Blackwater Draw, Sheriden Cave) is **"associated with Clovis artifacts, representing the level at which the Clovis culture disappeared"** and other sites in the southeastern U.S. **"are poorly or not directly dated and lack the black mat, but do provide a coherent Clovis archaeological record."** Sweatman (2021, p 5) highlights the Pt anomaly at **"the Flamingo Bay site in South Carolina"** claiming **"a platinum abundance nearly 100 times the average crustal value was found in association with the youngest Clovis artefacts."** But the context of the archaeology and the Pt zones is both mixed and confused (Table 4 and Moore et al., 2017, p 6). Setting aside the YDIH requirement that the YDB represents the termination of Clovis, a basic tenant of many versions of the YDIH (Section 3.1), and the likelihood that the **"youngest Clovis"** would be post-YDB (Waters and Stafford, 2007; Waters et al., 2020), both authors neglect to note this comment from the main text in Moore et al. (2017, p 6) **"Early Archaic artifacts in the same levels as Clovis at Flamingo Bay"** indicate that **"these surfaces were stable to slowly accreting for several millennia before being buried incrementally through a combination of slopewash and aeolian accretion…"** The authors seem to believe the self-contradicting notion that a mixed archaeological assemblage spanning thousands of years of Clovis and Early Archaic time somehow provides a precise stratigraphic age indicator for the YDB.

Further, Moore et al. (2017, SI, p 5) observe **"Many sandy sites in the eastern US contain Paleoindian and Early Archaic components within the same stratigraphic zone or with very little separation (e.g., Topper, Kolb, and Flamingo Bay). As a result, Pt anomalies may be expected to occur in some sites within stratigraphic sequences that contain both Paleoindian and Early Archaic artifacts or with Early Archaic artifacts sitting immediately above YD-age sediments. Archaeological occupations at Squires Ridge, beginning with Early Archaic side-notched stone tool industries, *are found only within and above the deepest Pt anomaly* and only pre-cultural, archaeologically sterile zones lie underneath the deepest Pt anomaly. This is consistent with post-depositional processes and reworking of Pt-enriched sediments during periodic landform aggradation events during and after the YD event"** (emphasis added). This astonishing admission demonstrates that the Pt zone is mixed among Early Archaic (post-YD/GS-1) artifacts and thus does not represent a discrete stratigraphic context, nor (contrary to Powell, 2022, p 24) a **"coherent Clovis archaeological record"** (Table 4). This conclusion is yet another among publications where dating is based on assuming that because a zone *could be* YDB in age, it *must be* the YDB (Table 2). In other words, there is no clear age control. Given the dating problems noted above, the reference to "**widespread platinum abundance in bulk sediments near the base of YD-age black mats on at least four continents, confirmed by several independent research groups"** (Sweatman, 2021, p 17) is not supported by the evidence.

5.5. Inconsistent dating of nanodiamond zones

Firestone et al. (2007) claim recovery of nanodiamonds from the YDB but present no data. Kennett et al. (2009a) first presented data claiming recovery of nanodiamonds in purported YDB zones. They discuss six sites across North America but provide plots (their fig. 1) from only three sites (with no context on stratigraphy or depths) (Table 4). The Bull Creek site in Oklahoma is one of the three sites. Subsequent searches for nanodiamonds at Bull Creek were reported by Bement et al. (2014), Kinzie et al. (2014), and Sexton (2016). The research and discussions by the three groups is confusing and contradictory, however (see ENDNOTE 9 and Section 12.6).

Originally, Kennett et al. (2009a) claimed a nanodiamond peak of

100 ppb at 13.0 ± 1 ka cal BP with ≈ 25 ppb of nanodiamonds at ≈ 10 cm above that level (plotted in their fig. 1). Kennett et al. (2009a, fig. 1 caption) write, **“Stratigraphic profiles** [showing no stratigraphy] **on left show NDs only in the YDB”** and hence identifies the YDB as spanning those two levels. Subsequently, Bement et al. (2014, table 1) reported orders of magnitude greater abundance of nanodiamonds at 307–312 cm below surface (cmbs) but that layer was undated. Bement et al. (2014, table 1) identified the layer 298–307 cmbs as corresponding to the 100 ppb nanodiamond peak layer of Kennett et al. (2009a, fig. 1) (Table 5) and also dated it to 11,070 ± 60 ^{14}C yr BP (~12,990 cal yr BP). However, Bement et al. (2014, table 1) attributes to Kennett et al. (2009a), without explanation, a different shaped nanodiamond peak than what is plotted in Kennett et al. (2009a, fig. 1). The attributed peak has 100 ppb of nanodiamonds at 298–307 cmbs and 90 ppb at 307–312 cmbs. This attributed peak now overlaps with Bement et al.’s main peak position at 307–312 cmbs, whereas the peak plotted by Kennett et al. (2009a, fig. 1) plots 0 ppb of nanodiamonds below the main 100 ppb peak and does not overlap with Bement et al.’s main peak. Thus, Kennett et al. (2009a) illustrate a YDB spanning at least 10 cm at and above a date of ~13 cal ka BP whereas Bement et al. (2014, table 1) identify a YDB spanning 14 cm at and *below* the ~13 cal ka BP date, with the orders of magnitude predominant peak in nanodiamonds clearly below that date (see further discussion in ENDNOTE 9).

Kennett et al. (2009a, 2015a), LeCompte et al. (2012), Bement et al. (2014), Wolbach et al. (2018b, 2020), West et al. (2020a), Powell (2020, 2022), and Sweatman (2021) all accept Bull Creek as evidence in support of the YDIH. However, as discussed in Section 12.6, the purported YDB nanodiamond concentration measurements are not credible. But even if the concentration measurements are accepted as accurate, as believed by YDIH proponents, the depth of the nanodiamond peak layers are clearly below the presumed YDB at Bull Creek and are inconsistent with the YDIH. ENDNOTE 9.

Other claims regarding dating are equally curious, if not spurious. Kinzie et al. (2014) present data from 24 sites purported to show YDB nanodiamond spikes (but see also Table 3 and Section 12.6 regarding unreliability of those measurements). Eighteen of the claimed YDB zones are poorly dated, not dated, associated with a disconformity (Table 4), or from the Usselo soil, which formed through the Allerød and YD/GS-1 (Section 5.6). ENDNOTE 10.

5.6. Logical lapses in dating and interpreting Usselo and Finow soils

Following on comments elsewhere (Holliday et al., 2020, p 87), YDIH proponents perpetuate logical lapses in the interpretation of radiocarbon dates and the dating of soils along with no understanding of soil forming processes in discussions of dates from the Usselo (Hijszeler, 1957) and Finow (Schlaak, 1993) soils (here simply referred to as the Usselo soil). These soils are charcoal-rich sands that act as distinct stratigraphic markers within widespread and genetically related layers of laterally continuous post-glacial eolian “coversand” sheets distributed across much of northwest and northcentral Europe (e.g., van Geel et al., 1989; Hoek, 1997; Vandenberghe et al., 2013; Kaiser et al., 2009; van Hoesel et al., 2012, 2014; Andronikov et al., 2016a). The soils are not a **“charcoal boundary layer”** (contra Sweatman, 2021, p 15).

Proponents of the YDIH (Firestone et al., 2007; Wittke et al., 2013b; Wolbach et al., 2018b, p 190) argue that the charcoal in the soil is evidence of catastrophic biomass burning at the YDB or during the YD/GS-1 (depending on the author). Kennett et al. (2015a, fig. 1) claim Usselo soils at Lingen (Germany) and Aalesterhut (The Netherlands) are of YD/GS-1 onset age based only on two dates from among scores of dates for Bayesian analyses (e.g., Hoek, 1997; Kaiser et al., 2009). However, the geomorphologists and soil stratigraphers who were the principal investigators of the Usselo soil and know it best based on both field and laboratory research clearly demonstrate that the soil is just that: a zone of pedogenic weathering including accumulation of organic matter such as charcoal over time (van Geel et al., 1989; Hoek, 1997; Kaiser et al., 2009; van Hoesel et al., 2012, 2013, 2014). Kaiser et al. (2009, fig. 8) illustrate the radiocarbon dating of 63 samples from the Usselo soil. The full range of dates spans almost 2000 years, but the bulk of the dates are from the Allerød interval (pre-YDB); far fewer date to the YDB or YD/GS-1. The dating is consistent with the field interpretation of prolonged pedogenesis before and during the YD/GS-1. These data also directly contradict claims that half of the charcoal dates are at or near the YDB (Sweatman, 2021, p 15) or that most of the charcoal is in or on top of the upper soil zone and marks the YDB (Kinzie et al., 2014, p 447; Kennett et al., 2015a, p 4347, 4350; Wolbach et al., 2018b, SI fig. A6) (Table 6). Based on the dating, including OSL ages for the eolian deposits above and below the Usselo soil, and the evidence for pedogenesis, van Hoesel et al. (2012, p 7651), van der Hammen and van Geel (2008, p 360), and Kaiser et al. (2009) all reject the claim that the Usselo soil is a rapidly deposited YDB “event” layer.

YDIH proponents nevertheless persist in using the Usselo soil as a YDB marker (Sweatman, 2021, p 12, 14, 15; Powell, 2022, p 5). Sweatman (2021, p 12), for example, suggests that the soil represents **“YDB sediment”** based on no data and misunderstanding what a soil represents. He argues that the **“conclusion that these sites are not synchronous should be considered inconclusive”** even though the published field data and geochronology establish the Usselo as one of the best dated and stratigraphically consistent post-LGM terminal Pleistocene marker soils in Europe.

In his review of the YDIH, Sweatman (2021) includes a number of other critiques of the radiocarbon dating of the Usselo soil. The large body of consistent data generated by multiple investigators, which inconveniently contradict the YDIH, is offhandedly dismissed by an unsubstantiated remark that critics misunderstand **“the nature of variance in the radiocarbon dating of sediments”** (Sweatman, 2021, p 14). The dating of the Usselo/Finow soil is based on scores of dates on individual fragments of charcoal, not sediments, from multiple sites.

Table 5

Bull Creek profile BC1 dates and nanodiamond content (from Bement et al., 2014, table 1) with soil horizonation (Bement et al., 2014, table S1.2) (calibration added).

Soil Horizon	Level	Depth, cm below surface	Kennett et al., 2009a Nanodiamond ppb	Attributed to Kennett et al., 2009a[1] Nanodiamond ppb	Bement et al., 2014 Measured Nanodiamond ppm[2]	^{14}C age, yr BP	^{14}C age, cal yr BP[3]
2Akb8	BC22	289–298	25 ppb	No data	0	10,870 ± 70	12,805, 12,740–12,838 1sd
	BC21	298–307[4]	100	100	1.9	11,070 ± 60	12,991, 12,957–13,081 1sd
2ACb8	BC20	307–312	0	90	190		
	BC19	341–351	0	1	0		

[1] Reported values in table 1 of Bement et al. (2014) attributed to Kennett et al. (2009a) but differ from that plotted in fig. 1 of Kennett et al. (2009a).

[2] 1.9 ppm = 1900 ppb, 190 ppm = 190,000 ppb

[3] Calibrated with CALIB http://calib.org/calib/ using the calibration datasets from Reimer et al. (2020).

[4] Depth of the radiocarbon sample inadvertently listed for BC21 (i.e., Lab #Beta-184,854) as **“289–298”** in Bement et al., (2014, table S1.1).

Sweatman (2021, p 12, 15, 16, 17, 20) also criticizes reliance on single radiocarbon dates. Multiple samples for numerical age control are ideal, but in the early decades of numerical dating, not common. A single date is not by definition in error. There are many examples of reliable single dates. Indeed, Sweatman (2021, p 2–3, 16) embraces the dating of the **"black mat"** by Haynes (2008) (Section 6) even though most of that dating is based on one or a few dates, and he expresses no concern over the issue of variance in that dating.

The dating of the Usselo soil also raises an important point. Sweatman (2021, p 12) argues **"the uncertainty in the age of YDB sediments is rarely captured by a single radiocarbon measurement at a specific site. Indeed, it is standard practice to take in the region of 10 measurements at any site to create proper age-depth models so that the true age uncertainty in a boundary layer can be reliably reported. Reliance on single measurements from any site is unwise, as we can expect such an approach to give the false impression of asynchronous local events for a synchronous widespread event across all sites."** This passage is rife with misleading inferences and a fundamental lack of understanding of the Usselo soil. More broadly, this sweeping statement is a rather remarkable claim in support of the YDIH given that almost no site presented in the YDIH literature meets the requirement for 10 ^{14}C measurements for each YDB or black mat section. This argument could be used to consider the notion of the YDIH equally inconclusive. He further argues (p 15) that in dating the Usselo soil "**only single measurements were made at each site. Proper age-depth models that intersect the boundary were not generated for any of them, leaving open the possibility that the sites are synchronous and the dispersion in dates they found was due to natural processes."** This is another unsupported assertion with no factual basis. Again, similar to Kennett et al. (2015a), an assumption is made that because it could represent the YDB, it therefore must be the YDB. More to the point, an impact, representing a moment in time (similar to a volcanic eruption such as the Laacher See; Baales et al., 2002, and Section 5.8) produces radiometric dates that vary around a mean. The Usselo soil, in contrast, produced many non-overlapping radiocarbon dates spanning 1400 ^{14}C years because it is a soil. There is no possibility that the sites are synchronous. The Usselo soil does not represent a moment in time.

Sweatman (2021, p 15) also asserts "**the precise boundary layer at each site corresponding to the depth of geochemical markers, rather than charcoal which is not diagnostic for the impact event, was not determined for any site studied, and therefore it is not possible to know if any charcoal samples were taken directly from the Younger Dryas boundary."** That is another example of circular reasoning, however. Sweatman claims the geochemical signatures define the YDB, while at the same time they are interpreted as impact markers largely because they occur at the YDB and are synchronous with the YD/GS-1 onset, which is claimed synchronous with the megafauna extinctions. While we agree that charcoal is not an impact marker (see Section 9.3), YDIH proponents repeatedly claim that it is produced in great quantity by the YDB impact. For example, **"Wildfire … at the Younger Dryas boundary"** is the title of Kennett et al. (2008a) and the titles of both Wolbach et al. (2018a, 2018b) begin **"Extraordinary biomass-burning … triggered by Younger Dryas cosmic impact."** Sweatman (2021, p 12) earlier cites these and other papers purporting peaks in charcoal at the YDB. Therefore, charcoal is certainly an appropriate material to use for dating the soil and the dates clearly show that it is both older than and younger than the YDB. Confusingly, some YDIH proponents explicitly claim the black mat lies directly above the YDB, while others claim it is the YDB (see Section 6). Sweatman (2021, p 16) on the other hand apparently alludes to both, claiming the base of the black mat (and the Usselo Soil) is the YDB and the remaining majority formed over the YD/GS-1. The Usselo Soil cannot (and does not) represent both the YDB impact and the YD/GS-1.

The obvious conclusion based on all geologic and pedologic data, including the dating of deposits above and below the charcoal-rich soil horizon is that the Usselo soil reflects fires (along with pedogenesis) spanning at least ~1400 ^{14}C years, largely in the Allerød but continuing into the YD/GS-1 (i.e., across the YDB). A bigger issue among YDIH proponents is a fundamental misunderstanding of the nature of pedogenesis, which is a time transgressive process on stable or quasi-stable landscapes. ENDNOTE 8. "[T]**here is no need to invoke an extraterrestrial cause to explain the charcoal in the fossilized soils"** van der Hammen and van Geel (2008, p 359).

5.7. *Improved dating of Clovis sites and Clovis archaeology*

Clovis is a term given to the oldest well-dated, widespread, and recognizable archaeological technocomplex in North America (Haynes, 2002; Smallwood and Jennings, 2015; Meltzer, 2021). Proponents of the YDIH use their perceived connection between the disappearance of the Clovis lithic tool style and the onset of the YD/GS-1 stadial at ~12.9 ka BP as evidence for an environmental catastrophe (Sections 1, 3.1 and 3.2) (e.g., Firestone et al., 2006, 2007; Anderson et al., 2011; Wolbach et al., 2018b). Powell (2020) repeats the notion of a non-existent mystery regarding the disappearance of Clovis archaeology (Section 3.1). Subsequently, Powell (2022) is quite emphatic on this point. He refers to **"the fall of Clovis"** (p 35) and claims (p 36) **"just at its prime** [~13 ka], **Clovis suddenly fell"** and **"No Clovis artifacts have ever been found in place above the YD"** (presumably referring to the YDB). These claims are false. The YDC is a time interval spanning ≈ 1200 calendar years. A broad variety of artifacts styles appeared and disappeared during and after the span of the YD/GS-1 in North America. For example, most of the data used by Anderson et al. (2011) are undated (and undatable) artifacts found on the surface. Radiocarbon dating of Clovis sites (below) shows that Clovis persisted beyond the YDB.

Powell (2022, p 36) offers other unsubstantiated and factually incorrect claims regarding Clovis archaeology. **"At the Topper site, LeCompte et al. [2012] found impact microspherules touching Clovis artifacts, but no microspherules below the artifact layer."** He apparently is unaware that the archaeologist who excavated the Clovis and younger components at Topper documents the mixing of the assemblages (Miller, 2010). The context of spherules in a single sample column is meaningless. Powell (2022, p 36) further claims **"In the Southeastern US, near the onset of the YD, the Clovis suddenly abandoned a dozen Paleo-Indian chert quarries"** with no citation. Topper was a quarry but also a primary habitation. No Clovis quarries with firm age control are reported. More generally he notes **"In the eastern US, Clovis artifacts have been found from Maine to Florida, where average yearly temperatures differ by much more than the ~10°C change at the beginning of the YD. Could such a relatively small temperature change, even one that occurred rapidly, by itself have destroyed such a well-adjusted and widespread culture**?" Leaving aside the bizarre comment about a **"well-adjusted"** culture, the quote reveals a misunderstanding of the difference between the annual cycle of temperature and changes in long-term mean global annual temperatures. In any case, no data are provided to support these assertions. But Fastovich et al. (2020) and Griggs et al. (2022) show that environmental conditions across eastern North America before, during, and after the YDC varied significantly in space and time. But the point is essentially moot. Clovis populations survived across North America in highly varied and changing environments from before and into the YDC (e.g., Haynes, 2002; Smallwood and Jennings, 2015).

Radiocarbon dating shows that there is no correlation between the YDB and the end of the Clovis archaeological style. The work of Waters and Stafford (2007) was accepted as a standard for the dating of the Clovis occupation of North America by the YDIH proponents (e.g., Firestone et al., 2010a; Wittke et al., 2013a; Kennett et al., 2015a) although their dating did not quite support the YDIH claims. Waters and Stafford (2007) suggest that Clovis occupied a narrow time window between ~13.0 ka and ~ 12.6 ka. That age range was revised/updated and now indicates that Clovis largely post-dates 12.9 ka by up to several centuries (Waters et al., 2020). That paper (published before Sweatman, 2021, was submitted) proposes a maximum calibrated age range for Clovis of ∼13,050 to ∼12,750 cal yr BP. Their fig. 2 shows that most of their dated sites post-date the YDB. Only one is clearly older. Further, Buchanan et al. (2022), using recent dating of Folsom archaeology (Buchanan et al., 2021) along with the work of Waters et al. (2020) demonstrate an overlap of the two artifact traditions by as much as 200 years, discrediting the notion of an abrupt cultural termination at the YDB (and the notion of some sort of occupation hiatus after the Clovis occupation, Section 1) (see also Barlow and Miller, 2022).

One notable example of flawed dating ignored by the YDIH proponents is from the Gainey archaeological site in Michigan (Table 4). This badly mixed Clovis site was repeatedly presented as a key locality supporting the YDIH (Firestone et al., 2007; Bunch et al., 2012; LeCompte et al., 2012; Wittke et al., 2013a; Kennett et al., 2015a, 2015b) although the absence of intact context at the site was emphatically stated by the archaeologists that investigated it and repeatedly stated by YDIH critics (Holliday and Meltzer, 2010; Boslough et al., 2012; Holliday et al., 2014; Meltzer et al., 2014). Significantly, the site is the only YDIH locality where purported impact indicators are directly dated, yielding ages of ∼200 and − 135 ^{14}C yr BP (Table 4). They are clearly not YDB age and one, from R. Firestone, must be from a modern sample that included “bomb carbon” (from atmospheric testing of nuclear weapons) which yields radiocarbon dates from the future, a well-known problem in radiocarbon dating of young samples (Taylor and Bar-Yosef, 2014, p 23). Despite the obvious damning data on the context and age of the site, some years after it was published, YDIH proponents continued to maintain that it is a YDB site (Kennett et al., 2015a, SI p S34; Powell, 2022).

5.8. Radiocarbon simulations of the YDB

Inter-site variability in radiocarbon dates on purported impact proxies has remained problematic for the YDIH, suggesting that those layers were deposited asynchronously (Holliday et al., 2014; Meltzer et al., 2014). However, YDIH proponents continue to argue that the layers were deposited synchronously and have generally supported this argument by citing Kennett et al. (2015a; but see Boslough et al., 2015; Holliday, 2015). Using a OxCal, with a Bayesian age-model implementation, Kennett et al. (2015a) estimate upper and lower chronological boundaries for a hypothetical temporal phase containing supposed impact-indicators from 23 sites in addition to seven paleoclimatic proxy markers of the YD/GS-1 onset. Kennett et al. (2015a, p E4352) estimate a temporal difference between the start and end of the proxy phase somewhere within 0–130 years (95% probability interval), concluding that synchronous deposition of all 23 layers is plausible since the range of possible years includes zero. Unfortunately, Kennett et al. (2015a) neither plot nor describe the mean, median, or mode of this interval, so it is difficult to assess which temporal distances are most probable–while zero years may be plausible, this interval is also consistent with distances exceeding a century. Further, the assumptions and decisions involved in the creation of this phase model render its inferences problematic.

Given the many parameters and assumptions required to model the 23 site chronologies, it is unclear to what degree this 0–130-year estimate is contingent on modeling decisions. These decisions include the placement of stratigraphic breaks, the inclusion/exclusion of horizontally disparate samples, chronometric hygiene protocols, as well as the distributions, types, and prior parameter values of site-specific age models and of sample-specific outlier models. Additionally, the probability distributions of the start and end of the proxy phase result from the choice to include the 23 modeled YDB ages in a single temporal phase, which itself has multiple possible distributions and prior parameter values that must be specified by the user. In combination, these decisions compound potential problems stemming from assumptions at

Table 6
Black mats & pseudo–black mats.

Site	Reference	Claims & Comments
Abu Hureyra, Syria	Bunch et al., 2012, SI p 2	**“All such YD-aged pit-houses at Abu Hureyra and their immediate environs contained a dark charcoal-rich layer indicating extensive burning that the excavators previously attributed to residue from cooking fires…, but which is also consistent with broader-scale biomass burning at 12.9 ka.”**
	comment paraphrased from Moore et al., 2000	Charcoal in occupation levels in an extensive, intensely occupied tell is to be expected and was recovered along with carbonized grain and burned bone from multiple occupation levels from >13.0 cal ka BP into the early Holocene.
Arlington Canyon, CA	Kennett et al., 2008a	Multiple “**black mats**” through a 4 m section with the two lowest considered the YDB.
	Kennett et al., 2008a, SI p 12624–12625	Dates from all black mats are **“statistically similar.”**
	Wittke et al., 2013c, p E3897	**“The radiocarbon dates from Arlington Canyon were never used to date the YDB.”**
	Meltzer et al., 2014, SI p 9	**“**[T]**he radiocarbon ages at the top… and bottom… of the section are statistically indistinguishable.”**
	Comment	Multiple black mats of the same age but only the lower 2 have purported impact indicators.
Chobot, Alberta	Wittke et al., 2013a, SI fig. 5	**“YDB layer…0.12 m beneath a carbon-rich black mat/layer”** with **“observed Clovis artifacts located at the base of the black layer,”** but admit they **“were unable to date the site radiometrically because of bioturbation by plant roots. However, the stratigraphic position of the spherule layer is immediately above the uppermost level containing abundant Clovis points and artifacts.”**
	comment paraphrased from Ives and Froese, 2013	Archaeologists that documented the site noted the “black mat” is simply the surface leaf litter and humic materials (the LFH horizon typical of Luvisols and Brunisols), whereas the underlying “YDB” layer likely reflects pedogenically translocated clays and organics, residues from slope wash, or deposits from a recent higher stand of Buck Lake.
Folsom, NM	Wittke et al., 2013b	**“Nonalgal black mat present at the Folsom site, NM.”**
	Meltzer et al., 2014, footnote SI p 29	**“**Wittke et al. [2013b] **assert there is a nonalgal black mat present at the Folsom (NM) site. That is neither correct… nor relevant.”**
	Comment	Wittke et al. (2013b) explicitly states that a black mat at Folsom is reported by Haynes (2008), but the latter explicitly lists Folsom among sites without black mats in his table 3. Meltzer (2006) reports on the most comprehensive investigation of the site and likewise shows that there is no black mat.
Gainey, MI	Wittke et al., 2013a, fig. 1	Black mat noted for the site.
	Wittke et al., 2013a, SI fig. 7	Spherules from **“dark layer”** < 35 cm below ground surface.
	Comment	No black mat, darker soil or other organic-rich deposit is documented for

(*continued on next page*)

Table 6 (*continued*)

Site	Reference	Claims & Comments
		this shallow, mixed archaeological site (Table 4).
Indian Creek, MT	Baker et al., 2008, abstract	An unnamed mammoth site (Indian Creek?) with a **"black mat"** containing claimed impact indicators dated to **"11.5 ka (C14) before present"** and at the Indian Creek site itself, **"below the cultural layers and below a 11.2 ka (C14) volcanic ash layer"** were more alleged impact indicators (Baker et al., 2008).
	Comment	Mammoth in a black mat at one site and claimed impact indicators at two sites; both older than the YDB.
MUM7B, Venezuela	Mahaney et al., 2010a, abstract	"[A] **'black mat' candidate correlative with Clovis Age sites in North America"** dated <13.3 cal ka BP and "**carbon-rich black layer encrusted on a sandy pebbly bed."**
	Comment	The **"black mat"** is an undated carbon-, manganese-, and iron-encrusted pebble zone 20 cm above peat and alluvium dated ~13.7–13.3 k cal yrs. BP. The crust is a post-depositional coating and not a primary organic-rich deposit.
Newtonville, NJ	Wu et al., 2013, SI fig. S1	The **"inferred YDB"** layer (a few centimeters thick) at the contact with the black mat?
	Comment	The SI fig. S1 from Wu et al. (2013) clearly illustrates that the undated "YDB" is below the A-horizon of the modern local and regional surface soil.
Santa Maira Cave, Spain	Kinzie et al., 2014, SI App B	Sample was **"darker"** than others and thus **"analogous to the dark YDB"** at Daisey Cave and Sheriden Cave.
Usselo Soils, Northwest Europe	Wolbach et al., 2018b, SI fig. A6	**"Photographic examples of black mats in northwest Europe. Charcoal-rich black layers (arrows), or 'black mats,' lie at the boundary between the underlying Usselo Formation and overlying sandy sediment. This layer marks the onset of the Younger Dryas climate episode and precisely coincides with the impact-proxy-rich Younger Dryas boundary layer."**
	Comment	No radiocarbon ages are presented to show that all or any of the illustrated charcoal zones date to the YD/GS-1. Kaiser et al. (2009) show that most charcoal associated with the Usselo soils predate the YDB and some post-date it.

different modeling levels. Despite these issues, subsequent works that favor the YDIH cite Kennett et al.'s (2015a) model as confirmation that the proxy layers represent one event (e.g., Israde-Alcántara et al., 2012; Moore et al., 2017), while it remains, at best, only plausibly consistent with synchroneity.

In addition to the previously discussed evidence contradicting the assertion that the purported proxy layers were deposited by a single event (Section 5), simulations published by Jorgeson et al. (2020) illustrate that the impact proxy radiocarbon dates used by Kennett et al. (2015a) are far more dispersed than expected for a synchronous event. Jorgeson et al.'s (2020) simulations iteratively sampled radiocarbon ages from a hypothetical synchronous event, accounting for uncertainty in the radiocarbon calibration curve, laboratory measurement error, and old wood effects. The authors compare age dispersion in the simulated samples to the age dispersion of the observed YDB radiocarbon sample dataset and to the age dispersion in observed radiocarbon samples of a known synchronous event, the Laacher See volcanic eruption in Germany. The YDB radiocarbon dataset shows far more age dispersion than the simulations, while the Laacher See volcanic eruption radiocarbon dataset displays age dispersion similar to the simulations.

YDIH proponents - mainly Sweatman (2021, 2022), yet see also Powell (2020) - raise four objections to Jorgeson et al.'s simulations, but each lacks merit. The objections involve old wood effects (see also Section 12.4) for the Arlington Canyon radiocarbon dates, the effects of catastrophic geomorphic processes on the integrity of radiocarbon samples, inadequate chronological modeling, and a failure to address the supposed geochemical evidence for the hypothesis.

Concerning Arlington Canyon, Sweatman (2021, 2022) argues that the old-wood offsets used in Jorgeson et al.'s (2020) simulations are not sufficient to account for pine species at the site, which can live up to 1000 years. As such, Sweatman argues that a synchronous event should produce radiocarbon samples more dispersed than those simulated by Jorgeson et al., as larger old-wood offsets would generate more temporal variability. As reported in Kennett et al. (2008a, table 4), 13 of 16 radiocarbon samples from Arlington Canyon are wood or charcoal. Kennett et al. (2008a) reject one wood charcoal for being out of stratigraphic sequence. The remaining 12 wood/charcoal" samples consistently predate Kennett et al.'s (2015a) modeled YDB age by only ~0–450 ^{14}C yrs., an offset that is well accommodated by the simulated old-wood offsets (Jorgeson et al., 2022). As such, the high dispersion in YDB radiocarbon ages is not explainable in terms of the Arlington Canyon samples alone.

The remaining three (of 16) Arlington Canyon radiocarbon samples comprise, a "**carbon spher**[ule]", a "**carbon… 'elongate'**", and a "**glassy carbon**" sample (Kennett et al., 2008a, table 4). Supporters of the YDIH claim that carbon spherules, carbon elongates, and glassy carbon are remnants of burned tree sap (e.g., Firestone et al., 2006, p 343; Israde-Alcántara et al., 2012, p E745; LeCompte et al., 2018, p 169; Wolbach et al., 2018b, SI p S27, Wolbach et al., 2020, p 99; but see Scott et al., 2010, 2017 and Sections 9.3 and 12.4), and that these Arlington Canyon samples were produced by biomass burning in the wake of the impact event (Kennett et al., 2008a). The spherule dates to 11,440 ± 90 ^{14}C yr BP, corresponding to a calibrated 95% interval of 13,458–13,163 yr BP (UCIAMS-36961; Kennett et al., 2008a), well prior to the proposed impact. The carbon elongates and glassy carbon samples also have similar dates of 11,110 ± 35 and 11,185 ± 30 ^{14}C yr BP (UCIAMS-36962 and UCIAMS-36960; Kennett et al., 2008a), corresponding to calibrated 95% intervals of 13,100–12,924 and 13,162–13,085 yr BP, respectively. Like the wood samples, these samples are consistently older than the proposed YDB age. If these specimens are burned tree sap as YDIH proponents (including many coauthors of Kennett et al., 2008) claim, unlike the wood samples, *they would not be subject to old wood effects*. In an example of self-inconsistency, Kennett et al. (2008a, table 4) exclude the problematic radiocarbon dates on their glassy carbon, carbon spherule, and carbon elongate from the **"average age of the lowest stratigraphic unit *due to 'old wood' effect*"** (emphasis added).

Kennett et al. (2015a) rely on treating these three specimen types as wood charcoal with potentially large age offsets, incorrectly allowing for a younger age for claimed impact indicators at Arlington Canyon consistent with the YDIH. If the Arlington Canyon layer dates to Kennett et al.'s (2015a) proposed YDB age, and if the carbon spherules/elongates as well as glassy carbon are wildfire products (as YDIH proponents claim), then those from earlier wildfires were mixed into that layer. In that case the carbon spherule/elongate and glassy carbon concentration profiles that have been published in support of the YDIH cannot be used the test the YDIH. This is because those specimens are not impact markers, thus in order to correlate any particular carbon spherule/elongate or glassy carbon to a possible YDB impact event (and to the same specific wildfire), it is then necessary to date that particle to the YDB. To test the YDIH, it is then necessary to radiocarbon date each and every carbon spherule/elongate and glassy carbon counted through the sediment profile to construct a meaningful concentration profile based on age-correlated abundances. This clearly has not been performed. Given the current evidence, the parsimonious interpretation of the Arlington Canyon "proxy layer" is that it predates the hypothesized event, consistent with Jorgeson et al.'s (2020) simulations.

If the samples from Arlington Canyon indeed attest to an interval of increased wildfire, as YDIH proponents claim, there is an alternative hypothesis. The calibrated ages of the tree samples are consistent with sharp increase in Greenland $\delta^{18}O$ values between the cooler GI-1b (the IACP, see Sections 3.3 and 9.2) and the warmer GI-1a interval (Rasmussen et al., 2014). At that time, Santa Barbara Basin ocean-surface conditions, which apparently vary synchronously with Greenland climate (Hendy et al., 2002), would have abruptly changed from cool to warm (and from cooler and drier to warmer and wetter conditions on adjacent land areas). The samples could well be the product of wildfire favored by that climate change and would not require an exotic explanation.

Sweatman (2022, p 3) also contends that inconsistent radiocarbon samples from Murray Springs and Big Eddy should have been discarded from the simulations. Jorgeson et al. (2020) consider the exclusion of questionable dates from Murray Springs and Big Eddy in their supplemental simulations - exclusion of these dates does not affect the main conclusions drawn from the simulations.

In the second objection, YDIH proponents blame catastrophic geomorphic processes for high variability in radiocarbon ages between layers containing purported impact proxies. Sweatman (2021) initially argues that the radiocarbon record is consistent with synchronous deposition. Yet, one year later, Sweatman (2022, p 1), in response to Jorgeson et al.'s simulations, argues that high dispersion in radiocarbon dates should be expected, given the dramatic effects of the proposed impact: "**The asteroid impact… …would alter the environment catastrophically through a hierarchy of interlinked events and processes, many of which could lead to an increase in the distribution of radiocarbon dates relating to the event. Ancient forests might be felled, tsunamis, earthquakes and landslides might mix and redeposit soils, and old sources of carbon might be redistributed. Even if some of these catastrophic processes might be modelled, there will always remain some doubt about the suitability and completeness of such models.**"

Jorgeson et al. (2020) considered such a catastrophic event, the Laacher See volcanic eruption. The eruption felled trees, created a temporary lake through damming of the Rhine Valley, produced a 50-m thick tephra near the eruption center, and generated 1-m thick pumice deposits up to 120 km from the volcano (Bogaard and Schmincke, 1985; Baales et al., 2002). These processes left unambiguous features visible on Central Europe's modern landscape. Even with these dramatic eruption effects, the Laacher See tephra contains radiocarbon samples consistent with simulations of a synchronous event (Jorgeson et al., 2020). By contrast, evidence for the catastrophic geomorphic processes suggested by Sweatman are lacking for the proposed YDB impact (Sections 3.3 and 13.7). Impact proponents, in essence, argue that the impact produced global catastrophic effects far exceeding those of the Laacher See eruption, while paradoxically leaving no evidence for changes to the landscape. To our knowledge, YDIH proponents have not offered evidence for impact related tsunamis or earthquakes.

In the third objection, Sweatman (2022) questions the very idea of modeling the YDB radiocarbon dataset. Since every physical process relating to chronology cannot be known with certainty, he argues that any unexplained variation in radiocarbon dates is unproblematic. For example, regarding the modeling of old wood effects with an exponential distribution, Sweatman (2022, p 3) states that **"the exact 'old wood' model for AC** [Arlington Canyon] **is unknown, nor is it known whether any exponential distribution with any value of λ** [rate parameter of the exponential distribution] **is adequate"** and Jorgeson et al. (2020) "**did not explore all possible forms of 'old wood' model. They only discuss simple exponential forms.**"

An "exact" model cannot be known for most physical processes as models are, by definition, reductionist representations of the physical world. There are infinite possible old wood models that could be defined; although there are theoretical reasons to expect the distribution of old wood effects to be approximately exponential (Nicholls and Jones, 2001). Consequently, the exponential distribution is a standard model for old wood effects as implemented in OxCal (Bonk Ramsey, 2009), the application used to estimate many chronologies in archaeology, paleontology, and paleoclimatology. Impact proponents themselves used OxCal to model the age of the hypothesized event, and the vast majority of their radiocarbon samples were modeled with exponential old wood offsets (Kennett et al., 2015a).

While all models are imperfect and incomplete, the strength of the evidence produced by a model with well-justified assumptions can be probative. Jorgeson et al.'s (2020) simulations are not just broadly inconsistent with a synchronous YDB, they demonstrate that the likelihood of a synchronous event producing the dispersion seen in the YDB dataset is astronomically low. The simulations account for many sources of variability in radiocarbon dating; while there may be other sources that are not included in the simulation, they would likely have only marginal effects on the results.

Sweatman (2022, p 2) raises a final objection against Jorgeson et al. (2020) on the grounds that their simulation **"does not explain the physical evidence for the YD impact event at numerous YDB sites found, and confirmed, on multiple continents as reported in dozens of papers."** Regardless of the numerous problems with the purported physical evidence and dating enumerated throughout this and other papers, the objective of Jorgeson et al. was not to "explain" the claimed evidence for a YDB impact, only to illustrate that the YDB radiocarbon record is statistically inconsistent with a synchronous event. Neither Sweatman nor other YDIH proponents have demonstrated otherwise.

6. Misinterpreted black mats

Firestone et al. (2007, p 16016) begins their introduction, **"A carbon-rich black layer, dating to 12.9 ka (12,900 calendar years B.P.) …, has been identified by C. V. Haynes** [2008]**, at 50 sites across North America as black mats… …the base of this black layer coincides with the abrupt onset of Younger Dryas (YD) cooling, after which there is no evidence for either in situ extinct megafaunal remains or Clovis artifacts."** Firestone et al. (2007, p 16017) clamed, **"Directly beneath the black mat, where present, we found a thin, sedimentary layer"** that contains impact markers and "[w]**e identify this** [sedimentary] **layer as the YD boundary."** Prior to Firestone et al. (2006, 2007), Brakenridge (1981) proposed a Late Quaternary supernova event in which he speculated black mats **"are terrestrial records of the Vela supernova"** (p 90) and included a photo of one at the YDIH Murray Springs site. Cosmic-catastrophe proponents focus considerable attention on the **"black mat"** (e.g., Brakenridge, 1981; Firestone et al., 2006, 2007; Wittke et al., 2013a, 2013b; Mahaney et al., 2010a, 2010b, 2013, 2017, 2022; Kennett et al., 2008a; Firestone et al., 2010a, 2010b; LeCompte et al., 2012, 2013; Pigati et al., 2012; Kinzie et al., 2014; Israde-Alcántara et al., 2012, 2018; Wolbach et al., 2018b; Sweatman, 2021; Powell, 2020, 2022; and many others). A black, organic-rich stratum covering the Lehner Clovis site in Arizona was first described by Haury et al. (1959) and termed **"black swamp soil"** (Antevs, 1959). Similar organic-rich layers are known by other names (see Quade et al., 1998) including the **"black mat"** (Haynes, 1968, 2008; Haynes and Huckell, 2007), which has become the dominant term used for these stratigraphic entities.

Powell (2022, p 5) refers to the black mat as **"enigmatic."** The only thing "enigmatic" about the black mat is its attribution to an impact. Nothing is particularly unique about the black mat other than its appearance in some stratigraphic sections along drainages in southeast Arizona, and the High Plains of Texas and New Mexico. Discussions of the black mat by impact proponents are grossly oversimplified with critical data misstated or ignored as repeatedly pointed out by Meltzer and Holliday (2010) and Holliday et al. (2014, 2020). Some YDIH proponents claim the black mat lies directly above the YDB, while others assert, as discussed below, that it represents a layer of purported impact debris (i.e., the YDB). For example, Kennett et al. (2009b, p 12623) erroneously state "**This biostratigraphic marker dates to ~12.9 ± 0.1 ka (10,900 ± 100 ^{14}C years).**" However, dating by Haynes (2008) clearly shows that many are not unique to the YD/GS-1 or the YDB for that matter. Some started forming far earlier than the YD/GS-1, others persisted beyond the YD/GS-1, and yet others have no clear dating to the YD/GS-1. Another key mischaracterization by YDIH proponents is that it is essentially a continuous stratigraphic entity: a "**stratigraphic marker that covers much of the Clovis-age landscape of N. America**" (Sweatman, 2021, p 2) and describes it as "**spanning the entire continent"** of North America (p 3). Haynes (2008) does not say that. A close reading of his text and tables show that the black mat is not continuous (and certainly does not span the continent) and represents a variety of geologic processes in a variety of landscape settings. The genesis of these soils and deposits varies significantly from location to location (Haynes, 2008; see also Harris-Parks, 2016). Some are algal mats, others aggrading wetland deposits or lowland soils, or lacustrine deposits including white to light gray diatomites, and still others are well-drained upland soils (see also Meltzer and Holliday, 2010).

The notion among some YDIH proponents of a continent-wide black mat with origins linked to an impact that spans the YD/GS-1 also directly contradicts the concept of an environmental catastrophe at a specific time of ~12.9 cal ka BP. The black mat as conceived by those YDIH proponents is a kind of soil horizon spread across the continent and which should therefore indicate continent-wide landscape stability. But there is no such indicator of regional stability, nor any evidence of geomorphic disruption across the continent at the YDB or through the YD/GS-1 (Meltzer and Holliday, 2010; Holliday and Miller, 2013) (see Section 13.7). Like today and throughout the Quaternary, a broad variety of both local and regional geomorphic systems driven by their respective environmental processes affected the landscapes of North American as well as elsewhere. Specific geomorphic processes and the rates at which they operated varied spatially and through time. No evidence shows a single continent-wide geomorphic event at the YDB or through the YD/GS-1 (e.g., papers in Gillespie et al., 2004; Straus and Goebel, 2011; Eren, 2012).

Sweatman (2021, p 2) comments, "**Around one hundred black mat sites across N. America have been discovered. Most in-situ Clovis sites are found directly under the black mat.**" Kennett and West (2008, p E110) and Wolbach et al. (2018b, table 1) make similar assertions. Both statements are wrong. Haynes's (2008) supplemental table 2 lists 72 sites with black mats and supplemental table 3 lists another 27 without YD/GS-1 black mats. Haynes' supplemental table 2 includes 13 Clovis occupations buried by **"black mats"** (including white diatomite). ENDNOTE 11. His supplemental table 3 also describes 13 Clovis occupations without black mats. The number of **"black mat localities"** rises notably if the localities reported by Holliday (1995) and Mandel (2008) (further discussed below) are included. With the exception of the Clovis type site and the Lubbock Lake site, no Clovis sites are reported from any of the scores of sections they report, however.

The radiocarbon age variation of black mats is also well documented by Quade et al. (1998) and Pigati et al. (2012) who identified black algal mats in North and South America ranging in age from 40,000 cal yr BP to modern. Further, Quade et al. (1998) clearly document and state that the most common age range for black mats in southern Nevada centers on 10,000 ^{14}C yr BP (~11.5 cal ka BP, i.e., post-YDB). Sweatman (2021, p 20) dismisses the conclusions of Pigati et al. (2012) but on the same page asserts that their work **"actually supports"** the YDIH (Table 8). Some YDIH papers identify a generic black or gray layer (i.e., an organic-rich or otherwise dark colored zone) as the YD-aged black mat with no evidence that it is in fact a YD-age zone (Tables 6 and 7). Impact markers are purported below, at the base, or even within this perceived black mat and taken as prima facie evidence by many YDIH proponents that this dark layer represents the YDB (Table 2).

The YDIH is rife with further contradictions regarding the black mat. Firestone et al. (2010a) wrote (abstract, p 30), **"At many locations the impact layer is directly below a black mat"** and (p 57) **"The black mat which overlays the YDB layer at many sites… … was not formed by the impact and appears to consist mainly of algal material produced by dying organic matter and burned material."** Bunch et al. (2012, p E1903) and Moore et al. (2017, p 7) also describe the black mat as overlaying the YDB layer. Pino et al. (2019) wrote, **"Most classic black mats in the United States do not contain much charcoal…, but it is sometimes** [i.e., not often] **abundant immediately below the black mat…, where the YDB layer typically is found…"** However, that is at odds with Firestone et al. (2007) because Pino et al. (2019) described weak evidence of wildfire in the YDB and weaker evidence in the black mat.

In contrast, others describe the black mat as both the YDB and an impact debris layer. Mahaney et al. (2013, p 100) claimed **"Recent analyses of black mat beds in the northwestern Venezuelan Andes … show conclusive micrographic and chemical evidence … that could only be produced by an ET airburst/impact"** and (p 103–104) **"The black mat beds, dated to 12.8 ± 0.2 calibrated ka, have yielded aerodynamically modified Fe spherules that most likely formed in a local airburst, resulting from a fragmented asteroid or comet."** Mahaney et al. (2017, p 68–69) further claimed, **"The airburst often produced a dark layer sometimes called the 'black mat', which in the Alps is represented by carbon encrusted grains in rinds and in paleosols. As elsewhere, the affected sediment typically contains high-temperature carbon (charcoal, soot, carbon spherules, glass-like carbon, melted, welded and quenched grains) and is common across Europe and western North America, but less common across eastern North America."** Wolbach et al. (2018b, p 195) assert **"YD onset is marked by the widely distributed deposition of black-mat layers across North America… The presence of these organic-rich sediments is consistent with an abrupt episode of large-scale biotic degradation that resulted from YD climate change and a major increase in biomass burning…"** For the Sheriden Cave site, Wolbach et al. (2018b, p 200–201) purports, **"A charcoal-rich black mat dates to the YD onset and contains peak abundances of charcoal, AC/soot, carbon spherules, and nanodiamonds** [repeatedly claimed by YDIH proponents to form by impact and not by wildfire] **that are closely associated with the last known Clovis artifacts in the cave. The black-mat layer is in direct contact with the wildfire-charred bones of two megamammals… … the last known examples anywhere in the world of those extinct species."** Wolbach et al. (2018b, SI fig. A6) consider a charcoal zone associated with the Usselo soil (Section 5.6) an equivalent to the black mat and of YDB age (Table 6).

Israde-Alcántara et al. (2018, p 60) claimed, **"at several Clovis Palaeoindian sites in the USA (Murray Springs, Arizona and Topper, South Carolina)** [although there is no black mat at Topper] **…, the black mat forms a distinctive stratigraphic marker at the onset of the YD climate change and is marked by peak abundances of charcoal fragments from a major episode of biomass burning."** Israde-Alcántara et al. (2018, p 76) concluded, **"An anomalous black sediment layer, produced during the YD interval, was recognized in three different lake sites from central Mexico (Lakes Acambay, Cuitzeo, and Chapala)… These black mat layers contain large amounts of organic material, charcoal, soot, nanodiamonds (only studied at the Lake Cuitzeo site, Israde-Alcántara et al., 2012), magnetic Fe-rich microspherules (some with aerodynamic shapes and evidence of high-velocity collisions) are a common feature in four of the five sites analysed. *These unusual materials were not observed above or below the black mat sediments at these sites*** [emphases added]**."** Only one of the sections described in that work could be

YDB age, however (Table 4).

Confusingly, Firestone (2020, p 3358) contradicts his previous work when he cites, **"YD impact layer is precisely dated to the onset of the YD, *exists only within the black mat,* and consists of PGE elements, spherules, nanodiamonds, aciniform carbon, and other impact indicators observed at over two dozen sites on four continents (Firestone et al., 2007…)** [emphasis added]." Firestone et al. (2007, p 16017) purported, **"six of 10** [sites] **have a *black mat overlying the YDB*. At Blackwater Draw and Murray Springs, *the YDB is found directly beneath the black mat*** [emphasis added]."

Carbon is a ubiquitous component of sediments and soils across the Earth's surface and has been since plant life first appeared. As such, sediments and soil horizons high in organic carbon (i.e., "black mats" in a literal sense) are ubiquitous in late Quaternary stratigraphic records (e.g., Quade et al., 1998; Pigati et al., 2012; Israde-Alcántara et al., 2018; Holliday et al., 2007; Haynes, 1968; Mandel, 2008; Holliday, 1995; Rachal et al., 2016) and most have no connection to the YD/GS-1. Charcoal can induce dark coloration, but it is not a significant component of black mats. Evidence for burning is mentioned nowhere by Haynes (2008). Subsequently, Haynes et al. (2010, p 4014) noted, **"Over the past four decades the lead author has chemically pretreated hundreds of black mat samples for multifraction ^{14}C dating…. Very few YD-age black mats were found to contain adequate charcoal"** for dating (see also Table 6). Furthermore, Harris-Parks (2016, p 102) studied black mats microscopically at YDIH sites Murray Springs, Blackwater Draw, as well as Lubbock Lake and reported, **"the absence of ash and near-complete absence of charcoal in all of the samples do not support the idea that black mats formed by regionally extensive fires caused by an extraterrestrial impact."**

On the other hand, a wide array of sites and settings with YDB- and YD/GS-1-age deposits have no "black mats" (Meltzer and Holliday, 2010; Holliday, 1995; Holliday and Miller, 2013). Of the 29 localities with claimed evidence for impact proxies tabulated by Holliday et al. (2014, SI table S1), independent of the reliability of the dating or stratigraphic context, only about half exposed a "black mat." Local environmental conditions likely control their genesis. In what inadvertently became a search for black mats inspired by the geoarchaeological record at the Blackwater Draw Clovis site (a YDIH "type section" of sorts) and the Lubbock Lake site, Holliday (1995) reports on a study of "draws" (dry valleys) on the High Plains of northwest Texas and eastern New Mexico as part of the Brazos and Colorado drainage systems. These valleys aggraded through the latest Pleistocene and Holocene. Among 110 localities (representing >400 exposures and cores) along >1400 km of draws, only 16 sites contain black or gray organic-rich deposits that overlapped the YD/GS-1. A number of sections contained black mats that persisted into the early Holocene. Their occurrence was apparently controlled by the presence of seeps or springs. Similarly, late Holocene wetland muds, constituting another sort of "black mat" are common along the draws in proximity to historic springs.

In contrast, Mandel (2008) reports a variant of the black mat from the Central High Plains, based on work at 49 dated localities from 37 stream valleys, draws, and fans in the Kansas and Arkansas drainage systems. At the close of the late Pleistocene the meandering streams stabilized except for incremental additions of flood deposits. The result was development of an over-thickened (up to 2 m) black-to-dark gray soil A-horizon forming a distinct stratigraphic marker. Stabilization and soil cumulization began as early as ~15,600 cal yrs. BP but was underway in most sections between 13,300 and 12,900 cal yr BP; hence the onset of this process was time-transgressive and largely pre-YDB. The cumulic soils were buried by flood deposits in a likewise time-transgressive process varying from ~11,400 to ~10,200 cal yr BP, post- YD/GS-1. The period of alluvial stability and concomitant soil cumulization includes the YD/GS-1 but is not synchronous. Formation of this stratigraphic marker was due to localized changes in floodplain geomorphic process, not to any sort of ET process. This stratigraphic research by both Holliday (1995) and Mandel (2008) is well published and widely known except by YDIH proponents. ENDNOTE **12.**

Other inconsistencies abound in using the black mat as some sort of proof of a YDB impact (see also Section 13.3). Sweatman (2022, p 22) notes problems with dating soil organic matter in an attempted rebuttal to Jorgeson et al. (2020) but wholly accepts dating of black mats by Haynes (2008), which includes dating such material. Wolbach et al. (2018b, table 1) claim a direct link between sites with black mats and extinct fauna immediately below. Sweatman (2021, p 2), following Haynes (2008), states **"at 27 black mat sites mammoth bones are blanketed directly by the black mat."** Powell (2022, p 3) and other YDIH proponents make similar claims. A look at supplemental table 2 in Haynes (2008) clearly contradicts that interpretation and linkages between black mats and extinct fauna (Table 7). The only sites where an organic-rich layer directly covers mammoth or other megafauna are in the San Pedro Valley of Arizona.[5] At many sites elsewhere the "layer" in question is the A-horizon of a soil. Such zones are superimposed into sediment, i.e., they are not layers of sediment. ENDNOTES **8, 13.**

One observation is clear; YDIH proponents have never been in consensus regarding the role of the black mat in the hypothesis. Some believe it is in the impact debris layer (e.g., Mahaney et al., 2013, 2017, 2022; Israde-Alcántara et al., 2018; Wolbach et al., 2018b; Firestone 2020) while others believe it is not (e.g., Firestone et al., 2007, 2010a; Bunch et al., 2012; Moore et al., 2017; Pino et al., 2019). Some believe it is unique to the YD/GS-1 onset and is a global stratigraphic layer (e.g., Firestone et al., 2007; Mahaney et al., 2013) while others believe black mats form at different times within different regions but only those that contain YDB-aged impact markers are associated with the YDB impact (e.g., Israde-Alcántara et al., 2018; Wolbach et al., 2018a, SI). Most YDIH proponents, but not all (e.g., Pino et al., 2019), claim the black mat is rich in charcoal, but that has been refuted by independent studies (Haynes et al., 2010; Harris-Parks, 2016). Authors common to YDIH-proponent papers with opposing black mat interpretations appear confused and lacking in credibility.

7. Multifarious YDB impact scenarios

As noted by Boslough et al. (2012, p 13) **"there is not one single Younger Dryas (YD) impact hypothesis but several that conflict with one another regarding many significant details."** This is due to the fact that different impact scenarios are necessarily required by YDIH proponents to explain the disjointed contradictory evidence that is purported to support an impact. Firestone and Topping (2001, p 15) speculated a supernova shock wave **"gouged out"** the Carolina Bays to explain purported abnormal ratios of uranium isotopes and elevated plutonium at Clovis sites. To explain microspherules and other purported impact markers along with the radiogenic isotopes Firestone et al. (2006) then speculated in their book that a supernova **"can knock asteroids and comets out of orbit to collide with the Earth"** (p 21) and the **"supernova may have bathed a meteorite or comet with powerful radiation that altered its chemistry to form the ^{40}K that was carried to Earth in an impact"** (p 93). Later in the book, Firestone et al. (2006) apparently selected what they considered a more likely scenario and wrote, **"We suggest that the comets came directly from the supernova"** (p 264), presumably as exosolar objects.

These various suggestions and interpretations are contrary to common knowledge about supernovae, impacts, comets, and related phenomena. They represent pure fiction with little science, defying the laws of physics and logic (also noted by Morrison, 2010). For example, the origin of the Carolina Bays is controversial, but they do not have

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[5] At the site of El Fin del Mundo, a Clovis Gomphothere kill in Sonora, Mexico, the bone were buried by alluvium, but larger elements that protruded above the alluvial sediments were subsequently buried by lake beds (Sanchez et al., 2014), but the dating of the bone and basal lake beds is not well-constrained (Holliday et al., in press).

Table 7
Black mats and extinct megafauna reported by Haynes (2008, table 2).

Site	Stratigraphy & Chronology[1]
Black Mountain	Folsom archaeology and Cumulic Mollisol[2]
Carter/Kerr-McGee	Camel within Mollisol[3]
Chalk Rock	Mammoth at “contact” below Leonard paleosol[4]
Chapo Ranch	Mastodon >YDC below middle/late YDC Mollisol
Clovis/Blackwater Draw	Megafauna encased in alluvium below YDC lake beds
Dutton	Megafauna >YDC encased in lake beds below Mollisol
Elgin	Mammoth below Mollisol (no dating of bone or soil)
Feterman	Mammoth and cumulic Mollisol[5] (no dating of bone or soil)
Gilcrease	Mammoth & YDC black peats[5]
Huntington Canyon	Early Holocene lake beds over >YDC peat deposits with mammoth
Hiscock	Mammoth in lake clays ≥YDC
Kanorado	Mammoth & Camel >YDC below Mollisol[6]
Lamb Springs	Mammoth below Mollic cienega paleosol[5]
Lange-Ferguson	Megafauna encased in spring alluvium below diatomite and late YDC cumulic Mollisol
Lindsay	Mammoth below Leonard paleosol >YDC[4,5]
Lubbock Lake	Megafauna encased in alluvium below YDC lake beds
Marias River	Three Mollisols with mammoth[5]
OTL	Mammoth below ≥YDC Mollisol
Southeast Great Basin	Megafauna in stratified wet meadow and spring deposits ≥YDC
Sun River	Alluvium over mammoth in organic rich clay >YDC
Sunshine	Camel below dark brown marsh deposit[7]
Willcox playa	Mammoth below Mollic paleosol[8]

[1] See Endnote 8 for further explanation of soil terminology.

[2] Relationship of Folsom archaeology to soil unclear.

[3] Folsom and Clovis archaeology and camel bone in same soil horizon (Reider, 1980).

[4] The Leonard paleosol formed in Peoria Loess. Deposition of the loess ended and soil formation began before the YDC (Mason et al., 2008; Tecsa et al., 2020) and it was buried at locally variable times through the Holocene (Mason et al., 2003). The base of the soil zone is not a geologic contact.

[5] Relationship of mammoth to soil unclear.

[6] Clovis archaeology within soil; mammoth 2.5 m below archaeology (Mandel et al., 2005).

[7] Camel in alluvium below early Holocene marsh/cienega soil (Beck and Jones, 2009).

[8] Mammoth in alluvium at depth below poorly dated soil (Haynes et al., 1987).

attributes associated with known impact structures (Section 13.1) nor are they of YDB age (Table 4). Supernova shock waves cannot “gouge” the Earth’s surface. Any close enough to be felt would blow away the atmosphere and create far more damage than simply create the Bays.

Firestone et al. (2006, 2007) shifted focus from the supernova to the comet in order to explain a range of claimed impact markers. However, the lack of any known YDB-aged crater (Section 8) presented a serious challenge to the YDIH. To explain purported impact markers in YDB sediments and the lack of associated crater(s), Firestone et al. (2007) speculated, **“one or more large, low-density ET objects exploded over northern North America”** (abstract) and **“if multiple 2-km objects struck the 2-km-thick Laurentide Ice Sheet at <30°, they may have left negligible traces after deglaciation”** (p 16020).

To explain their claimed Ir and Ni concentrations in the YDB Firestone et al. (2007, p 16019–16020) narrowed down that their proposed **“ET objects”** were the comets from Firestone et al. (2006), **“The relatively low Ir and Ni peaks associated with the YDB are more consistent with the generally proposed composition of comets and inconsistent with the high-Ir content typical of most stony, nickel-iron, or chondritic meteorites.”** Firestone et al. (2007, p 16020) also reported that **“some megafaunal bones in the YDB are highly radioactive”** and **“high concentrations of U and Th were found in the YDB sediment at six of six Clovis-age sites analyzed and in four of four** [Carolina] **Bays”**. They speculated (p 16020) **“elevated levels of U and Th may result from … dispersal of ejecta from the impactor and/or the target area”**. This would require improbable scenarios where either the comet was radioactive, or it struck relatively-rich U deposits forming a problematic yet to be discovered crater.

Embracing the ideas of Donnelly (1883) that the Great Lakes were formed by a comet impact, Firestone (2009a, abstract) also speculated that the **“comet fragmented and exploded over the Laurentide Ice Sheet creating numerous craters that now persist at the bottom of the Great Lakes.”** This followed Firestone et al.’s (2007, p 16020) presupposition that oblique impacts of comet fragments on the ice sheet produced **“enigmatic depressions or disturbances in the Canadian Shield (e.g., under the Great Lakes or Hudson Bay).”** Firestone (2009a, section 6) falsely claims, **“Charity Shoal, a 1 km crater in Lake Ontario, has already been identified as dating from the time of the YD impact (Holcombe et al., 2001)”**, when in fact Holcombe et al. (2001, abstract) reported, that the **“feature may be extraterrestrial impact crater, but other origins… are not ruled out. Time of formation is not known.”** Subsequently Firestone et al. (2010a, p 57–58) suggest **“deep holes”** beneath four of the Great Lakes could represent impact craters and the **“Finger Lakes region of New York radiate out from the hole in Lake Ontario as if they were formed by the force of the impact pushing water and ice to the south.”** They dismiss the possibility that these holes were the result of glacial erosion, citing only the latest edition of a 19th century book by Dawson (1891). As summarized by Holliday et al. (2014, p 517), the problem with that speculation is that they provide no evidence that these depressions date to ~12.9 ka and at that time only the Lake Superior basin was still under glacial ice (Dyke, 2004). Further, the Great Lakes basins are elongated, oriented parallel to local ice flow and the “deep holes” are in the up-ice end of the respective lake basins. Thus, the “enigmatic depressions” are probably the result of glacial erosion and not the missing YDB craters that could explain many of the claimed impact markers purported in the YDB.

While Firestone and many other YDIH proponents continue to propose an airburst to explain lack of a crater and purport microspherules as evidence of the ET event, the microspherules lack a meteoritic component expected for bolide debris (see Section 10). For example, Bunch et al. (2012, p E1907) concluded that YDB **“SLOs and spherules are terrestrial in origin”** and (Sweatman, 2021, p 1) claims, **“Elemental analysis shows most microspherules are consistent with a terrestrial source.”** These purported markers cannot be the product of an airburst over an ice sheet or an impact that did not penetrate an ice sheet, and so require a solid-earth crater-forming impact (but see Section 8 and 13.7). Nevertheless, to explain YDB microspherules, Firestone et al. (2007, p 16019) speculate they **“resulted from the influx of ejecta from an unidentified, unusually Ti-rich, terrestrial source region and/or from a new and unknown type of impactor** [/bolide]**.”** Since no YDB-aged crater is known, Firestone et al. (2010a, p 56) argued the latter option of an airburst claiming, **“relatively little terrestrial ejecta were created due to the shielding of the airburst from the ground by the ice sheet”** and that “[m]**icrospherules from various sites … are comparable to lunar KREEP** [acronym for composition rich in potassium (K), rare-earth elements (REE), phosphorus (P) and a component of some lunar impact breccia and basaltic rocks] **and inconsistent with other terrestrial or meteoritic sources except for meteorite SAU-169.”** Firestone et al. (2010a, p 23) further write, **“It seems unlikely to have come directly from the moon however it is coincidental that SAU-169** [Lunar meteorite] **fell in Oman near the time of the YD**[B] **impact”**. They appear to reject but also suggest the bolide is of lunar origin rather than a comet, **“low-density object”** (Firestone et al., 2006, 2007), or **“very low density and/or unusually high velocity”** object (Firestone, 2009a, conclusion) that was proposed earlier. Teller et al. (2020, p 77) supports that scenario, **“We concur with Firestone et al. (2007), who concluded that elevated concentrations of these elements most likely resulted from processes related to cosmic impacts/airbursts including… an influx of meteoritic material from the impactor … For an example**

of the later… lunar meteorites typically enriched in Th, U, Hf, and La.” However, most YDIH proponents report microspherules have terrestrial elemental composition (see Section 10), inconsistent with an airburst/impact not penetrating the ice sheet.

Proponents of the YDIH claim that purported Pt anomalies at the YDB (Section 11) are strong evidence of meteoritic material and an ET event (e.g., Moore et al., 2017; Sweatman, 2021; Powell, 2020, 2022) and ardently cite measurements of Greenland ice by Petaev et al. (2013a). However, that Pt anomaly would require a surface impact not an airburst, and the lack of an identified YDB crater is again a serious problem for the YDIH. Petaev et al. (2013a, p 12918) observed, **“the highly fractionated Pt/Ir ratio rules out mantle or chondritic sources of the Pt anomaly”**, excluding icy comets with chondritic dust. Petaev et al. (2013a), in evaluating the YDIH calculated that if the Greenland Pt measurements are representative of a global anomaly, then the **“Pt budget at the YDB”** would **“require an iron meteorite… of ~0.8 km in diameter”** that is **“expected to form a crater of a few kilometers in diameter.”** In attempts to explain without any experimental or theoretical support the purported diamondoids in Lake Cuitzeo, Mexico YDB sediments (see Section 12.8), Kinzie et al. (2014, p 487) speculates another scenario that **“an impact took place in deep, petroleum-rich offshore sediments”**, yet no YDB-aged submarine craters were identified. Pino et al. (2019, p 21–22) wrote, **“Cr-rich spherules are found in the YDB layer at Pilauco** [Chile]**, but not found at the ~50 other sites on four continents, suggesting… airbursts occurred in the Cr-rich basaltic terrain circa Pilauco.”** However, no YDB-aged crater was identified as the source of the **“Cr-rich basaltic terrain”** ejecta.

In an attempt to explain skulls buried with microspherules, Hagstrum et al. (2017) with Firestone as a coauthor propose yet a different impact scenario where impacts/airbursts repeatedly occurred from ~46 kyr to ~11 kyr BP causing the megafauna extinctions over that time (Section 3.2). This contrasts with most YDIH versions that speculate a single impact event involving multiple fragments occurred at the YD/GS-1 onset and caused the megafauna extinctions. Pino et al. (2019, p 21) proposed a prodigious number of impacts at the YD/GS-1 onset, **“There is a reasonable probability of one or more encounters within the last 13,000 years with debris swarms from the Taurid Complex or other large fragmented comets, and such an encounter would be hemispheric in scope, lasting for only a few hours. The resulting debris field would be a mixture of dust and larger fragments, potentially equivalent to the impact of ~1000 to 10,000 destructive airbursts, such as occurred in Tunguska, Siberia in 1908… If such an event occurred at the YD onset, larger objects in the debris swarm could have created craters on land, struck the world's ice sheets, and/or impacted the world's oceans”.** A possible motivation for such claims might be found in the claims of nanodiamond-containing carbon spherules at 14 purported-YDB sites across the globe (Kinzie et al., 2014). Impact proponents claim that while carbon spherules and glassy carbon formed by common wildfires, only those that contain nanodiamonds formed within the fireball of an impact (e.g., see Kinzie et al., 2014; Wolbach et al., 2018b). Their formation would require a scenario of at least 14 separate but associated impact events across several continents. However, see Sections 9.3, 12.4, and 12.5 regarding the misidentification/ misinterpretation by impact proponents of purported YDB nanodiamonds and carbon spherules (one supplied by A. West was dated at 207 ± 87 yr BP by Boslough et al., 2012).

Sweatman (2021, p 17) supports scenarios of numerous impacts with the concept of **“coherent catastrophism”**, **“Based on 30 years prior research into the Taurid meteor stream and comet Encke, and the theory of ‘coherent catastrophism’”,** citing Asher et al. (1994), Clube and Napier (1984), and Napier (2001), then states Napier (2010) “**proposed this meteor stream as a potential culprit, citing an encounter with the equivalent of 2000–10,000 Tunguska-like objects over about an hour was a ‘reasonably probable event’.”** Sweatman (2021, p 18) also asserts, **“Napier's ‘coherent catastrophism’ scenario is later boosted by Hagstrum et al. (2017).”** However, as noted in Section 3.2, Hagstrum et al. (2017) is highly speculative.

Sweatman (2021) castigates **“Holliday et al. (2014), with Boslough as co-author”** for ignoring “coherent catastrophism” (p 18). Holliday et al. (2014) ignore it because “coherent catastrophism” is a speculative hypothesis that is unsupported by observational data and inconsistent with the cratering record. Sweatman (2021) misunderstands or misrepresents the objections by orbital dynamics and impact physicists to the extreme version of the coherent catastrophism hypothesis, which postulates without evidence that the current impact rate is grossly underestimated. As with many of the YDIH proponents' exaggerated claims, there is a grain of truth to dynamic arguments for resonant Taurid swarm and the possibility of transient increases in airburst rates when it intersects with Earth. Far from ignoring coherent catastrophism, Boslough and Brown (2018) spearheaded an effort for an observational campaign in the summer of 2019 to conduct astronomical surveys with the aim of detecting possible objects in the hypothetical Taurid resonant swarm, which is foundational to coherent catastrophism. They used computational models to show that the Tunguska airburst effects were indeed consistent with the trajectory of a Beta Taurid. Clark et al. (2019) subsequently calculated the observability of the postulated resonant swarm and recommended an observational campaign to document it in the summer of 2019. There were no reports of significant discoveries of predicted Taurid swarm objects in 2019, however. The lack of observational evidence for the predicted high-density swarm of such objects is inconsistent with the models of Napier (2010, 2019) that were invoked by Sweatman (2021) in support of the YDIH. Coherent catastrophism is also discussed in Section 5.2.

Multifarious and conflicting impact scenarios (airbursts or impact cratering) involving different impactors (exosolar comet, solar comet, lunar meteorite, iron meteorite, as well as **“new and unknown type of impactor”** (Firestone et al., 2007, p 16019) that strike at the YD/GS-1 onset or over tens of thousands of years are needed to explain the collection of otherwise disjointed indicators that impact proponents also claim can only be explained as occurring together by some unspecified mutually-compatible impact scenario. Despite the diverse scenarios, with impacts in terrains of various geologies, that are required and have been proposed for the YDIH, (Sweatman, 2021, p 1) in his review of the YDIH inaccurately proclaimed, **“The YDIH *explicitly* claims the impact event was caused by one or more low density ET objects falling onto the Laurentide Ice Sheet”** and that **“Elemental analysis shows most microspherules are consistent with a terrestrial source”** (emphasis added). Together Sweatman's statements necessarily suggest the impact must have penetrated the ice sheet leaving a yet to be recognized YDB-aged crater in North America. Sweatman (2021, p 5) asserts the **“Greenland platinum abundance** [of Petaev et al., 2013a] **is one of the key pieces of evidence”** and he incorrectly paraphrases Petaev et al. (2013a) that they **“maintained it** [the YDB impact] **must have been a massive event, likely caused by a ~ 0.8km iron-rich meteorite”** (p 4). Sweatman fails to provide a viable explanation of how the Pt anomaly in Greenland, which he misreads as attributed by Petaev et al. (2013a) to a massive iron meteorite, could be evidence for the impact of **“low density ET objects.”**

Petaev et al. (2013a) did not conclude a massive iron meteorite was responsible for the Pt measured, they only estimated its size to test a scenario that assumed a global Pt distribution commensurate with that measured in the single Greenland ice core. They subsequently pointed out that a 0.8 km iron object was unlikely to disintegrate before it struck the ground, and that no YDB crater has been found to support that scenario. The lead author of that paper who is also a coauthor of this review (MP) now attributes it to a small local event, probably the one associated with the Cape York meteorites as suggested by Boslough (2013). Despite specifically proposing the Laurentide Ice Sheet was impacted, Sweatman (2021, p 18) suggests the subglacial Hiawatha crater in Greenland is the **"YD**[B]**-age impact structure"** (Section 8.1). However, Sweatman (2021, p 19) later confounds the confused issue further by proclaiming, **"in principle no craters are required for the** [YDIH] **theory"** leaving many claimed YDIH impact markers unexplained enigmas.

8. Generally accepted impact indicators

The most direct evidence of a YDB impact is conspicuously lacking, a young and minimally-eroded YDB-aged crater. In contrast to many other bodies with solid surfaces in the solar system, the recognition of impact craters on the Earth is difficult, because active geological and atmospheric processes on our planet tend to obscure or erase the impact record in geologically short time periods. Yet of all impact structures on the Earth, if a YDB crater existed, it should be among the easiest to identify (similar to ~50 ka Barringer crater, Arizona; Yilan crater, China; Xiuyan crater, China; Lonar crater, India; and ~ 21 ka Tenoumer crater, Africa) due to its young age and minimal erosional degradation. While surface topology and remote sensing can identify localities of interest, impact craters must be verified from detailed geochemical and geophysical study of their rocks. Craters of any type and morphology are not a common landform on Earth. Impact craters (before post-impact modification by erosion and other processes) occur on Earth in two distinctly different morphological forms: simple craters with diameters up to about 2 to 4 km, and complex craters, which have larger diameters. Complex craters are characterized by a central uplift in the form of either a central peak or a central ring of hills. Recognition of geological structures and ejecta layers on Earth as being of impact origin is not easy. Even though morphological and geophysical surveys are important for the recognition of anomalous surface or subsurface structural features, which may be deeply eroded craters or impact structures entirely covered by post-impact sediments, definitive confirmation of an impact origin requires the presence of specific evidence (e.g., French and Koeberl, 2010).

Data are required to understand the ultra-high strain rate, high-pressure, and high-temperature impact process. This involves evidence of either shock-metamorphic effects in minerals and rocks, and/or the presence of a meteoritic component in these rocks. In nature, shock-metamorphic effects are uniquely characteristic of shock levels associated with hypervelocity impact. A wide variety of microscopic shock-metamorphic effects have been identified. The most common ones include planar microdeformation features; optical mosaicism; changes in refractive index, birefringence, and optical axis angle; isotropization (e.g., formation of diaplectic glasses); and phase changes (high-pressure phases; melting). To confirm an impact origin of a geological feature, proper identification of either shock-metamorphic evidence or the presence of ET component is necessary. For example, presence of melt/glass/SLOs (Section 4.2), "spherules" of any sort (Sections 11 and 12.4), and nanodiamonds (Section 12), often cited in favor of an impact origin, by themselves are NOT unambiguous or unique evidence for impact (see below).

Although projectile fragments rarely survive an impact event, detectable amounts of melted and recondensed projectiles are often incorporated into impact-produced breccias and melt rocks during crater formation. This dispersed projectile (meteoritic) material can be conclusively identified by distinct chemical and isotopic signatures in the host rocks, thus providing reliable evidence for a meteorite impact event. Geochemical lines of evidence can include the following: elevated platinum-group element (PGE) abundances and interelement ratios (with the caveat that in some cases terrestrial geological processes can lead to increased abundances) and (better) various isotopic compositions, such as characteristic Os, Cr, or W isotopic ratios (e.g., Koeberl, 2014, and Koeberl et al., 2012, and references therein). Similar to other aspects of impact studies, geochemistry is vulnerable to over interpretation and wishful thinking. Data must be carefully obtained and verified using independent methods at multiple laboratories as well as calibrated with the appropriate methods and standard reference materials. All lines of evidence must be seen in context and not in isolation. Any "new and unique" methods or observations must first be verified at confirmed impact sites. This is of course a main problem with many of the more outlandish claims made about the YDB "impact evidence". For a discussion of the problems associated with viewing some potential impact-characteristic criteria in isolation, see section 8.4. in French and Koeberl (2010), and the discussion in Reimold et al. (2014).

8.1. *Proposed YDIH craters*

Discovery of a possible large terrestrial crater in Greenland generated interest on behalf of YDIH proponents as a potential YDB "smoking gun." Sweatman (2021 p 18) notes **"**[Kjær] **et al. (2018) report the discovery of a large impact crater beneath Hiawatha Glacier in northwest Greenland. From airborne radar surveys, they identify a 31-km-wide, circular bedrock depression beneath up to a kilometer of ice. They further suggest the impactor… is less than a few million years old…. This maximum age is confirmed a year later (Garde et al., 2020). Clearly, this crater is a candidate YD-age impact structure"** (see also Table 8). This is a pointless endorsement. Garde et al. (2020, p 870) clearly state **"In summary, the age of the organic carbon at Hiawatha is probably 3–2.4 Ma, and we favor the younger, 2.4 Ma age as the simplest interpretation and a realistic maximum age of the impact."** This is no embrace of a middle or late Pleistocene age for the crater, much less a terminal Pleistocene or YDB age.

Speculations of a very young age for the Hiawatha Crater were abandoned before the peer-reviewed discovery announcement by Kjær et al. (2018) (Boslough, 2019), but the mere possibility of a recent impact was embraced by YDIH proponents whose opinions were uncritically played up in news reports. James Kennett stated, despite lack of evidence and extremely low probability, **"I'd unequivocally predict that this crater is the same age as the Younger Dryas"** (Voosen, 2018). Such expressions of certainty influenced others. Powell (2020) devoted an entire chapter to Hiawatha, justifying the lack of debris in Greenland ice cores by citing a model showing that an impact into ice inhibits ejection of material. Of course, the material blasted out of the crater would have had to go somewhere—even if it was not to the ice summit–or the crater would not exist. Powell (2020), seemingly rejecting the model he had just cited for lack of ejecta, concludes by suggesting that if the crater were young, then cores of YDB age from the seafloor of Baffin Bay should be **"full of the characteristic impact markers"** (p 123).

As YDIH proponents demanded elsewhere, precise age control is essential to support the YDIH. A maximum age of 2.4 Ma for the crater renders the possibility that it is 12,900 yr old as statistically highly improbable. This improbability is verified by the dating that now shows it is ~58 million years old (Kenny et al., 2022). In addition, YDIH proponents hypothesize or propose that the Carolina Bays (see Section 13.1), depressions in the Great Lakes (see Section 7), the Bloody Creek structure Canada, Coroossol structure Canada (Wu et al., 2013), and even kettle lakes (Ballard, 2017) are YDB-aged craters, but there is no evidence to support any of these claims.

In the absence of a young, minimally-eroded crater (see Section 13.7), impact proponents presented a wide and sometimes wild range of claimed particulate and geochemical indicators of a cosmic impact (e.g., Firestone and Topping, 2001; Firestone et al., 2006, 2007; Sweatman, 2021; Powell, 2022). Purported impact indicators include: charcoal, magnetic grains and spherules, various forms of melt glass spherules, elevated concentrations of platinum group elements, nanodiamonds, as well as carbon spherules and glass-like carbon containing nanodiamonds. In addition to these, fanciful and peculiar indicators have also been claimed that appear to be largely abandoned (see Section 13). Furthermore, Sweatman (2021, p 5) claimed, **"simultaneous and dramatic onset of the Younger Dryas cooling and extensive**

wildfires recorded in the Greenland ice points, rather, to a massive impact event", echoing implicit claims by YDIH proponents that products of global wildfire are impact indicators (but see the next Section). Firestone et al. (2007, p 16019) proposed **"glass-like carbon, carbon spherules, and nanodiamonds were produced in the YDB by high temperatures resulting from the impact and associated biomass burning."**

9. Purported YDIH evidence of impact-induced wildfires

Proponents of the YDIH assert that there is abundant evidence for wildfire over one or more continents at the beginning of the YD/GS-1 (e.g., Kennett et al., 2008a; Wolbach et al., 2018a, 2018b; Sweatman, 2021). James Kennett reportedly claimed with respect to North America, **"the entire continent was on fire"** (Pringle, 2007). Such assertions are inconsistent with observations from multiple indicators of paleofires that no such peak in biomass burning exists. This notion of widespread fire as an element of the YDIH seems to arise from misinterpretation of global syntheses of paleofire data, misinterpretation of the Greenland ice-core record of fire, and miscellaneous misapprehensions of the literature. Purported indicators of wildfires include: microcharcoal, soot, carbon spherules, and glass-like carbon (in addition to sedimentary charcoal and ammonium ions in ice cores). While these are not unique to an impact, YDIH proponents claim they are indicators of impact-generated wildfires based on their purported near global and synchronous distribution.

9.1. Misperception of Global Charcoal as evidence of impact

In his discussion of the compilation of charcoal records by Power et al. (2008) (which led to the first Global Charcoal Database, GCD v. 1), Sweatman (2021, p 12) misinterprets fig. 2 of Power et al. (2008): **"Power et al. (2008), working with the World Charcoal Database (WCD)** [sic], **find conspicuous peaks in charcoal abundance between around 13 and 11 kyr (their Fig. 2), the highest over the entire duration of their record (24 kyr)."** What fig. 2 of Power et al. (2008) actually shows are "raw" or as-published charcoal values, which range over ten orders of magnitude, arising from the diverse ways in which charcoal is measured and reported (e.g., as influx, concentration, or percentages, based on counting both macro- and microscopic particles, and also based on chemical analyses). Power et al. (2008) use that figure to motivate the standardization approach they applied (their p 890), not to represent a global summary. Failure to standardize charcoal records would be analogous to attempting to calculate average precipitation over a region using records expressed sometimes as inches and sometimes as millimeters, without converting from one unit of measurement to the other (Power et al., 2010).

Sweatman (2021, p 12) goes on to state that **"However, after application of several data transformation techniques, these peak abundances are no longer apparent in their regional plots of charcoal anomaly (their Fig. 5). Instead, we see a weak signal in the period 13.5 to 12.5 kyr in most regions of the world…"**. He suggests that (p 12) **"Probably, the weakness of this signal, given the abundances evident in the original database, indicates that their data analysis methods are not suited to isolating and highlighting the main charcoal anomalies over the last 24 kyr."** This amounts to saying that because fig. 5 of Power et al. (2008) did not look like what Sweatman expected it to look like, based on his misinterpretation of their fig. 2, Power et al.'s data analysis protocol must therefore have been inappropriate. A more parsimonious interpretation is that the peaks expected by Sweatman are simply not there. (This leaves aside the observation that the bin width of the data summarization in Power et al.'s fig. 5 is 500 years, and therefore not likely to show prominent peaks in the first place.)

Interestingly, Sweatman (2021, p 12) buttresses his assertion that there is an issue in Power et al.'s (2008) failure to portray the expected peaks by citing Marlon et al. (2009): **"Using the WCD** [sic] **again, Marlon et al. (2009) examine 35 high resolution charcoal records for North American lake sediments, finding a strong signal for anomalous fire frequency in the range 13,100 to 12,700 cal BP, described as '*… the largest and most rapid change in biomass burning during deglaciation*'** [his emphasis added]**."** What Marlon et al. (2009, p 4) actually said was **"A particularly steep increase in charcoal influx occurred at 13.2 ka (Fig. 1C); this is the largest and most rapid change in biomass burning during deglaciation."** They quite clearly relate this increase in fire not to the onset of the YD/GS-1 but to the abrupt cooling and subsequent warming associated with the occurrence of the "Inter-Allerød Cold Period" (IACP, now referred to as GI-1b, Rasmussen et al., 2014) (Figure 1). The cold phase, 13,311 to 13,099 yr [b2k, GICC05], was followed by GI-1a, the abrupt-warming phase, 13,099 to 12,986 yr [b2k, GICC05] (Rasmussen et al., 2014). Marlon et al. (2009, p 2522) stated: **"The timing and distribution of fire activity at 13.2 ka is consistent with the IACP—an abrupt short-term climate reversal recorded in the GISP δ^{18}O ice-core data…."** This is a robust result. Indeed, Wolbach et al.'s (2018b) reanalysis of Marlon et al.'s (2009) results (over a narrow time window, ~13,440 to ~12,280 cal yr BP, their fig. 3), which includes more records and an alternative "Bayesian errors-in-variables model" (that explicitly accounts for age uncertainties), clearly shows that the increase in charcoal influx begins hundreds of years before the onset of the YD/GS-1.

The amplitude of the oxygen-isotopic increase from the "coldest" part of GI-1b to the end of GI-1a is roughly half of that for the warming at the end of the YDC, and rivals those of many of the Dansgaard-Oeschger events (e.g., see fig. 1 of Rasmussen et al., 2014). Daniau et al. (2010) using long sedimentary charcoal records, and Fischer et al. (2015) using ammonium-ion data from the NGRIP and GRIP ice cores both reach the same conclusion: abrupt climate changes correlate with parallel abrupt changes in biomass-burning records. The biomass-burning signature in those records increases in response to abrupt warming, as most recently at the *end* of the YD/GS-1 (11,703 ± 4 yr [b2k, GICC05]), and decreases in response to abrupt cooling, as most recently at the beginning of the YD/GS-1, i.e. *after* the post-IACP increase discussed above.

9.2. Misinterpretation of the NGRIP ammonium-ion record

Sweatman (2021, p 14) reiterates Wolbach et al.'s (2018a, 2020) assertions about the significance of ammonium-ion (NH_4^+) peaks near the beginning of the YD/GS-1: **"But when we discount the Holocene period, which will be affected by anthropogenic biomass burning and a much warmer climate (and is therefore not a fair comparison), there are few other major peaks in ammonium ion concentration observed in these ice cores over the last ice age, and it is clear none are as significant as that at the YD onset (see Figs. 2 to 4 of Fischer et al. (2015)."** Holliday et al. (2020) refuted claims such as these in their table 8. For example, Wolbach et al. (2018a, p 169, fig. 3 caption) state **"NH_4 peaks** [in the GISP2 core] **marked with triangles are the two highest within the 120,000-y record"**. Holliday et al. (2020) noted that **"The two values are 77.3 ppb (at 12,711.5 BP) and 49.6 ppb (at 12,805.5 BP), the 8th and 25th largest values in the record"** based on the primary data from the cores. In turn Wolbach et al. (2020, p 97) replied **"biomass burning proxies have been investigated extensively in three Greenland ice cores spanning the past ~120,000 y. The result is that YDB peaks in ice-core concentrations of ammonium, nitrate, oxalate, acetate, and formate are greater than or equal to ~99% of all other peaks elsewhere in the ice cores, and in some cases, the peaks are the largest in the entire ice record, confirming that these peaks are extraordinary."** Simple inspection of the data files discredits their assertion. Further, using the NGRIP ammonium ion record (Fischer et al., 2015) as an example, in a record with 95,036 data points, 950 values will be above the 99-th percentile, which is an extraordinarily large number on which to base a claim of exceptionalism.

Because these assertions can so easily be refuted by simple inspection of the data (Holliday et al., 2020, tables 7 and 8), we wonder why they are still being made. A clue may be found in Sweatman's (2021, p 14) attempt to explain the discrepancy between Wolbach et al.'s (2018a) plot of the NGRIP NH_4^+ and the published data (in their fig. 3), where Wolbach et al. evidently smoothed the data: **"this is consistent with the smoothing applied by Fischer et al.... used to highlight prominent peaks in ammonium abundance in the GRIP and NGRIP ice cores (see Fig. 4 of Fischer et al. (2015)."** This statement is similar to Wolbach et al.'s (2020, p 98) contention that the **"running averages"** plotted in Fischer et al.'s (2015) fig. 4c show that **"the peaks at the YD onset are more than twice as large as any other peak within the past 10,000 to 100,000 y"**.

However, the smoothing (actually an averaging) in Fischer et al. (2015) was not intended to "highlight peaks" in plots of ammonium from ice cores, but instead was used to decompose the NH_4^+ record into background and peak components by determining outliers (i.e., peaks) from the mean. The latter of which represent the contributions from fires: **"To this end, we use the peak-insensitive running median in a 101-yr window to quantify changes in *background concentrations at the source derived from soil emissions* and use a robust outlier detection method (see Methods and Supplementary Information) to identify *fire peaks in the source concentration, quantify their concentration above the corresponding background and calculate the FPF*** [Fire Peak Frequency]**"** (Fischer et al., 2015, p 724, emphasis added). This two-component (background and outlier) model of the NH_4^+ record, is illustrated for the present day by analyses of six shallow firn cores from Greenland (Kjær et al., 2022). They write "**NH_4^+ has a distinct maximum in the late spring and early summer months (April–June, Fig. 4, top, middle) associated with high biological activity, while minimum concentrations occur in a wider part of the year from late autumn and early winter (October–December). The variability is high between the individual years... a result of an additional source in summer and early autumn, namely the Canadian forest fires, and the uneven seasonal shape is more clearly evidenced in the cores closest to the Canadian forest fire source area**" (p 2216). The decomposition approach followed by Fischer et al. (2015) is the same as that applied to individual terrestrial charcoal records to separate peaks in charcoal influx records related to local fires from the "background" charcoal influx contributed from distant or extra-local fires (Higuera et al., 2010; see Marlon et al., 2009, SI for examples). In both applications (to individual charcoal records and to the Fischer et al. NH_4^+ ice-core data), a smooth background curve is created, and fire peaks are identified as those values that exceed the background by a specific threshold value. The smoothed curves in Fischer et al.'s (2015) figs. 2-4 panel b (in red), and panel c (in orange) therefore do not represent biomass burning, but instead are indices of the emissions of NH_4^+ from soils. Peaks in NH_4^+ emissions related to fires represented by the outliers are shown in red panel a of their figs. 2–4 (along with the median absolute deviation (MAD)-derived thresholds) and are summarized as fire-peak densities (peaks per 201 yr intervals) in panel f of their figs. 2–4. Like the raw data, neither of these series (NH_4^+ emissions related to fires, or fire-peak densities) show exceptionally large values at the beginning of the YD/GS-1.

To further investigate Sweatman's (2021, p 14) claims of significance, we can examine the 2925 samples falling between the beginning of the Bølling–Allerød chronozone (or GI-1) (at 14,642 ±4 yr [BP1950, GICC05], Rasmussen et al., 2014) and the end of the YD/GS-1 (at 11,653 ±4 yr [BP1950, GICC05]) (Figure 1). Inspection of the data in the Supplementary Information of Fischer et al. (2015) shows that of the ten highest **"fire event concentrations... at the source in excess of background concentrations"** during that interval (see Fischer et al., 2015, figs. 2–4, panel a), nine occur *before* the beginning of the YD/GS-1, with the youngest of those falling at 13,094 yr [BP 1950, GICC05]. The eighth-highest peak, at 12,645 yr [BP1950, GICC05] occurs 200 yr after the beginning of the YD/GS-1. The highest peak within a window 100 yr

Table 8
Critiques said to support the YDIH.

Critique of YDIH	Sweatman (2021) comments (Italics in quotes added for emphasis)
Gill et al., 2009	p 12: **"Even casual inspection of their radiocarbon measurements in their supplementary information... suggests this charcoal layer is not inconsistent with a Younger Dryas age, and, moreover, it appears coeval with the onset of a period of dramatic change in vegetation around the lake apparent between 850 and 780 cms whose duration correlates well with the Younger Dryas period.... This is an important observation because their highly-cited work is often used to refute the impact theory. Rather, this work could be viewed as strongly supporting it."** Caption of fig. 9 "**there is sufficient uncertainty in this data such that 850 cm might correspond to the onset of the Younger Dryas cooling."**
Response	No proof of YDB position without circular reasoning.
Haynes et al., 2010	p 12: **"Andronikov and Andronikova (2016)... concluded that the base of the black mat at...** [Murray Springs] **likely contains an abundance of microscopic charcoal particles with a similar trace element signature, with elevated levels of REEs, to macroscopic charcoal particles from the same location and from the Ussello horizon in Europe, but distinct from modern charcoal particles. This might explain why macroscopic charcoal pieces are not often observed within the boundary layer at Murray Springs – an observation used by Haynes et al. (2010) to dispute the impact theory. Instead, the charcoal expected at this site is, it appears, mostly dispersed as microscopic dust."**
Response	What Haynes does not see or report therefore supports the YDIH (i.e., on the basis of no data).
MacGregor et al., 2019	p 18–19: "**MacGregor et al. (2019) report the discovery of a possible second subglacial impact crater... southeast of the Hiawatha impact crater."** Sweatman then goes on to argue on the basis of no field data that **"the probability that the Hiawatha crater, and its potential twin, are YD impact structures".**
Response	The entire issue is moot. The Hiawatha crater has been dated to ~58 Ma (Kenny et al., 2022).
Pigati et al., 2012	p 6: **"concluded that a meteoric origin for the Younger Dryas boundary microspherules is unlikely. However, this argument is flawed, as it is well known that magnetic microspherules can be produced by several routes. Their existence within other stratigraphic horizons actually says nothing about the origin of microspherules at the Younger Dryas boundary. Only more detailed examination, such as microspherule surface texture analysis and elemental abundance, can determine this, which they did not do."** p 18: Hagstrum et al., 2017 **"searched the Fairbanks and Klondike mining districts of Alaska, USA, and the Yukon Territory, Canada, and found large quantities of impact-related microspherules in fine-grained sediments retained within late Pleistocene** [megafauna]. **Raised levels of platinum were also found. These deposits are then reinterpreted partly as blast debris that resulted from several episodes of airbursts and ground/ice impacts within the northern hemisphere during the Late Pleistocene epoch (~46–11 ka BP).** **This result supports earlier observations by Pigati et al. (2012) who.... concluded their data was inconsistent with a cosmic impact origin, but their implicit assumption is that multiple cosmic impacts over this time are extremely unlikely. Clearly, they did not consider coherent catastrophism, which might partially explain their data, as a potential scenario."**
Response	Hagstrum et al. (2017) might partially explain findings of Pigati et al. (2012), but Hagstrum et al. have no specific numerical age control for their field site; simply an interpretation of "blast deposits" due to purported airbursts or impacts. Sweatman (p 6) rejects Pigati et al. and then tentatively accepts (p 18, 20).

(*continued on next page*)

Table 8 (*continued*)

Critique of YDIH	Sweatman (2021) comments (Italics in quotes added for emphasis)
Sun et al., 2020	p 19: **“propose a volcanic origin for the geochemical signals at the YDB. In their work focused on Hall’s Cave, Texas, they examine the osmium isotope record of the cave’s sediment and find that, at the YDB, sediment samples with osmium isotope anomalies don’t appear to have corresponding anomalies in other PGEs. Despite all the preceding evidence in this debate to the contrary, they interpret this as evidence for a volcanic trigger for the YD climate event. However, in their table S1 there are five sediment records at 151 cm corresponding to the depth of the YDB, one with a platinum anomaly (435.1 ppt) but no osmium anomaly, and one with an osmium anomaly ($^{187}Os/^{188}Os$ ratio of 0.41) and no platinum anomaly. *Probably, as these five measurements were all taken laterally at the depth of 151 cm, this variation in the data reflects the slowly undulating nature of the stratigraphy of the Hall’s Cave sediments and/or the ‘nugget’ effect.* Clearly, Sun et al.’s (2020) decision to focus on the sample with the osmium anomaly is selective and unjustified.”**
Response	Contradictory data “probably” represents a sampling issue but that is not based on direct proof or any experience seeing the samples in place.

either side of the beginning of the YD/GS-1 is the 21st largest peak within BA/YD interval, and the 241st largest peak in the record overall. Fire-peak-frequency (FPF), expressed as the number of peaks per 201 yr intervals (plotted in 50 yr increments in Fischer et al., 2015, figs. 2–4, panel f), is also quite low at the beginning of the YD/GS-1: 6 peaks / 201 y, the 52nd largest value within the BA/YD interval, and the 1512th highest among the 1947 50 yr bins in the whole record. Notably, in the FPF record, distinct minima correspond to the cold phase of GI-1b (the IACP) and to the onset of the YD/GS-1. Neither the magnitude of the individual fire peaks, nor the fire-peak frequency at the beginning of the YD/GS-1 are **“quite special”** as claimed by Sweatman (2021, p 14). The background levels of source NH_4^+ emissions (the orange curves in Fischer et al., 2015, figs. 2–4, pancel c) do rise to a maximum around 12,740 yr [BP1950, GICC05] but to reiterate, Fischer et al. (2015) attribute those background trends to emissions from soils, and not to fires.

Fischer et al. (2015, p 726) observe **“This temporal evolution of average NH_4^+ emissions for fire events from 15 to 10 kyr BP is overall in line with the NA** [North American] **charcoal flux record** [Marlon et al., 2009]**.”** The two records differ in their depiction of fire-peak frequencies, which is not surprising given the decadal (sedimentary charcoal) vs. the annual (ice core) resolutions of the data sources. However, Fischer et al. (2015, p 726) note that their FPF record **“… shows a rather immediate response to the BA climate variations and a clear YD minimum”**. If anything, the NGRIP NH_4^+ record offers strong evidence *against* a biomass-burning peak at the beginning of the YD/GS-1.

9.3. *Miscellaneous wildfire misapprehensions*

There are other misapprehensions about wildfire as an indicator of an impact, and about the nature of the paleofire record in general throughout the YDIH literature. For example, Sweatman (2021, p 12) wrote, **“…evidence for extensive wildfires is an essential component of the Younger Dryas impact hypothesis.”** Firestone et al. (2007) in their version of the YDIH claimed **“charcoal, soot, carbon spherules, and glass-like carbon, all of which suggest intense wildfires”** (p 16017) and **“superheated ejecta** [from an unknown crater] **which would have decimated forests and grasslands, destroying the food supplies of herbivores and producing charcoal, soot, toxic fumes, and ash.”** (p 16020). Soon after Kennett et al. (2008a) wrote, **“we present evidence for the co-occurrence of massive wildfire, abrupt vegetation change, Mammuthus exilis extinction and disruption in human use of California’s Channel Islands at ∼13–12.9 ka.”** (p 2531) and the YDB contains **“high concentrations of charcoal, ‘elongate’ carbon particles, and carbon spherules indicative of intense biomass burning”** (p 2542). Furthermore, Kennett et al. (2009b, abstract) wrote, **“shock-synthesized diamonds are also associated with proxies indicating major biomass burning (charcoal, carbon spherules, and soot).”** The titles of Wolbach et al. (2018a, 2018b) are **“Extraordinary Biomass-Burning Episode and Impact Winter Triggered by the Younger Dryas Cosmic Impact…”**, no less. Sweatman (2021, p 5) echoing these assertions makes a sweeping claim linking YD cooling and wildfires and wrote **“the apparently simultaneous and dramatic onset of the Younger Dryas cooling and extensive wildfires recorded in the Greenland ice points… to a massive impact event”** citing Petaev et al. (2013a). But Petaev et al. (2013a, p 12918) explicitly reject the idea that their Pt discovery was linked to wildfires stating: **“The Pt anomaly precedes the ammonium and nitrate spike in the GISP2 ice core…** [Mayewski et al., 1993] **by ∼30 y and, thus, this event is unlikely to have triggered the biomass burning and destruction thought to be responsible for ammonium increase in the atmosphere and the Greenland ice** [Firestone et al., 2007]**.”**

Sweatman (2021, p 12) appears to argue charcoal, soot and carbon spherules are markers of global wildfire, **“…if the impact occurred, we should find a coetaneous abundance of charcoal, soot, and other wildfire indicators, such as carbon-rich microspherules, widely distributed across a large fraction of Earth’s surface. It is not necessary to demonstrate they were produced by a cosmic impact, because charcoal and soot are not, by themselves, diagnostic of an impact.”** The last sentence is a non sequitur, and clearly contrary to the scientific method. These materials commonly occur in sediments and cannot be used to support any proposed environmental process (e.g., cosmic impact, volcanism, excessive atmospheric O_2 build up) without evidence linking those materials to that process. In any case, there is no compelling evidence of wildfire synchronicity across a large area of Earth’s surface such as a hemisphere.

In any one year, fire occurs over many areas of the Earth’s surface (Bowman et al., 2009; Scott, 2020). Many of these are started by lightning strike and have occurred on Earth for more than 400 million years (Scott, 2000, 2018). An additional ignition source has come in the form of humans (Bowman et al., 2011; Roos et al., 2014). An extra-terrestrial impact represents another albeit rarer potential ignition source (Jones and Lim, 2000) but the problems with the suggestion of a major fire triggered by an impact are many. We consider a number of issues.

The major difficulty in ascribing continent-wide impact-related wildfires at the beginning of the YDC is that the data presented comprises out-of-date concepts, misidentified products and misinterpreted data and we find no fundamental reason to believe that the fires, as interpreted, existed. None of the papers proposing wildfire ignition by an impact/airburst provide any evidence demonstrating the widespread ignition of fires from an impact.

Firestone et al. (2007, p 16020) state, **“Svetsov… calculated that a Tunguska-sized airburst would immerse the ground with a radiation flux severe enough to ignite 200 km^2 of forest within seconds. Thus, multiple, larger airbursts would have ignited many thousands of square kilometers.”** Firestone et al. (2007, p 16020) further wrote, **“At greater distances, the reentry of high-speed, superheated ejecta** [from an unknown crater] **would have induced extreme wildfires** [Schultz and D’Hondt, 1996]**… The number of ET airbursts or impacts necessary to induce the continent- wide environmental collapse at 12.9 ka is unknown.”** These speculative comments contain significant problems in their calculations and inferences (Belcher, 2009; Belcher et al., 2003, 2005, 2009, 2015). Instantaneous ignition of fires across a continent due to an impact is unlikely given the diversity of fuel types, amounts and condition (Belcher et al., 2015).

While there are striking witness accounts of fires caused by the 1908 Tunguska bolide (most are in the Russian literature, but see Krinov, 1966; Rubtsov, 2009; Jenniskens et al., 2019), questions on the magnitude and extent of the fires have been raised (e.g., see Jones, 2002). Scientific expeditions to the Tunguska taiga shed insight on the wildfire question, however they occurred decades after the event. The first was in 1927 led by Leonid Kulik who observed that **"…the majority of leveled trees were not charred; instead, they were just singed, but traces of this singeing could be seen everywhere to a distance of 10–15km from the center of the flattened forest area"** (Rubtsov, 2009, p 148). Krinov (1966, p 249) wrote with regard to those expeditions, **"first striking observation… was that only those dry trees which have bark remaining on them show signs of scorching. Otherwise, if the bark has been broken off the trees, (which is most frequently observed), the wood of the trunk itself does not show any scorching."** Krinov further wrote, **"most characteristic feature of this scorching was that the end of every snapped-off branch from the dead trees held a charred cinder… This suggest that the scorching occurred from the momentary effect of high temperature and not from ordinary forest fire** [including any caused by an impact] **… if the scorching had occurred from the flames of an ordinary fire, the slender twigs would have been completely burned by heat sufficient to char the end of a thick twig."**

Florenskiy (1963, p 7) wrote, **"fire expert N.P. Kurbatskiy (The Forestry and Lumber Institute of the Siberian Branch of the Academy of Sciences) draws the following conclusion: …the region of meteorite impact in 1908 was basically a fire-devastated area that had been subjected to a treetop fire during the first half of the last century. A partly flattened dead and rotting forest was standing in this area. New forest growth had appeared among the dry and charred trees."** Florenskiy (1963, p 5) also wrote, **"The presence of live trees at the center of the catastrophe… bears witness to the comparatively low level of any possible flash burning."** Therefore, past fire damage could cause a misimpression of greater fire levels caused by the Tunguska event. Vaganov et al. (2004, p 392) wrote, **"the role of the Tunguska event in widespread wildfire seems to have been overestimated (**[Nesvetajlo], **1998). Tree-ring-dated fire histories indicate that 1908 was characterized by markedly greater fire occurrence than the long-term average at widely separated locations between the Tunguska and Angara rivers as well as on the left bank of the Enisey river, about 600 km to the west from the epicenter (Arbatskaya and Vaganov, 1996, 1997)."** Nesvetajlo (1998, p 156) wrote, **"'telegraph pole' forest zone… is a concentration of dead standing trees, which unlike the trees which die naturally and have dry branches, were stripped of branches by the explosive wave"** and **"highest concentration of dead standing trees are found in the epicentral zone of the impact site. The increased number of dead standing trees in some other peripheral areas of the felled tree zone are associated with earlier fires and inexplicable by hypothesis of central explosion. … there are also traces of fire prior to 1908 in the 'telegraph pole' forest area (Furjaev, 1975)"** Nesvetajlo (1998, p 157) further wrote, **"Despite 80 years that have passed since the 1908 catastrophe year, the dead standing trees unlike those that were felled by the explosion, have preserved very well and one can reconstruct whether the branches were dead or living at the moment of the impact by analyzing the peculiarities of the annual tree rings. Application of this simple technique has proved that the charcoal occurs only at the ends of those branches, that had already been dead by 1908. All test lots proved that there was no trace of thermal damage at the ends of the branches that were living in 1908, which fact imposes certain restrictions on the size of the thermal impulse, that caused tree damage. Distribution character of charcoals at different heights in the test lots permits to the assertion, that these damages were generated by an upward thermal flow, directed from the surface of the ground; whereas if it has been a hurricane of red-hot gases or a gas-dust cloud, charcoal would have been distributed evenly within the area. However, the damage does not occur everywhere, and the distances between the lowest branches are charred, can be as short as 30 to 50 m (Nesvetajlo, 1986)."** While the Tunguska bolide did cause fires, physical evidence of extensive wildfires is lacking. As is evident from the dendrochronology, which showed there were trees in the blast area that survived, igniting live healthy growth is difficult.

The idea of high-intensity severe and widespread wildfires ignited by an impact is attractive but is a claim based upon misidentification and misunderstanding of the nature of wildfire products. This is the case with all the YDIH papers (e.g., Firestone et al., 2007, 2010a; Firestone, 2009a; Kennett et al., 2008a, 2009b; Wittke et al., 2013a; Wolbach et al., 2018a, 2018b). For example, impact proponents claim carbon spherules and glassy carbon are products of wildfire and are purported to peak in abundance at the YDB. Carbon spherules are not an indicator of wildfire. They have nothing to do with high temperature or the intensity of fires. Many of those reported and illustrated by YDIH proponents are fungal sclerotia, many belonging to the genus *Cenooccum* (Scott et al., 2010, 2017). The numbers occurring in any sample bear no relationship with a large wildfire. In addition, Kennett et al. (2008a, 2009b) also claim an elongated form of carbon spherules are products of wildfire and peak in abundance at the YDB. Carbon elongates are not an indicator of wildfire. Many are identical to arthropod coprolites and termite frass (Scott et al., 2010, 2017). Similarly, glassy carbon is not indicative of high temperatures in wildfires (McParland et al., 2010; Scott et al., 2017).

In the SI Text, Research sites, Firestone et al. (2007) state: **"Two surface samples also were taken from recent modern fires in Arizona; they were the Walker fire, which was a forest underbrush fire in 2007 and the Indian Creek Fire near Prescott in 2002, which was an intense crown fire. Trees mainly were Ponderosa pine and other species of yellow pine. Only the crown fire produced carbon spherules, which were abundant (≈200 per kg of surface sediment) and appeared indistinguishable from those at Clovis sample sites. Both sites produced glass-like carbon fused onto pine charcoal."** However, no evidence was provided to indicate that these were high-intensity fires nor what was meant by such a term (see Scott et al., 2014 and Scott, 2020 for a discussion of wildfire). The authors apparently were unfamiliar with modern wildfires and their residues (see also Scott, 2010 for a discussion and illustration, and Glasspool and Scott, 2013). Scott et al. (2010, SI fig. S7) clearly show that carbonaceous spherules collected from modern low-temperature surface fires are fungal sclerotia, a fact ignored by all the YDIH authors. In addition, 'glass-like carbon' does not indicate high temperature – in fact it occurs widely in moderate temperature charcoalification of woods (see McParland et al., 2010).

Firestone et al. (2007, p 16018) wrote, **"High-temperature PAHs, which were found at the K/T boundary** [Venkatesan and Dahl, 1989], **are present in the YDB… suggesting that intense fires occurred"**. However, Venkatesan and Dahl (1989) only examined marine K/T specimens. Belcher et al. (2009) examined non-marine K/T specimens with higher PAHs concentrations and demonstrate that many of the pryosynthetic PAHs occurring in the K/T (K/Pg) impact horizon are a result in the vaporization of hydrocarbon source rocks at the impactor site and not related to wildfire. Wolbach et al. (2018b, p 200) wrote, **"Peaks in YDB biomass burning proxies include charcoal, carbon spherules, glass-like carbon, AC/soot, fullerenes, and PAHs."** Both soots and PAHs are combustion products and may have a variety of sources (acknowledged by Wolbach et al., 2020, p 96). However, PAHs can also have petrogenic or biogenic origin (e.g., Tan et al., 1996; Zhang et al., 2014; Wakeham and Canuel, 2016). Abundant "carbon spherules" are often purported at the YDB by YDIH proponents and sclerotia, ubiquitous in soils/sediments, provide precursors for production of the PAH perylene during their diagenesis (Itoh et al., 2012). Therefore, not all PAHs are related to impacts nor the burning of vegetation and there is little understanding of the relationship of their quantity and distribution, particularly in relation to fire size. Smoke plumes may cause concentration or wide dispersal and finding the origin of such materials is not simple and for the most part impossible (Scott et al., 2014).

Furthermore, YDIH proponents that purport PAHs in YDB sediments never identify the specific PAH molecules and their molecular distribution. Consequently, the sources of these PAHs have not been identified (e.g., see Wang and Fingas, 2003), and in principle they can be mostly biogenic in origin.

Smoke contains other chemical tracers from biomass burning such as black carbon, charcoal and kerogen (e.g., Andreae et al., 1998) but the amounts and composition of these vary depending on the nature of the fires. Much of the black carbon is a sub-micron aerosol that can be transported over hemispheric distances (Slater et al., 2002). Sweatman, (2021, p 12) even states, **"it is clearly difficult to distinguish charcoal burned by natural wildfires from those incinerated by a cosmic impact…"** Andreae et al. (1998) reported that the major ionic species from savanna fires were Cl, SO_4^{2-}, K^+, and NH_4 but the amount of these as well as black carbon species depended on the nature and type of fire.

Ammonia is only one of the aerial tracers from biomass burning (Ryu et al., 2004). Ammonium is initially emitted as ammonia (HN_3) in the atmosphere (Kellerhals et al., 2010) and can be produced by lightning, but only a fraction of that lightning *might* produce wildfires. Polycyclic aromatic hydrocarbons from biomass burning may be varied (Simoneit, 2002) and are recovered in the fossil record (Marynowski and Simoneit, 2009). However, there are many issues in using these to interpret wildfires and interpretations must be made with care (Marynowski et al., 2011).

The potential diversity of both the vegetation types and fuel types across a large area such as a continent makes an all-consuming fire unlikely. Live fuels are very difficult to ignite (Belcher et al., 2015) as evident at Tunguska. A fire is very unlikely to start instantaneously across a continent given the diversity of fuel types, amounts and condition. In none of the papers by those proposing wildfires ignited by an impact/airburst has any evidence been presented that demonstrates the widespread ignition of fires from an impact.

Sweatman (2021, p 14–15) commented on the reanalysis of charcoal data by Wolbach et al. (2018b), "**Holliday et al. (2020) first question the selection by Wolbach et al.… of 30 additional North American lake records, implying their choice might be biased. But this accusation is not substantiated. For example, Holliday et al.…. do not provide examples of any North American lake records without a charcoal abundance at the required time.**" Such examples of charcoal records without peaks were already in the literature, e.g., Marlon et al. (2009, fig. 2 and SI figs. S2 and S3). As pointed out previously, the reanalyses of lake sediment charcoal data by Wolbach et al. (2018b) do not appreciably differ from that in Marlon et al. (2009).

Sweatman (2021, p 14) revisited his take on Marlon et al.'s (2009) explanation for the rise in charcoal prior to the onset of the YD/GS-1, this time hedgingly agreeing, stating **"They** [Holliday et al., 2020] **further suggest that the peak in charcoal abundance identified by Wolbach et al… spanning the onset of the Younger Dryas period identified in 65 North American lake sediment records might instead reflect the improved climate that peaked during the Bolling-Allerod period several hundred years earlier. Certainly, this is a possibility,"** and he continues **"but as has already been argued, it is a moot point since charcoal is not by itself an impact proxy. All Wolbach et al…. need to show is that a widespread abundance of charcoal near the onset of Younger Dryas cooling, within dating uncertainty, exists, and this is clearly accomplished."** Sweatman (2021) seems to be arguing that a) everyone agrees that fires are not a direct impact indicator, b) the increase in biomass burning in North America observed in charcoal data by Marlon et al. (2009), Wolbach et al. (2018b), and in NH_4^+ FPF data following the cold phase of GI-1b could be climatically driven, and so c) despite (a) and (b), there was a peak in biomass burning at the beginning of the YD/GS-1 that could only be generated by an impact.

Given the abundance of charcoal in late Quaternary stratigraphic records globally, a simple occurrence of a fire "near" the YDB says nothing about the origin of the fire. Fires are claimed to be a direct, unambiguous result of a YDB impact, but fires are commonly recorded in lake and other sedimentological records and don't require an impact origin. Sweatman (2021, p 14) offers a convoluted argument on this point. **"Finally, Holliday et al…. argue that the abundance of charcoal… near the onset of YD cooling is not special, as other similar abundances occur at other times, including at the onset of significant climate warming events such as the end of the Younger Dryas period. But this is an unfair complaint because the rate of biomass burning on Earth during these periods is so high. Indeed, the annual rate of biomass burning at these times is around 2% per year** [no data are offered to support this assertion]**. Considering the uncertainty in radiocarbon dates is of the order of a few hundred years, and that an impact event cannot burn more than 100% of Earth's biomass, it will obviously not be possible to distinguish burning events on this basis."** That is exactly the point. How can any fire or fires be attributed to an impact when fires appear to be common through the final millennia of the Pleistocene and into the early Holocene? He further notes (p 14) **"this is precisely why ammonium ion records in ice cores are valuable. They are sampled with much higher resolution than is possible with radiocarbon dating. And it is clear from Figs. 2 to 4 of Fischer et al. (2015) that the burning event at the onset of YD cooling is special."** But, as we have seen, what Sweatman (2021) is interpreting in Fischer et al.'s (2015) figs. 2–4 (presumably panels b and c) is evidently the "background" emissions of NH_4^+ from soils, and not the fire peaks (panel a) or FPF (panel f). In any case, as can be confirmed by a reanalysis of the data in Fischer et al.'s supplementary information (or close inspection of the figures), the first NH_4^+-documented fire peak occurs 30 yrs after the onset of the YD/GS-1.

10. Purported YDIH evidence of impact: spherules/microspherules

Spherules and microspherules feature prominently in the YDIH. Determination of their origin is experimentally challenging, prone to misinterpretation, frequently debated, and can be a confusing topic. Impact proponents claim many different types, but they often talk about them collectively with sweeping statements. The following discussion is a clarification of what microspherules are and their utility as impact indicators.

YDIH proponents claim that various spherules/microspherules are indicators of comic impact or subsequent impact-generated wildfire (e.g., Firestone et al., 2007; Firestone et al., 2010a; Kennett et al., 2009a; Bunch et al., 2012; Israde-Alcántara et al., 2012, 2018; Wittke et al., 2013a; West et al., 2020a) and they describe different types at the YDB: carbon (see Sections 9 and 12.4), copal, magnetic (i.e., Fe-rich), Si-rich, Al-rich, Al-Si-rich, and Cr-rich. Impact proponents often use the term spherule or microspherule (e.g., written alone and not with adjectives such as carbon, magnetic, etc.) to discuss these different materials collectively, with the implicit presumption they are physically similar and of common origin. However, they exhibit compositional, mineralogical, as well as microstructural differences and, as such, should be presumed of different origins unless proven otherwise. For example, Wittke et al. (2013a, p E2091) wrote, **"Nearly all of the largest YDB spherules (maximum: 5.5 mm) are vesicular, consistent with outgassing at high temperatures, followed by rapid cooling that preserved the gas bubbles, and in some samples formed quench crystals within the bubbles. The prevalence of vesicles decreases with** [smaller] **spherule diameter, and most small spherules <50 μm in diameter are solid. All Fe-rich spherules and some Al-rich ones display dendritic crystals on their surfaces, consistent with high-temperature melting and quenching** [Bunch et al., 2012]**. Most Al-Si–rich spherules are smooth, but sometimes display flow marks, or schlieren, along with melted SiO_2 (lechatelierite) inclusions, both indicative of high-temperature melting at >2,200 °C** [Bunch et al., 2012]**."**

Even though small (mm to cm-size), often glassy impact melt bodies ("spherules") may be ejected from the cratering site during impact, and form (often geographically extended) ejecta deposits, their existence as melt particles is not as such diagnostic for impact, and their identification as impact products depend on association with other, confirmed impact-produced features, such as shock effects. Tektites and microtektites are the best-known and most-studied of these ejecta deposits (e.g., Koeberl, 1994), but a variety of other glass-rich ejecta deposits have also been noted. The identification of such glasses as impact or non-impact products is difficult and commonly controversial as discussed in the extensive review by French and Koeberl (2010).

Some studies, in which such spherule layers have been carefully examined by geological, petrographic, and geochemical techniques, provide strong evidence that they formed by meteorite impact events. In most cases, however, the confirmation of the impact origin did not come from the spherules themselves, but from associated minerals or geochemical anomalies. **"Microspherules are not, by themselves, diagnostic indicators of impact events, because similar objects can be produced by a wide range of geological and artificial processes… Identification of microspherule-bearing layers as impact ejecta needs additional evidence: geological context, association with genuine quartz PDFs** [planar deformation features]**, high-pressure minerals, or definitely extraterrestrial siderophile-element anomalies"** (French and Koeberl, 2010, p 151–152). Only evidence based on highly siderophile elements and/or isotope ratios, such as Os or Cr isotopes, can provide unambiguous evidence of the presence of a meteoritic component in spherules. For example, the ratio of Ni/Fe provides no evidence for a potential meteoritic component.

As summarized by French and Koeberl (2010, p 145–147), **"There are several major problems in attempting to use spherules as independent evidence for meteorite impact events… Spherules alone do not provide diagnostic evidence of origin by impact. Like other impact melts, droplet spherules generally preserve no evidence of shock processes or of their original ultrahigh-temperature origin…. In many distal ejecta layers, spherules are not accompanied by other materials that show distinctive and unambiguous shock-metamorphic effects. Exceptions include the occurrence of coesite and shocked quartz grains with microtektites… An especially severe problem in using spherules as a unique impact criterion is that the spheroidal shape by itself is not a unique indicator of impact or even of melting. A wide variety of nonimpact spherical, spheroidal, or droplet-shaped bodies, both natural and artificial, are abundant in the geological environment. Such features can easily be (and frequently have been)** [mis]**interpreted as impact-produced objects… Natural glassy spheroidal objects in the same size range as impact-produced spherules include volcanic droplets and lapilli… and meteorite ablation debris… In addition, natural nonmelting processes can produce a wide variety of similar spheroidal objects. Low-temperature sedimentary and diagenetic processes can produce spheroidal oolites, fecal pellets, spherulites, fossils, algal structures, and other organic and inorganic constructions… Other spheroidal objects in sediments can include organic pollen and plant spores… siliceous plant phytoliths…, and objects produced by the alteration of hydrocarbon deposits."** In addition, a dismayingly large variety of artificial spherules (cf. Marini, 2003), produced intentionally or accidently by various melting and manufacturing processes, even containing the high-temperature silica melt glass lechatelierite (Marini and Raukas, 2009), are being increasingly recognized as contaminants in geological samples and laboratories (French and Koeberl, 2010).

The paper by Firestone et al. (2007) was the first widely published study to claim magnetic and carbon microspherules as evidence for a YDB impact. However, the critically important question on the origins of the microspherules was not answered in the Firestone et al. (2007) study. Firestone et al.'s (2007) elemental measurements of magnetic spherules were indeterminate (p 16019) stating, **"composition of YDB magnetic microspherules and magnetic grains… cannot be explained at this time"** and potential shock effects in associated minerals were not investigated. Firestone et al. (2007, p 16019) wildly speculated they **"most likely resulted from influx of ejecta from an *unidentified, unusually Ti-rich, terrestrial source region* and/or from a *new and unknown type of impactor*"** (emphasis added). The latter improbable claim can be made of nearly any mineral specimen found on the Earth's surface. As for the carbon spherules, Firestone et al. (2007) speculated they were products of impact-generated wildfires based on finding them in wildfire-impacted forests (see Section 9.3). However, no systematic control study was performed on forests not impacted by wildfire, for if they had, they certainly would have found sclerotia (see Section 12.4). Curiously and equally revealing, no YDIH impact proponent that has studied the YDB (including those that investigated "carbon spherules") has made any mention of also finding sclerotia in over 50 examined YDB sites (e.g., see Tables 3 and 4), despite the fact sclerotia are ubiquitous in soils and sediments (see Section 12.4).

The only purported characteristic of the YDIH microspherules that could potentially suggest an impact was Firestone et al.'s (2006, 2007) claim that their concentration spiked at the YDB. Surovell et al. (2009) was the first attempt to reproduce that claim by examining two of Firestone et al.'s (2007) sites and five additional sites. The test failed. Among sites studied by Firestone et al. (2007), Surovell et al. found no spherules at Topper. At Blackwater Draw they found them only above the YDB and at concentrations an order of magnitude less than Firestone et al. purported at the YDB. LeCompte et al. (2012, p E2964-E2967) responded with five perceived issues regarding the methodology used by Surovell et al. (2009). Sweatman (2021, p 6) summarized, **"They concluded that there were significant deficiencies in the analytical methods used by Surovell et al. (2009)."** Surovell et al. (2009) outline their protocols and Surovell (2014) carefully responded to the critique. Sweatman (2021) and others ignored Surovell's comments and failed to consider the fundamental issues. Surovell et al. (2009) designed their study to follow the protocols as described by Firestone et al. *(*2007, SI) in an attempt to reproduce their results, a fundamental practice in scientific research. Improving and optimizing the methodology of Firestone et al. (2007) was not their goal. It was to see if the controversial results could be independently reproduced. However, the criticisms by LeCompte et al. (2012) grossly mischaracterize and revise the protocols described by Firestone et al. (2007). More problematic, following the publication of Surovell et al. *(*2009), A. West dramatically revised his protocols for collection of spherules. Other researchers also note changing criteria for collection of carbon spherules (Hardiman et al., 2012) (Section 12.4), another problem in YDIH research not addressed by YDIH proponents. ENDNOTE **14.**

While visual-based quantification of the spherule abundance is subject to selection bias of the investigators, the magnetic grains isolated from the sediments (from which the spherules were selected for counting) are not subject to same selection bias. The concentration profiles of the magnetic grains at Blackwater Draw and Topper (and at five additional sites) measured by Surovell et al. (2009) showed no peak at the YDB but rather occurred throughout the layers investigated, contrary to results of Firestone et al. (2007).

Powell (2020, 2022) likewise devotes considerable space to a critique of Surovell et al. (2009). In his book Powell (2020, p 146) writes, **"LeCompte et al. sampled** [Topper and Paw Paw Cove] **the very YDB sites where Surovell et al. could find not a single microspherule and found them in abundance. No scientist who convincingly located the YDB and used SEM and XRS has failed to find ET microspherules"**, although LeCompte et al. (2012) interpreted the spherules as likely to be terrestrial. Powell (2022, p 12) states, "[a]**t Topper … Surovell et al. found no microspherules at all"** and **"Surovell et al. failed to sample the YDB and/or erred in their procedures. When dealing with objects on the scale of tens of microns, avoiding such errors requires punctilious care."** Powell (2022, p 14) concludes, **"The simplest explanation is again that Firestone et al. sampled the YDB at Topper while Surovell et al. did**

not." The condescending argument about procedural errors is an after-the-fact explanation of inconvenient data. The Clovis and post-Clovis levels at Topper are mixed (Miller, 2010; see also Section 5.7), a point Powell (2022) may be unaware of, and this mixed stratigraphy could well explain the discrepancy in results.

Regardless of methodological details, critics fail to note that Surovell et al. (2009) did in fact recover both microspherules and magnetic grains (confirmed by A. West; Table 1), claimed to be two of the most reliable markers from the Firestone et al. *(*2007) study. The key issue is that they were unable to recover spherules in YDB zones with purported dramatic spherule spikes at sites studied by Firestone et al. (2007) (Blackwater Draw and Topper). At additional sites not studied by Firestone et al. (2007), Surovell et al. (2009) recovered no spherules from two sites (Paw Paw Cove, MD and Shawnee-Minisink, PA), but at three other sites (Agate Basin, WY, San Jon, NM, and Lubbock Lake, TX) spherules were recovered outside the YDB at abundances similar to those at the YDB. Lubbock Lake was selected for further study (Holliday et al., 2016, see also Section 12.6) by Surovell et al. and J. Kennett (a leading YDIH proponent). They analyzed splits of the same samples collected continuously across the YDB. The difficult question on origin of the microspherules was not addressed; instead the study focused on quantification within the sediments. They recovered similar levels of microspherules from samples spanning ~13 to ~11.5 cal ka BP (<0.4 g/kg), indicating that the methods used by Surovell et al. were adequate. More significantly, Kennett's analyses recovered very high amounts (roughly an order of magnitude higher) in a layer dated <11.5 cal ka BP (Holliday et al., 2014), but Surovell et al. (2009) recovered none. Powell (2022) repeatedly raises issues of reproducibility but ignores this study. The methods used to recover spherules by Surovell et al. (2009), following Firestone et al. (2007), works but is unable to reproduce the concentration profiles purported by Firestone et al. (2007). In another study at Blackwater Draw, Andronikov et al. (2016b) had mixed results. The concentration profile of magnetic grains was consistent with Surovell et al. (2009) and showed no peak at the YDB, while the profile of the spherules showed a peak at the YDB consistent with Firestone et al. (2007). ENDNOTE **15.**

Other problems in the identification and dating of microspherules abound (see Tables 3 and 4). For example, Wu et al. (2013) **"analysed microspherules from a range of YDB sites in North America and Belgium**" and claim only the Melrose, PA and Newtonville, NJ sites suggest an impact and **"the impact took place near the southern margin of the Laurentide Ice Sheet"** (p 3565). Similar to the first reports of discovery of the Hiawatha Crater (Section 8.1), YDIH proponents greeted the news about these microspherules as **"unequivocal evidence for an impact"** (Jones, 2013). Compositional and Os isotopic measurements of the microspherules yielded no evidence of a meteoritic component, however, and their possible association with shocked materials was apparently not investigated. Wu et al.'s (2013) identification of the microspherules as impact products was assumed from textures and presumed upper-bound melt temperatures. Since there is no age control at Melrose and Newtonville (see Table 4), the presumed impact indicators were used by Wu et al. (2013) as stratigraphic markers to identify the YDB, again representing circular reasoning. Further, the purported but undated YDB zone at Newtonville produced about the same number of spherules as the deeper and dated "Late Wisconsin" layer (Table 4). No objective evidence is presented to support a YDB age for any part of either the Melrose or Newtonville sites (Holliday et al., 2014; Meltzer et al., 2014). **"Thus, the conclusions of Wu et al. (2013) are drawn from two undated sections correlated to an unknown crater"** (Holliday et al., 2014, p 518).

Israde-Alcántara et al. (2018) report on YD-age black mats from five sites in northern Mexico (Table 4). Four of the sites yielded microspherules and other claimed impact indicators, three of those sites yielded microspherules dating younger than the YDB (Table 4). Only four sites (including the one with no microspherules) had black mats and they produced YDC ages with only one of them (Tocuila) that may date to the YDB. Israde-Alcántara et al. (2018) argue the microspherules formed by an impact based on their textures and claim their impact origin **"is confirmed by comparing the geochemical composition of the microspheres to those from known impact events, as discussed in previous studies, including Bunch et al. (2012), Wittke et al., [(2013a)], and references therein"** (p 76). However, Israde-Alcántara et al. (2018) did not detect a meteoritic component in the spherules, and they apparently did not search for associated shock features. This is also the case for Bunch et al. (2012), Wittke et al. (2013a), Andronikov et al. (2016b), Hagstrum et al. (2017), LeCompte et al. (2018), Kletetschka et al. (2018) and Pino et al. (2019) yet they concluded an impact origin based on several or all of the following factors: presumed melting temperatures; the similarity of the internal and surface morphologies as well as elemental composition of YDB spherules to those from known impact sites. However, Niyogi et al. (2011) found that the **"shape, size, surface features and chemistry of spherules are not diagnostic of impact cratering process and cannot distinguish microtektites and impact spherules from the coal fly-ash spherules produced from natural wildfires and thermal power plants."** As Jaret and Harris (2021, p 1) point out, **"presence of only non-diagnostic features – even if these same features sometimes occur in shock materials – is not sufficient to claim impact."** While some YDB microspherules were purported to contain lechatelierite (Bunch et al., 2012; Wittke et al., 2013a; Wu et al., 2013; LeCompte et al., 2018), 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. At the Blackwater Draw site, Andronikov et al. (2016b) failed to detect a meteoritic component in microspherules where the **"overall low *platinum group elements (PGEs)* concentration in the microspherules… slightly above detection limit"** (emphasis in citation) precluded any interpretation. They also did not investigate shock effects and presumed an impact origin based only on non-diagnostic features.

Impact proponents invoke the presence of any kind of spherules as definite proof of impact events. In many of these studies, the characterization of the alleged spherules is superficial and/or incomplete, and their use to support the existence of impact events is just speculation (see Detre and Toth, 1998, for a large collection of studies of varying quality, on natural and artificial spherules). A theme running through the publications using microspherules as evidence of a YDB impact (e.g., Firestone et al., 2007; Bunch et al., 2012; LeCompte et al., 2012, 2018; Wittke et al., 2013a; Wu et al., 2013; Israde-Alcántara et al., 2012, 2018; Andronikov et al., 2016b; Hagstrum et al., 2017; Kletetschka et al., 2018; Pino et al., 2019; Sweatman, 2021; Powell, 2020, 2022) is failure to address the accepted criteria for the identification of impact markers (French and Short, 1968; Stöffler, 1971; Grieve et al., 1996; Langenhorst, 2002; French and Koeberl, 2010; Ferrière and Osinski, 2013; Stöffler et al., 2018). Proponents of the YDIH tend to focus on the shape, textures, and/or presumed melting temperatures of spherules as well as their presence in presumed YDB sediments, but none of those are diagnostic of an impact origin.

Most YDIH proponents propose an airburst event to explain the lack of a YDB-aged crater. This is despite the fact that YDB microspherules (used as evidence of an ET event) lack a meteoritic component as expected for bolide debris. Even assuming that microspherules are from an airburst and evidence of a meteoritic component was missed there is no reason to associate them with a large, global event. There is a non-negligible probability that sampling a random 360-year core in a random place on Earth will turn up condensed debris from a more frequent, small nearby bolide. The "YDB", when defined recursively this way, will always be found in core samples of sediments with approximately the right age.

There are many small airbursts every year that contribute to the meteoric debris that constantly falls onto Earth as part of disintegration of small meterorites during atmostpheric passage. Larger airbursts (e.g., Chelyabinsk) take place on time scales measured in decades or centuries. Their contribution to the accretion of meteoritic material on Earth depends on the mass of the object. The debris fields from these events are of limited geographic extent and thus just contribute minor amounts to the "background noise" of meteoritic material accreting onto Earth, so apparent concentrations cannot be correlated and assumed to be a single event in the geologic record.The analogy would be to see evidence of a burning event in two tree cores from widely separated forests. It would be foolish to claim they were from the same fire, especially if they were not dendrochronologically correlated. Even if they occurred at the same time, it is far more likely that the trees were burned in different small fires rather than one continent-wide conflagration. A layer of meteoritic material that is within a few hundred years of the YDB, cannot simply be assumed to be a YDB layer. Small airbursts happen all the time, everywhere. YDIH papers clearly do not have independent radiocarbon ages with low enough uncertainty to show that their "YDB" layer is coeval or correlates with the actual start of the YDC (Meltzer et al., 2014) (Section 5; Table 4). ENDNOTE **16.**

As described above, determining if any spherules formed synchronously is not possile due to the problematic dating of the presumed YDB sediments at the various sites. That is with the exception of one spherule type that can be directly dated, the carbon spherules. While they are not impact indicators,YDIH proponents repeatedly claim they are tree sap thermally altered by the impact fireball (Sections 5.8 and 12.4), and as such not subject to "old wood" radiocarbon dating artifacts. To support the YDIH they must date synchronously. Sweatman (2021) ignored data from carbon spherules generated by both YDIH proponents and critics that provides one of the most compelling pieces of evidence against the YDIH. Purported YDB samples from the Gainey site (Table 4) prepared by the YDIH proponents contained carbon spherules of very recent age based on radiocarbon dating by Boslough et al. (2012). This demonstrates again that the purported YDB contains carbon spherules (and other materials) that do not date to that period. Because of this, and as explained in Section 5.8, the published carbon spherule concentration profiles cannot be used to test the YDIH. LeCompte et al. (2013) maintain these originated from a YDB impact and point to a non-peer-reviewed paper by Firestone (2009a) that also reported anomalously young ages (~135 years in the future!) and listed physically impossible hypotheses including **" hydrogen in the comet might undergo a D+D fusion process on impact producing neutrons that would make ^{14}C in the atmosphere"** or **"the impacting object was ejected by a recent near-Earth supernova in which case carbon is expected to be enriched in ^{14}C**…" Neither of these hypotheses is viable. With regard to the latter, Firestone (2014, p 5) wrote, **"no mechanisms for such an event has yet to be established."** The best explanation is that the purported YDB carbon spherules are not all YDB age and none have association with an impact. Their origin is discussed in Section 9.3 and in further detail in Section 12.4.

11. Purported YDIH evidence of impact: platinum group elements

Following the study of Greenland ice cores by Petaev et al. (2013a), YDIH proponents focused on platinum (Pt) and added platinum group elements (PGEs) as YDB impact indicators (Andronikov et al., 2016a; Mahaney et al., 2017; Moore et al., 2017, 2019, 2020; Wolbach et al., 2018b; Pino et al., 2019; Thackeray et al., 2019; Teller et al., 2020). However, at least two principal aspects of the Pt anomaly in Greenland ice were misinterpreted by the YDIH proponents. The first mistake is using the Pt spike alone as an indicator of ET matter. As stressed by Jaret and Harris (2021, p 2), **"To convincingly show evidence of meteoritic components, the full suite of PGEs should match known meteorite groups."** Petaev et al. (2013a) explicitly stated that while the Pt spike in ice requires an injection of Pt-rich matter, it does not identify the nature of that matter because both ET and crustal Pt-rich materials exist, but they have quite different Pt/Ir and Pt/Al ratios. It is the extremely high Pt/Ir ratio at the Pt anomaly in the Greenland ice that **"rules out mantle or chondritic sources of the Pt anomaly (Fig. 2). A further discrimination between Pt-rich crustal materials like Sudbury Footwall ore… and fractionated extraterrestrial sources such as Ir-poor iron meteorites… is difficult because of the comparable magnitude of the Pt/Ir fractionation in these materials. Circumstantial evidence hints at an extraterrestrial source of Pt, such as very high, superchondritic Pt/Al ratios at the Pt anomaly and its timing, which is clearly different from other major events recorded in the GISP2 ice core, including well-understood sulfate spikes caused by volcanic activity and the ammonium and nitrate spikes associated with biomass destruction"** (Petaev et al., 2013a, p 12918). The later investigation of YDB sediments at Hall's Cave and Friedkin sites by Sun et al. (2021, p 70) showed that highly siderophile element **"analysis including Os isotope measurement is needed to provide a clear picture of the source of the geochemical signatures as either being extraterrestrial or mantle-derived material."**

Another aspect is the magnitude and duration of the Pt anomaly with the maximum Pt concentration of 82.2 ppt and Pt/Ir ratio of 1265 in an ice layer of 12.5 cm in thickness precipitated over ~3.5-year period. As the injection event has likely lasted much less than 3.5 years, the peak concentrations of Pt and Ir at the anomaly could be even higher due to the dilution effect of Pt-free ice accumulated before and after the injection in the sample analyzed, but the Pt/Ir and Pt/Al ratios should remain the same. The situation is different for YDB sediments where a much longer accumulation time (hundreds of years) expected for a ~ 1 cm-thick layer of sediments. The longer accumulation time allows for any PGE spike in the sediment (corresponding to the ice core spike) to be diluted and the Pt/Ir altered by minerals from various sources deposited in the sediments. For example, Os isotopes and PGE data of Sun et al. (2020, 2021) for sediments below, above and within the YDB layer from the Hall's Cave and the Debra L. Friedkin site do show several Pt spikes, with one sample (BMC16_11.D – Sun et al., 2021) having very high Pt/Ir ratio of 1937 and very low Pt/Lu ratio of ~0.0007 due to the dominance of terrestrial silicate matter in sediments. Based on the Pt/Ir ratio alone, the nature of this Pt-rich and Ir-poor material cannot be resolved. It is the dominance of silicate matter in sediments that rules out usage of the Pt anomaly alone or even with Ir as a proxy of ET matter in them. Sweatman (2021, p 2) describes PGEs (**"especially platinum itself"**) as "**the most robust impact proxies"** but clearly this is not the case here unless a comprehensive analysis of PGEs and siderophile elements is performed. For example, a volcanic source of PGE anomalies at Hall's Cave and the Friedkin site was deduced based on a wider examination of ^{187}Os/^{188}Os isotopic ratios as well as abundances of Os, Ir, Ru, Pt, Pd, and Re (Sun et al., 2020, 2021).

Sweatman (2021, p 8) falsely claimed that Holliday et al. (2014) stated that in the Greenland ice **"the platinum anomaly is around 30 years too late."** No such statement was made by Holliday et al. (2014). Perhaps Sweatman misread or misunderstood the Holliday et al. (2014) reiteration of the Petaev et al. (2013a) finding that the Pt anomaly **"precedes an ammonium and nitrate spike in the core by ~30 years."** In this context, "precedes" would imply "too early." However, the Pt anomaly appears in the ice core about 1 m above the YDB, which corresponds to about 20 years (Section 5.1). The ammonium and nitrate spike appears in the ice core even higher corresponding to about 50 years later than the YDB. This suggests three independent events with three different causes.

Sweatman (2021, p 20) also falsely claims that Holliday et al. (2014) misrepresented Petaev et al.'s (2013b) conclusions. Holliday et al. (2014, p 522) states, **"In response to Boslough (2013)** [commentary of Petaev et al., 2013a]**, Petaev et al. (2013b) accept arguments against the Pt- depositing event being the cause of the YD cooling."** The first sentence of Petaev et al. (2013b) is, **"Besides providing additional arguments against the Pt depositing event as a cause of the Younger Dryas cooling, Boslough's letter raises an important question about the scale of this event."**

Sweatman (2021) and Powell (2022) highlight the work of Moore et al. (2017). Sweatman (2021, p 5) states that those investigators **"reported the discovery of a widespread platinum anomaly at the base of the YD black mat in several locations in North America."** As discussed (Section 5.4), the timing and context of Pt deposition is far from clear. The work reported by Moore et al. (2017), like most other YDIH studies, does not meet the dating criteria set out by Kennett et al. (2008a) (Table 4). The Pt zone is assumed to be the YDB because it has Pt (circular reasoning) and is stratigraphically about where the YDB should be. The accurate and precise dating required to identify the YDB does not allow for dates that are "about" right, however. Further, out of 11 sites with Pt reported by Moore et al. (2017), only four have black mats (and Arlington Canyon has multiple zones dating to the YDB over a section 5 m thick) (Table 4).

At the archaeological site of Abu Hureyra, Sweatman (2021, p 5) note that Moore et al. (2020) **"analysed debris from the burned layer… at Abu Hureyra, Syria, dated to 12,825 ± 55 cal BP, finding elevated platinum at 6.2 ppb."** There are other (younger) "burned layers" at Abu Hureyra (Section 4.2) and other sites in the region. Were they sampled, too? The Usselo soil (Section 5.6) in northwest Europe was sampled for Pt. Sweatman (2021, p 5) refers to **"elevated levels of platinum, other PGEs and REEs (rare-Earth elements)"** citing the work of Andronikov et al. (2016a) but fails to mention that none of the samples in that study are directly linked to YDB dating, and some of the Pt spikes are above or below the soil. Further, the Pt spike reported in Andronikov et al. (2016a) is not remarkable and that study concluded that their evidence for an ET impact is equivocal (Table 4).

Finally, the link between elevated Pt and the beginning of the YD/GS-1 is far from clear, contrary to statements by Moore et al. (2017), Sweatman (2021) and Powell (2022). Cheng et al. (2020) (published over three months before Sweatman's paper) present **"speleothem oxygen-isotope data that, in concert with other proxy records, allow us to quantify the timing of the YD onset and termination at an unprecedented subcentennial temporal precision"** (abstract). Their work includes identification of the YDB and examination of the Pt record in the Greenland Ice Sheet. Their observations and conclusions on this particular issue (p 23414) is worth quoting in full because they are directly germane to both the onset of the YD/GS-1 and the significance of Pt in the YDIH debate:

> **"an ~20-y-long Pt-anomaly** [highlighted by Sweatman, 2021, p 4, 5, 14, 17] **was identified in the Greenland Ice Sheet Project (GISP2) ice core…, which was attributed to injections of Pt-rich dust from the** [YDB impact] **event and subsequent deposition at a depth of 1,712.375 to 1,712.000 m, or at ~12,820 B.P., based on synchronization to the GICC05 chronology…. A closer look, however, found that the immediate hydroclimatic impact, if any, was likely minor as inferred from GISP2 δ^{18}O record (corresponding to a <1‰ drop…). In the same ice core, the Pt-anomaly occurred at the middle of a gradual increase in Ca^{2+} (dust proxy) from ~1,714.00 to 1,709.90 m (~12,870 to 12,765 B.P. on GICC05 chronology) without disrupting the course… Provided that the GISP2 and NGRIP records were synchronized precisely…, the Pt-anomaly did not disrupt NGRIP and AM** [Asian Monsoon] **δ^{18}O records either…** ***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. Moreover, the YD as a millennial-scale perturbation during the last deglaciation has a previous analog: a YD-like event occurred at ~245,000 B.P. during glacial termination-III (the third to the last deglaciation)… Based on this paleoanalog and the preponderance of geochronological data, we contend that the YD Impact Hypothesis remains untenable and offers a less parsimonious explanation for the global timing and structure of the YD event, and the data presented here provide a precise timing framework for further research in the area*** [emphasis added]**."**

12. Purported YDIH evidence of impact: Nanodiamonds

Impact proponents enthusiastically describe nanodiamonds at the YDB and claim that they are impact markers apparently basing this on their presence in Cretaceous-Tertiary (KT) boundary sediments (e.g., Kennett et al., 2009a, 2009b; Kurbatov et al., 2010; Israde-Alcántara et al., 2012; Bement et al., 2014; Kinzie et al., 2014) and their dubious synthesis experiments (Kimbel et al., 2008). The presence of nanometer-sized diamond of the cubic 3C polytype in sediments is not necessarily an indicator of an impact. Diamond is chemically inert, highly resistant to weathering (e.g., decomposition and transformation) and will persist in the surface environment. Erosion of diamond-bearing source rocks and transportation by wind or water could widely redistribute nanometer- to submicron-sized diamonds into distant alluvial deposits and sediments that bear little resemblance to the diamond source rocks (Section 12.1). A similar case could be made for micron-sized host grains containing nanodiamond inclusions, and those inclusions would be released when the host grains weather. Also, those inclusions would be extracted from their host minerals during laboratory acid dissolution as has been applied to study YDB sediments. The use of the rare hexagonal 2H polytype of diamond as an impact marker can be questioned as well, but for different reasons (Section 12.2).

Impact proponents further claim that at multiple sites across the Northern Hemisphere, there is a peak in nanodiamond concentration at the YDB usually in the hundreds of ppb range (Kinzie et al., 2014), but upwards of 190 ppm (e.g., Bement et al., 2014), with the absence (or near absence) of nanodiamonds immediately above and below that horizon. Such a spike in the concentration of nanodiamonds at the YDB would represent a strong indicator that a highly unusual event occurred at that time horizon. However, the measurement of ppm/ppb concentrations of nanodiamonds in sediments is technically very challenging, and the methods used by the impact proponents have numerous problems with both identification and quantification that render their approach impractical (Section 12.6). Consequently, published nanodiamond concentrations from purported YDB sediments are completely unreliable and scientifically meaningless.

12.1. Cubic nanodiamonds

In arguing that nanodiamonds are impact markers, Sweatman (2021, p 8) claimed, **"Almost all terrestrially formed diamond is microscopic or larger, >1 µm, and of cubic form. Naturally formed terrestrial nanodiamonds, 2 nm to 100 nm, are extremely rare…,"** which echoed earlier unsupported claims of LeCompte et al. (2018, p 165) (see Table 2). The distribution of nanodiamonds in terrestrial sediments and rocks remains largely unknown due to the severe experimental challenges that have limited their study (e.g., Daulton et al., 2017a, 2017b). Because of this, the rarity/abundance of nanometer-sized diamond relative to the total terrestrial diamond population across their entire size range is unknown. Nevertheless, recent studies have begun to examine natural terrestrial diamond of nanometer to submicron-size, and various formation conditions/mechanisms (including those exclusive of shock transformation) have been proposed based on their petrological context (e.g., see Simakov et al., 2015; Farré-de-Pablo et al., 2018; Pujol-Solà et al., 2020 and references within Daulton et al., 2017a). However, the literature is complicated by the varying strength of the published data due to the difficulty in micro/nanoanalysis of nanodiamonds.

Sweatman (2021) cites only several select studies and only those he claims support the YDIH. Nanometer- to submicron-sized diamonds were reported within 0.1–1.5 mm-sized carbonaceous particles, similar in description to carbon spherules reported in YDB sediments, but from modern forest soils in Germany and Belgium (Yang et al., 2008). Sweatman (2021, p 9) wrote, "**Yang et al. (2008) suggest these nanodiamonds are likely to have been produced by another cosmic impact or detonation of explosives during modern wars.**" The origin of these nanodiamonds remains undetermined, however. Sweatman (2021, p 9) also wrote that Tian et al. (2011) **"confirmed the existence of abundant cubic nanodiamonds at the Ussello horizon, often thought to be the continuation of the YDB…".** These claims were repeated from his book where he wrote (Sweatman, 2019, p 102), **"nanodiamond abundance peak was also confirmed… in the Ussello layer"** and (p 155) **"the Younger Dryas black mat is found in Belgium, where it is called the Ussello Horizon."**

The Usselo soil suffers the same confused interpretation by YDIH proponents as does the black mat, however, being perceived to be the impact debris layer (i.e., the YDB) or not (see Sections 5.6 and 6). For example, Firestone et al. (2010a, p 40) wrote, **"magnetic grains and spherules, charcoal, iridium, and rare earth elements peak *beneath the Usselo layer, the European analog to the black mat*** [emphasis added]**."** In subsequent papers coauthored by Firestone inconsistencies and contractions regarding the Usselo soil abound (Kennett et al., 2015a; Wolbach et al., 2018b, 2020). Kennett et al. (2015a, p E4351) wrote, **"The charcoal-rich *YDB layer occurs at the top of the Usselo horizon* … and contains peaks in impact-related spherules, carbon spherules, and nanodiamonds** [emphasis added]**."** Wolbach et al. (2018b, SI, caption fig. A6) wrote, **"Charcoal-rich black layers…, or 'black mats,' lie at the boundary between the underlying Usselo Formation and overlying sandy sediment."** Wolbach et al. (2020, p 99) wrote, **"impact material fell on and mixed into the top of the Usselo** [horizon]***, which existed before the impact event*** [emphasis added]**."** Regardless of this confused interpretation, Usselo soils are not uniquely linked to the YDB, and their formation neither began nor ended at the YDB. Rather, they formed before and during the YD/GS-1 (Section 5.6). This fact may contribute to the YDIH proponents confused interpretation. Furthermore, the main conclusion of Tian et al. (2011) is prominently emphasized in the title of their paper **"Nanodiamonds do not provide unique evidence for a Younger Dryas impact."** Tian et al. (2011, p 44) wrote that this conclusion was reached because, **"… the present variety of crystalline structures observed in the black** [presumed] **Younger Dryas boundary in Lommel does not provide sufficient evidence to conclude an exogenic impact as the origin of these structures."** Furthermore, as pointed out by van Hoesel et al. (2012, p 7648), **"no age control was presented"** by Tian et al. (2011) to support the identification of the black layer in the Usselo soils as the YDB.

12.2. Hexagonal nanodiamonds (lonsdaleite)

The hexagonal (2H polytype of) diamond, lonsdaleite, was first discovered by laboratory shock synthesis and then subsequently found in shocked meteorites as well as within impact structures. This lead to the perception that its formation was exclusively associated with shock processes. However, the literature also contains reports of natural lonsdaleite with no direct connection to shock processes (see references within Daulton et al., 2017a). It is difficult to evaluate that literature, because some (but not all) published data identifying natural lonsdaleite is not rigorous or convincing, with identifications sometimes based on several diffuse X-ray lines or a few Transmission Electron Microscopy (TEM) electron diffraction patterns. In some studies (e.g., Koeberl et al., 1997; Masaitis et al., 1999; Titkov et al., 2001), no data are presented to support the lonsdaleite identification. Lonsdaleite is almost always reported intergrown with cubic 3C diamond, which complicates microanalysis, and literature reports often lack details of the nano/microstructure (e.g., stacking-domain size and volume/mass fraction of hexagonally and cubic stacked layers). Whether a specimen reported in a study is best described as an intergrowth of discrete cubic and hexagonal polytypes, a highly stacking disordered tetrahedral-diamond layer structure, or a microstructure in between (e.g., see Murri et al., 2019) can be indeterminate. Moreover, stacking faults in diamond, e.g., *{ABABC}*, can form several unit-cell thick lamellae with the 2H or 3C polytype structure. In nanocrystals, whether these thin lamellae constitute a phase a particular polytype or disorder can be a matter of definition, perhaps dependent on which stacking sequence is more prevalent. In fact, Németh et al. (2014) have gone as far to speculate that lonsdaleite does not exist as a discrete phase and is merely fine-scale stacking faults and twinning in cubic diamond, but this has been challenged with contrary evidence (e.g., see Kraus et al., 2016; Daulton et al., 2017a; Turneaure et al., 2017; Volz et al., 2020; Volz and Gupta, 2021; Tomkins et al., 2022). Nevertheless, speculation by some that lonsdaleite does not exist reflects the lack of sufficient nano/microcharacterization of lonsdaleite in the literature due to the experimental difficulty.

Therefore, in the literature on specimens obtained from sites either exhibiting or lacking shock indicators where details of the micro/nanostructure are lacking, presence of lonsdaleite is uncertain in the specimens studied. Consequently, it is also uncertain if lonsdaleite in nature is exclusively (or predominantly) associated with impact structures and, in turn, the extent and circumstances under which it can be used as an impact marker are uncertain. A recent study of ureilite meteorites further reinforce the questions of if and when lonsdaleite can be used an impact marker. In that study, Tomkins et al. (2022, abstract) proposed lonsdaleite formation **"by psuedomorphic replacement of primary graphite, facilitated by supercritical C-H-O-S fluid during rapid decompression and cooling"**, and described the **"process is akin to industrial chemical vapor deposition but operates at higher pressure."** They wrote (p 6), **"Shock-induced conversion of graphite to lonsdaleite or diamond produces a large volume decrease reflecting their respective densities (graphite = 2.26 g/cm^3, lonsdaleite and diamond = 3.52 g/cm^3), so the observed volume increase requires addition of carbon, such as by fluid-mediated pseudomorphism"** and **"In the meteorites examined here, polycrystalline lonsdaleite tends to occur in fully annealed ureilites (NWA 5996, NWA 7983), or in domains of annealing associated with smelting (NWA 2705, NWA 11755), which formed after the primary shock event."**

Whether or not lonsdaleite can be used as in impact marker is a moot point, however, given that there is no viable evidence of the presence of lonsdaleite in YDB sediments (Daulton et al., 2010, 2017a, 2017b; van Hoesel et al., 2012; van Hoesel, 2014). The published YDB data is thus inconclusive for lonsdaleite, inconsistent with lonsdaleite, and/or a misidentification as lonsdaleite (Daulton et al., 2010, 2017a, 2017b).

In response to Daulton et al. (2017a) regarding the misidentification of lonsdaleite in YDB sediments, Sweatman (2021, fig. 8 caption) declared **"Daulton et al., [(2017a)] focus on the *single*** [emphasis added] **missing diffraction ring between the 100 and 110 rings…"**, and (p 11) **"the key issue identified by Daulton et al., [(2017a)] is that the diffraction patterns of Kennett et al. (2009b), Kurbatov et al. (2010) and Kinzie et al. (2014) appear to be missing diffraction rings at 0.15 nm expected for Lonsdaleite. By scaling these diffraction patterns by a factor of 1.054, Daulton et al., [(2017a)] claim a better match is obtained to an assembly of graphene/graphene** [/graphane] **layers. But this is a matter of judgment based on a rather fuzzy diffraction image (see Fig. 8)."** These statements are only marginally correct. Contrary to Sweatman (2021), Daulton et al. (2017a) did not rule out the lonsdaleite identification of Kurbatov et al. (2010) based on the lack of a (102) reflection at 0.15 nm. The only diffraction pattern supporting a lonsdaleite identification presented by Kurbatov et al. (2010) is their fig. 6. That figure contains structural data from two nanocrystals isolated from residues of ice sampled at a purported YDB-dated margin site east of Kangerlussuaq, West Greenland. In fig. 6c of Kurbatov et al. (2010), a diffraction pattern of the first nanocrystal is shown. As discussed later in this section, a single zone axis diffraction pattern from a nanocrystal is insufficient to base conclusive mineral identification. Thus, the identification of this nanocrystal is undetermined. The figs. 6b and d of Kurbatov et al. (2010) display a high-resolution (HR)-TEM lattice image of the second nanocrystal and presumably its fast Fourier transform (FFT). The lattice image and FFT are unquestionably inconsistent with the crystal structure of lonsdaleite. Daulton et al. (2017a, p 12) wrote, **"No crystallographic zone axis of lonsdaleite exists that can display two differently oriented sets of 2.06 Å -spaced {002} planes because there is only one such set of planes in the structure (Fig. 3)."** In other words, there is only one unique {00ℓ} direction in the hexagonal system. Thus, this nanocrystal cannot be lonsdaleite. Furthermore, there is reason to believe that the identification of nanodiamonds in Greenland ice could not be reproduced by the Kurbatov group (Section 13.4).

Sweatman's (2021, p 11) remark above about **"scaling diffraction patterns"** misleadingly implies the scaling was performed to achieve a better match of the pattern to graphene/graphane than lonsdaleite. Daulton et al. (2010, p 16044) clearly wrote in the caption of their fig. 3, **"Peaks measured from the doubled diffraction lines in Fig. S2B of Kennett et al., [(2009b)] are shown (we calibrated the reported {100} reflection to 2.189 Å, and the line widths represent the error in our measurement)."** The length scale of the diffraction pattern was calibrated by assuming the diffraction ring labeled {110} by Kennett et al. (2009b) was the (110) reflection of lonsdaleite at 1.260 Å (Table 9). This ring was selected because it had the strongest intensity of those rings that did not overlap with other rings. This calibration set the ring labeled {100} at 2.189 Å, very close to its predicted value (Table 9). Despite calibrating the pattern with the initial assumption that the diffraction lines were from lonsdaleite, the diffraction lines more closely matched that of graphene/graphane.

One point Sweatman (2021) correctly stated is that the polycrystalline diffraction pattern shown in fig. 8 of Sweatman (2021) (originally from Kennett et al., 2009b, and Fig. 4 of this review) does appear "fuzzy". However, it contains a wealth of structural information. Specifically, the diffraction pattern is azimuthally asymmetric with partial and double rings of variable width (i.e., "fuzzy"), which indicates heterogeneity in the form of texturing. Texturing (defined as a distribution of crystallographic orientations of polycrystalline grains, in which all possible orientations do not occur with equal probability) can produce asymmetric ring intensity. Sweatman (2021, fig. 8 caption) attributes the pattern to texturing of lonsdaleite by stating, **"The axial asymmetry in this diffraction pattern, highlighted by the ellipses can be explained by a non-uniform distribution of crystal grain orientations."** However, texturing of single-phase lonsdaleite can be ruled out because this diffraction pattern completely lacks intensity from many (not just one) lonsdaleite reflections including, but not limited to, the (101) at 0.193 nm, (102) at 0.150 nm, and (202) at 0.096 nm. Furthermore, the lonsdaleite (202) reflection is predicted to be similarly as intense as the (200) reflection (Table 9). Kennett et al. (2009b) indexes a diffraction ring as lonsdaleite (200) (Fig. 4 and original source). If the grain is lonsdaleite, and the (200) diffraction ring has sufficient intensity to be visible, so should the (202) diffraction ring have sufficient intensity to be visible. There is no hint of diffraction intensity from the lonsdaleite (202) (Fig. 4). The (203) lonsdaleite reflection is predicted stronger than the lonsdaleite (210) and Kennett et al. (2009b) identify a visible diffraction ring as lonsdaleite (210). If this identification is correct, the lonsdaleite (203) reflection should have sufficient intensity to be visible as well, but it is missing. However, Sweatman (2021) misleadingly implies Daulton et al. (2017a) claims only one reflection, the (102), is missing despite Daulton et al. (2017a, p 15) stating, **"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 further implausible that this is the case for every aggregate that was examined). On the other hand, the observed diffraction lines more closely match that of a mixture of graphene and graphane having an unremarkable texturing geometry.

Sweatman (2021, p 11) subsequently stated, **"one potential resolution of this data is that lonsdaleite-like crystals in question have a disordered sequence of AB and ABC layers."** A disordered diamond polytype stacking structure would have diffraction contributions from 2H diamond lamellae as well as 3C diamond lamellae. Thus, it would have more reflections than expected for 2H diamond, not less. Consequently, a disordered diamond stacking structure would be inconsistent with fig. 8 of Sweatman (2021) since that diffraction pattern is missing many 2H diamond reflections.

Kinzie et al. (2014, p 492) perplexingly conclude after discussing their measurement of the purported (100) lonsdaleite spacing of the same grain Daulton et al. (2010) demonstrated was missing 2H diamond reflections, **"Although the lonsdaleite-like crystals may be some other unknown carbon-based mineral, there is no current evidence that excludes the possibility that it is lonsdaleite."** However, this is because Kinzie et al. (2014) also ignore and fail to address the missing 2H diamond reflections. Further, we object to the term "lonsdaleite-like" first used by Kinzie et al. (2014) and subsequently used by Sweatman (2021) to replace the word "lonsdaleite" when describing certain materials in the purported YDB (see also Section 12.7); either the mineral phase is hexagonal 2H diamond or it is not. The term "lonsdaleite-like" by definition would encompass any material with similarities to lonsdaleite. To demonstrate the absurdity of the term "lonsdaleite-like," consider that electron diffraction and elemental composition are among the primary observables in electron microscopy. Thus, in the context by which the "lonsdaleite-like" is used by Kinzie et al. (2014) and Sweatman (2021), that of phase identification by electron microscopy, graphene/graphane aggregates can certainly be termed "lonsdaleite-like."

Independent studies (including those of impact proponents) have confirmed graphene/graphane aggregates that resemble lonsdaleite are present in YDB sediments (Madden et al., 2012; van Hoesel et al., 2012; Kinzie et al., 2014; Bement et al., 2014; van Hoesel, 2014) and these studies (with the exception of Kinzie et al., 2014) have also failed to observe lonsdaleite. Furthermore, if one accepts Sweatman's (2021) argument that the diffraction pattern of the purported lonsdaleite grain shown in Kennett et al. (2009b, SI figs. 2a-2c, S2b), Kinzie et al. (2014, fig. 15), and Sweatman (2021, fig. 8) is too "fuzzy" to definitively identify the specimen as a polycrystalline aggregate of graphene/graphane, then conversely it must be too "fuzzy" to definitively identify the specimen as lonsdaleite. This is an example of the self-inconsistent arguments frequently presented by impact proponents.

Sweatman (2021, p 11) also wrote, "**Moreover, Kinzie et al. (2014) provide further evidence of Lonsdaleite-like crystals** [in the form of single zone axis diffraction patterns, their fig. 18] **from two caves, Sheriden and Daisy, in North America, and this data is not contested by Daulton et al., [(2017a)]**." As discussed in Daulton et al.

(2010, 2017a), a single zone axis diffraction pattern (or high-resolution phase-contrast image) from a nanocrystal is insufficient to base conclusive mineral identification. In fact, Kinzie et al. (2014, p 485) wrote, **“By themselves, SAD** [selected area diffraction] **patterns are insufficient to identify NDs”** and this statement is quoted in Daulton et al. (2017a). Therefore, the Sheriden and Daisy data are inconclusive for the identification of lonsdaleite.

12.3. Controversial ‘n-diamond’ and ‘i-carbon’

One important point to clarify is that the majority of the reported YDB nanodiamonds is not diamond, but rather are a controversial, proposed modified form of diamond termed “n-diamond” (Kennett et al., 2009a, 2009b; Kurbatov et al., 2010; Israde-Alcántara et al., 2012; Kinzie et al., 2014; Bement et al., 2014) and another controversial, proposed form of carbon, termed “i-carbon” (Israde-Alcántara et al., 2012; Kinzie et al., 2014). While neither is a diamond polytype, and their existence, identification, as well as structure are debated, impact proponents describe them as nanodiamonds (Kennett et al., 2009a, 2009b; Kurbatov et al., 2010; Israde-Alcántara et al., 2012; Kinzie et al., 2014; Bement et al., 2014). In YDB sediments, n-diamonds are usually reported at significantly higher abundances than diamond (Israde-Alcántara et al., 2012; Bement et al., 2014; Kinzie et al., 2014), and occur at 22 out of 24 purported YDB sites (see table D2 of supplemental materials of Kinzie et al., 2014). In fact, at 14 of those 24 purported YDB sites, n-diamonds but not diamonds are reported. Following n-diamond, i-carbon is reported as the next most abundant. In all but two purported YDB sites where n-diamonds are reported, i-carbon is also reported. To emphasize our point, we will use ‘nanodiamond’ to refer to predominantly n-diamond and i-carbon along with minor amounts of diamond, if any.

12.4. ‘Nanodiamond’ host grains – carbon spherules

Millimeter-sized carbon spherules feature prominently in YDIH. Firestone et al. (2006, 2007) claimed that impact-induced wildfire products charcoal, soot, carbon spherules, and glass-like carbon containing ‘nanodiamonds’ were among the materials found at the YDB at a number of North American sites. Later, the carbon spherules and an additional purported wildfire product carbon elongates (see also Sections 5.8 and 9.3) were claimed to be host grains to ‘nanodiamonds’ and the ‘nanodiamonds’ were also purported in bulk sediments (Kennett et al., 2009a, 2009b). Note that Kinzie et al. (2014), with Kennett as a coauthor, reclassified carbon elongates as carbon spherules (see Section 12.7), so the following discussion applies to both morphological forms. In 16 of the purported YDB sites studied by Kinzie et al. (2014), carbon spherules were examined for ‘nanodiamonds’, and in most (13 out of 16) of those sites carbon spherules were reported to contain ‘nanodiamonds’. The predominant form of ‘nanodiamonds’ reported in the carbon spherules was n-diamond and i-carbon. In 10 of those 13 sites, n-diamond, but no diamond, is reported in the carbon spherules. In one of those ten sites (Sheriden Cave), Kinzie et al. (2014) claim recovery of lonsdaleite, but as discussed in Section 12.2, that identification is not supported. Any discussion of YDB ‘nanodiamonds’, particularly the purported n-diamond and i-carbon, must necessarily include a discussion of their host grains, the carbon spherules.

As to the origin of the carbon spherules, Firestone et al. (2006, p 343) speculated, **“…** [carbon] **spherules may be droplets of tree sap flash-cooked by the impact’s thermal pulse or by the intense heat of the shock wave.”** Kennett et al. (2009b, p 12626) wrote, **“… hexagonal diamonds and other nanometer sized diamond polymorphs also co-occur with high concentrations of charcoal and other forms of particulate carbon (carbon spherules and elongates) that are indicative of major biomass burning …”** and they attribute the biomass burning to have been **“… ignited by an intense radiation flux associated with a cosmic impact.”** Allen West (PBS NOVA, 2009) stated, **“And we know from the chemistry that these** [carbon spherules] **are formed by burning pine trees, burning spruce seeds. This is tree sap, in effect, that’s been scorched, burned.”** This narrative is presented in Kinzie et al. (2014), **“carbon spherules containing NDs have been demonstrated to form from tree sap under laboratory conditions that duplicate the temperature, pressure, and redox values within an impact fireball (Israde-Alcántara et al., 2012)”** (p 476–477) and **“It is well established that carbon spherules can be produced in intense wildfires involving conifers (Firestone et al., 2007; Israde-Alcántara et al., 2012)”** (p 495). Kinzie et al. (2014, p 496) overstate the results of the cited references and from these surmises, **“… the best explanation is that ND-rich carbon spherules derive from conifers that were incinerated by the impact event (Israde-Alcántara et al. [2012]).”** Wolbach et al. (2018b), sharing many authors of Kinzie et al. (2014), similarly wrote in their SI (p 27), **“Investigating that hypothesis, Israde-Alcántara et al. (2012) reported lab experiments that produced nanodiamonds when carbon spherules were exposed to temperatures of ≥1200C in an oxygen-deficient atmosphere, as is expected to occur during impact events but does not occur during typical terrestrial wildfires.”**

Within the sources that were cited in the above quoted text, Firestone et al. (2007, p 16018) wrote, **“we recovered them** [carbon spherules] **from one of four modern forest fires…, confirming that they can be produced by intense heat in high-stand wildfires**” (see also Section 9.3), however, this is merely an inference that is questionable speculation at best. Israde-Alcántara et al. (2012, p E745) wrote **“experiments**

Table 9
Lonsdaleite Bragg reflections.

	Predicted†		Bundy and Kasper, 1967	Frondel and Marvin, 1967	Fedoseev et al., 1983	Bhargava et al., 1995	Ona et al., 2008
(*hkl*)	Intensity	d-spacing (Å)					
(100)	32	2.182	2.19	2.18	2.18–2.20	2.181	2.165
(002)	18	2.060	2.06	2.061	2.06–2.07	2.045	2.089
(101)	16	1.928	1.92	1.933	1.92	1.949	1.933
(102)	7	1.498	1.50	1.50	1.50–1.53		1.504
(110)	13	1.260	1.26	1.257	1.26–1.28	1.257	1.251
(103)	13	1.162	1.17	1.17	1.18	1.167	1.172
(200)	2	1.091					
(112)	8	1.075	1.075	1.075	1.06–1.07	1.073	1.076
(201)	2	1.055	1.055			1.067	
(004)	0	1.030					
(202)	2	0.964			0.970–0.985		
(104)	0	0.932					
(203)	12	0.854	0.855		0.870–0.880		
(210)	6	0.825	0.820		0.820		
(114)	0	0.825					

† lattice parameters: $a = 2.52$ Å and $c = 4.12$ Å (Bundy and Kasper, 1967).

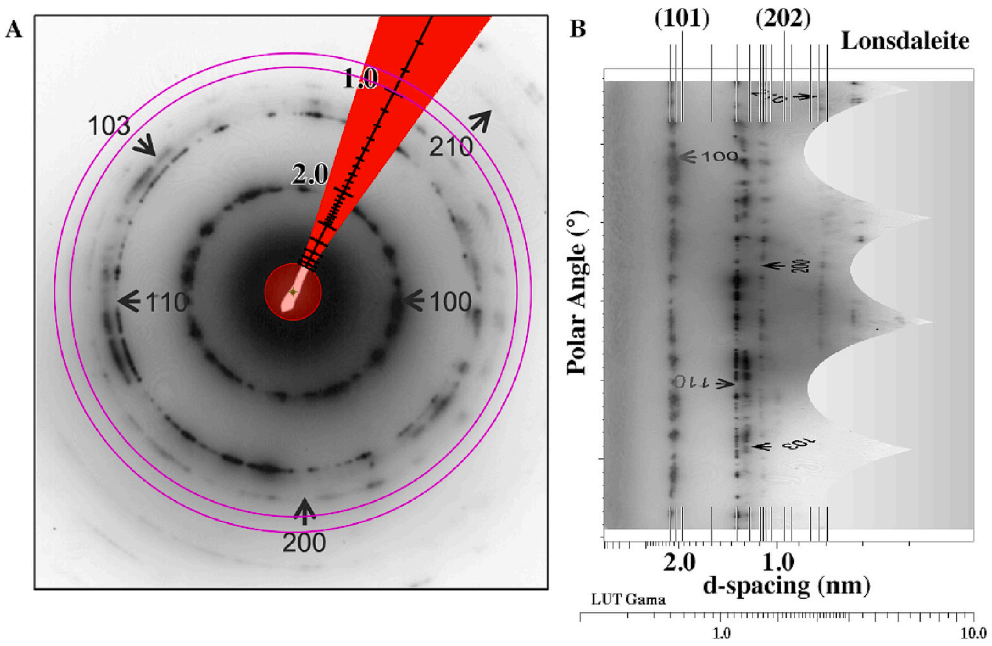

Fig. 4. A) The transmission electron diffraction pattern from SI fig. S2 (part B) of Kennett et al. (2009b). We modified the diffraction pattern from the original published by Kennett et al. (2009b) by inverting its contrast to aid in visual clarity (white – no diffraction intensity, darker grey scale indicates higher intensity) and by superimposing additional annotations on the pattern. A scale bar in units of d-spacing (nm) is superimposed over the needle blocking the non-diffracted beam. Two rings indicate a region of interest (ROI) defined between 0.9937 and 0.9381 nm (d-spacing) where the (202) lonsdaleite reflection would occur. That region is devoid of any detectable diffraction peaks/intensity. All adjustments of the brightness, contrast, and gamma of the image's grey-scale look up table (LUT) did not reveal any intensity features within the ROI. B) The same diffraction pattern displayed using a topological unroll mapping where the polar coordinates (r, θ) are mapped to Cartesian coordinates ($x = r, y = \theta$). To take into account the large dynamic range across scattering angles, the gamma of the LUT varied with scattering radii following a power law with exponent 2.5 (see lower scale bar). Newton rings in the diffraction pattern image, presumably created when the original TEM film negative was scanned, appear visible after the LUT enhancement. The predicted lonsdaleite reflections are denoted by the vertical lines (see Table 9). No discernable diffraction peaks are present at the radii corresponding to (101) or (202) lonsdaleite reflections.

have demonstrated the production of CSp [carbon spherules] **from charred tree resin at approximately 500 °C** [citing an abstract, Kimbel et al., 2008]. **These CSp are morphologically identical to those found in the YDB but** [curiously state] **contain no NDs.**" Kimbel et al. (2008) claims the formation of n-diamonds in charred **"coal, coconut shells, and wood"**, and claims their procedure **"is identical to the commercial process for producing activated charcoal, and in fact, samples of commercially available activated carbon manufactured by both Calgon Carbon Corporation and Norit Americas, Inc. were found to be enriched with n-diamonds."** Although Kinzie et al. (2014) do not cite Kimbel et al. (2008) they discuss the experiments of Kimbel et al. (2008) in detail. In fact, all the coauthors of Kimbel et al. (2008) are coauthors of Kinzie et al. (2014). Kinzie et al. (2014, p 500) wrote, **"…NDs grow within the activated carbon at abundances similar to those found in YDB carbon spherules…"** and **"The conditions required to produce NDs in activated carbon mimic those in a cosmic impact, e.g., anoxia and high temperatures."**

Firestone (2009a, section 4) also wrote, **"The carbon spherules are often found together with copal spherules… suggesting that they have a common origin in tree resin."** However, no analytical data are presented pertaining to the identification/characterization of the "copal spherules". This appears to be the first time copal (see Solórzano-Kraemer et al., 2020) has been reported as spherules in the literature, and no explanation is offered to explain how they survived the high-temperatures presumed produced by an impact. Further, this statement lacks support from any statistical data on spatial distribution and is also, at best, speculation.

In summary, impact proponents claim carbon spherules (and glassy carbon) are produced in wildfires, and those that contain 'nanodiamonds' formed only under conditions, **"within an impact fireball (Israde-Alcántara et al.** [2012]**)"** (Kinzie et al., 2014, p 477) and **"form only during impact events"** (Wolbach et al., 2018b, SI p 27). Kinzie et al. (2014, fig. 2) report 14 YDB sites with 'nanodiamond'-containing carbon spherules including: North America, Germany, Belgium, The Netherlands, United Kingdom, and Spain. Interestingly, while 'nanodiamonds' are purported in Greenland ice, no carbon spherules are reported (Kurbatov et al., 2010; Kinzie et al., 2014). If present, they should have been easy to recover. If a YDB impact occurred, this leaves three possibilities: the ice margin sampled was not of YDB-age, carbon spherules are not produced in an impact fireball, or, if produced by an impact fireball, they are localized to the impact site(s) and are not widely dispersed as distal ejecta. The latter would require a highly unusual event of at least 14 or more separate but associated impacts across the globe, which each leaving no physical evidence such as a crater or impactor material (other than purported trace PGEs).

While carbon spherules may have multiple origins (not necessarily impact related), most carbon spherules studied in YDB sediments have external and internal morphologies indistinguishable from sclerotia of saprobic (e.g., *Athelia rolfsii* – anamorphic form *Sclerotium rolfsii*), phytopathogenic (e.g., *Rhizoctonia solani*, *Botryotinia cinerea*) and ectomycorrhizal (e.g., *Cenococcum geophilum*) fungi (see Scott et al., 2010; Hardiman et al., 2012; Daulton et al., 2017a), to name a few. Sclerotia are ubiquitous in forest litter and soils, and even after death can persist for at least many thousands of years (see Trappe, 1969; Hormes et al., 2004; Benedict, 2011; McLaren et al., 2014). Their presence at archaeological sites has been recognized for many decades (van Zeist, 1981; McWeeney, 1989; Shay and Kapinga, 1997; Deal, 2005; Matsumoto et al., 2010).

Significantly, Sheldrick (1997) measured *C. geophilum* sclerotia concentrations in paleolake sediments at three European sites. At Whitrig Bog (Southeast Scotland) sclerotia **"are present only very occasionally during the early part of the Lateglacial Interstadial** [Bølling-Allerød]" (p 137) and a sharp increase in their concentration occurred in a lacustrine sediment layer that **"spans the period of soil instability and erosion associated with the lower boundary of the Younger Dryas"** (Sheldrick, 1997, p 136), see also Mayle et al. (1997). At Gransmoor (East Yorkshire) **"the onset of cooling in** [the Younger Dryas] **and the rise in *Cenococcum* numbers… are separated by some 275 calibrated radiocarbon years"** (Sheldrick, 1997, p 55) and at Llanilid (South Wales) sclerotia numbers similarly peak shortly after the onset of the Younger Dryas (Sheldrick, 1997; Walker et al., 2003). The interpretation is that "[c]**hanges in the environment are only reflected in proxy data** [e.g., sclerotia] **when they cross important thresholds. As these thresholds vary for different taxa, and may be mediated by a host of site-specific local factors, a gradual climate change will lead to these threshold being crossed at different times"** (Sheldrick, 1997, p 55). A marked increase in *C. geophilum* sclerotia following the Allerød and/or during the YD/GS-1 has been observed in many other lacustrine sites (Table 10).

Walker et al. (2003, p 489) observed, **"[a]s a mycorrhizal fungus the remains of *Cenococcum* are only found in lake sediments where there is a considerable input of material from surrounding soils, and hence the increase in *Cenococcum* numbers… provides unequivocal evidence for the onset of soil erosion around the pond catchment."** Sheldrick (1997, p 98) earlier inferred for Llanilid, **"[t]he abrupt nature of the rise and magnitude** [in *C. geophilum* numbers] **indicates the initiation of soil instability on such a scale that it is unlikely to reflect one of the short-lived interstadial climatic oscillations, (e.g. 'Older Dryas' or 'Killarney')**[.] **More probably it represents the onset of soil instability triggered by the more prolonged cooling that extends into the Younger Dryas chronozone." "The large size and number of the sclerotia in the early part of the Younger Dryas does however indicate that stable well developed soils were present during the** [preceding] **Interstadial"** (Sheldrick, 1997, p 137). This interpretation is supported by additional evidence. For example, **"[s]oil instability is also supported by the geochemical data, with high levels of the important erosion indicators aluminium and titanium"** in the YDB at Whitrig Bog (Sheldrick, 1997, p 146). Also, **"a number of plant macrofossil types** [in the YDB at Whitrig Bog] **not noted for their cold-adaption, e.g. *Urtica dioica*, may be largely due to the instability of the soils, a proportion of the recovered macrofossils being redesposited material first deposited during the Lateglacial Interstadial"** (Sheldrick, 1997, p 137). Further, plant microfossils with anomalously old ^{14}C dates ("old wood effect") were observed in Llanilid sediments that exhibited a peak in sclerotia concentration and **"according to paleoenvironmental evidence"** are associated with **"a cold period of considerable soil instability, which has been identified as the later part of the Lateglaccial Interstadial and the early part of the Younger Dryas chronozone. The anomalously old dates may therefore be due to the influx of 'old' (recycled) material during this period of soil instability in the catchment"** (Sheldrick, 1997, p 92). Birks and Birks (2013), Lascu et al. (2015), Słowiński et al., 2017, and Krüger et al. (2017) similarly all concluded that climate-change driven loss of vegetation caused Allerød-age soil to destabilize and wash into the lakes increasing sclerotia deposition into lacustrine sediments.

Furthermore, Usselo/Finow soils dated to the onset of the YD/GS-1 at Wolin Island (northwest Poland, Latałowa and Borówka, 2006) and Leusden-Den Treek (The Netherlands, Bazelmans et al., 2021) showed a peak in *geophilum* sclerotia and charred *Pinus* needles/seeds. Elevated abundances of carbon spherules (and other materials, such as charcoal) at purported YDB sediments could be explained by climate-driven destabilization of Allerød soils resulting in redeposition and concentration of sediment constituents including sclerotia as observed in various studies.

Israde-Alcántara et al. (2012, p E745) claimed carbon spherules exhibit **"no evidence of filamentous structure** [i.e., hyphae] **observed in fungal sclerotia,"** and this is incorrect. Fungal hyphae (and their sclerotia) have distinct pores within their septal walls (Figs. 5a,f) that allow movement of cytoplasm and organelles (see Reichle and Alexander, 1965; van Peer et al., 2009). A close examination of the published images of a carbon spherule and an 'elongated' variety of carbon spherules from the YDB of Arlington Canyon shown in the supplemental materials of Kennett et al. (2009b) clearly reveal small pores in the preserved cell walls (Figs. 5b,e). The presence of these pores in the YDB carbon spherules conclusively identify them as fungal sclerotia at some undetermined stage of diagenesis (M. Watanabe, pers. comm.). Septal pores are also clearly evident in YDB carbon spherules shown in a figure of Largent (2008) that is attributed to Allen West (Fig. 5c) and in fig. A2 of the supplemental materials of Wolbach et al. (2018b) (Fig. 5d). Septal pores are also visible in YDB carbon spherules provided to Scott et al. (2010) by G. J. West and J. J. Johnson (e.g., see fig. S5D of the supplemental materials of Scott et al., 2010).

Kinzie et al. (2014, p 496) offhandedly dismiss the possibility the carbon spherules (or any portion of them) are sclerotia stating the **"sclerotial hypothesis must account for the presence of millions of** [misidentified (see Section 12.5)] **NDs entrained within *each* carbon spherule (Kennett et al., 2009a)** [emphasis added]." Kinzie et al. (2014) ignore the inconvenient detail that they purport only a few carbon spherules contain 'nanodiamonds' (see Section 12.7), and speculate (p 496), "**There is no credible mechanism by which fungi can create NDs in sclerotia."** Wolbach et al. (2018b, SI p 27) repeats that invalid reasoning. In their reviews, Sweatman (2021) and Powell (2022) ignore the evidence that carbon spherules are sclerotia.

Since undoubtedly a significant number of carbon spherules were sclerotia and not wildfire products as impact proponents have claimed, their measured concentration profiles across the YDB cannot be used to test the YDIH. Furthermore, radiocarbon dating shows examples of carbon spherules, glassy carbon, and microcharcoal within the purported YDB that have ages outside of the YD/GS-1 onset (e.g., see Firestone, 2009a, 2009b and Section 5.8). Even *if* they were all wildfire products, their concentration at the purported YDB do not necessary reflect the number formed by impact-generated wildfire (should that have occurred). This is because individually radiocarbon-dated particles were not counted within a wide sediment band centered on the YDB to yield concentrations as a function of age. The absence of precision dating of each particle in a sample could misleadingly infer that products within a sediment layer potentially generated by different wildfires over a span of time were from a single event. Thus, published concentration profiles of carbon spherules, glassy carbon, and microcharcoal cannot provide a measure of wildfire product generation over an extremely narrow time interval as required to directly test the YDIH. Furthermore, such data if available could only record a wildfire event occurred. It would be difficult to infer the magnitude of the event from the data and the data would not record the cause of the event (see also Section 9).

12.5. 'Nanodiamond' misidentifications

Daulton et al. (2010, 2017a) examined acid residues of YDB sediments and YDB carbon spherules for nanodiamonds and did not observe diamond (or C phases consistent with the debated n-diamond and i-carbon). Sweatman (2021) attempted to discredit these critical studies by erroneously claiming the wrong specimens were collected and wrong locations were sampled. Regarding the samples Sweatman (2021, p 9) wrote, **"Daulton et al. (2010) were unable to reproduce these results** [observation of nanodiamonds] **but this was very likely due to collection of incorrect samples. Kennett et al. (2009b) reported nanodiamonds inside or adhered to specific kinds of glassy carbon particles, such as carbon spherules and glassy carbon 'elongates'. However, Daulton et al. (2010) analyzed microcharcoal aggregates from Murray Springs, which are not expected to contain any nanodiamonds."** Microcharcoal was not the only material studied by Daulton et al. (2010). They also examined carbon spherules as well as glassy carbon, and later acid residues of YDB dated sediment (Daulton et al., 2017a); microcharcoal was examined to be thorough given that impact proponents claim YDB nanodiamonds were formed through a process **"identical to the commercial process for producing activated charcoal"** (Kimbel et al., 2008, see also Kinzie et al., 2014; Wolbach et al., 2018b). Regarding the field sites, Sweatman (2021) wrote, **"…Wittke et al. (2013b) and Kinzie et al. (2014) show… that Daulton et al. (2010) did not, in fact, sample the same site as Kennett et al. (2009b) at Arlington Canyon."** The same AC003 site of

Kennett et al. (2009b) was sampled (Section 4.1), and no ‘nanodiamonds’ were observed by Daulton et al. (2010, 2017a, 2017b) in YDB-dated materials from Arlington Canyon.

Rather, within carbon spherules extracted from Arlington Canyon YDB-dated sediments, Daulton et al. (2017a, 2017b) observed graphene/graphane aggregates previously discussed, as well as Cu and CuO_2 nanocrystals that have identical diffraction lines as ascribed to n-diamonds and i-carbon, respectively, with plane spacing differing by ≈1%. Copper is present in sediments at relatively high concentrations relative to those reported for ‘nanodiamonds’. Trace Cu is present at 5–9 ppm in several-thousand-year old (preindustrial era) sediment deposits (DeLaune et al., 2016), presumably in a range of minerals. In comparison, the ‘nanodiamond’ peak concentrations in purported YDB sediments are claimed to have a smaller range of 66–493 ppb (Kinzie et al., 2014 and supplemental materials). Sclerotia-forming fungi, such as *Botrytis cinerea* and *Sclerotinia sclerotiorum*, utilize Cu to assist in infecting host plants (Saitoh et al., 2010; Ding et al., 2020), and sclerotia are efficient biosorbents of Cu(II) (Long et al., 2017). The mean concentration of Cu reported in sclerotia is between 43 and 152 ppm (Nyamsanjaa et al., 2021). Impact proponents have reported carbon spherules containing as high as 600 ppm to 0.06 wt% Cu (Firestone, 2009a; Firestone et al., 2010a). In comparison, the ‘nanodiamond’ peak concentration in purported YDB carbon spherules is again claimed smaller, 10 to 3680 ppb (Kinzie et al., 2014 and supplemental materials). Since the purported n-diamond and i-carbon in YDB carbon spherules and sediments can be easily confused for the relatively more abundant Cu minerals (Daulton et al., 2017a, 2017b), the identification of the controversial n-diamond and i-carbon is necessarily placed into question.

To further demonstrate that impact proponents most likely misidentified Cu and its oxides as ‘nanodiamonds’ in sediments and carbon spherules (i.e., sclerotia), consider the international patent application (Provisional US application No. 61/062,350 filed on Jan. 25, 2008; Patent Cooperation Treaty No. PCT/US09/31731 filed Jan. 22, 2009), e.g., (West and Kennett, 2009a, 2009b, 2009c, 2011) to name a few. This patent is mentioned in Kimbel et al. (2008) and was submitted by two major coauthors of the key papers on purported YDB ‘nanodiamonds’ (Kennett et al., 2009a, 2009b; Kurbatov et al., 2010; Israde-Alcántara et al., 2012; Kinzie et al., 2014; Moore et al., 2020).

In addition to claiming a process for forming nanodiamonds during charcoal production, the patent (West and Kennett, 2011, p 6) also claims the following process:

> **“[0070] A 3-mm-wide grid for observing samples in a transmission electron microscope (TEM) was used. The grid was constructed of a thin copper support structure with about 90-micron-square holes in it, and which supported an approximately 50-nm-thick amorphous carbon film. Neither the copper nor film contained diamonds originally. Next, a drop of dilute hydrochloric acid (HCl) with a pH of 0.5 was deposited on the grid and immediately afterward, dried it at atmospheric pressure and room temperature over a span of several minutes. [0071] Upon viewing the grid by TEM, diamonds had grown as nanometer-sized fibers at the junction of the copper and the carbon film. In some cases, the HCl had not dried completely, and in those cases, the active diamond growth process was observed by TEM. As observed, the diamonds writhed as if living, grew longer, became wider, and some times several fibers coalesced into one large fiber. Within a few minutes, the HCl dried and the diamond synthesis ceased. The process produced a large number of nanodiamonds on a 3-mm-wide grid within minutes.”**

Nanodiamonds were similarly claimed to form through the same process using a slightly different solution (West and Kennett, 2011, p 6), **“Carbon dust from charred coconut shells was collected and tested to determine that it did not contain diamonds. Next, slurry was made by combining the carbon with 0.5-pH HCl. Then, a drop of the carbon-HCl solution was added to a 3-mm-wide copper grid without a carbon film.”**

Rather than forming diamond under nonsensical, by any standard, formation conditions for diamond, it is far more likely the Cu of the TEM grid and its surface oxides were dissolved by HCl, which then precipitated out as the solution evaporated. The reported nanowire growth under the electron beam of the TEM instrument by West and Kennett (2011) may have involved mechanisms similar to that investigated for Cu by van der Meulen and Lindstrom (1956), Glad et al. (2020), and Hamdan et al. (2020). The West and Kennett (2011) patent application, now apparently abandoned by its authors and based on experiments reported in Kimbel et al. (2008), certainly place into question the credibility of all the results claimed in Kimbel et al. (2008). Consequently, the identification of ‘nanodiamonds’ in key YDB ‘nanodiamond’ papers sharing coauthors of Kimbel et al. (2008) are also placed into question: Kennett et al. (2009a, 2009b), Kurbatov et al. (2010), Israde-Alcántara et al. (2012), Kinzie et al. (2014), and Moore et al. (2020).

Table 10

Lacustrine sites that display elevated concentrations of *C. geophilum* sclerotia following the Allerød and/or during the YD/GS-1.

Location	Sampling	Reference
Bølling Sø Lake Jutland, Denmark (56°10′35.4”N, 9°22′20.9″E)	Sediment cores of lake that was drained around 1870.	Bennike et al., 2004
Slotseng southwest Denmark (55°19′43”N, 9°16′8.24″E)	Excavated lacustrine deposits.	Mortensen et al., 2011
Kråkenes Lake western Norway (62°02′N, 5°00′E)	Sediment cores of south-east basin from marsh that developed over exposed lake sediment after water levels were lowered one meter around 1913.	Birks and Birks, 2013
paleolake Măgheruș Valley Romania (47°05’942 N, 24°23′618E)	Măgheruș river exposed sediment outcrops.	Lascu et al., 2015
postglacial paleolake Czarne Poland (54°17′47”N, 22°03′33″E)	Sediment cores of bog.	Karpińksa-Kołaczek et al., 2016
paleolake Trzechowskie Poland (53°52’22 N, 18°12′58E)	Sediment cores of deepest part of lake basin.	Słowiński et al., 2017
submerged Doggerland North Sea (55°44′36.98”N, 3°46′27.88E) (56°20.788′N, 6°6.615′E)	Inferred lacustrine deposits in two North Sea cores.	Krüger et al., 2017
paleolake at Gourd des Aillères mire Central Massif, France	Sediment cores of mire.	Cubizolle et al., 2021

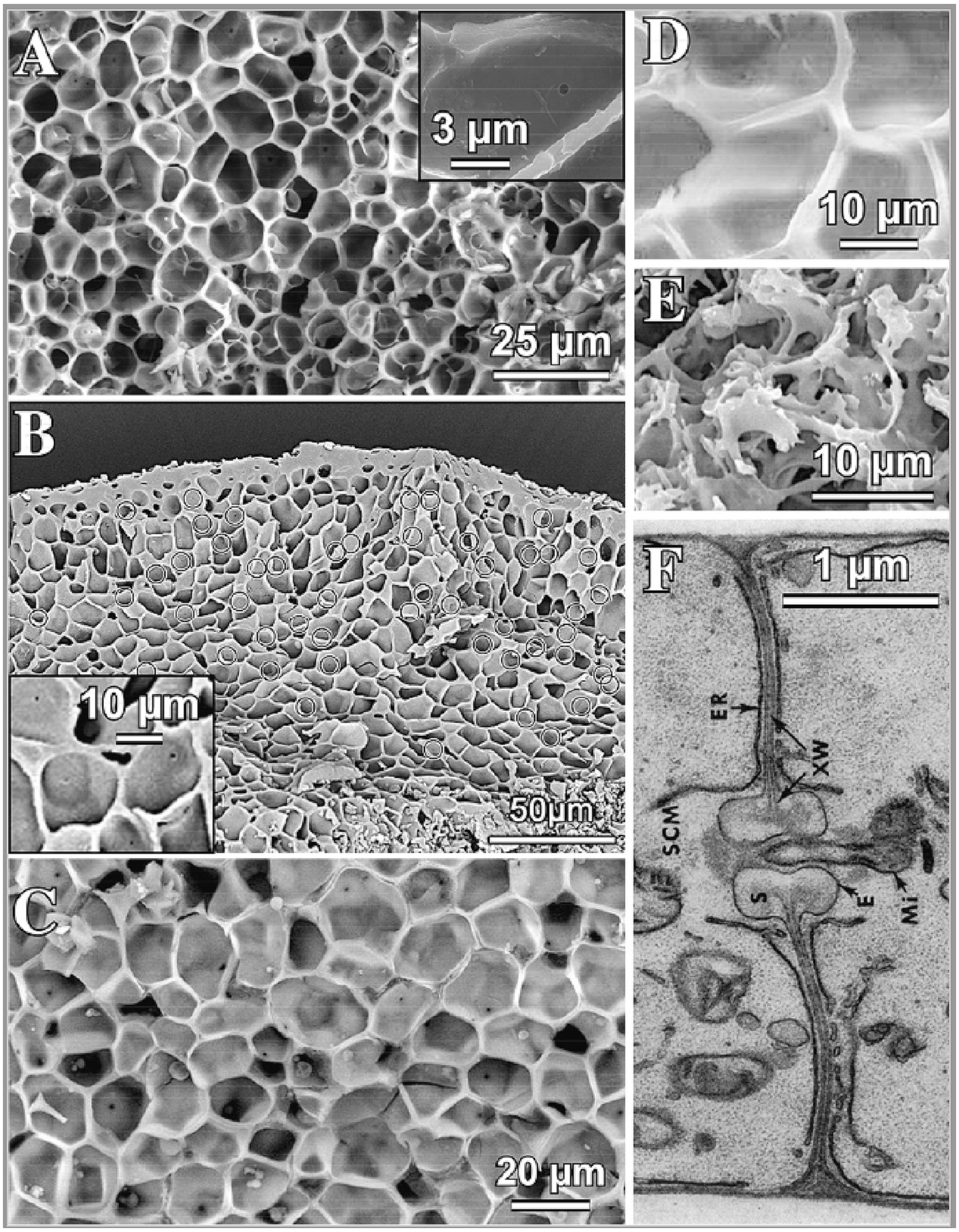

Fig. 5. a) SEM image of the interior of cross-sectioned *Cenococcum geophilum* sclerotium displaying micron-sized holes (septal pores), which are morphological features characteristic of sclerotia (image courtesy of M. Watanabe). Inset in the panel is a SEM image of cell in a *C. geophilum* sclerotium displaying a septa pore from fig. 4.1 of Nonoyama and Narisawa (2021) (after being cropped with scale bar added). b) SEM image of the interior of a carbon spherule from Arlington Canyon YDB sediments (AC-003) from SI fig. S6 (part F) of Kennett et al. (2009b) with circles overlaid to denote several (not all) of the submicron-sized holes present in the cell-like walls (original panel label replaced). Their figure caption states, **"(F) Close-up of carbon spherule interior shown in E with well-organized reticulate (honeycomb) structure and thin, nonreticulate crust"**. Inset in the panel is a magnified area of the carbon spherule. c) SEM image of the interior of a carbon spherule from a figure in Largent (2008) attributed to Allen West (original SEM image after being cropped with scale bar and panel label added). D) SEM image of the interior of a carbon spherule from fig. A2 of the supplemental materials of Wolbach et al. (2018b) (after brightness/contrast/gamma enhancement of the grey scale look up table (LUT) to bring out the contrast within the cell interiors, and after being cropped with scale bar and panel label added). e) SEM image of the interior of an "elongated" variety of carbon spherules from Arlington Canyon YDB sediments (AC-003) from SI fig. S5 (part E) of Kennett et al. (2009b) displaying submicron-sized holes present in the cell-like walls (after being cropped with scale bar and panel label added). Their figure caption states, **"(E) Irregular, complex, nonreticulate interior of carbon elongate shown in D that illustrates well-vitrified and brittle thin walls of amorphous carbon separating voids."** The presence of the holes in b)-e) provides a conclusive identification of the carbon spherules as sclerotia (M. Watanabe, pers. comm.). f) TEM image of a cross section of a hypha of the sclerotia-forming *Rhizoctonia solani* displaying the septal pore apparatus with a mitochondrion (Mi) passing through the septal pore from fig. 20 of Bracker and Butler (1963) (after being cropped and rotated with scale bar and panel label added).

12.6. 'Nanodiamond' concentration spike at YDB

A compilation of all the available 'nanodiamond' evidence claimed to support the YDIH was presented by Kinzie et al. (2014). This included measurements of the 'nanodiamond' concentration at purported YDB sites using techniques based on electron microscope estimations of implied modal abundances of diamond in crushed spherules, acid dissolution residues, and melted ice. Kinzie et al. (2014) purport a spike in the nanodiamond concentration at the YDB at multiple sites across the Northern Hemisphere. Sweatman (2021, p 9) wrote, **"A coetaneous abundance of nanodiamonds dispersed across a large area at Earth's surface, therefore, is an excellent proxy for a cosmic impact, especially in the absence of evidence for volcanism, such as sulphate and tephra abundances."** Accurate dating of the stratigraphic record of the purported YDB sites is problematic in many cases and reported ages have been questioned (see Section 5 and ENDNOTE 10). Thus, there is no clear indication that the YDB layer was sampled in many of those sites. More importantly, in a critical review, Daulton et al. (2017a) describe in detail the microanalytical difficulties of identifying nanoparticles in acid residues of sediments and in crushed carbon spherules (see also Section 12.4) and the difficulties in their quantification. These experimental challenges render electron microscopy estimations of modal abundances of diamond within those materials, as performed by impact proponents, technically impractical/impossible (Daulton et al., 2017a). Consequently, the reported high concentrations of 'nanodiamonds' at the YDB and complete (or near complete) absence immediately above and below this level are completely unsupported.

The material recovered from crushed spherules or acid dissolution residues of sediments contains a wide range of mineral species. Kinzie et al. (2014, p 480) state, **"Typically, NDs represent <50% of the residue, and the remaining non-ND residue can mask the NDs, thus making them difficult to identify."** The greatest limitation of the approach of Kinzie et al. (2014) and others is that detailed laborious measurements must be performed on each individual nanoparticle in order to correctly identify whether it is diamond or not. Kinzie et al. (2014, p 480) acknowledge the experimental challenge of identifying nanodiamonds by writing, **"In addition, there are inherent difficulties and uncertainties in correctly identifying tiny crystals <2 nm in diameter."** They further state (p 485), **"By themselves, SAD patterns are insufficient to identify NDs, and so further investigations, such as those using HRTEM, FFT, EDS, and EELS, were performed on these nanoparticles to confirm that they are NDs and not some other mineral."** In their conclusions, Kinzie et al. (2014, p 500) specifically described their methodology as **"The identification of the isolated NDs involves two main methods, electron microscopy imaging and electron spectroscopy, using up to nine imaging, analytical, or quantification procedures: scanning electron microscopy, STEM, TEM, HRTEM, EDS, SAD, FFT, EELS, and EFTEM. The entire procedure is labor-intensive and technically demanding. Even so, it has proven to be effective and replicable by skilled independent groups based on the processing of more than 100 samples."** However, Kinzie et al. (2014) perplexingly describe in their supplemental materials (p 9), **"… for the purpose of estimating abundances, we assumed that all rounded particles were NDs. We also observed abundant amorphous carbon nanoparticles, but almost none were rounded, and therefore, we discounted them.** ***This estimation procedure focused solely on the presence or absence of rounded particles*****.** [emphasis added]" The methodology actually employed was stated only in the less accessible supplemental materials and starkly contradicted the methodology Kinzie et al. (2014) described in their main text, which is a troubling contradiction. In this light, one could interpret the Kinzie et al. (2014) paper as deceptive. We reiterate that Kinzie et al. (2014) measured projected areal densities of "rounded particles," not necessarily nanodiamonds, and they certainly did not measure modal mass abundances. This is a critical flaw, given that the acid-dissolution residues and crushed spherules are not pure diamond and contain a multitude of different minerals.

For measuring 'nanodiamond' abundances Kinzie et al. (2014, SI p 10–11) estimated the area fraction of TEM grids that contained "rounded particles" in mounted sediment acid residues and crushed carbon spherules. This is neither a measurement of mass or even volume fraction of "rounded particles" in those specimens (see Daulton et al., 2017a). Mass or volume fraction is needed to accurately determine abundance in the source specimen. Instead, the area fraction on TEM grids was normalized by the mass fraction of recovered residue from processed sediment and the mass fraction of carbon spherules from 1 kg of sediment, respectively. Using the latter must necessarily assume that all carbon spherules contained "rounded particles", but the fraction claimed to contain 'nanodiamonds' significantly changes in different impact proponent publications (Section 12.7).

It is worthwhile noting that nanodiamonds isolated by acid dissolution from sediments at the KT boundary and inferred associated with the Chicxulub impact, are not reported rounded. Carlisle and Braman (1991, p 708) wrote, **"3-5 nm in size and, wherever any morphology could be discerned, octahedral in form"**. Gilmour et al. (1992, p 1624) wrote, **"~6 nm in size and vary in morphology from irregular to near-cubic crystals."** Hough et al. (1997, p 1020) wrote, **"polycrystalline diamond aggregates ranged from 1–30 μm and some displayed a hexagonal platy shape (Fig. 2A), which may indicate that graphite was the precursor carbon material. Individual diamond crystals in these aggregates, have grain sizes in the range 0.1 to 1 μm."** In contrast to these descriptions, Kinzie et al. (2014, p 491) wrote, "**in most cases, YDB NDs** [consisting mostly of n-diamond and i-carbon, Section 12.3] **are rounded to subrounded**", which by the way is consistent with the rounded morphology of Cu nanocrystals observed in carbon spherules by Daulton et al. (2017a, fig. 9).

Of all the YDB sites, three sites should, by all reason, potentially offer the most compelling 'nanodiamond' 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 published by the YDIH proponents further illustrate that those measurements are unreliable.

Bull Creek, Oklahoma was one of the early sites where 'nanodiamonds' were purported, with a spike in the 'nanodiamond' concentration of 100 ppb at the YDB (Kennett et al., 2009a). In a subsequent study of the same section, Bement, a coauthor of Kennett et al. (2009a), purported a three order-of-magnitude larger 'nanodiamond' spike of 190 ppm (Bement et al., 2014) that was higher than that purported at or around the YDB of all other sites (see Kinzie et al., 2014). However, this 'nanodiamond' peak was purported in sediments older than the YDB (Table 5, also Section 5.5). Nevertheless, subsequent attempts by the Bement group to further study the YDB 'nanodiamonds' at Bull Creek were unsuccessful because – in the same 'nanodiamond' sediment isolate previously purported to contain 'nanodiamonds' (Bement et al., 2014) – the 'nanodiamonds' could not be found (Sexton, 2016). Sexton (2016) is a thesis where L. Bement and A. Madden, coauthors of Bement et al. (2014), were thesis advisors. Following publication of Sexton (2016) and citation of that study by Daulton et al. (2017b), impact proponents (e.g., LeCompte et al., 2018; Wolbach et al., 2018b supplemental; Wolbach et al., 2020; West et al., 2020a; Powell, 2020, 2022; Sweatman, 2021) continue to claim the results of Bement et al. (2014) support the YDIH. No impact proponent has cited Sexton (2016), including Powell (2022), who cites Daulton et al. (2017b) and thus, must clearly be aware of Sexton (2016) and yet he still cites the irreproducible results of Bement et al. (2014).

The Bull Creek results also illustrate a characteristic shared among many YDIH proponent papers: self-inconsistency and circular arguments. Bement et al. (2014) also reported a high 190 ppm concentration of nanodiamonds in each of two adjacent levels in modern to Late Holocene sediments, which must be viewed as unreliable in light of Sexton (2016) and, as discussed, the inappropriate TEM methodologies utilized. Nevertheless, Kinzie et al. (2014, p 478) accepts these concentration peaks as accurate and wrote, **"In addition, Bement et al. (2014) observed an ND abundance peak of similar amplitude to their YDB peak in two contiguous samples of late Holocene surface sediments (0–10 and 10–20 cm below surface). They suggested that this younger ND peak may have been produced by a nearby cosmic-impact event within the past several thousand years."** In an example of self-inconsistency Kinzie et al. (2014, p 483) misleadingly wrote **"our group and others have measured marker abundances** [including 'nanodiamonds'] ***in several stratigraphic profiles that span as much as the past 30,000 yr. These proxies reached maximum abundances only in the YDB layer and are not known to peak individually or collectively anywhere else in that span*** [emphasis added]**, making the YDB highly unusual.** See Section 4." In an example of circular reasoning LeCompte et al. (2018, p 165) claimed, **"If nanodiamonds could be produced in natural fires, they should be common and ubiquitous in sediments of all ages, but instead, they range from nonexistent to extraordinarily rare, being found in high abundances only in known or *proposed*** [emphasis added] **impact-related sedimentary layers…"** The flawed logic is that if one assumes any 'nanodiamonds' in sediments were formed by impact, then one will misconstrue 'nanodiamonds' are only found in known or *proposed* impact-related sediments, and hence the 'nanodiamonds' must be formed by impact.

A further indication of the unreliability of the nanodiamond concentration measurements is found in a study of the Lubbock Lake archaeological site in northwest Texas (Johnson, 2012; Holliday et al., 2016). In 2007, a blind study of a stratigraphic section sampled across the YDB at Lubbock Lake was performed by two independent groups (Surovell and Kennett) for joint publication (see Sections 10 and 14). Only the Kennett group attempted the measurement of 'nanodiamond' concentrations, reported by Holliday et al. (2016). They reported a dramatic 'nanodiamond' spike with near complete absence of 'nanodiamonds' in other levels they analyzed. Again, the concentration purported for the 'nanodiamonds' was no less extremely high (3000 ppb) relative to that at all other global YDB sites (66–493 ppb) where 'nanodiamonds' had been measured (Kinzie et al., 2014), and second only to Bull Creek (Bement et al., 2014). Given that the Bull Creek measurements were not reproducible (Sexton, 2016), Lubbock Lake then becomes the highest purported 'nanodiamond' concentration of all YDB sites. However, the concentration spike at Lubbock Lake occurred at a stratigraphic level dated ≤11.5 cal ka BP, at least 1300 years younger than the YDB.

As noted previously, Sweatman (2021, p 9) claimed that coetaneous YDB nanodiamonds **"across a large area at Earth's surface... is an excellent proxy for a cosmic impact."** However, in addition to numerous unresolved issues of dating purported YDB layers (Section 5), 'nanodiamond' concentrations as reported by impact proponents are not coetaneous even at the local scale of a single purported YDB site. Arlington Canyon is the one site where the concentrations of n-diamonds, 3C polytype nanodiamonds, and 2H polytype nanodiamonds were individually purported (Kennett et al., 2009b, SI table S1). The concentrations of n-diamonds in carbon spherules and in 'elongated' variety of carbon spherules were separately measured. They are not reported with broad distributions that overlap, but rather entirely different with well-defined, disparate peak positions. In carbon spherules, n-diamond concentrations peak at 480–485 cm and at 493–498 cm below surface. In elongated carbon spherules, n-diamond concentrations peak at 392–396 cm and at 498–503 cm below surface. Also in elongated spherules, nanodiamonds of the 3C polytype are purported only at 383–386 cm below surface. The 2H polytype nanodiamonds purportedly peak at 459–463 cm below surface. If these nanocrystals were all formed by a single impact event, why would their concentrations peak at different stratigraphic levels, and why do the n-diamonds have different bimodal peaks depending on slight variations in their host grains? Sweatman (2021) also makes coetaneous claims for other purported impact markers, but many do not overlap in their stratigraphic levels (see Daulton et al., 2017a).

12.7. *Redefinitions of 'nanodiamond'-related markers*

In response to challenges to their results and claims, YDIH proponents progressively redefine (see also Section 10) and then draw back the evidence in subsequent publications. Firestone et al. (2010a, p 35) first wrote, **"Many carbon spherules contained nanodiamonds which are clear evidence of production during an impact."** After the identification of 'nanodiamonds' (including lonsdaleite) was challenged by Daulton et al. (2010), the relative proportion of carbon spherules containing 'nanodiamonds' was significantly reduced. Kinzie et al. (2014, p 483) wrote, **"For carbon spherules, 111 of 153 samples investigated (73%) contained no detectable NDs."** However, the supplement materials of Kinzie et al. (2014, p 5) reduced without explanation the fraction of carbon spherules with nanodiamonds further, **"only a small fraction of carbon spherules contains NDs (average ≈5%; range ≈2% to 19%)."** In addition, Kinzie et al. (2014, p 475) redirected discussion of lonsdaleite to a vaguely-redefined hypothetical mineral. In their abstract they wrote, **"Observed ND polytypes include cubic diamonds, *lonsdaleite-like crystals*** [emphasis added], **and diamond-like carbon nanoparticles, called n-diamond and i-carbon."**

In addition to carbon spherules, Kennett et al. (2008a, 2009b) claimed similar but morphologically distinct carbonaceous materials, termed carbon elongates, were also present in YDB sediments at *greater* concentrations than carbon spherules. Carbon elongates were also purported to host 'nanodiamonds' at over an order of magnitude *higher* ppb concentrations than carbon spherules (Kennett et al., 2009b). One difficulty with the YDIH is that if 'nanodiamond'-containing carbon spherules and carbon elongates were formed by the same event, why were they reported with disparate concentration profiles in the sediments (Section 12.6). Scott et al. (2010) challenged the identification of carbon elongates and carbon spherules by YDIH proponents. Afterwards, Kinzie et al. (2014) made no reference to carbon elongates in the main text, but discussed carbon spherules at length. Based on a comparison of purported concentrations in supplemental table D of Kinzie et al. (2014) and table 3 of Kennett et al. (2009b) it appears Kinzie et al. (2014), with Kennett as coauthor, reclassified the purported more abundant and more 'nanodiamond'-enriched carbon elongates as carbon spherules. However, no explanation is provided for this reclassification. The reclassification seemingly removes the problem that several markers have different concentration profiles in the sediments. However, the new, redefined singular marker has a problematic purported bimodal distribution in the sediments. More importantly and despite that reclassification, impact proponents still purport that differences in the morphology of these carbonaceous materials correlated to differences in their purported 'nanodiamond' concentrations as well as their concentration profiles within the sediments (Kennett et al., 2008a, 2009b), and this is difficult to reconcile with them all being formed by a single abrupt event.

12.8. *Diamondoids*

Sharing several key authors with a number of major YDIH papers, Bunch et al. (2021) purport evidence of a cosmic airburst at ~1650 BCE in the Jordan Valley (but see Jaret and Harris, 2021; Boslough, 2022). Bunch et al. (2021, p 11–12) wrote, **"To search for nanodiamonds in TeH** [Tall el-Hammam] **sediment, we followed the protocol of Kinzie et al.** [(2014)]**... In six samples of TeH bulk sediment from the temple (LS42J), we searched for, but were unable to detect the presence of nanodiamonds..."**. Bunch et al. (2021, p 12) however purport, **"Diamonoids were observed in all samples investigated, but abundances peaked at ~ 3 ppm in the temple destruction layer."** Presumably they are referring to diamondoids, which are small clusters of *sp*^{3}-bonded carbon atoms fully terminated by hydrogen (i.e., a series of hydrocarbon molecules), which represent a fragment of a unit cell (e.g., adamantine, $C_{10}H_{16}$) up to several unit cells of hydrogen-terminated diamond, and are thus the smallest possible nanodiamonds (see Schwertfeger et al., 2008; Stauss and Terashima, 2017). Bunch et al. (2021, p 12) wrote, **"Analyses by transmission electron microscopy (TEM) and selected-area electron diffraction (SAD) indicate that the structures are composed of quasi-amorphous carbon that does not produce SAD patterns (Fig. 8c), even though organized, short-range structures are present (Fig. 8b). This material is commonly referred to as diamonoid or diamond-like carbon (DLC)... representing the smallest unit observed in a diamond crystal lattice."** Amorphous/disordered carbon is categorically not commonly referred to as diamondoid or diamond-like carbon. Diamondoids are discrete hydrocarbon molecules with *sp*^{3}-bonded carbon and differ from DLC, which is a continuous amorphous network characterized by a large fraction of *sp*^{3}-bonded carbon. Bunch et al. (2021, p 12) offer no tenable data to support the presence of either. They wrote, **"When the same field of residue was exposed to ultraviolet light sources, the carbon-rich residue luminesced (Fig. 8e) at some of the characteristic luminescence bands for diamond, 365 nm (long-wave UV) and 440 nm..."** Luminescence intensity at those wavelengths is not unique to diamond, and other trace minerals could have been responsible (e.g., see MacRae and Wilson, 2008). Further, a wide range of luminescence spectra is possible for diamond depending on the nature of the defect centers (e.g., see Bruce et al., 2011; Hainschwang et al., 2013) rendering the use of luminescence spectra for diamond identification difficult, and Bunch et al. (2021) published no spectra for possible analysis. Bunch et al. (2021, p 12) also wrote, **"One fragment of melted pottery from the palace exhibits the results of the impact of a 30-μm-wide carbon-rich particle (Fig. 9)."** The caption of fig. 9 states (p 13), **"Diamond-like carbon embedded in pottery from the palace. (a) Pure carbon aggregate, *likely*** [emphasis added] **a diamonoid cluster…"**. No data are presented on this grain to support that it is likely a diamondoid cluster rather than common amorphous/disordered carbon.

In an earlier paper sharing many coauthors with Bunch et al. (2021), Kinzie et al. (2014, p 487) made similar speculations with regard to YDB specimens and wrote, **"the residue between NDs appears to consist of diamond-like nanocrystals arranged in short-range ordering that causes them to appear amorphous. It is possible that these are diamondoids",** but also offers no tenable evidence in support. In fact, they concede, **"More work is necessary to determine the nature and identity of these small nanoparticles"**, although it is unclear if **"the residue"** is a disordered carbon network rather than the surmised discrete nanoparticles. Kinzie et al. (2014, p 487) wrote without any supporting citations, **"Because 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, especially if an impact took place in deep, petroleum-rich offshore sediments."** This scenario would result in a large and undiscovered oceanic crater but that is inconsistent with other YDIH arguments that the lack of a YDB crater is due to single/multiple bolide air burst(s) or shallow impact(s) on the Laurentide Ice sheet. Bunch et al. (2021) liberally paraphrased Kinzie et al. (2014) and wrote, **"Kinzie et al… concluded that impact-related nanodiamonds and diamond-like carbon (DLC or diamonoids) are produced from the pyrolysis of carbon sources, e.g., vegetation and carbonate rocks that were pyrolyzed during high-temperature, high-pressure airburst/impact events."** With regard to their conjecture on diamondoid formation, while diamondoids have been detected in KT boundary sediments at Kawaruppu, Hokkaido, Japan, their concentration at the KT boundary was an order of magnitude lower than above and below the boundary (Shimoyama and Yabuta, 2002). Shimoyama and Yabuta (2002, p 188) concluded that production of **"diamondoid hydrocarbons showed no complete recovery to the abundance levels of the Cretaceous"** ca. 550 kyr following the impact. Diamondoids form in the subsurface through diagenesis of organic precursors involving clay mineral superacids (Dahl et al., 1999, 2003; Wei et al., 2006, 2007), and Shimoyama and Yabuta (2002) attribute the decrease in diamondoids at the KT boundary as resulting from reduced biomass input into sediments as they were deposited.

13. Fanciful YDIH indicators, abandoned claims, and mislaid or missing evidence

A broad array of claims for evidence of some sort of ET cataclysm was presented in the early YDIH publications (e.g., Firestone and Topping, 2001; Firestone, 2002, 2009a; Firestone et al., 2006, 2007; Section 1). These publications are the foundation for the different versions of the YDIH that henceforth evolved and are currently proposed. The early claims include interpretations of geomorphic records, stratigraphic sections, and geochemical data that were speculative and sometimes contradictory. Most of this alleged evidence disappeared from the current YDIH literature with no comment, but its highly speculative nature certainly reflects on the credibility of the authors that were involved. A review of these claims is thus instructive for that reason and because it demonstrates fundamental weaknesses of the hypothesis from the outset.

13.1. Carolina Bays and High Plains playas

The Carolina Bays and the High Plains playas are depressions widespread across eastern and central North America that were invoked early in the YDIH debate to support it. The Carolina Bays are thousands of shallow elliptical to circular depressions with elevated rims scattered across the Atlantic Coastal Plain (see Brooks et al., 2010). Some YDIH proponents embraced and combined earlier ideas that the Carolina Bays are Late Pleistocene impact structures (Melton and Schriever, 1933; Sass, 1944; Eyton and Parkhurst, 1975) and that a supernova irradiated the Earth in the Late Quaternary (Brakenridge, 1981). (Firestone and Topping, 2001, p 15) claimed Carolina Bays were **"gouged out"** by a supernova shock wave (see also Firestone et al., 2006). (Firestone and Topping, 2001, p 2) speculated, **"The enormous energy released by the catastrophe at 12,500 yr B.P. could have heated the atmosphere to over 1000°C over Michigan, and the neutron flux at more northern locations would have melted considerable glacial ice. Radiation effects on plants and animals exposed to the cosmic rays would have been lethal."** Firestone and coauthors then claimed the supernova shock wave perturbed the orbit of a comet that struck Earth (Firestone et al., 2006) to explain purported impact markers present in the purported YDB of Carolina Bays (and other sites) (Kobres et al., 2007; Firestone et al., 2007, 2010a; Firestone, 2009a). Subsequently, Bunch et al. (2012) purported concentration spikes of high-temperature, siliceous SLOs and microspheres at the Blackville site in South Carolina, which is claimed to be a YDB-dated rim of a Carolina Bay. Platinum anomalies were claimed at the purported YDB of the Carolina Bays: Flamingo Bay and Johns Bay (Moore et al., 2017, fig. 3) as well as White Pond (Moore et al., 2019). Wittke et al. (2013a, fig. 2) purported concentration spikes of impact spherules and Kinzie et al. (2014, fig. 2) purported concentration spikes of nanodiamonds at the purported YDB of two Carolina Bay sites Blackville and Kimbel Bay. Additionally, Kinzie et al. (2014, p 489) claimed to identify nanodiamonds near the surface of **"glass-like carbon extracted from the YDB layer at the M33 site, the rim of a Carolina Bay in Myrtle Beach, South Carolina"** (initially studied by Firestone et al., 2007). Firestone et al. (2007, p 16016) also claim the YDB **"layer extends through at least 15 Carolina Bays"** and **"15 Carolina Bays studied contain peaks"** (p 16019) in impact markers but published no stratigraphic or geochronological data (see Table 4). Zamora (2017) promoted an earlier argument that the Bays are aligned and oriented toward the Great Lakes (see Firestone et al., 2010a, p 41), concluding that they resulted from impacts by glacial ice ejected from the Laurentide Ice sheet following an ET impact.

As discussed by Pinter et al. (2011), the major axes of the elliptical bays do not truly **"point towards the Great Lakes and Hudson Bay** [proposed impact/airburst site]**"** (Firestone et al., 2010a, p 41), but rather vary in orientation both locally and regionally (Johnson, 1942; Thom, 1970). More significantly, the Carolina Bays did not form synchronously. Recent dating shows multiple periods of bay-rim accretion through the late Quaternary with intervening intervals of erosion (see, e.g., Brooks et al., 1996, 2010; Grant et al., 1998; Rodriguez et al., 2012). In fact, Firestone (2009a, table 3) measured ^{14}C dates of various carbon forms collected from four Carolina Bays claimed to be YDB-sites, that yielded ages contradicting synchronous formation and ranging from a maximum of 6565 ± 15 ^{14}C yr BP to −755 ± 15 ^{14}C yr (in the future). Early on Firestone et al. (2007, p 16019) conceded, **"we cannot yet determine whether any Bays were or were not formed by the YD event"** and while this presently remains the case, many YDIH proponents continue to claim Carolina Bays as important YDB sites.

The smaller and generally more circular "playa" basins are scattered by the thousands across the High Plains of North America (e.g., Sabin and Holliday, 1995; Bowen et al., 2010). Their origins were long debated (Gustavson et al., 1995), but they are clearly not impact structures. Nevertheless, Firestone et al. (2006, p 216, 218) completely misstate playa chronology, indicating that they may date to ~12.9 cal ka BP (citing Holliday et al., 1996; see also Holliday et al., 2008) and therefore likely result from an impact (Table 4). To the contrary, most all dated basins were present before the YDB, filling with sediment before and after that time with no disruption. There is no evidence for playa formation at ~12.9 cal ka BP. Claiming that a paper states the opposite conclusion to make a point about purported impacts represents scientific malfeasance. Similarly misleading statements about the playas in the context of the YDIH appear in other papers as well (e.g., Firestone, 2009a).

13.2. Fullerenes with ET helium

Firestone et al. (2006, 2007) purported fullerenes containing ET helium at the YDB. Indeed, it was one of the seven primary lines of evidence Firestone et al. (2007, abstract) used to argue for **"an ET impact and associated biomass burning at ~12.9 ka."** Their SI table 4 indicates recovery of this material, but no methods are discussed. The contents of SI table 4 refers to a **"companion fullerene paper"** but no such paper is known and since 2007 no supporting evidence for fullerenes containing ET helium was published in the peer-reviewed literature. Moreover, the three coauthors that previously coauthored papers on fullerenes with ET helium never published with the YDIH proponents again and have not responded to requests for information about the subject. Fullerenes with ET helium continue to be cited as evidence by third-party review authors (e.g., Sweatman, 2021; Powell, 2022) but appear to have been abandoned by many authors of previous YDIH papers, even though they never stated so or reported any negative results.

13.3. More pseudoscience (fringe) evidence and conjecture

Additional unconfirmed and abandoned evidence includes Paleoindian chert artifacts with purportedly high-velocity particle tracks with embedded chondritic micrometeorites and isotopic anomalies in K, U and Pu (Firestone and Topping, 2001; Firestone, 2002, 2009a; Firestone et al., 2006; Firestone et al., 2010a). Also purported were iron micrometeorites and mammoth tusks with rusty pits (Baker et al., 2008), radioactive sediment, and radioactive mammoth bones and teeth (Firestone et al., 2006; Firestone et al., 2007, 2010a, 2010b). Firestone et al. (2006) claimed (p 51), **"There is some evidence suggesting that the black mat once contained dangerous heavy metals and toxins"** and pondered (p 50), "[c]**ould the toxic black mat be one answer to what had happened to them** [megafauna]**? Did the giant animals become extinct because they drank water containing high levels of algal poisons - or of toxic metals, like titanium and arsenic - or was their demise due to high levels of radioactivity?"** Firestone et al. (2006, p 342) further speculated carbon spherules **"could be algal colonies that reached great size during the period of explosive growth following the impact"** and **"scanning electron microscope (SEM) images (fig. 34.3) … reveal an apparent biological structure."** Originally, Firestone was close to the correct identification; they are mostly fungal sclerotia (see Section 12.4). Firestone et al. (2007, SI) wrote with regard to Murray Springs, **"A distinctive black mat, most likely of algal origin drapes conformably over bones of butchered mammoths"**, and with regard to Chobot, **"there is a black mat similar to other sites."** Firestone (2009a, section 6) wrote, **"the black mat was deposited after the impact and is an algal mat mixed with ash from forest fires."** In contradiction, Firestone writes of Chobot, **"We do not claim that the** [black] **mat is algal in origin** [Wittke et al., 2013a]**, nor is that a** [YDIH] **requirement"** (Wittke et al., 2013b, p E3900). Harris-Parks (2016, p 104) concluded from her detailed study of black mats, **"Contrary to previous studies, fluorescing algal colonies and charcoal are effectively absent in all of the samples, indicating that black mats did not form exclusively as algal blooms or as fires related to a meteorite impact. Rather, black mats represent a facies system dominated by organic material derived from herbaceous plants. The abundant microscopic evidence for sustained wet periods, necessary for black mat formation, is a clear indication that the localized effects of the YDC induced significant and sustained hydrologic changes to the southwest United States and High Plains. … These sediments represent naturally occurring organic-rich deposits that formed in response to changes in effective moisture, in a similar fashion both before, during and after the YDC"** (Section 6).

The most sensational pieces of evidence of a YDB-aged impact claimed by Firestone et al. (2006) were **"Five mammoth tusks display embedded magnetic particles with raised charred rims"** (p 65) and **"to embed themselves so deeply in these tusks, the particles must have been traveling very fast, maybe at supersonic speed"** (p 60). Photographic evidence for several tusks was presented with **"dark ring"** (p 55), **"large dark spots"** (p 59), **"entry craters"** (p 61), and **"large Vs-inch** [visible inch long?] **split particle embedded in the tusk"** (p 62). Firestone et al. (2006) also reported embedded metallic particles in a horn and skull of a bison. However, the bison skull and at least one mammoth tusk were later radiocarbon dated to over ten thousand years prior to the YD/GS-1 onset by Firestone (Hagstrum et al., 2010). The tusk shown in fig. 4.3 of Firestone et al. (2006) is shown in fig. 1 of Hagstrum et al. (2010). Similarly, the bison skull shown in fig. 4.4 of Firestone et al. (2006) is shown in fig. 4 of Hagstrum et al. (2010). Hagstrum et al. (2010, p 129) concluded, **"We propose the metallic particles found embedded in late Pleistocene mammoth tusks and bison skull (assuming an incorrect age** [for the bison skull dating of 26.3k ^{14}C yr BP]**) are micrometeorites from low-level airburst that occurred over Beringia sometime between 31 and 35 kyr ago. The result of these impacts likely caused the death of these seven Alaskan mammoths and one Siberian bison, as well as the overall decline in megafaunal populations observed throughout Beringia."** The mammoth tusks studied by Firestone et al. (2006) appear to have been lost. Richard Firestone (personal communication) believes one is in possession of Allen West. Allen West (personal communication) does not have it nor does he know where it is. How such a key component of the YDIH was lost is unclear.

In addition to "coherent catastrophism" and interpretations at the Göbekli Tepe archaeological site (Sections 5.2 and 7), preposterous fringe ideas continue to plague the YDIH. In a non-peer-reviewed essay, Ballard (2017) hypothesized that excavated broken mammoth bones from the U.S. Midwest are evidence of a Laurentide ice sheet impact and ice-boulder ejecta (see also Zamora, 2017) that crashed down killing mammoths across the continent and forming craters. These "craters" are well documented ice-melt landforms known as kettles. No crushed mammoth or other faunal remains are known from these or any other settings in the region. To account for claimed YDB craters in South America beyond the range of ice boulder ejecta, Jaye (2019) claimed a comet broke up during entry and delivered sufficient water-ice to cause world-wide flooding.

13.4. Mislaid Greenland ice expedition

Sweatman (2021) gives very little attention to one of the most celebrated claims by the YDIH proponents: hexagonal nanodiamonds in Greenland ice, which they claim to be from the YDB (Kurbatov et al., 2010). (See also Section 12.2 with regard to their incorrect and/or inconclusive identification of lonsdaleite.) The ice samples were collected during the summer of 2008 in a PBS-funded expedition for the filming of an episode the series NOVA, "Megabeasts Sudden Death". Within a few months, they purported high concentrations of lonsdaleite nanodiamonds. In August 2009, members of the group returned to the location where samples were collected one year earlier (Heidari, 2010; Allen West email to Mark Boslough, September 25, 2010). They collected and processed samples seeking more evidence but never published any further findings. Thus, it appears that the report of hexagonal nanodiamonds in Greenland Ice by Kurbatov et al. (2010) has not been replicated despite at least one attempt. After NOVA WGBH senior management learned about problems with the hypothesis and data discrepancies (Dalton, 2011), they removed the access to the program from NOVA Online (Evan Hadingham email to Mark Boslough, June 15, 2011). A link to the show with a transcript (PBS NOVA, 2009) is all that is now available.

13.5. Mislaid contrary evidence

Proponents of the YDIH fail to report negative or conflicting results, for example, dating of Carolina Bays (Section 13.1), fullerenes with ET helium (Section 13.2), 'nanodiamonds' in Greenland ice (Section 13.4), and 'nanodiamonds' at Bull Creek (Section 12.6). More troublesome is that YDIH proponents continue to report original results as valid even after failed attempts to reproduce those results. This failure to report negative results has the unfortunate effect of giving the impression that there is more supporting evidence for the hypothesis than there actually is, as the apparently abandoned claims and irreproducible results continue to be cited. For instance, Sweatman (2021) in his comprehensive review of the YDIH acknowledged only one contrary result, "**Apart from this one research paper** [Holliday et al., 2016]**, the overwhelming consensus of the evidence from scores of YDB sites across nearly half the world's surface is that a major cosmic impact occurred around 10,785 ± 50 BP (2 sd)"** (an unusual way to refer to the YDB, as noted in Section 5). Other contradictory data abound. Not the least of which is the work reported from Bull Creek, Oklahoma (Bement et al., 2014) (Section 5.5, Table 5), which is repeatedly misrepresented in the YDIH literature. Further, Sweatman (2021, p 20) argues "**Even work purported to contradict the impact hypothesis, when examined closely, actually supports it (Gill et al., 2009; Haynes et al., 2010; MacGregor et al., 2019; Pigati et al., 2012; Sun et al., 2020; van Hoesel et al., 2012).**" This remark is a gross overstatement and aspects of it are factually incorrect. As indicated in Table 8, discussions of these various papers are convoluted and rife with "if" and "might be" statements or simple conjecture to explain away inconvenient data that contradict the YDIH.

13.6. Lack of transparency in YDIH evidence

Publications in support of the YDIH often claim experimental results/interpretations from data that displays inconsistencies or is based on data that is not published. For example, fig. 1 of Firestone et al. (2007) is problematic in several aspects. The identification of the YDB in the sediment profiles is ambiguous: at Chobot no dates are given; at Morley and Gainey only one date is given for the YDB: 13 ka and 12.4 ka (respectively); and at Topper the YDB is undated and lying above 15.2 ka. Some of the data points at any given locality do not line up horizontally for each of the studied markers, suggesting several different vertical samplings were collected or indicates the data are not accurately plotted. See also ENDNOTE 7. Data supporting the claim of fullerenes with extraterrestrial helium have never been published (see Section 12.2) and never made available.

Kurbatov et al. (2010) used confused units to report nanodiamond abundances in Greenland ice, writing (p 752), **"contain total ND abundances of 5–50 parts per billion (ppb), equivalent to 1–10 × 10^9 per cm of ice (Fig. 5; Table 2)."** It is unclear exactly what **"per cm"** represents since concentrations must be normalized to volumetric units. Their fig. 5 plots concentrations as L^{-1} while their table 2 lists the same values as mL^{-1}. However, see Sections 12.6 and 13.4 regarding the flawed experimental methodology and irreproducibility of those results. Raw data supporting the interpretations presented by Kurbatov et al. have never been available.

Further, methodologies used for data collection are misstated/obscured (e.g., nanodiamond concentrations – see Section 12.6), inadequately stated, vary (spherule collection – see Section 10), or missing entirely (e.g., fullerenes with ET helium – see Section 13.2). As enumerated throughout this review, YDIH publications contain misstatements of facts and assumptions stated as facts. This naturally induces skepticism of results derived from data not presented, containing inconsistencies, or collected with ambiguous methodologies. Nearly all requests to YDIH proponents for clarification of data inconsistencies and requests for specimens (e.g. nanodiamond isolates from Greenland ice) for independent study and verification of results have either gone unfulfilled or ignored.

13.7. Conspicuously missing impact evidence

Several types of the mineralogical 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). In addition to the lack of any identified impact structures (craters) dating to the YD/GS-1 onset, conspicuously missing in YDB sediments are well-recognized and established impact markers such as shatter cones, tektites, shocked minerals, and meteoritic fragments of an impactor (e.g., see French and Koeberl, 2010; Reimold et al., 2014) (Section 8). van Hoesel et al. (2015) specifically searched for shocked quartz in YDB-dated sediments at eleven sites including the black mat at Murray Springs, four Usselo soils, and three Finow soils.

As discussed, some YDIH impact proponents consider the black mat and the Usselo soil complex to be impact debris or a burned layer (Mahaney et al., 2013, 2022; Wolbach et al., 2018b, and Israde-Alcántara et al., 2018) (Sections 5.6 and 6). Among all the sites, van Hoesel et al. (2015) found only one shocked quartz grain from an Usselo soil. The grain exhibited amorphous lamellae known as PDFs that were devitrified (i.e., healed) by post-impact alteration. van Hoesel et al. (2015, p 495) concluded, **"Although healing can occur immediately following the impact, healed PDFs are most common in older impact material (i.e., several million years)…. This suggests that the shocked grain might be older than the Late-Glacial period. Shocked quartz grains can be eroded from older craters or distal ejecta layers and incorporated into the sediment… and the rounded shape of the grain suggests that it has been transported either prior to, or after impact."** The only other claim of shocked quartz in purported YDB sediments is Mahaney et al. (2010a). Mahaney et al. (2010a, p 48) purported PDFs in quartz from a **"'black mat' candidate"** layer in the Venezuelan Andes. However, the identification of PDFs was not convincing (van Hoesel et al., 2015). Furthermore, Mahaney et al. (2010b, p 39) later concluded in a follow up study to that site, **"we have detected no irrefutable pdfs, shock-melted quartz, iridium or nanodiamonds in the samples analyzed thus far."** No shatter cones or melt glasses have been purported in YDB sediments. The early claims of micrometeorites have never been confirmed and are apparently abandoned by YDIH proponents (see Section 13.3).

As pointed out by Holliday et al. (2020), an extraterrestrial impact event hypothesized to cause significant alterations to climate, flora, and fauna at a near global-scale certainly must have been recorded in paleobiological, geomorphologic, and stratigraphic records. However, evidence of abrupt changes emerging at the YDB are not observed. Issues of changes in human populations, faunal extinction, and climate are addressed in Sections 3.1–3.3. Wolbach et al. (2020, p 100) reject this criticism, responding **"On the contrary, it is well known that widespread major changes occurred at the YD onset…"** They go on to note ice-dam failure on proglacial lakes, continent-wide hydrological changes, and destabilization of ice-sheet margins at the YD onset across North America. But they fail to note the rest of the comment by Holliday et al. (2020, p 90) "**The literature on YDC (and preceding and superseding) conditions in unglaciated North America south of the continental ice sheets is extensive… The landscape of North America from the post-LGM terminal Pleistocene into the early Holocene underwent rapid reorganization because of warming and related climate changes; melting of glaciers; rise in sea level; and changes in alluvial, eolian, and lacustrine geomorphic systems and in plant and animal communities. However, the magnitude, direction, and pace of change in each of these systems varied in time across the continent.**" A few examples are in order.

Meltzer and Holliday (2010), Holliday and Miller (2013) and papers in Orme (2002), Easterbrook (2003), Gillespie et al. (2004), Straus and

Goebel (2011), Bousman and Vierra (2012), and Eren (2012), among others, document a broad array of biological and geomorphic changes through the post-LGM into the early Holocene. Changes, often rapid and dramatic, were the norm. Many rivers across the continent were undergoing dramatic changes in discharge, channel form, and mode (incision, aggradation, or equilibrium) before and after the YDB (Holliday and Miller, 2013). Both the Laurentide and Cordilleran Ice sheets were quite dynamic following the end of the LGM, retreating and re-advancing (including surges) over the course of thousands of years (e.g., Dalton et al., 2020; papers in Ehlers et al., 2011). Pro-glacial lakes appeared and disappeared (e.g., Teller, 2004; Fisher, 2020; papers in Karrow and Calkin, 1985; Teller and Kehew, 1994, and associated papers in the same volume of *Quaternary Science Reviews*). Failures of dams along these lakes and related mega-floods are well documented through the post-LGM into the early Holocene (Baker, 2020) beginning with the initial phases of deglaciation (e.g., Clayton and Knox, 2008; Curry et al., 2020).

Wolbach et al. (2020) and Teller et al. (2020) note the coincidence of catastrophic outflow of Glacial Lake Agassiz just after the YDB, but one of the largest floods on the Mississippi River resulted from drainage of Glacial Lake Agassiz just before ~8.2 ka (Fisher, 2020), triggering the 8.2 ka event. The repeated collapse and re-formation of an ice dam on glacial-lake Missoula produced the largest floods documented in the Earth's geologic record, dated between ~17,500 and ~ 14,500 cal yrs. BP (Baker et al., 2016). As noted by Wolbach et al. catastrophic drainage into the Arctic or the North Atlantic could be drivers of YDC cooling but considering the ubiquity of changes in ice sheets and release of large amounts of water throughout post-glacial time, there is no reason to necessarily invoke an ET explanation for one particular mega-flood.

14. Same specimens and specimen splits studied by different groups

One revealing aspect of the YDIH debate is that contradictory results are obtained when different groups study the same specimens or splits of specimens. In addition to the data published by Surovell et al. (2009), strongly criticized by YDIH proponents (discussed in Section 10 and ENDNOTES **14, 15**), such studies have been conducted on specimens from Murray Springs, Howard Bay, Blackwater Draw, Arlington Canyon, and Lubbock Lake (Sections 10 and 12.6).

In the purported YDB at Murray Springs, Firestone et al. (2007) claimed a spike in the concentration of magnetic grains and charcoal as well as an Ir and radiation anomaly among other claimed indicators. Haynes et al. (2010, p 4010) wrote, **"**[o]**n three occasions Haynes and Ballenger escorted Allen West and associates… to the Murray Springs site and collected sediment samples at, above, and below the LYDB** [lower YDB] **at Profile B and Trench 22 North (Fig 1)."** The objective of Haynes et al., was an **"attempt to reproduce some of their** [Firestone et al., 2007] **most readily tested findings. Where they collected, we collected, and, therefore, we have essentially identical samples"** (p 4010). Haynes et al. (2010, p 4010) reported, **"Magnetic microspherules have terrestrial origins but also occur as cosmic dust particles. We failed to find iridium or radiation anomalies. The evidence for massive biomass burning at Murray Springs is addressed and found to be lacking."** Paquay et al. (2009) also attempted to reproduce results of YDIH proponents and wrote (p 1 SI), **"Allen West provided samples of the black mat layer from Howard Bay, NC (level HB-11d2) and Blackwater Draw (NM) (levels BW-DT, D/C and BW-B/A), similar to those measured in Firestone et al., [(2007)]"** and **"Dolores Hill provided the Murray Springs samples that are splits of those used in the Firestone et al., [(2007)] study."** Paquay et al. (2009, p 21505) reported, **"our results do not reproduce the previously reported elevated Ir concentrations. Second, ^{187}Os/^{188}Os isotopic ratios in the sediment layers investigated are similar to average crustal values, indicating the absence of a significant meteoritic Os contribution to these sediments."**

To refute contradictory results at Arlington Canyon (Scott et al., 2010, 2017; Daulton et al., 2010, 2017a, 2017b; Sections 9.3 and 12.5), YDIH proponents incorrectly argued at length that the wrong specimens were sampled there (Section 4.1). Impact proponents involved in a blind study of a specimen split at Lubbock Lake (Johnson, 2012; Holliday et al., 2016) never acknowledged the contradictory nature of those results with the YDIH. These contradictory results were acknowledged, but then dismissed, in a summary review of Holliday et al. (2016) authored by an independent YDIH proponent M. B. Sweatman. Sweatman (2021, p 19–22) wrote, **"the nanodiamond and magnetic microspherule evidence from** [Kennett's] **lab is new and, given the strength of the impact hypothesis, we can expect it to be consistent with other YDB sites** [Table 2]. **Instead, an abundance of nanodiamonds and magnetic spherules is reported at a level…corresponding…to near the end of the Younger Dryas period, and not its onset. These results can be considered inconsistent with the impact hypothesis. Considering the uniqueness of these results, this work should be repeated, *taking care to make direct radiocarbon measurements of the sediments rather than relying on the similarity of the stratigraphy to other sites*"** (emphasis added). However, these comments again demonstrate a common problem in the YDIH literature. Sweatman was not paying attention to what is in the 2016 paper and apparently not aware of the literature on the locality. As clearly stated by Holliday et al. (2016), the zone in question post-dates the YD/GS-1 and the dating is based on unambiguous stratigraphy, numerical age control, and archaeology directly from the site, documented in widely published literature (e.g., Holliday, 1985, 1997; Holliday et al., 1983, 1985; Johnson, 1987).

15. Unparalleled promotion of the YDIH outside of scientific literature

The first YDIH paper to attract any serious notice in the scientific community was Firestone et al. (2007) due to the considerable media coverage of the 2007 AGU symposium. Due to the sensational nature of the YDIH, the news media continued to provide coverage of subsequent claims of YDIH proponents. Furthering this attention, the principal YDIH authors created and ran websites to promote the YDIH and other fringe science outside the constraint of peer review, raise money, and engage in personal attacks on skeptics. Martin Sweatman in preparing his review of the YDIH (Sweatman, 2021), which has the appearance of being independent, interacted with webmasters of one of these sites and used their resources. Sweatman (2021) acknowledgment states **"I am grateful … to Marc Young and George Howard for their assistance with the literature search."** George Howard is a coauthor of Firestone et al. (2007) as well as many other YDIH papers. He is director/cofounder of the Cosmic Research Group (CRG) and runs the website Cosmic Tusk. A blog on that website states (Oct 21, 2019), **"Martin** [Sweatman] **used the** [Cosmic] **Tusk's Complete YDIH bibliography to stage his investigation, which is gratifying, and well timed. Just this weekend Marc Young and I updated the bibliography and I created a dedicated page for it, which will be more prominently featured on the Cosmic Tusk in coming days."**

Popular press books have been written to promote the YDIH by scientists/academics. However, disclosure of conflict of interest (either potential or the appearance of) is lacking in YDIH publications in the scientific literature. Firestone et al. (2007) stated, **"The authors declare no conflict of interest"** despite the fact the paper furthers and promotes the main subject of an earlier for-profit book *The Cycle of Cosmic Catastrophes…* (Firestone et al., 2006). Sweatman (2021) also stated, **"The authors declare that they have no known competing financial interests or personal relationships that could have appeared to influence the work reported in this paper"** despite his YDIH-supporting book, *Prehistory Decoded* (Sweatman, 2019). The week that a preprint of Sweatman (2021) was posted on Sweatman's website, Cosmic Tusk published an announcement regarding Sweatman, describing the author as **"indefatigable genius and digital friend of the Tusk."** The announcement included several links to *Prehistory Decoded*, including the Amazon page from which it can be purchased. When the book was first published, Cosmic Tusk posted a review of the book. In the YDIH review paper by Powell (2022), Powell fails to disclose his self-published book *Deadly Voyager* (Powell, 2020), which promotes the YDIH. The Cosmic Tusk announced the publication of Powell (2022) the week it came out, providing a link to a site from which *Deadly Voyager* can be purchased. Non-scientists (e.g., Collins, 2014; Hancock, 2015 and others) have also written popular press books that promote the sensational nature of the YDIH and often the concept of coherent catastrophism (see Section 7). These books blur peer-reviewed scientific literature together with imaginative speculation and influence the perception of the news media. As an example, while the age of the Hiawatha crater remained undetermined (see Section 8.1), the journal *Science* listed its discovery as one of the runner ups to the Breakthrough of the Year for 2018 and that it would **"vindicate proponents of the controversial Younger Dryas impact theory"** (Hand, 2018, p 1346).

16. Discussion

Some YDIH impact proponents link the debate over the YDIH to the well-known debate in geology over catastrophism vs. uniformitarianism (e.g., Powell, 2014), casting critics in a fossilized uniformitarian stance (e.g., Powell, 2020; Mahaney et al., 2022). **"The progress of science has sometimes been unjustifiably delayed by the premature rejection of a hypothesis for which substantial evidence existed and which later achieved consensus.…** [t]**he Younger Dryas Impact Hypothesis (YDIH) is a twenty-first century case"** (Powell, 2022, abstract). This is not correct. Previous hypotheses that were later found correct were initially not accepted because of the lack of "substantial evidence." They were subsequently accepted when unequivocal data reproduced by other scientists emerged, which is a key point Powell makes in his own publications. Debates over continental drift (along with the orbital theory of climate change) dragged on for decades until technology (e.g., numerical dating methods and deep-ocean drilling) provided the requisite data necessary to settle the debate. The K/Pg extinction debate was settled much more quickly because reproducible data appeared more quickly. The YDIH debate continues precisely because, as described and discussed in this review, fundamental flaws in the original hypothesis and the data used to support it persist.

The implication of Powell's arguments is a sort of hindcasting. Because a hypothesis was initially rejected and then eventually proven, its truth was obvious at the outset. The initial critics were somehow narrow-minded and unreasonable, never mind the absence of solid data, that the basic tenets that science is supposed to be skeptical, and the burden of proof is on those attempting to change paradigms. Powell (2022) appears to suggest that potentially paradigm-shifting hypotheses will eventually fail to be rejected, and move into the class of theories, and thus so is the case with the YDIH. While science has leaped forward through paradigm shifts, those shifts are somewhat rare and just because a hypothesis might shift a paradigm does not mean that it is true.

This review and prior publications show that few YDIH sites have unambiguously dated YDB layers, none of the purported impact indicators can be uniquely related to an impact, and no data show that the suites of claimed indicators are unique to the YDB. Nevertheless, Sweatman (2021) refers to evidence for an impact across multiple or four continents and concludes (p 17, 19) that "**No YDB site has yet been found to be obviously inconsistent with a synchronous event."** This misuse of the concept of statistical consistency – that as more information accrues, an estimator gets better – is obviously and grossly in error. Subsequently, Powell (2022) discusses the YDIH in the context of **"Premature rejection in science."** He states (abstract) **"By today… many independent studies have reproduced that evidence** [supporting the YDIH] **at dozens of YD sites."** Most of these studies are not independent, and the claims of Sweatman and Powell are based on dating and purported impact indicators that we show are spurious.

Sweatman (2021, p 20) further claims, "**Another common strategy used by opponents has been to make misleading spurious and fallacious arguments."** While we also claim YDIH proponents make spurious and fallacious arguments, in this review as well as in others (e.g., Meltzer et al., 2014; Holliday et al., 2014, 2020) ample details are provided to justify those claims, which is not the case for most YDIH proponent claims. ENDNOTE **16, 17.**

Our review and discussion show that little solid evidence to test the YDIH (much less support it) has been forthcoming. The YDIH is a concept originating from earlier baseless ideas that merged together and morphed into its present disjointed form. An impact event is central to the YDIH; yet conflicting scenarios are required to explain the incompatible impact markers that have been reported (Section 7). While its present form has been out for 15 years, it differs from publication to publication and there is yet no comprehensive statement of what the hypothesis is nor any comprehensive statement dealing with the many contradictions in the data offered to support the hypothesis nor any attempts to deal with most of the critiques of the hypothesis and its many ramifications. Sweatman (2021) and Powell (2022) in their summaries seem unaware of the critical and fundamental importance (primacy) of skepticism in science. Rather than simply assert that critiques and contradictory data were dealt with (or should be ignored), they need to be fully addressed in peer-reviewed literature.

Claims of broadly "consistent" or "not inconsistent" data, common in the YDIH literature (Table 2), provide weak endorsement of the YDIH. Clearly dating a specific sample zone to the YDB requires more than statistical consistency, as correctly argued (and then ignored) by YDIH proponents (Kennett et al., 2008a). Accurate, high-precision dating is required as is clear stratigraphic context. An irony in the debate over numerical age control is that key papers that challenge fundamental assumptions in the YDIH (the age of the Clovis occupation [Waters et al., 2020] and dating megafauna extinctions [O'Keefe et al., 2023]) employ the radiocarbon dating methods and standards set out by Kennett et al. (2008a, b), but no papers in support of the YDIH do so. Modeled age ranges for sample zones of the presumed YDB at nine sites with standard deviations (at 68% confidence) far in excess of 300 years are presented by Kennett et al. (2015a). Setting aside the many problems of accuracy of the initial dating (enumerated by Meltzer et al. [2014], and Holliday et al. [2014, 2020], and dismissed with little to no discussion by Kennett et al. [2015a], Wolbach et al. [2018a, 2018b, 2020], Sweatman [2021], and Powell [2020, 2022]), that means that the age of the sample zone at those sites is somewhere in the range of >600 years at 68% confidence and > 1200 years at 95% confidence. The dating is consistent with the notion that sample zones *could* be of YDB age, but Kennett et al. (2015a) and Sweatman (2021) then assume that the sample zones *must* be of YDB age. That is a logical fallacy and a misuse of the statistical concept of a standard deviation, because they could also be consistent with a different age, or with no common age at all. For example, dating of the purported impact layer at the Melrose site (12,255 ± 2405 cal yr BP, Kennett et al., 2015a, fig. 2; based on questionable age-modelling results, Meltzer et al., 2014; ENDNOTE **6**) is consistent with the YDIH but also consistent with the ages of most of the Pt spikes of Moore et al. (2017). Subsequent OSL dating of the inferred YDB with 'nanodiamonds' at Melrose yielded a date with an age range of 13,547 to 9855 yr BP (Kinzie et al., 2014, SI App B), which is similarly "consistent."

The dating issues alone render the YDIH more than problematic. For example, among sites with purported impact indicators: 11 have low precision dates (SDs >300 years, Kennett et al., 2015a), 13 have no dates (9 from Kennett et al., 2015a; 2 from Wu et al., 2013; 2 from Moore et al., 2017), at least 28 (7 of the original 10 from Firestone et al., 2007, along with 15 Carolina Bays mentioned by Firestone et al., 2007; 3 from Moore et al., 2017; 3 from Israde-Alcántara et al., 2018), have no clear, direct association with the YDB (including the majority of all Usselo/Finow dates; Kaiser et al., 2009), and 2 have dated layers of purported impact indicators older or younger than the YDB (Bement et al., 2014; Holliday et al., 2016).[6] These considerations strongly suggest that the "indicators" have nothing to do with ET processes. This is even more likely given that post-glacial climate reversals were not unique to the last glacial-interglacial transition (e.g., Carlson, 2008; Cheng et al., 2009, 2020; Martrat et al., 2014) (Section 3.3).

Pino et al. (2019, fig. 12) present a map of 53 sites across the globe with claimed evidence for a YDB impact but provide no listing of the sites (the same map is used by Sweatman, 2021, fig. 2, and by Thackeray et al., 2019, fig. 1, likewise with no identification of sites). We identify 52 sites and two regions (following Firestone et al., 2006, 2007; Wittke et al., 2013a; Kinzie et al., 2014; Andronikov et al., 2016a; Moore et al., 2017; Pino et al., 2019; Sweatman, 2021) with claimed dated evidence for an impact at ~12.9 cal ka BP (Table 4). Almost none meet the standards of accuracy and precision in numerical age control established by YDIH proponents (Kennett et al., 2008b). Of these 50 sites, in our view, only Daisey Cave, Lake Hind, Sheriden Cave, Pilauco, and Hall's Cave (including possibly Blackwater Draw, Murray Springs, and Tocuila) have reasonably accurate as well as reasonably precise numerical age control in secure stratigraphic context, although none meet the standards set by Kennett et al. (2008b). While Hall's Cave is the best dated and has the best resolved stratigraphy, most results purported there in support of the YDIH are presumably abandoned (or not reproducible) and inconsistent with YDIH (Table 4).

Multiple sites have clear evidence directly contradicting key assertions of the YDIH (Table 4). At the Lubbock Lake site, lab data were generated by both Surovell et al. (2009) and by J. Kennett (in Holliday et al., 2016) as a blind test. Neither study produced magnetic microspherules at the YDB, but Kennett's lab claimed significant spikes in both magnetic spherules and 'nanodiamonds' (however, see Section 12.6) from an early Holocene level (Sections 10 and 12.6; ENDNOTE **15**). Three sites from northern Mexico (Lakes Acambay, Chapala, and Cuitzeo) have microspherule peaks that do not appear to date to the YDB.

The data from these sites demonstrate that the layer of purported abundance spikes of claimed impact markers cannot be used to define the location of the YDB as is often the case in many YDIH papers (Table 2). Similarly, the argument that a sample zone could be YDB so therefore must be YDB, essentially wishful thinking, is clearly untenable but is repeated in the YDIH literature and used to support claims of global synchroneity (e.g., Kennett et al., 2015a, Sweatman, 2021, p 3,5,16,19, fig. 2) (Table 2; see also Holliday et al., 2020, table 5). Carried to its logical end, no more field work or laboratory analyses are required in scientific debate.

A number of microscopic or geochemical indicators of an impact and resultant wildfire have been proposed (magnetic grains with iridium, magnetic microspherules, charcoal, soot, carbon microspherules/glass-like carbon containing 'nanodiamonds', and fullerenes with ET helium), but none are shown to be unequivocal indicators. Furthermore, the identification and quantification of a number of them by YDIH proponents have been called into question. Nevertheless, this suite of indicators subsequently was proposed as unequivocal evidence for a YDB impact and otherwise unknown in the late Quaternary stratigraphic record. But this assertion has yet to be documented. Indeed, there are few sites with direct high-precision dating of the YDB along with the suite of "indicators" while data from a number of sites (Lubbock Lake, Bull Creek, Gainey, those discussed by Pigati et al., 2012; and as discussed above, likely some listed by Firestone et al., 2007; Wittke et al., 2013a, 2013c; Kinzie et al., 2014 and Moore et al., 2017) have purported "indicators" dated to zones other than the YDB. Only Lubbock Lake is acknowledged by Sweatman (2021).

These issues highlight some of the reasons for our frustration in dealing with the YDIH and results in some of our strong wording. We have repeatedly offered data and interpretations of data that challenge the YDIH yet most of these are misstated, ignored, or simply declared as wrong, without evidence, and errors of fact are offered instead. Further, claims that critiques were dealt with in detail are demonstrably baseless (Table 1).

17. Conclusions

The YDIH evolved directly from pseudoscience. As such, the initial publication in scientific literature was seriously plagued by poorly documented interpretations and baseless assertions. As outlined in this paper and in other publications, a broad array of serious flaws persists in the YDIH:

A. The YDIH attempts to solve assumed problems that do not exist.

1) The YD/GS-1 in its climostratigraphic sense, i.e., a 1000-yr plus cool interval during the LGIT, is not unique, either throughout the last glacial cycle, or the last 0.5 Ma, and therefore does not require a singular explanation.

2) The idea that the Clovis archaeological record was terminated by some sort of environmental catastrophe is baseless (Section 3.1) and contradicts documented age range of Clovis artifact styles beyond 12.9 ka (Section 5.7).

3) Extinctions of individual species of megafauna varied in space and time around the world and cannot be explained by a single event and environmental changes (Section 3.2). Points 1 and 2 show that the YDIH attempts to solve problems that do not exist.

B. Most sites claimed to support the YDID have poor numerical age control.

4) Impact proponents (Kennett et al., 2008b, p E107) correctly argue that in the YDIH debate, **"Only ^{14}C dates with measurement precisions <100 years, and preferably <60 years, should be used because larger error margins blur probability distributions… Only bone dates processed with modern techniques** [e.g., XAD… or ultrafiltration…] **are valid because of the catastrophic consequences of poor chemical preparation… Stratigraphic associations between radiocarbon dates and cultural residues need to be demonstrated…"** In addition, the dating should be clearly linked to the claimed YDB. No sites used to support the YDIH meet these standards (Section 5).

C. Purported Evidence for a YDB impact is not unique to impacts:

5) Purported impact indicators have not been shown to be uniquely associated with an impact and the claim that a suite of proclaimed indicators is unique to the YDB within the context of the past ~110,000 years is not substantiated with data. Furthermore, the suite of proclaimed indicators require conflicting impact scenarios in order to explain their presence (Section 7).

6) Very few sites with accurate and high precision dating of the YDB are shown to have high levels of any of the purported indicators at the YDB. Perhaps less than a dozen sites out of scores of sites in the YDIH literature have the requisite high-precision dating of the YDB in clear stratigraphic association with claimed indicators (Section 5).

7) The Usselo and Finow soils of Northwest Europe are not unique to the YDB but represent biologic and pedogenic activity spanning the Bølling-Allerød Chronozone into the early Holocene (Section 5.6).

8) So-called "black mats" are not unique to the YDB or YDC and do not represent a synchronous stratigraphic marker horizon that spans one or more continents nor are similar soils or sediments unique to the YD/GS-1 (Section 6).

9) There is no viable evidence for synchronous wildfires across one or more continents at any time including at the onset of the YD/GS-1, and much of the ice-core geochemical or sedimentary charcoal data have been misinterpreted (See Section 9).

10) Many of the purported impact indicators: cannot be used to test the YDIH (e.g., published abundances of carbon spherules/elongates, glassy carbon, and microcharcoal); were misidentified (e.g., 'nanodiamonds'; carbon spherules/elongates; glassy carbon); were

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[6] Four sites from Moore et al. (2017) have low precision dates, 3 of which are not directly associated with a claimed YDB.

irreproducible (e.g., fullerenes; concentration spikes in 'nanodiamonds', carbon spherules/elongates, microspherules); were from sediments/ice with poor age control (e.g., all of them); or are explained by contradictory impact scenarios (Sections 5.5, 7, 10, 11, 12 and 13).

11) More generally, an issue not confronted by YDIH proponents, is that if the Earth was subjected to some sort of geologically-recent terrestrial, then why is the primary evidence presented in support of an impact mainly at the geochemical or microparticulate level (and inconclusive). No obvious evidence in geomorphic or paleobotanical records (e.g., Meltzer and Holliday, 2010; Holliday and Miller, 2013; papers in Straus and Goebel, 2011, and Eren, 2012) supports the YDIH and the paleontological evidence is weak, at best. Those publications and others document a wide range of changes in these systems through the Bølling-Allerød and Younger Dryas chronozones well into the Holocene. If there was an ET impact, it had no terrestrial consequences (Sections 5.6, 6 and 13.7).

These conclusions and the data on which they are based argue that testing of the YDIH has failed, repeatedly. To paraphrase Sweatman (2021, p 20), **"Mistakes like these, and those above, ultimately lead to a loss of confidence in the objectivity of impact hypothesis"** proponents. The burden of proof is on the proponents of the YDIH to change paradigms regarding drivers of climate change or exceptional explanations for extinctions or human activity.

We await a full summary discussion that offers a coherent hypothesis and deals with the many contradictions that have been fully outlined since 2008. We urge the YDIH proponents to follow the "method of multiple working hypotheses" (Chamberlin, 1890), which has well served scientific research for over a century. Many YDIH papers follow a standard template: materials from a new or revisited site are examined, a few samples from close to the estimated YDB location are examined (often using the samples themselves to define the YDB), a suite of purported impact-related indicators (not recognized by the general impact community) are reported, and the hypothesis confirmed. No attempt is made to test alternative hypotheses, or to significantly expand the temporal scope of the analysis to include detailed sampling of stratigraphic units above or below the assumed YDB. This narrow, single-hypothesis-focused design can never discredit or falsify the hypothesis, but instead leads to impression of more-and-more support for the YDIH.

In this review, we document many (but not all) instances where research papers and reviews published by YDIH proponents have misstatements of fact that are often repeated in subsequent publications. Over time, these misstatements can be misconstrued as actual evidence that is then perpetuated in the literature providing false support to hypotheses, which ultimately undermines the scientific process and its credibility. We implore future investigators on both sides of the YDIH issue to do their homework. Based on the many misstatements of fact documented here, many YDIH proponents do not pay attention to the scientific geological and archaeological literature cited in the papers they read and write. Assessment of the YDIH should include some familiarity with late Quaternary paleoclimatology, terrestrial stratigraphy, geomorphology, the soils of the study sites including their settings, geomorphic mechanisms, soil-forming processes, Quaternary dating methods, as well as, ideally, some familiarity with the local and regional archaeological record of terminal Pleistocene foragers. Researchers familiar with impact physics, diagnostic evidence for impacts in the geological record, as well as geochemistry and mineralogy in Earth surface contexts are also required. Furthermore, in the case of the YDIH, the peer review process has obviously failed to prevent publication of misstatements of fact. While novel potentially paradigm-shifting ideas should not be suppressed, they must be still be held to the same scientific standards as all other investigations.

To confirm the YDIH and overturn current paradigms regarding environmental change, extinctions, and archaeology from the B-A/GI-1 to the YD/GS-1, proponents of the YDIH must provide unambiguous evidence from unambiguous stratigraphic context unambiguously dated using high precision numerical age control from multiple sites across at least North America if not other continents. So far none of those requirements is met.

Powell (2022, p 2) asks **"how scientists can so thoroughly reject a hypothesis, even write its requiem, only to have it emerge in little more than a decade strengthened…"**. The first widely read paper on the YDIH (Firestone et al., 2007) had a range of serious weaknesses, reviewed here and by others. Although the 2007 paper did not contain all the pseudoscience of its predecessors, it was nevertheless plagued with poor age control, faulty assumptions, and misstatements of fact. As we detail here, the YDIH was subjected to continued skeptical scrutiny, a hallmark of the scientific method (particularly for paradigm-changing hypotheses). Since 2007 (and before regarding the earliest iterations of the YDIH), critics have pointed out continuing weaknesses, such as misstatements of fact and poor age control, which plagued the YDIH and was rarely confronted by YDIH proponents. The YDIH is based on questionable impact indicators from sites with problematic age control. The result is continued and enhanced skepticism.

Endnotes

ENDNOTE **1** Wolbach et al. (2020, p 99-100) state that the claim "[n] **o stratigraphic or chronologic data exist to indicate a post-Clovis population decline"** by Holliday et al. (2020) is false and further claim that Holliday et al. (2020) neglect **"to mention Anderson et al. (2011), who report widespread evidence for post-Clovis population decline."** Holliday et al. (2020, p 88) clearly critique Anderson et al. (2011). The paper is seriously flawed. There is virtually no numerical age control or stratigraphic context for the southeastern point styles or archaeological sites mentioned by Anderson et al. (2011). Topper is the one exception but the sand layer with the Clovis artifacts (~1 m thick and spanning the Holocene) was heavily bioturbated (Miller, 2010) (Section 5.7). Moreover, use of summed probability analyses to link frequencies of radiocarbon dates as indicators of population density over broad regions is significantly hampered by issues of local site and sample preservation through space and time. It is also hampered by "radiocarbon plateaus" for the time just before, during and just after the YDC (Section 5.1). Dated stratified archaeological records show no population decline (Holliday and Meltzer, 2010). Further, in an analysis of projectile point technology in the Southeast U.S. through the YDC, Barlow and Miller (2022, abstract) conclude that **"there are no abrupt technological changes coeval with the Younger Dryas onset."**

Wolbach et al. (2020, p 100) further claim **"Holliday et al. (2020) also neglect to mention LeCompte et al. (2012), who reported the results of Al Goodyear's experiment to examine sediment samples taken from directly above and beneath Clovis chert waste flakes (debitage), which are the youngest Clovis artifacts in the Topper quarry in South Carolina."** No numerical age control documents the sequence of events that resulted in the Clovis occupation, the accumulation of purported impact markers or the depositional processes that buried the two. The authors simply assume that the "indicators" and the Clovis archaeology are contemporaneous, reflecting circular reasoning. As noted above, the zone with and above the Clovis archaeology is mixed.

ENDNOTE **2** Paleoindian occupations around the Blackwater Draw site were numerous but geographically isolated (Hester, 1972; Holliday, 1997) and, therefore, few localities contained a stacked sequence of archaeological features. A more fundamental issue is that single or a few occupations are the norm at the majority of Paleoindian sites with well-dated, stratified records (Holliday and Meltzer, 2010). Some sites have only a Clovis occupation, while others have only a Folsom or a single late Paleoindian occupation. The absence of occupations before or after is not an indication of catastrophe. Claiming otherwise demonstrates not only a fundamental misunderstanding of the geoarchaeological record, but also of the process of science in any discipline that uses sampling as a technique for data collection.

ENDNOTE **3** Other comments by YDIH proponents are similarly unambiguous in making an impact-extinction link. Kennett et al. (2009b, p 12623-12624) further state: **"The connection of impact-to-extinction and the presence of several of the same impact proxies in this widespread 12,900-year-old sedimentary layer provide an empirical basis for the Younger Dryas boundary (YDB) impact hypothesis. Massive North American animal extinctions could have resulted from the direct effects of these airbursts/impacts (shock-waves, heat, wildfires) and subsequent cascading ecological changes…"** Subsequently, Moore et al. (2017, p 2) state **"The Younger Dryas impact hypothesis proposed a causal link between a cosmic impact event and a) the onset of the YD climate cooling episode at ~12,800 calendar years BP, b) a peak in continental-scale biomass burning, c) extinction of more than 35 genera of North American Pleistocene megafauna."**

ENDNOTE **4** Wolbach et al. (2020, p 97) asserts that claims by Holliday et al. (2020) of selective sampling of YDIH sections by YDIH proponents are inaccurate. Table 3 in the current paper (updating table 4 in Holliday et al., 2020 listing "well-dated YDB Sites" of Wolbach et al., 2018a, SI table A1) clearly supports exactly what was stated. Out of 48 sites, 35 have 20 samples or less (21 have 10 samples or less) and 7 have no information on sampling. ENDNOTE **10** Specifically, table 1 in the reply by Wolbach et al. (2020) further makes the point for us. Of the 23 sites listed with their age span, 16 span no more than ~12,000 years and of those most are much shorter. In addition, the dating at Melrose is seriously flawed (Table 4; Meltzer et al., 2014; ENDNOTE **6**). Lake Hind was subjected to additional research (Teller et al., 2020) but sampling for claimed impact indicators focused on the zone just above and below the YDB (Table 2). In any case, of the 48 sites only the cores from Lake Cuitzeo, White Pond, and Stara Jimka (Tables 3 and 4) could be candidates for continuous sedimentation. Like most YDB sites, however, sampling focused on the purported YDB and the zones above and below. None of the 48 sites was sampled continuously over any significant span of time. At the Bull Creek site, Oklahoma, Sweatman (2021, p 10) claims that a section covers **"a timeframe of around 20,000 years"** but that is demonstrably in error. Six sections were sampled and dated. Five are alluvial sections scattered along the creek. Alluvial section BC1 was sampled more or less continuously, but that section spanned only the past ~13 kyr (Bement et al., 2014). The oldest alluvial section dates to ~14 ka, and all the alluvial sections exhibit discontinuous deposition as indicated by buried soils and erosional disconformities (Bement et al., 2014, SI). Only one exposure dates to ~20 cal ka BP and that is a dune over 5 km from the creek. Bull Creek BC1 is one of the very few exposures with a long, dated section in the YDIH literature. However, the two irreproducible 'nanodiamond' peaks there that are claimed by YDIH proponents as real (see Section 12.6) are purported below the YBD and in the late Holocene, as pointed out by Holliday et al. (2014, 2020) (See Table 5) ENDNOTE 9. No site in the Americas, Europe, Africa or Asia with 50 kyr of continuous sedimentation preserved and sampled is reported in the YDIH literature. Ice cores have a continuous record, but none of the usual proxies offered as evidence of an impact (e.g., nanodiamonds and microspherules) have been reported present.

Wolbach et al. (2020, p 97) further states **"Coeval peaks in the suite of impact-related proxies have never been found outside the YDB and adjacent strata in any of those records."** But as noted, continuous sampling of sediments through a column representing continuous sedimentation for thousands of years above, below, and through a clearly dated YDB has yet to be conducted. That is the only means of supporting the claim of uniqueness of the suite of indicators. While coeval peaks are often claimed, they are rarely documented at the YDB because most of the marker horizons and most of the purported YDBs are not accurately and precisely dated to the YDB.

ENDNOTE **5** The number in the prefix of specimen names (e.g., SR-09, SR-10, etc.) from Arlington Canyon refers to year of collection not the actual location or position of sample. This is shown on the lithological logs that Sweatman (2021) ignored. He also ignores the fact that one of the samples at Arlington was from G. James West as part of the original material collected from the same site as Kennett et al. (2008a) (AC003 = Site III of Scott et al., 2017). He does not comment that in Scott et al. (2017, SI) their log and photos are shown next to the corresponding log from Kennett et al. (2009b) and photos from Wittke et al. (2013a, SI).

ENDNOTE **6** A broad array of statistical approaches were used to model the dating at sites with claimed evidence for a YDB impact because few zones with purported impact indicators can be directly dated (e.g., Wittke et al., 2013a; Meltzer et al., 2014; Kennett et al., 2015a). Problems with these approaches are discussed by Meltzer et al. (2014) and are summarized as follows (p E2165): **"various regression models (linear, logarithmic, and polynomial) were used by YDIH proponents to derive an age–depth model for a stratigraphic section and thus interpolate the depth and age of the supposed YDB layer… The ages for the supposed YDB layer… share four significant flaws with their age–depth regression data and analyses: ages are omitted from the models without explanation or justification; depth measures are arbitrary; the regression results cannot be replicated even using the same age–depth data and in most cases are statistically insignificant; and, perhaps most critically, the statistical uncertainty that necessarily accompanies all radiometric dates (luminescence ages have at least 10% error) is** [ignored in many]." For example, at Melrose, Bunch et al. (2012) recovered an OSL date 16.4 ± 1.6 ka) from beneath the purported impact layer, assumed **"a modern age for the surface layer"** (Bunch et al., 2012, p E1905) or **"0 cal ka BP"** (SI fig. S5), and used a **"linear interpolation"** to date the alleged YDB zone. Such a model must assume continuous deposition, which can't be known given that the surface could have been stable or eroded. Based on one date with a large standard deviation and an unsubstantiated assumption about the age of the surface, the resulting "date" cannot be accepted as meaningful.

The Wonderkrater site (Scott, 2016; Scott et al., 2003; Thackeray et al., 2019) illustrates a very different set of problems: a) multiple models are applied; b) the reasons why some dates are rejected and others accepted is not clear; and c) the reason why one model is accepted and others rejected is not clear. Sweatman (2021, p 5) admits that **"radiocarbon dating of this site is highly uncertain… but nevertheless is consistent with a YDB age"** (Table 2). The model is based on dating in core B3 from the site. In examining Scott (2016), understanding where the B3 core age model comes from is difficult. But if the age model selected by Thackeray et al. (2019) is correct, the Pt spike is after the YD/GS-1 onset.

ENDNOTE **7** Sweatman (2021, p 20) also complains that Meltzer et al. (2014) "**even uses unpublished data and field notes to make its case."** He does not explain why primary data are problematic, but the use of "unpublished data" apparently refers to Meltzer et al. (2014, SI p 30) citing TL/OSL ages from the Gainey site. That dating was used in the original YDIH article *published* by Firestone et al. (2007)! Unpublished data are also cited by Kinzie et al. (2014, SI) and Pino et al. (2019, SI). ENDNOTE 16. Fifteen years after the publication of Firestone et al. (2007) the marker concentration and depth data used to generate graphs for sediments from 9 locations, including the lab measurements, lab notes, and field notes used to calculate them, remain unpublished. The data has never been made available to independent researchers despite requests.

ENDNOTE **8** Radiocarbon dating of soil organic matter and the broader issue of soil formation vs. depositional process are repeatedly misunderstood or ignored in the literature supporting the YDIH (e.g., the dating of the Bull Creek section ENDNOTE 9, the Black Mat, and the Usselo soil). Further elaboration is provided here. A fundamental tenant of radiocarbon dating is that radiocarbon dates on soil organic matter do not and cannot represent a moment in time. The carbon in soils (usually in the A-horizon; discussed below) is a mix of all of the organic matter that accumulated within the sediment via biological activity following deposition of the sediment (Schaetzl and Thompson, 2015, p 586-591). A radiocarbon date on a soil A-horizon tends to represent a minimum age of the sediment in which it formed. Thus, the age of the underlying sample zone could be significantly older.

Soils are produced by the alteration of sediment; i.e., they form within the sediment (Holliday, 2004; Schaetzl and Thompson, 2015). The alteration represents a specific type of surface and near-surface weathering over time under conditions of relative landscape stability (minimal erosion or deposition) that is driven as well as controlled by climate, bioactivity, landscape position, and the nature of the sediment in which the soil forms.

The A-horizon is the upper part of a soil dominated by biological activity. As such, it is characterized by increased organic matter (including organic carbon) and tends to be gray, dark gray, or black (the "topsoil" in gardening or farming). Thus, in a stratigraphic section where a soil is buried by younger deposits, the buried A-horizon can be a distinct stratigraphic marker. But it is not and should never be confused with a depositional "mat." A-horizons start to form at the surface and evolve downward into the host deposit (the "parent material" in soil parlance) due to biological activity (e.g., bioturbation) over time. The top of a soil is the ground surface of a landscape. In a buried soil, that contact (the paleo-surface) has stratigraphic significance (Holliday, 2004). But buried or unburied, the lower limit of an A horizon is a soil boundary not a geologic contact. It simply represents the lower limit of biological activity. A lower soil boundary has no stratigraphic relation to any bones or artifacts at or near it. They were buried within the deposits (the soil parent material) as those deposits accumulated and before soil development began to alter the deposits.

There are some soils that represent an intergrade of soils and deposits where slow aggradation of sediment keeps pace with organic matter production due to soil formation. They are common in some floodplain settings (e.g., Mandel, 2008) and in some wetland settings. These soil/sediments are the Aquolls and Mollic cienegas of Haynes (2008) (Table 7 of this review).

ENDNOTE **9** Wolbach et al. (2020, p 98) perpetuate a strange misunderstanding about the nanodiamond data and dating in table 1 from the Bull Creek study by Bement et al. (2014): **"Holliday et al. misrepresent the results of Bement et al. (2014), the independent site investigators, incorrectly implying that those nanodiamonds were found below the YDB layer."** Specifically, table 1 in Bement et al. (2014) clearly shows that the data generated is exactly as stated by Holliday et al. We encourage readers to verify by examining the table as published (and see Table 5 in the present article). Kennett et al. (2009a) dated his 100 ppb peak at 13.0 ± 1 cal. yr BP with an additional ≈ 25 ppb of nanodiamonds accumulating over ≈ 10 cm above that level. No stratigraphic depths were specified and the ≈ 10 cm thick section containing the nanodiamonds was identified as the YDB. Bement et al. (2014) identified the stratigraphic depth of Kennett et al.'s (2009a) peak at 298-307 cmbs and also dated it to 11,070 ± 60 ^{14}C yr BP (∼12,990 cal yr BP). Bement et al. (2014) reports nanodiamonds with a main peak of 190 ppm (per m*illion* compared to Kennett et al.'s per *billion*) at 307-312 cmbs with 1.9 ppm at 298-307 cmbs (∼ 20 x larger than Kennett et al.'s main peak at the same level). Bement et al. (2014) identify the 14 cm thick layer from 298 to 312 cmbs as the YDB despite the fact the section dominated with nanodiamonds at 307-312 cmbs is undated. He assumes the lower layer containing the main nanodiamond peak must lie within the YDB presumably because it contains nanodiamonds (circular reasoning). However, it is undated and lies below sediments dated to ∼13 ka. Bement et al. (2014, p 1730) confusingly attempts to mitigate this discrepancy by claiming, **"Our study identified a nd spike of 190 ppm immediately below a soil horizon interpreted as the YDB"**, but Bement et al. (2014, table 1) clearly claim the undated layer 307-312 cmbs as part of the YDB not below it. However, questions on the position of the YDB are rather moot given that the detection of nanodiamonds by Bement et al. (2014) could not be reproduced by that group (Sexton, 2016).

Considerable confusion about the dating and the location of the nanodiamond spike further confound the issue. While Kennett et al. (2009a) identify the YDB as the 298-307 cmbs layer (as stratigraphy specified by Bement et al., 2014) and the layer 10 cm above that, Kennett et al. (2015a) treat only the 298-307 cmbs layer (the lower radiocarbon-dated zone) as the YDB, but that is problematic given issues with dating soils ENDNOTE **8**. To make things even more confusing, Kennett et al.'s (2015a) OxCal code contains errors that make their model inconsistent with the stratigraphic statements in their main and SI text as well as every other preceding publication.

Further, the dates from Bull Creek are on soil organic matter, which is commonly younger than the sediments containing the organic matter ENDNOTE **8**. Thus, the stratigraphic association of spherules and purported nanodiamonds (i.e., whether they are primary deposits the same age as the sediments containing them or secondary post-depositional material similar to the organic matter) is unclear. That means that the radiocarbon ages of the organic matter cannot be confidently linked to either the sediment or the soil.

ENDNOTE **10** Many sites with purported nanodiamond concentration peaks at the YDB include sites with: "inferred dates" (Chobot, Kangerlussuaq, Kimbel Bay) or no direct dates on the "YDB" zone (Newtonville, Ommen) (Kinzie et al., 2014, SI table D3); poor age control and/or stratigraphic context (Gainey, Chobot, Melrose, Topper) (Kinzie et al., 2014, p 481; Meltzer et al., 2014; Holliday et al., 2014, 2020); unpublished data (Indian Creek) (Kinzie et al., 2014, p 478; Pino et al., 2019, SI); "YDB" zones represented by a disconformity (Blackville, Lindenmeier, Murray Springs) (Meltzer et al., 2014; Holliday et al., 2020); and dates from the Usselo soil, which spans ∼1400 ^{14}C years (Aalsterhut, Lingen, Lommel, Ommen; Section 5.6 and Table 4). Following Table 4 in this paper, other sites in Kinzie et al. (2014) with "YDB" zones not directly dated include Abu Hureyra, Blackville, Bull Creek, Santa Maira, and Watcombe.

ENDNOTE **11** Haynes (2008, table 2) includes the Aubrey, Dutton, Nall, and Thunderbird sites. The Aubrey site does not appear to contain a black mat over the Clovis occupation. Ferring describes soil weathering modifying the artifact assemblage (Ferring, 2001, p 41), but describes overbank alluvium as burying the occupation zone (Ferring, 1995, table 1; Ferring, 2001, p 47). Stanford (1979) provides no indication of a black mat at the Dutton site. There is no YD/GS-1 black mat at Thunderbird (Carr et al., 2013). The Topper site can be added to table 3 in Haynes (2008) as a Clovis site with no Black Mat.

ENDNOTE **12** Indeed, the work of Holliday (1995) and Mandel (2008), along the valleys of the Great Plains, was recognized with prestigious awards from the Geological Society of America: the "Kirk Bryan Award for Research Excellence".

https://community.geosociety.org/qggdivision/awards/kirkbryanaward

ENDNOTE **13** Black mats as defined by Haynes (2008) comprise two distinctly different sorts of geologic entities: 1) deposits with significant amounts of organic matter (i.e., the classic black mats of the San Pedro Valley of southeast Arizona as well as diatomaceous earth and diatomite on the Southern High Plains) laid down atop older deposits; and 2) soil A-horizons which include organic matter mixed down into older deposits (e.g., the Mollisols and Leonard Paleosol of the Great Plains). Soils are distinctly different from deposits, as discussed in ENDNOTE **8**.

ENDNOTE **14** According to Holliday et al. (2016, p 6), following (Surovell, 2014), this work "**gets at a more fundamental issue: the identification of magnetic spheres is subjective. In an unpublished methods document titled 'Separation of YD Event Markers (8/10/2007)' provided by Allen West, 'typical magnetic spherules' were illustrated with the same microspherule images published in Firestone et al.** [2007]… **In January of 2010, after the publication of Surovell's study, West provided an updated and unpublished version of the protocols titled 'Younger Dryas Boundary (YDB) Markers (Version 1-1-2010)' in which 'typical magnetic 'spherules' were illustrated as having dramatically different morphologies including particles that have rough surfaces and are nonspherical, even including teardrop-shaped sedimentary grains…** [fig. 5 from

Holliday et al. (2016)] **illustrates magnetic particles from** [Lubbock Lake] **sample at 73-75 cm. Following the identifications** [from the original 2007 protocols] **the sample in** [fig. 5 from Holliday et al. (2016)] **contains very few spheres. But using the illustration** [from the 2010 protocols], **then almost all of the photomicrographs** [from Lubbock Lake] **contain magnetic spheres. This example clearly shows that sphere counts could vary substantially between samples, even those counted by the same individual.”**

Further, Holliday et al. (2016, p 7-8) note a critical issue ignored by critics of Surovell et al. (2009): **“Species of magnetic microspheres and nanodiamonds were not differentiated. In part, this is because the work by** [Surovell] **was carried out prior to arguments over the origin of various species of microspheres.** [Kennett] **likewise did not perform species identification of either magnetic microspherules or nanodiamonds as part of the most recent study** [reported by Holliday et al., 2016]. **In any case, the initial arguments that nanodiamonds are clear indicators in support of the YDIH... were made before the significance of species identification was fully articulated.”**

ENDNOTE **15** Powell (2022, p 14) charges, **“Surovell et al. should have declared the matter unsettled and called for more research, including blind tests, sample exchange, and the like.”** That is exactly what happened at the Lubbock Lake archaeological site in Texas. As discussed by Holliday et al. (2016, p 2) **“In 2007, J. Kennett... and** [V. Holliday] **met at Lubbock Lake to discuss sampling of the section as a test of the YDIH. All agreed that blind splits of samples would be collected and one set sent to** [Kennett], **and the other to T. Surovell...** [Lubbock Lake] **was considered a good candidate for testing the YDIH because the site stratigraphy for the time period of interest is very similar to that at the Blackwater Draw site... 120 km northwest of Lubbock in eastern New Mexico and in the same drainage system as Lubbock Lake.... Data from Clovis was presented in the original publications in support of the YDIH... and continues to be used as a key site in support of the YDIH....”**

Holliday et al. (2016, p 4) also wrote, **“Six samples were collected from upper stratum 1, stratum 2A, and overlying 2B in the trench 65 section... to test for the reproducibility of lab methods used to extract purported impact indicators and to generally reproduce the stratigraphic sequence at Clovis** [the YDB should be at the base of stratum 2A]**. The samples were assigned random numbers in the field and sample splits of ~1 kg each were sent to** [Surovell] **and to** [Kennett] **for analysis. Both labs analyzed samples for magnetic grains and magnetic microspherules. Unknown to** [Holliday], [Kennett's] **lab also analyzed samples for nanodiamond content... The analyses by** [Surovell] **were reported by Surovell et al.** [2009]. **The analyses by** [Kennett] **were completed in 2010. An intermediary (W. Alvarez) was selected to compare the stratigraphic and chronologic data provided by** [Holliday] **with the results generated by** [Kennett]. **That mediator produced a report submitted to** [Kennett] **and** [Holliday] **(19 February, 2010). The results were never published by** [Kennett]. **The sampling at** [Lubbock Lake]**, however, was conducted under a Texas Historical Commission Antiquities Permit 4196. The data, therefore, are public information and were published in a report to the Texas Historical Commission.”**

That report was used as a basis for the paper by Holliday et al. (2016). The results of neither lab could be reproduced by the other, and neither lab… produced evidence to support the YDIH at Lubbock Lake. Instead, purported impact indicators were recovered by Kennett's lab at a level well over 1000 years younger that the YDB, but no spike in “indicators” was recovered by Surovell et al. (2009) even though low levels of microspheres were found in multiple levels. In 2011, however, Kennett proposed a visit to Lubbock Lake by Holliday and himself to resample together, with the field work, sample identification (blind), and recording by a staff member at Texas Tech University. That work was conducted in May 2011.

Holliday could find no lab that would or could conduct microspherule or nanodiamond analyses. In the Fall of 2013 Holliday and Kennett attended a conference together. Kennett was asked about the status of his analyses. He told Holliday that he had “moved on to other things.” So much for blind tests, sample exchange, and the like.

The manuscript on the blind test published by Holliday et al. (2016) was originally submitted to the *Proceedings of the National Academy of Sciences*. It was rejected without review because it was **“unsuitable for publication in PNAS”** (email from PNAS to Holliday, December 18, 2015) despite that journal publishing two dozen papers on the topic beginning in 2007.

ENDNOTE **16** In their study of the Pilauco site, Chile, Pino et al. (2019) use magnetic microspherules as notional impact indicators, but in their Supplementary Data they seriously misstate their case in citing other sites in support, especially the data availability and dating. The following are examples:

Indian Creek site: “Baker et al. [2008] **reported finding a YD-age site in Montana, writing that *‘the black mat contains … unrusted iron micro-meteorites [native iron magnetic spherules]. SEM photos of iron micro-meteorites reveal fusion crusts, flow lines, and micro-impact craters–direct evidence for an extraterrestrial origin.’*”** (emphasis and bracketed text in citation) Pino et al. (2019, SI, p 1).

Baker et al. (2008) were only reported in an abstract with no subsequent publication of data and discussion.

Murray Springs site: “Fayek et al. [2012] **found that *‘impact material contains iron oxide spherules (framboids) in a glassy iron-silica matrix, which is one indicator of a possible meteorite impact. … Such a high formation temperature is only consistent with impact … conditions.’* Because the framboids were encased in meltglass, they are inferred to have resulted from a cosmic impact.”** (emphasis in citation) Pino et al. (2019, SI, p 1).

Only one sample was collected by Fayek et al. (2012) from the **“black mat.”** No comparison samples from the rest of the section were reported.

Multiple localities: “Ge et al. [2009] **reported YD-age *‘microtektite-like glassy spherules’* from 3 widely separatd sites, in France, in the Caspian Sea, and in the Peruvian coastal desert. They state that the evidence ‘*supports an impact origin from an ejecta plume.*’”** (emphasis in citation) Pino et al. (2019, SI, p 1).

Ge et al. (2009) is only reported in an abstract with no subsequent publication of data and discussion.

“Surovell et al. [2009] **were unable to reproduce the spherule data of Firestone et al.** [2007]**, and, instead, reported finding YDB spherules heterogeneously distributed throughout the sediment profiles at seven sites. They concluded that YDB spherules are common and not restricted to the YDB layer, and, therefore, cannot be impact-related. Their Methods section states that the group used an updated spherule protocol from Firestone et al.... sent by one of the co-authors (A.W.) of this contribution. However, Surovell et al. did not conduct any SEM-EDS analyses of the candidates, as specified in the Firestone et al.... protocol. Thus, it is unclear what this group found, but they likely misidentified some detrital grains and/or framboids as ‘YDB spherules.’”** Pino et al. (2019, SI, p 1).

As noted by Surovell et al. (2009) and Surovell (2014) (see Section 10), “Allen West” confirmed the identifications by T. Surovell (email A West to T Surovell, July 15, 2008).

Hiscock site: “Laub [2010] **investigated sediment samples from the Hiscock archaeological-paleontological site in western New York state. He reported that *‘iron-rich spherules, 50-65 μm in diameter, were found in the Pleistocene horizon’* spanning the YD onset, as reported by Firestone et al.** [2007]**”** (emphasis in citation) Pino et al. (2019, SI, p 1).

Laub (2010) reports iron-rich spherules at two levels in **“the Pleistocene horizon”** and **“late-Holocene levels”** (p 168) but links no radiocarbon dates to those samples and further discusses the poor numerical age control at the site (p 169). He also notes (p 168) that none of the spherules **“have the surface smoothness of those figured by**

Firestone and his colleagues” and that the older ones **“may have originated geochemically.”** He concludes (p 169) **“it is surprising that evidence of the putative catastrophe is not more obvious here.”**

MUM7B site: **“Mahaney et al.** [2010a, 2010b] **found in Venezuela ‘*a mixed assemblage … of Fe spherules*’ in a YD-age layer ‘*with a frequency higher than chance occurrence.*’ Mahaney et al.** [2010b] **concluded that the ‘*new evidence … point tentatively to either an asteroid or comet event that reached far into South America.*’”** (emphasis in citation) Pino et al. (2019, SI, p 1).

Mahaney’s interpretations are based in part on presence of a **“black mat”** but as noted (Holliday et al., 2014; Meltzer et al., 2014), there is no direct age control on the **“black mat”** at this site and that zone is a manganese-iron concretion having no genetic similarities to the **“black mats”** of Haynes (2008) (Table 4).

Newtonville and Melrose sites: “Wu et al. [2013] **reported YDB spherules from two sites in North America and found evidence of high-temperature melting under low-oxygen conditions, as at Pilauco. They concluded that ‘*the [Fe-rich] spherules could be generated in a meteorite impact.*’”** (emphasis in citation) Pino et al. (2019, SI, p 2).

As discussed by Meltzer et al. (2014), neither of the two sites investigated by Wu et al. (2013) have direct numerical age control on zone with purported impact markers. Melrose produced a single OSL date with large uncertainty. Age of the “impact layer” was modelled on the basis of that one date (and assessed as “Low Quality” in terms of dating by Kennett et al., 2015a). Newtonville yielded no numerical age control.

Blackwater Draw and Topper sites: “LeCompte et al. [2012] **compared the results of Firestone et al.** [2007] **and Surovell et al.** [2009] **at two sites: Blackwater Draw, NM and Topper, SC. They reported that their ‘*spherule abundances are consistent with those of Firestone et al.… and inconsistent with the results of Surovell et al.*’ They concluded that Surovell et al. were unable to reproduce the results of Firestone et al. ‘*primarily due to their failure to adhere to the [Firestone] protocol*’, mainly by omitting SEM-EDS analyses.”** (emphasis in citation) Pino et al. (2019, SI, p 2).

See comments by Surovell (2014), Surovell et al. (2009) and Section 10. Dating of the purported impact markers at Topper is unclear and from a mixed context (Miller, 2010).

Lubbock Lake site: “**In a blind test, Holliday et al.** [2016] **reported that both participants found ‘YDB spherules’ in non-YDB sections of a sedimentary profile in Texas, but one participant** [Surovell] **performed no SEM-EDS analyses to confirm that claim. The other participant** [Kennett] **used SEM EDS to identify found an unpublished peak in spherules in the YDB layer.”** Pino et al. (2019, SI, p 2).

Kennett found spherules (using SEM-EDS) in abundances similar to Surovell (who did not follow methods of Kennett) except in a layer <11.5 k cal yrs. BP, but neither Kennett nor Surovell reported any spherules in the YDB layer (Holliday et al., 2016). Kennett never mentioned, shared, nor published peaks from the YDB layer.

Wolbach et al. (2020) repeat all of these misstatements from the Pino et al paper, apparently having never read the cited publications. Pino et al. (2019) conclude that **“the majority of independent studies (8 of 13) that conducted SEM-EDS analyses confirmed the presence of YDB spherules in layers that date to ~12,800 cal BP. The other five studies did not conduct SEM-EDS correctly and could not confirm the presence of YDB spherules.”** But the above discussion shows that 5 of the studies had poor or nonexistent age control, 2 were published only in abstracts with no supporting data nor full, subsequent publication, and one was based on a single sample. Therefore, the results of 8 of the 13 studies produced negative or inconclusive results.

ENDNOTE **17** Sweatman (2021, p 20) alleges several **“fallacious”** claims or arguments. For example, he states “**Strangely, it has even been argued that cosmic impacts do not produce extensive wildfires (Holliday et al., 2020)”** but provides no references documenting such extensive fires. That paper does not claim that impacts may not produce extensive fires, simply that a link between ET impacts and wildfires is weak, with citations (Holliday et al., 2020, p 84-85). On the same page, Sweatman asserts that “[o]**ther fallacious arguments against the impact hypothesis include the occurrence of multiple black mats** [of non-YDB age] **with a few similar geochemical signals** [that occur in purported YDB black mats] **(Pigati et al., 2012)**.” Those **“few similar geochemical signals”** are iridium in bulk and magnetic sediments, magnetic spherules, and/or titanomagnetite grains within or at the base of black mats. As of the timing of publication by Pigati et al. (2012) all of those geochemical signals were cited as reliable indicators of an ET impact (Firestone et al., 2006, 2007, 2010a; Israde-Alcántara et al., 2012; LeCompte et al., 2012; Kennett et al., 2009a). Indeed, on the same page, Sweatman asserts that the work of Pigati et al., **“actually supports”** the YDIH (Table 8).

Declaration of Competing Interest

The authors declare that they have no known competing financial interests or personal relationships that could have appeared to influence the work reported in this paper.

Data availability

Data will be made available on request.

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