



Rebuttal of Sweatman, Powell, and West's "Rejection of Holliday et al.'s alleged refutation of the Younger Dryas Impact Hypothesis"

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ABSTRACT

We stand by our original review. There is no support for a cosmic-origin catastrophe at ~12,850 cal years BP. There is also no support that at ~12,850 cal years BP human populations diminished, late Pleistocene megafauna were wiped out or reduced, and an unique global climate change occurred. The comments are largely built around the same claims we previously rebutted (and rebut here again) based on a broad range of scientific research published in long-standing and recognized journals on impact cratering and mineralogy/geochemistry, as well as late Quaternary geology, paleoclimatology, paleobiology and archaeology. Evidence and arguments purported to support the YDIH involve flawed methodologies, inappropriate assumptions, incomplete comparisons, overgeneralizations, misstatements of fact, misleading information, unsupported claims, irreproducible observations, misinterpretation of fundamental data, logical fallacies, and selected omission of contrary information. These issues are discussed within broader themes in the conduct of scientific research. The burden of proof is on the developers and supporters of the YDIH to critically test their own hypothesis and to fully respond to a large, diverse body of critiques, observations and contradictory evidence. To date, they have failed to do this.

1. Introduction

We appreciate the opportunity to amplify key issues we raised in our comprehensive critique of the Younger Dryas Impact Hypothesis (YDIH) and to address the broader issues raised by Sweatman et al. (2024) (hereafter "SPW"), including the definition of peer review, obligations to share evidence, the relationship of the YDIH to aspects of fringe science as well as to existing paradigms, and their perspective on the scientific method. Here, we discuss some of the overarching issues that have arisen in discussions of the YDIH and respond briefly to some of their

assertions.

SPW dismiss, out of hand, our comprehensive refutation of the YDIH, criticizing it, among other things, for its length, which reflects the multitude of problems with the purported evidence and its interpretation. They argue that it is the task of the scientists who are skeptical of new, extraordinary, and would-be revolutionary claims, to provide alternative explanations for evidence (in this case even if it is questionable) and prove those claims incorrect (i.e., falsification). They further suggest that the proponent's task is the simple verification of their hypotheses, a process that inevitably leads to confirmation bias

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(Nickerson, 1998). Their notion that it is the job of the skeptic to falsify the proponents' hypothesis is the opposite of the usual scientific method (Gauch, 2012).

The defense of the YDIH by SPW appears to hinge on unconventional definitions of peer review, corroboration, replication, evidence, the notion of "well established" hypotheses, and falsification. The bulk of their criticism repeats claims already addressed (the entire focus of Holliday et al., 2023; henceforth "HEA") and which remain unanswered. We stand by the data and interpretations we presented, which are based on peer-reviewed, reproducible, widely-accepted scientific data. Several of their criticisms are based on, and purportedly supported by, the unpublished manuscript that they cite. Since this material is presently inaccessible, our response to those points is limited. We object to these criticisms being published because they lack evidentiary foundation and are effectively unsubstantiated and unsupported.

Space limits a detailed response to their individual comments, and our experience shows that such a response is futile. Detailed criticisms or comments on the sources of contradictory evidence (URLs, quotations, references), such as those in Holliday et al. (2020), are ignored or simply declared as wrong (e.g. Wolbach et al., 2020). Consequently, we focus here on two themes that emerge from papers supporting the YDIH: 1) the conduct of science, and 2) recurrent misapprehensions by proponents. However, we have made additional material available through a website¹ consisting of two tables that present a listing of critical comments by SPW and our replies (Table 1), and that provide links to post-publication review comments on 33 papers supporting the YDIH that document problems and issues in those papers (Table 2). Throughout the paper we include callouts to these tables along with the web link.

2. Conduct of science

2.1. Peer review

Peer-reviewed publishing is a necessary but insufficient means of getting scientific evidence released to the scientific community. There are several false premises associated with SPW's repetitious appeal to their "peer reviewed evidence". First is the implication that if something has been peer reviewed, it must be true, with no false information, and accepted in the larger scientific community. For example, in SPW Section 13.2, they claim highly controversial measurements of fullerenes were not attempted by opponents because the **"original finding, being peer-reviewed, is accepted."** Second is the notion that everything published in a professed peer-reviewed journal has been subjected to actual peer review by appropriate subject matter experts in legitimate peer-reviewed journals.

The publication history of papers advocating the YDIH reveals a pattern that suggests an avoidance of rigorous peer review. Our brief mention in HEA of the group's exploitation of the PNAS "pal review" system understated the pervasive and enduring nature of their peer review circumvention. Seven papers by the YDIH group were published in PNAS between 2007 and 2013. In an article published in *Nature*, Jones (2013) made the following revelation:

"Steven Stanley, a paleobiologist from the University of Hawaii at Manoa... acted as a 'personal editor' on several of the PNAS papers on this topic, including... the original 2007 paper proposing the idea. 'It has been very controversial,' he admits. 'It's my view that I should help to get this stuff published. It needs to be aired; it's not outlandish.' PNAS typically uses a 'personal editor' option for papers considered too controversial to receive a fair hearing from the standard review process."

This was followed up by another article that discussed related PNAS papers by the YDIH group. Flam (2013) wrote:

"Standard peer review isn't always required if a member of the National Academy of Sciences wants a paper to be published. Why would these impact papers fail peer review? Are scientists in this field unusually closed-minded or is there something wrong with this research? Is it possible the data don't support the conclusion?"

This publication loophole was generally curtailed in 2010 and closed to the YDIH authors in 2013 when editorial responsibilities for impact-related papers were transferred to impact-cratering expert H.J. Melosh.¹⁻⁸ The only paper the YDIH authors published in PNAS after 2013 was Kennett et al. (2015). The lead author is a member of NAS, but whether it went through standard peer review is not known. Three authors of this reply (RPB, AEF, and IAJ) have now shown that, in addition to inconsistencies in their code, the paper contains a major methodological problem that undermines its central findings.¹

With the closing of the PNAS "pal review", most of the YDIH manuscripts appear to undergo rigorous peer-review to evaluate them on their merits. However, the Comet Research Group (CRG, see Section 3.1) attributes its lack of successful publication to different reasons. After failing to get their papers into a special issue of the MDPI journal they were editing³ the CRG recently launched its own on-line journal, *Airbursts and Cratering Impacts*, through an open-science website, which is co-edited by the three authors of SPW. The CRG claims that **"publishing information from the CRG...has always been difficult. Some papers have required four years to pass the so-called peer review and be published, while others, once published, have sustained repeated baseless 'nuisance appeals' for their retraction. Each of the new papers this week [published in their new journal *Airbursts and Cratering Impacts*] were [sic, previously rejected from other journals because they were] subjected to a rigorous review from hostile academic cliques for years. ... In the case of these more recent submissions, for reasons we can only suspect, it had clearly become much more difficult due to the biased conduct of the editors and reviewers. Over time it became clear that the effort required to publish in 'established' but biased journals wasn't worth it...."**⁴ That statement suggests that the new journal was developed to publish without confronting negative reviews. The notion of **"hostile academic cliques"** is another baseless claim (see Section 2.2) belied by mention of publication of **"Scores of peer-reviewed articles in dozens of peer-reviewed journals"** (SPW abstract) promoting the YDIH and the fact that other than skeptical assessments of the YDIH few of the co-authors of YDIH critiques have published together, and most have never met, either in person or virtually.

2.2. Post-publication review

Because of the pattern of circumvention of standard peer review by YDIH advocates, beginning with Firestone and Topping (2001), both skeptics and advocates have taken advantage of the online post-publication peer review tool, PubPeer.¹ Contrary to the assertion by the founders of the YDIH advocacy journal *Airbursts and Cratering Impacts* that their papers have been repeatedly subjected to for retraction, only one (Bunch et al., 2021) out of more than 50 YDIH-related papers that have been discussed on PubPeer has had an explicit call for retraction. This is in part a consequence of inappropriate image manipulation (a fact that is uncontested by the authors), and other

² Melosh (1989) is a prominent source that describes the theory of impact cratering and was used to train the current generation of scientists who fully embrace what was still a revolutionary idea in the mid-20th century. Significantly, Melosh's book is largely ignored by the authors of all papers promoting the YDIH.

³ <https://www.impact-structures.com/2023/07/author-invitation-geoscience-special-issue-terrestrial-meteorite-impacts-and-impact-cratering/>.

⁴ <https://cosmictusk.com/airbursts-and-cratering-impacts/>.

¹ <https://doi.org/10.5281/zenodo.13924289>.

irregularities that are now under formal investigation by the editors (see Table 2 <https://zenodo.org/records/13924289>). Notably, a recent study reported that the purpose of about two-thirds of PubPeer comments is “to report some type of misconduct, mainly about image manipulation” (Ortega, 2022, Abstract). Another YDIH-related paper by Tankersley et al. (2022, RETRACTED) was retracted for reasons that did not appear in PubPeer (<https://asistdl.onlinelibrary.wiley.com/doi/epdf/10.1002/asi.24568>), and for which there was never an explicit call for retraction. A substantively similar version of that paper (Tankersley et al., 2024) was published by *Airbursts and Cratering Impacts*, co-edited by the paper’s lead author.

2.3. Evidence

SPW repeatedly refer to “peer reviewed evidence” and argue, “Firestone et al. (2007) were the first to introduce the YDIH in a peer-reviewed journal, and the analysis of the modern hypothesis must begin there.” However, much of the evidence that has been presented in support of the YDIH is based on data that have not been or not properly peer reviewed. For example, most of the evidence in that 2007 paper came from Firestone and Topping (2001) and Firestone et al. (2006), neither of which was peer reviewed. Furthermore, West (co-author of SPW) conceded in a New York Times article that Firestone et al. (2006) “was never intended to be a scientific book...” (St. George, 2024, p. 34), and its purported data may be unreliable having been obtained by nonscientific ad hoc methodologies. This might explain why those earlier publications and Firestone et al. (2007) did not provide any information on those measurement methodologies, but that information is needed for a proper evaluation of the data’s merit. One example is information about material collection, methods, and evidence from the Gainey Clovis site near Flint, Michigan, which can be thought of as the “type section” for Firestone and Topping (2001). The Firestone et al. (2007, p. 16016–16017) discussion of Gainey is limited to one sentence fragment: “In the 1990s, W. Topping... discovered magnetic microspherules and other possible ET evidence in sediment at the Gainey PaleoAmerican site...” along with a graph and a table, but no raw data or information on methods. The graph contains the reproductions of a magnetic grain plot from Firestone and Topping (2001) and a magnetic spherule plot from Firestone et al. (2006), although Firestone et al. (2007) omitted some data points, and apparently adjusted or transformed the remaining data to other units without explanation.¹

2.4. Fringe connections

In defending the YDIH, SPW reference examples and align themselves with other forms of unconventional scientific theories, including elements of pseudoscience that are connected in various ways to the YDIH.

Cold fusion As a contrasting example of a theory unequivocally refuted SPW, in their conclusion point to cold fusion. “[T]he concept was so simple that the chemists already had the necessary laboratory equipment. Initial results seemed to confirm cold fusion, but then more and more reported that they could not replicate it, and within a few months, cold fusion was refuted.” They appear to be unfamiliar with its history, which has striking parallels to the YDIH. Cold fusion was initially rejected by most physicists on theoretical grounds, because (like the YDIH) the experimental or observational results contradicted the prevailing understanding of the laws of physics.⁵ Physicists concluded that the most likely explanation was that the experiments were faulty or misinterpreted, which turned out to be correct. Likewise, physicists were initially skeptical of the YDIH claims because “consideration of basic laws of physics indicate that such a

fragmentation or high-altitude airburst event would not conserve momentum or energy, would lie outside any realistic range of probability, and therefore did not occur during the YD as described by Firestone et al. [2007]” (Boslough et al., 2012, p. 24).

Ironically, like the YDIH, the cold fusion proponents continue to refuse to accept the lack of replication, theoretical arguments, and evidence-based rejection of their hypothesis. Four decades after the refutation of cold fusion, versions of it are still being pursued and promoted at the for-profit pseudoscience-themed “Cosmic Summit” fan conventions organized by CRG cofounder and director George Howard, who has co-authored many YDIH papers.⁶ These conventions featured cold fusion proponents Malcolm Bendall and Bob Greenyer, YDIH proponents including most notably two authors of SPW⁷ who firmly declared “cold fusion was refuted,” and many fringe science speakers/exhibitors.

The cold fusion fiasco is not directly comparable to the YDIH but there are similarities. Both started with a handful of scientists and quickly grew. However, cold fusion was based on laboratory experiments in a narrow field of physics and was quickly dismissed by most physicists (Goodstein, 1994). The YDIH involves a broad range of disciplines and is based on: 1) faulty assumptions regarding Clovis archaeology, extinctions, and the nature of the environmental changes during the last glacial/interglacial transition; 2) indirect field evidence, none of which was conclusively related to impacts; and 3) weak numerical age control. Cold fusion was easy to refute and was done so quickly. The YDIH can be refuted, based on data from impact physics and mineralogy/geochemistry, as well as late Quaternary geology, paleoclimatology, paleobiology, and archaeology. The fundamental problem with the YDIH is that proponents repeatedly fail to deal with its many contradictions, discussed in detail by HEA, through their circumvention of peer-review.

Nonscience sources. The title of the unpublished rebuttal of HEA by the authors of SPW refers to it as a “Gish Gallop,” apparently unaware that this is a tongue-in-cheek name for the chaotic debating method of a cofounder of a young-Earth creationist organization that publishes catastrophist papers (Scott, 2004). The YDIH proponents’ understanding of cosmic catastrophes is strongly influenced by biblical catastrophist publications they cite, which often misrepresent cited sources.¹ A prominent example is their repeated citation of Silvia (2015), who pioneered the idea of using the Old Testament as a guide to our understanding of cosmic airburst phenomena (discussed further in Section 3.4). Brazo and Austin (1982) is another example. They are cited by Bunch et al. (2021), which includes West as a coauthor, in regard to the Tunguska airburst event, despite their demonstrably false claims. The Institute for Creation Research is listed as both Brazo’s and Austin’s affiliation, which publishes exaggerated versions of known catastrophic events. Brazo and Austin (1982) appears in the journal *Origins*, which like the new journal *Airbursts and Cratering Impacts* (see Section 2.1) claims to be peer reviewed, but the effectiveness of their peer review is questionable.

The YDIH proponents are entitled to their faith, but their beliefs should not be propagated into the scientific literature for the YDIH and related cosmic catastrophes as arguments (unless they can be tested, in which case they no longer rely on faith) or claimed as “peer reviewed” evidence (unless they have been subjected to actual peer review). The definition of faith excludes the requirement for evidence, which may partially explain why YDIH advocates do not seem to feel that it is necessary to provide the underlying data on which their claimed evidence is based. Because of this, we feel that it has become futile to continue the repeated cycles of line-by-line refutations and rebuttals of this data-free “evidence”.

⁶ <https://cosmicsummit.com/thunderstorm-generator/>.

⁷ <https://martinsweatman.blogspot.com/2023/09/my-presentation-at-cosmic-summit-2023.html>.

⁵ http://www.its.caltech.edu/~dg/fusion_art.html.

2.5. Mistakes or misconduct?

SPW falsely state that we made “**repeated accusations of scientific misconduct...**” We report multiple instances of data inconsistencies and misstatements of fact in YDIH publications, but the words “scientific misconduct” do not appear in HEA. SPW apparently interpret what we report as examples of “scientific misconduct”. We offer no opinion beyond our factual reports and we made no accusations of misconduct because it is not our role as scientists to determine if apparent violations of various journals’ materials and data availability guidelines or inappropriate image manipulation rise to the level of misconduct. Our position is that we are professionally obligated to provide data that allow others (e.g., journal editors) to decide for themselves. The editors of the relevant journals (*PNAS*, *Scientific Reports*, and *J. Glaciology*) have been made aware of these apparent violations, which we have also documented on PubPeer.¹ Other data discrepancies were noted by Holliday et al. (2020).

We always encouraged the authors to make their raw data and methodology available so we can confidently evaluate the evidence, but our requests have been rebuffed or ignored. Until we are allowed to see the data, engaging in further detailed discussion of whether this claimed evidence supports the YDIH is premature.

2.6. Logical fallacies

There are several misunderstandings or misapplications of logical fallacies by SPW.

Strawman fallacy. They state that Firestone et al.’s (2007) YDIH and presumably subsequent versions “**did not predict or require the presence of an impact crater.**” SPW appear unfamiliar with the work of Firestone and colleagues while confused regarding their own work.

Firestone and others adopts many ideas from among the earliest versions of the YDIH, the speculative book by Donnelly (1883), which claims a comet struck North America forming the Great Lakes (see Section 13.3¹). Firestone et al. (2006), on which Firestone et al. (2007) is based (both coauthored by West), has chapters entitled “**The Carolina Bay Craters**” and “**The Main [YDB] Craters**” that argues the Bays and the Great Lakes are YDB impact structures. Firestone (2009) continued to predict craters and wrote (abstract), “**a 4.6 km-wide comet fragmented and exploded over the Laurentide Ice Sheet creating numerous craters that now persist at the bottom of the Great Lakes**” and it also (conclusion) “**likely formed the Carolina Bays**” (see also Firestone et al. 2010). Powell (2020, p 37) in his book made several YDIH predictions, “**Prediction 4. Scientists may find a crater or craters of YDB age.**” Sweatman (2021, p 19) confusingly predicted that an unconfirmed crater “**increases the probability that the Hiawatha crater, and its potential twin [the unconfirmed crater], are YD impact structures.**” Bewilderingly, West in Mahaney and Somelar (2024, p 4) wildly predicted, “**features indicating a young age for the Hiawatha Crater, seemingly incompatible with a [now well-dated] Paleocene age, render ... prime evidence for the elusive YDB impact crater**” (see Section 8.1¹). Not to mention that the leading YDIH proponent, J. Kennett, told a reporter, “**I’d unequivocally predict that this [Hiawatha] crater is the same age as the Younger Dryas [emphasis added]**” (Voosen, 2018) and “**the older date for the crater is a surprise...**” (Voosen, 2022, p 1076). Furthermore, the putative YDIH impact markers include materials that have been attributed to and claimed to require a crater-forming impact (e.g., microspheres; see Sections 7 and 10 of HEA).

Argument from ignorance. SPW uses as their example the statement by Surovell et al. (2009, Abstract) regarding their attempted spherule recovery “**We were unable to reproduce any results of the Firestone et al. study and find no support for Younger Dryas extraterrestrial impact**” and counter it by stating that “**Absence of evidence is not evidence of absence**” (see also Conclusions). But the example they provide is logically equivalent to the unequivocally true

statement, “absence of evidence is evidence of absence of evidence.”

2.7. Reproducibility

Reproducibility of results is a key component of hypothesis testing. The work reported by Surovell et al. (2009) was one of the first attempts to do so in the context of the YDIH. SPW repeatedly state that YDIH investigators replicate lab data that supports the hypothesis and that critics offer no contradictory data. Both claims are false assertions. The raw data claimed to support the YDIH is rarely available and, therefore, most claims of reproducibility can be dismissed (see links to the post-publication reviews in Table 2 <https://zenodo.org/records/13924289>). Further, as discussed in a variety of publications, there are examples of analysis of materials examined by YDIH proponents as well as critics and other independent researchers (i.e., splits of samples) that raise questions about the validity of the reproducibility of results and the YDIH (Section 14 of HEA). This and other contradictory data is systematically ignored, grossly misrepresented, and/or misunderstood by YDIH proponents: e.g., Arlington Canyon site (Section 4.1 of HEA), microspherules (Sections 10, 14 and End Notes 14, 15 of HEA), and lonsdaleite (Section 12.2 of HEA) to name a few. One egregious example is the irreproducible Bull Creek nanodiamond results. Re-analysis of the same samples failed to produce such material (Sections 5.5 and 13.5 of HEA and Table 1 <https://zenodo.org/records/13924289>).

The Gainey site was highlighted as a key YDIH site from Firestone et al. (2006, 2007) through Kennett et al. (2015), despite well-documented evidence for mixing. The “impact indicators” from the Gainey site were claimed to be YDB spherules yet were shown by Boslough et al. (2012) to date to the late Holocene (and “modern” when dated by Firestone, 2009).

SPW specifically mention the work of Surovell et al. (2009), the first attempt to reproduce recovery of microspherules from YDB zones. SPW note that “**It turned out that in identifying YD microspherules, Surovell et al. (2009) had not used the same procedures as Firestone et al. (2007), and when others did, they replicated them in abundance.**” However, YDIH proponents including SPW fail to discuss the changing protocols for spherule recovery, summarized by HEA. Nevertheless, SPW fail to recognize that the protocols used by Surovell et al. successfully recovered claimed impact-derived microspherules and magnetic grains from Lubbock Lake, Texas as confirmed by SPW co-author West (email A. West to T. Surovell, July 15, 2008). Even if only a subset sampling of a larger population was collected by the protocols Surovell et al. used, their profile should mirror that of the larger population. While Surovell et al. did recover spherules, they found no concentration spikes within the YDB at sites studied by Firestone et al. (2007). Further, proponents claim (Section 14 of SPW) that “**Surovell et al.’s (2009) results cannot be replicated.**” That is false. At Blackwater Draw, Andronikov et al. (2016) measurements of the magnetic grain concentration profile was consistent with Surovell et al. (2009) and showed no peak at the YDB, however curiously their profile of the spherules (collected from the magnetic grain isolates with no peak at the YDB) showed a peak at the YDB consistent with Firestone et al. (2007). At Lubbock Lake, Surovell et al. (2009) measured minimal levels of spherules near the YDB consistent with measurements by YDIH proponent J. Kennett (reported in Holliday et al., 2016). The striking contradiction in spherule recovery at the Lubbock Lake site revealed by Holliday et al. (2016) is the low level of spherules throughout the section reported by Surovell et al., while dramatically higher levels (along with claimed nanodiamonds) were measured by J. Kennett in the strata dated younger than ~11.5 k yrs. (results accepted by Sweatman, 2021).

3. Recurrent misapprehensions

Since its inception the proponents of the YDIH have held on to several notions or assertions that have been continuously refuted by evidence that is easily accessible and understandable. These include the

relationship between the age of the Younger Dryas/Greenland Stadial 1 (YD/GS-1) transition and the time span of Clovis culture, fundamentals of Quaternary stratigraphy and dating, interpretation of biomass burning records and the relationship of the YDIH to existing paradigms.

3.1. YDIH and Clovis technology

Among the fundamental assumptions by YDIH proponents are that the timing and cause of the end of Clovis technology is a mystery, that late Pleistocene extinctions were synchronous, and that the climate episode known as the Younger Dryas was unique. These assumptions all contradict a broad and well-documented array of data, but the proponents assert that they are all best explained by an impact at precisely 12,850 cal years BP (e.g., Firestone et al., 2007; Powell, 2020). In SPW (Section “Analysis”) they propose that these invalid assumptions are “possible secondary effects...” of an impact and should be distinguished from the “evidential basis for the YDIH.” This seems to be an attempt to ignore inconvenient data presented by us. Their erroneous assumptions have been at the heart of the YDIH since the early publications by Firestone et al. (2006, 2007) and continue to be so in the most recent publications (e.g., Powell, 2020; Sweatman, 2021, SPW) (see Section 1¹). They are also highlighted on the website of the CRG,⁸ which was founded by the lead authors of most of the YDIH papers, and which provides funding for much of their current research through donations.

In other publications, in well-documented circular reasoning by YDIH proponents (Table 4 of HEA), they use those assumptions as evidence of an environmental catastrophe at the YDB (e.g., Wolbach et al., 2018b; Powell, 2022, SPW). Sweatman in his publications repeatedly quotes Haynes Jr. (2008, Abstract), “...a remarkable major perturbation occurred at [the YDB] that needs to be explained.” As summarized by us (Sections 1.0, 4.0, 5.7 of HEA), and as well established and widely published in books and papers by many investigators in a broad array of the Quaternary sciences: Clovis technology did not disappear at 12,850 cal years BP; the late Pleistocene megafauna extinctions varied by individual species from continent to continent and through time; and while globally synchronous in its timing, the environmental changes during YD/GS-1 were not globally homogeneous in sign or magnitude, and other environmental changes began earlier.¹ The burden of proof is on SPW and other YDIH proponents to demonstrate that those interpretations are in error, that all those events are clearly synchronous, and that Clovis technology ended due to a catastrophe and did not evolve into other technologies.

3.2. Stratigraphy and dating

Another fundamental issue that SPW misconstrue revolves around methods of late Quaternary numerical age control. They and Sweatman (2021) suggest that geologists, paleobiologists, paleoclimatologists, and archaeologists do not understand the concept of “error” and “uncertainty” in dating (without offering their understanding of these concepts in the context of numerical dating methods). They argue or imply that age-depth modeling is the only way to deal with “error” and attack critics for not creating age-depth models (see Sections 5.3, 5.6, 5.8 of Table 1 <https://zenodo.org/records/13924289>).

They seem to use “error” in two ways. One is the sense of “statistical error” (which is an incorrect use of “error”; radiocarbon practitioners prefer “analytical or measurement precision” (Taylor and Bar-Yosef, 2014, p 27) (see also Section 5.6¹). But in this sense, many YDIH papers are rife with misunderstandings of statistical uncertainty. Kennett et al. (2015) and Sweatman (2021), among others, repeatedly claim that measured dates that overlap with very large standard deviations offer some sort of confirmation of synchronicity. Uncertainty in radiocarbon

dating is largely related to radiocarbon-decay counting statistics (Taylor and Bar-Yosef, 2014, p 27). The mean or median reported as a radiocarbon date is the midpoint of a symmetric probability distribution. The mean together with its standard deviation, traditionally reported as 1 standard deviation, indicates a 68 % probability that the true (radiocarbon) age is within the range of the deviation (Taylor and Bar-Yosef, 2014, p 128; Bradley, 2015, p 63–64). The reported radiocarbon age therefore does not indicate a specific moment in time but rather an age range where the true age has a 68 % probability of being within. Other sources of “error” include contamination or samples that are not related to the materials or strata of interest (documented in Table 4 and Section 5 of HEA).

The true test of synchronicity relies on high accuracy and precision measurements to achieve standard deviations sufficiently small that using age ranges of several standard deviations (with higher probabilities that they contain the true age) can allow discrimination between possible asynchronous events. Accurate, high-precision dating from secure stratigraphic contexts in direct association with the YDB is crucial in arguing for a catastrophic event at a specific moment in time in the geologic past. However, the very standards set for accuracy and precision in numerical age control by YDIH proponents themselves (Kennett et al., 2008a, p 2531; Kennett et al., 2008b, p E107) have not been met by any YDIH proponents, as documented by Holliday et al. (2014, 2020, 2023), Meltzer et al. (2014), and van Hoesel et al. (2014). These issues have never been directly confronted by YDIH proponents (Tables 1, 2, 4 of HEA).

Many examples from the YDIH literature expose fundamental misunderstanding of the importance of a secure sample context for numerical dating even though sites are described as “well-dated” or exhibiting “good” age control (Table 4 of HEA). As documented by Meltzer et al. (2014) and us, 26 sites have no direct age control for the purported YDB, 7 have no numerical age control whatsoever, 6 have mixed archaeological context, 4 have mixed sedimentological context, 4 have claimed impact indicators older or younger than the claimed YDB, and 3 have confused or complex microstratigraphy. Wittke et al. (2013) and Moore et al. (2017) openly state that some of the key strata in their study sites are mixed but are still used as proof of well-dated YDB contexts. A variety of other potential problems of stratigraphic context, mixing, and contamination with sampling sites reported by Firestone et al. (2006, 2007) and Bunch et al. (2012) are noted in PubPeer postings (Table 2 <https://zenodo.org/records/13924289>).

SPW further argue in their Section 5.6 that we “fail to acknowledge that the intrinsic radiocarbon measurement uncertainty is not a good estimator for the true sample age uncertainty” (which is a baseless assertion that contradicts late Quaternary research worldwide). Yet they do not hold YDIH proponents, their data, and data of others to the same standards. For example, Firestone et al. (2007), Haynes Jr. (2008), Bunch et al. (2012), and Moore et al. (2017) provide no age models and some of their sites have no numerical age control whatsoever. Such inconsistency is highlighted by Sweatman’s (2021) acceptance of the dating and stratigraphic record at Lubbock Lake, Texas, showing few spherules or claimed nanodiamonds at the YDB but large purported spherule and nanodiamond spikes (based on data from J. Kennett) in sediments younger than ~11.5 k cal yrs. (based on data from Holliday et al., 1983, 1985; Johnson, 1987).

Their discussion of dating is completely contradicted by their subsequent claim (Section 5.8 of SPW) that “Essentially, there is no way that a common layer of impact proxies dated to within a thousand years across four continents can signal anything other than a singular event, or highly correlated series of events.” This rather remarkable and baseless statement, carried to its logical end, implies that so-called impact proxies are incapable of being dated with any degree of accuracy or precision to ~12,850 cal years BP! They seem to make a self-contradictory argument that they know that there was some sort of ET impact at ~12,850 cal years BP based on the dating at dozens of sites, but because radiocarbon dating is so imprecise we cannot date

⁸ <https://cometresearchgroup.org/comets-diamonds-mammoths/>.

anything with that sort of precision, thereby explaining the apparently poor age control.

A key point of the YDIH is that some sort of “catastrophic event” happened at ~12,850 cal yrs. BP. The SPW comment about “**dated to within a thousand years**” comes across as another attempt to deal with the inconvenient truth about the poor age control for most YDIH sites. More broadly, the remark is completely contradicted by the large body of published data illustrating intercontinental comparisons if not correlations of late Quaternary geological, paleobiological, paleoclimatic, and archaeological sequences based on accurate, high precision radiocarbon and other numerical dating methods (e.g., [Sibrava et al., 1986](#); [Soffer and Gamble, 1990](#); [Straus et al., 1996](#); [Markgraf, 2001](#); [Viau et al., 2002](#); [Barnosky et al., 2004](#); [Koch and Barnosky, 2006](#); [Ehlers et al., 2011](#)).

3.3. Biomass burning

Another element of the YDIH, present since [Donnelly \(1883\)](#) and adopted by [Firestone et al. \(2006, 2007\)](#), is the claim that there were widespread wildfires, multi-continental in scale, at the beginning of the YD/GS-1. As evidence [Firestone et al.](#) cite the ammonium “spikes” reported for the GISP-2 ice core by [Mayewski et al. \(1993, 1997\)](#). [Firestone et al. \(2007, p 16020\)](#) state that “**superheated ejecta would have induced extreme wildfires, which would have decimated forests and grasslands, destroying the food supplies of herbivores and producing charcoal, soot, toxic fumes, and ash.**” The apocalyptic depiction of a biomass burning peak (“[t]he entire continent was on fire,” [J. Kennett, in Pringle, 2007](#)), was further elaborated by [Wolbach et al. \(2018a, Abstract\)](#), who cited “**highly anomalous**” peaks in biomass-burning indicators that “**may have consumed... ~ 9 % of Earth’s terrestrial biomass**” at the beginning of their hypothesized “Impact Winter” (see also [Wolbach et al., 2018b](#)). These peaks in ice-core indicators of biomass burning that were described as “**quite special**” relative to those that occurred over the whole of the last glacial-interglacial cycle ([Sweatman, 2021, p 14](#)).

However, the ice-core evidence has been badly misinterpreted by YDIH proponents (Section 9.2 of HEA). They confuse the smoothed background component of ammonium in the GISP2 ([Wolbach et al., 2018a, fig. 3C](#)) and NGRIP ([Fischer et al., 2015](#)) ice-core records (which is produced by soil and biomass production of ammonium) with a record of biomass burning, which it is not. They also misinterpret the peaks-above-background component that *does* represent wildfire episodes in North America ([Fischer et al., 2015](#); [Kjær et al., 2022](#)), as a record of local wildfires (in Greenland!), stating that we (HEA) “**prefer to focus on frequency data for local wildfires. But this is inappropriate for two reasons: (i) because of the location of the NGRIP ice core near the middle of a massive ice sheet, and (ii) because local fire frequency data will not detect distant YD impact-induced wildfires which could all have begun synchronously**” (italics in original). Ignoring the assertion that we attribute the NGRIP peak-frequency to local fires in Greenland, which clearly neither we nor [Fischer et al. \(2015\)](#) do, this statement is significant because it invokes the proponents’ often-cited assertion of synchronicity of fires at the beginning of the YD/GS-1 (e.g. [Wolbach et al., 2018b, Abstract; pp. 191, 196, 202](#)). However, ice-core records from NGRIP ([Fischer et al., 2015](#)) *do not* record a peak in biomass burning at the beginning of the YD/GS-1. Both fire-peak frequency and the peak-detection threshold *decrease* during the YDC ([Fischer et al., 2015, fig. 4a](#)).

Other evidence cited by YDIH proponents for a widespread episode of biomass burning at the beginning of the YD/GS-1 is sedimentary charcoal from lakes and bogs. Again, we have pointed out fundamental misinterpretations of the basic data, as well as misquotation and misquotation-by-omission, of [Marlon et al. \(2009\)](#) (Section 9.1 of HEA). [Wolbach et al.’s \(2018b\)](#) and [Marlon et al.’s \(2009\)](#) analyses are not inconsistent; both *are* consistent with the NGRIP ice-core fire-peak frequency record ([Fischer et al., 2015, p. 726](#)). Neither the ice-core nor the

charcoal record provide much support for an apocalyptic-scale episode of synchronous wildfires at the beginning of the YD/GS-1.

This situation leaves the YDIH proponents in a difficult position. The YDIH and its corollary “Impact Winter” propose that there was a remarkable episode of synchronous biomass burning across multiple continents at the beginning of the YD/GS-1, which is a testable and falsifiable hypothesis. But ice-core and sedimentary charcoal observations do not support this idea. In practice, that situation would usually lead to rejection of the hypothesis by its proponents, or its modification. Indeed, we can note a gradual erosion of YDIH proponents’ confidence in the hypothesis of an exceptional and synchronous peak in biomass burning from the early papers, to [Wolbach et al.’s \(2018b, supplemental materials, fig. A10\)](#) hedging on the widespread nature of wildfire (“[a]lthough some lakes lack charcoal peaks across the YD interval, there is no requirement that impact-related YD boundary (YDB) wildfires were ubiquitous”), to [Sweatman’s \(2021, p 14\)](#) assertion that “[a]ll [Wolbach et al. \(2018b\)](#) 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”, to SPW’s claim (Section 9.3) that “**the best that any YDIH study can expect to achieve is to show that there is evidence for extensive continental wildfires near the YD onset, within error bounds, and this has clearly been achieved....**” They appear to be saying that the occurrence of *any* fires near in time to the beginning of the YD/GS-1 would therefore support their hypothesis, and because wildfires occur all the time that would surely be the case. However, a hypothesis that can never be falsified or rejected is scientifically useless ([Chamberlin, 1890, 1899](#)), and cannot be used to support the main hypothesis, nor can it verify the main hypothesis. There is no support for an exceptional, widespread, and synchronous episode of wildfires at the beginning of the YD/GS-1.

3.4. Existing paradigms

SPW coauthors previously promoted the notion that the YDIH represents a paradigm shift that would overturn our current understanding. [Sweatman \(2021, p 1\)](#) begins his review with the assertion, “[t]he occurrence of a global cosmic catastrophe... represents a paradigm-shift in understanding with profound consequences....” Another irony that is missed by SPW is that the YDIH would supplant the current theoretical foundation of one of [Powell’s \(2022\)](#) examples of a recent scientific revolution, impact cratering. The theory of impact cratering is based on a body of facts that have been repeatedly confirmed through observation and experiment. It incorporates the theoretical framework of shock wave physics, which would be contradicted by the YDIH’s notion of “superheated air.” This idea began with [Silvia \(2015\)](#). He invoked faith-based understandings of airbursts by citing [Collins \(2002\)](#), who described a Biblical airburst in his fig. 1 caption: “[R]egardless of the nature of the destruction that befell the Cities of the Plain, one thing is clear from the biblical text: the fiery blast came from above. Superheated air and/or some kind of impact, like that of a disintegrated comet fragment moving at a high rate of speed, could have obliterated virtually everything....”

[Collins’s](#) and [Silvia’s](#) notion of “superheated air” is based only on their intuitive interpretation of the Old Testament book of Genesis, which is not supported by shock temperature calculations based on the Rankine-Hugoniot equations, governed by the laws of physics (conservation of mass, momentum, and energy), and equation of state of air. Nor is it supported by the observations of phenomena at Tunguska. The initial description of the hypothesis by [Firestone et al. \(2006, p 138\)](#) resembles [Collins’ \(2002\)](#) interpretation of the verse in Genesis, “**Within seconds after the impact, the blast of superheated air expanded outward at more than 1,000 miles per hour, racing across the landscape, tearing trees from the ground and tossing them into the air, ripping rocks from mountainsides, and flash-scorching plants, animals, and the earth.**” This Bible-based misconception remains

deeply embedded in YDIH publications.

According to Wheeler and Mathias (2019, p 86), “[t]o approximate the areal extent of observed Tunguska tree damage, we used a 4-psi blast radius range of 10–30 km....” Glasstone and Dolan scaling for circumstances with an optimal height of burst that produces 4 psi at this distance, does not generate a blast anywhere that exceeds about 10 psi overpressure. If Tall el-Hammam were between 10 and 30 km away from the source, the peak overpressures from a Tunguska-like airburst would therefore be between 4 and 10 psi, which corresponds to a maximum possible transient temperature (for no more than a few seconds) increase of between 20 and 45 degrees C (Brode, 1968). The blast wave would not be hot enough to melt pottery or mudbricks, nor to be described as “superheated air”.

These biblically-based notions in the YDIH suggest that the observations and experiments supporting shock wave theory are wrong and that the many successful predictions were just “lucky guesses,” for example, the successful and robust prediction of visible plumes from the impact of Shoemaker-Levy 9 on Jupiter (Boslough et al., 1994). Another successful prediction of impact cratering theory was that the Hiawatha crater would be found to be much older than the Younger Dryas onset (Boslough, 2019).

Further misunderstanding of the results of an impact can be seen in the paper on Tall el-Hammam (Bunch et al., 2021) (and potential relevance to the Biblical story of Sodom). As noted by prominent blast wave physicist Charles E. Needham (e.g., Needham, 2010):

“I do not see how a blast wave of ANY yield could cause that kind of debris accumulation. As you point out, the timing is all wrong; the shock front is moving supersonically but the wall responds at about 1/2000 the air speed. By the time the wall moved one wall thickness, the wind velocity would be considerably reduced (or reversed). An accumulation of debris, such as pictured in figure 16 is more characteristic of a pyroclastic flow or a flood or mud slide.” (email C. Needham to M. Boslough, January 2022).

Another potentially paradigm-changing hypothesis discussed by SPW is “coherent catastrophism” based on a hypothetical dense concentration of comet fragments called the “Taurid swarm” which would radically alter our current estimates of risk from near-earth objects (NEOs) and the overturn our understanding of impact frequency on which the field of planetary defense is based. Section 7 of SPW claim that the reason the Taurid swarm was not observed was because a 2019 observation campaign “never took place as expected.” One proposed element of the campaign using one telescope on Mauna Kea was not executed as hoped, but the normal NEO surveys continued using other survey telescopes. The observers were aware of the discovery opportunities and analysts were specifically looking for them. According to JPL’s Center for NEO Studies (CNEOS) web page (<https://cneos.jpl.nasa.gov/stats/>), 936 NEOs were discovered during the months that objects in the Taurid resonant swarm would have been visible from Earth. None of them were in a Taurid orbit.

4. Summary and conclusions

SPW did not specifically address most of the points we made in our review. In the interest of brevity, we chose to limit our response to the issues they did bring up. Many of these matters have little to do with our detailed critique, but instead get to the heart of the sharp difference in our understanding of how science is done and how these divergent opinions have played a role in the YDIH controversy.

We approached this debate evaluating the purported evidence and claims by proponents of the YDIH within the framework of various existing paradigms that form the backbone of our respective fields of expertise. This includes the widely accepted physics-based paradigm of impact cratering and associated phenomena, which have been validated by laboratory experiments, nuclear tests, craters on the moon and other

planets, the impact of Comet Shoemaker-Levy 9 on Jupiter, and most recently the DART mission. Proponents of the YDIH that argue for evidence that does not fit within these paradigms are obligated to provide direct evidence that would overturn them. Furthermore, we expect any proponent of the YDIH, at a minimum, to demonstrate a working understanding of the paradigms they are attempting to overturn. Most of all, we insist that they show their data and their work, as we have done in our own research in support of our current paradigms, all of which has gone through legitimate peer review. After nearly two decades of revolutionary hypothesizing, they have failed to do any of these things.

All publications supporting the YDIH suffer the same flaws as Firestone et al. (2007). As presented in peer-reviewed papers and synthesized by HEA, the dating of the purported YDB is poor (in terms of stratigraphic association, accuracy and precision) at most of the >50 YDIH sites reported. There are no smoking gun indicators of an impact at the YDB. There is no evidence of a catastrophe nor a crater. Nor is there evidence of disruptions of streams, glaciers, or other geomorphic systems, and there is no evidence of continent-wide vegetation changes. SPW brush aside our criticisms stating “differences in interpretation are part and parcel of science and do not lend themselves to the refutation....” Near the end of their response, they complain “opponents of the YDIH seldom do original research on the hypothesis. In the nearly 100,000 words of the HEA article, no new data is [sic] reported.” SPW forget that the name of the journal with our review, their attempted response, and our rebuttal are being published in is *Earth-Science Reviews*, which is not a primary venue for new research. They largely ignore, dismiss, or misrepresent the research data and results in our publications cited in HEA.

HEA reached the size that it did (96 k words) for two reasons: 1) the number of issues with the YDIH that needed to be addressed, and 2) the abundance of relevant contradictory information in the literature. The YDIH is based on poor age control, faulty reasoning, lack of purported unambiguous impact indicators, flawed methodologies, inappropriate assumptions, questionable conclusions, misstatements of fact, misleading information, unsupported claims, inconsistent or contradictory data, logical fallacies, and selected omission of contrary information. Critiques of the YDIH are based on a large body of data largely from veteran, well-published investigators on original research in impact physics and mineralogy/geochemistry as well as late Quaternary geology, paleoclimate, paleobiology, and archaeology, including research germane to the YDIH. Moreover, YDIH critiques are backed up by a vast body of data in all those fields generated by hundreds of scholars over many decades. That body of research was and remains the basis for skepticism of the YDIH. The larger problem, as documented in HEA, here, and in other critiques is that YDIH proponents are unaware of or ignore this extensive body of contradictory data and rarely directly refute those data.

SPW demand through their remarks that we provide “dispositive evidence” for seven critical comments we made. In their Conclusions they argue that “The YDIH evidence is reproducible, and no hypothesis other than a cosmic impact has been offered to account for it. Those who claim to refute the YDIH are obligated to provide a plausible alternative to explain this abundant evidence.” Proponents of the YDIH often forget that they are proposing a hypothesis and hypotheses need to be tested. Critics have found that the purported evidence is critically flawed and is “explained” by misidentification, misinterpretation, or as being completely unreliable. In other words, much of the evidence *does not exist*, and thus *does not require explanation*. In hypothesis testing, there is no requirement that those who test it (critics) explain the data or offer a plausible alternative. <https://www.logicallyfallacious.com/cgi-bin/uy/webpages.cgi?logicalfallacies/Shifting-of-the-Burden-of-Proof>.

We and other critics repeatedly devoted our critiques to 1) point-by-point evidence documenting the problems in dating of most YDIH sites, 2) pointing out well-known, non-impact related explanations for all their purported “impact indicators” and 3) have pointed out

misunderstandings or misinterpretations of data. YDIH proponents, in turn, have yet to offer direct evidence of an impact scenario consistent with all the evidence for a multi-continent catastrophe that disrupted or wiped out human populations and megafauna and changed climate at ~12,850 cal yrs. The burden of proof for such an event lays with the proponents, proof that we have repeatedly shown to have been misunderstood, misrepresented, and misaligned. There is no credible evidence of a catastrophic impact at the YD onset.

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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