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Hazards of the ASTM/G4 Deplorable² Peer Review Process: Personal Case Studies

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ABSTRACT: In the mid-1990s, the peer review procedures of ASTM Committee G4 were unilaterally altered (debased) in a manner argued herein to be procedurally improper, and consequentially counter and even destructive to the pre-established safety mission of the committee.

KEY WORDS: Peer review, Dunning-Kruger Effect, confirmation bias, cognitive bias, narcissistic personality disorder.

An important mentor often told and repeated an aphorism to this worker: “technical papers are not like mystery novels. You do not [and should not] have to read to the end to find out who dunnit”. The bottom line conclusion of this commentator here is that for too long, the ASTM Committee G4³ peer review process has been manipulated in harmful service to intellectual bullies. It did not start out deplorable, it was made deplorable. In this case, you may, indeed, have to read a good bit more of what seems like a soap opera or droning saga, a marathon of sour grapes, more than you want to in order to appreciate who dunnit and how they dunnit. Alas, sometimes it takes a lot of work to prosecute and provide thorough bases for an allegation such as the blunt opinion offered here.

A possible moral of this story: “When peer-review and/or firearms do good it is because good guys use them; When peer review and/or firearms do bad, it is because bad guys use them.” Guns don’t kill people, peer review doesn’t censor (or in some cases kill) people, people kill and people censor people.

The current (2025) Wikipedia definition claims:

“Peer review is the evaluation of work by one or more people with similar competencies as the producers of the work (peers)....It functions as a form of self-regulation by qualified members of a profession within the relevant field”.

¹ No mailing address. This is an archival paper intended to outlive its author.

² “Deplorable” meaning herein “deserving strong condemnation” sense.

³ American Society for Testing and Materials Committee G4 on Compatibility and Sensitivity of Materials in Oxygen-Enriched Atmospheres.

There are many who praise peer review (perhaps disingenuously) as a way to keep science honest and productive. However, peer review can also be abused and can have a diametrically opposite effect to keep science dishonest and unproductive. Examples are rife. The Wikipedia definition would be more accurate if it were to add the qualification:

“However, it has also functioned equally well as a detrimental tool to misinform and control discourse (i.e. for science fraud).”

This commentator has also often heard another related aphorism: “yes, peer review has flaws, but it is the best system we got.” Indeed, during two of the egregious case studies addressed herein dating to 1995-1998, the Chairman of the ASTM Committee on Publications, Kenneth R. St. John, defended an organizational devotion to its peer review mechanism process seemingly clueless or uncaring about any abuses that could or *had* occurred or might occur and did sustain into the future, perhaps even to this date. The St. John/ASTM perspective back then was that “high quality technical publication are the direct result of thorough peer review” and that “it would be completely unreasonable to remove the rejection option from the reviewers.”

Those who subscribe to this lofty affirmative perspective need to weigh not only the good that is done, they need to weigh the bad. They need to weigh if this mechanism is abused (as I shall allege and argue was the cases herein) and leads to a direct result of *lower-quality publication* or wrongfully censored non-publication, *or worse*, that is to say censorship of important material. In the case of ASTM G4, one must ask how much death and injury, capital loss or interrupted production counter to the mission statement of ASTM G4 is acceptable due to peer review “flaws”. This commentator is pretty sure that those who die in oxidant incidents are every bit as dead as those who are shot with misused firearms.

Hence this commentary is about when and why “the best system we got” leads to potentially un-remedied harmful consequences that are at times hazardous. In terms of the ASTM Committee G4 mission, that is to say it can work (and has worked) for *and against* saving lives and injuries, capital and production losses.

Nonetheless, in this judgment, in this blunt opinion, this peer review, the 1995-2026 ASTM Committee G4 peer review system and its ASTM oversight has been deplorable and needs remedy that may not be fully possible. It did not start out deplorable, it was corrupted⁴ in an insurrection, a coup d'etat in the mid-90s. In this judgment, it has become a crucial impediment to the otherwise noble and life-saving effort to achieve oxidant safety. G4's agents of corruption have not been the worst, but sadly have not nearly been trivial. So this opinion hereby damns both ASTM and G4 and specific actors. This opinion/peer-review builds a large case against them. This fifty-year G4 member fears the damage that has been done is irreconcilable and irreparable and to the extent that the works of Committee G4 were once upon a time dedicated to its trinity (save lives and injuries, capital and mission) that goal has been impaired, perhaps grievously.

⁴“Corruption” being used here in the sense of abuse of entrusted power authority or position for personal gain or private benefit involving unethical conduct.

It is perhaps curious that in the world at large, today, something akin to “peer review” is also being called things like “disinformation panels” that seek to facilitate censorship of information that has not been approved by the overlord cognoscente peer powers. And indeed, those who endorse ASTM/G4 peer review, may honor this commentary as very much a peer review of *them*. A peer review of the peer reviewers, but one without the censorship power and intent like that which has been exercised within and hangs over current ASTM/G4.

People Problems

This angry diatribe examines the potential reasons why peer review that is capable of doing so much good is vulnerable and prone to so much abuse and how G4’s peer review efforts came to be corrupted. While base malice and outright fraud are real things, those reasons of most potential interest here seem more like what is called narcissistic personality disorder (NPD)⁵ that can include perhaps principally flawed people problems like: The Cognitive Biases of Dunning Kruger Effect and Confirmation Bias, promotion in employment, but also tenure and dissertation pressure, and this worker’s favorite, the world-wide quality movement.

And in the case of Committee G4, this worker believes a self appointed cadre of members he calls NASA+ (some but not all NASA workers, some former NASA workers, NASA contractors, groupies and minions) have become the core of its people problems. Although NASA+ workers have always taken great and justified pride in their substantial even awesome skill sets, this worker adjudges that in the late 1980s the NASA+ egos were getting out of hand, and they were variously exhibiting many of the cited characteristic elements below in some cases to extremes incompatible with consensus based activities.

*Cognitive Bias (CB)*⁶ is cited as “systematic error in thinking often based on faulty memory or experience”. However the “error in thinking” can also be deliberate, even devious. This may seem a curious soft-science heading in a paper that seeks to be purely scientific like this paper will try to be. Nonetheless, CB is real and this worker concludes cognitive bias and two of its characteristic elements: Dunning-Kruger Effect (DKE), and Confirmation Bias (CB), may be (have been) important and demand a good bit of description to provide context and defend and characterize the material to be reviewed herein.

*Dunning-Kruger Effect (DKE)*⁷ is cited in the verywellmind website and elsewhere as:

⁵ “As of this writing, The Mayo Clinic website cites characteristic elements of NPD that this worker finds uncannily consistent with the NASA+ clique including: “unreasonably high sense of their own importance”, “need and seek too much attention and want people to admire them” but very importantly “behind this mask of extreme confidence, they are not sure of their self-worth and are easily upset by the slightest criticism”. The Mayo Clinic site lists 21 symptoms that are all too familiar to this worker.

⁶ <https://www.verywellmind.com/what-is-a-cognitive-bias-2794963>

⁷ verywellmind website: <https://www.verywellmind.com/an-overview-of-the-dunning-kruger-effect-4160740> which in turn cites McIntosh (McIntosh RD, Fowler EA, Lyu T, Della Sala S. Wise up: Clarifying the role of metacognition in the Dunning-Kruger effect. *Journal of Experimental Psychology: General*. 2019;148 (11):1882-1897.]

“a type of cognitive bias in which people believe that they are smarter and more capable than they really are. Essentially, low ability people do not possess the skills needed to recognize their own incompetence. The combination of poor self-awareness and low cognitive ability leads them to overestimate their own capabilities.”

However, it is also possible for the smartest and most capable of people to believe they are even smarter and more capable than they really are, perhaps as a result of DKE. Sometimes,

“It may be plainly evident to everyone in the room that this person has no idea what they are talking about, yet they prattle on, blithely oblivious to their own ignorance”.

And yet sometimes:

“no matter how informed or experienced we are, everyone has areas in which they are uninformed and incompetent. You might be smart and skilled in many areas, but no one is an expert at everything”.

Indeed, as a result:

“People who are genuine experts in one area may mistakenly believe that their intelligence and knowledge carry over into other areas in which they are less familiar. A brilliant scientist, for example, might be a very poor writer.”

It can be daunting to work with Dunning-Kruger Effect egos in a collaborative environment for in this worker’s experience DKE’s can be resistant to all external inputs, and take great (NPD-like) umbrage when their proclamations are not heeded. Do not ever try to disagree with them (see later Case Study 1). In this experience, status (being lettered, employed by a prestigious concern [like NASA], being a distinguished professor) can magnify the effect to the point of thinking their superiority is tantamount to infallibility and leading to discounting all input, including valid input, entirely.

*Confirmation Bias (CB)*⁸ is cited as “favoring information that conforms to your existing beliefs and discounting evidence that does not conform.” This worker translates this to mean being deaf to that with which you disagree. This is toxic in a community where conflicting opinions and beliefs *must* both thrive (see later Case Study 2). CB is likely the point where DKE victims begin discounting all outside data.

Academic Career Success (Dissertation Pressure, Academic Promotion, and Tenure Pursuit), by its nature fosters a mindset and urgency that is narcissistic. It is in many ways antithetical to collegial and collaborative efforts. Industrial demands have their own career-success corresponding focus but it does not impact ASTM G4 in the same way. Indeed, this worker had found the industry career pressure often shifts the focus away from efforts with

⁸ <https://www.verywellmind.com/what-is-a-cognitive-bias-2794963>

G4. So for some G4 members, *Publish or Perish* motivates an intense desire to be *the* SGR (Smartest Guy in the Room), to publish much and often and to consider fellow members competitors for the credentials they covet that translate into career success. This can compromise, even compete with, the goals embodied in the public service G4 mission trinity.

Quality Pressures are effected upon yet another sector of the G4 membership. In the 1980s “Quality Process” became a crucial driver of workers. Rule 1 became a prime ethic. “The customer is always right, ...even if he is dead wrong”. And within ASTM Committee G4 NASA was the biggest customer. Numerous members were sponsored by companies that had serious contracts with NASA, and many more wanted some. This gave NASA workers in particular a degree of influence that was at best unhealthy. Allan McDonald [I]⁹ describes his worries when he was opposed to the fatal launch of Space Shuttle Challenger in 1986. If it did *not* blow up, would he be fired? Would NASA take away his company’s contract for his insubordination? Numerous members of G4, including this worker, have experienced similar, albeit less grave, situations. Add to this the legendary reputation of NASA (alleged to have gone to the moon, more than once) it put NASA workers in a situation of undue influence, even if not sought by those workers. However, in this experience, many NASA+ workers largely relished and sought and wielded as much influence as possible. Push back was not appreciated. Indeed, today this commentary is only submitted because for the last 26 years this worker has been retired and has become NASA’s customer through the taxes he is paying. ASTM does not appear to have resolved, or even recognized, this dilemma.

This commentary and its related website have been a *very* long time in coming. The delay by this commentator was not specifically intended to let the ASTM/G4 censorship take enough rope to hang itself, though it has taken plenty, but rather to allow it plenty of time to clean up its own mess. It has not. Deference to customers like this was a major ethic in the “quality” 1990s. In the 1980s, the vogue “quality revolution” had created a dominant-customer/supplicant-vendor relationship. NASA was a major customer and many ASTM/G4 co-members (like this worker) were employed by vendors loathe to challenge NASA and related workers even when they were wrong, ...even when lives were potentially at stake.

Conflicts of Interest. Career success and quality process produced conflicts of interest outside of ASTM/G4 ideal operations. However, now in this paper, this perspective is offered as a taxpayer/micro-customer. Over the years since then, this worker has shifted from a cog in the commercial wheel, and a clog in the promotional pipeline, to a taxpayer who has a legitimate interest in how his largesse is being used. That is to say, a shift from vendor to customer (those things that are always right, even when they are wrong). Furthermore, former deference can allow and I believe has allowed the NASA+ sphere of influence to control and corrupt the definition of “standards” shifting it from a full or at least largely held consensus opinion to a defined-consensus opinion reflecting too much NASA+ control and imposing too narrow an aerospace perspective (which this worker argues hearkens back to the Hydrolevel/ASME issue of years bygone).

⁹ Bold-face, italic numbers in square brackets refer to the References list at the end of the paper.

Early Reality Bites.

This worker has had the distinct honor and pleasure-of-a-sort of working with and observing the thought patterns of some truly world-class intellects and scientists in the oxidant safety field (and others as well). Irvin Glassman, Chester Grelecki, Clyde McKinley, P. L. Thibault Brian, Jack Gilbertson, to drop just a few important names in the field. I have witnessed how they do science. But I have also endured the onus of working (if one can call it that) with some highly credentialed people that seem to exhibit all of the biases cited above. All of these latter examples qualified as scary smart, even smartest guys in the room.

On one occasion, working on a low rung on the ladder at the historically prestigious Bell Telephone Laboratories in 1968, this worker participated in a meeting led by a wunderkind from HQ, where at that time Penzias and Wilson (1964) had just discovered the cosmic microwave background that is such a big deal today in cosmology and would in 1978 win them the third of something like eleven Nobel Prizes coworkers had and would receive, if that sort of thing impresses anyone. It was when the drive to integrated circuits was in full thrust and our goal was to pack more circuits on smaller and smaller substrates.

We were in a race to the moon (for some unseemly reason) and while the Soviet Union had much bigger rockets, they needed bigger rockets because they had no miniaturization technology. And not only did Bell Labs lead in integrated circuits to make telephones work better (we were entering the Touch-Tone era) but ATT was the prime contractor for the anti-ballistic missile multiple-array radar systems needed to sort dummy MIRV warheads from real nukes.

It became quickly apparent to everyone in the room, except the wunderkind, how to pack a fifth circuit onto a thumb-sized 800A switching circuit substrate (the core of the then-recent Touch-Tone technology) that at that time had only four circuits (the roughly one square inch substrate today would contain millions of transistors). Yet our wunderkind clearly had no use for anyone else in the room. His DKE and CB was thorough. All of our inputs were not being heard. Over and over the solution had been delicately broached without impact until finally, while leading us to lunch, something clicked in wunderkind's brain and he threw his hands up in Messianic pose halting our movements, turned and proclaimed shouting as I recall "I got it." (rather than the more traditional "Eureka"). And he explained to us dolts how to squeeze in an extra circuit. He was very proud of *his* accomplishment and of *he* himself, no thanks to the dolts. In that case I found myself the lowest ranking guy in the room (as often happened including within ASTM Committee G4) with a revived appreciation of how eggheads can foster animosity for themselves. Hence if academic credentials are the only thing that impress the reader, this would be a place to exercise your own cognitive bias and stop reading. You will not like the rest of this essay.

This is to contend that ASTM Committee G4 morphed into and has for some time operated as that kind of classist society in which a cabal of admittedly super-smart, self-anointed smartest guys in the room, wunderkinds all who see themselves as Alpha Wolves, have assumed what they see as their rightful place at the top of the food chain. And at times they can seem so weary with the dolts among us. Yet in my judgment I allege and conclude,

and shall explain my bases, that anyone is free to ignore, that serious cases of group Cognitive Bias, specifically Dunning-Kruger Effect have lured these elites into confirmation bias and peer review abuse. You get to weight the facts and decide for yourself, the same as you get to balance the hazards and risks (those that you are these days allowed to know about) in designing oxygen systems though I suspect they would prefer you just do what *they* tell you.

And yet this worker is fearful, for these cases are not the only downsides that he has witnessed. Sadly there are cases where piercing of severe Dunning Kruger Effect among the super smart can have devastating consequences. Some of the smartest and most capable people this worker has known were brittle, have literally shattered (much like glass shatters) when their facades of superiority that they have depended upon proved imperfect. Some may need omnipotence to sustain them and lacking or losing that may just (as has been witnessed) throw in the towel. Drop out of college. Do drugs. Change professions. Even end their existence. Yet there is a limit to how much charitable accommodation is reasonable and tolerable. So one can only hope a Butterfly Effect result does not come from this too-long-deferred exposé.

G4's Auspicious Beginnings

In the 1980s, G4 decided to establish a symposium series and as designated volunteer, the planning fell to this worker. Ultimately symposia fell under the Education subcommittee and for a time I was its second chair. Papers at these symposia were “peer reviewed” using the basic ASTM mandated system. That system gave G4 the ability to wean out commercial papers, or totally irrelevant materials, or previously published materials, all good things to do. It also allowed reviewers to request changes (even calling them “mandatory”) *or to reject manuscripts outright, in some cases even labeling them unredeemable*. And in the fourth symposium, 1989, G4's first serious technical controversy obtained. A paper submitted for STP 1040 by distinguished, respected and regarded authors, Barthelemy, Delode and Vagnard [2] made a statement: “aluminum pressure regulators can be safely used in oxygen service equally well as brass regulators”. Peer review took exception to the scope more so than the sentiment of the comment. This worker (not among the initial protestors) also felt the assertion might lend a false impression to some less-sophisticated readers. Yet it was felt the writers were entitled to express their opinion and that was their wish.

As a result, lead editor Joel Stoltzfus (a NASA+ mainstay) and ASTM Committee on Publications contact for STP 1040, Sheldon Dean (a co-worker of this worker), invoked the addition of a “Discussion” to the published paper [as a form of rebuttal] in which the G4 Committee as a whole cautioned the reader not to take the statement as equating the overall fire behavior of aluminum with that of brass, or even the specific compatibility of aluminum regulators, to that of brass regulators:

“a well designed brass regulator offers advantages when exposed to a number of ignition stimuli, and indeed in any subsequent fire.

In many regulator applications, it is very difficult to ensure that the author's stipulation on cleanliness is met throughout a regulator's life. Hence while

aluminum may give satisfactory service in a number of carefully selected applications, our peer reviewers are concerned that the author's conclusion might be extrapolated more broadly than is justified in cases of severe regulator applications.”

The Main Committee at the meeting voted on the discussion and it was published under G4 Chairman Dr. Robert Lowrie's signature. The author's accepted the clarification graciously and in this view may have actually appreciated it for the topic of the aluminum hazard and the two perspectives on its scale continues to this day (2026). Just a few years ago (about five) this worker cast a negative ballot on a Standard G 94 revision regarding flagging of aluminum hazards therein (that remedy still pending).¹⁰

G4 standards were and still are considered voluntary materials that users choose to use or reject for their own use. Hence there is not just one solution to any problem. In this case users had and deserved to be advised of both perspectives and any dissents thereto to choose from or reject as they see fit. This was a full-consensus solution, not that there was agreement on the precise technical content but that there was full agreement (consensus) that there were differing views both of which had merit and were adequately presented.

At about that time (late 80s), this worker as Chair of the Education Subcommittee which at that time included the symposium effort, received a phone call from NASA WSTF's Frank Benz indicating that Dr. Ted Steinberg, Dr. D. Bruce Wilson and he had submitted a paper to *Combustion and Flame* that had been rejected by a distinguished peer reviewer, a peer reviewer whose work their paper had challenged. What could I do to help them? G4 had no influence with Elsevier, the publisher, but at a regular meeting of G4 (probably the 1990 meeting), we considered both of these events and how G4 wanted to manage its own forum.

We recognized major difference between G4 and forums like *Combustion and Flame*. Their audience was huge and largely academic researchers. Our's was largely non-academic with engineers and mechanics and industrial workers and consumers and just a few academics. Furthermore we needed input from all of them and needed to rely on all of them for our forum. Still furthermore, we recognized diverse opinions and approaches to oxidant safety. Our previously discussed episode anticipated that we might need to provide two or more perspectives on numerous issues potentially with rebuttals or discussion addenda for context. Our audience was voluntary oxidant safety practitioners, who deserved to know *all* perspectives.

Indeed, whereas publications like *Combustion and Flame (C&F)* are quite prestigious and are viewed as “publish or perish” vehicles with vast numbers of academics seeking to have their works and analyses published to enhance their stature, inflate CVs and their position, their promotion and tenure situations, G4's vehicle was exactly the opposite. *C&F* might sell ten or even hundreds of thousands of copies *each* month while G4 has often

¹⁰ In 2021 this worker cast a negative ballot on aluminum hazards regarding ASTM Standard G94 and for about four decades has been apprehensive about the use of aluminum in NASA and especially in the LOX tanks of the space shuttle and now SLS vehicle.

sweated if a given symposium would produce enough papers to warrant publishing a book at all and if it did it would be lucky to sell a thousand copies in its entire print lifespan.

Furthermore, we realized in part from the *C&F* case how destructive and alienating a rejection could be. Author umbrage and indignation was extreme. But just as publication in *C&F* might benefit an academic worker's career, it would have little benefit to workers in the oxidant safety community whose principal function had nothing to do with publication and Curriculum Vitae (CVs), but an effort to publish a paper and have it rejected would clearly be very damaging. In other words, we needed them but they did not need us.

Ultimately, *C&F* published the contested paper [3], but allowed a rebuttal by the reviewer Prof. Dr. Irvin Glassman [4] whose early work it disparaged and a closure by them [5]. In this worker's opinion, the original paper challenged Glassman's early work in a fashion that had validity but also was flawed and came with a snide feel. Glassman's rebuttal similarly found valid flaw in their critique but by that time was likely felt to have its own snide flavoring, that the authors closure again responded to with even more snide. The exchange was unseemly. G4 leadership back then sought to avoid such unpleasantness.

As a result we adopted a "sense of the committee" posture that recognized reviewers need to flag commercial, irrelevant, copyrighted, or previously published materials, and in the possibly faulty memory of this worker there had been only a few papers that failed that kind of muster in G4's past. However at that point, virtually every author for every other paper to date in the forum was considered a "distinguished, respected and regarded resource and colleague". So we discouraged rejection on technical grounds specifically because as we had seen in the Benz, Steinberg, & Wilson case, rejection taken so *personally* had engendered rather severe reaction and hostility because rejection is a rather defamatory and even insulting gesture unlikely to foster the increased participation and growth of our small community that we needed. Indeed, we deliberately chose to *not* presume that our reviewers or even our community was in a position to dictate absolute accuracy, but the option to present alternative perspectives where there was debate as had been done in 1989 was always an adequate and desirable incentive to collaborate.

This approach was not broached to ASTM at large, nor was that deemed necessary for G4 had been a very cordial theatre and the "sense of the committee" approach allowed us to cooperatively identify and flag suspected (even strongly felt) dangerous or erroneous or just embarrassing mistake issues or missteps *as a service to the authors* as much as to the audience. And perhaps, most importantly it allowed us a mechanism to recognize, address, and amicably deal with controversial materials. G4's products were always based upon consensuses. In the earliest of its days the consensuses sought were "full agreement". In developing voluntary safety standards, no plausible perspectives were censored. Hence peer review is capable of doing great good. This resolution continued and was revisited at times but was not tested until 1995 by this worker. In 1995, the consensuses were unilaterally shifted into controlled dictates from a clique of committee bullies and actual censorship became a fact. And that brings us to a series of case studies for context.

Case Study #1 (The Personal Mega)

In 1995, this worker was skeptical of a published NASA+ discovery of “excess oxygen” in burning iron slag [6] and with a coworker (Joseph W. Slusser) who later withdrew attempted to capture some excess oxygen, but obtained results that were not consistent with its existence. The results were presented as a paper [7] at a regular Fall ASTM G4 Committee meeting (1995) as part of an effort to reinvigorate¹¹ its Seminar Series forum designed to enable a greater volume of papers and less burdensome peer review crush for its publication series. It would be the first paper to experience peer review long before the next symposium scheduled for Fall 1997. What followed was:

- Days later, a fax arrived asking for a phone conference to which was attached a mathematical analysis disputing my paper (incorrectly) and a NASA+ paper that was undergoing peer review, was revised and later published as reference [8] that had been submitted to *Combustion and Flame* that was claimed to lower the estimate of excess oxygen in slag (but did not) and that was undergoing peer review.

- The phone conference was with a gang of people at NASA+ WSTF that can be best described as a technical intervention (like are used with drug addicts or alcoholics) all of which insisted my paper must be withdrawn for being wrong. Were they trying to spare me embarrassment? Knowing the caliber of corporate people who had already reviewed my findings before approving it for presentation, I defended it and declined. If this conclave was *a service to this author to spare me embarrassment as opposed to an intervention and censorship effort, that should have been the end of it?*

- Two of three assigned peer reviews had no big problem with the paper, but one peer reviewer (Dr. D. B. Wilson of NASA+) rejected it for publication for technical bases. That peer reviewer had conflicts of interest, he was the PhD advisor for the worker (Dr. Ted Steinberg) whose dissertation discovered the “excess oxygen” and was a coauthor of the C&F paper [6] that proclaimed the discovery. He was also the same interventionist whose analysis had claimed my paper was wrong, but who admitted a revised analysis no longer supported that proof. Nonetheless, without completing the review, the reviewer filed a rejection of the paper for being wrong and appeared to assign or request yet another peer reviewer. His and the later peer review may warrant a detailed peer review for a future paper.

- The new added fourth peer reviewer also suffered from conflicts of interest: was a coworker of Dr. Steinberg at NASA WSTF and was at the time a coauthor of the follow-up work to C&F [8] attempting to verify the earlier excess oxygen discovery (but does not). His peer review ravaged my paper with claims of massive and even egregious error throughout

¹¹ In 1995, the G4 forum and its energies were flagging. Fewer papers were being offered at its seminar series and symposia, peer review was becoming too onerous in a hectic period before every symposium. The prospect of increasing the interval between symposia or even ending the activity were being considered. Instead, a decision had been made to (1) identify and distribute a list of “gaps” that needed filling in the Committee’s work so that targeted presentations could be pursued, (2) we would launch peer review of papers presented at meetings immediately to reduce the peak rate of work if not the total amount of work needed from our small peer reviewer base, and (3) to thereby feed some number of “finished” papers into the can so that our symposia would not face inadequate content volume in the future yet with less hectic demands on the community. This change is detailed in the overview to the 1997 *ASTM Special Technical Publication 1319*.

while admitting in bold face type: “these observations are very important”. However, he also alleges this worker was so “careless and sloppy” and played “fast and loose” with the details. It was so massively negative in its rejection I felt I could not withhold it (cover it up) from my management but submitted it to them with my own peer review of his review in which I disputed *every* claim he made some of which bordered IMHO on being ludicrous.

- My management noted that NASA was a good customer of our company and as a “quality” company “customers are always right ...even when they are wrong” and that created an uncomfortable situation. I was advised my employment was not at risk and they decided to let the peer review process play out.

- For about a year, reconciliation of the paper was coordinated by editor Ting Chou who supported publication but who kept reporting strong opposition from rookie editor Dr Ted Steinberg in his first role as a G4 editor. Ultimately Dr. Chou withdrew in frustration.

- Reconciliation passed for the better part of another year to seasoned editor William Royals (letters uncertain) who also reported support for publication but who also indicated extreme resistance from Dr Ted Steinberg. Ultimately, Royals reported that Dr Steinberg has said if the paper were to be published he would see to it that every NASA paper in the symposium would be withdrawn. Royals noted that without the NASA papers (more than half) the whole symposium proceeding could be cancelled. So he also withdrew in frustration.

- Finally reconciliation passed to Dr. Steinberg, himself, undaunted by his conflict of interest as the discoverer of “excess oxygen” yet feeling no need to recuse himself. So with the final drop-dead date for revised papers looming weeks away, Dr Steinberg sent me a letter giving me three options

“Some action is necessary prior to acceptance of this manuscript for publication. It appears, after discussion with various individuals, that there are several ways to proceed including; A) withdrawing the manuscript, B) Rebutting the comments of Reviewers #1 and #2, to the satisfaction of the STP Co-editors, and the manuscript will be published as is. C) Incorporating the ideas/comments of the reviewers (#1 and #2) and submitting a rewritten manuscript for consideration. A combination of B) and C) would, of course, also be acceptable.”

“...Another option has also been presented to the editors during the original review of this manuscript. One of the reviewers (#2-Dr Kurtz) thought the data presented was very interesting and unique. This reviewer volunteered, along with the submission of their review, to aid the original author in the rewriting of this manuscript due to the problems he saw concerning the presented data. This option would essentially result in option C) outlined above. Should you choose to pursue this option, the reviewers contact details would immediately be supplied so the manuscript can be quickly modified to meet the publica-

tion deadlines imposed by ASTM.”

“...Since we feel we may have delayed in responding to this issue, we are prepared to extend our publication deadlines to the maximum extent possible to allow you ample time to address our concerns regarding this manuscript.”

Since two of the editors had already agreed to publication, this meant Dr. Steinberg would be the sole arbiter of publication as he had been all along. Due to a perceived anticipated tad of difficulty in convincing Dr. Steinberg to allow publication as well as a similar tad of doubt as to the nature of any rewrite by my most negative peer reviewer, I withdrew the paper. “I could read the writing on the wall”.

Note that the list of options did not include publishing it with a rebuttal and closure, as Dr. Steinberg and his peers had enjoyed in 1992 with their rejected paper in *C&F* and as had been exercised by his coworker and ally and co-interventionist Joel Stoltzfus had done as lead editor for the earlier problematic paper in STP 1040 in 1989, just two G4 symposia earlier. In that earlier symposium was a publication in which Dr. Steinberg was coauthor for the first paper [9] (possibly one of his own first papers in the G4 forum), a paper that first speculated *in error* that excess oxygen *must* be present in steel combustion.

About a year later (six months after the symposium) I requested and received permission to distribute the paper as a corporate publication at the 1998 G4 Meeting in Atlanta. I then moved a good bit of heaven and earth to attend that meeting for this and several other reasons including the resolving of the issues in Case study #2 below. And so at the 1998 regular G4 meeting with Dr. Steinberg, himself, in attendance, I provided copies revised to acknowledge the peer reviewer comments and reminded the Committee of its goal to “no fault” publication and its merits. At that time, Dr. Steinberg stressed a concern that the G4 forum had no credibility in his circles (the academic community). Since then Dr. Steinberg has continued as an editor of every G4 symposium publication since and enjoys something of a Head Editor reputation (officially or not).

In addition, in this judgment the content of G4 proceedings ever since have taken on a distinctly academic tone. Numerous papers are included by postgraduate candidates for whom Dr. Steinberg was their advisor often with Dr. Steinberg as a coauthor and with Dr. Steinberg as the volume head editor. And this worker can not but note the unseemly appearance of G4’s forum being a self-publishing tool for the good doctor. Whether improper or not the appearance of impropriety is unfortunate. And while all of those papers are and should be welcome, there has been, again in this judgment, a distinct decrease in the number of practical papers of the kind that were often present prior and often sought most of all prior to Dr. Steinberg’s arrival.

Case Study #2

The earlier cited 1995 reinvigoration of the ASTM G4 seminar series and symposia (footnote 11) presented this worker with a motivation to anticipate and prepare for his own retirement. While soliciting and collating a G4’s “gaps” listing this worker began a corpo-

rate audit of topics within his sponsor company that could form useful papers nobly benefiting the entire oxidant safety community and G4 via the G4 forum. A primary goal was to facilitate ways to present papers remotely so that presentation-in-person did not consume days of travel away from the job and expenses to share technology. Virtually every co-worker tagged was functionally nonacademic. Not one had a personal incentive to publish or perish, and indeed if anything saw publishing as a risk rather than a remedy. In addition organizational support was provided to presenters so that generating and formatting text and the like would minimize impact upon their competing primary duties. And personal assurances were given that presenting at G4 was not burdensome but was more like sharing within a warm and friendly nurturing camaraderie that would help save lives. Innocent lives. Yes, that was guilt-tripping, ...and it worked.

A Corporate list of about thirty+ topics was generated along with target workers to cajole, guilt trip, and even extort into presentation. Indeed practical benefit rather than academics were a focus and the potential presenters boasted several hundred cumulative years of air separation industry experience, and many of which would therefore soon be approaching their own retirements and potential company and world-wide loss of their skill-sets. Many were role models.

Several of these topics [10-15] would publish in STP 1319, some of which were remote presentations that were made during regular G4 seminar sessions, and were advance-peer-reviewed and in the “can” long before the 1997 symposium. In this way they helped assure sufficient papers for a publication, and very crucial to the Committee at the time, helped to spread out the time needed for peer review, ...helping to achieve, *for the first time*, a long-time goal of having the publication finished in time to distribute *at* the symposium. One paper suggested approaches and considerations to design distance-volume pieces [12] and an expansion of the G4 PC utility G4Math (now defunct) was prepared and balloted to support users. Another was an approach to avoiding the use, risk and expense of one-time-operation bypass valves [15] and again an expansion of the G4Math PC utility was proposed. This second paper was during the tense period when the peer reviews of Case Study 1 were still raw and still chafing. And one of the negative peer reviewers (Dr. D. B. Wilson) was again a peer reviewer and he *again* cast yet another rejection. Indeed, his opposition may have clogged the process. His somewhat angry argument was that the authors (D. G. Castillo and I) used an incorrect inappropriate equation (The “Bernoulli Pitot-Tube Equation) in our calculations and in the PC utility. I was at that point involved in two paper rejections that may have been a record for both G4 and my employer. This was received by both my coauthor and I as an assertion of incompetence. I ultimately found my self on a phone conference hearing data produced by a Crane Valve Company valve selection PC utility being recited to disprove the paper. At that point unkind words were exchanged and I committed to present a paper proving the equation was correct at an early G4 meeting after the symposium. The paper [15] was reluctantly allowed and a revision of the G4 PC utility (G4Math12) was later balloted to facilitate use of the strategy.

That equation (the bone of contention) was Bernoulli’s pitot tube equation for predicting the pressure produced by an impacting compressible gas stream, an equation not

quoted in many references at the time besides Perry's [16]. In our paper we used it not to determine the pressure rise of a gas stream impact, but to rather insert the total pressure and solve back for the incident stream velocity that produced it and therefore was the velocity it could produce in a reflected flow. Indeed this is reflective of the equivalence commonly employed in piping design between the stagnation (total) pressure a gas exerts impinging on a surface which is related to the velocity it can produce if reflected off that surface. It is perhaps shocking that the NASA+ entourage (including NMSU Professor Emeritus Dr. D. Bruce Wilson and self appointed lead editor Dr. Ted Steinberg) were acting as though they were clueless about. Is that possible? Indeed, the decision to take this route was not that of my coauthor and I. It had been decided years earlier in proposing criteria for using fire limit test data in piping design in a paper in 1989 published in a NASA+ edited ASTM G4 STP 1040 [17] by coauthor and future full-Committee-chair J. W. Slusser. And furthermore Slusser had confirmed its use with not one but two Professors Emeriti (PE) from MIT. In other words, this worker was pretty damn sure the equation was correct as I defended us against Dr. Wilson's latest repudiation. But I enjoyed the claim that the two MIT PE's were wrong.

This worker researched the origins of the equation dating back to the work of Prandtl in the 1920s, and presented the promised paper [18] at the Spring 1998 meeting and followed up with a further exploration [19] of its implications. Unfortunately Dr. Wilson was not in attendance to learn about this apparent gap in his knowledge, but thankfully, key NASA+ member Dr. Ted Steinberg *was* in attendance and it was a privilege to tutor those concerns. The equation has since been included in ASTM standard G88 for that exact use. Those papers were *not* provided for future publication within the G4 forum, ...fool me twice.

Dr. Wilson's rejection and rather un-collegial and un-collaborative hostility (he was a core NASA+ ideologue and interventionist) had thoroughly undermined the G4 image I had vouched for among my company peers. Furthermore, I can assure you I did not welcome this second (perhaps apparently strategic) condemnation any more than the first condemnation of case study #1. Was this an effort to intimidate me? Distract me? Play committee politics? Whatever it was, it was not helpful.

I can fully assure the reader I was not again able to sell coworkers on the idea of a warm, friendly G4 forum. In the 27 years since my retirement, it appears only two or three papers have published from that community. What a loss! Meanwhile few if any of those seasoned veterans remain active. Some are fully gone. And in those 27 years I judge that the flavor of papers in the G4 forum has shifted away from practical advice to measurements of angels on the heads of pins. Those "angel" data may not be irrelevant, but I consider that a shameful shortfall in the Committee's mission to fight injury and loss of life, loss of capital and loss of production. This is tragedy but even more so a damn shame. Hopefully the academics got their full benefit from the new forum they has seized.

But in a greater sense, this worker's mind has been boggled at the extent to which the NASA+ cognoscente were willing to go to prevent questioning of their axiomatic excess oxygen "discovery". And it is difficult to believe that so many highly lettered workers were ignorant of the equivalence between static and dynamic pressure. Hence one can only won-

der if this second defamatory rejection was a deliberate exercise of the flaws in the “flawed but best system we got”? Perhaps Dunning-Kruger Effect is the motivation that fits best?

Case Study #3

Case studies #1 and #2 relate to peer review abuse to *censor* papers that challenged NASA+ hegemony. This third case study involves peer review abuse to *facilitate* a paper that supports NASA+ hegemony. The former involved assigning conflicted (and surplus) peer reviewers to condemn and defame unwanted submitted papers, the latter involved avoiding and thereby censoring dissident peer review that might condemn wanted papers. In the process, because of case study #3, G4 Standard G124 was, in this worker’s opinion/peer-review, rendered unfit for use.

In 2008 or 2009, NASA+ wanted to reduce the amount of combustion used to indicate a fire-limit (the self-sustaining propagation threshold). For nearly two decades, NASA+ had repeatedly sought to change the G124 standard’s burn criterion from “complete” combustion of a 6-in./150mm specimen to various short burns ultimately settling on 30-mm: just 20% of the specimen length.

In the early 1980s, this worker was participating as an invited guest in a NASA WSTF RTOP (Research Technical Operating Plan) committee and was promoting therein as well as within ASTM Committee G4 the use of fire limits for ranking of metals using the same spirit of flammability-limit testing that dates back a century or two and that was showing great promise at the time in his own work [20] and especially in new testing by his co-worker, John Zabrenski whose data would be published in 1987 [21] and 1989 [17]. Initially, NASA+ workers were not especially interested in this push for a fire limit test and were instead initially advocating fire spread-rate testing as a way to rank metals [22]. However, industry-sponsored testing at NASA WSTF [23] insisted that test schedules allow for at least coarse inference about fire limits that proved the value of such data, and NASA+ came to not only agree but also came to a somewhat narcissistic assumption that they were not just the creators of the G124 procedure but were authoritative as the “creators” of this fire limit testing approach.

Fire-limit testing (called flammability-limit testing for materials that exhibit flaming combustion) has numerous origins including massive works from the U.S. Bureau of Mines [24,25] for gases and some powders, and the development in 1970 of the ASTM Oxygen Index test (D2863) for bulk materials, but the important NASA contribution was a test vessel design (developed for their fire-spread rate testing) that was used as an example of a suitable (but not exclusive) vessel and which was also the vessel in which the basic data they collected under contract for G4 for standard G94 were measured, as well as much of the basic data for the G 124 testing to this day. However, the amount of testing they had conducted at the time was but a small fraction of what Zabrenski [21,17] had already done prior to the creation of G 124. Due to a number of surprises these kinds of data presented, in some cases the NASA+ early learning-curve understanding of it was in those cases simply flawed.

Coworkers and this worker had gone through similar stages of flawed surmise in our

early learning curve. And I, and others, tried to share some of those results with the NASA team at the very first RTOP meeting and several times in several ways thereafter.¹² However, NASA+ workers have tended to be resistant to outside input. In this judgment they tended only to believe their own work (to hear their own voices ala Dunning-Kruger Effect), and perhaps most tragically even when their own results were in error. Because of this worker's situation, charter, and experience, this worker believes some of Zabrenski's discoveries are as important as any oxidant safety discoveries in the past hundred years. Zabrenski's results explained and foreshadowed critical technology and helped explain incidents spanning two decades. Yet NASA does not appear to this day to have discovered some of them either on its own or in reading materials from outside its own bubble (or hopefully is being curiously or coyly silent about them). Perhaps this is more confirmation bias (cherry-picking)? In some crucial cases, NASA+ does not appear to have embraced these data unless that embrace has for some reason been kept deliberately secret or obscure.

In this worker's early G4 participation he was an industrial worker for whom NASA was an important customer, and industrial quality practices ("the customer is always right") were a serious deterrent to open dispute, even when NASA was wrong technically. So even important advisories were always discreet, polite and deferential. The data were made available in the exact spirit of ASTM G4 *voluntary* collaboration, but NASA+ workers accepted or rejected it (often appear to have ignored it) and in some cases (like those addressed here) where there was disagreement, it appears they not only did not accept but they actively sought to suppress dispute. This made collaboration within the ASTM paradigm difficult and at times impossible. Today this continues to be a serious bone of contention, especially because lives and more are literally at stake.

In the early 1990s, the first ballot of Standard G124 was issued and NASA+ worker Joel Stoltzfus was the team leader and had proposed a short burn criterion. This worker cast a negative ballot based upon (1) this worker's experience, (2) Zabrenski's extensive data [21,17], (3) the open literature (especially Bureau of Mines testing [24,25] and (4) text-book teachings on flammability testing such as Glassman [26] but most especially because in anticipation of the ballot, (5) this worker, himself, had performed a rather long post-Zabrenski sequence of tests (in normal gravity), one of the longest to date in the 1980s (not formally reported) specifically in support of selecting the burn criterion for the forthcoming original NASA drafts of G124 in those early 90s. These bases and the last-cited tests clearly disputed the validity of a short criterion. They demonstrated that, even as in traditional flammability limit tests of other materials significant short propagations can result when there was little to no chance of longer combustion that might qualify as "self-sustaining" therefore a potential cause of extensive, even massive, destruction in a fire incident.

To this date, this commentator is aware of only two bases auguring for a short criteria: less expensive testing, and the possibility of assigning formal fire limits to specimens burned in drop tower (micro-gravity) testing where the maximum potential time to ignite and burn a specimen is about 2.2 seconds (not enough time to burn 6-in./150-mm of numerous metal specimens even at pressures quite a bit above their fire-limit.

¹² Besides the series of cited papers, personal contacts were initiated multiple times to a series of persons.

The negative vote prevailed and from 1995 through 2009 ASTM G4 Standard G124 used “full 6-inch combustion” (with exceptions) as the criterion to define self-sustaining combustion. However, apparently throughout that period, NASA+ clearly yearned and even intrigued, for a short criterion.

On three or four occasions NASA+ re-balloted G124 with a change to a short criterion, and every time this worker cast a negative vote and reiterated the bases for a long burn criterion. At one point many of the bases were folded into an ASTM-style paper-format that could be referenced [27] rather than re-creating the extensive bases for the negative in the vote, itself. However, by this time, NASA+ had learned that in the ASTM ballot system, if a ballot is withdrawn, its negatives do not have to be resolved. It is a good way to find gross problems with a new standard. However in this case all three or four ballots were withdrawn, and the negatives were not resolved, ...they were ignored. Did they hope I had changed my mind, or had retired or died, or gave up or maybe just forgot to vote or shamefully decided to defer to them as “customers” who are always right?

Finally in 2009, Sparks, Stoltzfus, Steinberg and Lynn [28] presented a paper at the ASTM Committee G4 Twelfth Symposium in which they claim:

“A recommended length of test sample that must be consumed to be considered a flammable material was determined based on the preheated zone measured from these tests. The length was determined to be 30 mm (1.18 in.)”

The most interesting feature of this paper in comparison to the papers of Case 1 and 2, is that in seeking to censor those papers multiple peer reviewers were assigned whomsoever had conflicts of interest but also reputation for being advocates of the “excess oxygen” allegation. In this case, the peer reviewers are not known, but as the staunchest and most outspoken of the critics of a short criterion this worker was *not* sought out for a peer review. Indeed, this worker’s input was unwelcome. Hence in the earlier cases the peer reviews were used to exclude dissent while in this case the omission of this opposing voice can only be interpreted to instead exclude dissenting voice. This peer reviewer did in fact review papers for this symposium but not the subject paper. And as a defender of dissent would not have and to this day, does not oppose to this day, the presence of this seriously flawed paper (in his humble opinion) in the G4 forum. However, in this judgment the paper is dead wrong and the conclusion it was created (fabricated might be a more accurate perspective) to support is simply wrong.

Within a year a new ballot was issued with a rather misleading rationale statement that appeared to suggest it was only a re-approval vote but with a few “long-overdue changes” (nothing to see here). One “long-overdue change” that was included was a burn criterion change from complete (~6-in./~150-mm) burn length to just 30 mm/1.18-inch. Once more they had connived and manipulated to get their way, and the science and dissent be damned. However, one can not but wonder how these shenanigans would play in a lawsuit should this wrongful imposition lead to an incident.

This worker yet again voted negative. This time however, the ballot was not with-

drawn and the negative ignored. Instead a copy of the 2008 paper was sent to this worker to seek withdrawal of the negative. The exalted NASA+ had spoken. Bow down. This worker was being asked to in effect ignore the rather large body of data that argued a short criterion was wrongful, and ignore his own testing in effect, and instead accede to the assertion from the paper of the glorious NASA+ co-authored by the distinguished authority Dr. Ted Steinberg that they alleged had measured the *true* definitive criterion. Case closed. What incredible egos! What amazing confirmation bias! What quaint Dunning-Kruger Effect! What the paper proved was clearly that the robust self-confidence in Dr. Steinberg's DT had not mellowed at all in the following two decades.

This worker provided a quick overnight mini-peer review on some of the several serious flaws that render the paper's conclusion invalid and the experimental approach "sloppy and careless" and "fast and loose". Flaws reflective of the similar absence of error analysis in the DT. But one does not challenge the great and glorious NASA+. How dare this worker deign such impertinence? Especially when it contains the unchallengeable co-authorship of Dr. Ted Steinberg. Negative peer review that had been used so effectively to censor earlier work abruptly came to have no effect at all.

Frankly in this peer reviewer's judgment that 2009 analysis [28] was an embarrassment. However in this case, suddenly, peer review that did not serve the powers-that-be did not count. One does not challenge G4's premier NASA+ clique. A somewhat mocking basis on which to find the negative ballot non-persuasive was generated and at the last part of the last day of meetings when only ten of perhaps 40 or so original attendees out the 100+ full committee membership were still present (apparently not including the chairman) a subcommittee of something like five or six members probably all of them NASA+ and then remaining full committee members (about 4 more) voted to find the negative non-persuasive and PRESTO! G124 got a new *defined consensus* burn criterion. This is how NASA+ handled dissent, and a formal appeal to ASTM Committee on Technical Committee Operations (COTCO) found no flaw in G4 leadership substituting self-defined last-minute quorums of six and ten as resolving dissent and a negative ballot. Clearly ASTM no longer pursued its once-cherished full-consensus standards nor did Committee G4. This is the status of peer review today.

And to this day, this episode still and again boggles the mind of this worker. It is truly impressive how so much contrary data could be ignored in favor of a "sloppy and careless" effort thrown together in a "fast and loose" manner yet so reminiscent of the excess oxygen hornswoggle. This ability to embrace such weak data while disregarding so much history, experience and contradictory data including from this rather unwelcome worker seems to exemplify the alleged confirmation bias among devout practitioner's of Dunning-Kruger Effect.

Following the unqualified G124 victory/smackdown/beatdown, co-author of the "definitive" paper [28] Lynn would comment in his PhD Dissertation thesis [29] that the revision of the G124 burn criterion has "...the added benefit of enabling the conduct of reduced gravity metallic material flammability testing in strict conformance with the ASTM G124 standard..." perhaps verifying one of the main, if not *the* main, incentives for the un-

seemly decade of standards development chicanery.

Shortly after that vote of non-persuasiveness, when G124 had been finally changed, another of the “definitive” paper’s coauthors, Sotltzfus (a key NASA+ member), coauthored with Lewis and Jeffers [30] a paper that examined how the fire-limits of 300 series stainless steel and Inconel 718 were affected by the newly revised (and re-approved with a few “long overdue changes”) standard. They report the “real” fire limit pressures were actually a whopping 80% lower than had been previously measured. and suggest in a section titled “Effect on Industry”:

“When considered solely from a fire hazard viewpoint, the new burn-length criteria will have a positive effect on industry, Materials will now be found flammable at lower pressures than before. This will tend to make new oxygen systems more burn resistant, and therefore safer. On the other hand, the new burn criteria may also compel some to re-examine older oxygen systems. This examination may reveal that flammability risks previously thought to be acceptable are no longer acceptable.”¹³

In that paper, they cited that the fire limit of 300 series stainless steel which had been previously reported to be 500 psig/3.4MPa was actually just 200 psig/1.4MPa. So in effect this standard to measure when self-sustaining combustion (the kind that yields massive oxygen incidents) is possible had been massively in error (~150%) before the change. This paper could have sought data or generated new data (like those this worker generated in the mid-1980s in support of the original criterion). Yet alas, they did not. However there were some data available.

In 2005 NASA MSFC workers, Engel, Herald, Davis published numerous tests of 12-inch long 347 alloy stainless steel [31]. And this worker was given the raw data from Stephen Herald (letters uncertain) and referred to it in subsequent repudiations of the revised G124 standard that he now considers unfit for use [32]. Those data indicate that at the 250 psi level, 25% *above* the “new” reported fire limit) the combustion of a complete 12 inch specimen at 250 psig would be outside the test population’s 60 sigma level. According to the Google AI that is a probability event of 1.24×10^{-784} for a 12 inch self-sustaining (complete) combustion. At 350 psig a 12-in. self-sustaining (complete) combustion would be a less than a 9.81×10^{-198} probability. Keep in mind that when NASA experienced the Challenger failure it was a one out of 25 event (0.04 probability). However, at the 500 psig test pressure (the prior criterion fire limit) the MSFC workers obtained 8 out of 44 test results (0.2 probability) at the 12 inch complete-combustion burn level.

Indeed, this worker has come to compare this to the mechanics of the decisions to launch Challenger (1986) and to ignore the Columbia damage (2003). It is possible to paint spots on mice. It is possible to jerry-rig tests and theory to appear to say other than valid things. All in the goal of seeking funding or avoiding insult to customers. And what is worse, there are people willing to do those things. Peer review abuse is rife. It is supposed to

¹³ One wonders if they also compelled NASA to re-examine and change their older selection of the Shuttle and now SLS LOX-tank metal. This worker has long dreaded the shuttle and now dreads the new SLS vehicle.

fight that possibility, but as practiced today within G4 and ASTM and apparently many other systems, it not only does not do that, it places what this worker considers to be fraudulent imprimatur on sloppy-and-careless fast-and-loose work, Dunning-Kruger Effect work, Confirmation Bias work, and even uncollaborative work.

For eighteen years, this worker has repudiated, without waver, the new ASTM/NASA+ cleverly defined-*consensus* G124 standard. It is in this dissenting opinion unfit for use. All of the original reasons and data from 1992 are still valid and undisputed, this worker's testing back then remains valid, testing at NASA-MSFC [31] has bolstered this. Yet today the appropriated G4 peer review process and ASTM oversight not only find it acceptable to swat away dissent like annoying bugs, they seem to find it is their duty to ignore it. As a result this worker today dissents not only to G124 but to the G4 Committee and ASTM itself.

The first generation of G4 feared peer-review abuse and sought to prevent it. But the parent ASTM system allowed NASA+ to overrule history and the entire early committee. ASTM has been scrupulous in warning committees about violations of its Antitrust statement, but in this opinion, ASTM and G4 and NASA are at far greater risk, *and should be at far greater risk*, for this mockery of science and its own history.

The fact that an alpha wolf of NASA+ has been an editorial czar (IMHO a misguided czar) within G4 since 1995, and has done what this commentator considers extensive damage to the community and its primary mission is beyond tragic in the truest definition of that word. NASA+ has every right to participate and to be heard, it has no right to dictate and censor. It is a customer with all the problems that entails but it should not be an emperor with new clothes.

Can ASTM and G4 Be Fixed?

This worker considers the ASTM and G4 peer review process badly constructed, badly operated and is badly broken. In this opinion, it has been used to hide sloppy and careless experimentation, damaging the community discourse irreparably, and has been used to manipulate G4 and therefore ASTM standards compromising the ability of the oxygen user community and the G4 prime mission of saving lives and injuries, saving capital and saving production. The worst downside could therefore be death, injury, destruction and lost sales.

This worker considers himself a volunteer public-service scientist these days and does not expect to control dialog, might not even affect it, but would like to actually advance the science of oxidant safety. This analysis has made a case against what I believe was deplorable peer review abuses (PRA) citing three egregious examples within ASTM Committee G4 (two related to phantom excess oxygen [6], one related to burn criteria [28]). Are there options left to clean up the mess with limited energies and time?

The conviction here is that the current narcissistic generation of NASA+ bullies, have come to see themselves as the cream inevitably rising to the surface where it belongs. They are merely attempting to do what they are destined to do (manifestly destined to do) because of their superior intellects. They are just trying to ignore annoying irrelevant incom-

petent noise from those pissants like this worker that distracts them from their obvious destiny. The perhaps inevitable resulting peer review abuse (PRA) is bad, but it is among other even greater issues and is just one symptom of a larger concern that is the basis for a related dissent web-site. But this commentary deals specifically with PRA that emerged first after an unthinkable challenge to their supremacy in a single “Brief” paper [7].

That “Brief..” paper, even just the pictures therein apart from the other issues, unknown to this worker at the time, severely contradicted the absurdity in Dr. Steinberg’s Dissertation Thesis [33], a document I now believe to be grievously flawed to a degree that might explain why so much effort (rooted perhaps as much in simple narcissistic denial as in Dunning-Kruger Effect and confirmation bias) appears to have been exerted to suppress revelation. Clearly any wunderkind would and should be expected to recoil at the prospect that his own founding credential might be largely crapola. Extreme denial may be the least ugly reaction to be expected. And the embarrassment reflects not only on him but so many NASA+ perhaps explaining the NASA+ coalition. In 1995 this worker had no idea just how shabby that thesis and its resulting spin-offs were. Critique having been shunned to this day, the foundation built on Dr. Steinberg’s Dissertation Thesis and thereafter may indeed prove to have been a house built on sand destined to collapse.

This worker concludes Dr. Steinberg did *not* identify massive nor even copious excess oxygen in his thesis [33] or follow-up work [8], and that his own data if anything argue the opposite (whether any lesser amounts of dissolved oxygen do actually exist) [34]. Compelling ways of proving that point are possible, have been promised, but avoided and are now being considered by this worker should further proof be needed. ASTM G4 needs to acknowledge the mess that has been made of collegial oxygen safety therein.

This worker renounces the ASTM/G4 and other peer review systems. Based on the content of the G4 forum since 1995, and the contributors, crucial participation and crucial contributions that have been lost ...forever. Furthermore, a recent suggestion has been heard that the public-service function of G4 may have effectively been co-opted into an aerospace activity. G4’s STPs have become a form of self-publishing for NASA+. Peer review within G4’s tiny pool of peer reviewers has never been consistent with any “self-regulation by qualified members” even though efforts at “regulation” is something *voluntary* G4 must strive to avoid. And when G4’s potential problems with NPD are factored in, an objective peer review process can not be redeemed and was never realistic.

In the early 1900s, lowly patent clerk Albert Einstein living in Germany during its famous period of rabid anti-Semitism, submitted ridiculous papers on Special and General Relativity to the world’s most prestigious scientific journals that no one in the world believed (probably not even Einstein himself for whom they might have been whimsical clever exercises in Devil’s Advocacy), and yet he had less problem publishing them than this worker did with the “Brief” paper under peer review. Einstein was perhaps lucky that there was no Newton+ entourage at odds with him seeking not only to dissent to his musings (and there was a lot of dissent, some even persists to this day) but to go to the mat to shield the world from being exposed to them, and then to hold a grudge. As a result, there was a spirited debate on their validity that is currently in Einstein’s favor. So how does a committee fix

an atmosphere, an atmosphere that has become rather poisonous, that was intended to be and needs to be collegial and collaborative to achieve its noble mission, in spite of the NASA+ contingent seeking to serve as overlords?

ASTM G4 and its ASTM oversight need to face the unflattering reasons that have led the NASA+ team to alienate important allies (former friends no more)? Why their concerns were so grave that they could not be handled with a rebuttal and closure and collaboration? Why so much overflow animosity? Why the intellectual vendetta that overflowed into virtually all of its pursuits.

Oxygen continues to be hazardous. In 2023 a quick internet search found claims that oxygen was a factor if not cause when “a disastrous fire in an Iraqi hospital took the lives of 82 people”, when “Home Oxygen Fires Claim a Life Every Four Days”, when an apparent LOX explosion in 2019 (presumably with aluminum/LOX issues) kills 15-24 in China.

Therefore, it would seem as it was when ASTM G4 first formed more than 50 years ago, there is still a need for an objective resource on oxygen safety. A resource that provides something closer to if not full consensus (rather than G4’s current “defined consensus”) opinions and the context for them. Hence this worker’s recent website that this commentary supports presents some of the dissent that G4 has shunned. Dissent the outside world and maybe portions of G4 itself may not be aware of. It reveals messes that G4 largely through its NASA+ leadership have ignored and gives oxygen safety practitioners an ability to see G4’s standards in a better (at least different) light when voluntarily choosing how to achieve their own safety and to evaluate breaches there of.

ASTM and its Committee G4 are not the warm friendly nurturing forums once sought, and the page has turned on any hope for that. First of all, over-inflated egos need a serious smack-down. They need to realize they are not premiere, not the only cream. And ASTM needs to live up to its earlier goals rather than supplicate to special interests.

The peer review system within G4 must end, it has not, does not and can not produce the mythical flawed benefits claimed for it. It has in fact been used (actually miss-used) diametrically. Nor in the case of G4’s needs, should it effectively censor voluntary information. G4 is not an ultimate arbiter. NASA+ are not technology overlords. G4’s standards are not and should not be codes. G4 develops “voluntary-use standards”. Opinions. This is not the first time this dilemma has been broached. In 1997 the previously examined peer-review contention brought more than the spirited peer-review defense from then COTCO chairman St. John. A part of the St. John defense of peer review, was the observation that ASTM publications are not what he called “proceedings of a symposium” despite the fact that that is exactly what G4 considered them to be and that most publications at that time did indeed contain peer-reviewed symposium papers. Today this appears to be emphasized in that ASTM catalogs and sells papers individually and that Special Technical Publications now can contain papers (much like a standards compilation) not presented at the symposium that dominates and demarcates the STP content as described regarding the G4 seminar series revitalization in the very overview for STP 1319 circa that very year: 1997.

The St John perspective was that “proceedings” are *not* peer reviewed. They were

whatever material the speakers were presenting. They could go onto peer review and publication in the ASTM Journal. They were not settled science but could with peer review become more like settled science. He suggested G4 consider whether they wished to change to a proceedings format (which is very likely what G4 was assuming STPs were). However, he warned that printing costs to enable distribution at symposia would be the Committee's obligation and an expense at that time of \$25 to \$50 per copy (in 1997 for 400 copies). Today with less expensive on-demand printing the cost would likely be comparable or far less than the pricing of past STPs (\$100-200). If Amazon or other On-Demand printing were used, the net would be a reduction in costs to symposium attendees and no increase in costs for further purchases of the "proceedings" (though such vigorish could be built in).

Some effort in that regard went nowhere back then. However, in retrospect today, apart from what this commentator considers to be inappropriate peer review abuse, the mere idea of "self-regulation by qualified members of a profession" creates a false light perspective on the G4 "collegium" when some of those members assume overlord status. To wit when an editor as described herein can effectively censor and exclude criticism of his own work, that control of the community dialog is antithetical to the goals of a collaborative, voluntary, transparent collegium.

So is there any chance that a collegial collaborative forum can be resurrected despite this commentator's belief that warm and friendly nurturing has been lost under the "fool me once" ethic? A change to St.-John-style proceedings may be worth trying. Indeed, in recent years G4's Minutes indicate there has been a problem assembling even the scant and often inadequately qualified reviewer bases G4 has employed in the past. These shortcomings are tolerable when the peer reviewers act in support of the authors rather than as their censors. But it is destructive when it purports to "settle science". So not only has peer review failed but the mechanics of the mechanism are struggling. In the case of the St. John suggestion, symposium chairs can still be called upon to review accepted manuscripts and to submit a perspective that can place the collection into a context, that can be presented as a paper, a context that might even be reviewed by the committee at large and might also engender rebuttal papers that can be accepted and encouraged and even sought out.

And there is a self-policing benefit that might accrue. In 1997, if this worker's "Brief.." manuscript had been automatically included in a St.-John-style "Proceedings", this worker doubts that either of his two most rabid peer reviewers would have been willing to present their ridiculous rejection bases as public rebuttals. Indeed, this worker is considering publishing full rebuttals to their rejections as quaint history artifacts.

Let this worker speculate on an approach. First instead of having proceedings of a "symposium", G4 can change to a proceedings of "Committee G4". Special Technical Publications can be treated as a portion of the G4 Proceedings that can include earlier seminar papers and other documentary works as "transparency" timed to make papers available at the symposium at which they are presented. Editors/Chairs can be designated to present specific perspectives. Full or partial Committee discussions can add to that transparency. In this case, a Call for papers, even targeted papers with targeted presenters can still ensue and the symposium Editors/Chairs can wean out the commercial, unrelated, or prior-published/

copyrighted submissions. Accepted papers can be presented at regular meeting seminar sessions or the periodic Symposia, and the papers can publish ...in context. And conceivably still other “documents” of the Committee as papers can be added. However, the proceedings can carry a disclaimer, and when papers are examined editors can effect an option to provide “context” in the form of editor or targeted rebuttals. The rebuttals can be shared with the authors as a collegial/collaborative gesture to inform etc., but if the authors elect to publish their own opinions as opposed to the peer reviewer suggested opinions without changes, the contextual material of the editors can be published in proximity, and authors can be grated closures.

Indeed this allows a sort of welcoming cordial environment wherein disagreement is allowed for context, where peer review does not dis-incentivize (intimidate) presenters, but censorship is excluded. This worker would have welcomed rebuttal to his most hated paper rather than the shunning distraction and defamation that resulted.

Indeed, if his paper was so very bad, even as one peer review rejection [35] admitted: “Since steel burning at this pressure is very relevant to many applications, **these observations are very important**”. The bold-face emphasis was present in the peer review! He then goes on to justify rejection based on his disagreement with this workers interpretations, opinions and style in much the same way that this worker disagrees with his interpretations, opinions and style. One wonders how much damage to the G4 mission (death, injury, damage and lost production) the peer reviewer was willing to accept over the censorship that may have resulted then and thereafter.

If ASTM and the G4 leadership believe their peer review machinations are serving the G4 mission and settling science to boot, they may face a rude awaking some day, perhaps in a court room. G4 should not be in essence certifying its products as absolute, while labeling them voluntary. While our NASA+ contingent is very very self-confident, or perhaps is very narcissistic, their pronouncements are not yet gospel (though they may still not realize that). Nor should they be the essence behind G4’s products. G4 Peer review must end, and switching to a “proceedings” may be a more valid alternative than scrapping the publications.

Closure

Thirty-one years ago, this worker collected experimental data contradicting earlier data published by exalted NASA+ workers who recoiled at the idea that their own analysis could possibly be flawed (whether it was or not). Their successful efforts to thwart publication of the data included an abusive use of the ASTM/G4 peer review process, abusive regardless of the degree of which their condemnation was correct. In the intervening years more and more of those data have proven correct and more and more of the NASA+ data have proven faulty. Seventeen years ago a “sloppy and careless” NASA+ test program purported to measure a definitive burn criterion for fire-limit measurements. The paper co-authored by the ASTM lead editor and NASA+ workers again ignored contrary data and unsolicited negative peer review and it was used as a sole basis for changing the burn criterion

in G 124 in spite of negative ballots/peer-review and the wealth of contrary data contesting the change. In this judgment oxidant safety was sabotaged.

These episodes and an emerging self-assumed presumption of their own superiority in all aspects of the Committee subject has in this opinion and for the numerous bases presented herein, damaged the operations, the collegiality, the quality of collaboration and has tarnished the products of the Committee G4 as well as their efficacy for use ever since. A fairly diverse perspective on oxidant safety has been replaced at least in part with a NASA+ perspective. And while NASA and all the elements of the NASA+ clique are impressive in their credentials, NASA+ and even NASA-At-Large are not infallible as recollection of Apollo 1, Apollo 13, Challenger, Columbia, and numerous lesser events attest [36].

The fallout of this history and the ingenuity used to thwart and even just sidestep the presumed protections of peer review within G4 and ASTM, dishonor the good works (no more) of ASTM G4.

ASTM G4 needs to reclaim its good name, to clean up its mess. It will not be easy when NASA is a major customer, enjoys funding by government, and NASA+ has exhibited a willingness to exploit those factors whether they are aware of them or not.

ASTM which is so scrupulous in its cautions about antitrust issues, exemplified also by its concern about liability issues in G4's past distribution of software, that once boasted of full consensuses that are only a memory these days, might wish to consider its own exposure if a lawsuit alleges G4's standards especially G 124 were contributory to an accident. When the ASTM system and peer review process are used to manipulate standards and publications, does ASTM bear any liability? Can they just blame NASA+?

In this opinion the default esteem in which NASA and NASA+ is held and the robust self-esteem NASA+ exhibits can imbue an appearance of infallibility, but in this workers experience oxygen has too often found ways to thwart most efforts to control its risk. Indeed, as noted variously, this commentator has been wary of the NASA basis for its Shuttle and SLS aluminum/LOX tanks (based upon those admittedly potentially incomplete data he is aware of for the aluminum-lithium variant in the Space Shuttle [37,38]).

For all these reasons, in this judgment, the ASTM/G4 peer review process has failed to serve the mission of ASTM G4 and has proven to be both deplorable and hazardous.

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