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## The NASA+ 800 Pound Gorilla

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**ABSTRACT:** ASTM Committee G4 launched in 1975 with a mission to save lives and injuries, capital, and operations. Key players included NASA engineers and scientists who were always destined to play an important and vital role. As the reputation and prestige of G4 grew, NASA and its affiliates (NASA+) influence also grew to the point where its members and those it influences increasingly dominate, dictate, and unfortunately, seek to dictate committee operation and products even when they are acting out more from ego than from knowledge. This massive influence is oftentimes indispensable but at times can be and at times has been ill-advised and even detrimental to the G4 mission. NASA is not yet infallible and has its own significant record of oxidant mishaps some of which are examined here and that auger for a greater degree of collegiality within the committee. NASA+ needs to police the privilege and esteem it is granted.

**KEY WORDS:** NASA, gorilla, undue influence, ego, ASTM, G4.

NASA is in a terrible situation. It is expected to do great things. Alas, it *has* done great things. Among them, NASA taught us when “failure is not an option.” It has (allegedly) put men on the moon. It has threaded satellites through cosmic needles. And the list is long. As a result, those who are NASA scientists, engineers, affiliates and even toadies enjoy an enviable default credibility, respect, esteem and deference. For fifty years, NASA and former NASA and NASA-Affiliated members (NASA+) have been crucial contributors within *ASTM Committee G4 on Compatibility and Sensitivity of Materials in Oxygen Enriched Atmospheres*. And their influence has grown for a range of good reasons but also a few not-so-good reasons. Today within ASTM G4, NASA+ et al. has become what Wikipedia defines as: “an entity that dominates its environment”, it is a quintessential 800 pound gorilla.

This analysis is part of a formal targeted dissent and as such will not focus on the greatness of NASA at large while stipulating to it. However, this is intended to explore the exact opposite, those cases where the NASA and NASA+ sphere of influence has had feet of clay. Where its well-deserved self confidence has been seriously flawed. To wit: Apollo 1, Apollo 13, Challenger, Columbia and more. But not just big events, little ones also.

During the course of fifty years since ASTM Committee G4 was first formed,

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NASA's members on G4 have been key, vital, major contributors. G4 might not even have been possible without them. Their contributions especially those from the NASA White Sands Test Facility, a facility devoted to experimental testing, have grown along with the reputation and prestige of the G4 Committee itself. That influence is a form of power and Lord Acton taught us how power corrupts.

The operations, the products, the posture of Committee G4 have taken on a distinct shift in recent decades and have become ever more reflective of NASA+'s perspective and goals, not to mention its adherent's personal goals, rather than for the Committee's founding goals. A core of very self-confident (doubtless at times too self-confident) NASA workers from WSTF and a core of associates (often former NASA and NASA related ) referred to herein as the NASA+ (that forms the 800 pound gorilla) have assumed a degree of tacit control and undue influence over the Committee's operations. They would almost certainly disagree ...and they would very likely disagree with great self-confidence. That influence does not have to be a bad thing if indeed those doing the assuming and controlling have a clear and warranted transcendent ability and/or humility (as opposed to toxic examples) and an appropriate focus. However, in other words, this is to make the case that the influence and assumed control by this current NASA cooperative, talented though it be, is at times flawed, and even wrong. And the oxidant safety subject has suffered.

Yes, in some ways NASA at large has been a gold standard. But that is not serendipity. In no small part it is because enormous amounts of taxpayer money (including a small portion, possibly only hundreds of dollars from the writer) are funneled to it, nonetheless, it does attract many highly skilled and motivated people. Yet it is possible to level a few crucial criticisms here, some petty, some quite serious. So how does NASA+ in ASTM G4 today compete with its own legendary NASA past within G4 and at-large. Does it avoid drinking too much of its own Kool-Aid?

Both today's NASA as was the case for past NASA, it turns out, is actually not infallible, it has had its share of blunders. Its default elite reputation is *not* always justified, *not* always earned. Legendary parent NASA has also had its share of tragic blunders, some major: Apollo I, Apollo 13, Challenger, Columbia and many numerous less spectacular, less well-known, events<sup>2</sup>. And among NASA's icons and even rank-and-file are strong, forceful, leader types whose individual and collective documented high degree of self-confidence has on occasion exceeded their talent, even when their talents were considerable (literally a personal and collective local Dunning-Kruger Effect [*I*]<sup>3</sup>). Humility and modesty suffered as hubris thrived.

It stresses the limits of this taxpayer's own talent to address this issue but few peers seem to be in a position to do so. And frankly, Macro-NASA is in some ways harmed and at times thrust into roles it should seek desperately to avoid for its own good due to the default deference it enjoys. It has experienced bad consequences, some of the worst, but how much has it learned from them? The Dunning-Kruger-Effect crew by its very nature suffers from an ability and predilection to ignore mistakes.

<sup>2</sup> The writer has his own fears for potential comeuppances that may add to his own list of errors.

<sup>3</sup> Italic numbers in brackets refer to the reference list at the end of the paper.

So there can come a time [that time is now!] when one has to push back at the bullies on the playground. And this among other efforts [2,3] support and compile a related dissent that seeks to do that. Not just to be nasty (though the writer may seem nasty in numerous analyses like this, nasty would have emerged decades ago) but this is in the spirit of collegiality, yet another virtue that has suffered for quite a while.

### **NASA Fallibility/Issues**

As noted, NASA at large is not infallible, as the Apollo 1 and 13, Challenger, Columbia and other episodes confirm for NASA's biggest embarrassments are not secret and they are not flattering. This diatribe is intended to explore highly specialized issues related to the rather esoteric field of oxidant safety work. These issues are certainly less well known than NASA's greatest blunders (although Apollos 1 and 13 *were* both oxygen related). Indeed, in the case of oxygen safety, this does not make NASA unique or worse, but it argues they are not inherently nor greatly better. Many, maybe most, if not all of those who work a lot with oxygen have felt its bite. However, unlike NASA, usually when industry feels the sting, the whole world is not watching.

The writer has known, respected, and been honored to work with truly awesome NASA+ folks, no small reason for so much past default esteem and deference. So why write an obvious effort to beat up on NASA+ and its most ardent admirers? Why accuse many respected and once upon a time considered friends to be guilty of joint or several "undue influence", "toxic hubris", "confirmation bias", and that Dunning-Kruger Effect? Tragically it is because hero worship gives too much power to those who are revered and who exercise the power unchallenged as a result. In addition, NASA is a major customer in many industries and industry largely respects Rule 1 ("The customer is always right"). Both of these allow NASA actors and their disciples and toadies to easily fall prey to Lord Acton's prophesy ("Power corrupts and absolute power corrupts absolutely"). And some, too many, *have in this judgment* fallen prey. The precise mechanisms that likely lead to or magnify the process include the Cognitive Biases of Dunning-Kruger Effect and Confirmation Bias which are examined in more detail in other commentary [3]. This worker has witnessed them all and not only among NASA+ protégés to be examined here.

The contention here is that the NASA+ clique within ASTM Committee G4 has lapsed into both undue default influence and toxic hubris, a viral mixture like Fentanyl and alcohol. They appear to believe in their opinions to extremes (reminiscent of the decisions higher ups made about the Challenger launch and Columbia damage), and have no small reason to believe they are the smartest guys in the room (and often they are), however, it is poisonous to the goal of building true *full* or even the current vogue *defined-consensus* oxygen safety. In this judgment, for a long time, ASTM G4 has been shifting from pursuit of a high degree of consensus in oxygen safety to a NASA+ dominated even dictated style of oxygen safety. And it is worth repeating that would not be so bad if NASA oxygen safety is the best, most appropriate, there is. Of course, in this opinion, as will be argued, that is *not always* the case, hence the need for this exposé (this examination of dirty laundry).

ASTM G4 is a committee staffed by “volunteers”. What this means is that ASTM pays nothing for the members services G4 performs. It does *not* mean that those who perform services for G4 are not paid. Except for a very few, most, maybe all as was the case for the writer himself early on, are typically funded (employed) by various partisan organizations. Hence ASTM G4 relies on energies that are donated by these partisan organizations, and in most cases that which gets done gets done because some sponsoring interest needs it and only far more rarely is feeling generous, much less often because it is rooted in a purely noble public-service nature. This allows for (orphan) gaps to develop in the overall approach to oxygen safety technology.

Today the writer has no doubt the clique he refers to as the NASA+ largely directs ASTM Committee G4. And it is elite and sadly sees itself as elite. It has been sliding into its present autocratic mode for many years: It pretty much decides what G4 will do. How G4 will do it. When they will do it. What criticism (dissent) is allowed and what is censored. It has come to expect G-4 at large to be its rubber stamp and will play (and sadly *has* played) politics, even some pretty sleazy politics, to get its way when that does not happen. And the 1980s “quality movement” that creates undue deference to customers therein has served to facilitate and feed this resulting NASA+ hubris.

However, this worker has acknowledged that the dictator model for governing countries whether the dictator is an individual or a politburo, is a quite workable system when the those in the leadership are good or benign. But the alternative is precisely when Lord Acton’s narcotic prophesy threatens.

So has the NASA+ been good or benign on this playground? Does the good it does offset the bad? Has its hubris been justified? Is it actually infallible? Or does it make mistakes, not just the previously mentioned big mistakes, but the normal everyday mistakes of ordinary mortal folks? Hence this is intended to examine some of its specific results where in the writer’s judgment, it has dropped the ball, ...made significant mistakes. Both big and little and often forgivable but seldom acknowledged and in some cases denied with rabid passion<sup>4</sup>. This is not to humiliate miscreants whose talents and energies are substantial and much needed, even vital, but who can need occasional tutelage and fine-tuning, and restraint, for whose self-confidence has just gotten a bit out of whack but hopefully is still capable of redemption.

Hence, what follows is a series of examples (some trivial some serious), the writer considers to be blunders big and small, many like he himself has made, that are, to repeat, not intended to embarrass or humiliate, but which demonstrate (in addition to push-back, if they have any merit at all) the need for a renaissance within ASTM Committee G4. They are emblematic of errors anyone could make, the writer also not being immune and having made more than a few, yet have been poisonous because few if any are willing to challenge NASA+ and because some of the most extreme members therein are *not* willing to be challenged. They need to be called out for their own and other’s benefit.

<sup>4</sup>Full disclosure (as examined elsewhere), the writer at one point felt compelled to defend a paper he had written to his management because it drew such extreme negative reviews from the very NASA+ workers whose data it deigned to question lest it appear to be something he covered up: to wit NASA+ workers proclamations on excess oxygen.

## Copious Excess Oxygen in Iron Slag

“Copious excess oxygen” is listed first as a major personal thorn, a pet peeve, perhaps sour grapes, a near-Waterloo moment in fact, as well as serious damage in the writer’s long life. So much so that *very* detailed historical examinations of the fallout have been prepared [2,3]. Tragically, for as destructive as this subject has been, this presently heretical commentator considers the topic to be almost solely academic (angels on pin heads) and of little practical importance to the pursuit of safety in oxygen systems. Even if it were proven real beyond some de minimis degree (which in this heretical judgment it has not whether it exists at all<sup>5</sup>) ...it may only be a curiosity. However, blow-back from its severely flawed “discovery” has in this judgment had harmful ancillary effect on issues that *are* important to oxygen system safety.

To wit: the claim of copious excess oxygen has contributed significantly to discouraging thermo-chemical equilibrium analysis in oxygen hazard technology and both poisoned working relationships and corrupted the ASTM forum. This worker does take umbrage.

While working at NASA WSTF and pursuing his PhD in the 1980s, Dr. Ted Steinberg (an important but by no means exclusive villain of this piece) was variously viewed within Committee G4 and by the writer and including by he himself as an up and coming oxygen safety wunderkind.<sup>6</sup> A top-gun wunderkind among all the other NASA self-perceived lesser wunderkinds. Although great things were anticipated from him, he and his own NASA+ entourage have proved to be personal nemeses that this worker is at great odds with. Dr. Steinberg is admittedly a towering intellect, but Dr. Steinberg and his elite entourage do not suffer fools gladly. However, he may not be quite as towering as many of his associates and he himself believes? However, he is not the only, nor most impressive, towering intellect the writer has known and worked with. Nonetheless, dueling towering intellects is not the issue here. The issue here is that towering intellect is an especially tough gig to challenge especially when it exhibits fragile arrogant Dunning-Kruger behavior and is revered in cliques with “followers” and even more especially when it both benefits from a vendor/customer relationship and carries the legendary NASA brand. Only a fool would deign to question. And as impressive as Dr. Steinberg has been in many ways, in this worker’s judgment he and his entourage among NASA+ may have [No!, in this judgment actually did] miss a number of marks and excess oxygen was the first of them and a watershed for Committee G4.

Dr. Steinberg proclaimed in his dissertation thesis [4] his discovery of truly massive, one might say shocking, amounts of oxygen in burning iron slags ...way beyond any amount that might react with the iron. Quickly, dare one say too quickly and with too little skepti-

<sup>5</sup> The writer doubts massive excess oxygen or even copious excess oxygen is present, though there may be some oxygen present, and it is known that in some cases oxygen for reaction is carried by other species such as CO<sub>2</sub> and CO. More definitive experimentation has been possible but undone and is under reconsideration.

<sup>6</sup> It may seem that in referring to Dr. Steinberg as a NASA+ “Wunderkind” may be more than a little sarcastic, as it certainly is, but it is also accurately reflective of the great expectations and respect for him that would inure to an actual wunderkind.

cism, he and cohorts heralded the discovery in a paper published not in the narrow at-the-time forgiving ASTM G4 forum but in a prestigious international journal [5] where “sloppy and careless” error or even naive experimental design would prove massively embarrassing and prone to trigger denial in many workers even without the NASA+ imprimatur. Several years later, major skepticism in this worker, led to a brief whimsical effort to capture some excess oxygen, but instead results conflicted with the Steinberg/NASA+ discovery, and the results were presented at the first “revitalized” ASTM Committee G4 Seminar Series held within a regular committee meeting in Fall 1995.

The associated paper presented an opportunity for dialog with the NASA+ folks who could reconsider their spectacular discovery or to critique the conflicting results even with rebuttal in the G4 organ. What did follow was both marathon and what this worker considers to have been as egregious an example of peer review abuse/censorship [3] and collegiality abuse as he is aware of not only within the small ASTM G4 forum and antithetical to its founding principles, but in numerous published case studies-at-large that condemn corrupted peer review processes. This worker was an early and substantial contributor to G4 oxygen safety publications, but this worker has repudiated further publication in the ASTM G4 forum that he judges has been operated in a distorted and aberrant fashion under NASA+ control ever since (~30 years). Doubtless, numerous other potential sources of quite significant abilities appear to have been similarly dissuaded from contributing important insight while the publications within the G4 forum have swung to less practicality, more academia, and worse as a self-publishing organ for NASA+ to inflate their curriculum vitae (CVs).

Among the numerous abusive practices employed in this case was what may have been the first technical “peer intervention”. Ultimately, the efforts did succeed in forcing withdrawal of the paper within the G4 forum. However, after some 30 years in which Dr. Steinberg and the NASA+ entourage have had copious time to clean up their mess, this worker offers new commentaries that now argue regardless of any innocent, naive, or uninformed error in the early excess oxygen experimentation, along with the ill-advised efforts [3] to prevent criticism, it has since become a hornswoggle [2]. Indeed, as will be noted later, the arrogant superiority confronted throughout from NASA+ in this case regarding massive or even just copious excess oxygen seems emblematic and comports with the similar arrogant superiority issues within macro-NASA that so many appear to claim factored in so gravely in NASA’s Challenger and Columbia incidents.

### **Smacking Down an Alpha**

While the excess oxygen “breakthrough” was still new and exciting, elements of Dr. Steinberg’s entourage (he, Frank Benz and Dr. D. B. Wilson) decided to smack down [6] both Dr. Irvin Glassman’s early metals analyses [7] for rocket-fuel use and more recent use of “burn-ratios” formulated at the Southern Research Institute [8]. Glassman whom many consider to be one of the (perhaps *the*) world’s leading authorities on combustion had examined reasons why metals might burn rapidly or slowly, the former more necessary for rocket fuel use while the latter *relatively* preferable for possible oxygen hardware use. Both the Glassman approach and SRI approach had become, and presumably still are, rules of thumb among many oxygen safety practitioners. However, the NASA+ team had found legitimate

flaws (imprecision) in both quite useful perspectives. However, they appear to have made some similar errors of their own and employed both snark and an exposé approach.

Glassman posited that rapid combustion (useful in rockets, bad in industrial accidents) required the vaporization (he called it “boiling” as was common forty years earlier and even to this day<sup>7</sup>) of the metal into gases that could quickly mix with oxygen (gaseous diffusion). This was in keeping with earlier teachings of Davy [9] (to the effect that combustion requires an incremental release of heat sufficient to initiate combustion of a similar increment of metal) and was precursor to the related teachings of Munro [8] (defining “burn ratios” as having enough heat release during combustion to either melt or boil incremental metal). Glassman argued that since some, perhaps many metal oxides dissociate upon vaporization (i.e. do not exist as gases) the most flammable metals (good for rockets bad for O<sub>2</sub> hardware) would have oxide “boiling points” greater than the boiling point of the base metals. This was not precise for at least some metals. Similar imprecision was cited for the burn ratio approach. However the text came across as an expose and led to unseemly rebuttal [10] and response [11] in print. And it appears the dispute suffers its own flaws, errors and imprecision.

Alas as was the case with the earlier work and its imprecise approximations, NASA+ made its own new claims. One tragedy involved the paper [6] introducing the use of thermochemical software principles to provide a more precise characterization of a metal’s combustion, but then discouraged such use. This might well have been a game changer, but for a likely series of blunders and errors the initiative was not only discouraged, it was fully side-tracked disdained and then lost, and ultimately this approach was actively opposed by NASA+ for development, its moribund situation to this day, detailed below, is more than tragic<sup>8</sup>.

### **Bi-Polar Thermo-Chemical Equilibrium (TCE) Software**

Thermo-chemical equilibrium calculation software (in particular NASA’s own CEA code based upon its earlier Gordon-and-McBride code [12]) has apparently been qualified from within NASA+ itself with at least two bipolar perspectives. On the one hand it seemed at first to offer great hope and promise, but on the later other hand it could be woefully unable to deal with any important situations and so was of little practical use? Reports are that NASA WSTF itself still uses the software for oxygen system analysis, but only in some [apparently rare] cases. NASA+ citations in papers couch its results in skepticism. Certainly there has been not just zero interest in participating but conscious effort to dissuade G4 ac-

<sup>7</sup> Such imprecise abuse of the term “boiling point” is common. The explosion of liquefied flammable gas vessels (perhaps most commonly propane) is commonly referred to as a BLEVE (a Boiling Liquid Expanding Vapor Explosion), however, high temperature rupture of liquefied flammable gases does not produce true “boiling”, it does not generate gas as the result of heat input, instead it produces gas while cooling as the result of a change in the thermodynamic state as the pressure falls. Industry and science call this flashing, So to some extent they are more likely FLEVE (Flashing Liquid Expanding Vapor Explosions). Dictionary definitions of boiling include mere “swirling upheaval.”

<sup>8</sup> There is merit in the NASA+ arguments despite their unpleasant (not-so-collegial) tenor, and these should all be explored if a serious TCE effort can be inspired. Several efforts to re-introduce this efforts into G4 were swatted away by G4 leadership like they were an annoying bug.

tivity despite any availability of energies to do so, energies that were curiously declined.

These bipolar perspectives are in the 1992 paper [6] in which the NASA+, itself, introduced the use of TCE software into the oxygen safety community for what appears to have been the first time. Indeed, in challenging the previously-cited early work (1950s) of Dr. Irvin Glassman [7], the authors: Dr. Steinberg, Dr. Wilson and Frank Benz, proclaim both views after using it themselves [6] :

“The ready availability of computer codes for calculating complex chemical reaction equilibrium suggest that this thermodynamically consistent approach be used for discussing combustion of metals and alloys. Although there are limitations to the computer code used in this work, these limitations are computational and not conceptual.”

Hence this rousing endorsement of the code included the immediate hedge regarding the “limitations” that would later be magnified and would discourage its use as well as other less restricted software. The “limitations” alluded to were soon said to thwart most, maybe all, of the seeming value. And it would be cited, but rarely, thereafter (for 30+ years). Finally this commentator’s repeated solitary albeit less than ideal efforts to support and promote such software for oxygen materials compatibility data were fully outright declined, ignored and discouraged several times.

Reiteration of these disqualifying software limitations are cited as “restrictions within the Gordon-McBride Computer Code used on handling condensed phases” [6] and “The Gordon-McBride code is limited for application to iron burning because it does not handle multiphase systems well” [13] and is incomplete “because of lack of data, these ferrite ions will not be considered in the thermodynamic analysis” [13]. However, these phase limitations do not appear to have been detailed. A new effort related to this dissent [14] seeks to place phases into context and demonstrate that at least some of these phase limitations can be dealt with meaningfully in at least some, if not all, cases. Indeed, for at least some applications, the NASA software yields data for a pertinent use that are not greatly different than a more flexible Outokumpu HSC software [15] that does claim to allow for some distinct phase formations.

Another aspect allegedly disqualifies the software in an even more fatal way in that NASA+ came to ordain the prior-cited presence of copious excess oxygen in burning iron (and perhaps some but not all other metals also) implied that something (they concluded to be ferrite ions) had to be present to hold onto the “excess oxygen” (which I now allege elsewhere [2] is not importantly present). If they (this writer argues) erroneously concluded there is copious (possibly even majority) excess oxygen in iron slag, and since there are no thermodynamic properties tabulated (solid, liquid, or gas specific heats, melting points, boiling or dissociation points or latent heats) for the ferrite ions that they conclude would form, even if the software were perfect it could not address iron combustion [or perhaps anything else?] for lack of this property data by which to calculate these “major” fractions. The 1992 proclamation was thereby followed by various comments like: “...because of a lack of data, these ferrite ions will not be considered in the thermodynamic analysis” [13] and as a result

alludes the analysis would apparently be too crude to allow anything but perhaps coarse trend surmise. Notwithstanding dispute over the presence and scale (if present) of ferrite ions, curiously, perhaps most tellingly, there appears to have been no efforts to measure or theoretically generate the needed property data in the intervening thirty years by the self-appointed cognoscente.

Finally, besides the valid criticism of the apparent NASA software quirkiness and complexity (and doubtless similar behaviors of other software) these challenges appear to have entrapped the NASA+ clique into misinterpretation of some data. Yes, the NASA CEA software appears much more quirky than the HSC software (for O2 safety use) with both being very challenging to use, especially for the less-than-fully skilled worker ...like this worker. Nonetheless, the NASA+ trashed and thwarted use of NASA's own software or anyone else's and this commentator can not wonder but that the basis might have been to avoid exploring its own copious excess oxygen machinations.

Today, this writer argues the NASA+'s perspective (call it a blunder) on TCE software is wrong in four ways: (1) its "excess oxygen" argument (2) its opinion that the treatment of mixed and condensed phases is always disqualifying, (3) NASA+ has shown an apparently flawed understanding of oxide dissociation, and (4) NASA+ has misused the software (almost as badly as this user at times) in asserting its demur. The writer admits being less-than-ideal with talent to definitely address all the issues with this software, and his efforts may contain offenses to skilled thermo-chemistry practitioners (who have been welcome to participate but haven't for at least 20 years and are hereby still encouraged to participate), but even if grievously limited himself, these several issues have been examined and seem to prove the fallibility in the too-self-confident NASA+ approach and the very real need for TCE application to oxidant safety technology.

### **Blown Polytropic Equation?**

Since this treatise is devoted to illustrating instances of the non-infallibility of the NASA+ clique, a brief examination of polytropic equations is in order. Earlier sections developed the issues that elements of the NASA+ had with software. However, even something like the humble polytropic equation for the adiabatic compression of gases has had issues that software might resolve. It has been perhaps the most important equation in the oxygen safety subject.

$$T = PV^\gamma$$

Where P is the absolute pressure of an ideal gas before and after compression, V is the volume of the gas before and after compression, and T is the absolute temperature of the gas before and after compression, respectively, and  $\gamma$  is the ratio of the gases specific heat at constant pressure and volume, respectively, and it has been cited numerous throughout the community including in ASTM Design Guide G88.

It is not however, the most pleasant equation to manipulate, including for large portions of the oxygen safety community. And so in the early 1990s, this worker coded a Visual

Basic utility to do the math (G4Math.exe) and it was vetted and balloted and then was distributed as a courtesy to all attendees of the G4 Education Course “Fire Hazards in Oxygen” from the late 1980s through the mid 2000s. At that end-time ASTM apparently appealed to G4 to stop distribution on liability grounds (for some reason they assume a wrong utility answer has less liability if printed in a standard rather than presented on a screen).

Nonetheless, in 2000 [16] three variously-lettered NASA+ scientists would publish a paper to address real-gas isentropic behavior with more validity than the ideal gas polytropic equation provides. Its Table 5 purports to present results from the polytropic equation for adiabatic compression of ideal oxygen gas using the commonly cited value for  $\gamma$  of 1.4 compared to three other more sophisticated methods of calculating adiabatic compression of real-oxygen gas. However, the polytropic results appears to be in error. They appear to have been calculated with a  $\gamma$  value of 1.445 rather than the purported and commonly applied 1.4. They do not square with the results of Leslie, this worker, or the G4Math12.exe algorithm.

In other words while seeking to improve the calculation of adiabatic compression temperatures, the NASA+ team appears to have erred in use of the polytropic equation that they sought to correct. Their real-gas calculations were more complex and esoteric to effect than the polytropic equation and this worker has not attempted to employ them, and might not try without software aid. As a result, the polytropic equation results they cite as being up to roughly 32% in error compared to their real-gas results but appear to actually be to this worker more like 24% different. And this is in a paper with three apparently lettered authors, that was peer reviewed by three unknown ASTM G4 peer reviewers (though this worker was *not* one of them.)

Now this is clearly a petty gripe. Clearly the writer has made far more frequent and serious errors than this. This may be nothing more than just another “sloppy and careless” mistake. But it does address infallibility while at the same time exhibiting why carefully vetted software should be a feature of the G4 mission, ...if the prevention of fires and explosions is truly a goal.

This is not to belittle highly skilled NASA+ members, for they are more highly credentialed than this worker, but possibly less seasoned. This opinion may not be without flaws (perhaps a defense is possible), but rather this is to stress that achieving even rank and file stakeholder oxygen safety (folks more nearly like me) may not just mean coping with highly complex software like CEA, and HSC, but it may also mean bringing very basic software like G4 Math to the aid of even our community's most lettered and prestigious members. By the way, this is not a solitary example.

## **A Non-Climate Carbon Crisis**

Among rather troubling results coworker John Zabrenski discovered was cited in a joint 1989 paper [17] in a section titled “Annealed Stainless Steel Had Lower Thresholds”. In this case Zabrenski found a surprisingly high combustion threshold for 0.25-in (6.4-mm) diameter 304 stainless steel specimens that were found to have a grain size befitting hard-

ened alloy. When annealed the pressure threshold dropped from >1500 psig (>10.34 MPa) to 725 psig (5.0 MPa).

Curiously, in about 1985, NASA WSTF personnel had cited threshold pressure results at a G4 meeting in Ft Lauderdale for testing of 440C alloy steel of 2500 psia (17.2 MPa), that similarly fell to about 1000 psia (6.9 MPa) after decarburizing. Annealing and decarburizing both involve exposing the metal to high temperatures often in a controlled gas ambient atmosphere. So was it annealing or decarburizing, or both, that caused either or both results?

During this worker's efforts to capture "excess oxygen" (cited earlier) that led to significant animosity from NASA+ players, I was edified on the crucial role carbon plays in steel production by highly credentialed steel-making coworkers and the Internet. It turns out that carbon is protective of combustion. It is the reason high pressure pure oxygen can be injected directly into molten iron (something ASTM Committee G4 would NEVER recommend as safe to do) without explosion or combustion of the iron. That was 1995. Indeed, in the later 1990s industry sought to test stainless steel at NASA WSTF, a result that appears to have been published [18]. At that time this worker argued that carbon should be an examined variable and carefully reported in the testing. However, push back from WSTF NASA+ workers resulted in yet another instance of "being the bigger man" (despite being the "customer") and deferring to what "NASA says".

The carbon/iron role in component flammability apparently remains to be unexplored to this day. Here again undue deference to NASA's prestige (and status, nay undue influence, as the bigger customer) carried the day. The steel/carbon issue that NASA+ has scoffed at has since been again broached by this worker [19].

## **Pitot Tube Math**

At the same time that excess oxygen, and the effects of carbon issues, were straining relations (actually fomenting hostility), Dave Castillo and I submitted [20] one of what was intended to be thirty+ practical papers from non-academic-like front-line workers, but which became the last paper I would submit to G4 based upon what I came to consider to be G4's corrupted peer-review process [3]. In it we applied the reversibility principle that applies to the relation between static and dynamic pressure. We cited the equation for calculating stagnation pressure in a pitot tube (since the equation did not appear to be cited in any reference re-solved for velocity as a function of pressure instead of for pressure as a function of velocity).

This was not new territory and the same thing had been done in an earlier paper for the exact same reason [17]. It had been suggested by a team of lettered experts (two who were Professors Emeritus from MIT) and passed peer review in the earlier paper that I was also associated with prior to my unwelcome disrespecting of the copious excess oxygen machination. Nonetheless NASA+ reviewer Dr. D. Bruce Wilson (a highly negative peer reviewer earlier on my "Brief.." paper [21] that deigned to challenge the concept of excess

oxygen) again protested mightily that the equation was wrong. This actually led to discussion in a G4 regular meeting session of what to do with the dreaded Castillo/Werley paper as well as to a session in which a Crane Valve Company computer program for selecting valves was submitted to support the paper's rejection (second rejection in a row for me ) that led to some rather hostile exchanges and a commitment from me to edify G4. The paper was reluctantly allowed into *ASTM STP 1319*.

So, under the ethic "my experts can beat up your experts", I took the opportunity to research the basis for the Pitot tube equation and at the 1998 G4 Seminar Session delighted in the opportunity to teach numerous lettered opponents of my paper (including NASA+ Dr. Ted Steinberg, an editor for STP 1319) on why stagnation (static) and dynamic gas pressure are functionally related. Five years later that exonerated equation was added to ASTM G88 for estimating maximum gas velocities. That paper and two follow-up papers [22,23] were not submitted to Dr. Steinberg's peer review process, hence are *not* in the G4 STP series. And that is a loss.

It was not a good episode for NASA+. However, the issue of excess oxygen (That I now deem a hornswoggle) is worse [2].

### **Flow Friction**

Not all fire accidents in oxygen systems are understood [24]. Even in the case of some very important, large scale instances where massive costly investigations were conducted, the causes have remained elusive. The writer considers these to be nightmares. Furthermore, in some cases the events are repetitive and may obtain in the form of clusters of events.

As reported to this worker, in an unfortunate NASA+ effort to address these types of events in the G4 Education course, at least one of the possible candidate causes came to be referred to as flow friction, a real phenomena. Over time, it became comfortable for NASA+ to use this term in this way and in 2003 it was proposed for addition to ASTM G88 as an actual cause of incidents. However, it was controversial. My own attempts to flow-induce ignition were unsuccessful. However key push-back [25] obtained from Eddie Davis of NASA MSFC, and later efforts at NASA-WSTF to validate the mechanism produced some heating but no ignitions [26, 27]. "Flow friction" and unique Corporate experiences that might help understand it were among a series of 30+ corporate topics this worker had been promoting in the latter 1990s, but like most of them, was lost in the poisonous peer review atmosphere that developed. Hence, since 2003, its mention in G88 has been only as an unverified prospect. Apparently to this day, flow friction has still not been proven and yet no better explanation has been found that might actually qualify as an observed validated mechanism.

### **Cosmic Rays and Atom Decays**

As noted in the previous item, among the most frustrating of oxidant fire incidents are those that resist explanation. In some cases where the incident is important (has a high

human or economic risk) avoiding repetition can result in extensive efforts to replicate the cause. And in some cases such efforts have failed. In one case used as an example in the ASTM G4 Education course, such investigative failures apparently left “flow friction” as the only unverified candidate that could be suggested.

In an extreme incident, the writer participated in an effort that was vast and no known cause had replicated a major fire event. Literally a large Task Force at one time sought to speculate on even untested ignition mechanisms (even Flow Friction) and found none to be viable.

Years later during a leisurely retirement, two linked related random thoughts came to this mind. Particle impact has been validated as an ignition source, ...where particles were present. Both errant thoughts involved high energy particles: Cosmic rays and random atom decays. Analyses were prepared [28,29]. And after several years I suggested that as had been done with “flow friction” the same section of the ASTM G4 Design Guide Standard G88 similarly cite them as similar unproven but not implausible contributory mechanisms if not central factors.

And yet again a NASA+ cognoscente resisted arguing that a NASA physicist must first consider and approve the speculative mechanisms for plausibility. To see “if NASA says so”. And that has apparently pigeonholed any further action for more than another decade (13 years so far and counting). One can only assume that the past ten years were not enough time for NASA physicists (maybe they work very slowly). But of course it is much more likely to have just been a political move to exercise NASA+ control. G4 is a small pond with room for only so many big fish.

These factors (cosmic rays and random decays) indeed continue to be prospects that NASA WSTF would be ideally suited to experimentally evaluate. They would be mechanisms very difficult to prove as invalid but could conceivably be positively affirmed and might even present a prospective factor in the NASA+ promoted ignition mechanism of “flow friction”. As cited in one of the papers, one can envision arrays [29] of test cells divided into fresh and stale acetylene flammable gas mixtures at or near its explosive decomposition condition soaking in reality in the New Mexico desert for cosmic rays to impinge upon or random decays to stimulate to see not only if any might ignite under cosmic ray impact or if something like a carbon 14 decay might cause more random ignitions among fresh and more vulnerable acetylene exemplars.

However, NASA+ has shown no apparent interest and when their leadership role demurs, it is dead in G4. And so today NASA has launched the JWST and Hubble telescopes to search out things like gamma ray beams that might vaporize our entire planet if we are ever hit, they track asteroids that might extinctify all life on the planet if we are ever hit, and we wait to see if they rule cosmic rays and random atom decays are plausible igniters or contributors to ignition of oxygen system hardware. And that wait is a message all by itself. Or maybe this issue is just yet another that has magically fallen through the G4 cracks.

## Burn Criterion

A second-to-last example of what may be NASA+ Dunning Kruger effect is the saga of ASTM G124 burn criterion. A promising early effort to identify a fire threshold for carbon steel [30] led to Michael Benning's and John Zabrenski's seminal work on metal fire limits [31,17] in general with its stunning results, one being a push to standardize metal fire limit testing that motivated development of ASTM G 124 that was based upon later related embryonic work at NASA WSTF.

Zabrenski's early testing respected traditional gas flammability limit testing protocols and solid polymer fire limits (ala ASTM D2863 oxygen index methods) and sought to identify equilibrium (self-sustaining) burning as a means to ensure that the fire limit measured was not based upon a transient non-equilibrium combustion mode induced perhaps principally by ignition energy disruptions. This was historically demarcated by observing carefully a fairly long period of specimen combustion using as long a burn criterion as possible.

However there was a contingent of advocates, principally NASA+, for a short burn criterion. Easier to do, more economical, and apparently also if not exclusively inspired by a desire to add credence to drop-tower testing that allowed only for brief test durations, therefore short specimen burns. And although the G124 standard issued in 1995 with a fairly long burn criterion of ~6-inches (~150 mm), the short burn advocates repeatedly pushed balloting, and employed politics (unseemly gamesmanship and noncollegiality), to get it incorporated into G124. The whole shameful saga is reported in detail [32].

Furthermore, while the broader incentives within the Committee for this push were not huge, nonetheless a dogged contingent of NASA+ by 2009 designed a seriously flawed but beautifully executed experiment that purported (incorrectly) to prove that a six inch (~150 mm) specimen burn length criterion could be reduced to ~1.2 inch (30 mm) [33], and in the process ignored peer opposition, again hearkening to the "NASA+ says so" ethic. It was a sad chapter.

## Aluminum Flammability

Aluminum has been the most controversial of commonly used metals in oxygen. It is perhaps the second or third most widely used metal. However, its performance has been fickle. It has been launched in numerous applications in which it initially appeared fully acceptable yet has experienced numerous fire and explosion incidents, sometimes extremely grave and/or destructive, that have often resulted in its removal from those applications. However, in many applications today its use is dichotomous. In some cases it is used where it is below its fire limit. In some cases it has a statistical basis. However, in some cases it is used where it may be acknowledged that an ignition would produce an extreme hazard and risk, but as in the case for the most commonly used metal (steel) it is felt that ignition can successfully be prevented. Ignition in these latter cases has not always been prevented, and unlike the comparably based steel usage, in some of those cases where ignition was not prevented, the scale of the incident was huge.

In the early 1970s, Dr. Clyde McKinley and Dr. Abraham Lapin [34] adopted oxygen index test results as desirable parameters for evaluating nonmetallic materials for oxygen service. At the time, it had only been used for plastics and there had been some dabbling with liquids. McKinley and Lapin had always wanted to extend it to pressurized testing and use with metals. In the late 1970s the writer conducted early tests on bulk steel at pressure [32] and in the early 1980s led pressurized testing of polymers for Michael Benning that was published [35], and later supported John Zabrenski's breakthroughs [31,17].

In those 1980s, when John Zabrenski and Michael Benning did extensive testing of numerous metals [17,31], it was the basis for much pressure this writer applied to develop the ASTM Standards G94 and G124. Zabrenski discovered the extreme subtlety (to the point of insidiousness) of the flammability of aluminum especially in high purity oxygen. He and this writer shared the same lab at the time. And contrary to some spectacular but dubious "discoveries" back then (like cold fusion and excess oxygen), that enjoyed too-rapid publication, I witnessed how he and Michael Benning (whose earlier work [35] had already provided an explanation, had to run numerous gauntlets to satisfy numerous skeptics (including himself) in verification of its validity before being allowed to publish his results to a skeptical oxygen safety community in 1987 [31]. As one of his strongest (and earliest) supporters, I was among those commissioned later to disprove his results (as a way of confirming them). Their work has withstood the test of time, has led to the revision of Glassman's standard text [36] and along with this worker's later testing that witnessed a highly explosive aluminum/LOX scenario [37] and assorted testing after that led to the development of a number of Compressed Gas Association Pamphlets for addressing aluminum system safety with high purity and high-pressure oxygen. I consider them the most crucial oxidant safety discoveries of this era.

Because NASA's central role and perhaps more so because NASA was an important corporate customer (though not for oxygen), prior to publication, we promptly brought this early discovery to NASA's attention in numerous ways, including a carefully vetted letter (in the mid-80s) and personal contacts encouraging similar testing thereat (1989, 1992), and complete publication of the experimentation as cited herein. In the spirit of collegiality with both myself and coworker Dr. Sheldon Dean as members of a NASA WSTF RTOP (Research Technical Operation Plan) to re-evaluate NASA's methods of testing for oxygen safety (which led later to the replacement of NASA Document 8060.1), we discreetly repeatedly prodded its examination. And finally later when we became confident (when we surmounted our own Great Wall of Default Skepticism) of the validity of these data with publication of the several papers in the G4 collegium. None of which "seems" to have led to similar testing at NASA (this worker does not have data on unpublished or obscure NASA work). From the (possibly incomplete) data this worker has on the NASA+ and NASA-at-Large perspective on aluminum, it appears macro-NASA may have ignored all outside data on aluminum testing. Indeed, in testing NASA has mis-stated data on the oxygen they used [38,39] and in at least one case did not specify it at all [40]. This worker is unaware of indication NASA has tested or even appraised the extreme purity hazard. He can only hope they have.

Hence, this worker feels today and felt during the time much as the guy must have

felt who tried to warn a too-confident NASA management of the hard O-rings before the launch of Challenger. And indeed, my initial fear upon seeing the Challenger incident was whether it was an aluminum LOX-tank explosion (thankfully it was not). Indeed, over the years I have followed those data I have had access to. Test results and papers in many of the G4 symposium series, materials and related references in the NASA+ generated Manual 36, first and second edition. Some of these were devoted to lithium-aluminum alloys that were first used occasionally on the shuttle and were considered for the Artemis missions. I remain concerned but it seems NASA+ is committed and once again confident.

After the discovery of these aluminum data, I dreaded every subsequent Shuttle launch especially any that had the lithium-alloy LOX tank (though those were often not known). However, I must stipulate that NASA may have more-than-adequately addressed the dichotomous risk of these tanks and may have just not shared its analyses (despite being a more-than-suitable and worthwhile topic for a G4 symposium compared to all the many other NASA papers published so copiously). However, it is this worker's fear that based upon what has been published, in the ASTM G4 forum and its first two editions of ASTM Manual 36 is whether the hazard review of these tanks was adequate. On more than one occasion, full appreciation of aluminum compatibility was not achieved until the hazard was personally witnessed in laboratory testing or field incidents. It would be a damn shame to gain that appreciation during an Artemis launch. As a result this worker shall now dread Artemis launches with its even bigger aluminum-alloy LOX tank, and not just as a taxpayer, for if an undesired worst-case incident occurs, if NASA-at-large has not done its due diligence, it could be a more spectacular and defaming tragedy than Challenger by far.

Of course it bears repetition again, this last conclusion is highly speculative drawn without full knowledge of NASA's precautions, based only on the substantial but nonetheless scant published material and my personnel experience both in the lab and in working with NASA+ within ASTM G4 and NASA-WSTF including within its RTOP Committee. This worker hopes he is wrong on this one. It will be way beyond tragedy if the nightmare obtains.

## Closure

Big men, overly attractive women, important customers, and the too-smart among us are intimidating whether they try to be or not. The same can be said of NASA-at-large and the apparently similarly arrogant NASA+ clique defined herein. Yet, NASA is *not* omniscient. Despite its often extreme talent and self-confidence, NASA does err at times. Yes, we really do know NASA+ puts its pants on one leg at a time, just like so many of the rest of us but we try to think the best of folks. So if NASA+ drinks too much of its own Kool-Aid, it does itself, its own, as well as the rest of us (taxpayers) an undue disservice.

And yes, it is no more sporting of this worker to beat up on NASA+ when it can not defend itself here any more than it was sporting years ago when this worker was unable to defend his work. Neither is fair, but it was a key NASA+ figure who stressed in G4 meetings: This world is *not* fair.

Comeuppance can come for us all. In the macro-NASA-at-large case the world-wide worst Dunning-Kruger-like example has to be Challenger. It appears competent warnings were ignored by arrogant superior too-self-confident minds [and one can only wonder if the company that warned has been shunned, mocked and punished for violating Rule 1]?

So yes there are many good reasons, as well as not-so-good reasons, to revere NASA and even the flawed NASA+ contingent. But that can not ensure against a very unfortunate comeuppance if I am not wrong. Maybe NASA+ has got this stuff knocked, and I am just being a cruel and envious incompetent spoilsport. However, I would argue this dissent is *not* spoilsport. It is for the right reasons. It does not seek to spoil other's pleasures especially by *not* joining into an activity. If anything, my failed efforts to join in have been seen as being more like unwanted stalking than simple disagreement. Or more like being shunned. But the dissent this paper supports recognizes and acknowledges the evolution and imperviousness of today's G4 even as it disagrees strenuously with that evolution and hence disengages. And as a result, claims of "consensus" standards from the NASA+ dominated ASTM G4 forum are seriously weakened. The G4 consensuses today heavily influenced by imperfect NASA+ influence are not full, nor robust. They are defined and exclusive, and quite possibly severely flawed.

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