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The Great Excess-Oxygen Hornswoggle?[2]

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ABSTRACT: For more than 35 years, three documents have been repeatedly cited for the discovery of large-scale, even massive, amounts of "excess oxygen" in the burning droplet slags of iron in oxygen [oxygen-to-iron-ratios greater than relevant iron oxides (1.0-1.5) in a homogeneous liquid]. Numerous combustion issues have been therefore attributed causally to "excess oxygen" that have affected oxygen hardware hazard evaluations and how they are done. Examination of error sources and accuracy in methodology indicate the data cited in the three sources to support this "discovery" are within and in some cases are much smaller than reasonably expected error rates that would have been present. Some data are contrary. Such faulty analyses have been cited to support the existential conclusion while data that dispute it have been ignored and even censored. Re-examination of these data suggest massive or copious excess oxygen was _not_ found and may not be likely to have been present nor was the slag likely to have been homogeneous. If anything, data from the steel-making industry, at least one censored study and this paper's re-examination suggest that *excess iron* [iron-to-oxygen ratios greater than relevant stable oxides (0.66-1.0) typically in (often two-phase) liquid slag] is more likely present and is often substantial and that the liquid oxide fraction may have only small amounts of oxygen as either dissolved oxygen or as purported ferrite ions.

KEY WORDS: excess oxygen, iron combustion, ASTM Committee G4, NASA White Sands Test Facility, WSTF

Good news! this worker has just used a trusty yardstick to measure the distance from here to the moon. It is nine feet plus-or-minus 239,900 miles. Or might this just be a hornswoggle? This breakthrough is dedicated to what this worker calls the NASA+ clique, a collection of very self-confident (sometimes too-self-confident) NASA workers, former NASA workers, affiliates and groupies who for more than thirty years have perpetrated what this worker now considers to be a hornswoggle on the oxygen and other oxidant safety community whether initially intentionally or not. I could be wrong, I am not perfect, and I am currently old, perhaps to the point of dementia . But hey, I am entitled to my opinion. NASA+ is also allowed to disagree and has had those thirty years to clean up what is considered and doubtless proven here to be a mess.

One could similarly wonder if Albert Einstein's original publications on the bizarre topics of Special and General Relativity were actually perpetrating a hornswoggle of his

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[1] No mailing or email address as this is permanent archive opinion.

[2] Opinions, analysis, and speculation in this paper are on a public service, "as-is," use-at-your-own-risk basis.

own. Was he an early form of what would become the Internet troll. No one at the time, possibly including Einstein himself believed his ridiculous musings. Was he merely playing Devil's Advocate to make a point on how physical sciences can be portrayed in differing even preposterous formats? Did he really believe it? Was it a practical joke on the gullibility of the less-smart masses that has now become Gospel science? Does this mean the last laugh is on Albert?

Excess oxygen is a real thing. It is present when one adds too much air to a flame. And enough of it, too much oxygen, can result in the excess acting like a diluent and sucking up heat needed for propagation. The very stuff that makes fire possible can also extinguish that flame and do other stuff as well. So what?

In 1989, right around the time Pons and Fleischmann were announcing how to achieve cold fusion, perhaps a hornswoggle of another kind, workers in the NASA WSTF Lab were reporting at an ASTM G-4 symposium how they burned 316 stainless steel in oxygen [***1***] [3]. They calculated what the size of the droplets that form and fall should be and the droplets were bigger than they should have been. *A lot bigger*. Two and a half times bigger. Since there was only steel and oxygen present (they thought, it turns out in error) the extra volume had to be oxygen. EXCESS OXYGEN. This was an awful lot of liquid-density oxygen to form at temperatures approaching 3000K. It was in this humble mere-BSc opinion a shocking preposterous discovery, if true. A PhD candidate at UNMS was a coauthor , largely considered to be a wunderkind who was pursuing his PhD on work of interest to WSTF at both WSTF in New Mexico and at NASA Lewis (now NASA Glenn) in Ohio.

By 1990 that now established wunderkind, brand-spanking new PhD Ted Steinberg[4], would complete his dissertation thesis, (*DT*) [**2**], in it thanking NASA for the funding and with a series of peers from his PhD Committee-in charge (including coauthors Frank Benz of NASA WSTF, Dr. D. Bruce Wilson, and Dr. George Muhlholland) and later also with coworker Dr. Joseph Kurtz, would soon be publishing derivative papers. Before long he joined the University of Queensland in Australia where he would supervise a parade of PhD candidates doing similar combustion research using a drop tower there. He would author, coauthor and be the connective tissue with a massive "publish or perish" CV (Google Scholar listed 186 published papers as of 2022).

This worker would run afoul of Dr. Steinberg and his NASA+ entourage in 1995, when he would submit to the revitalized ASTM G4 seminar series a not-so-brief paper [***3***] on a quite-brief, mere several-week, study that conflicted with some of the results in Dr. Steinberg's Dissertation Thesis and a consequential derivative paper with three coauthors that published in the prestigious journal *Combustion and Flame* [***4***]. My brief paper was one of the few papers, and maybe the *only* paper at the time, rejected for publication for technical reasons in the ASTM Committee G4 Peer Review Process in more than a decade of operation and finally by editorial obstruction by Dr. Steinberg, himself, in his first outing as a dominant editor (a role he has apparently continued in ever since 1998). The unseemly peer

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[3] Italic numbers in brackets refer to the reference list at the end of the paper.

[4] The use of the description "Wunderkind" started out as admiration but over time has shifted to also reflect sarcasm.

review details are covered elsewhere [***5***]. Suffice it to say, Dr. Steinberg's entourage/cronies/hench-men proved to be harsh peer reviewers (the kind pre-Steinberg ASTM Committee G4 had explicitly sought to avoid) and Dr. Steinberg proved a diligent watchman at the gate. The paper and its "ridiculous" challenge to *his* work was un-welcome on *his* watch.

I have often heard and repeated the oft-cited aphorism (drilled into me by my boss and mentor: Dr. Clyde McKinley): technical papers like this are not like mystery novels. You do not have to read to the end to find out who dunnit. It is my suspicion (but I should say judgment) that Dr. Steinberg and his entourage screwed the pooch with poorly designed and executed experimentation and then, apparently expecting a breakthrough and thinking they had one, brashly heralded their discovery of massive excess oxygen in the slags of burning iron (like it was cold fusion) with a badly flawed DT and derivative publications that compounded the errors, and it would not be the only misstep they would be unwilling to fess up to or to even contemplate criticism of (they were not about to clean up their own mess). Therefore be advised I damn the earlier peer reviews and censorship of my paper, I fear the vitriol, arrogance and censorship levied about it permanently poisoned the ASTM Committee G-4 collegium and were not intended to elevate the discourse and technical merit but to simply prevent publication of the contrary result from revealing their shabby work. This is my mere-BSc opinion and I shall provide many bases for this opinion that the reader can evaluate though you may be unlikely to be willing to endure the work. That's okay.

In my humble opinion, the NASA+ blundered and for very human and very usual frailty (denial/ego) or less forgivable frailty such as Dunning-Kruger Effect opted not to acknowledge its blunder and bad luck but to instead (like so many of today's politicians) cover it up with peer review abuse and editorial privilege abuse and these relevant shenanigans are equally important but not the main thrust here. That is for the other commentary elsewhere [***5***]. This focus here is about experimental process.

As of this day in 2026, some decades later, after much deference in the spirit of "quality process", I shall herein return the favor by peer reviewing the *DT* [***2***], key initial papers [***1,4***] and followup [***6***] that purported to discover massive or maybe just large-scale (copious) or maybe even just substantial excess oxygen in the slags of burning iron rods that my work disputed in such unwelcome fashion. A claimed discovery that I, but not I alone, believe is wrong yet has had unfortunate staying power (that I attribute to the peer review and perhaps other abuses) and has been cited many times by many lettered workers albeit most often by those in the NASA+ clique many of whom share Dr. Steinberg as a co-author or in some cases as an advisor for their own PhD candidacies.

Whether there is actually any excess oxygen (as they variously defined it) or even mere lower-level dissolved oxygen present in those slags or not, I shall seek to prove Dr. Steinberg and his minions did *not* discover *massive* excess oxygen nor did he discover merely really *large-scale* copious excess oxygen in those slags. Alas!, I can argue and do believe the case is compelling that there is negligible excess oxygen by even the most limiting definition present in some circumstances (namely, at low pressures), and I can offer strong surmise against copious excess oxygen ($CXSO_2$) being present at higher pressure. However, absence of proof of existence is not proof of nonexistence. And if required, I shall contemplate and seek ways to build even more compelling future proof to actually definitively resolve the dilemma.

I do wish to apologize to the subjects of this critique, especially Dr. Steinberg (whomsoever I shall not always continue to treat with default prior deference). I fear they may have gotten the impression that I am a loose cannon (or door mat) who in this and related contemporaneous episodes to be addressed in other opinions, fired off a brain fart mindlessly challenging their distinguished intellects and making a decision to simply censor my vacuous efforts easy rather than take them seriously. Unfortunately that was *not* the case. My employer at the time had a rather conservative constrained approach to its publications and experimentation, and collegiality and oversight, and especially "quality", were staunch corporate ethics. In preparing my submissions, I *always* conferred with numerous highly credentialed associates, all competent, many world-class, in some cases affiliated with leading national academic institutions, some of which will be reviewed in detail. Although personally quite shy and insecure, and out of respect for the lettered among us, I felt it unseemly to name-drop as a way to gain credibility for my work and in so ***not*** doing may have made it easier for the G4 Committee's growing NASA+ cognoscente to dismiss me as irrelevant rather than objectively evaluating my results against their own results. I am truly sorry for that and do insist I was not baiting any traps to spring here after nearly thirty years of polite and quality deference.

Dr. Steinberg's Dissertation Thesis (*DT*)

In the spirit of both revealing who dunnit early on and in observing the high level of scientific rigor that has been expected from this lowly commentator, with this unsolicited peer review of *Metals Combustion at High Pressure in Normal Gravity and Reduced Gravity: Model and Experiment*, I hereby reject this manuscript for it declared purpose: the partial fulfillment of the requirements for the degree of Doctor of Philosophy. I do not seek to disparage Dr, Steinberg's giant intellect which I do believe to be fully awesome. However, I have worked with several giant intellects in my day whose grasp of experimentation fundamentals were weak yet who had no perception or appreciation of their own limitations (Dunning-Kruger Effect[5] to be addressed elsewhere). Extensive bases for this opinion are given later. Indeed, while I find the technical merit in the experimentation lacking, it is even more troubling that the six members of the Committee-in-Charge comprising at least five members with doctorates and NASA WSTF's Frank Benz (whom I know personally and whose talent I once respected), that the latter who funded the work through WSTF, allowed such shabby work to proceed much less to find its way into anointing an NMSU doctorate. Is it possible that Dr. Steinberg is some kind of technical Svengali, a cult leader, or a Frank-Abignale-scale imposter? More likely he comes across as a star worth hitching one's wagon to. But in my experience he does not suffer fools (like this commentator) kindly and was far too resistant to this challenge of his work which would have granted him an opportunity to redeem himself, if the work was redeemable. Perhaps he was just so occupied with critical really important NASA work that he could not spare the time to swat away a bug like me,

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[5] The Dunning Kruger Effect is "a cognitive bias where individuals with low competence in a specific area tend to overestimate their abilities, while those with high competence may tend to underestimate their expertise." In this case one fears a high competence type appears to have led to excessive overestimation.

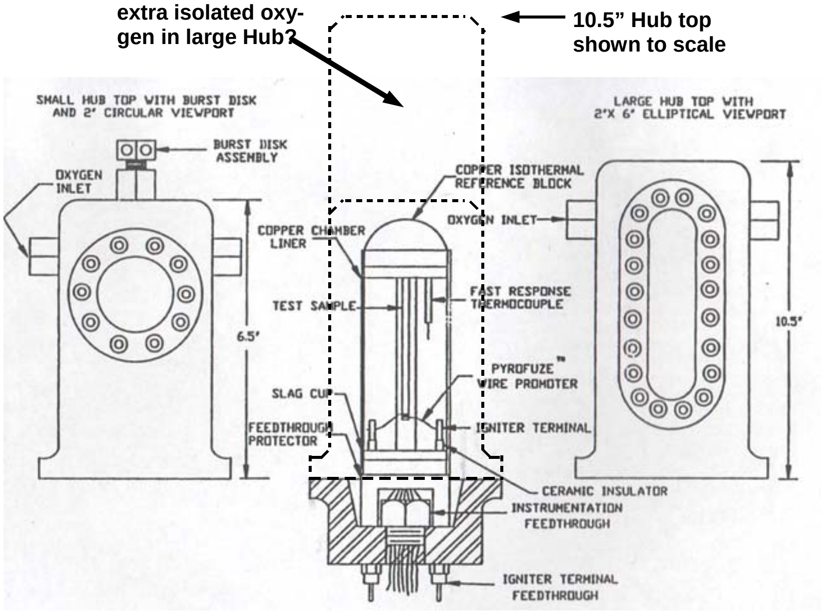

FIG. 1 —Annotated t*est vessels shown in Dissertation Thesis* [***2***].

but for the purposes of this analysis, I rather suspect he was embarrassed at his blunder (a category of blunder I suspect he was again a victim of years later in his career) but was experiencing a quite normal denial, if anything cubic-magnified by his lofty status.

Three members of the Committee-in-Charge joined Dr. Steinberg in a fantastical derivative *Combustion and Flame* paper [***4***] that also has that most spectacular massive excess oxygen “discovery” as an important if not central focus. I reject it also for all the same underlying reasons. One wonders what the *Combustion and Flame* peer reviewers were thinking.

Dr. Steinberg’s purported “discovery” of massive excess oxygen in burning iron was based upon tests of upward burning iron rods in ABO, Aviator’s Breathing Oxygen, at pressure between ~250 psia (1.8 MPa) and 1000 psia (~7 MPa). In 1989, Benz, Steinberg, and Janoff [***1***] had first signaled the “excess oxygen issue” (ASTM STP 1040, page 206) when they calculated the expected volume of a molten droplet of 316 stainless steel slag and measured actual droplets only to claim:

> “...the volume of the molten mass is 2.5 times greater than the volume that is occupied by the metal and metal oxide. This can only occur if excess oxygen exists in the molten mass.”

This paper is cited in the *DT* but I have not found this same 2.5-times-greater claim asserted in the *DT* dated one year later but perhaps written much earlier than that. Notwithstanding the huge red flag this should be (even among those of us lacking so many letters). Nonetheless the *DT* describes major effort focused on measuring such excess oxygen. Work that ignored the counsel of Occam’s razor to explore easy answers before pursuing complex answers.

Dr. Steinberg describes tests conducted in the apparatus of Fig 1 with two top "hubs". A specimen was suspended vertically as shown and ignited at its bottom in the central assembly in oxygen at pressure. In a series of micro-gravity (free-fall) drop tests, the short top "hub" was used (presumably to reduce drop weight) with a small view window because the drop time was very short (2.2 seconds[6]) to allow combustion of an entire specimen. In normal gravity tests the tall top "hub" was used with a long window to allow longer combustion photography.

Note: the NASA drawing of the long hub appears not to be to scale. If the short hub is 6.5 inches then the long hub as shown would only be about 7.5 inches tall rather than the 10.5 inches noted. I created dashed outlines to show how the two hubs would situate over the test specimens to actual scale. The short hub setup was measured to have a 39.6 cubic inch water volume and the long hub setup was measured to have a 79.6 cubic inch water volume which suggests the cited dimension rather than the drawing is correct. But this was not verified.

In Dr. Steinberg's approach, an ideal gas calculation was used to deduce the dynamic oxygen inventory in his test vessels during combustion based upon their water volume, and the oxygen pressure and temperature, then figuring inventory losses by taking differences from the initial volume. Therefore, by difference, the amount of oxygen reacted from the inventory and/or dissolved into the slag to form the excess oxygen presence in the slag was determined. It was based upon this dynamic calculation of the oxygen inventory in the vessel during combustion. In the ideal gas law, pressure (P), Temperature (T), volume (V), mass or moles (N), and the universal gas constant (R), are related as :

$$N = PV/RT$$

Hence, if oxygen is reacted with the iron into a liquid of low volume (slag), or if excess oxygen is absorbed into the slag at liquid density, the calculated vessel oxygen inventory drops, reducing N. Hence, any errors in measuring P (*which decreases with decreasing N*) that are in error on the low side, or more likely, measuring T (*which increases with decreasing N*) that are in error on the high side will produce or magnify a false appearance of excess oxygen. This is basic mere-undergraduate-style experiment error analysis absent in the subject works.

Figure 2 shows the test results from Steinberg's specimen test #2 in the *DT* [***2***] and from Figure 4 in the *C&F* paper [***4***]. Note that at approximately 7 to 8 seconds a minimum is seen in the oxygen mass at about 112.3 grams down from an initial 119.3 grams. This is at a time where the stoichiometry curve marked Fe_2O_3 indicates the inventory would be about 117.8 grams if only Fe_2O_3 (not likely present at slag temperatures) has been produced and 118.3 grams if FeO (almost certainly produced at slag temperatures) has been produced. Hence only about 1.0 to 1.5 grams, maximum, of the loss could have reacted with the iron to form oxide. In comparison, the calculated oxygen inventory has fallen by about 7 grams which is 4.6 times as much as needed to form Fe_2O_3 and about 7 times the amount needed to

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[6] This short available combustion time may have added greatly to efforts from within the NASA+ contingent within ASTM Committee G4 over a decade to push for adoption of a short combustion criteria (challenged then and herein) to define combustion above a specimen's fire limit.

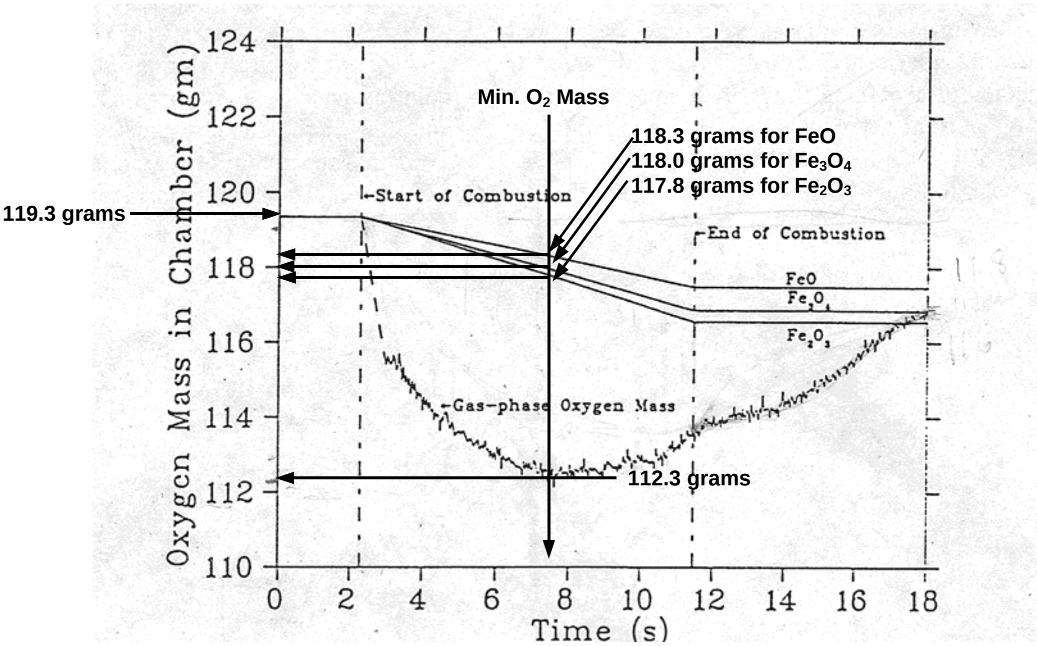

FIG. 2 —Annotated s*pecimen Test 2 in DT* [**2**] *and Fig 4 in C&F Paper* [***4***].

form FeO the only oxide likely to be present at combustion temperatures. Furthermore this indicates the total assumed reduction in oxygen is distributed throughout the series of droplets that formed up to that time. If all of the fallen slag droplets have released all their excess oxygen back into inventory, then the oxygen in the pending droplet could be 10 to 20 or more times greater. This is even more amazing! Indeed with seven moles of monatomic oxygen for every mole of iron it is not oxygen dissolved in iron, it is iron dissolved into oxygen!

Are These First Generation Data Valid?

In the past this worker has focused on two error mechanisms that appear to have been present but were not adequately addressed in the paper or *DT* nor has he seen them addressed since: (1) *Finite Element Errors*, and (2) *Radiative Heating of the Thermocouples*. Having now examined the underlying dissertation [***2***] in this review I shall cite a third massive, if real, possibility: (3) *Isolated Oxygen*.

(1) *Finite Element Errors*. A comparison of both Dr. Steinberg's earlier (NASA-WSTF) and later (uncertain location) testing is in order. In both works, there would have been a layer of oxygen gas next to the surface of the burning rod, falling slag, and the slag puddle at perhaps up to or beyond 3000 K and a copper containment vessel surface gas envelope at perhaps ambient temperature.

Furthermore, it is known that in tests like this (I have personally witnessed such tests through a window above a burning specimen) when ignition is effected a strong central current of updraft of gases, often transporting entrained debris shoots upward and impinges on the top of the vessel. The velocity of this stream is substantial. This is due to elevated tem-

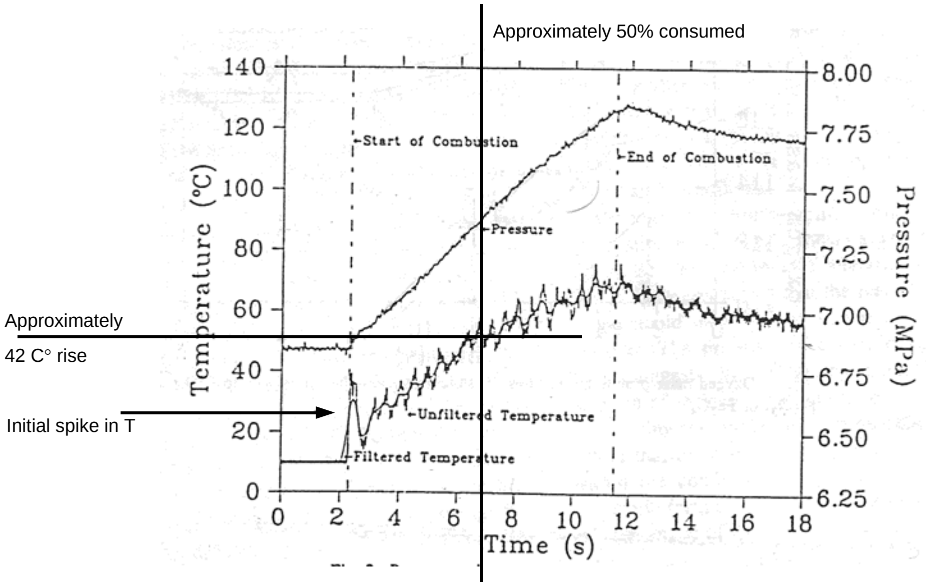

FIG. 3 —*Annotated Specimen Test 2 in DT* [**2**] *and Fig 3 in C&F Paper* [***3***].

perature and consequently reduced density of the hot gases. The buoyancy of these gases might easily produce a thermocline (a layer of hot gas) that could buoy up to fill the top region of the vessel and should be expected to do exactly that even though a closed loop of flow must also form in a closed vessel.

Convective swirling about inside the vessel would be at intermediate temperatures in the swirling gas inventory. In the reports, four thermocouples were used (their drawing, Figure 1, exhibits only one). Dr. Steinberg's dissertation and later paper cite these four thermocouples were at differing (apparently unspecified heights). It also notes that when two each were situated "top of vessel and half way", stratification at 840 psia produced a temperature difference of about 25 C°, which would be half the calculated average temperature change in some tests at the 50% combustion point. This should have been a massive red flag.

However, there are many ways in which the convection currents during such a test may act to cause non-uniform temperatures in the vessel. Indeed, in fluid-flow analysis an effort to estimate the quantity of gas in the vessel would best use a finite element method dividing the chamber into many (dozens, hundreds, thousands, or more small instrumented volumes then calculating the oxygen in each element and adding them. Using just four thermocouples is worse than naïve optimism in this opinion.

As shown in the *Combustion and Flame* paper [***4***] (Figure 2 and Figure 3 herein) and many similar figures in the dissertation [***2***], the calculation of the point of greatest excess oxygen absorption is predicated upon an "average' temperature rise of roughly 42 C° at the point where 50% of the specimen had converted into slag. This was in comparison to roughly a 25 C° rise expected if no excess oxygen were absorbed or reacted into the slag. This however, might greatly understate the actual absorption, if the NASA+ premise is correct. At the 50% conversion-to-slag point half the iron slag would have fallen to the vessel bottom, and begun returning the excess oxygen to the ambient atmosphere. If the desorption were immediate, then the excess oxygen in the single falling droplets might calculate out to being many times more than the stoichiometric amount.

(2) *Radiative Heating of the Thermocouples*. At the temperatures of burning metals, radiation is substantial. Impingement of that radiation onto a responsive surface can heat it significantly above its ambient. In my initial speculations, I noted that in similar tests I was associated with, a thermocouple was located in a long tube in which the isolated gas it contained was unlikely to change temperature during a test and the thermocouples looked at two regions of a specimen. As the specimen burned, radiation impinged on the thermocouple to produce a heating spike of several degrees and allowing velocities of combustion to be calculated.

If radiation impinges on the oxygen inventory temperature-measurement thermocouples cited in Dr. Steinberg's dissertation [***2***] they would produce a signal that indicates a higher temperature then that of the local ambient or bulk gas. Again the higher temperature error, would appear like a lower inventory of oxygen and a false excess oxygen calculation.

Dr. Steinberg's dissertation discusses radiation. His work used extremely small thermocouple wire (thinner than most hair) to achieve rapid response times. To address the problem of radiation, his preliminary testing placed two identical thermocouples side-by-side during a test, and one of them had some form of a ceramic shield in place to block radiation. He notes:

> "...the shielded and unshielded thermocouples responded similarly to the combustion event. Because of the similar responses exhibited by the shielded and unshielded thermocouples it is concluded the radiative component of the energy entering the gaseous oxygen from the molten ball, that is incident on the thermocouple junction, is nominal, and unshielded thermocouples are adequate to describe the temperature history of the gaseous oxygen." (*DT* [***2***], page 119.)

Fig. 4 exhibits the test data that Dr. Steinberg used to observe that the two curves were the same so radiation is unimportant. Nonetheless, in his actual testing, when describing the temperature plots, his page 131, he declares: "The initial peak displayed on most of the tests was due to the high luminosity of the aluminum palladium igniter." And on his page 123, he comments:

> "One problem encountered with the use of the unshielded thermocouples were spikes in the obtained data, with regular frequency, due to the bright flash associated with a molten ball detachment (and the igniter)."

The earlier Fig 3 exhibits the initial spike in temperature mentioned.

I submit, therefore, that the comparative testing of shielded and unshielded thermocouples did not prove radiation was irrelevant, but rather that the shield used was largely ineffective. Hence both thermocouples may have been similarly influenced to a large degree by radiation and were therefore a substantial source of oxygen inventory error.

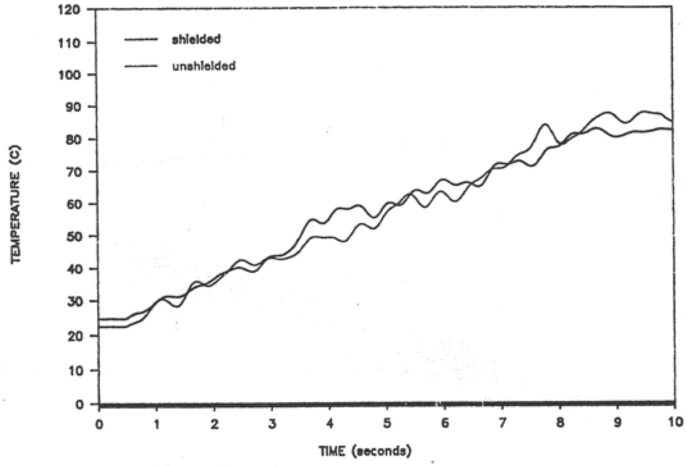

Figure 22 - Shielded vs unshielded thermocouples
after filtering

FIG. 4 —*Shielded versus unshielded thermocouples in DT page 121* [***2***].

(3) *Isolated Oxygen*. One especially nasty impediment to an accurate ideal gas analysis is gas that is isolated, stagnant in connection piping or in cracks, nooks, crevices etc. These fractions of the gas inventory, may be less susceptible or even immune to heating to the average temperature, may not mix into the swirling gases, and may remain near the initial temperature, hence their impact on the average can be substantial. Dr. Steinberg's dissertation indicates that all work was done in the same vessel base shown in Figure 1 herein and two upper hubs (left and right) were used. A short hub had a small window perhaps because the combustion time was short (the drop time) and extra weight is harder to catch. The more detailed "normal gravity" work used the tall hub with the longer window that would allow viewing the entire combustion event. If these data are correct a significant even major error mechanism may have been present.

The dissertation describes an internal copper shield around the burning-specimen perimeter (except for the window-slit opening), shown in Fig 1 used with both top and bottom shields. Figure 1 was modified herein and in this case, the outlines of the two hubs are shown in place on the base and the large hub outline has been lengthened to agree with data in the dissertation (it appears this dimension was not drawn to scale in the figure). Note the copper "shield" used to prevent burning metal contact with the pressure containing vessel. The tubular section wall is cited as being one-eighth or one-quarter inch thick copper.

This design creates an annular clearance between the shield and vessel hub with little circulation and potentially significant isolated volume, for which no measures are cited to prevent oxygen isolation and stagnation. Furthermore, in determining the open volume of the two vessel configurations to use in doing the ideal gas oxygen inventory calculations, an equilibration (dissertation Appendix B, pages 249- 250) was made against a calibrated volume and the small hub-version (used for the drop tests) measured out at 39.67 cubic inches while the tall hub version measured out at 78.36 cubic inches. Thus it appears that use of the large hub increased the annular potentially-stagnant volume of the vessel by more than the volume of the active test region volume. Maybe the figures are inaccurate, or their depiction is faulty, but there does not appear to be discussion of any thermocouples trying to measure these annular volume temperatures. Nor do the four temperature traces in a typical test suggest any of the four thermocouples were measuring an annular volume. Of course, again, the vessel description may be in additional error.

If this annular volume is treated as being at the initial 10°C (the initial temperature for the test of Fig 3 then the measured average temperature of 52°C (42 C° rise) at the mid-point of combustion (Fig. 3) would calculate out to have been only 21°C if it had been mixed in with the stagnant gases (if this surmise is correct) and Presto! suddenly the calculated drop on oxygen inventory is halved. Due to just this one potential error mechanism. Even if some excess oxygen were present (using valid methods) it is doubtful this experimentation *could* have found it.

In my NASA+ peer- and editor-censored paper [***3***], I had suggested the value of running the test with a thermite specimen and inert gas (which can not absorb oxygen from the inert atmosphere). Thermite reacts iron oxide with aluminum in a way in which no argon or helium would react nor should significant oxygen have been released. However all three of the above important, apparently ignored, error mechanisms would still be present and might lead to a massively depleted “false-excess-oxygen” inventory calculation, possibly approaching the same “depletion” that was being measured for oxygen atmospheres. This apparently did not occur to Dr. Steinberg or his many associates and coauthors. Nor sadly has this suggestion been picked up on in the intervening 27 years.

It is perplexing that this step does not appear to have been taken in preparing the dissertation. It is even more perplexing that when the issue was raised in 1995, that the entire NASA+, especially Dr. Steinberg, did not jump on it. Regardless of whether it would have proven or disproved the excess oxygen “revelation”, it is better to find one’s own errors and correct them (to clean up your own mess) than to have an outsider do it, especially a low-class outsider like me, find and correct it for you. Unless of course the NASA+ knows they have blown it and are in denial, and just do not want to fess up, and chose to rather cover it up, especially when they have a dominant editor on every symposium proceedings since then who has demonstrated a willingness to act as a censor (rather than recuse on the basis of conflict of interest) even when it benefits he, himself .

Perhaps all three of these issues can be explained away, ...certainly, they should have been. They do not appear to have been. This work has been cited repeatedly as authoritative in numerous DTs (admittedlysome by PhD candidates for which Dr, Steinberg was their advisor) and papers for more than two decades. Ideally one might publish a paper about them in the G4 collegium (which at one point endorsed “heretical” papers) and an opportunity for rebuttal might allow the authors to enjoy a “teaching moment”. However, in this case great energies were exerted to ignore the Sense-of-G4 Committee position favoring no-fault publication and to welcome even heretical papers but apparently there are some for whom heretical commentary must be silenced and shunned. Hence these issues can only be presented here outside the ASTM G4 forum as speculation. But they do look very much like the Emperor’s new clothes.

If these aspects can not be reconciled, in this peer-reviewers opinion/judgment (a judgment I am as happy to make as were the very negative peer reviewers for my efforts), these early papers are excessively flawed (and in their word “careless and sloppy” and “played fast and loose with the data”). In keeping with the high non-collegial standards of my peer reviewers from the very principled Dr. Wilson and Dr. Kurtz, and my ultimate censor Dr. Steinberg for my paper [***3***], I must return the favor and offer this peer reviewer rejection of the dissertation thesis (and the derivative paper) for it does not justify the award of a doctorate degree. The failure of collegiality is perhaps the worst outcome, but a negative impact on oxygen safety technology over the years is not to be ignored either.

The NASA+ and most especially wunderkind Dr. Steinberg has had 30 years to clean up what I have referred to here as his own mess, and the reader gets to decide if that is what this is. So were there any new developments during those next 30 years? We shall see next.

A Disguised Effort at “Correction”?

When I presented my highly offensive brief study [***3***] (that I submit still holds up well today) a series of efforts were launched to persuade me to withdraw the paper. I received a fax from Joel Stoltzfus (NASA-WSTF) whom I consider a charter NASA+ member. As described in more detail elsewhere [***5***], the fax summoned me to what I now consider to have been a technical intervention (a conference call with a lot of helpful NASA+ folks on the other end, all of which found my paper without merit and who like agents in a drug-addict intervention sought to berate me into withdrawing it). The fax included (1) a pre-publication copy of a follow-up paper that had been submitted to *Combustion and Flame* authored by none other than Dr. Steinberg, Dr. D. Bruce Wilson, Dr. Joseph Kurtz and Joel Stoltzfus: “**The Solubility of Oxygen in Liquid Iron Oxide During the Combustion of Iron Rods in High Pressure Oxygen”** said to be already undergoing peer review at *Combustion and Flame* and (2) a written analysis from Dr Wilson that argued my observations were wrong. The cover fax that summoned me to the conference-call meeting/intervention called my attention to the new paper:

> “Please note that Ted [Dr. Steinberg] has relaxed his assertion regarding the exact quantity of oxygen that is contained within the slag system formed on the burning iron rods.” (Dr. Kurtz would also later make this claim.)[7]

Was Dr. Steinberg cleaning up his own mess? ...Co-author Stoltzfus’s assertion turns out was not quite accurate (nor would he be an author listed on the final paper).

Both the initial and final “**Solubility**...” paper [***6***] did not retract nor correct any prior conclusions or observations, did not “relax” any data but did embed the phrase (page 34):

> “...no quantitative values of this ratio [the ratio of oxygen to iron] can be determined *during* combustion, due to temperature gradients in the gas...”

Since this is exactly what the DT and derivative paper purported to do, this may imply repudiation without actually saying the words? Does this admit the Steinberg *DT* methodology was

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[7] Dr. Kurtz later also asserted this work: “...does acknowledge problems in their previous analysis, but which also gives strong evidence for the existence of a lesser amount of excess oxygen in the molten mass.”

invalid without retracting the data? Perhaps this was somewhat an indication of recognized vulnerability of the Generation 1 paper and the dissertation upon which it was based, even if just subconsciously. However, the conclusion of that very sentence is:

> "...the trend indicated by the experimental traces (e.g. Fig. 2 [in the source paper]) clearly indicate that even higher oxygen-to-iron molar ratios could be present during the actual burning."

They can't be determined (sort of like a Heisenberg's Uncertainty) but they might still be there. My "controversial" paper was challenging the dissertation thesis [***2***] and the derivative paper [***4***] and perhaps this newest perhaps "more enlightened" paper [***6***] as well. Hence my results must have been seen as reflecting badly also on this as-of-1996 yet unpublished work. Please recall, this is the paper that supposedly "relaxed" Dr. Steinberg's "assertion regarding the exact quantity of oxygen that is contained within the slag system formed on the burning iron rods" but, as just noted actually did no such thing.

Note that this Generation 2 experimentation, was apparently similar testing to the "normal gravity" work in Dr. Steinberg's dissertation thesis except apparently this normal gravity work was conducted with the long hub (of Figure 1 though cited differently in some versions) on the apparatus that has the larger water volume and so one of the several temperature-error mechanisms would have had significantly less effect. There may have been a lot of isolated oxygen as well as turbulence. Did this change in procedure reflect an appreciation of that problem? However, in Gen 2 testing, the oxygen inventory was apparently calculated the same way except at ten minutes *after* the specimen combustion was complete in what was said to: "definitively achieve thermal equilibrium". In addition, the mass of the slag produced was recovered, weighed and compared to the consumed mass of the specimens that produced it.

Figure 5 exhibits data from their Table 2 for this follow-up two prong approach to assert that there is at least some (even if less) "excess oxygen" in even cooled solidified slag specimens. In this second generation approach, the same combustion methods used in the first papers and dissertation were apparently used, but instead of a dynamic oxygen inventory calculation *during* combustion, the slags were allowed to cool for ten minutes so that a claimed "equilibrium" deals with the "temperature gradients in the gas" issue. After up to ten minutes the oxygen inventory was calculated and then, the slag mass was recovered, measured and compared to the iron specimen mass from which it was derived.

Since solid Fe_2O_3 oxides can form naturally, in this case, the term "excess oxygen" could only be argued if a slag oxygen-to-iron molar ratio in the molten reaction product which exceeds the oxygen-to-iron molar ratio for FeO, Fe_3O_4 and Fe_2O_3, which is to say greater than 1.5. Excess oxygen (beyond the most stable oxide) was asserted for the data of columns 8 and 9 of Figure 5 (Table 2 in the first-version and final-version paper [***6***]) if values above 1.5 were observed. In column 8, calculations for all 21 tests are above 1.5 (ranging from 1.6 to 2.2) and this column is based on the measured reduction in the oxygen inventory *after* the test compared to the initial specimen mass. In column 9, four of the 21 test results narrowly exceed 1.5 (ranging from 1.52 to 1.57) and these were calculated from

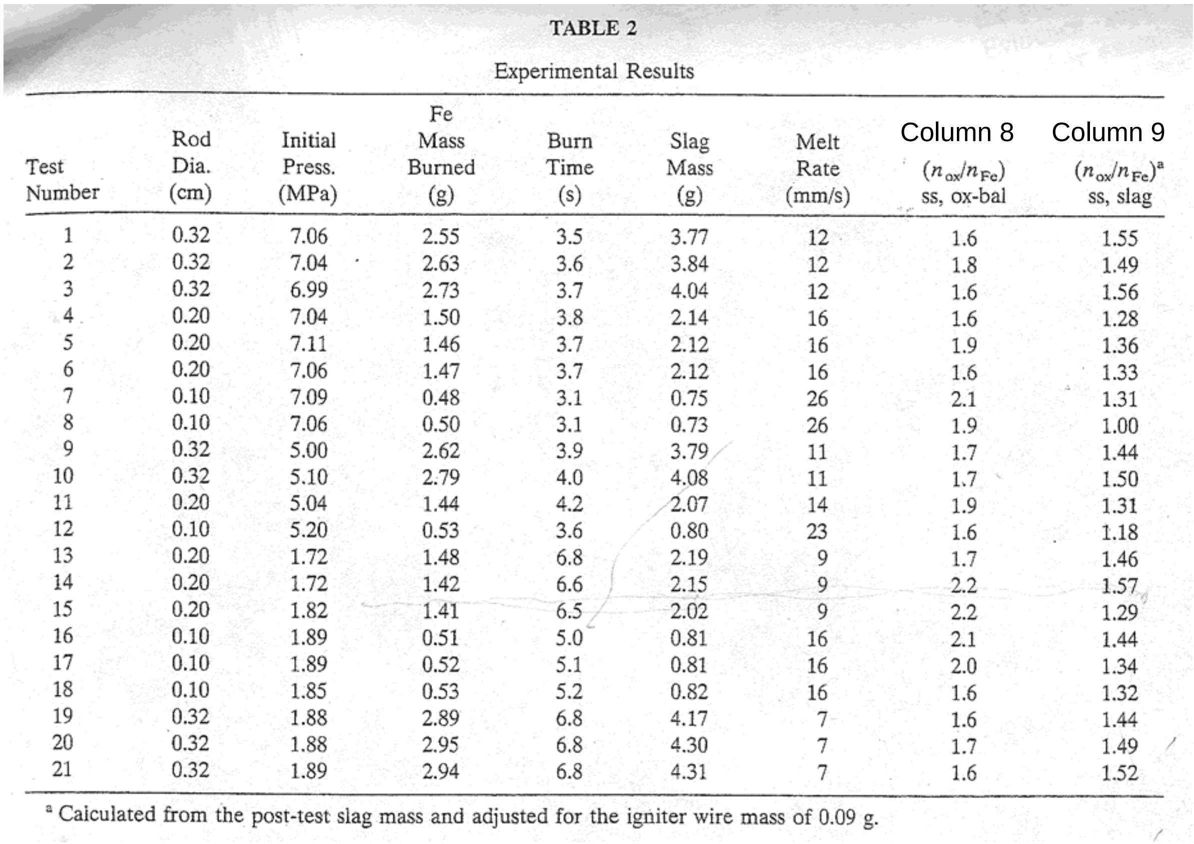


TABLE 2

Experimental Results

Test Number	Rod Dia. (cm)	Initial Press. (MPa)	Fe Mass Burned (g)	Burn Time (s)	Slag Mass (g)	Melt Rate (mm/s)	Column 8 (n_{ox}/n_{Fe}) ss, ox-bal	Column 9 (n_{ox}/n_{Fe})[a] ss, slag
1	0.32	7.06	2.55	3.5	3.77	12	1.6	1.55
2	0.32	7.04	2.63	3.6	3.84	12	1.8	1.49
3	0.32	6.99	2.73	3.7	4.04	12	1.6	1.56
4	0.20	7.04	1.50	3.8	2.14	16	1.6	1.28
5	0.20	7.11	1.46	3.7	2.12	16	1.9	1.36
6	0.20	7.06	1.47	3.7	2.12	16	1.6	1.33
7	0.10	7.09	0.48	3.1	0.75	26	2.1	1.31
8	0.10	7.06	0.50	3.1	0.73	26	1.9	1.00
9	0.32	5.00	2.62	3.9	3.79	11	1.7	1.44
10	0.32	5.10	2.79	4.0	4.08	11	1.7	1.50
11	0.20	5.04	1.44	4.2	2.07	14	1.9	1.31
12	0.10	5.20	0.53	3.6	0.80	23	1.6	1.18
13	0.20	1.72	1.48	6.8	2.19	9	1.7	1.46
14	0.20	1.72	1.42	6.6	2.15	9	2.2	1.57
15	0.20	1.82	1.41	6.5	2.02	9	2.2	1.29
16	0.10	1.89	0.51	5.0	0.81	16	2.1	1.44
17	0.10	1.89	0.52	5.1	0.81	16	2.0	1.34
18	0.10	1.85	0.53	5.2	0.82	16	1.6	1.32
19	0.32	1.88	2.89	6.8	4.17	7	1.6	1.44
20	0.32	1.88	2.95	6.8	4.30	7	1.7	1.49
21	0.32	1.89	2.94	6.8	4.31	7	1.6	1.52

[a] Calculated from the post-test slag mass and adjusted for the igniter wire mass of 0.09 g.

FIG. 5 —*Table 2 from Reference* [***6***].

the mass of the slag recovered compared to the mass of the iron specimen consumed.

Keep in mind that at the temperatures obtained during combustion, that the oxides Fe_3O_4 (magnetite) and Fe_2O_3 (hematite) would ***not*** tend to be present. FeO is cited as the only stable oxide *at those temperatures* and so even if there were only oxygen equivalent to FeO or even Fe_3O_4, that data above a ratio of oxygen to iron of 1.5 would represent a substantial amount and even ratios above 1.0 and 1.33 might be "excess" oxygen *relative to a pure FeO slag*. If a droplet contained significant amounts of un-combusted iron, something ruled out for at least some specimens in earlier NASA+ work [***1***] the associated slag might contain more oxygen than needed in the slag fraction to make any of the oxides. On a mole basis, magnetite levels of oxygen would produce Wustite plus 33% additional monatomic oxygen while Hematite levels would provide 50% additional monatomic oxygen. Indeed, in some early papers excess oxygen was treated as more oxygen than needed to produce wustite in the molten droplet. However in this case, during cooling the slag might conceivably burn in ambient oxygen to produce Fe_3O_4 or even Fe_2O_3 hence only values above 1.5 would be determinative.

Case closed; The main conclusion based on columns 8 and 9 of Figure 5 is that like the earlier work that examined molten slag and claimed to find huge amounts *during* combustion, there is at least some "excess oxygen" even in cooled iron slag and hence:

> "...This paper presents additional experimental results confirming the proposed mechanism for the burning iron rod of these experiments. The proposed mechanism is that the molten iron oxide exothermically dissolves oxygen, or equivalently, is further oxidized to ferrite species of molar oxygen-to-iron ratios higher than the known stable species of iron oxide"

The conclusion was bolstered in the first-version paper by a consideration of the heat balance in iron combustion. What would make oxygen gas want to enter a blazing hot iron-oxide slag in potentially awesome amounts. Thermo-chemical theory teaches that heat and entropy (free energies) are the drivers. Heats of solution can be positive or negative depending upon the metal. Perhaps the incorporation of oxygen releases a heat of solution or even more impressively a heat of reaction. And this follow-up paper introduces the argument that iron oxide (FeO) might just react with oxygen dimers (O_2s) to create transient ionic compounds like $FeO + O_2 \rightarrow FeO_3$, or $FeO + 2O_2 \rightarrow FeO_5$, or $FeO + 3O_2 \rightarrow FeO_7$, etc.). Indeed, some references are cited and claimed to make the same suggestion and seem to bolster their case.

For example, the authors go on (*in the initial version paper*) to make a powerful (but perhaps dubious) argument;

> "The existence of these species in the molten product must be accompanied by other, ongoing reactions other than the primary combustion reaction which may result in the net release of more energy than if the combustion reaction stopped with the production of FeO. If FeO was the only combustion product the droplet temperature should never exceed the adiabatic flame limit of approximately 3100K for $Fe(s) + O_2(g) = FeO(l)$ as calculated by the Gordon-McBride computer code [27 is the reference number in their paper)]. Ongoing experimental work has measured higher combustion temperatures at the surface of the molten drop which can be explained by further oxidation of the primary combustion products or, equivalently the exothermic dissolution of oxygen into the molten iron oxide [28 is the reference number in their paper, [***7***] is the reference number in this paper]".

However, this powerfully stated argument would be curiously deleted from the final paper. The potential reason "why?" may be important.

This "correction" paper would not publish for some three years and would undergo several interesting changes. In 1998 *Combustion and Flame* would publish: "**The Solubility of Oxygen in Liquid Iron Oxide During the Combustion of Iron Rods in High Pressure Oxygen"** [***6***] except that Joel Stoltzfus would no longer be a cited co-author and it would contain some interesting changes possibly influenced by my censored paper, but I can not prove they were. To wit: this is the first paper that sought to measure excess oxygen in molten iron slag using both the oxygen inventory measurement and a gravimetric slag measurement ten minutes after the combustion had apparently ceased (at a point the authors describe as "after thermal equilibrium is achieved" and in the final revision claim it was chosen to "definitively achieve thermal equilibrium "[its page 33] but for which they did not cite verification). At that point their combustion model involved the ignition of an iron rod at its bottom followed by a series of presumably (what an earlier paper from their associates asserted was) thoroughly mixed slag forming as it burned that contained a lot of excess oxygen falling to the bottom and immediately beginning the desorption of all that excess oxygen as it cooled while the slag chemistry converted some of it into higher oxides (Fe_3O_4 and Fe_2O_3) during cooling. In this case however, optimistically allowing that process to proceed for ten minutes in hopes the slag and system would come to a condition (thermal equilibrium if not chemical equilibrium) in which not all the absorbed oxygen had desorbed:

> "...the results give a statistically significant indication of excess oxygen in the combustion product at the test completion. Though no quantitative values of this ratio [the ratio of oxygen to iron] can be determined *during* combustion, due to temperature gradients in the gas, the trend indicated by the experimental traces (e.g. Fig 2 [in the 1998 paper] clearly indicate that even higher oxygen-to-iron molar ratios *could be present* during the actual burning." Emphasis added.

One can, as I did and do, read this *speculation* in part as a repudiation by implication of a principal portion of both the Steinberg PhD Dissertation [***2***] and the derivative *Combustion and Flame* Paper [***4***]. Suddenly "no quantitative values of this ratio [the ratio of oxygen to iron] can be determined ***during*** [emphasis added] combustion, due to temperature gradients in the gas" and yet this is a principal result from the previous papers and dissertation that have been cited so often as proof in subsequent publications. Nonetheless, "the trend indicated by the experimental traces (e.g. Fig 2 [in the paper, Fig 5 in this paper] clearly indicate that even higher oxygen-to-iron molar ratios ***could*** be present during the actual burning." They can not be measured but they *could* still be there. I take this as a left-handed repudiation of the dissertation methodology coupled with an intense desire/denial (wishful thinking) that the result from the repudiated methodology might be, nonetheless, correct, and most certainly it is *not* an effort to relax Dr. Steinberg's "assertion regarding the exact quantity of oxygen that is contained within the slag system formed on the burning iron rods" but argues that even though the previous testing may have been, was, invalid, that the conclusion may still be largely correct. And this conclusion is cited frequently in papers by the NASA+ entourage ever since as being settled science.

Curiously, it did this by reference to Fig 5, Columns 8 and 9, which indicates a drop in oxygen density after combustion (from 0.091 g/cm^3 to 0.089 g/cm^3), but that very drop size in column 8 was apparently calculated with the same methods just admitted to be invalid for earlier tests "due to temperature gradients in the gas" (the finite element issue) but also potentially due to the radiation and oxygen isolation error mechanisms reviewed earlier (though hopefully mitigated by the time element). And this final paper was revised *after* they had seen my paper and ignored my suggestion for validating their data. Nonetheless this work claims that even after thermal "equilibrium" obtains, there is still some of that excess oxygen present in the slag that like a reverse-harbinger predicts the past. My interpretation is that the early dip in calculated inventory says nothing because the three previously reviewed potential error mechanisms that suggest the early calculated dip is meaningless. Furthermore I argue that the latter equilibrium calculations must be [and will later be] subjected to their own error analyses.

Also, in the final edition of the paper, following exposure to and censoring of my paper, in an expanded section discussing “**Apparent density of the molten drop**” may have also been inspired by my paper but I can not prove that for it was not cited (is one allowed to cite censored papers). This new argument applied the Harkens-Brown equation for droplet density based upon the data that had been generated for droplet density and surface tension to claim that stoichiometries up to at least 2.1 had been obtained. It also added a conclusion that oxygen in the droplets (not to be confused with other gases like CO and CO_2) could not be present in the form of bubbles. Actual photos of censored bubbles in my paper apparently do not count.

In a final declaration of confidence, the paper (as for its initial text) claims agreement with several other works, and collaborative work between UQ and NASA WSTF and that:

> “ The future availability of a fully instrumented, real-time gravimetric system for metal combustion studies [30,31 are the reference in the paper] will provide direct measurement of the excess oxygen in burning iron-iron oxide and allow the work to be extended to other burning metal systems.”

The references [30,31] referred to are references [***8,9***] in this paper and refer to an ability to weigh burning rods as they form droplets as Sircar et. al. [***8***] reported four years earlier in ASTM STP 1111, and Steinberg and Scown [***9***] had predicted forthcoming in 1995 in ASTM STP 1267, and that I had clumsily attempted to crudely do in [***3***]. This worker is unaware of any such later predicted works in the following 30 years.

Are the “Correction” Data Valid?

My perfunctory testing [***3***] (the censored paper) argued that (at least at atmospheric pressure) many of the assumed and deduced behaviors of specimen combustion are flawed. And that balloon-like inflation can explain “oversized” slag droplets. In this case, there is a dichotomy of models to choose from. The case for that perspective is argued elsewhere in a discussion of peer review dysfunction [***5***]. I argue these “correction” data like the data they seek to correct are again *not* valid.

The crux of the final 1998 *Combustion and Flame* paper [***6***] lies in its Table 2 data (Figure 5 herein), specifically columns 8 and 9. Column 8 presents the oxygen-inventory-based calculations related to iron specimen initial mass and by golly, all oxygen:iron ratios are above the 1.5 boundary for Fe_2O_3. Column 9 presents the same goal but based upon the mass of the resulting slag, and, by golly, again a few are slightly above the critical 1.5 threshold.

Dr. Steinberg et al., inferred that at least some droplets form to convert completely into FeO iron oxide slag that dissolves, or more likely reacts, a significant maybe even massive amount of oxygen into it. But maybe it doesn’t. It detaches itself when its weight becomes too great for surface tension of the oxide to retain it and it falls to the bottom of the

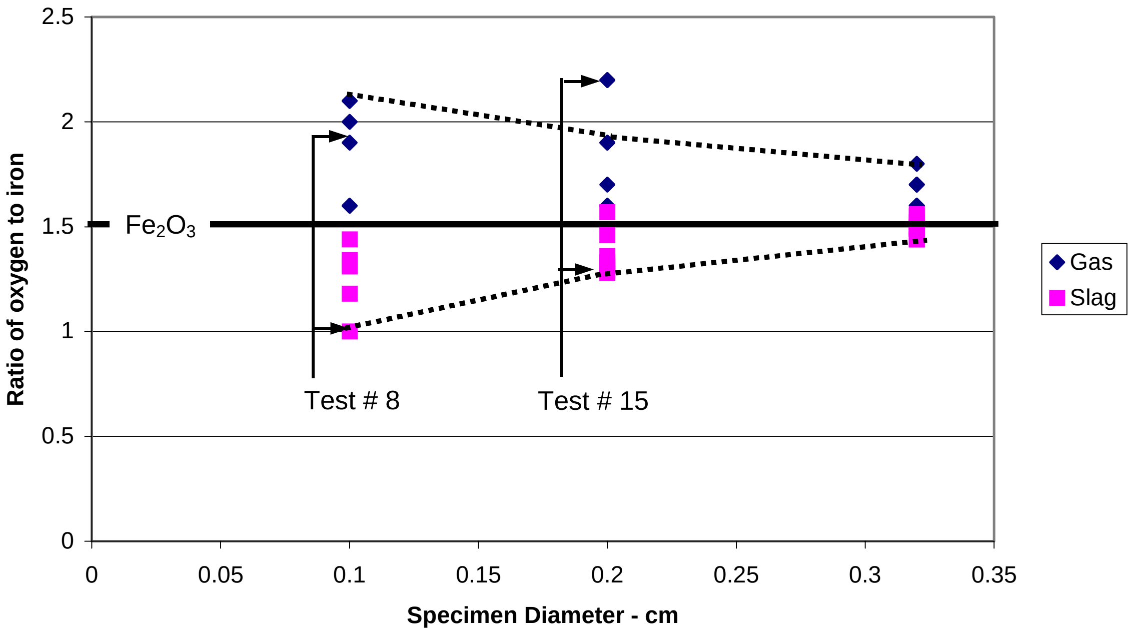


FIG. 6 —*Data from Columns 2, 8 and 9 of Table 2 in Reference* [***6***], *Figure 5 herein.*

vessel. There it begins to cool and its iron oxide burns in the excess dissolved oxygen, cooling, and forming higher oxides while some left-over surplus oxygen is released (bubbles) to the ambient. Hence, since complete combustion to an oxygen:iron ratio of 1.5 is possible, only oxygen in the cool slags *greater* than that needed to form Fe_2O_3 is significant to proving the prior presence of an excess.

Hence when Figure 5 (Table 2 in the subject paper) presents columns 8 and 9 with values greater than 1.5, these data are taken to imply excess oxygen must have been captured in the hardened slag and that the earlier invalid work is, nonetheless, exonerated. The earlier work was not able to make the measurements but that does not mean its results were wrong! Absence of proof does not imply proof of absence.

Figure 6 plots the data of Figure 5 columns 8 and 9. The data are plotted in terms of specimen diameter on the bases that larger specimens means more oxygen consumption that is easier to measure.

This perspective is revealing. When one burns iron in abundant oxygen one expects it will try to get to its most stable oxide (thermo-chemical equilibrium software confirms this). Hence the oxygen-to-iron ratio would tend to seek out a value of 1.5. Indeed, the excess oxygen proponent’s “proof’ of excess oxygen lies in data points greater than 1.5, even though there are scenarios in which the molten slag might have more oxygen than it can accommodate as oxides (might have ratios above 1.0 even when only FeO can form).

Figure 6 exhibits an important trend and allows some coarse surmise. Plotting the Reference [***6***] oxygen-to-iron ratios of Fig. 5 columns 8 and 9 versus specimen diameter, ex-

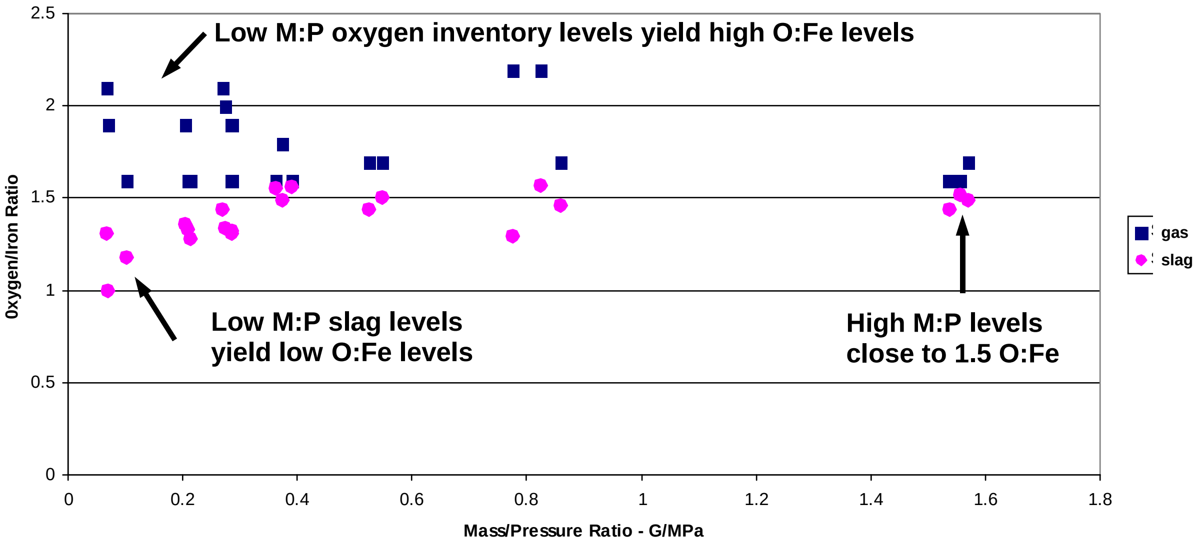


FIG. 7 —*Data from Columns 8 and 9 of Table 2 in Reference* [***6***], *Figure 5 herein.*

hibits a greater scatter at smaller specimen diameters and a convergence of data to an intermediate value as specimen diameter increases. Smaller diameter specimens yield smaller slags and smaller pressure and temperature changes during a test that are both more difficult to measure accurately. So greater scatter should be expected. Note that all these data are said to have been measured ten minutes *after* specimen combustion. During that time higher oxides of iron can form which can tend to reduce the test vessel oxygen inventory. Smaller specimens implies quicker quench so smaller slags might freeze to lower oxides and yield still smaller decreases in oxygen inventory both of which will also exhibit greater error margins. Note that none of the gas markers fall below 1.5 while only a few slag markers for the larger diameter specimens fall above lending a (probably false) impression (that will be explained) that these data are converging to a value greater than 1.5.

Figure 7 is plotted in terms of specimen mass to test pressure ratio on the bases that again larger specimens means more oxygen consumption that is easier to measure, and lower pressure means greater fractional oxygen consumption that is also easier to measure and this elaborates on the surmise of Figure 6. All of the column 8 data from Figure 5 (blue/square markers) lie above the limiting 1.5 oxygen-to-iron ratio of both figures. The highest O:Fe values tend to be at the smallest diameters and lowest m/p ratios, the most difficult to measure with the most scatter. Most of the column 9 data lie below the 1.5 ratio on both figures.

Yet these data are complimentary. They are alternative pairs of the same measurement, and in almost every case they mirror-contradict each other. The highest, easiest to measure, m/p ratio data are close to the 1.5 O:Fe value. All of the column 9 data from Figure 5 lie close to or below the 1.57 O:Fe level. The smallest values of O:Fe lie at the lowest values of diameter or m/p. At the largest values of diameter or m/p the markers are again close to the 1.5 O:Fe ratio. Even more significant, is that as the m/p ratio increases the upper gas inventory points come down and the lower slag points go up, and they tend to converge

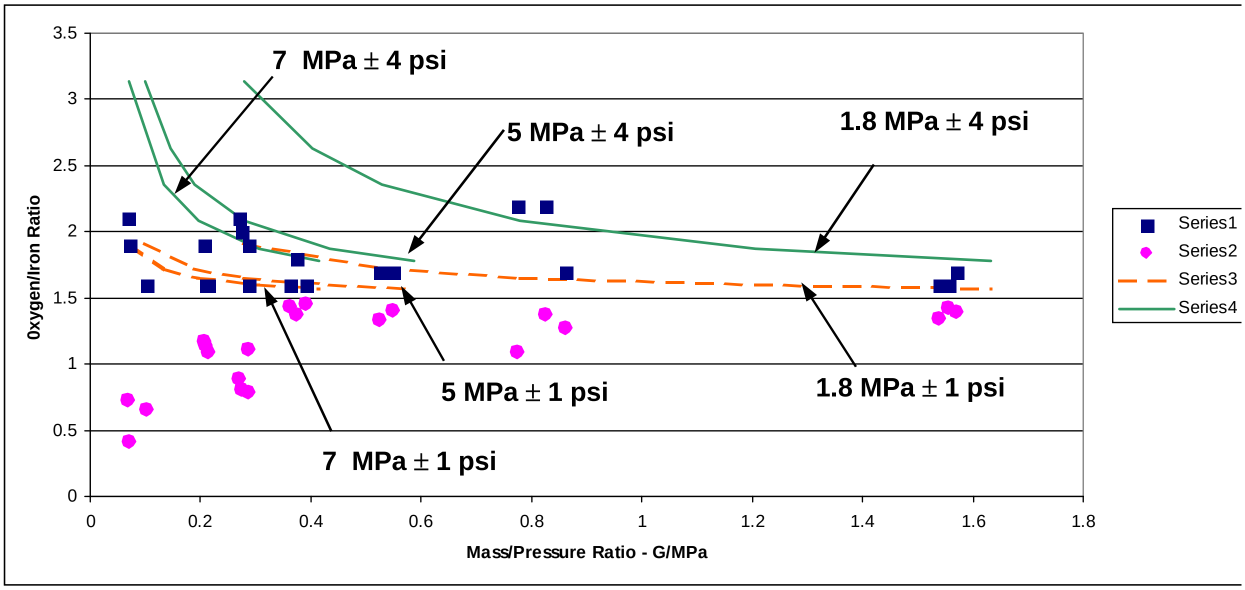


FIG. 8 —*Data from Fig. 5 with error margins and corrections applied.*

towards the ~1.5 O:F boundary. Two example data points on Fig. 6 illustrate this and are compelling. The complimentary data points for test numbers 8 and 15 are indicated. For test #8 the slag indicates a 1.0 ratio while the gas analysis *for the very same test* indicates a 1.9 ratio. For test 15 the slag indicates a 1.29 ratio while the gas analysis *for the very same test* indicates a 2.2 ratio. The most precise data say there is very little, if any, excess oxygen proof. Yet the authors accept and often cite the gas analysis results as “proof”, when the mass measurements say the exact opposite. I submit Dunning-Krager Effect or denial may be a factor. This is “cherry-picking” data that can be “cherry-picked” in reverse order.

The measurement scheme for data in Figure 5, column 8 have been repudiated herein for real-time oxygen inventory measurement due to at least three error mechanisms that were not accounted for. However, this second-effort data taken up to ten minutes after the specimen combustion ceased would have greatly attenuated convective swirl due to less chimney effect over the specimen. They would have a much more uniform temperature throughout and reduced finite-element issues. They would have much less, maybe no, radiation to artificially heat the thermocouples, and finally if truly equilibrated throughout (though I seriously doubt it was at ten minutes) isolated oxygen should be no different in temperature than that at the central core of the vessel. Again the recommended easy verification of the thermal equilibrium was not pursued. Hence instrument errors can also not be ruled out. Nor can promoted reaction of copper surfaces exposed to heat that may consume oxygen in forming surface or dissolved copper oxides.

The instrument error point can also be seen more directly in Figure 8, which plots the measured oxygen to iron ratios of Figure 7 with estimated instrument error bands. In this case the effect of instrumental error is not small in comparison to what is being measured but in some cases is actually greater. Though, this is not as flawed as measuring the distance to the moon with a yardstick, the instrumental error can not be excluded as the entire source of the measured changes.

Recall once again that per ideal gas law the oxygen inventory is estimated as

$$N = PV/RT$$

Pressure is more easily measured and more likely uniform than temperature, so errors in temperature will still be a focus. Column 8 indicates oxygen-to-iron ratios of up to 2.2 were measured. However curiously, one might expect column 8 data to be its most accurate when the specimen mass is large (to lock up a maximum amount of oxygen) and the oxygen pressure is low (to produce a maximum pressure drop). If equilibrium temperature truly obtains throughout, then any errors in inventory calculations will largely result from the accuracy of the pressure and temperature instrumentation, which Dr. Steinberg asserts on page 109 of his thesis is:

> "The theoretical accuracy of the system was calculated: pressure ±4 psia, temperature reference ±2°C, gas phase temperature ±4°C, rod temperature ±6.4°C"…. ….“The system was checked at two temperatures and for a range of pressures function much better than the calculated errors: Pressure ±1 psia, temperature (freezing and boiling point) ±1.5°C.”

In comparison when Figure 5, Column 9 is examined, its data seek to measure the same ratio of oxygen-to-iron as does Column 8 except it presents measurements of the oxygen in the slag recovered after the test based on weight divided by the molar amount of iron (based on specimen initial weight less igniter wire mass). With standard laboratory balances these masses are easily measured to within ±0.0001g. There may be issues with ability to collect all of the slag or constituents that may have evaporated, but these data should largely be spot on.

Figure 8 applies a correction to the data described below and exhibits error envelopes for the data of Column 8 at the three pressures tested and for the specified pressure accuracy cited (green solid bars) and for Dr. Steinberg’s own estimates of the accuracy (orange dashed bars) relative to a measurement of “excess oxygen” (oxygen greater than the stoichiometry for hematite (1.5) for the range of mass-to-oxygen inventory for the specimens of Figure 5. Taking temperatures to be exact and in equilibrium, and using Dr. Steinberg’s assertion of actual observed accuracy, one would expect the best accuracy and precision in pressure of either ±4 psia or ±1 psia, to yield the bars shown for an exact 1.5 stoichiometry. Similarly using an analytical balance, one would anticipate error bars for the column 9 data almost indistinguishable from the data themselves (0.01% to 0.0025% different). Hence results inside these column 8 bars might well be due to instrument accuracy errors (like measuring the distance to the moon with a yardstick). Clearly it indicates anticipated instrumental errors in columns 8 data should be from equal to several times greater than the values measured, while the column 9 data should be far more accurate and the data at higher specimen-mass/pressure should be the most likely to yield accurate results.

For the data of Figure 8 exhibits, columns 8 and 9, these point-by-point in individual tests are measuring the same data because:

oxygen removed from inventory =

=oxygen absorbed during specimen combustion =

=oxygen added to specimen slag mass

As a result, if an extreme specimen test (high or low result >>1.5 or <<1.5) is measured in one of the two test approaches, you would expect a corresponding high or low result in the other test approach. After all if a large amount of oxygen has disappeared, it has to have gone somewhere! As a result Figure 8 is a revelation. Notice that virtually all of the stoichiometries calculated with the oxygen inventory method (square markers) lie above 1.5 while *all* the more accurate data stoichiometries measured on the basis of specimen mass and slag mass (triangle markers) were below a stoichiometry of about 1.5 on Figure 8. Indeed, the further the gas inventory value is *above* the boundary, then the more likely the further the contradictory mass result is *below* the threshold. This is undergraduate level error analysis.

Also noteworthy, is a final correction added to the data of Figure 5, Column 9 data. The footnote on the Reference [***6***] figure indicates that post-test slag mass measurements were reduced by 0.09 grams, to compensate for that amount of Pyro-fuze igniter wire. The data confirm the correction. However, Pyro-Fuze is an aluminum wire coated with a coaxial layer of palladium. Both metal's mass would not count towards a calculation of the oxygen-to-iron stoichiometry, but the mass of both would be an error source that would tend to elevate the circular data points.

Internet data indicates how aluminum and palladium react to form an intermetallic metal even in inert gases and a YouTube video [***10***] shows how a crude home-made bimetallic device reacts in air. The precise "stoichiometery" for the resulting inter-metallic is apparently flexible. And available data do not indicate how much of the Pyro-Fuze wire is aluminum, however, palladium is very expensive and, like thermite, it appears Pyro-Fuze reaction does not require a precise fraction to be effective, so its volume might be minimized. Furthermore, palladium has two known oxides (PdO and PdO_2) and aluminum typically combusts to produce Al_2O_3. Hence there is a high probability that the resulting fiery-hot alloy following ignition may well combust in oxygen, perhaps even bolstered by palladium catalytic effects. If a worst-case assumption is made that all 0.09g of Pyro-fuze were aluminum then an amount of Al_2O_3 would form that would contain additional oxygen mass of:

[0.09 g-Al / 26.98 g-mol Al/ g-Al] x

[1.5 g-molsO/g-mol-Al] x

16 g-O/g-mol)] = 0.080 grams-O

And this oxygen should also be removed from the gaseous state, hence not be a part of either calculated oxygen-to-iron stoichiometry. While this is a small upper limit amount, when this is subtracted from the mass of the slag in calculating column 9 of Figure 6, and plotted, as in Figure 8, it only lowers the value of $n_{ox}/n_{fe\text{-}slag}$g for most data by a small amount but prevents it from exceeding 1.5. However that small amount provides the appearance of an asymptotic relationship that homes in on a rather precise final oxide of Fe_2O_3 from below, as one might otherwise expect of a large mass burning in abundant oxygen. That little amount is also enough to virtually eliminate any credible proof that even a trace quantity of excess oxygen is present as Dr. Steinberg has defined it in what are likely his own most accurate data (again 10 minutes *after* combustion ends). Furthermore in the case of the smallest, most easily quenched, specimens, the final correction lowers the mass stoichiometric average to in the lowest case about 0.4, which would imply that if the quench trapped only FeO oxide, that the final slag might be more than more half unoxidized iron. Apparently these slags were not chemically analyzed. For indeed, if they were *not* largely Fe_2O_3, it might have helped argue the presence of excess oxygen, but instead do not again prove the hypothesis. Its absence along with a pattern of overlooked and ignored and even obvious steps that could have been taken lead this worker to question the thoroughness and even fear a tendency to cherry-picked data to obtain desired results among these workers. Would they be willing to paint dots on mice in other occupations?

One can argue, as this worker has years earlier only to have been censored, that if a significant portion of the molten droplet is pure iron, then the remainder oxide might *in some cases* have an oxygen-to-iron ratio indication of excess oxygen telative to FeO at the time of release. But this is not what Dr. Steinberg et al's paper and it predecessors seem to swear to.

The first version of this "**Solubility**.." paper made a powerful argument, cited earlier, to the effect that if massive oxygen enters the melt it most likely has to react to form exotic ferrite ions as the "**Solubility**…" paper [***6***] speculates with a release of additional heat and producing increased droplet temperature. They note in text they later removed:

> "The existence of these [ferrite ion] species in the molten product must be accompanied by other, ongoing reactions other than the primary combustion reaction which, may result in the net release of more energy than if the combustion reaction stopped with the production of FeO. If FeO was the only combustion product the droplet temperature should never exceed the adiabatic flame limit of approximately 3100K for $Fe(s) + ½O_2(g) = FeO\ (l)$ as calculated by the Gordon-McBride Computer Code [their reference no. 27]. Ongoing experimental work has measured higher combustion temperatures at the surface of the molten drop which can be explained by further oxidation of the primary combustion products or, equivalently, the exothermic dissolution of oxygen into molten iron oxide [their reference 28, reference ***7*** in this paper]."

This is indeed a very powerful argument that was deleted from the final paper and likely for good reason. In numerous papers from the NASA+, Gordon McBride code is invoked (apparently not always skillfully). In an early controversial paper Dr. Steinberg et al. [***11***], probably for the first time in the ASTM collegium introduced the Gordon-McBride Code for various calculations and in that paper concluded almost as a final glowing thought:

> "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." (page 207 in the paper [***11***]).

The limitations referred to are cited in numerous NASA+ papers as; “restrictions ... on handling condensed phases”, “not handling condensed phases well’, “not handling mixed phases well”. And unfortunately in many instances the code may have been used incorrectly (including by Dr. Steinberg’s group) seeking data for some situation, “A”, while inputting data for a different situation “B” [***11***]. Other perhaps flawed papers [***12***] and this worker’s Web Site try to go into these issues in more detail with less than ideal talent.

In the present case, in the initial “**Solubility**…” paper text, the cited 3100K adiabatic combustion temperature calculated with G-M code and listed in numerous references is for combustion in atmospheric pressure. In comparison, the Kurtz et al paper [***7***] cites measured temperatures of up to ~3800 K but that is for combustion in 1.8 MPa (~250 psia). When G-M code is properly used to predict pressurized results, its actual result (notwithstanding any issues with condensed or mixed or whatever phases) is rather consistent. I surmise this error was detected before the final text was prepared and that may be why it was deleted (self-censored).

However, though it was removed, it has a legacy. If excess oxygen implies higher temperatures (a logical material implication) then the contra-positive obtains: Not higher temperature implies NOT excess oxygen. And this observation therefore flips to become a powerful argument *against* the existence of, at least large-scale, excess oxygen. Hence we see the “pro” argument is dropped and the strong “anti” argument is censored (un-cherry-picked). Not cool!

Dr. Steinberg et al make what may be a last attempt in their paper to argue the presence of excess oxygen in a section titled: “**Apparent density of the molten drop**” [***6***], their page 34). Herein they revisit and attach much importance to earlier and extensive discussion in the paper of the Harkens-Brown (H-B) theory for the mechanics of droplet detachment. H-B apparently developed an equation for relating the point at which the mass of a liquid droplet achieves a weight greater than the force surface tension provides to suspend the droplet as:

$$\rho = 6H\sigma d_{orifice} / g d^3_{drop}$$

where ρ = droplet density , H = the Harkens-Brown factor, σ = surface tension, $d_{orifice}$ = diameter of suspending orifice, g = gravitational acceleration, and d_{drop} = the diameter of the detached droplet. To wit:

> “...These drops have an estimated total mass of 0.520g, is comprised of 0.325g of iron and therefore, 0.195g of oxygen. These values give an oxygen-to-iron mole ratio (n_{ox}/n_{fe}) of 2.1.”

However, in this case, this presumably calculates the ratio just after detachment,

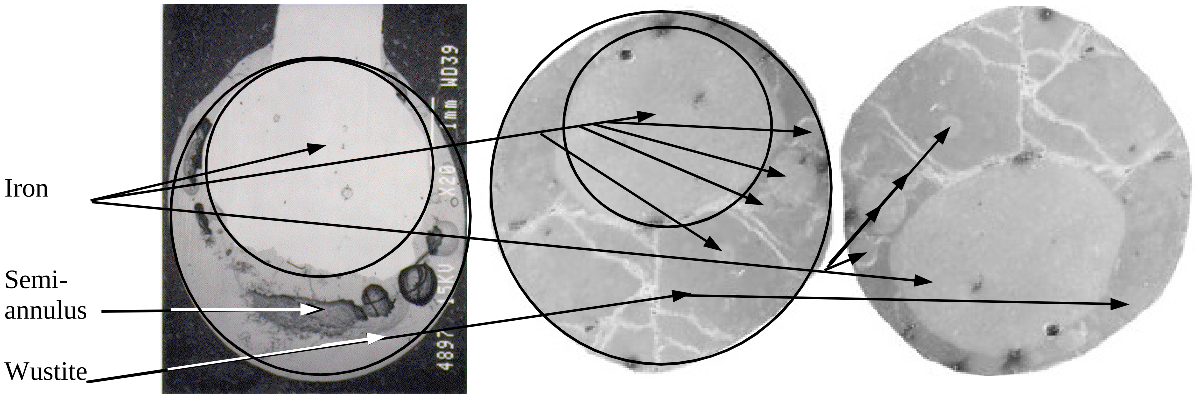


FIG. 9 —*Atmospheric-pressure test droplets from reference* [***3***].

when presumably the droplet may contain even massive oxygen, producing a droplet size up to 2.5 times that expected. If the detached droplet has the same ratio as the slag has ten minutes later, it implies no oxygen has eluted from the droplet, in contradiction to their other earlier analyses, nay *conclusions*. Furthermore, if the droplet is inflated with gas, or if a major portion of it is entrained iron (as my "**Brief...**"[***3***] paper has pretty much proven is the case for atmospheric pressure tests), then surface tension and parameter data may be much different than cited. Indeed, if as the photos of Figure 9 from [***3***] depict for atmospheric pressure tests, there are cases where the droplet severance is largely due to the pinching-off of liquid iron, yielding a two-phase iron/iron-oxide droplet then the H-B equation would need to be based upon a much more complicated interface surface tension between iron and iron oxide, and so the differential surface tension of iron and density of iron may be a much more complex scenario than H-B addressed. That analysis is above this worker's pay-grade.

Finally, there is the previously-cited (p.17) proclamation in both the early submission and final revised "**Solubility**.." papers [***6***], that a

> "..fully instrumented, real-time gravimetric system for metal combustion studies [30,31(in the paper)] will provide direct measurement of the excess oxygen…"

Indeed, even my own crude efforts have signaled agreement that great benefits would derive. Furthermore the work with the Sircar et al [***8***], system referred to was described earlier in a 1991 paper that warrants discussion. However curiously in that paper only two metals were tested. They chose aluminum (perhaps the second or third most used material in oxidant service), to exhibit anticipated vapor burning (fast) combustion behavior. And to contrast with that, they chose not iron (undoubtedly the most used material in oxygen service by far) to exhibit its well-known liquid burning, but instead selected titanium (perhaps the only common metal that this worker has never known to be used in *any* oxygen service) to exhibit liquid burning. At the time iron was a hot topic and its behavior was under much scrutiny at NASA WSTF. Forgive the cynicism but one can not but wonder if perhaps iron was deliberately avoided? Cherry-un-picked? Or worse, perhaps *was* tested with undesirable results and then censored?

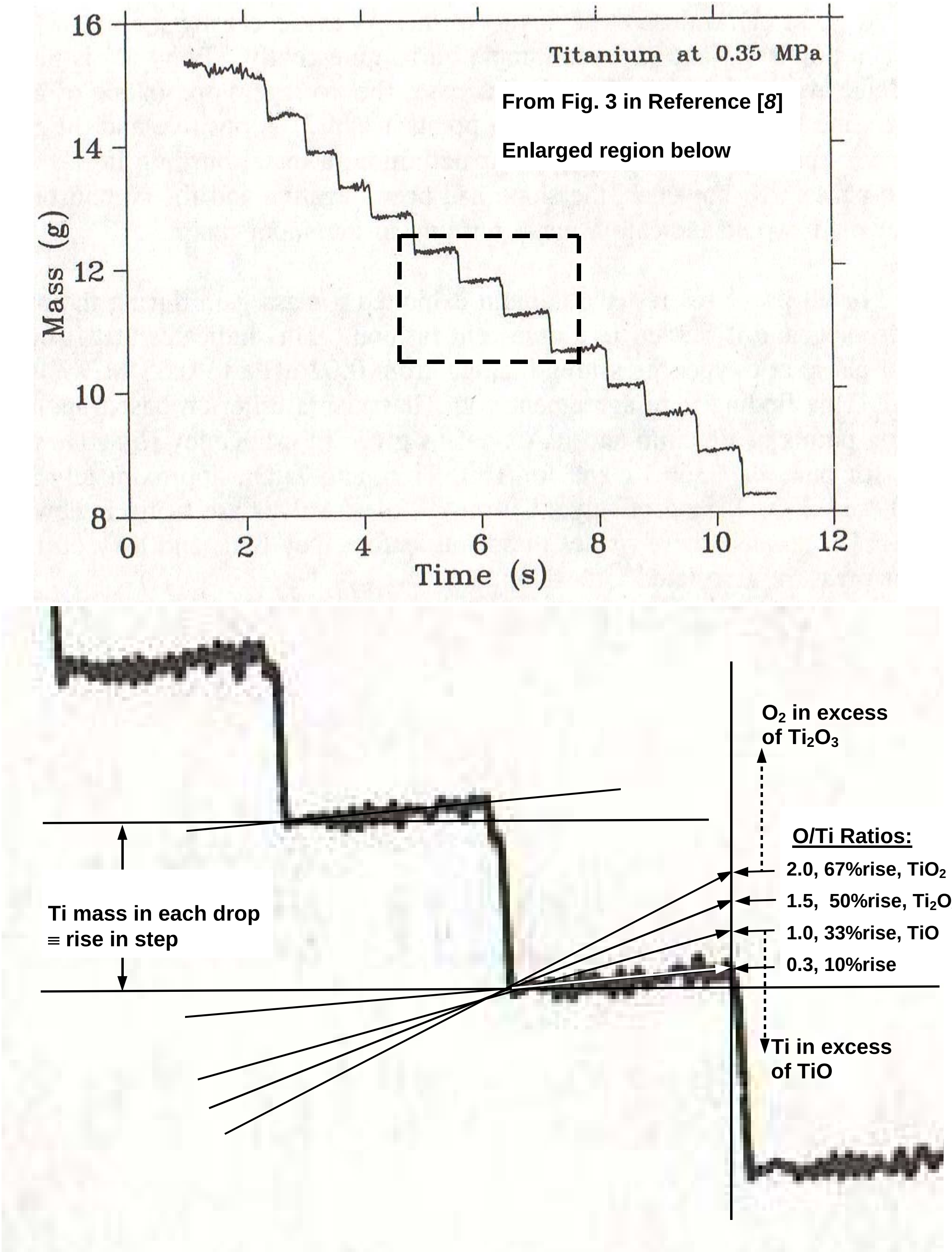


FIG. 10—Gravimetric testing staircase from 1989, Sircar et al. [*8*].

Fig 10 is a cropped exhibit of data for liquid-burning titanium from the Sircar et al paper, and it exhibits a step like “staircase” weight trace as droplets of molten titanium slag fell from a specimen. Below it is an enlarged section. Deviations from a perfect staircase would be expected to indicate the nature of the combustion as oxygen reacted or dissolved into each droplet ...if well mixed. Any oxygen reacted into the droplet should add weight (ignoring buoyancy effects and aerodynamic drag due to the chimney effect).

Titanium apparently has three common oxides that might obtain on combustion: TiO, Ti_2O_3, and TiO_2, relating to oxygen-to-titanium ratios O:Ti = 1.0, 1.5 and 2.0. Hence for a droplet metal mass of, M, which is the rise of each step in an ideal staicase:

[M/(atomic weight Ti)] x O:Ti x 16 weight units /mole monatomic oxygen

This indicates that as shown in Fig. 10, if the added weight results in an upward slant equivalent to 33.4, 50.1 or 66.9% of the height of the step then oxygen was taken on equivalent to complete droplet combustion to TiO, Ti_2O_3 or TiO_2 respectively. It does not say that any of the oxygen is reacted though all of it may indeed have reacted (and probably did). But at rise values below 33.4% rise, not all of the Ti could possibly be reacted, and similarly if greater than 66.9 then at least some of the oxygen is not reacted and would be called excess oxygen in the parlance of Dr. Steinberg et al.

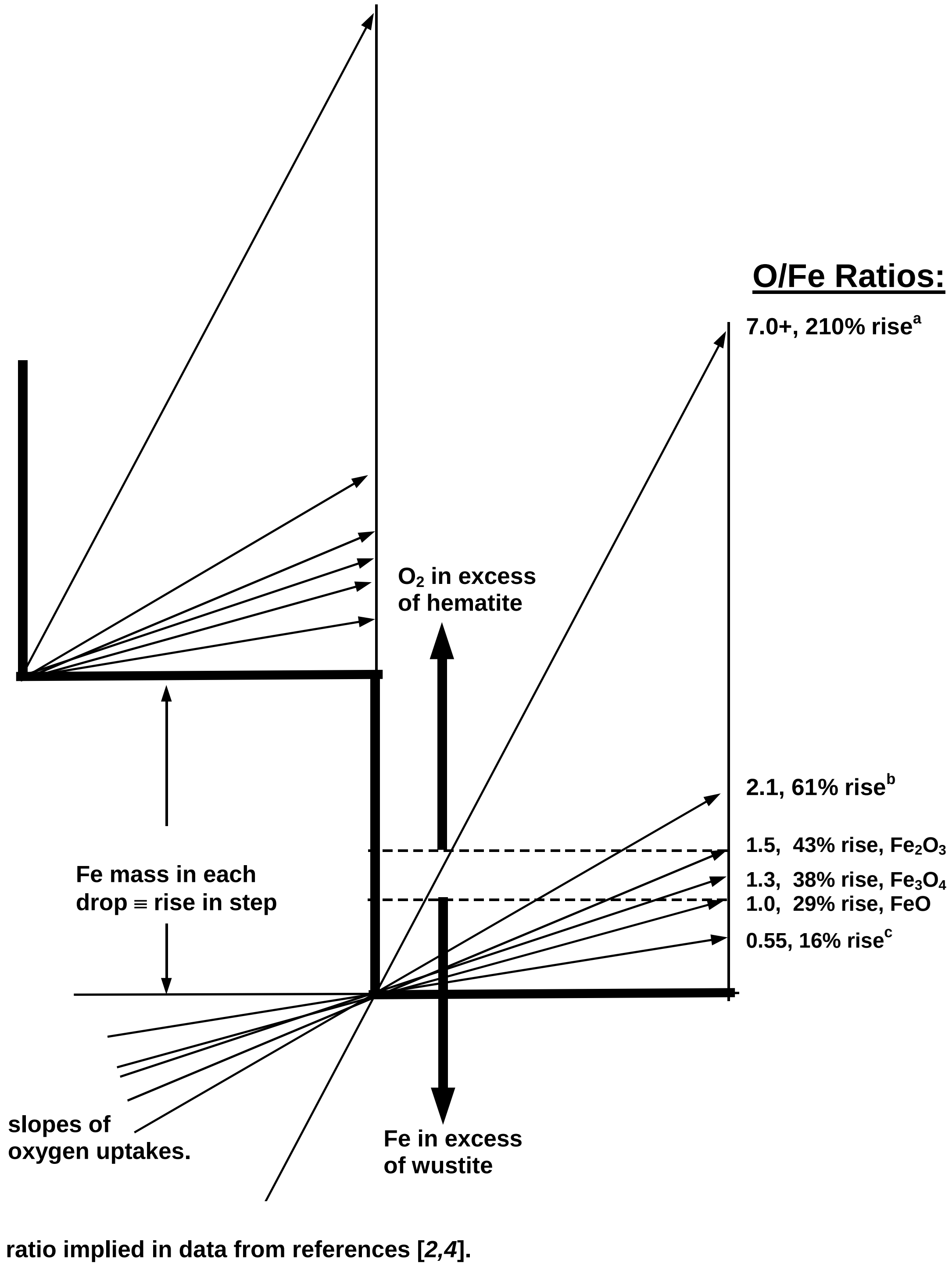
Hence if we had similar plots for iron/steel specimens and if their combustion behaved as well as (or better than) titanium has exhibited herein, then the staircase plots for single-phase oxide droplets of FeO, Fe_3O_4, and Fe_2O_3, relating to oxygen-to-iron ratios O: Fe = 1.0, 1.333 and 1.50 would suggest a calibration as shown in Fig 11. For a mass of iron, M, the mass of oxygen in a droplet is:

[M/(atomic weight Fe)] x O:Fe x 16 weight units /mole monatomic oxygen

Of course droplets may be two-phase like those of Figure 9. Indeed, Dr. Michael Lanyi has often stressed to this worker the incredible solvency of liquid FeO oxide. Hence the droplets may have both bulk and dissolved iron in them (which might preclude “excess oxygen”). Might some of that dissolved iron burn on or near the outer surface of the droplet rather than burning at an excess-oxygen-laden liquid-oxide liquid-iron interface of entrained phases? Might this explain the previously-reviewed roughly adiabatic-combustion-temperatures Kurtz et al [*7*] measured on droplets?

Shown also on Figure 11 are temperature rises that would be expected for excess oxygen levels of 2.2 (as was reported in **Solubility..** “[*6*] and for excess oxygen levels of 7 times the FeO case as the invalidated data in Dr. Steinberg’s DT [*2*] would indicate. Levels this worker must again, 27 years later, challenge in a way not subject this time to censorship from the NASA+. And this worker’s crude, highly unwelcome efforts to cobble measurements in “**Brief…”[*3*]** at atmospheric pressure, flies in the face of any substantial excess-oxygen claim.

These assorted contradictions and coincidences are a giant red flag regarding the validity of the conclusions drawn from the reported data. They do not prove or even hint that



[a] O/F ratio implied in data from references [*2,4*].

[b] O/F ratio (max.) implied in data from initial and final reference [*6*].

[c] O/F ratio implied in data from reference [*3*] at atmospheric pressure.

FIG. 11—*Ideal potential data staircase expected in iron combustion results if well-behaved.*

excess oxygen is present in the original or later contexts, whether any lesser such excess oxygen actually is or is not present in fact and in any degree.

Blow-Back and Busting the $CXSO_2$ Myth

For more than 30 years the copious excess oxygen hornswoggle has been pushed. Numerous papers from the NASA+ clique and several Dissertation Theses from University of Queensland (that have at least often had Dr. Steinberg as their advisor) have parroted declarations like:

> “Continuing work over recent years has postulated and confirmed the concept that under a wide range of environmental conditions burning metals contain “excess oxygen” within the formed molten mass present when metals burn” [***13***]
>
> “...there is a lack of data on the ferrite ions. There interest lies in the fact that during burning there is more oxygen in the molten oxide than required by stoichiometric haematite, that is an oxygen atom to iron atom (or mole) ratio of 1.5. Analysis has shown n_{ox}/n_{Fe} , ratios of 2.1-2.3” [***14***]

Within the knowledge of this worker, the most problematic cited bases are the three Dr. Steinberg et.al. works [***2, 4, 6***] that are all challenged herein.

Assume for the moment that even if there is no copious excess oxygen or there is no significant local excess oxygen in iron slag. Does it matter if anyone thinks iron sucks up excess oxygen when it burns? Who cares? If Dr. Steinberg and his NASA+ entourage dropped the ball, why cruelly burst their “bubble”? If they published ludicrous results in a highly respected journal and are in denial about it, does it matter if the NASA+ correctly or not is married to a flimsy, or even crackpot, theory of excess oxygen? It is not like a Committee of Oxygen System Design Engineers (OSDEs) would ever sit around musing whether they should opt to design a piping system with less excess-oxygen risk. And just like the case with cold O-rings, wasn’t it NASA’s manifest destiny to decide whether to launch Challenger any which way they wanted? So even if there is full merit to this analysis, why not just let NASA+ have its excess oxygen fantasy. If that is what it is?

It is this opinion that these experiences expose how the NASA+ contingent became the bullies (rather than the alphas) of the ASTM Committee G-4. And that their legacy continues to this day. In pre-quality-process days, the time-honored response to bullies on the playground was to push back and even bloody their noses, and after this long nightmare of turning the other cheek in the pursuit of quality, this is a too-much delayed metaphorical push-back. In it this worker must also stipulate that sometimes bullies are not capable of learning despite push-back with real or metaphorical broken noses. Some like broken noses. Some just can’t help themselves.

This paper suggests that the NASA+ did ***not*** discover excess oxygen or large scale ferrite ions in its widely cited papers [***1,2,4,6***]. But that does not mean that it, or at least some of it, is not there. To repeat: "absence of proof of existence is not proof of nonexistence."

If it *is* there as copious exotic ions then it does indeed interfere with meaningful use of thermo-chemical equilibrium concepts to address oxidant fire risks for the lamented reasons [***13***] cited including that today's thermo-chemical-equilibrium software does not include data (that apparently does not exist and maybe can not exist) for the key excess-oxygen constituents (those exotic ferrite ions) when they may be the majority species present during iron combustion. It would be like calculating the combustion of oxygen and hydrogen but not allowing the software to factor in any product as H_2O.

However, if it *is not* present in any major degree, then for two decades, the oxidant safety community at least in part has been ignoring some very powerful and useful tools and that equates to not doing its job well. It equates to harm that has been consequential. It equates to dereliction.

But beyond that, the modest paper I submitted [***3***] has in this opinion led to so much intrigue, so much effort to prevent its publication, so much shunning and hard-feelings and censorship and a poisoned collegial environment, resulting in this workers pariah ever since. Forcing this worker at the time to defend his competence to his own management. And for the last 27 years, ASTM Committee G4 bully leadership has had virtually nothing to do with me, while at the same time lamenting lack of energy to get things done and reducing the scope of its mission. No matter what the issue, they (acting on behalf of G4 at large) were simply not interested. It has been like being shunned by an Amish Mafia. I have had negative ballots found non-persuasive for lame and even mocking bases. At the same time, I am convinced that positions they have taken, data I fear they have cherry-picked or been too-gullible about, realistic projects they have ignored (in effect censored), have compromised G4's work product and the general safety in using oxygen. In this judgment safe oxygen use is less possible to achieve today than it should be due to the poisonous atmosphere that started back then not in a resistance to excess-oxygen hypothesis, but in seeking to prevent any challenge to it.

But this worker may be wrong, and that is why there should be no censorship of pro-excess oxygen advocacy, ...or dissent thereto. That was the ASTM Committee peer review ethic up until NASA+ seized control. This may be a mess better cleaned up by its creators but it has not been. Its creators could have sought to edify this miscreant and others like him rather than shun them and rely on their pronouncements from Olympus as infallible. This history makes it much easier for this worker to believe the claims about parent NASA arrogance during the Challenger launch episode and the its refusal to take Columbia heat sgield damage seriously.

But the exposure of what this putative miscreant considers to be the excess-oxygen blunder (if that is what it really is) was not the only emerging issue. As I detail elsewhere [***5***], I believe the NASA+ are a perfect storm. That the NASA "rocket scientist" image conveys a default prestige (whether deserved or not) that draws prone individuals into cognitive bias that then manifests itself through *Dunning-Kruger Effect* and other confirmation bias as I believe has been manifest and is being reviewed in much greater degree in [***5***].

If this opinion is fully correct, then things are more than dreary. If the Oxidant hazard is real, if shutdowns have occurred, if capital has been lost, if people (including astronauts) have been injured and even died, moon missions thwarted (and I submit all of that is not merely realistic but historic fact), then perhaps getting this right is not trivial after all. It might just be more important than the NASA+ egos.

Yet it occurs to this worker that for these past decades I have evolved from an obedient vendor of parent NASA into the customer. Before retirement I worked for a company that had a strong incentive to defer to NASA (Rule 1) for NASA was a good customer and therefore always right. Quality demanded it. Today I pay obscene taxes into a budget for NASA (currently under an attack that this worker now fully endorses) and am become the customer even if only micro-fractionally. The customer is right (Rule 1) and when the customer is wrong, even if about excess oxygen, refer to Rule 1. So free agent that I now am, I can argue as I do here that excess oxygen is a hornswoggle. They can now refer to Rule 1. So if copious excess oxygen is the hornswoggle argued herein, what hath it wrought?

(1) *It **Has** Distorted Our Understanding of the Metal Hazard*

In “definitive” Steinberg et al paper [***6***] that proclaims definitive proof of excess-oxygen existence (earlier shown here to be selective and faulty or more likely misinterpretation of their own data at best), droplets of iron slag are asserted to have at least excess oxygen content to beyond the oxygen-to-iron ratio of 1.5 and to at least 2.1-2.2 or maybe much higher at the earlier instant of release.

Such copious excess oxygen, if valid, is an amazing discovery that any PhD candidate would be delighted to find and could be tempted to facilitate (to fudge facts, to paint spots on mice). In the Steinberg et. al. scenario, the droplets throbbing and bulging with excess oxygen, full of “ferrite ions” fall to the vessel bottom and begin to cool (1) reacting some of it with FeO into higher oxides and (2) venting some oxygen during the quench-and-cool, and (3) retaining some in the solidified slag. In their “definitive” testing they claim some tests capture enough of the excess oxygen to record O:Fe ratios at values as high as 2.2. In this opinion these data were “cherry-picked” and misinterpreted. However, whereas those data that were asserted to prove excess oxygen presence did not do so on closer examination, the remaining data collected by the proponents of excess oxygen themselves, can be used to construct an analysis that *copious* excess oxygen does *not* exist, even though some dissolved non-ferrite-ion oxygen at a much lower level (that would not thwart meaningful thermo-chemical equilibrium math) may be, perhaps must be, present.

Figure 8, page 20 exhibited how the oxygen inventory data (column 8 of Fig. 5) were typically smaller than the scale of likely error margins while the much more precise (corrected) slag mass measurements (column 9 of Fig. 5, page 14) were ***not*** above the 1.5 oxygen-to-iron ratio required to prove even a small amount of excess oxygen was present. What else do they show?

Figure 6, page 18, also identifies two specific test results for tests 8 and 15. These more precise data for the slags indicate ratios well below 1.5 while the less precise gas inventory measurements indicate ratios well above 1.5, ...for the exact same tests. Yet ever since the authors have ignored the contradiction (wishful thinking?) and cited the gas measurements as definitive proof, ...settled science. This worker would cite the other results as lack of proof.

This is to suggest (the authors are free to propose alternatives) the smallest (0.1-cm) rods produced the smallest droplets and the smallest (as measured) slag mass that would be the most rapidly quenched when it contacts the catch receptacle. Hence, in that case these data represent the best estimate of the droplet average composition. Yet test 8 slag produced the very lowest estimate of excess oxygen: zero. Of course if the droplets contained a pure iron phase then any oxygen that might have reacted with the pure iron fraction might have been present as excess oxygen relative to the oxide fraction but might also have been present as either of the more stable oxides (Fe_3O_4 or Fe_2O_3) or a combination that may have formed during quench as the FeO slag reacted in the excess oxygen or ambient oxygen.

This may indicate why so much effort was exerted to discourage and even censor the offending paper [***3***] rather than allow readers in the voluntary oxygen user community to decide for themselves. This excess oxygen nightmare and inference of ferrite ions causation for which there are no available thermodynamic data has deflected hazard research and safety practices in untoward directions. *These are distortions of our understanding of the metal hazard and much more.*

(2) *It Has Discouraged the Deployment of Thermo-Chemical Analysis*

One of the research and practices deflections is in the way the related software and even the theory upon which it is founded has been defamed (marginalized and excluded). It does not appear to this observer that any new oxygen system safety ethics or practices can be traced to thermochemical equilibrium software or the base theory for two+ decades. Efforts by this worker to launch subcommittees, task groups are even informal discussions throughout that period have been met with indifference. One perspective was that every now and then some calculations are done with it therefore so what?

Key to the core leadership of G4 (NASA+) are the exact members who have been so aloof and even published the marginalization and exclusion based on the previously cited beliefs in excess oxygen and a vague criticism of how some software does phases. An attempt by this worker to explore phases has been recently prepared [***15***]. However, this worker argues that G4 produces defined consensus (but *not* full consensus based upon his own censorship) voluntary-use standards, and that it is not the prerogative of self-appointed cognoscente to rule on for in this case it has weakened the defined consensus (not-full-consensus) for the committee’s standards.

Nonetheless, figures produced with thermo-chemical equilibrium software that doubtless appear much like Figures 12, 13 and 14 are real and valid if approximate and can be of great use to the oxygen safety practitioner. The precise form of these figures is deter-

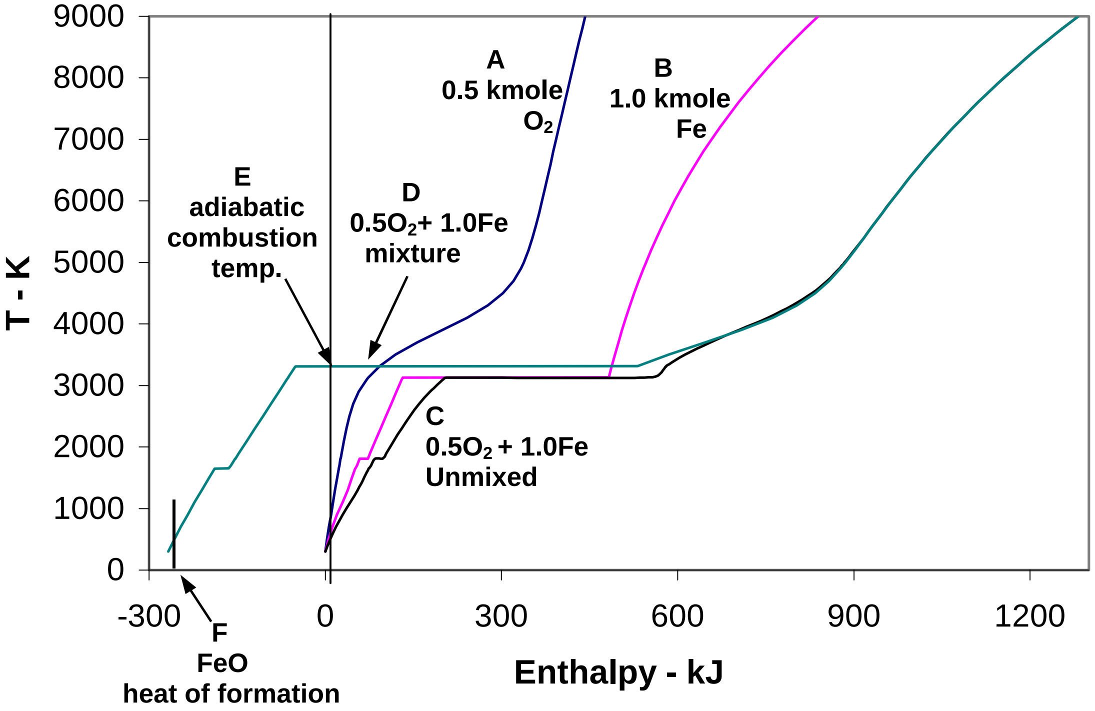


FIG. 12 —***Thermo-Chemical Equilibrium Thermal Profile for Atm. Pressure Iron*** .

mined by astute application of thermo-chemical equilibrium theory and today (2025) software like HSC Chemistry, CEA for Applications, FactSage and others can aspire to facilitate such theory and despite any caveats in their use produce useful product. These three figures[8] (Fig. 12, 13, 14) are produced with HSC by this less-than-optimum operator but are nonetheless quite plausible.

Figure 12 is what this worker began calling a TCE thermal profile in 2008. Something difficult to construct without software but not so difficult to understand once produced. The curve “A” is the heat to warm 0.5 moles of oxygen. “B” is the heat to warm 1 mole Iron, “C” is the heat sum of O_2 plus Fe un-mixed. “D” is the equilibrium heat for the mixture. “E” is the adiabatic combustion temperature. “F” is the heat of Combustion/Formation for FeO. With this one can estimate numerous combustion parameters previously cited, as well as, third generation burn-ratio analysis (called BR3 in a related commentary [***16***]) which examine the heat needed to achieve a given state, factoring in the Second Law of Thermodynamics (heat flows from high temperatures to low temperatures). In this case one can flip the equilibrium curve about an appropriate axis typically the origin (ala Lanyi [***17***]), so that it can show how the Second law of Thermodynamics reveals how heat may transfer

[8] This worker is not sufficiently astute on TCE theory and software to make stronger statements, but the figures do reflect what is generally expected to be seen, even if they are probably not as precise as is possible.

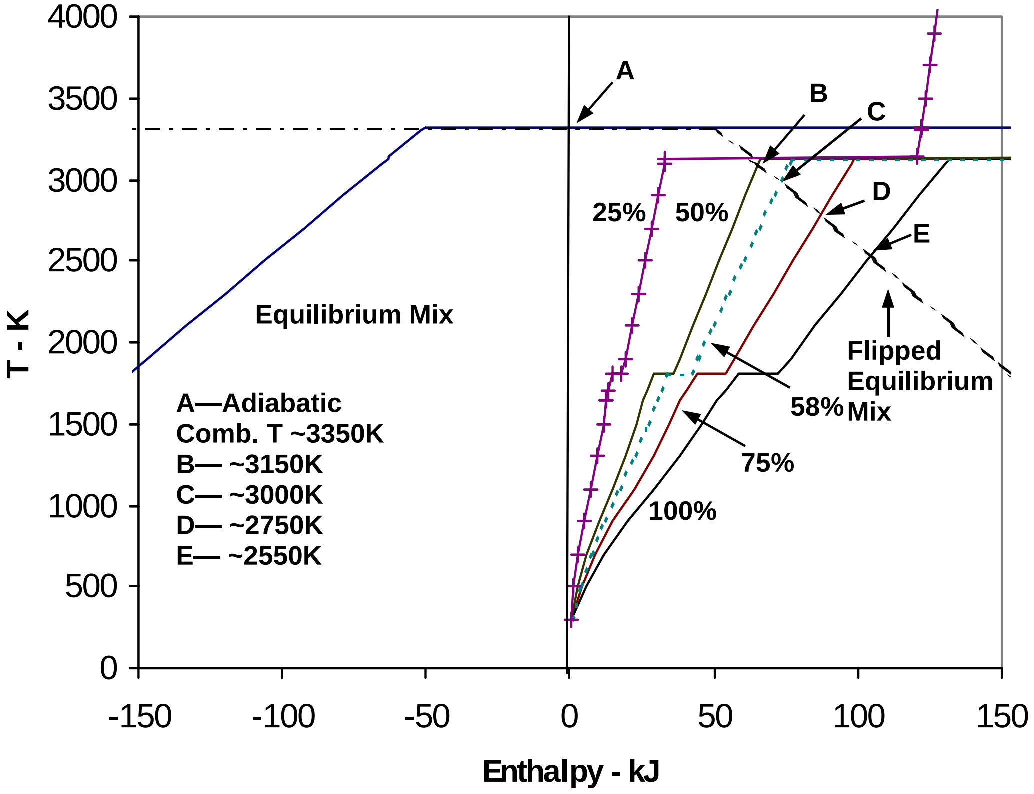


FIG. 13 ***Flipped Thermo-Chemical Equilibrium Thermal Profile for Atm. Pressure Iron.***

between materials.

Figure 13 (dash-dot) exhibits a flipped equilibrium curve for the mixture enlarged and a series of heating curves for five iron masses (100, 75, 58, 50, and 25%) related to the original amount of iron (100%) that reacted to produce the slag. In this case, the adiabatic combustion temperature point (A thru E) decays to where it balances against the warming pure iron/O_2 materials. This worker calls them useful, and this analysis was possible two decades ago. Indeed, it was produced with software from two decades ago.

Note that if a slag droplet contained a molten iron core as an inclusion or mixture equal to the original iron mass (100%) at point "E" the slag droplet average temperature could be no warmer than about 2550K. Similarly included cores of various percentage could at most yield about 2750K average for a 75% inclusion at point D, about 3000K average for a 58% inclusion at point C, about 3150K average for a 50%, 25% or smaller inclusion at point B.

Note that the middle droplet photographed on Fig. 9, page 25 from reference [***17***] includes added circles used to estimate the relative amount of iron in both the inclusion and the outer FeO oxide using density factors for room temperature materials and it suggests the iron in the inclusion is 58% of the iron in the outer slag. And that compares nicely with outer

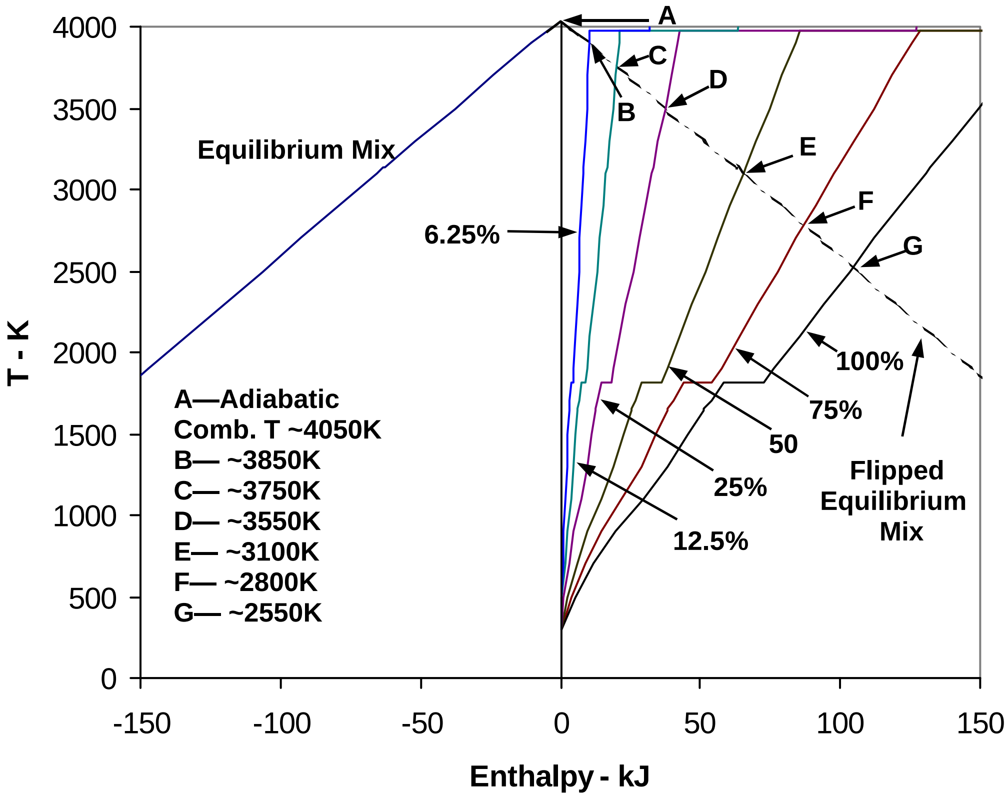


FIG. 14 ***Flipped Thermo-Chemical Equilibrium Thermal Profile for 250 PSIA Pressure Iron.***

surface temperatures reported for atmospheric-pressure combustion of about 3000K.

When the TCE calculation is performed at elevated pressures of 1.8 MPa (~250Psia), one obtains the very similar Figure 14, in which the biggest differences are in elevation of the boiling point temperature for the iron due to pressure. Fig. 14 shows the boiling point of Fe at about 4000K. However the intersection points are remarkably similar for the larger iron inclusions but due to the elevated adiabatic combustion temperature, higher average droplet temperatures at release would be possible for smaller inclusions. And indeed, Kurtz et al. [*7*] claim that at 205 psia, the observed surface droplet temperatures of 3800 –4000K, indicating pure iron inclusions would be smaller. Adiabatic combustion temperature is similar at about 4050K. Adding a large amount of reacted “excess oxygen” demands higher temperatures and smaller inclusions of iron neither of which have been seen.

While the NASA+ cognoscente of ASTM G4 may find tools like this so irrelevant as to discourage and ignore their creation for more than two decades (obstinacy), avoid validation or use, and even to thwart such development within G4, this worker can attest that these data would have found great use in his decades of system evaluation and design and incident investigation and is willing to speculate that his highly skilled coworkers would have also thoroughly exploited these data, ...even when they did not take them as gospel. *And yet for decades the deployment of thermo-chemical analysis like this has been discouraged and the purported existence of possible-hornswoggle excess oxygen is a big factor*.

However, even if this analysis has superb merit, the most one can do is provide an opinion in a manner like this that is alternative to Committee G4's censors. That is the best this worker can do to let those who have a duty to do oxygen safely chose how to do that for themselves. This worker also supports the NASA+ coalition doing their own oxygen safety as they judge fit, even if they disdain such data.

Future Effort

This has argued Dr. Steinberg et. al. did *not* discover *copious* excess oxygen, or even somewhat lesser amounts, as reported in any of the four most cited papers [***1,2,4,6***] in which that claim is made. This has sought to prove with low doubt that dissolved oxygen in iron slag at atmospheric pressure is not copious even if some amount is vital to enable combustion, and quite amazingly as a small portion of the work in a small project on a shoestring budget.

What about combustion in high pressures? Two analyses have argued against the presence of copious excess oxygen even at elevated pressures. Do any, can any, other, intervening projects suggest cracks in that hypothesis?

There are some data this worker is aware of that hint at cracks in the status. In a 2007 Dissertation Thesis [***18***] Dr. Terese Suvorovs reported on attempted rapid quenching of slag droplets in water at high pressure much as this worker had done at atmospheric pressure in 1995. I had anticipated problems with water quench, and so used liquid nitrogen and argon and gaseous argon for my paper, only trying water as an after thought and suspect other methods would have been preferable for the Suorovs's work though perhaps less workable. However, with a much less effective quench approach and with curious results, she achieved some degree of quenching but encountered the same problems I had. My water quenching efforts were very interesting but not real productive nor informative. However, the quenching she obtained did yield benefits though the small amount of quench capacity also produced not droplets but spatter: an experimental mess. She found that within the mess her testing produced, were remnants of unburned iron. This is a crack in the droplets are-completely-burned-to-FeO-with-extensive-excess-oxygen mantra. Dr. J. R. De Wit also is reported to have done similar contemporaneous testing but his Dissertation Thesis has been less available.

In his 2011 Dissertation Thesis [***19***], Dr. David Lynn reviews the earlier Suvorovs and De Wit work and with attribution to Drs. Suvorovs and De Wit, provides (on his page 45) an artists conception of a "new" combustion model shown here as Figure 15, saying: "Based on this microscopy work, Suvorovs et al. [her reference 133]/De Wit proposed a similar model extending the concepts developed by Steinberg's previous model."

It has been more than a little amusing that reference [***3***] is not among the 156 references cited in Dr. Suvorovs's *DT* or the 159 references cited in Dr. Lynn's *DT.* This worker is painfully aware of the effort Dr. Steinberg and his associates applied to exclude it from the G4 collegium. Perhaps it has been banned in Australia or even just in select quarters there. Nor does either Bibliography cite what this worker considers to be one of the most important papers in the entire oxidant fire and explosion safety collegium: Dr. Michael D.

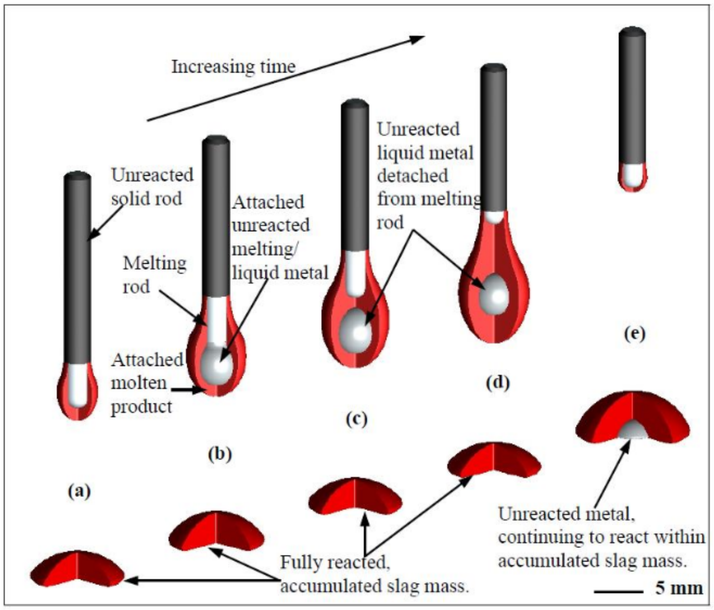


FIG. 15 —”New” model based upon Suvorovs and De Wit data from Lynn *DT* [***19***], page 45.

Lanyi’s 1999 paper [***17***]. Perhaps Drs. Suvorovs, and Lynn have never heard of it. However both cite reference [***13***]. Dr. Suvorovs cites it as reference 52 and Dr. Lynn cites it as reference 86 in their bibliographies and Dr. DeWit was a coauthor. It is the paper immediately after the Lanyi reference in *ASTM STP 1395*. Or perhaps they were just censoring it in a show of solidarity with their PhD candidacy advisor. So it is not difficult for this worker to surmise ignoring it must be either a gross oversight or a calculated ignorance.

If Dr Lynn had been aware of those references (or had not decided to ignore them as the case may be), it might have made his work easier for it would have allowed him to reference actual 1995 photos like those shown in Figure 9, page 25, to lessen the amount of speculation in their DTs (speculation that is so disdained by some of their local peers).

These are cracks in the façade and help make the opposing case but the topic has been fairly quiet for the past decade. Indeed, it appears the dreaded POP (Publish or Perish) environment has led to a stunning growth in several burgeoning CVs, but has had a quite deleterious effect on pushing safety out to the multitudes (from whence the oxygen incident victims are produced). Rather than invest thousands of dollars to keep up with the recent publishing machinations of Dr. Steinberg et al and the NASA+ entourage, this worker would rather invest in actual experimentation to resolve any remaining dispute. So is further testing necessary? Are there any other options, opportunities to (metaphorically, of course) punch these bullies in their metaphoric noses?

Odds are small for any future work in the existing NASA+-dominated community. The community has had decades to put the subject to bed, to clean up what this worker argues is their mess, and has apparently passed on many opportunities to do so. Dr. Steinberg has boasted about acquisition of a "fully instrumented, real-time gravimetric system for metal combustion studies" (similar to a NASA WSTF system) that might well be definitive yet very curiously neither appears to have studied iron. Thereby, they are by default in bed with or a part of the NASA+. This worker can only assume that either they suffer confirmation bias related to Dunning-Kruger Effect or they do not want to risk the outcome. That may be smart.

Admittedly, it is often said it is not so much the crime as the cover-up that is the worst problem. If this is a house of cards, and this worker surmises it is, and it finally crashes, and may have already crashed, it will be unfortunately devastating and that is also addressed in [***5***].

Replicating the key portions of the scant work in Reference [***3***] would be worth confirming but not persuasive. The extent of that testing was not nearly robust. However, unlike the complexity applied to measuring excess oxygen, when one quenches burning slag more effectively and gets different conflicting results, it is a potent indictment of the earlier contrary results.

A series of photos like those in Figure 9 and others from reference [***17***] showing droplets rapidly quenched at higher pressure that exhibit only wustite and iron (as opposed to frequently published slow "quenched" droplets that also show magnetite and hematite that could have burned on the slags contained "excess oxygen" during quench, but may have actually burned in the ambient oxygen would be the last nail in the coffin, the wooden stake driven through the vampire's heart of an excess-oxygen bad dream. Repeating that would be more than equally potent evidence at elevated pressure but is a much more challenging experiment. This worker has not supervised nor populated a combustion lab for more than two decades. I would certainly like to replicate my old work or take on gravimetric methods and often ruminate if a project small enough to be personally financed or performed is possible. It would be especially gratifying if a simple test could "definitively" prove large-scale elevated-pressure excess-oxygen is largely bogus. But it would be more gratifying to get ASTM G4 back on track.

Hence, a somewhat less imposing "proper" experiment along the lines of Sircar et. al. [***8***] might be most convincing. That apparatus was expensive in 1989 but much less expensive options might be possible today and are being looked into. If funding were possible perhaps it might entice, for example, the Penn State Combustion Lab if hopefully outside the NASA+ intimidation or funding influence to take it on perhaps with a new wunderkind PhD candidate of their own. Or perhaps an emerging consultant group seeking to make a name and willing to crack some eggs. And maybe among those who so liberally aid education are some who might be willing to divert some to a GoFundMe project like this, especially if there are any NASA haters (like moon-landing deniers) who would like to see them take one on the metaphoric nose. All options are under consideration.

If personal funding and performance are indeed possible, any resulting unimpeachable proof might help motivate the oxidant safety community to take on other important practical projects that have been ignored for too long, if they do not really agree with the current mistreatment of priorities in the field.

However, if the “excess oxygen science” is now a hornswoggle as this worker believes based on the analysis here, it deserves closure.

The writer can not pretend to have the skill necessary to formulate complex theoretical combustion mechanisms. However, he thinks Dr. Michael D. Lanyi’s 1999 paper [***17***] was a crucial contribution that appears to have been snubbed perhaps in part because of his association with me. Indeed, Dr. Steinberg who attended the presentation may have been the only one to cite it [***13***] and he cites it as supporting his own copious excess oxygen machinations when in fact it was offered to dispute them. That would be going beyond confirmation bias into confirmation revisionism (like painting spots on mice). I fear much of the effort to understand iron and steel combustion has been misdirected and misplaced. Iron and steel still deserve much study aimed at safe use [***20***] both in containing and understanding existing risks and expanding its safe use.

Closure

The shadow of copious excess oxygen is ugly even if ultimately proven valid and has hung over and interfered with several potentially important oxygen safety initiatives. If copious excess oxygen is real in the large-scale degree that is promoted, then the interference is unavoidable and *must* be endured. But if it is largely bogus, or even the hornswoggle as has been the hypothesis here, then the harm it has done and is doing is substantial. No wonder its proponents are so unwilling to admit or even consider even the remote possibility they could be wrong, yet so willing to work so hard to censor rather than openly debate.

No small number of workers in this field (perhaps not all objective or impartial) swear to copious excess oxygen existence, perhaps because NASA+ is in many cases a major customer and therefore always right by default, regardless of how wrong they may believe them to be. The World-wide quality movement has lived by this “Rule 1”, but it has a dark side. The customer is always right. If NASA wants to do something dumb and if you are not their “Yes”-Robot, you may lose their business. If you protest cold O-rings when they are about to launch Challenger, you must fear for your job if it does *not* blow up. You do yourself no good by discouraging them from buying your products. However, notwithstanding this potent disincentive, I am not the only skeptic to copious excess oxygen theory. It is not the settled science they have tried so hard to assert.

These days one can hear outrage when a vendor legally sells an AR-15 and a thousand rounds of ammunition to a Rule 1 customer who actually has more right to that purchase than the NASA has to use taxpayer money to launch cold O-rings. Historically ASTM G-4’s first generation of NASA members were scrupulous, even paragons of virtue. This worker wishes he had a nickel for every time a NASA member spoke of wanting to avoid the “fact or appearance of impropriety”. Today “smug” and “arrogant” are better descriptions. NASA needs to learn that this image, their history, their purchasing power gives their people a power, that magnifies the NASA aura, that is easily abused, even unintentionally. It wrongfully taints some of their best workers. It raises the need to be right. It makes it possible to do sloppy faulty research like I allege for the copious excess oxygen issue, among others. And it makes it possible to ram it down associates throats even when it does not serve NASA’s best interests. Macro-NASA needs to clean up its act and for its own damn good.

This gripe is not intended as excess oxygen and phase aversion diatribe. To wit, this commentator has dreaded every one of the 134 Shuttle missions because of LOX-tank oxygen safety issues unrelated to excess oxygen and elaborated upon elsewhere [***21***]. There is still more NASA research I am aware of that is in my judgment flawed. Efforts of this worker and others to discreetly alert NASA workers so as to not offend the vendor-customer quality balance appear to have been ignored. This worker really hopes he is wrong on this point. For they have begun launching of manned Artemis missions, and I am still unaware of valid efforts to address its even bigger LOX tank and environment. Maybe they exist in some obscure place. Maybe its missions will go flawlessly due to unpublicized diligence or just dumb luck. If even one doesn't it could prove to a be yet another well-deserved comeuppance of little consolation.

Consequently, this paper makes the case that the four main papers citing proofs of large-scale excess oxygen do not prove anything. I argue that I have refuted its presence "definitively" at low pressures, and have found valid new arguments that tend to refute it at elevated pressures also.

If excess oxygen is the hornswoggle it appears to be to this commentator, it will reflect badly on numerous lettered workers, but it is also emblematic of greater concerns. So there is a potent incentive to in the style of politicians to stonewall, launch into denial, and do whatever it takes to prevent the inevitable rather than fess up and face the music. Nonetheless, if copious excess oxygen can finally be refuted then it must be, regardless of how embarrassing that would prove to be in some quarters. There is too much at stake not only in the grandiose projects that NASA undertakes but in the everyday lives that encounter the hazards of commercial oxygen ...to not get it right. I welcome being proven wrong (It would not be the first time) as opposed to being declared wrong and censored administratively.

Either way, those who are accountable for the oxygen fire hazard (including folks at NASA-at-Large) get to chose sides for as noted herein they are accountable to get their part right. They and their potential victims literally have the most "skin" in this game.

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