Received OSRV
September 17, 2007

Comments on the Seal Strength Requirements in MSHA's ETS on "Sealing of Abandoned Areas"

By
Murali M. Gadde, Dave Beerbower and *John Rusnak*
Peabody Energy, Saint Louis, MO.

The Mine Safety and Health Administration (MSHA) has issued an Emergency Temporary Standard (ETS) on sealing of abandoned areas in May, 2007 [1][*]. MSHA sought technical comments on several provisions included in the ETS on sealing and related aspects. This note here specifically addresses the validity of the tiered approach on seal strengths proposed in the ETS.

In general, we think the tiered approach and the respective seal strength requirements are reasonable given the complications involved in predicting the explosion loading in a real coal mine situation and compared to what other coal producing countries all over the world are doing. We, however, feel that the overpressure requirements set in the ETS lack enough support when one considers the real-world mechanics. In order to understand our reservations on the validity of the prescribed explosion loading in the ETS, it is imperative we take a close look at the NIOSH report [2] on "*Explosion Pressure Design Criteria for New Seals in U.S. Coal Mines*" which apparently formed the basis for the ETS.

When NIOSH produced a draft report on the explosion loading criteria at the beginning of 2007, the reviewers submitted several comments on the draft, which are also provided here at the end of this document. Since then, we have had an opportunity to re-examine several aspects of the NIOSH study more thoroughly. In the mean time, NIOSH finalized the seal design report in July 2007 with few modifications based on comments submitted by researchers, practicing engineers, policy makers and several other interested parties. Therefore, our comments below address the final NIOSH report.

The reviewers believe that the research contained in the NIOSH seals report [2] is a good start to address an exceedingly complicated problem but is well short of a dependable study to guide policy. In the NIOSH report, extremely idealized and hypothetical theories formed the basis for their design criteria with very little supporting data, when the preferred approach is just the reverse, i.e., real data guiding the development of a theory. We, however, think NIOSH deserves credit for providing the necessary foundation for more involved research and for initiating a debate on the topic at the national and international levels.

Similar to our previous set of comments, we will address different issues related to the NIOSH seal design report on a point-by-point basis. By this critical examination, the reviewers also intend to shed more light on why the detonation loading suggested in the NIOSH report [2] is extremely unlikely and how actual explosion studies in experimental galleries and real mine cases do not support such a conclusion.

---

[*] Reference number given at the end

1.0 Existence of "Standard Air" in Abandoned Workings

One of the major and fundamental objections to all the work in the NIOSH report [2], including past laboratory experiments or studies conducted in experimental galleries, is with regard to the type of atmosphere considered. In almost all these studies – both on methane and coal dust explosions – **standard air** is included as the oxidant. The standard air contains about 21% Oxygen and 79% Nitrogen with comparatively negligible presence of other gases. For instance, the 1992 edition of the *CRC Handbook of Chemistry and Physics* [3] lists dry standard air mole fractions as below:

Nitrogen : 0.780840
Oxygen : 0.209476
Argon : 0.009340
Carbon-dioxide : 0.000344

and some negligible traces of

Neon : 1.818×10^{-5}
Helium : 5.24×10^{-6}
Krypton : 1.14×10^{-6}
Xenon : 8.7×10^{-8}
Methane : 2.0×10^{-6}
Hydrogen : 5.0×10^{-7}

Obviously, because of the extremely small levels of the last six gases, they can be safely ignored from any explosion studies.

Now contrast the above data with a set of atmospheric composition measured close to a seal in a coal mine gob. The data (expressed in percentage by volume) reads as

Nitrogen : 74.01
Oxygen : 10.96
Methane : 10.01
Carbon-dioxide : 4.432
Ethane : 0.1884
Carbon-monoxide : 0.0004
Hydrogen : 0.0003

With the help of another U.S. coal company, the reviewers have collected thousands of actual gob composition data sets from a few mines. It must be mentioned that almost all of these numbers were determined long before the ETS and were collected, most likely, for the purpose of early detection of spontaneous combustion. Also, these numbers represent the conditions close to a seal in the gob. Obviously, the mixture further into of the gob will be even more inert. Analysis of this data clearly shows that among the thousands of samples collected, only a handful had oxygen above 10% when methane was between 8% and 12%. From this data, we infer that for the mines considered, it is extremely difficult to have both sufficient oxygen and around 10% methane at the same time. For the remaining data, even when the methane content was greater than 5% or below 16%, the atmospheric composition was such that the estimated explosion loading was lower than that obtained for methane concentrations between 8% and 12%.

When one considers the experimental gallery studies conducted in the past, using standard air in those studies was totally relevant because the purpose of most of the earlier research was prevention of explosions in active workings. Due to ventilation, active workings receive fresh air from surface and thus the standard air assumptions would not invalidate the data. But, when the focus is on abandoned workings, any studies conducted without considering the actual gob atmosphere will not be meaningful.

To illustrate the effect of standard air assumptions on the explosion output, the reviewers used NASA Lewis thermodynamic program [4] to estimate the value of the constant volume explosion pressure in actual gob atmospheres. The results are shown in Figure 1. For all these samples, the methane content was between 8% and 12% by volume and oxygen above 10%.

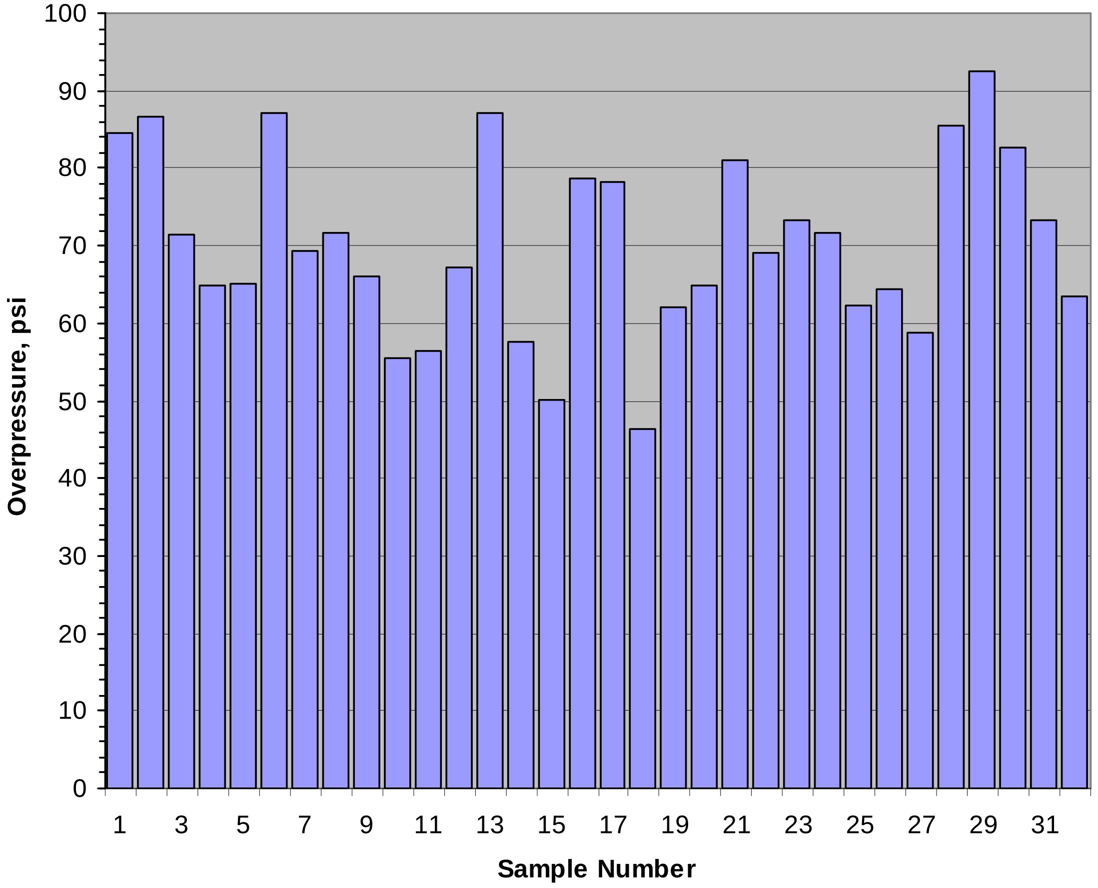

Figure 1. Constant volume adiabatic explosion overpressures considering actual gob compositions.

Using standard air assumptions and 10% methane, the NIOSH report [2] estimated the constant volume explosion overpressure to be 120 psi. The real data in Figure 1 shows even under the conservative assumptions of a constant volume, adiabatic process and complete combustion, the actual pressures are significantly less than the 120 psi value given in the NIOSH report.

The results in Figure 1 should not be a surprise. In fact, the effect of mine atmospheric composition on flammability has long been recognized. In our previous review on NIOSH draft seal report, we mentioned the research conducted by R. V. Wheeler and his colleagues in early 1900s on how the flammability of methane changed with oxygen content. Of course, there are more detailed studies done subsequently like Cowards and Jones [5], Zabetakis [6] and several others [7]. The lack of any reliable data on actual gob compositions might have prompted NIOSH engineers to use standard air in their present study. But, as the results in Figure 1 indicate, ignoring the more likely atmospheres in sealed areas will result in erroneous and excessive design standards.

When the NIOSH report [2] refers to several studies conducted in general gas explosion research, one must realize that in addition to using more reactive gases than methane, those experiments also used either pure oxygen or standard air as the oxidant. Therefore, results of such studies do not directly apply to an explosion in an abandoned working where the gas composition is significantly different from the "idealized" mixture studied. The same argument applies to methane and coal dust explosion studies – both in the lab and in experimental galleries – conducted by mining researchers under standard air conditions.

The reviewers fully recognize that there will be many combinations of gases possible when you consider all of the variables that affect gob atmospheres in the hundreds of coal mines across the country. The reviewers also recognize it is practically impossible to monitor every gob to identify the full range of atmospheres for consideration in research. While there is no need to consider every possible gob composition to suggest some valid explosion loading, it is definitely necessary to collect data from a few coal mines in the country under diverse geologic and mining conditions with different levels of methane content. By conducting gas sampling at these select mines over a reasonable amount of time, it should be possible to construct a probability distribution curve for explosion loading. Using such a probability curve, one can prescribe design loads with an acceptable level of risk. Additionally, it will also be possible to develop a "standard gob atmospheric model" which in our opinion will be significantly more realistic to use in experimental and numerical investigations.

The NIOSH report [2] also mentions that pressure piling is one mechanism by which the estimated constant volume explosion pressures might get magnified. In this connection, the NIOSH report [2] says, "*Combustion of precompressed unburned gases leads to pressures greater than the 908 kPa (132-psia) CV explosion pressure. For example, if pressure piling has increased the pressure to 300 kPa ahead of the flame front, then the pressure immediately behind the flame front will be 300 kPa x 9, or 2.7 MPa (392 psia)*." This statement is contradictory and misleading. If NIOSH's constant volume assumption is correct, then <u>any increase in pressure will lead to a proportional increase in the initial temperature</u> of the gases ahead of the flame front. In effect, they both cancel out and the final pressure will remain exactly the same as 132 psia. Does that mean, pressure piling is impossible under constant volume conditions? We certainly don't think so. If anything, we believe the constant volume assumption is the one that needs to be questioned. However, this contradiction underscores how unrealistic the simplified are the assumptions made in the NIOSH report.

2.0 What about 50 psi Seals?

The 50 psi standard for monitored seals has no scientific basis given in the NIOSH report [2]. The requirements of less than 5m explosive zone or less than 40% explosive volume also did not have any explanation. In the preamble to the ETS, it is written "*Use of a 50 psi overpressure seal requires the mine operator to maintain inert atmosphere in the sealed area since explosions cannot occur within inert atmospheres*" [1]. If there is no chance of an explosion in a managed gob, what is the need for a 50 psi seal? This requirement might be to have additional safety to account for some unknowns. If one keeps on making worst-case assumptions, practical results will never be found or implemented.

Our Australian counterparts are doing an impressive job with gob atmospheric management when using 20 psi seals. But the NIOSH study and the ETS have conveniently underemphasized the conditions under which a 50 psi seal is installed in Queensland. In fact, Section 325 of Queensland *Coal Mining Safety and Health Regulation 2001*, which deals with "*Types of seals for particular circumstances and parts of mines*" states:

> "*(1) The underground mine manager must ensure a seal installed, other than at the surface, at the mine is, a minimum, of a following type –*
>
> *(a) if the level of naturally occurring flammable gas at the mine is insufficient to reach the lower explosive limit for the gas under any circumstances – type B;*
>
> *(b) if persons remain underground when an explosive atmosphere exists and there is a possibility of spontaneous combustion or incendive spark or other ignition source – type D;*
>
> *(c) for an underground mine, or part of an underground mine, not mentioned in paragraph (a) or (b) – type C.*"

Schedule of 4 of the Queensland mine regulations explain the different seal design criteria as given below.

Type B seal – Capable of withstanding an overpressure of 35 kPa
Type C seal – Capable of withstanding an overpressure of 140 kPa
Type D seal – Capable of withstanding an overpressure of 345 kPa

The above requirement basically means the Australian regulations consider a 50 psi seal strong enough to resist an explosion occurring in the gob. It is also important to realize that the Queensland standards allow people to stay underground even if explosive atmosphere exists as long as the seal has a 50 psi rating. While we extol Australian practice of gob atmosphere management in conjunction with 20 psi seals, why don't we also discuss, in the same tone, their rationale for adopting a 50 psi standard for seals with explosive mixtures behind them?

3.0 Possibility of Detonations in Methane-Air Mixtures

We commented on the lack of support for the deflagration-to-detonation "run up" length prescribed in the NIOSH report. The principal explanation that NIOSH had [2] in this connection was the limited diameter of the pipes used in the methane explosion testing. When compared with the size of the detonation cell, the pipes were too small to allow development of a detonation in such experiments. While this may be a possibility, there is no data to support such a claim.

It is not accurate to use data derived for one type of gas and extend those relations between detonation cell size and pipe diameter and make claims on a different type of gas. Therefore, at least a few laboratory scale experiments must be conducted with large diameter shock tubes to conclusively demonstrate the possibility of detonation in methane-air mixtures.

Even if it is assumed that the data derived for different explosive gases is applicable for methane, for the reasons mentioned in section 1.0, the validity of the data derived using standard air or pure oxygen are questionable when applied to abandoned mine workings. Indeed, if detailed studies using gob atmospheres are conducted, the relations derived under standard air conditions for "run up" length, detonation cell size and the magnitude of detonation pressure may well be proved inapplicable. Therefore, for gob explosion studies, it is extremely important to consider the realistic atmosphere composition in any experimental investigations.

In our prior comments on NIOSH draft seal report, we pointed out an inaccuracy in the equation used for the estimation of C-J detonation pressure. The final NIOSH report has the correct equation in it. Despite the correction, it is interesting that the estimated magnitude of the C-J detonation pressure did not change! After close scrutiny, the reviewers found that the inputs used for the equation were changed in the final NIOSH report [2] in such a way that the final C-J pressure was the same as it was under the incorrect equation. For a ready reference, the C-J pressure equation and the inputs used in the draft and final versions of the NIOSH report are reproduced below.

$$\frac{P_2}{P_1} = \frac{1+\gamma_1\left(\frac{D}{c_1}\right)^2}{1+\gamma_2} \qquad (1)$$

P_1 and P_2 = Pressure ahead and behind the detonation wave,
γ_1 and γ_2 = specific heat ratios of reactants and products, respectively,
c_1 = speed of sound,
D = detonation wave speed.

Draft report inputs:

γ_1 = 1.34
γ_2 = 1.28
c_1 = 341 m/sec
D = 1800 m/sec

Final report inputs:

γ_1 = 1.39
γ_2 = 1.17
c_1 = 352.6 m/sec
D = 1815 m/sec

No explanation was provided in the NIOSH final report on the reasons for the change. What is interesting though is that when the magnitude of the reflected pressure was estimated in the NIOSH final report [2] (page 35), the specific heat ratio for the combustion products was taken as 1.28, exactly the same as in their draft report! If their estimate of specific heat ratio for combustion products is 1.17, then the ratio of reflected to incident pressure ratio should be equal to 2.57 as opposed to 2.54 given in the NIOSH report. Of course, the difference is no big deal but this example illustrates the inconsistency of NIOSH's analysis.

Again, the inputs for C-J detonation pressure in the NIOSH report were derived using standard air conditions. Using the NASA Lewis program, we estimated the specific heat ratios for the combustion products for the measured gob atmospheric data mentioned before. The results are shown in Figure 2. This analysis shows that the specific heat ratio used in the draft NIOSH report is close to that estimated using real gob data.

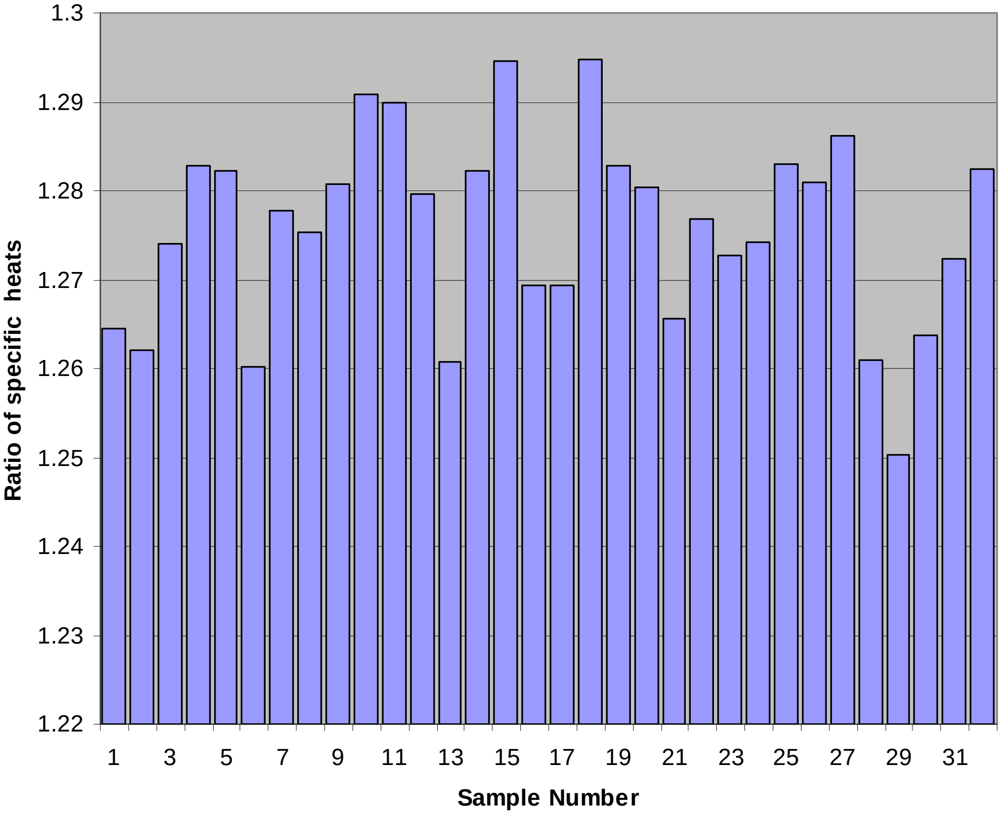

Figure 2. The range of specific heat ratios determined for actual gob compositions.

Additionally, as pointed out in our comments on the NIOSH draft report, the C-J detonation equation requires several assumptions that may not be borne out in reality for coal mine explosions. For instance, the C-J theory treats the detonation wave as a plane with an infinite rate of reaction at the shock front. However, when NIOSH talks about the detonation cells, it is automatically implied that the shock front is a 3D structure as opposed to the C-J assumptions. Similarly, chemical dissociation at high temperatures such as those seen during combustion will further alter the thermodynamics of the reaction. Moreover, it is well known that the specific heat ratio values change with temperature. It is not clear from the NIOSH report if the specific heat ratio estimate accounts for the very high temperature of the combustion.

Another major objection to NIOSH's analysis stems from the fact that the explosion event inevitably creates some turbulence soon after ignition, resulting in a large cloud of dust (rock and coal) being generated. Thus, the real situation is a two-phase medium (gas + solid) while all the NIOSH analysis treats it as a single-phase. When the presence of dust in the mixture is a certainty, how can we ignore such an important factor from analysis and yet rely on such studies for rule making? The presence of incombustible solid particles in the mix will absorb some of the heat released, while the coal dust will add heat. Thus the net effect depends on the amount of incombustible matter present. With the large amount of rock dust that is routinely applied in U.S. coal mines, there is a good chance that such dust, when suspended in air, will act as a heat sink during an explosion. Further, the suspended dust will increase the density of the medium and the pyrolisis of dust will alter the thermodynamics of the reactions. All these real events are completely ignored in the NIOSH studies. It is exactly these insurmountable real-world mechanics that forced early researchers to discount theoretical formulations. They resorted to expensive and cumbersome lab and experimental mine studies for additional data. Despite the availability of this wealth of knowledge, it is extremely troubling that significant policy decisions are being made based on those very theoretical equations that the earlier mine explosion researchers deemed unreliable for the purpose.

4.0 <u>Is there Enough Evidence to Support Gas Detonations in Mines?</u>

Evidently, the NIOSH report suggests so. In fact, the report says, "*Test explosions conducted at experimental mines in the United States and abroad confirm the reality of these pressures*" [2]. Further, in support of the detonation pressures estimated, NIOSH report mentions the Blacksville shaft explosion case where allegedly pressures over 1000 psig were developed. To a casual reader these numbers sound realistic and could lead one to believe that there is enough data to support detonations in methane-air mixtures. However, except for the German works, when we went through each of the papers reported in the NIOSH final report, we found the data was not quoted accurately. While the numbers included in the NIOSH report are accurate, it is the conditions under which those pressures were measured or the way those high pressures were estimated that casts doubt on NIOSH's conclusions.

4.1 <u>Nagy's report</u>

While Nagy [8] did not provide any data to support methane detonations, he did give numbers that are in close agreement with NIOSH's constant volume combustion pressure. This is no surprise because the experiments in the laboratory or in the Bruceton experimental mine that Nagy refers to, use standard air as the oxidant. But as shown in section 1.0 in this report, the gas composition in abandoned workings can differ substantially from standard air. And for the same reasons outlined in section 1.0, the results described by Nagy may not totally apply to abandoned workings.

4.2. <u>Cybulski's 1967 Research</u>

Please refer to the comments at the end of this report that the reviewers submitted in March 2007 on NIOSH draft seal report. There, we made extensive comments on the experimental conditions used in Cybulski *et. al.*'s 1967 studies [9]. Basically what we said was that the methane explosion in that particular test, where high pressure was generated, was initiated by using solid explosives. Further, the change in the cross-sectional area near the damaged stopping and the presence of arch supports around the tunnel aided the development of significant turbulence. All of these events led to the extremely high pressures noticed in the test. We also commented that the data from this test was not useful in making any conclusions about the "run up" length for DDT as the test itself was started as a detonation.

As we read that 1967 Cybulski *et. al.*'s [9] paper again, we found a few more points that are relevant to our discussions here.

In connection with the bulkhead design, Cybulski *et. al.* [9] write, "*The concrete and steel portions of the bulkhead were designed by the methods of civil engineering for a static head of 10 at. (142 psi)*." The key words to note in the previous sentence are "static head" and "methods of civil engineering".

Because of the damage to the 142 psig rated bulkhead in one of the tests, the structure was beefed up to a rating of 426 psig. Again, this structure suffered some damage when the whole 60m of the enclosed zone was filled with 12% methane [9]. Based on this observed damage, Cybulski *et. al.* [9] inferred the explosion loading must have been at least 426 psig.

Unfortunately, no engineering details on the bulkhead design were provided in the paper to conduct a full three-dimensional dynamic structural analysis to realistically estimate the peak capacity of the bulkhead. Additionally, a network of pipes was inserted through the bulkhead for several purposes related to the test and those openings would definitely create stress concentrations which can not be easily accounted for without conducting a numerical stress analysis. Therefore, observed structural damage during an explosion event is not entirely helpful unless a realistic structural analysis is conducted considering as many factors involved in the process as practically possible.

If one uses a simple single-degree-of-freedom elastic model, with a dynamic load factor of 2.0, the peak reflected explosion pressure on the bulk head will be 213 psig, which is about one-third of the estimate given in the NIOSH report.

4.3. Cybulski's 1975 Research

The reviewers were surprised to see the importance given to these Cybulski's studies [10] in the NIOSH report. We still struggle to comprehend how Cybulski's data on coal dust explosions are directly relevant to a study whose focus is mainly on methane explosions. The only connection we can think of is if a methane explosion triggers a full blown coal dust explosion, which then propagates by itself. But, the context in which the Polish data was given in the NIOSH report did not give such an impression. Whatever logic NIOSH applied, it is necessary to examine the conditions under which Cybulski's data was obtained and how the explosion pressures were determined to judge their relevance to the current debate.

Undeniably, Cybulski was a pioneer on mine explosion studies and his contributions to the field are enormous. It was an absolute intellectual delight to read his translated text on "*Coal Dust Explosions and Their Suppression*" [10]. As the title of the book suggests, the main emphasis of the research described in the text was on coal dust explosions. As a matter of fact, the 595 psi detonation pressure that NIOSH report quoted in support of their theoretical predictions on reflected detonation pressure in a methane explosion was <u>indirectly</u> estimated during a study on coal dust explosion. It is important to note that no methane was involved in this particular test. Cybulski and his colleagues conducted a series of nine tests to study how coal dust explosions develop if the point of ignition was at some point outby the face and the explosion was given a chance to propagate towards a blind face.

Some other relevant conditions specific to the test no. 1397 are [10]:

- The explosion was started using a very strong initiator composed of a combination of 10 m of pure Polish Barbara coal seam dust with 85% of the sample being below 75 μm size. This dust was very conveniently placed on roof and on side shelves that facilitated easy dispersion when disturbed by ignition of 750 grams of black powder.
- The explosion was initiated at a distance of 200 m from the blind end of the 400 m long test tunnel.
- The whole section of the tunnel from the blind end to the location of initiator was filled with medium fine Barbara coal seam dust deposited in the most favorable locations for an easy dispersion.
- The 400m tunnel opens to the surface through a raise bore at the end of the experimental section.
- Normal air supplied the necessary oxidant.
- No rock dust was applied in the section where coal dust was present.
- Between 220m and 320m from the blind end of the tunnel, pure stone dust was applied to suppress explosion from propagating outby the initiation point and force it towards the blind end.
- The tunnel was horse-shoe shaped with supports installed.

The results obtained from test no. 1397 are shown in Figure 3. The 1600 m/sec flame velocity was measured in a small tunnel parallel to the 400m test tunnel. Both the 2000 m/sec and 1875 m/sec flame velocities were measured at the same location in the 400 m tunnel with two different techniques. Considering the above test conditions, it is totally meaningless to say the measured flame velocities in this test support calculations on methane detonations.

Cybulski [10] also reported that during this particular test, a 1.4 m^2 emergency stopping was damaged as shown in Figure 4. In connection with this damage, Cybulski notes "*According to computations the shearing force, necessary to cut out a gap in the door of the emergency stopping, had to be equal to not less than 424 kN and the static pressure at the blind end to not less than 4100 kN/sq m*" [10]. The key word to notice in the preceding sentence is "static pressure." From the discussions in the text [10], it is not clear whether the static pressure Cybulski refers to is explosion static pressure or the static pressure needed to punch the hole. Barring that one sentence quoted, no explanation was given on how the numbers were derived. Considering the unavailability of sophisticated dynamic structural analysis tools at that time, we suspect the estimated force needed to fail the stopping might have been computed from static stress analysis.

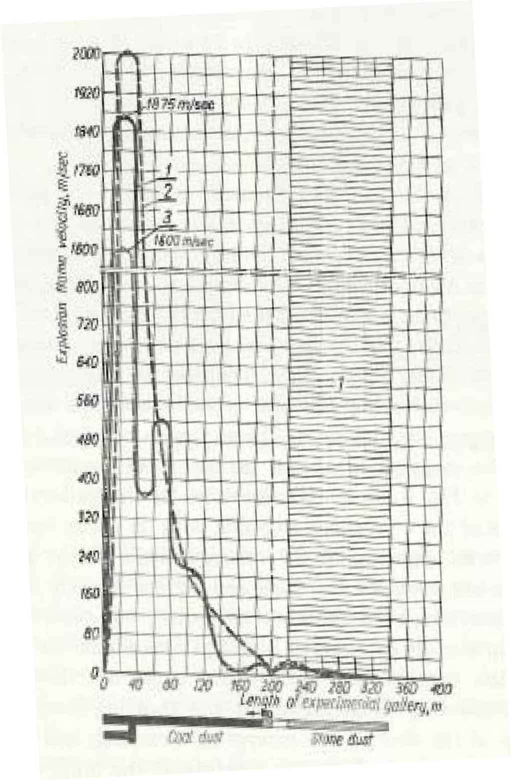

Figure 3. Flame velocities measured in the Polish test no. 1397 [10].

If one assumes a dynamic load factor (DLF) of 2.0, the estimated reflected explosion pressure would equal 297.5 psi, which is less than half the detonation pressure estimated in the NIOSH report. With the availability of realistic three-dimensional dynamic structural analysis codes, it is now possible to more accurately estimate the magnitude of explosion loading necessary to cause the type of damage seen in Figure 4. Unfortunately, Cybulski did not supply any details on the construction of the stopping door or its material properties to conduct a structural analysis. Of course, as mentioned before, we don't think it is necessary for us to conduct any structural analysis as the test is irrelevant to what we are discussing now.

As an explanation to the unusual events noticed in test no. 1397, Cybulski writes [10], "*...during the initial slow development of the explosion the blast wave reached the outlet of the gallery creating a reflected wave, which was a rarefaction wave, and behind this wave a pressure wave moved into the gallery supplying oxygen to coal dust undergoing already the process of deflagration. This wave caused an adiabatic rise of the pressure in the explosive medium and a turbulent motion of the very well dispersed coal dust. In consequence conditions were created conducive to the detonation of the dust*." What Cybulski is saying is in this particular test, some very unusual test conditions were created because of the layout of the experimental gallery that resulted in a detonation event. So, it is up to all of us to figure out how those unusual conditions created in a study of a pure coal dust explosion is applicable to methane explosions in fully rock dusted, abandoned workings in U.S. coal mines.

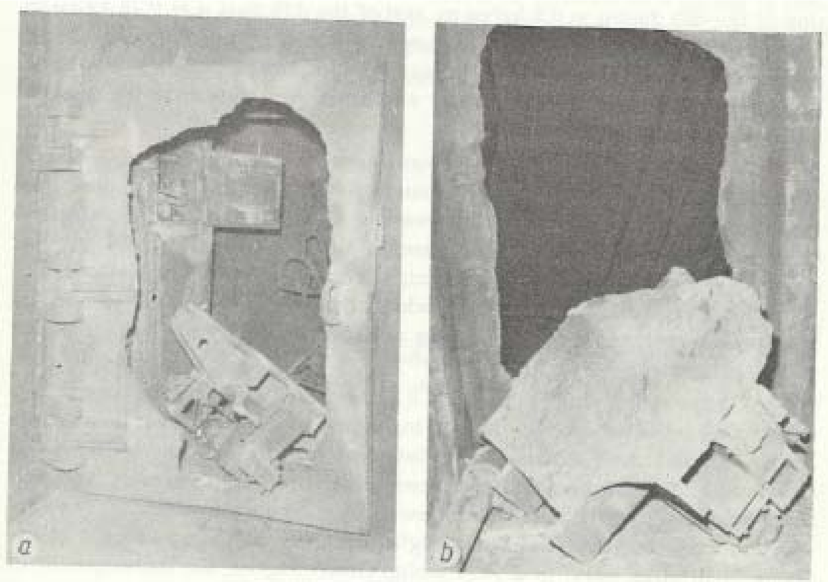

Figure 4. Damaged emergency stopping in the test tunnel during the Polish test no. 1397 [10].

4.4 *Blacksville Shaft Explosion*

The summary of different explosion occurrences in the abandoned areas of U.S. coal mines given in the NIOSH report [2] shows one case where the explosion overpressure was said to be 1000 psi. This particular event generated substantially greater loading than any other explosions in a real mine situation. This Blacksville No. 1 shaft explosion was cited as one of the supporting evidences for the possibility of detonations in methane gas explosions. The anomalously higher explosion magnitude prompted the reviewers to investigate further. We found that the Blacksville shaft case was subjected to an extremely simplistic study with insufficient data to draw any meaningful conclusions on the explosion magnitudes.

At Blacksville No.1, welding arcs ignited methane gas in a shaft and the resulting explosion pressures completely destroyed the shaft cap, a portion of the shaft collar and the headframe over it. Based on the observed damage, MSHA investigators back calculated the explosion overpressures and the results were provided in the report, "*An Elementary Structural Failure Analysis of the Reinforced Concrete Shaft at the Blacksville No. 1 Mine*" [11]. A review of this report shows several major problems with the analysis as evidenced in the following statements reproduced from the report [11]:

- "*Detailed design and/or as-built drawings of the subject shaft, which was reportedly constructed in 1969, were not available. Furthermore, it was impossible to obtain from the remaining shaft and destroyed portions thereof, a clear idea of the pattern of steel reinforcement in the concrete shaft.*"
- "*In this report, an elementary, structural, static failure analysis was conducted utilizing analytical techniques and formulae, whose accuracy is commensurate with the accuracy of the available investigative structural data.*"
- "*It was those portions of the shaft which were above grade that failed catastrophically.*"

- *"The compressive strength of the concrete should be a minimum of 3,000 psi (20,688 kilopascals). In all analysis herein, 3,000 psi (20,688 kilopascals) is used as the minimum compressive strength of the concrete in the subject shaft."*
- *"In all analyses herein, it is assumed that the steel reinforcement provided the concrete in the shaft with an overall 'tensile strength' equal to the compressive strength of the concrete alone (i.e., a balanced design), namely, 3,000 psi (20,688 kilopascals)."*

The above reproduced assumptions from the MSHA report [11] imply very little was known about the shaft design or material properties used in the shaft lining. Further, the analysis was based on elementary static analysis to estimate the failure loading. There were three different types of analyses conducted for shaft lining failure: thin-wall tube, thick-wall tube and bursting pressure analysis. Each of these analyses, used to explain the same failure, resulted in very different internal pressure estimates.

A shaft is a complex structure. The loading on a shaft lining could come from several sources, namely, lateral earth pressure, shaft cap, the headframe structure standing over it and, of course, the explosion loading. The structural behavior of this particular shaft is further complicated by the fact that a portion of the shaft collar is above ground and the rest below. Under such conditions and without knowing anything about the material properties or the construction details, how can anyone conduct a reliable analysis? What is more amazing is how can such a case history be cited as the evidence for a problem as complicated as methane explosions?

To further illustrate how flawed the whole analysis is, we conducted a dynamic structural analysis for the shaft collar while keeping everything else as assumed in the MSHA report [11]. The modeling results were first verified for the static pressure case, to see if the modeling is giving acceptable results. Using thick-wall tube analysis, the MSHA report estimates the internal pressure necessary to cause failure in the tube by

$$p = \sigma_{failure} \frac{\left(b^2 - a^2\right)}{\left(b^2 + a^2\right)}$$

Where p = internal pressure, psi
$\sigma_{failure}$ = tangential failure strength, 3000 psi
a = inner radius, 120 inch
b = outer radius, 180 inch

The above equation produces an internal pressure of 1,154 psi.

If the same problem using the same inputs was solved by a numerical model, then the estimated pressure required to produce the kind of plastic failure in the shaft lining as shown in Figure 5 is equal to 1210 psi. This value differs from the 1154 psi theoretical value by less than 5%. Of course, by taking smaller size elements than those in Figure 5, the accuracy of the numerical solution can be improved.

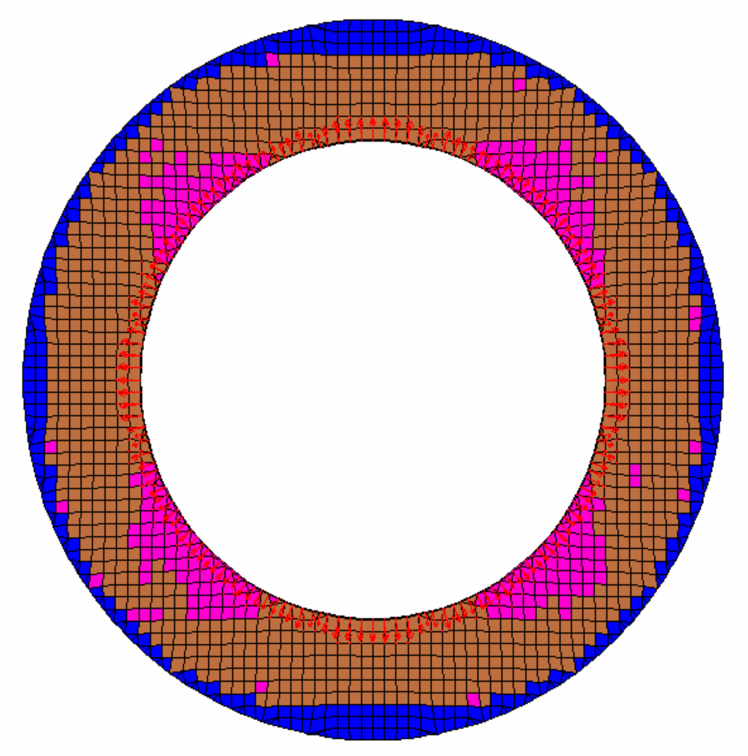

Figure 5. Plastic state noticed in the thick walled tube when 1210 psi static internal pressure was applied [blue – elatic; purple and brown – plastic state].

If the same numerical model is solved in full dynamic mode, then the required internal pressure to cause same level of failure in the shaft collar falls down to 825 psi. The plasticity state for this condition is shown in Figure 6. For this analysis, the pressure-time curve used was a suddenly applied pulse lasting over a total time period of one second.

If the tensile strength of the shaft lining is reduced to 2000 psi, then the required dynamic internal pressure falls to 625 psi. Similarly, when the tensile strength is 1000 psi, the failure pressure is only 325 psi. It must be noted that all these pressures are reflected dynamic pressures. The plastic states corresponding to the later two pressures are shown in Figure 7 and Figure 8.

Within a conceivable range of tensile strength values alone, the explosion pressure necessary to blow the shaft collar apart has shown such a drastic reduction by conducting proper dynamic analysis. We believe that if all the necessary details on the shaft construction and material properties were available and if a detailed three-dimensional dynamic structural analysis considering different dead loads acting on the shaft was conducted, the back calculated explosion pressure would have been substantially less than what was reported in the MSHA study.

It is really puzzling why the NIOSH report has given so much credibility to a very rudimentary and highly suspect structural analysis when there was virtually no input data available to provide any meaningful conclusions. Further, the MSHA report itself explicitly lists eight reasons which could substantially alter their conclusions [11]. This kind of misuse of data leads a casual reader to believe the quoted numbers were authentic.

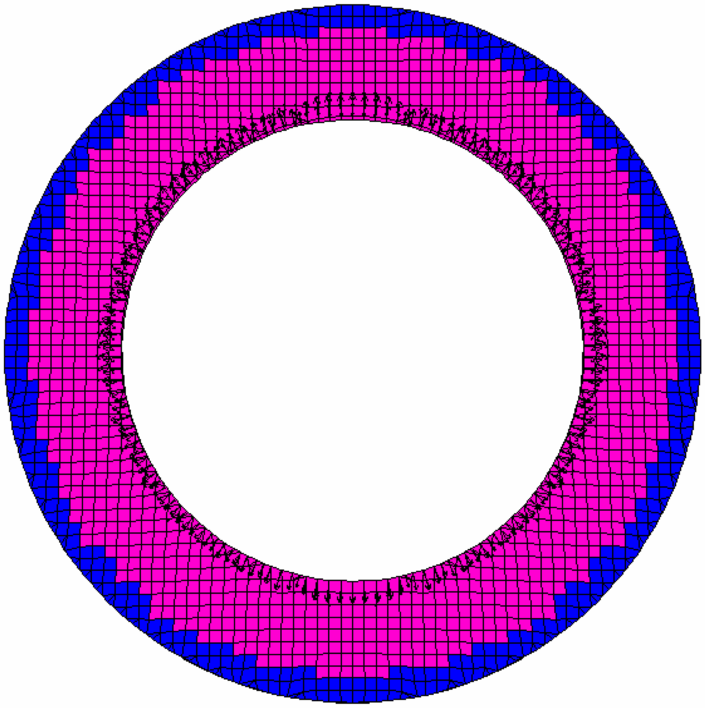

Figure 6. plastic state for a fully dynamic solution at 825 psi instantaneously applied internal pressure [blue – elatic; purple – plastic state].

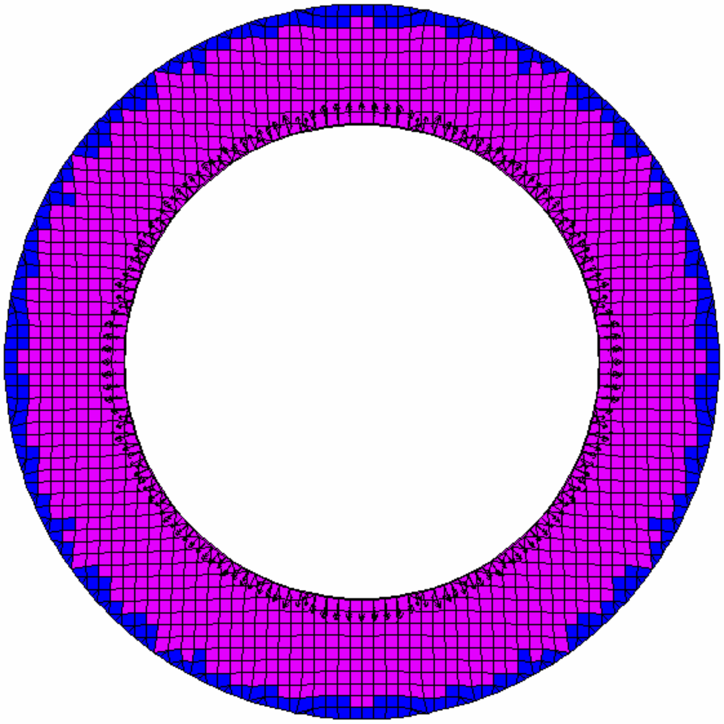

Figure 7. plastic state for a fully dynamic solution when the tensile strength was reduced to 2000 psi and at 625 psi instantaneously applied internal pressure [blue – elatic; purple – plastic state].

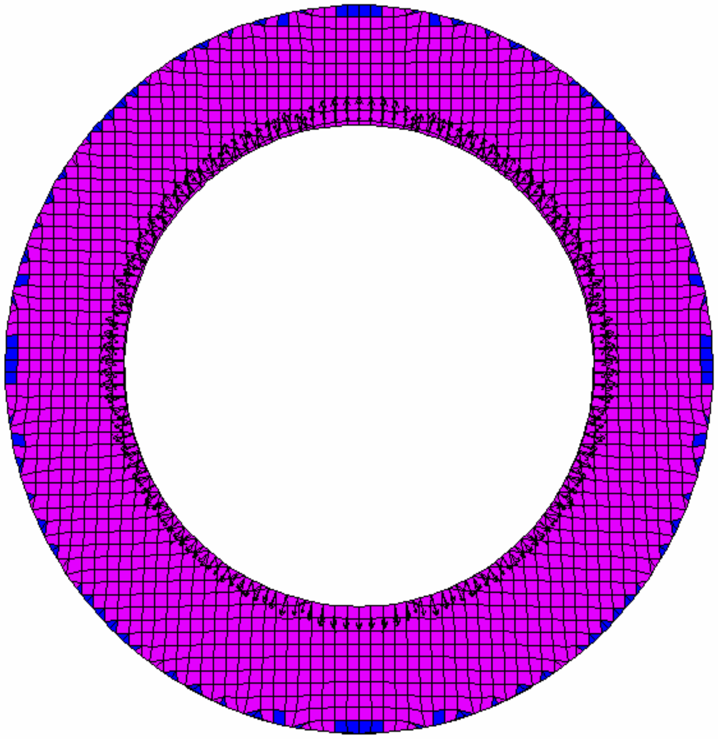

Figure 8. plastic state for a fully dynamic solution when the tensile strength was reduced to 1000 psi and at 325 psi instantaneously applied internal pressure [blue – elatic; purple – plastic state].

5.0 Conclusions

In general terms, the reviewers think the seal strength requirements given in the ETS are reasonable. However, for the reasons given in this report, we are not convinced there is enough evidence to support higher explosion pressures like those prescribed in the NIOSH report.

We urge all the researchers, policy makers and other interested parties engaged in this extraordinary effort to carefully read all the original references yourself before reaching a conclusion on the validity of the quoted experimental results – both small and big numbers. We also request you to carefully consider the assumptions, experimental set up, instrumentation and the analysis methods used in each of those tests to understand under what conditions those test results would be meaningful. It is only by such close scrutiny that one will be able to make an objective assessment of the situation. The same requirement extends to theoretical studies.

Our research on coal mine explosions clearly shows that the bulk of the pioneering studies were conducted on this deadly hazard long before 1975. The painstaking experiments that those great scientists conducted show their dedication toward unraveling the mysteries involved in mine explosions. The theoretical treatments that formed the backbone of the current NIOSH studies were available to those pioneers also. Yet, it is striking that those early experts did not rely solely on theories. Do we know more about the constant volume combustion, C-J detonation pressures or the reflected pressure equations now than our predecessors did? We don't think so. It is very important that we all think why the early researchers did not rely entirely on theoretical predictions and resorted to extremely difficult physical testing. We believe that was because the mine explosion pioneers realized the limitations of the theoretical predictions when applied to complex underground mine environments.

Why do we now traverse the path that our predecessors deemed inappropriate to address the same issues that existed in their times? We sincerely urge NIOSH and other researchers to focus on developing some real data first and then formulate valid theories to explain the reality. This is the backbone of the scientific process. It is not in the best interests of anyone in the mining community to force policy decisions based on some extremely simplistic and idealized theories.

As our analysis in this review shows, ignoring gas composition in abandoned workings is one fundamental discrepancy that could invalidate much of the available research on gas explosions. This is as basic as conducting strength tests on coal when the purpose is to design coal pillars. Of what value will it be if all the testing is done on limestone when the ultimate goal is to design coal pillars? While this analogy is a little exaggerated, it certainly serves to drive home the point we are trying to make.

Since we don't believe the NIOSH study has provided convincing answers to several challenging questions related to explosions in abandoned mine workings, we think it is prudent to include a provision in the final rule that allows MSHA to revisit the seal regulations within a reasonable time frame. At that point, based on the research that NIOSH plans on conducting in the next few years and any other research done by universities, coal companies and several others, it may be possible to formulate a policy that is based on real data. In the meantime, it is reassuring that the ETS will provide the highest level of protection to the U.S. coal miners than any other coal producing country in the world.

References

1. Department of Labor, Mine Safety and Health Administration, Sealing of Abandoned Areas; Final Rule. Published in the Federal Register, Vol. 72, No. 98, Tuesday, May 22, 2007.
2. Zipf, R.K., Sapko, M. J., and Brunde, J.F. *Explosion Pressure Design Criteria for New Seals in U.S. Coal Mines.* NIOSH Information Circular 9500, July 2007, 76 p.
3. Lide, D. R. Ed., *CRC Handbook of Chemistry and Physics*, 73rd Edition. CRC Press, Boca Raton, 1992.
4. McBride, B. J., and Gordon, S. Computer program for Calculation of Complex Chemical Equilibrium Compositions and Applications, NASA reference publication 1311, 1996.
5. Coward, H.F., and Jones, G.W. Limits of flammability of gases and vapors. U.S. Department of Interior, Bureau of Mines, Bulletin 503, 1952.
6. Zabetakis, M. G. Flammability Characteristics of Combustible Gases and Vapors. Department of Interior, Bureau of Mines, Bulletin 627, 1964.
7. Ramlu, M. Mine disasters and Mine Rescue. University Press, India, 2006, 460 p.

8. Nagy, J. The explosion hazard in mining. U.S. Department of Labor, Mine Safety and Health Administration, IR 1119, 1981.
9. Cybulski, W. B., Gruszka, J.H., and Krzystolik, P.A. Research on firedamp explosions in sealed-off roadways. Paper in the proceedings of the 12th International Conference on Mine-Safety Research Establishments, Germany, 1967, Vol. 1, paper 22.
10. Cybulski, W.B. Coal Dust Explosions and Their Suppression. Translated from Polish and published for the Bureau of Mines, U.S. Department of Interior and the National Science Foundation, Washington D.C., by the Foreign Scientific Publications Department of National Center for Scientific, Technical and Economic Information, Warsaw, Poland, 1975.
11. Sawyer, S. G. An Elementary Structural Failure Analysis of the Reinforced Concrete Shaft at the Blacksville No. 1 Mine. Report No. 03-206-92. Pittsburgh Safety & Health Tech. Ctr., Mine Safety and Health Administration.

Comments on the *NIOSH* Draft Report, "Explosion Pressure Design Criteria for New Seals in U.S. Coal Mines"

By

Murali M. Gadde,
David A. Beerbower,
John A. Rusnak,
Jay W. Honse,
Peabody Energy, St. Louis, MO.

1.0 Introduction

The National Institute for Occupational Safety and Health (*NIOSH*) has recently produced a draft report [1][*] on new seal design criteria for U.S. coal mines. *NIOSH* has requested public comments on the research presented in this report. The following sections in this review address some of the problem areas in the *NIOSH* report, which may require further research before finalizing the seal design criteria document.

First of all, the authors of the *NIOSH* report must be applauded for their effort to standardize the terminology used in connection with mine sealing process and related explosion events. This report is perhaps the first document that has made such an effort to address this long overdue requirement. The report also provides a nice summary of seal design practices in different major coal producing countries in the world, although South African experiences were not included. As is the case with mining practices, this *NIOSH* summary of worldwide experience also brings forth the contrasts in seal design practices that exist between the various countries.

The intention of the reviewers in providing the following commentary is to point out the deficiencies in the research presented in the *NIOSH*'s draft report. It is hoped that by addressing some of these problem areas, more realistic seal design criteria would be formed which will improve the safety of miners and promote the interests of the nation's coal mining community.

2.0 Review

In the following sections, a point-by-point approach is taken to bring out what the reviewers thought as problem areas that require further research and elaboration.

2.1 The Constant Volume Combustion

In deducing the pressures to be resisted by mine seals, the *NIOSH* report starts with the "worst-case" analysis of the methane-air deflagration. In the following,

---

[*] Numbers in the square brackets indicate the references given at the end.

the validity of some of the assumptions made in the constant volume explosion analysis is examined. The focus of this analysis is not to criticize the "worst-case" type analysis, but rather to highlight how unrealistic and improbable for some of the idealizations to be realized in a coal mine.

In estimating the constant volume explosion pressure, the following four key assumptions were made:

- the process was adiabatic,
- the gas mixture was homogeneous,
- the entry was completely filled with the gas mixture, and
- the methane content was at the stoichiometric level.

Let us examine each one of these assumptions in some details.

2.1.1 On the assumption of adiabatic combustion

An adiabatic process is one in which no heat is added to or extracted from the working fluid (methane-air mixture). While the assumption of adiabatic process greatly simplifies the calculations, one can easily see how unrealistic such an assumption is for coal mine strata, which have finite thermal conductivity properties. Considering the high thermal gradients created in the explosion process, it is physically impossible for heat not to transfer to the surroundings.

While the above arguments are common sense based, experimental data on gas explosions in closed vessels even under controlled conditions also disprove the adiabatic assumption. For instance, even in the first ever measurement of gaseous explosion pressure by Hirn in 1861, the noted differences between the values estimated based on adiabatic assumptions and measured values were ascribed to heat loss from the vessels [2]. To quote Bone and Townend [3], who discussed Hirn's experiments, *"He [Hirn] attributed the low pressures attained in such explosions to the fact that the metal sides of the explosion vessels were at so low a temperature compared with that of the explosion itself that the heat was rapidly conducted away."* In terms of heat conduction, coal mine entries are similar to the closed vessels used in Hirn's experiments.

To quote some recent data, Razus et.al.[4] have compiled constant volume methane-air explosion pressures measured at ambient temperature and pressure at nearly stoichiometric concentrations from ten different research works. These absolute pressures varied from 700 Kpa to 870 Kpa, much less than that estimated by the *NIOSH* report.

2.1.2 On the homogeneous gas mixture filling the entire coal mine entry

The density of methane is about 0.55 times that of air [5, 6]. As a result, there is a natural buoyancy effect that causes methane to accumulate more at the roof level, which is familiar to miners as 'methane layering.' In order to create a homogeneous mix, there would need to be sufficient ventilation air to disperse the methane layers [5, 6]. In a sealed-off panel, no circulating air is available to disperse and create a homogeneous methane-air mix. The buoyancy effect of methane also adds to the variability of the methane concentrations from roof to floor in an entry [5, 6]. Due to these well known processes, it is very unlikely to find a homogeneous methane-air mix that completely fills a coal mine entry from roof-to-floor and rib-to-rib in an abandoned panel.

In fact, as the constant volume calculations show, if the filling ratio (ratio of the area occupied by methane-air mix to the entry cross-sectional area) is less than 1.0, then the constant volume pressure is reduced by that extent. For example, if the filling ratio is 0.8, then the explosion overpressure becomes 20% less than that predicted assuming complete filling.

Also, due to the turbulent currents created by the explosion, rock and coal dust mix with the reactants to further reduce their homogeneity, if it exists.

2.1.3 On the Existence of "Normal" Stoichiometric Methane Air Mix in Abandoned Panels

In a regular mine atmosphere, if the methane concentration by volume is between 5% and 15%, the mixture is considered explosive. If the methane content is around 10%, the composition is considered stoichiometric. In the sealed-off area, however, due to the lack of any mechanical ventilation, the air composition changes for several reasons. This fact is very well known to coal miners and is demonstrated very nicely with actual data obtained at a South African coal mine through remote monitoring using the "tube and bundle" system [7]. The data shown in Figure 1 was obtained from an abandoned room and pillar mine, which has not been retreat mined. Curves in Figure 1 show how the percent of oxygen in the gob air decreased over time while the methane content progressively increased before stabilizing, over the same time period.

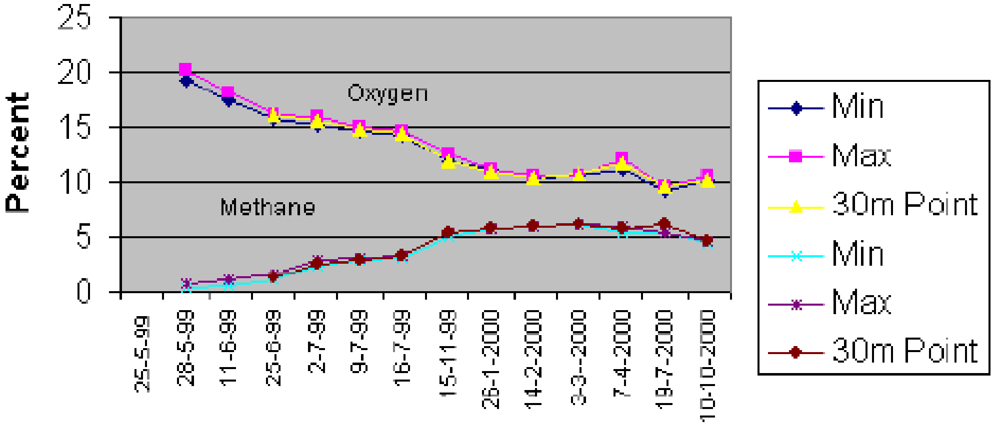

Figure 1. Change in the oxygen and methane content in gob air over time [7].

In the early 1900s, R. V. Wheeler and his colleagues working for the Safety of Mines Committee in U.K have shown that decreasing oxygen content in air has a drastic effect on the upper explosive limit (UEL) of methane while the lower limit is not affected to the same extent. Referring to Wheeler's work, Bone and Townend in their book on 'Flame and Combustion in Gasses' [3] have given the data in Table 1 below to show the effect of decreasing oxygen content on methane explosibility.

Table 1. The effect of decreasing oxygen content on methane explosibility [3].

Atmosphere		Percent Methane at the	
Oxygen percent	Nitrogen percent	Lower Explosive Limit	Upper Explosive Limit
20.90	79.10	5.60	14.82
17.00	83.00	5.80	10.55
15.82	84.18	5.83	8.96
14.86	85.14	6.15	8.36
13.90	86.10	6.35	7.26
13.45	86.55	6.50	6.70
13.25	86.75	No mixture capable of propagating flame at atmospheric temperature and pressure.	

Data in Figure 1 and Table 1, clearly show that what constitutes a stoichiometric methane-air mix in normal mine air is not the same in an abandoned mine working. Also, if the oxygen content falls below a certain limit, methane explosions will not even occur. Of course, in the Wheeler's experiments, the decreasing oxygen content was compensated for by an increasing percentage of inert Nitrogen gas, which may be the reason why no methane explosion was noticed at the lowest tested oxygen content.

Bone and Townend [3] also refer to a 1926 Safety of Mines Research Board paper in which the effect of black-damp (a mixture of nitrogen and carbon dioxide) on methane inflammability was studied. This study showed the higher extinctive power of carbon dioxide over nitrogen.

In an abandoned coal mine panel, the depleting oxygen content is most likely not due to an increase in the nitrogen but due to other gases like carbon dioxide, carbon monoxide and others. As a result, in an abandoned panel, the explosiveness of methane may be affected at least as much or even more than that indicated in Table 1. Despite this difference, Wheeler's data is very illuminating in pointing out the significant differences that might exist between methane explosiveness in 'normal' mine air and in sealed-off areas.

If the stoichiometric mix in a gob is not the 10% noticed in the normal mine air, then the final temperature of the combustion products calculated for a constant volume explosion may not hold good and thus the predicted explosion pressures may not be correct.

2.1.4 Some General Remarks on the Constant Volume Combustion

In addition to the above major assumptions, some more idealizations are inherent in the constant volume combustion process, which include a complete combustion of the gaseous reactants and no dependence of specific heats on the temperature [3]. Indeed, it was the departure from all the foregoing assumptions that was responsible for the measured **lower pressures** in the constant volume explosion experiments compared to their corresponding theoretical estimates [3].

Additionally, the strata surrounding a coal mine entry consists of a number of minor and major discontinuities, which include cleats, bedding planes, joints etc. When an explosion occurs, owing to the high pressures developed, some of the combustion products may find their way through these fractures thus reducing the estimated overpressure further. Although, as far as the reviewers are aware, no real mine-scale explosion studies were conducted under ideal constant volume conditions, for all the above reasons, it is probably a more realistic estimate that the actual deflagration overpressures will be in the range of 60-70% of the theoretical maximum value given in the *NIOSH* report.

2.2 Deflagration-to-Detonation Transition (DDT), CJ Detonation Pressure and Reflected Pressure

2.2.1 On DDT

The selection of 50 m (150 ft) as the run-up distance required for the DDT event to occur is one aspect of the *NIOSH* report that has very little scientific basis. In the prelude to suggesting this DDT distance, the authors quote some research, which suggest a run-up distance of 50 to 100 times the pipe diameter. Similar run-up length to diameter ratios were also reported by Lewis and von Elbe [8]. In adopting these numbers for coal mine methane-air explosions, one must be aware that **a majority of the research on gas explosions was conducted on gas mixtures that are many times more reactive than methane-air mixtures**. In fact, the amount of research done on methane-air DDT process is negligible compared to other gases. Therefore, the data produced for highly reactive gases should not be applied for methane-air mix.

Research on DDT shows that the processes involved are not totally understood and thus there are many uncertainties associated with its prediction. A nice review of DDT research is contained in a recent paper by Oran and Gamezo [9]. Also, Li et.al. [10] have pointed out that even in controlled laboratory experiments, determining DDT is fraught with uncertainty due to several factors, the main emphasis being on identifying the DDT event itself.

The *NIOSH* report discusses the effect of detonation cell size on DDT and based on a λ value of 30 cm and a D/λ ratio of 5 concludes that DDT event may occur if the minimum size of a tunnel exceeds 1.5 m (5ft). While that is a possibility, research by Kuznetsov et. al. [11] shows that DDT event is correlated not only with the size of the detonation cell but also whether the cell is regular or irregular. Further, some other laboratory studies showed the dependence of DDT on the scale of the experiment [12].

The limited research done on DDT in methane-air mix shows that it is very difficult to create detonations in such "low" reacting mixtures. To quote Kuznetsov et.al. [13], *'Studies performed in tubes have shown that it is very difficult to achieve deflagration-to-detonation transition (DDT) in methane-air mixtures. The only reported observation of DDT in a methane-air mixture was by Lindsted and Michels (1989) in a very long tube equipped with a "Schelkin spiral".'* The conclusion being that laboratory experiments on methane-air mixtures could not produce DDT unless a large amount of blockage is included. In fact, Kuznetsov et.al's [13] research shows that at a blockage ratio of 0.3, although they considered the event as detonation, the detonation velocity measured was much less than the ideal Chapman-Jouguet (CJ) detonation velocity. Further, at a blockage ratio of 0.6, they could not observe any DDT [13]. Based on these experiments, Kuznetsov et.al. classified two types of steady-state explosions in methane-air, "quasi-detonation" and "Choking" detonation. In the first case, the measured flame speed was slightly less than CJ velocity, where as in the later, it is about half the ideal velocity [13].

In summary, DDT is a very complex process and the available data for methane-air mixtures is very limited. Therefore, more studies are required before formulating criteria to determine DDT in a real mine situation. Such a research study may start with laboratory experiments using **a network of rectangular pipes** – not the single circular tube used in gas explosion studies – with **different configurations of ignition points**. Data generated from such controlled studies may be used in conjunction with numerical gas explosion modeling to predict DDT on a coal mine scale. Although a majority of the available commercial codes still do not simulate DDT and detonation events well, recent research [9,14,15] on the subject shows promise.

2.2.2 On CJ Detonation Pressure

Assuming that detonation of methane-air mixture has occurred, the *NIOSH* report uses ideal Chapman-Jouguet (CJ) equation to estimate the resulting detonation pressure. The equation given in the *NIOSH* report is

$$\frac{P_2}{P_1} = 1 + \frac{\gamma_1}{(1+\gamma_2)}\left(\frac{D}{c_1}\right)^2 \qquad (1)$$

Where P_1 and P_2 are the pressure ahead and behind the detonation wave; γ_1 and γ_2 are the specific heat ratios of reactants and products, respectively; c_1 is the sound speed, and D is the detonation wave speed. Based on equation (1), a pressure ratio of 17.37 was computed.

Although no mention was made about the source of equation (1) in the *NIOSH* report, the reviewers found two more versions of the same equation. A 1968 USBM Report of Investigations [16] gives the equation as

$$\frac{P_2}{P_1} = \frac{1+\gamma_1\left(\frac{D}{c_1}\right)^2}{(1+\gamma_2)} \qquad (2)$$

For the same inputs as in the *NIOSH* report, equation (2) gives a pressure ratio of 16.8.

One more version of CJ detonation pressure equation was found in Landau and Lifshitz [17]. While the *USBM* version in equation (2) was not derived from the first principles, Landau and Lifshitz [17] give a step-by-step derivation of the equation. Their version reads as

$$\frac{P_2}{P_1} = \frac{\gamma_1}{(1+\gamma_2)}\left(\frac{D}{c_1}\right)^2 \qquad (3)$$

Again, for the same *NIOSH* report inputs, equation (3) gives a pressure ratio of 16.37.

Of course, the results produced by equations (1) to (3) differ only slightly. But, when calculating the reflected pressures, such small error could also be important. For instance, if equation (3) is used to calculate the CJ detonation pressure and if the reflected pressure equation given in the *NIOSH* report is used, then the peak overpressure will fall from 649 psi (this number is given as 653psi in the *NIOSH* report due to round-off errors) to 611 psi.

While the above discussion highlights the uncertainty about the form of the CJ detonation pressure equation, there are more problems with the assumption of the CJ conditions itself. A very enlightening review of the Chapman-Jouguet hypothesis is presented by Cheret [18], in which he states, "*…thus the idea of Chapman-Jouget 'law' flourishes as a belief that the work by the first as well as by the second guarantees some well-established law, according to which one and the same detonation velocity exists:* D_*." He further states that, "*Actually, such a gap between experimental results and the Chapman-Jouget 'law' is not unknown even by the most enthusiastic users, and does not leave them unconcerned. But, surprisingly enough, none of them goes back to the origins; all* *of them have chosen an escape way and state that, instead of being 'completely' true, the Chapman-Jouguet 'law' is 'asymptotically' true, i.e. when the flow is plane and steady."*

The point of the above discussion is that while the CJ detonation assumption may be used as a first approximation and is in tune with *NIOSH*'s over all "worst-case" analysis approach, the predicted pressures may not be accurate as the CJ hypothesis is not an irrefutable 'law' like Newton's laws. This is an aspect that requires attention in the future studies.

2.2.3 On the Reflected Pressure

There is a minor point to be made on the reflected overpressure computed in the *NIOSH* report. The equation used to compute the reflected pressure is given in the report as

$$\frac{P_R}{P_I} = \frac{5\gamma + 1 + \sqrt{17\gamma^2 + 2\gamma + 1}}{4\gamma} \qquad (4)$$

But, in the reference given in the *NIOSH* report for equation (4), the equation reads as

$$\frac{P_R}{P_I} = \frac{5\gamma + 1 + \sqrt{17\gamma^2 + 3\gamma + 1}}{4\gamma} \qquad (5)$$

Equation (5) is given in an example problem in Landau and Lifshitz's book [17]. If the subordinate equations used to derive (5) are solved, the correct solution is obtained as equation (4), which is what was given in the *NIOSH* report. A careful reading, however, shows that the subordinate equations used in the derivation of equation (5) or (4) may themselves be questioned, if the mechanics of the 'shock adiabatic' are properly followed. But in a paper by Shepherd et. al. [19], the derivation of equation (5) was credited to Zel'dovich and Stanyukovich, who derived the equation in 1948 long before the publication of Landau and Lifshitz's book. Since Shepherd et.al. reproduced equation (5) from the original authors' paper, it may perhaps be used in the *NIOSH* final report instead of equation (4).

Of course, the above discussion on the reflected pressures is only to be "technically correct." Otherwise, the difference in the pressure ratios estimated by equation (4) and (5) is unnoticeable.

Finally, in all the theoretical explosion pressure calculations, the *NIOSH* report did not explain how the inputs were estimated. This applies to final temperature of the combustion products (T_f), detonation velocity (D) and ratio of specific heats (γ_1 and γ_2). It will be useful if *NIOSH* includes references and complete calculations for the estimation of these numbers in order to allow for further and complete review.

2.3 Experimental Mine Explosion Studies

In the *NIOSH* report, much has been said about the Polish large-scale mine experiments conducted by Cybulski *et.al.*[20] in 1967. Drawing from these experiments, the *NIOSH* report says, "*Two tests in which the explosive mix completely filled the tunnel produced peak pressures greater than 3.2 MPa (450 psi).*" A careful reading of the Cybulski's paper shows that the data has been taken out of context. Let us reproduce some facts from the Cybulski's paper:

- "*The tunnel was narrowed down at the site of the blasting bulkhead.*"
- "*The ground support consisted of TH-rings lagged with reinforced concrete staves.*" (Examining the pictures of the tunnel in the paper shows that the tunnel is supported by arches, which creates a significantly higher blockage in the entry compared to a typical coal mine in the U.S.)
- Most importantly, the type of ignition used was, "*…three electric caps the shells of which were topped with fuse powder, were attached to the face of the rock tunnel at half height. A charge of 100 gram black powder and 3 strong electric blasting caps was used in all of the future experiments.*"
- In fact, in all the "Barbara" mine experiments, some kind of solid explosives were used as the ignition source.
- Finally, the reported pressures in the paper were basically the reflected pressures at the bulkhead location.

Let us combine the above experimental conditions with the following observations on DDT reproduced from Vasil'ev's paper[21] :

"*According to the current classification, a combustible mixture can be excited by three basic methods [1]:*

- *weak initiation (ignition) where only laminar burning is excited with velocities at the level of tens of centimeters per second;*
- *strong (direct) initiation where a self-sustained detonation wave (DW) is formed in the immediate vicinity of the initiator and then propagates over the mixture with a velocity at the level of several kilometers per second;*
- *intermediate case where the mixture is ignited at the initial stage and then the flame is accelerated owing to natural or artificial reasons up to (visible) velocities at the level of a hundred meters per second. Under certain conditions, even the deflagration-to-detonation transition can occur.*"

So, what essentially happened in the Polish experiments is the second condition described by Vasil'ev, whereas the real coal mine situation correspond to the third one. In other words, the Polish explosions, except probably for one test, were started as detonations or weak detonation waves, which were further reinforced by the turbulence created due to arch supports and finally culminating as a very strong detonation due to a sudden cross-section reduction near the bulkhead. Therefore, those results are not directly applicable to coal mine explosions. Even considering such worst-case conditions simulated in the Polish experiments, the measured reflected overpressures were in the range of 450psig as compared to the *NIOSH* 's recommended 640psig. Of course, in estimating the run-up distance for DDT, the Polish data is not useful at all.

Finally, in the Polish studies, the flame speed was measured as 1200 m/s. However, as the *NIOSH* report quotes, the CJ detonation velocity in Methane-air mixtures should be around 1800 m/s. This discrepancy suggests one of the two possibilities: either there was no detonation event or if there was a detonation, which is more likely for the reasons given above, then the conditions for applying CJ hypothesis did not exist.

2.4 Numerical Modeling of Gaseous Explosions

The *NIOSH* report deserves a lot of credit for initiating the numerical computational fluid dynamics (CFD) modeling as a means to study coal mine gas explosions. As Gadde *et.al.*[22] point out, despite the limitations, numerical modeling seems to be the best available tool for conducting gas explosion studies in coal mines. They also listed two major sources of errors that could cause problems with CFD modeling, namely, the selection of parameters for combustion and turbulence models [22]. The *NIOSH* report has made an attempt to 'calibrate' the inputs required for two commercial CFD packages with some success. These efforts must be continued as that is probably the best available course to address several challenges in predicting methane-air explosion output.

Even though the *NIOSH* report claims a good match between the modeled and measured pressure-time curves from Lake Lynn, major differences could be seen from the output given in the *NIOSH* report. Interestingly, the output from the two codes, AutoReagas and FLACS used in the report, also show noticeable differences. One reason for the discrepancy is probably the use of two different combustion models in these two codes. While the initial CFD modeling results in the *NIOSH* report are praiseworthy, they also reveal how much more work remains to be done in this direction before confident gas explosion output prediction is possible.

2.5 Design Pressure-time Curves

After the initial rise or fall period and attaining the constant volume pressures, the suggested pressure-time curves in the *NIOSH* report impose very high impulses on mine seals. As far as the reviewers could see, the only supporting statement for the long plateau potion of the pressure-time curves as given in the NIOSH report is, "*Several computed pressure-time histories from the large gas explosion models indicate that the initial pressure peaks equilibrate to the 800 kPa (120 psi) constant volume explosion overpressure after 0.1 second.*" From this and other statements in the *NIOSH* report, it seems that the numerical CFD modeling formed the basis for deriving the suggested design pressure-time curves.

Again, the suggested pressure-time curves represent the "worst-case" loading possible. Since in the CFD modeling in the *NIOSH* report, the process has been assumed adiabatic, the constant volume explosion overpressure will be maintained until the combustion products find someway to expand and cool. For the same reasons given in section 2.1 of this review, the validity of the *NIOSH* suggested pressure-time curves is also questionable.

2.6 Structural Seal Design

The structural analysis in the NIOSH report is extremely simplistic and hence may not be useful for practical designs. With the state-of-the art dynamic modeling tools available, it is not necessary to make so many unrealistic assumptions to solve the problem. For example, Gadde *et.al.* [22] have provided a comprehensive structural analysis approach for mine seals using three-dimensional numerical modeling. In the modeling approach, there is very little need to make many simplifying assumptions to find the solution. Nevertheless, in connection with the *NIOSH*'s structural analysis, some comments are in the order:

- The WAC code used is a single-degree-of-freedom (SDOF) analysis tool. Hence, it suffers with the limitations pointed out by Gadde *et.al.*[22];
- Since WAC is not a public domain code or is not available for purchase for a regular user, it is difficult to design seals using this approach for situations not covered in the *NIOSH* report;
- The one-way analysis used is conservative. Moreover, the rigid roof-seal and floor-seal links do not exist in a real coal mine unless the seal is 'hitched' to sufficient depth;
- The way the safety factor was implemented in the WAC analysis is not clearly explained. It is not immediately clear how a $\sqrt{2}$ scaling will double the applied load on the structure;
- The quasi-static analysis using 'plug' and 'arching' actions are unrealistic;
- The steel-bar reinforcement analysis is simplistic, conservative and is not based on seal deformation mechanics.

Finally, the recommended design safety factor value of 2.0 in the NIOSH report has very little basis. Similar safety factor value was recommended by Gadde *et.al* [22] but that was only to account for the uncertainty in the estimated pressure-time curves used in their analysis. If the NIOSH report has already undertaken a “worst-case” type approach to estimate the pressure-time curves, what is the need to have such a high safety factor as 2.0? Since the probability of having any pressures higher than suggested in the NIOSH report are extremely low, the only uncertainty to account for in the design is limited to the construction of the seal itself. For the latter purpose alone, there is no need to have a very high safety factor. Indeed, the “worst-case” pressure-time curves and a very conservative safety factor requirement will only make the built seals impractical.

3.0 A Proposition

Considering the importance of the *NIOSH’*s work and the possible implications for future seal designs in this country, the reviewers propose the following approach to answer several outstanding issues:

- A large number of studies have already been conducted on the methane liberation potential of many coalbeds in this country. It will be more realistic if the explosive potential is classified based on the methane liberation rates. Also, probabilistic modeling and risk analysis must be conducted for different panel and gob configurations while accounting for the methane liberation and other thermal properties of the surrounding strata. Such studies will help estimate the likelihood of an explosion and the resulting overpressures.
- As a part of the broader research study, systematic data acquisition must be done on the atmosphere in the abandoned panels for a select number of mines with different methane liberation properties to answer such questions as the filling ratio, homogeneity of the mixture, methane layering and change in the composition of the mine atmosphere.
- Laboratory studies must be conducted to study the influence of the change in the composition of mine atmosphere, particularly the effect of decreasing oxygen and increasing carbon-dioxide, on the explosibility of methane.
- Just after the initiation of an explosion, turbulent currents are set in motion which will make the rock and coal dust mix with the gob atmosphere. Some lab studies may also be conducted on the effect of different levels of coal and rock dust in the gob atmosphere on methane explosibility.
- Experimental studies may also be conducted using rectangular pipes for estimating the deflagration-to-detonation transition distances. Also, such studies may be conducted on a network of interconnected pipes instead of a single pipe, which has been used in the gas explosion studies. Such lab studies may also focus on the effect of different locations of the ignition sources.
- Since the adiabatic combustion is an extremely unrealistic model, thermal modeling of the coal mine strata must be undertaken to assess

the processes involved in the heat loss and its effect on the explosion pressures.

- CFD gas explosion modeling may be continued with an emphasis on conducting several parametric studies to predict the changes in explosion output for different panel and gob configurations, ignition location and several other variables identified, for example, by Gadde *et.al.*[22].
- A reasonable seal design criteria must be established considering the confidence in the estimates of the explosion overpressures and the quality control issues related to seal construction underground.
- Finally, the structural design of seal should be based on more realistic modeling of the real-world conditions. Based on the results of such an analysis, it may be necessary to explore the need for a multi-tiered seal design criteria based on the intended service and longevity of the seal installation.

4.0 Conclusions

- Anyone involved with research and writing papers would know it is a lot easier to "critically review" somebody else's work than to come up with a "perfect" research work of his or her own. The reviewers are very well aware of this. So, the comments in this review should not be seen as an attack on some work; rather, they should be viewed in the right spirit to spur further research before producing a document that might very well be the basis for formulating new statutory seal design guidelines.
- Admittedly, in a real coal mine situation, the possible combinations of situations that require analysis for explosiveness are too many to be included in a project that has strict time constraints. Also, there are uncertainties associated with obtaining some of the inputs needed for any reliable analysis. Therefore, it is perhaps acceptable to conduct a 'worst case' type analysis as an interim solution. But, for the reasons given in this review, even the worst-case type study should not make extreme assumptions that are unrealistic, have extremely low probability of occurrence or are not supported by real-world mechanics.
- The work presented in the *NIOSH* report is a good start and may form the basis for thorough research in the future to address several of the outstanding issues before formulating final seal design criteria.
- If the 20 psi peak overpressure criterion is criticized for its lack of sound scientific basis, then the results presented in the reviewed *NIOSH* report will also receive similar criticism, if the deficiencies cited here are not addressed and the objectives identified in section 7.3 of the *NIOSH* report are not achieved. It will be premature for the *NIOSH* report in its current form to be the basis for formulating final statutory seal design criteria.

5.0 References

1. Zipf, R.K., Sapko, M.J., Brune, J.F. Explosion Pressure Design Criteria for New Seals in U.S. Coal Mines. NIOSH draft report, 2007, 80p.
2. Hirn. *Zur Theorie der Lenoirschen Polytechnische Central-Blatt Gasmachinen*, Leipsic, 1861.
3. Bone, W. A., Townend, D. T. *Flame and Combustion in Gases,* London: Longmans, Green and Co. Ltd., 1927, 548p.
4. Razus, D., Movileanu, C., Brinzea, V., Oancea, D. Explosion pressures of hydrocarbon-air mixtures in closed vessels. *J. Hazardous Materials*, B135, 2006, pp. 58-65.
5. Creedy, D.P., Phillips, H.R. Methane Layering in Bord and Pillar Workings. Final Poject Report, COL 409. Braamfontein, Republic of South Africa: Safety in Mines Research Advisory Committee (SIMRAC), 1997, 97p.
6. Kissell, F. N. *Handbook for Methane Control in Mining*. NIOSH, IC 9486, 2006, 184p.
7. Hardman, D. R., Creedy, D.P., Phillips, H.R. Strategies to monitor non-homogeneous atmospheres in sealed off panels in coal mines. Draft Project Report, project COL 602. Braamfontein, Republic of South Africa: Safety in Mines Research Advisory Committee (SIMRAC), 2001, 31p.
8. Lewis, B., von Elbe, G. *Combustion, Flames and Explosions of Gases*, London: Cambridge University Press, 1938,415 p.
9. Oran, E.S., Gamezo, V.N. Origins of the deflagration-to-detonation transition in gas-phase combustion. *Combustion and Flame*, 148, 2007, pp. 4-47.
10. Li, J., Lai, W.H., Chung, K., Lu, F.K. Uncertainty analysis of deflagration-to-detonation run-up distance. *Shock Waves*, 14, 2005, pp. 413-420.
11. Kuznetsov, M.S., Alekseev, V.I., Dorofeev, S.B. Comparison of critical conditions for DDT in regular and irregular cellular detonation systems. *Shock Waves*, 10, 2000, pp. 217-223.
12. Dorofeev, S.B., Sidorov, V.P., Kuznetsov, M.S., Matsukov, I.D., Alekseev, V.I. Effect of scale on the onset of detonations. *Shock Waves*, 10, 2000, pp. 137-149.
13. Kuznetsov, M., Ciccarelli, G., Dorofeev, S., Alekseev, V., Yankin, Yu, Kim, T.H. DDT in methane-air mixtures. *Shock Waves*, 12, 2002, p 215-220.
14. Tegner, J., Sjorgreen, B. Numerical investigation of deflagration to detonation transitions. *Combust. Sci. and Tech.*, 174(8), 2002, pp.153-186.
15. Parra-Santos, M.T., Castro-Ruiz, F., Mendez-Bueno, C. Numerical Simulation of the Deflagration to Detonation Transition. *Combustion, Explosion, and Shock Waves*, 41(2), 2005, pp. 215-222.
16. Burgess, D.S., Murphy, J.N., and Hanna, N.E. Large-scale studies of gas detonations. USBM Report of Investigation 7196, 1968, 53p.
17. Landau, L., Lifshitz, E.M. *Fluid Mechanics*, Oxford:Pergamon Press, 1959, 536p.

18. Cheret, R. Chapman-Jouget hypothesis 1899-1999: One century between myth and reality. *Shock Waves*, 9, 1999, pp. 295-299.
19. Shepherd, J.E., Teodorcyzk, A., Knystautas, R., Lee, J.H. Shock Waves Produced by Reflected Detonations. Progress in Astronautics and Aeronautics, 1991, 134, pp. 244-264.
20. Cybulski, W.B., Gruszka, J.H., Krzystolik, P.A. Research on firedamp explosions in sealed-off roadways. 12th International conference of mine-safety research establishments, Dortmund, Germany, 1967.
21. Vasil'ev, A.A. Estimation of Critical Conditions for the Detonation-to-Deflagration Transition. *Combustion, Explosion, and Shock Waves*, 42(2), 2006, pp. 205-209.
22. Gadde, M.M., Beerbower, D., Rusnak, J., Honse, J., Worley, P. Post-PIB P06-16: How to Design Alternative Mine Seals? Paper presented at the SME annual meeting, Denver, CO, SME Pre-print No. 07-138, 2007.