TRANSCRIPT OF PROCEEDINGS

In the Matter of:)
)
PUBLIC HEARING ON EMERGENCY)
TEMPORARY STANDARD - SEALING)
OF ABANDONED AREAS)

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U.S. DEPARTMENT OF LABOR

MINE SAFETY AND HEALTH ADMINISTRATION

In the Matter of:)
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TEMPORARY STANDARD - SEALING)
OF ABANDONED AREAS)

Tuesday,
January 15, 2008

Conference Room G, 25th Floor
1100 Wilson Boulevard
Arlington, Virginia

The meeting in the above-entitled matter was convened, pursuant to Notice, at 9:02 a.m.

BEFORE: PATRICIA W. SILVEY
Moderator

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P R O C E E D I N G S

(9:02 a.m.)

MS. SILVEY: Good morning. My name is Patricia W. Silvey. I'm the director of the Office of Standards, Regulations, and Variances for the Mine Safety and Health Administration, U.S. Department of Labor. I will be the moderator of this public hearing today on MSHA's proposed emergency temporary standard -- I will later refer to it as "ETS" -- on sealing abandoned areas and underground coal mines.

On behalf of Richard E. Stickler, the acting assistant secretary of labor for MSHA, I want to welcome all of you here today.

The members of the panel are, to my right, John Urosek, who is from MSHA's Pittsburgh Safety and Health Technology Center; to his right, William Bachman, who is from my office; to my left, Deborah Green, who is our counsel on the project; to her left, Eric Sherer, who is from MSHA's Office of Coal Mine Health and Safety; and to his left, Ronald Ford, and Ron is the economist from my office.

MSHA published the ETS in response to the grave danger that miners face when underground seals separating abandoned areas from active workings fail. Seal failures at the Sago Mine and the Darby No. 1

Mine in 2006 raised MSHA's awareness of the problems with the construction and design of alternative seals.

MSHA investigated these and other failures of alternative seals and conducted in-mine evaluations of these seals. MSHA also reviewed the history of seals in the United States and other countries and the NIOSH draft report, Explosion Pressure Design Criteria for New Seals in U.S. Coal Mines, which was published on February 8, 2007, and finalized in July of '07. The report made recommendations for seal design criteria which would reduce the risk of seal failure due to explosions in abandoned areas of underground coal mines.

The purpose of this hearing, and MSHA did note, in the Federal Register notice reopening the comment period, that it was a limited reopening of the comment period. So, to that extent, the purpose of this hearing is to provide the public with an opportunity to testify on the U.S. Army Corps of Engineers Draft Report, "CFD [Computational Fluid Dynamics] Study and Structural Analysis of the Sago Mine Accident," and I will refer to that as the "report."

The agency posted the report on its website on December 7, 2007. The report summarizes the preliminary results of a study performed on the contract for MSHA's Technical Support Directorate by the U.S. Army Corps of Engineers. The Army Corps conducted research from August 2006 to April 2007 and provided a draft report of their findings to Technical Support in May of '07.

The report details the Army Corps' efforts mathematically model the methane explosion at the Sago Mine in January of '06 and potentially establish the seal overpressures. The report was not finalized.

I would like speakers to focus on the report, as it relates to the ETS. Please be as specific as possible with respect to the impact on the ETS and also on miner health and safety, mining conditions, and the technological and economic feasibility of your recommendations, if you have any. MSHA will consider your testimony to evaluate the requirements in the ETS and develop a final rule that protects miners from hazards associated with sealing of abandoned areas.

The format of the public hearing, for those of you who have participated with us in these public hearings before, you know that you are well aware of the format and are familiar with our format, but it is as follows:

The hearing will be conducted in an informal manner, and formal rules of evidence will not apply, as the rules of cross-examination do not apply. Presentations may be limited to 20 minutes, at the discretion of the moderator. The panel may ask questions of the witnesses, and the witnesses may ask questions of the panel.

Those of you who have notified MSHA in advance will speak first, and then others can make presentations, as necessary.

If you wish to present written comments or information today, please clearly identify your material. In addition, I would like to remind everyone that MSHA will accept written comments and other appropriate data from any interested party, including those not presenting testimony at this hearing. To be accepted, comments must be received by midnight, January 18, 2008. That's midnight, Eastern Standard Time. So that there is no confusion, for some of you, you've noticed that we have been delineating the time zones with which we want to receive comments also in the Federal Register.

MSHA will post the transcript from this hearing on the agency's website in approximately one week.

Now, we will begin with persons who requested to speak. Please begin by clearly stating your name and organization, to make certain we obtain an accurate record when you speak. And, please, if you would, spell your name for the reporter.

Our first speaker today will be Murali Gadde, and he is representing Peabody. I hope I pronounced it right.

MR. GADDE: You got it right. It's Murali Gadde. I work for Peabody Energy in the St. Louis office in Missouri.

First off, I would like to thank MSHA for this opportunity to express our views on the subject, which is real important for all of us, and it's getting really complicated.

First of all, I'd like to say a few words about the Corps' report that they recently submitted on the CFD modeling and structural analysis.

I have been a numerical modeler for almost 20 years now, so I have done a lot of structural modeling myself and have a good theoretical and economic background.

Overall, I think the Corps' report is a very interesting study. It shows that there are a lot of opportunities for us to use computation and fluid dynamics for our mine explosion studies. The value of the Corps' work, as I see it, at this point in time, is really academic. The reason I say it is because -- I will go into details as I speak in a few minutes, but the inputs that were used in the Corps' report were really the most ideal conditions.

As a part of our research, we connected Peabody into the explosions and seal design. We could, with the help of three other major U.S. coal producers, we could put together 15,700 data points from the actual gobs. They took the seal samples of the gob, and we have 15,700 points from 17 coal mines spread across the country. It represents all of the major coal fields.

What this database can show is the nine-and-a-half-percent methane that's the stoichiometric mix; the probability of finding it is simply zero. We have not found a single sample that came close to this stoichiometric mix. Also, in the Corps' study, the eight-percent and 17-percent model, which used standard air of the mining component, was not supported by the real data.

We are going to provide all of these data when we submit our written comments later this week. So we'll give full details on this data.

Before I go further, I want to clarify little things for the mining community about the CFD modeling, the way they do it in the explosion field. What happens is, in this reactive-flow problem, which is what is the explosion, the partial differential equations involved are so complex to solve for a real mine problem, so what people do is they will go for numerical methods and try to obtain approximate solutions to the actual problem. When I say "approximate," it still has an accuracy that is acceptable for practical applications.

So the CFD modeler's work is, for explosions, there are two different approaches. One is what I call "microscopic modeling," and the other is "macroscopic modeling."

Since the resolution that is required in an explosion model differs by two to three orders of magnitude, there is no way, even with the best of supercomputers we have today, we simply can't model all of the processes for a mine-scale, panel-scale models.

What it basically means is, let's say, the flame thickness is a fraction of an inch. On the other hand, we are trying to model a mine entry which is several feet in size. So the order-of-magnitude difference in the phenomena that we need to capture in a CFD model is tremendous. So we simply can't model a mine panel-scale phenomenon using this microscopic approach.

That means, the way the CFD modeling stands today, it's extremely difficult to solve the mine-scale-explosion problem using force for force.

So, as a result, what people do is they go with the macroscopic approach in which they try to approximate most of these microscopic phenomena. So that's what is done in the Corps' report. That's a macroscopic model.

So the success of a macroscopic model depends on the calibration. If you don't have proper data to calibrate your CFD model, its predictions will be highly questionable.

In the Corps' report, you'll see that they use two different actual data to calibrate their CFD models. One is the Lake Lynn experiments, which, of course, you all know that they are mainly deflagrations because of the limited volume of methane they used in those tests. The other test that they used was the Russian pipe test.

Now, I don't know how many of you read the actual paper of the Russian pipe test, but I did read it. The Russian pipe test will be conducted in an obstacle-laden pipe with systematically placed orifice blades inside the pipe. Why they do it is they wanted to create a very high level of turbulence inside of the pipe so they can created what they call "quasi detonation."

So that model is not applicable to a mine situation because you don't have that kind of very systematically placed obstacles in a mine.

So the calibration that was done against these Russian pipe tests is not totally applied to a mine situation. We're going to go into full details on these things in our written comments, and we'll provide all of the relevant references and excerpts from those papers so that you can also follow it easily.

Another point is that, in those obstacle-laden pipes, the mechanism of combustion is totally different. We have a lot of data to show from the general explosion literature that shows how these explosion variables change, depending on the obstacle configuration. So that's not totally applied to our mining situation here.

So the macroscopic modeling that was done in the Corps' report is not properly calibrated. That's the bottom line here.

Coming back to the three runs that the Corps did in their CFD models -- Run 1, 2 -- as you all know, are purely academic in their purpose because the nine-and-a-half-percent methane is not sufficient to match with the MSHA's measurement in the gobs of Sago. So those two runs were purely for academic purposes.

So if there is anything that is relevant to our discussion from the Corps' report, it is Run 3, which matches with the exact methane volume that was measured by MSHA as part of their accident investigation.

So, in all of these models, the problem is they used nine-and-a-half-percent methane or eight-percent and 17-percent methane, but the remaining gas is basically standard air. That changes the whole equation dramatically.

I'm going to show all of this data to you guys when we produce these written comments, but here are the 15,739 data points from 17 coal mines across the country. That red dot you see there is the stoichiometric mix. All of these blue lines are the real data. So you can see that not a single point is even close to the stoichiometric mix used.

I've blown up this graph in several ways to show how the Corps' assumptions match with the real data, and, as I said, not a single point is close.

So we don't have to make these extreme resolutions since we don't have data. We had to collect this real data first, and then we started doing the modeling.

So that's the problem with the Corps' report. I mean, I think the Corps did an excellent job in the CFD modeling, but they did not have the right inputs. It's not their fault.

If we provide the right inputs, CFD modeling could be a very valuable tool, but I strongly believe, as we commented in our 2007 SME paper, that the value of CFD modeling today for mines field applications is mainly for parametric studies; that is, to make a comparative analysis. Part of it, then, is a predictive tool. Because of the reasons I mentioned before, the microscopic processes cannot be simply simulated, even with the best of the massively parallel algorithms that we've got today.

So it will be a useful tool for comparative studies. By "comparative studies," what I mean is you have two situations. If you just want to compare the effect of changing one variable, for instance, if you want to put to gob oil in by the seal and want to see what is the effect on the explosion loads at the seal location, you can run two models, one with gop oil, one without gop oil. That's the kind of modeling that, I think, is more realistic with the macroscopic approach.

Now, what these real data also show is when you have this oxygen deficiency from the stoichiometric level in the gob, it's almost always accompanied by carbon-dioxide presence in the gob. Also, the nitrogen content used in these stoichiometric levels is not matched with real data.

To summarize, the story basically translates to you have either rich mixtures or lean mixtures in the gob, as opposed to stoichiometric. That means the equalizations are either Bilo-1 or Retalin-1.

What is the impact of this? The impact is the detonation cell size, if at all there is a possibility for detonation, even if you're talking in a theoretical sense. When you have a rich mixture as a lean mixture, your detonation cell size grows dramatically. We're going to give you some data to support that from the general explosion literature.

So the impact of that increasing detonation cell size is you will not have a chance to have a detonation, even with seven-to-eight-foot-high entries. So, normally, this stoichiometric level, the detonation cell size for methane gas is one foot, approximately one foot, or 30 centimeters. So that size keeps growing exponentially on both the rich side and the lean side.

So you need to have a certain ratio Between the entry size and the detonation cell size to have the possibility for detonation, even under the extremely worst-case conditions, if somebody wants to propose.

So this real data is the key here. We have to collect this kind of data more and more to support or dismiss the claims that people are making in these ideal models. So this data does not support or even come close to the assumptions that were made in the Corps' report, as far as the CFD modeling is concerned.

Another thing we found from this data is, of course, as you all know, we don't have data for gas sampling at different points inside the gob, but we do have some samples that monitor three corners of the longwall panel. So we try to see, like, in the Corps' report, they made a homogenous-mixture assumption in the Sago panel scale. So we looked at this data and see whether the homogeneous mixtures exist on a panel scale, and the data, as you will see, it doesn't. So we're going to provide all of those details to you in our written comments.

The next major problem with the CFD modeling is the Corps' study did not account for the effect of inert dust. Most of us think that we need to have a lot of inert dust to have a detonation quenching, but it doesn't have to be. We gathered a lot of research evidence to show that. We will provide the details, again, in the written comments.

What actually happens is when you even have, like, one of the results I remember is when you have a volume fraction of 10 to the power of minus 4, the inert dust particles, they can quench a detonation.

Detonation quenching doesn't mean explosive quenching. You might still have deflagration loading, but the possibility for detonation grows dramatically low in the presence of inert particles, and the particle sizes, the smaller they get, the more effect on the detonation quenching.

First of all, what I try to point out is if you consider all of the real world mechanics and true gas compositions, you simply cannot have detonation in a coal mine. That's the bottom line here. As I said, we don't just make these claims just like that. We have a lot of data to support these claims, and we'll provide all of the data in the written comments.

Now, as far as the explosion loading is concerned, our research really shows that, in the worst of the situations, probably this constant volume magnitude may be a realistic value to use because if you use the actual gop compositions that we collected and run these constant-volume condition models, like the -- Louis program, the deflagration loads were below 100 psi; 103 psi was the maximum I measured. So this 120 psi is really the upper limit for this kind of loading that can happen in a real gob, even for deflagration kind of situations.

But in a pipe, the real possibility is for a faster deflagration rather than for a quasi-detonation that the Russian pipe tests show, because of the different mechanics, as I said. In a fast deflagration, you might have a flame frame propagating at 1,000 meters per second, but, still, the pressures will be below constant-volume magnitudes, and we have a lot of data to support this. Again, we'll give a lot of references and figures that I reproduced from the international journals and articles from the international journals.

Again, the presence of inert dust will

definitely reduce the magnitude of even the deflagration loading. So that's why I think that 120 psi is really an upper limit for a realistic explosion in a gob.

Now, it's kind of strange that we all know from our day-to-day experience that a little change in the barometric pressure can cause hills to leak in and out, but we don't have any trouble accepting that a constant-volume condition exists by the seal, even under pressure differentials as high as 120 psi.

So what I'm trying to say is the constant-volume conditions simply do not exist underground. What exists is a faster deflagration regime, and close to the seals you will see a lot of leakage when you have this kind of 80-to-90 psi pressure differential in by and out by. So you simply can't have a constant-volume condition in a gob. So that 120 psi is a worst-case estimate for the constant-volume load.

Now, I'll turn to the structural modeling that is done in the Corps' report. There are only two major points that I wanted to make here because the structural modeling is pretty neat, the way the Corps did it. As I said, I have 12 years' experience of doing structural modeling myself.

When it comes to the seals by themselves, the omega block seals, that were modeled in the Sago report, two problems exist in the Corps' study. I think that's mainly because probably the Corps did not have the kind of right inputs for the modeling. It's not because of the limits of the model.

One is the boundary conditions. If you look at the Figure 1 in the Corps' report, you will see that the seal, when it is broken, it shows as pieces are flying all over, and then the boundaries on two sides were fixed in place. That boundary, that little layer, is basically the block-borne material. So what the Corps did is they used fixed boundary conditions on the floor and the two ribs, and they bled this pressure on the seal. So the block-borne is fixed in place while the seal starts to move.

So that's a very unrealistic boundary condition to use. The reason is that makes your seal stiffer so it can take more load than is real.

Now, another thing is the figure where they show pieces flying; it's not how it is modeled. That's simply a way to present the results. It's just a visualization method. It was not modeled to break the seal into pieces within the model itself. They simply used a continuum of finite-element codes, which can't model this kind of discrete fracturing. They did not use any particle-flow codes to do this discrete fracturing. It is simply a way of presenting results. So don't get to the belief that the seal fell into pieces within the finite-element model. It did not happen.

The second problem with the seal modeling is the constituting model they used is essentially a strain-hardening model. I gathered some data from the NIOSH research, basically, Tom Barczak, who did a lot of testing as a part of his Ph.D., and I collected data from him.

What the data basically shows is the real behavior of this omega block material is strain softening. What it is, after peak, the load-bearing capacity starts falling with strain. That's the strain-softening behavior. It's a brittle material with strain-softening behavior. If you see the Corps' report, they used a strain-hardening material, which basically means, after the failure, it starts taking more load. It gets hardened with the strain. So that's what is used.

What is the effect of this kind of improperly constituted model? The seal can take more load before it breaks into pieces or complete failure.

The real model is a strain-softening

material, but the Corps used strain hardening to simulate that material model. So because of those two, you need higher loads to fail a seal than if you model it as realistically as possible with proper boundary conditions and properly constituted model. But, again, if you -- the right inputs, it's a problem that can easily be fixed because it's not a problem with the modeling.

We're coming to the modeling of the plates, belt hangers, and the rock bolts. I don't have a lot of things to say there, except that the loads estimated were a little higher, even under the dynamic drag pressures. The reason is, if you read the Corps' report carefully and look at their pressure time curves, what you find is that, at each location in the mine, you could have multiple waves coming to that location.

Now, the second waves that are coming, the subsequent waves that come to this location, have much lower magnitude than the peak value sustained by the first instance wave.

So what happens is when this first wave passes these bolts, you look at the temperatures that were estimated in the Corps' model. They go to, like, 1,500 to 2,000 degrees Fahrenheit. When you have that kind of temperature when the first wave passes, the yield strength of the steel falls dramatically, and also its elastic modiolus also falls down with increasing temperature.

You don't have to heat the bolt to 2,000 degrees Fahrenheit. All you need is probably a 200-degree-Fahrenheit increase in the bolt's temperature. It reduces its yield strength, as well as the modiolus of elasticity. So when the second wave comes, or the reflector wave comes, that magnitude of loading is sufficient to bend the bolt.

So the thermal effects are extremely critical in this model, which were ignored. I'm going to give you some data. This is pretty common, and also there is a lot of data out there that shows the effect of temperature on the steel's yield strength, how it changes with temperature. So we're going to provide those details.

Now, the thing is, even without the proper structure modeling, the Corps' report clearly shows the loads required to bend the bolt are less than what is estimated by the CFD modeling. Of course, that's the dynamic pressure, not the peak pressure of the detonation frame that they came up with. But, still, if you do your properly constituted model and the proper boundary conditions and account for the thermal effects, then I think the loads that are required to bend these bolts are much lower than what is estimated.

Another point, when it comes to the belt hangers, is those belt hangers were subjected to service loads when the belt was suspended, and the belt was moving all the time. So you don't know really what was the amount of bending or deformation that was due to these light loads. We don't know that part. So all we are assuming is the final configuration that MSHA surveyed was totally because of the explosion loading, which may not necessarily be right.

So, in conclusion, all of the real data that we collected from sending coal mines, and, I think, one more coal company promised to send their data, and we might even increase the database by the time we submit our comments later this week, we think the probability of seeing a stoichiometric mix in the bog is simply zero. The data supports it.

The assumptions, as far as the standard air is concerned, are invalid. The data clearly show the presence of carbon dioxide in the gob when the oxygen is below the stoichiometric levels, and the nitrogen content in the gob is also higher than assumed in these models.

So the presence of higher inert gases is going to reduce your temperatures and increase the equivalent detonation cell size, which makes detonations almost impossible.

Then the presence of inert dust, even though in small fractions, is sufficient probably to quench a detonation, if it ever exists.

This also brings up another relevant point. When we assess the explosiveness, I think MSHA needs to really seriously consider giving an option for the industry to use. The reason is the presence of carbon dioxide is real, as all of these data show, so how can you ignore the presence of carbon dioxide in the gob when it is real? This research on the effect of the inert gases on explosiveness has been done four or five decades back. So why do we want to ignore it now?

Simply by following the Jones diagram, you would think that the mixture is explosive, but the moment you consider carbon dioxide into the equation, it may be hard explosive, and that's what we found for many samples we got here.

So that single thing, I think MSHA needs to consider because the real data show the presence of carbon dioxide, and we need to consider that.

Based on all of the research that we have been doing at Peabody, I don't think there is the possibility for having an explosion with a magnitude greater than 120 psi in a real coal mine situation.

I think, in all fairness, I truly believe that the Corps did a good job within the limits of their inputs, and I think the CFD modeling, as mining industry guys, we need to realize that CFD modeling has not come to the stage where you can use it as a predictive tool on a mine-scale basis. On a small scale, yes, it has gone to the stage where you can use it as a predictive tool, but when you try to simulate a mine-scale model, you need to have excellent calibrating data, to begin with.

So without conducting actual mine-scale explosion studies, I don't know how we can ever be able to calibrate a model.

So the CFD study still has a lot of value as a parametric study tool, where you can make a comparative analysis and come up with some ideas to mitigate the effects of these deflagration loads.

That's all I've got. If you've got any questions.

MS. SILVEY: I have a few comments, and, for everybody, first of all, even as we put this report on our website, and we open the comment period, and I think, if I'm remembering back from the Army Corps' report, and I'm going to pick up on what you said, Mr. Gadde, that the Corps -- I think it was their conclusion that additional research was necessary. So, to some extent, that's consistent with what you said.

In many places during your testimony, and I made a few notes -- I'm not sure exactly where I did them all, but you said that the data that you all had, and some of the conclusions that you came to from using your real data points, didn't support some of the conclusions in the Corps' report and that you were sending specific things to us. You specifically pointed out homogenous mixtures. I noted several other places that you pointed out maximum pressures that you thought would be realized in a real, live coal mine.

I would ask you, please, for the sake of making the record complete, if you would, before the record closes, when you send this specific remainder part of your comments in to us, for every conclusion, and you've made some this morning, and for every result, if you would make sure that you cite the specifics, and, as you said, you all took some data, cite the specifics to the data or specifics to the conclusions that you reach, and particularly if they were different from the conclusions that the Corps reached in its report.

MR. GADDE: Sure.

MS. SILVEY: I was going to ask you before you said it, I could probably deduce from what you said what your conclusion would be, but I was going to ask you, in light of all of the things you said, what impact -- even as I phrase this, I don't like it because, you know, they say you never ask a yes-or-no question, but I was going to ask you, what impact did you think the conclusions in the Corps' report would have on MSHA's ETS? You can either answer that now, or you can answer it -- I don't intend to put you on the spot -- or you can answer it in your written comments.

MR. GADDE: That's something for you to decide. We'll view the comments on what we think is right and the impact on the ETS part.

MS. SILVEY: No. Now, see, I go further, then. What I said that, I meant the impact on, because different people are reaching different conclusions, and I meant the impact -- I'll be specific, then -- impact on the strength requirement that MSHA included in the ETS. The reason I said I was going to ask you that is because, then later, and, still, I'm asking you that, so you answer it or not, because later you said that you didn't think that, in a worst-case scenario, the pressures would not be greater than 120 psi, and that's why I started out saying I was going to ask you.

So, anyway, when you send that in, if you would include specifics that would support that conclusion, when you say you didn't think they would be, in a worst-case scenario, in excess of 120 psi.

MR. GADDE: All of the comments that I made right now, I made in the same order as I'm going to present in my written comments. So the point is, so far, all of our discussions on the subject have been based on assumed models.

If you read our prior comments that Peabody submitted on the ETS as well as on the NIOSH report, we have been questioning the stoichiometric levels that are being repeatedly used in all of these models. At that time, we did not have enough data. In the comments we sent on ETS, we still had data from three mines, but we did not think that that was enough. But now I've got data from more mines that are scattered all over the country.

So the point is, we have to have real data first and then go from there and develop theories. Now we are doing just the reverse. So that's the trouble with all of these models.

As far as the impact, I think my research simply does not support any of these claims that are being made in these studies, especially about the detonations. I don't find any evidence to show that detonations are possible if you consider all of these real world data -- the actual gob composition, presence of inert dust, and multiple entries -- and you model from the first principles, and somebody has to show me that detonation is possible then. Without that, I think it's really unfair that we make all of these extremely ideal assumptions and try to come up with unbelievable numbers.

I was mentioning to one of you guys when I was talking before, as part of our research at Peabody, we are also trying to assess: Let's say there is a detonation at 640 psi, that magic number. What happens to your coal mine entries? What is the effect of that kind of high detonation of the dynamic loads on the stability of your coal mine entry itself?

We started doing that analysis. It's not, by no means, complete at this point in time, but I can tell you that, while the coal field itself will not fail or shatter, there will be huge roof falls at the corners, rebending the roof corners, because of this high dynamic loading. How many times did we see, in an explosion event, all of the roof contacts fail in a big manner?

As I said, I still have to finish this research, so I can't come conclusively say what are the final outcomes, but you can look at these things in many different perspectives, and that's the way to go forward. We have to look at the real conditions, look at everything that can be affected by these high loads, and see if any real explosion event is supporting this data. Without doing that, it's really very simplistic models that we are using right now.

MS. SILVEY: Okay. I think we have an understanding there. We will be very interested to get that, as you said, and have the rule-making process informed by the real data that you said you all are collecting and sending in to us and the conclusions that you draw from that.

MR. GADDE: But in this mine entry stability part, I will not be including in this because I did not finish that research.

MS. SILVEY: Yes. Okay. I understand.

MR. GADDE: The remaining things that I made a part of this public hearing, I'll provide all of the details.

MS. SILVEY: Okay, okay. I don't have any more. Do you have anything?

MR. UROSEK: I would just like to embellish little bit on what you had asked for and pay close attention to some of the things you said you would provide us.

You mentioned the gases. I was particularly interested about your comments on the inert gas and the effect on a detonation deflagration and your comments on whether a detonation could actually occur in an underground mine. As much information as you could provide in those areas would be really helpful to us.

MR. GADDE: Sure.

MR. UROSEK: You mentioned about having a constant volume during an explosion, and information, we're supporting that, as much as you can for us, would be very interesting.

On the temperature, you talked about the temperature and the thermal effects. If you can provide as much information as you can, especially when you consider the short duration of an explosion flame passing over these devices, what effect it has on different pieces of equipment. That would be helpful to us.

MR. GADDE: Actually, John, that's the reason I keep on saying that the thermal effects -- if you see the coal's pressure time curves at different parts from the CFD models, you see little bumps at later times. Those are the waves that are coming later or the reflector waves.

So I'm saying that even if the first peak duration is shorter, but the secondary waves that are coming could have further deformular flames. That's what the point was.

I'll view the data, how the steel strength falls with temperature and explain further on the written comments.

MR. UROSEK: I was looking more for the duration that the flames are actually there to cause a temperature change. That was all.

MS. SILVEY: Then I want to add one more. I wasn't going to go through all of the points because I figured you know the points. I'm sure you remember the points where you brought out areas with respect to the Corps' study and what your data found, so if you would make sure you could hit those points.

But also, adding on one other thing, I remember the point you made about the homogeneous mixtures that your data didn't show. Please make sure you include that, on the homogeneous --

MR. GADDE: Sure.

MS. SILVEY: That part will be very significant.

MR. GADDE: I already made the parts. I just need to --

MS. SILVEY: Thank you. That's fine. I just wanted to make sure.

MR. SHERER: First of all, I want to thank you for your information. It was very interesting.

The roughly 15,000 data points that you had; do you have any idea how many of those came from Spon Com mines?

MR. GADDE: I can't tell you that answer because I don't know. At least one of our mines has that one, but I can tell from the other cooperating coal companies that they did not give those details to us.

MR. SHERER: Okay. The other thing is, did you include any data from newly sealed areas, or are these all mature sealed areas?

MR. GADDE: It includes all of the data that we got until last week, so it automatically includes some of the new-sealed areas as well.

MR. SHERER: Okay.

MR. GADDE: But the majority of them may be the old gobs.

Some of the mines, they were voluntarily monitoring for over 12 years now, so they have kept track of their gobs for a period of time. It's not something that they did only after eight years.

So that data is really very complete. I got some of the data, like, they have been doing it as a routine practice for 12 years. So we have transfer from freshly sealed time to a 12-year period.

MR. SHERER: Why, may I ask, did they do that?

MR. GADDE: Just because they -- Spon Com, the one mine I talked about.

MR. SHERER: Okay, okay. Thank you.

MS. SILVEY: Okay, then. Well, we appreciate very much your testimony and appreciate the information. Thank you very much for your information, and we'll look forward to getting the additional comments before the record close on January

18th.

MR. GADDE: Sure. Thank you.

MS. SILVEY: We appreciate it.

At this point, is there anybody else who wishes to make a comment? Anybody else in the room who wishes to provide testimony?

(No response.)

MR. SHERER: It doesn't appear. So since there is nobody else in the room who wishes to provide testimony or comments, we will bring the hearing to a close.

Now, before I do that, I would like to say that we do appreciate Peabody and Mr. Gadde for providing their testimony, and we appreciate people who came today, who came to this hearing, and who, therefore, we know are interested in the rule-making but may not necessarily have provided comment or testimony here. But we expect that some people who are in the audience today either, one, have provided comment and testimony or will provide additional comment and testimony before the record closes on the 18th. So we look forward to getting that.

As usual, we do think that our rule-makings are better because of the public participation, and we appreciate you all being here.

I'm going to say this, which is a little bit unusual: If anybody happens to show up, because we are located in this building, if anybody happens to show up later this morning, let's say, and came to testify or something, we'll make arrangements that they can provide their testimony, either reopen the record -- if I have to do that, I'll do that.

But right now, I do appreciate everybody who came, and, at this point, on behalf of, as I said earlier, Assistant Secretary Stickler, thank you for your participation, and the hearing is concluded.

(Whereupon, at 9:50 a.m., the meeting in the above-entitled matter was concluded.)

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REPORTER'S CERTIFICATE

DOCKET NO.: n/a

CASE TITLE: Sealing of Abandoned Areas

HEARING DATE: January 15, 2008

LOCATION: Washington, D.C.

I hereby certify that the proceedings and evidence are contained fully and accurately on the tapes and notes reported by me at the hearing in the above case before the Mine Safety and Health Administration.

Date: 1/15/08

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