#2

THE NUREG/CR-6224 HEAD LOSS CORRELATION

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The NUREG/CR-6224 correlation is the basis for head loss analysis in the LANL research report[1] LA-UR-04-1227. It is also quoted in the "technical basis report" NUREG/CR-6808 and is the approach adopted in the new NEI guidance on sump blockage.

In this memo, I first review the correlation scheme as used by LANL and reach some conclusions. I also make use of a more comprehensive analysis of the LANL data that is presented in reference 2. I then review the original presentation in NUREG/CR-6224 and find that the conclusions are reinforced.

There are some features of this correlation which may be expressed in a more accurate form and some aspects of it that appear to be basically wrong. It is also evident from recent LANL data that relaxation phenomena, probably associated with migration of particles through the bed, can have a large effect on head loss. These effects are not covered by existing theory in any form. It is therefore doubtful if any realistically conservative estimates of screen performance can be made.

Pressure gradient or "head loss"

The head loss is given by the terms in square brackets on page 33 of the LANL report (see p.16 of the current document). The first term is linear in velocity and reflects viscous effects. The second term is proportional to the square of velocity and reflects the effects of inertia (not turbulence). This form is standard; for instance it resembles (6.4-14) in Bird, Stewart and Lightfoot's book. As discussed in reference 1, this equation really describes the pressure gradient, which varies through the bed. It is used in the NUREG in an averaged form that is assumed to describe the entire bed.

The specific surface area, S_v, may be converted to $6/D_p$, the equivalent factor in B.S. &L., where D_p is the effective particle diameter. Then the coefficient in the first term in B.S.&L., 150, becomes 150/6/6=4.17, which is roughly comparable with the 3.5 appearing in the 6224 correlation.

Similarly, the coefficient of the second term in B.S.& L., 1.75, becomes 1.75/6 = 0.29 which is not unreasonably far from 0.66 in 6224. One could argue that the coefficients in 6224 refer mostly to fiber beds, though the equation is also used for particulate beds and mixed beds of particles and fibers, while B. S. and L. consider particulate beds.

The differences between the approaches are mostly in the component factors in the two terms involving the porosity, ε. For the first term, B.S.& L. have $(1-\varepsilon)^2/\varepsilon^3$ compared with $(1-\varepsilon)^{1.5}(1+57(1-\varepsilon)^3)$ in 6224. For the ranges of porosity of interest, which appear to be 0.985 to around 0.85 in the LANL report (but they may be lower, because, as discussed below, the compression of the fiberglass mat near the screen is higher than the average throughout the filtrate), the difference between these terms is dominated by the factor $(1-\varepsilon)^{0.5}$ which varies by a factor of about 5 over the range of interest. Therefore the two theories, if they agree at one extreme, will differ by a factor of five at the other. This difference is clarified in the original NUREG/CR-6224 where it is explained that the chosen version is based on a correlation developed specifically for fiber mats. Correlation of the actual LANL data[2] for a mixture that is 1 part CalSil to 2 parts NUKON suggests that the index on $(1-\varepsilon)$ in the viscous term should be 1.83, which is intermediate between the above values of 1.5 and 2.

In the second term, B. S. & L. have $(1-\varepsilon)/\varepsilon^3$ compared with $(1-\varepsilon)/\varepsilon$ in 6224. This could be explained if the velocity, U, used in NUREG/CR-6224 were the actual average fluid velocity through the bed, U_f. Since $U_f=U/\varepsilon$ the two extra factors of ε would be accounted for. However, the NEI guidance specifically says that "U" in the equation is the "approach velocity" and the examples in the knowledge base report support this interpretation, though the "velocity" is not clearly defined in the presentation of the equation itself.

I am pretty sure that there should be three factors of ε and not just one. The first one comes about because as the bed is compressed the resistance to flow goes up as the passages get smaller. The other two factors come from the decreased flow area for the fluid, which increases its velocity through the pores. The effect is not very big over the range of ε cited above, but it could be significant if the bed is compressed and the pores filled with particulate dust. It is also likely[1] that the layer of the fiber bed next to the screen is compressed more than the average value and it may selectively filter out particles, leading to a locally low value of porosity, which will make the differences in this term between the two theories substantial. The LANL studies indicate that this effect (or some similar one not yet explained), for which there is no quantitative theory, can increase the head loss by almost an order of magnitude.

Compression of the fiber mat

On p.34 of the LANL report the compressibility of the fiber bed is described by

$$c = 1.3c_0\,(dH/dL_0)^{0.38} \tag{1}$$

c is the actual packing density (i.e. mass of fiber per total volume of fiber plus water) and c_0 is the "as-manufactured" density. The factor in parentheses is the pressure gradient based on the original thickness of the bed.

Now, a fiber bed laid down on a screen after being chopped up in a blender is not the same as the "as-manufactured" fiberglass, so the factor c_0 can only be interpreted as a reference value and not the value that would occur if there were no pressure gradient (actually the equation predicts c=0 when there is no pressure gradient, in which case the bed would be infinitely thick). If we put $c=c_0$ in (1) we can solve for the pressure gradient (close to 0.5 feet of water per inch) that will give the as-manufactured density.

My serious objection to equation (1) is that it is based on false physics. It is not the pressure gradient that compresses the bed, but the stress carried by the fiber matrix. One might as well argue (falsely) that it is the stress gradient that causes strain in a piece of steel. As explained on pages 202-204 of my book, the stress carried by the fiber matrix has to balance the difference in pressures in the fluid from the surface of the mat to its interior. The pressure drop over the entire bed is supported by the fibers compressed against the screen. There is no fiber stress, and no compression, in the layer at the surface of the bed. The sum of the fiber stress and the fluid hydrostatic pressure is constant throughout the bed in order to preserve the overall force balance. As the fluid pressure drops, the fiber stress goes up to compensate. The strain should be related to the "particle pressure" or "fiber stress", as in (8.78) in my book, and not to the pressure gradient.

In the LANL experiment 6H we have pressure drops of up to 13feet over a bed with "as-manufactured" thickness around 0.23inches. Then (1) would predict compression by a factor of 1.3 x $(13/0.23)^{.38} = 6$ which is far larger than the value of 0.23/0.13 =1.77 observed in the measurements of bed thickness in that test. LANL claims that the particles are compressed to "the slurry limit" although neither the actual measurements of compression nor realistic predictions of them support this hypothesis[2].

The statement in the NEI guidance that (1) gives too high a compression for thick beds appears to be wrong. The compression is dependent on the overall pressure drop, not the pressure gradient. For the same pressure drop, the thick bed has a lower pressure gradient and (1) predicts less compression (not more), when it should be the same as in the thin bed.

What does the evidence show? Since the bed thickness was reported by LANL it should be possible to test (1) and the alternative hypothesis that bed compression depends on overall pressure drop and not on pressure gradient.

In terms of the measured thickness, dL_m, and the "as-manufactured" thickness, dL_0, (1) can be expressed as:

$$R = dL_m / dL_0 = 1 / (1.3\ (dH/dL_o)^{0.38}) \qquad (2)$$

The alternative hypothesis, in similar form as far as the influence of head loss is concerned, tuned to fit the first data point, Test 6B, is

$$R = 0.87 / dH^{0.38} \qquad (3)$$

The table below gives values of R corresponding to the maximum pressure drop (unfortunately this does not vary much between experiments) in each experiment from Test Series 6 in LA-UR-04-1227, compared with predictions of (2) and (3). Since the bed thickness was measured to an accuracy of 0.6inches, the range of thickness used to compute R is the recorded value +/- 0.3inches. The 6H* data correspond to the values for the two points before the dramatic increase in pressure drop by a factor of about 10 that was observed, following a small increase in flow rate.

Test	dL_0 (in)	R	(2)	(3)
6B	1.6	0.26 - 0.29	0.29	0.28
6C	0.19	0.85 - 1.17	0.18	0.38
6E	1.1	0.32 - 0.37	0.29	0.32
6F	0.63	0.25 - 0.35	0.26	0.35
6G	0.236	1.48 - 1.74	0.17	0.32
6H	0.236	0.42 - 0.68	0.17	0.33
6H*	0.236	0.55-0.81	0.41-0.45	0.79-0.89
6I	0.86	0.26-0.33	0.29	0.35

Table 1. Estimates of bed compression at the maximum pressure drop.

This evidence does not conclusively support either theory. The results for the thicker beds are equally well predicted by both. Among the thinner beds, the results for 6C and 6G are anomalous as the beds appear to have grown rather than compacting. In 6H and 6H*, (2) significantly under-predicts the thickness both before and after the pressure drop increase; (3) under-predicts one and over-predicts the other.

There are several anomalies. In 6C the bed thickness reported at the first data point (low flow) is over twice the as-manufactured thickness. The minimum bed thickness is still around the as-manufactured thickness, showing no apparent compression. In 6E the bed thickness was still decreasing at the end of the test as the flow rate was reduced to low values. By contrast, in 6F the bed thickness rebounded as the flow rate was reduced. In 6G the bed thickness at the maximum pressure drop exceeded the as-manufactured thickness.

Unfortunately, it appears that these measurements were not accurate and definitive enough to provide a check on the theory. The comparison shown by LANL in Figure 5.6 as evidence of "adding to the confidence in applying the correlation" is overly optimistic and misleading. The authors have selected the particular data set which best fits their theory. The alternative hypothesis in (3) would predict this single data set just as well.

In reference 2 I make a more comprehensive comparison between an equation of the form of (3), but with the index 0.29 instead of 0.38, and all the data on bed thickness from Test Series 6. Data from six out of eight tests are fitted within the precision of the measurements. The form of correlation presented in (2) is not supported by the data.

The "sludge density"

There appears to be a fundamental error in the expression at the bottom of page 34 of the LANL report describing the limiting strain allowed by the "sludge density":

$$dL_m = dL_0 \ (c_o/c_{sludge})(\eta + 1) \qquad (4)$$

η is the mass of particles (e.g. CalSil) per unit mass of fiber, assumed uniformly distributed through the bed.

dL_m/dL_0 is the relative change in the bed thickness, actual divided by "as-manufactured", which I will call R for convenience.

The idea is that the compression of the bed cannot continue beyond the condition where the particulates are compressed to a limiting value, the sludge density, beyond which they would have to crush to increase the density further. c_{sludge} is the density of the sludge. For CalSil it is determined by what is needed to correlate the pressure drop data (reference 2 discusses why this may be inappropriate), which is around 20lb/ft^3. This implies that the particles, which have a material density of 115 lb/ft^3, have a spiky, porous, matrix-like, or some other form of open structure.

This sludge density is the mass of particles per unit volume of the space, containing water and particles, that is available between the fibers.

Start with a mass of fiber per unit volume of c_0 at a reference condition which is the "as-manufactured" thickness. Add a mass of particles per unit volume of ηc_0 and let the displaced water escape.

The volume of fibers per unit total volume is c_0/ρ_f

The volume of particles per unit total volume is $\eta c_0/\rho_p$

ρ_f and ρ_p are the material densities of the fibers and of the particles respectively.

Then the fraction of unit volume occupied by material and not water is

$$1 - \varepsilon = c_0/\rho_f + \eta c_0/\rho_p = c_0/\rho_f\,(1 + \eta\,\rho_f/\rho_p) \qquad (5)$$

When there are no particles, the fiber bed porosity at the reference condition is

$$1 - \varepsilon_0 = c_0/\rho_f \qquad (6)$$

so, (5) may be expressed as

$$1 - \varepsilon = (1 - \varepsilon_0)\,(1 + \eta\,\rho_f/\rho_p) \qquad (7)$$

When this mix is compressed to a fraction R of the initial thickness, the fraction of space occupied by the solid phase becomes

$$1 - \varepsilon_m = (1 - \varepsilon_0)\,(1 + \eta\,\rho_f/\rho_p)\,/\,R \qquad (8)$$

which is the same as the equation at the bottom of page 33 in the LANL report.

When the mat is compressed, the space taken up by the fibers per unit volume is

$$c/\rho_f = c_0/R\rho_f \qquad (9)$$

Therefore, the available space between the fibers for the water and particles, per unit volume, is

$$\varepsilon_f = 1 - c_0/R\rho_f \qquad (10)$$

The "density" of the particle sludge in the space between the fibers if then: mass of particles per unit volume / volume of space between fibers per unit volume

$$= c_s = (\eta c_0\,/\,R)\,/\,(1 - c_0/R\rho_f) \qquad (11)$$

Which may be rearranged to give

$$R = c_o\,/\,c_s\,(\eta + c_s\,/\rho_f) \qquad (12)$$

The maximum allowable compression R_{max} is then achieved when $c_s = c_{sludge}$.

This is not the same as (4), the term 1 being replaced by c_{sludge}/ρ_f, which, for the values of parameters given by LANL is about 20/175 or 0.114, much less than 1.

Note that we cannot take the general limit of (12) as $\eta = 0$ to get $R = c_0/\rho_f$ because c_s goes to zero with the same order as η at low values of compression. However, we can evaluate the limit of R at low η from (12) when the mat is highly compressed; it is c_0/ρ_f, indicating that the fiberglass has been compressed to its material density, leaving no space for the slurry at all (as would be necessary to achieve c_{sludge} when there are almost no particles). This is logically reasonable but physically impossible, there being some limit to the compressibility of the fiberglass matrix as well as to the compressibility of the slurry. By contrast, for the same limit of $\eta = 0$, (4) would predict that the fiberglass density, c, would be equal to c_{sludge}, which is unreasonable and makes no physical sense.

As an example, consider $\eta = 1$. We start with 2.4lb of fiberglass and 2.4lb of particles in a cubic foot of total stuff. Then (12) predicts that $R_{max} = (2.4/20) \times (1 + 20/175) = 0.1337$. At this compression, one cubic foot of stuff contains 2.4/0.1337 = 17.95lb of fiberglass and the same weight of particles. The fiberglass occupies a volume of 17.95/175 = 0.1026 cubic feet, leaving 0.8974 cubic feet for the slurry, which therefore has a density (of the particulate phase alone, not of the mixture of water and particles) of 17.95/0.8974 = 20 lb/ft^3 as expected.

The difference between (4) and (12) is less important as the particle loading increases. At lower loadings it can be significant; for example, with $\eta = 0.5$ the factor in parentheses in (4) is 2.44 times as large as the corresponding factor in (12).

Now, look at Test 6H in the LANL report. It is reported that the as-manufactured bed thickness if 0.23 inches and the maximum compression is 0.13inches, giving R = 0.13/0.23 = 0.565. For the given particle loading of $\eta = 0.5$, (12) gives the maximum R allowed as (2.4/20)(0.5+ 20/175) = 0.07 which is far below the measured value. Even if we use the false equation (4), the maximum R is predicted to be (2.4/20) (0.5 + 1) = 0.18, which is still much below the observed value. Therefore the claim in the text that the bed was compressed to a limiting thickness and constitutes a "thin bed" seems to be incompatible with the evidence.

The data also do not support the contention in the text that the sudden rise in pressure drop (by a factor of around 10!) in Test 6H was due to bed compaction. The data in Table F.8.1 show the bed thickness decreasing from 0.19 to 0.13 inches somewhere between the beginning and the end of the experiment. The apparent step change before the dramatic rise in pressure drop is due to the resolution of the measurements being only 0.06 inches. Therefore no sudden change in thickness is confirmed.

It is possible that in Test 6H the compaction of the part of the bed forming in a thin layer near the screen led to progressive filtering out of the particulate fines into this region, and perhaps some particle migration from the back of the mat (over a period of two hours!), which eventually plugged up the pores there. Explanation of this phenomenon, which appears to be compatible with observations by LANL[2], would involve considering the variation in compaction and particle loading through the filtrate, and their inter-relation with the pressure drop, as well as the size distribution of the particulates, none of which phenomena are part of the theory in the LANL report.

(There probably are variations in particle loading in the filtrate following a LOCA. If a few fiberglass fibers arrive and filter out most of the easily-transported particulates to form a dense "thin bed", the arrival of more fiberglass later will build up a superficial layer relatively free of particles. Averaging everything throughout the bed may disguise the presence of a thin layer with very high resistance to flow, being compressed by all of the pressure drop generated by the layers above it.)

Experimental evidence from data in the LANL report

The report describes experiments and provides tables of data. These should be useful for evaluating the theories. The following is a discursive review of the evidence. More quantitative comparisons and evaluations are given in reference 2.

The approach taken by LANL is to correlate the maximum pressure drop observed in each test by tuning some of the parameters in the theory. It seems to me that the theory should be able to handle the whole sequence of events as the flow rate is increased and then decreased. Comparisons should be made with all the data points. The pressure drop equation is supposed to work everywhere, not just at the maximum pressure drop, which is itself somewhat apparatus-dependent. The data about bed compaction and the amount of CalSil that is filtered out could be used to check the theories at every data point, but they are not used at all.

The theory contains several constituents; two terms in the pressure gradient, densities, surface areas per unit volume, compaction, sludge limit etc. The approach of fitting data with a set of values of the parameters describing these phenomena hides the details of which parts of the theory are working and which are not. It is quite possible that an error in one part of the theory is compensated for by adjusting the correlating parameter in another part of the theory. For example, in test 6H a high compaction of the bed is invoked to explain the high pressure drop, but it is not compatible with actual measurements of compaction. The data can be used much more intelligently to test parts of the theory independently. If this gives anomalous or unexplained results, perhaps due to inaccuracies or whimsical behavior in the data, this should be acknowledged and suggestions made for a test program to resolve them.

By using all of the observations and data in LANL's more comprehensive Series 6 tests it should be possible to predict the "hysteresis loop" traced by increasing and decreasing the flow rate, as well as several other features.

There are (at least) four features expected:

A. The bed is progressively compressed as the flow is increased. Therefore the particle volume fraction $(1 - \varepsilon_m)$ progressively increases.

B. The bed usually (but not always) appears to stay compressed as the maximum value $(1 - \varepsilon_m)_{max}$ as the flow is decreased. This is a reasonable hypothesis that can be checked by using it in the pressure drop correlation.

C. The CalSil is progressively filtered out into the mat as the flow is increased.

D. The CalSil mostly stays filtered out as the flow is decreased, though in several runs some of it is recorded to reappear in the water.

The bed thickness measurements ought to be suitable for checking A and B.

As discussed in reference 2, they appear to follow a consistent pattern in six out of eight tests. The data shown in Figure 5.6, which are for the thickest bed tested, are probably the most accurate in terms of relative bed compaction. They show an increase of (1 - ε_m) by a factor of four over the "as-manufactured" value at a pressure drop of about 20 feet of water. This contributes a factor of 2 , i.e. (1 -ε_m)$^{0.5}$, to the predicted pressure drop at low flow rates in the viscous regime (the other factor of (1 -ε_m) is cancelled by the accompanying change in dL_M/dL_0). One can use either the (bogus) LANL theory for bed compression, or a more realistic theory such as is presented in reference 2, to predict (1 - ε_m) and then use it in the pressure drop correlation.

Effects C and D can be assessed by using the values recorded in the logbook of the fraction of CalSil estimated to circulate in the loop. For example, in Test 6B, with 100g of NUKON, 90% of the CalSil was filtered out at the lowest flow rate, U= 0.1ft/s and almost all was removed at higher flow rates. By contrast, in test 6C with only 12g of NUKON, 70% of the CalSil was estimated to be still circulating around the loop at U=0.1 ft/s. Since the correlation takes account of the amount of CalSil in the mat, these values can be inserted and predictions made.

I made a qualitative and semi-quantitative assessment of these effects, following the sequence in the LANL report. More compete comparisons are made in reference 2.

Results for *Test 6B* are recorded in Figure 5.5 In the accompanying text it is stated that: "To predict the decreasing velocity data, the correlation compression must be fixed at the compression associated with the highest velocity". It is not specifically stated that this is the basis of all the predicted curves in Figures 5.5 to 5.16, but the implication is that it is. However, this hypothesis is challenged by the comparison in Figure 5.16 where predictions of the 6224 correlation for increasing and decreasing velocities appear to differ by a constant factor, presumably attributable to the specified use of a different value for the specific surface area, S_{vp}. If compression were considered, one would expect the data for increasing flow to be an additional factor of about two lower at the lowest velocities, as the data actually suggest.

The logbook record for *Test 6B* show that most of the CalSil is captured in the mat for all points. The bed thickness decreased by a factor of four and, as predicted, the head loss for increasing flow rate is about one half of the value for decreasing flow rate at low velocities, so it appears that the data for increasing flow could have been predicted quite well using the empirical value of S_v =543,000 /ft and taking account of the progressive bed compression.

It is rather disconcerting that it took 20 minutes for the head loss to settle down each time the flow rate was incremented. What are the implications for plant behavior when parameters are changing?

In *Test 6E*, Figure 5.7, the chosen value of S_v is 423,000 /ft, significantly less than in 6B. This might reflect the lower compression for a thinner bed, predicted by the more realistic theory, rather than a change in the real specific area (i.e. a coefficient describing one phenomenon is being used to compensate for a different one). The hysteresis loop is thinner than in 6B, with the results for increasing flow close to those for decreasing flow. If one accepted the LANL correlation for bed compression, there would be about the same maximum compression in this test as in test 6B, as the pressure gradient is about the same, and the hysteresis loop would be expected to be about the same whereas they look noticeably different.

Test 6I, Figure 5.8, has head loss with increasing flow greater than head loss for decreasing flow, though there is more CalSil filtered out and more compression on the decreasing side. This violates all qualitative predictions.

In *Test 6F*, Figure 5.9 there is little hysteresis. Almost all the CalSil is filtered out for all points. This is the same material as in test 6E but there is 57% as much of it. The predicted non-linear effect of initial bed thickness, presented in my memo on flow through a porous compressible mat, would predict that the pressure drop should be less than 57% as great for the thinner bed, whereas it is measured to be 71% as great. It would be better to find some physical reason for this unexpected result, rather than ascribing the difference to a different value of specific surface area, for which there is no basis.

Test 6C, Figure 5.11, displays a large leap in head loss at around U = 0.5 ft/s. The correlation shown is fitted to the highest point and there are no points with decreasing flow. The value of S_v that is chosen is 525,000 /ft, although this is said to be a "thin bed" for which later data suggest a value of 800,000/ft. Since this curve is supposed to also represent decreasing flow it is strange that the only point with increasing flow, apart from the highest one, that lies on the curve is at the lowest velocity of 0.1 ft/s. Yet the logbook reports that 70% of the CalSil was circulating around the loop then. So the remaining 30% in the mat is apparently behaving as if it had a S_v of about 1,600,000/ft, or even higher if bed compressibility is considered. Since one would expect the coarser particles to filter out first, this value should be lower than for the other points, not so much higher.

An S_v of 525,000 /ft correlated the upper curve. The lower curve drawn through the points with increasing flow rate suggests a much lower value. Since the as-manufactured thickness is 0.187 inches, if we believe Figure 4.1 and the accompanying text we can compute the pressure drop for NUKON alone to be 187/1.72 of the values in that figure at the same flow rate. Then interpolation between this and the upper curve, gives a value of S_v of about 175,000 for the lower curve.

So, we have three vastly different values of S_v for the same material in the same test.

Tests 6H and 6G produced the most dramatic leap in head loss at around U = 0.4ft/s. Using the same interpolation techniques as for 6C, I concluded that the CalSil behaves as if it had a S_v of less than 100,000 /ft for the first set of points with increasing flow. Then, over a period of two hours at around U=4 ft/s this changed to 800,000 /ft. This is not a good verification of a correlation for which the preferred S_v in earlier tests was 450,000 /ft. This is said to be a symptom of a "thin bed" but there is no criterion or mechanism specified for determining what is meant by this expression nor why the dramatic change in properties occurred.

Unfortunately, there were no data obtained for flow rates higher than those which produced the "leap". Therefore it was not determined if the data would then settle down to a more predictable pattern, show a further leap, or some other unexpected trend.

One might be tempted to conclude that the results are too sparse, whimsical and erratic for the kind of deductions that are attempted above to be meaningful. Yet LANL claim that: "The comparison of Test 6G with 6H results provides good evidence of repeatability in the head-loss testing".

The theory hypothesizes that there is a parameter, S_v , that characterizes a certain material and can be used to predict head loss. It is not supposed to correlate only some of the data in a given test, change its value from test to test, nor take on several different values in the same test. Aleatory variations in S_v are not evaluated by comparing values needed to fit data in different tests, as the cause could be some other effect, such as bed compressibility. They are best evaluated by running the same test a sufficiently large number of times. If the changes in S_v that correlate data from test to test are broader than this aleatory variation, it is an indication that something else is the cause.

My observation is that there is something anomalous, inconsistent, contrary to expected trends or predicted qualitative behavior, about almost all of the results. These observations raise questions, none of which has been pursued by further research or answered by rational explanation. The correlation procedure has not been validated.

Not only that. These results are history and procedure dependent. No effort seems to have been taken to introduce the material in different ways or to go through a different sequence of flow rates. For example, the flow rate could have been increased and decreased several times to see if the head loss curve finally settled down to a single location and if the hysteresis disappeared.

In the LOCA situation the sequence of events is quite different from what was used in these tests. The pumps turn on to their full flow rate while the screen is unblocked. Material arrives in a sequence that depends on how the various ingredients are formed and transported. Perhaps the particles arrive first and flow through the screen until enough fibers build up to filter them out. Fibers are not distributed uniformly on the screen. It is presumably not necessarily conservative to assume they are uniformly distributed, because a "thin bed" on the parts less well covered might be more effective in creating head loss. Given the difficulties in obtaining consistent patterns in the results in these controlled tests, I would expect a lot of variation in results from tests that are less well controlled or subject to more influences. In any case there is a need for a much expanded research program to resolve anomalies in the controlled tests and to investigate process-dependent effects that could influence performance following a LOCA.

In reference 2 I supply more quantitative analysis of the LANL data. This helps to support the conclusions drawn below.

Conclusions based on the LANL report

1. There appears to be an error in the pressure drop equation that may lead to significant effect if the mat is highly compressed on the screen or contains a dense layer close to the screen. The second term in the head loss correlation should have ε^3 in the denominator instead of ε.

2. The index on the term $(1-\varepsilon)$ in the viscous term may be 1.5 for fiber beds but there is evidence that it may increase to a value closer to 2, for particulate beds, when particles are added.

3. The equation given to describe bed compression is based on false physics. This conclusion is supported by analysis of the LANL data.

4. There is a basic error in the equation describing compression to the "sludge limit". It was originally postulated in NUREG/CR-6224 as a simple way of limiting the unrealistic predictions of compression for thin beds. There is no evidence that it actually occurred in the LANL tests.

5. Relaxation phenomena, probably associated with redistribution of CalSil within the bed, can have an order of magnitude effect on head loss.

6. Tuning the correlation to fit selected features of the pressure drop data by adjusting the specific surface area of CalSiL compensates for the errors 2, 3 and 4 and the absence of any theory for 5 by adjusting coefficients that describe other phenomena. This approach is inappropriate and provides no basis for extrapolation to conditions other than those tested.

7. The report is over-optimistic about predictive capabilities. Selected comparisons and conclusive-seeming statements unsupported by the data are misleading. There are many strange and unexplained features of the data and many inconsistencies. Many data that ought to be useful for checking details of the theory and the confidence with which it can be used are ignored.

8. It is uncertain how these results can be used to support plant predictions, since the nature of the fibers, the way they are deposited on the screen, and the history of the flow influence the results. These variables have not been

investigated as all the tests were performed in the same way that is not typical of events following a LOCA.

9. There is not a sound basis for claiming that guidance, based on these correlations and these tests, is adequately validated.

Review of NUREG/CR-6224

I obtained a copy of the original NUREG. The models for bed pressure drop and compressibility are in Appendix B.

The Kozeny-Carman Equation for viscous flow in porous media is quoted:

$$\Delta P/\Delta L = aS_v^2 (1-\varepsilon)^2 \mu U/\varepsilon^3 \qquad \text{(B-18a)}$$

and is said to become in the "turbulent" region, quoting Ergun,

$$\Delta P/\Delta L = bS_v (1-\varepsilon) \rho U^2/\varepsilon \qquad \text{(B-18b)}$$

"U" is defined as the "velocity". Now, in order for (B-18a) to be consistent with the quoted reference, U must be the approach velocity, or superficial velocity. In order for (B-18b) to be consistent with its quoted reference, U must be the average velocity in the pores. In the LANL report and the NEI guidance, U is treated as the approach velocity. In this case there must be an additional two factors of epsilon in the denominator in (B-18b) so ε should be ε^3 there.

(B-18a) is replaced by an alternative equation from Davies, the same reference as on page 204 in my book, that may be more appropriate for describing fibrous material (though some of the material is actually particulate). The resulting correlation for head loss is the one quoted by LANL (with a few small differences in notation, Δ's replacing d's):

$$dH/dL_0=C[3.5S_v^2(1-\varepsilon_m)^{1.5}[1+57(1-\varepsilon_m)^3]\mu U+0.66S_v((1-\varepsilon_m)/\varepsilon_m)\rho U^2]dL_m/dL_0 \qquad \text{(B-21)}$$

ε_m is the actual average porosity of the bed, made up of fibers plus particulates, and is given by (8) which is the same as (B-22). As discussed above, I believe that the ε_m in the denominator of the second term in brackets should be cubed.

In discussing bed compaction, the reference "theoretical fiber bed porosity", ε_0, is related to the reference density by (6) which is the same as (B-23a). When the bed is compressed, the packing density, c, is related to the reference density by

$$\Delta L_m = (c_0 / c)\,\Delta L_0 \qquad \text{(B-23b)}$$

which is the same as (9). Clearly, since c_0 refers only to the fiberglass, c must be the packing density of the fiberglass alone.

Ingmanson (reference B.40) is quoted as suggesting a relationship between the packing density and the head loss. The same author (different publication) is referenced in my book, for the same purpose, on page 204. It is also stated in the NUREG that "fibrous beds are highly compressible under the effect of differential pressure across the bed which acts as the compacting pressure". Despite the mechanism being compaction by the *pressure drop*, the equation given to describe compaction is

$$c = a\,c_0\,(\Delta H / \Delta L_0)^{\gamma} \qquad \text{(B-24)}$$

which relates compaction to the *pressure gradient* based on the original thickness. This is not the equation used by Ingmanson in reference 40 quoted in NUREG/CR-6224. Ingmanson's equation (4) is the same as the one in my book and it relates mat compression to the fiber pressure, not to the gradient of fluid pressure. It appears that the authors have been misled by loose use of the term "head loss" to insert the inappropriate pressure gradient into Ingmanson's equation rather than the actual particle pressure, as described in my book and used in the accompanying memo on flow through a compressible mat[2]. In that memo it is shown that the LANL data support the Ingmanson approach and do not support a relationship such as (B-24). Pressure gradient is an entirely different property from particle pressure. When a mat is compressed by an externally-applied load, as is a cushion when one sits on it, there is no pressure gradient.

In summary, there are three reasons for concluding that (B-24) is invalid:

1. It contradicts the principles of mechanics, which demonstrate that it is the fiber pressure, not the pressure gradient, that causes compaction of the bed.

2. It is inconsistent with the literature, including the reference quoted by the authors, which supports the mechanism set forth in 1 above.

3. It is not supported by the LANL data on mat compression, which are consistent[2] with the mechanism set forth in 1.

In (B-25) the coefficients a=1.3 and $\gamma = 0.38$ are used in (B-24).

In discussing the effects of particles and the limit to which a suspension of particles can be compressed to form a "sludge", the authors state, with no explanation: "a simple compression model was developed for mixed bed density, c,"

$$c = 1.3\ \rho_0\ (\Delta H / \Delta L_0)^{0.38} \text{ for } c < 65 /(1+\eta) \text{ lbm/ft}^3 \qquad \text{(B-27a)}$$

$$c = 65 \text{ lbm/ft}^3 \quad \text{otherwise} \qquad \text{(B-27b)}$$

The quantity "ρ_0" is not defined. In the rest of the document the symbol ρ is used for material density, which would make no sense in (B-27a). Perhaps this is a misprint for "c_0" which would make (B-27a) resemble (B-24). However, in that equation, c refers only to the fiberglass, whereas here it is called the "mixed bed density". 65 lbm/ft^3 is the density of the sludge and is supposed to be taken as the bed density given by (B-27a) when

$$c\ (1 + \eta) > 65 \qquad (13)$$

Now, if c is the fiberglass density, c $(1 + \eta)$ is the density (mass per unit total volume) of the fiberglass and particles and hence is the mixture density, so it appears that c is actually being used as the fiberglass density in (13). Yet there is no physical reason why either the fiberglass density or the bed density should equal the sludge density, since the sludge only comprises the portion of the bed between the fibers. Moreover, in order for (B-27b) to be compatible with (13), c must be the mixture density in (B-27b) and be the fiberglass density in (B-27a).

I note that on page B-46, (B-27a) appears as shown above. On page B-57, on the other hand, the criterion for use of (B-27a) is given as $c < 65$ lbm/ft^3. So in this context c is being used as the mixture density?

There appear to be two mistakes. One is to mix up the density of the mixture and the density of the fiberglass portion of the mixture so that it is unclear which one is being used. The other is the use of the criterion (13) which LANL have recast as (4). As discussed earlier, I believe the correct criterion should be (12). I would also recommend that "c" should be used consistently as the fiberglass density, not as the mixture density, in order to avoid misinterpretation and confusion.

I note in Figures B-14 and B-15 that the head loss versus velocity curves differ considerably depending on the source of the fibers and how they are prepared. The pressure drop for flow through "fibers" "shreds" or "air-blast" may differ by an order of magnitude (the differences between "air blast" and "small Shreds" in the international data base, shown in Figure B-23, appear to be smaller than this). This raises questions about what the appropriate correlation is for LOCA-generated debris, and how it relates to correlations based on fibers that have a different mode of preparation and deposition on a screen.

NUREG/CR-6224 Data

Bed Thickness

In NUREG/CR-6224, comparisons with data appeared to confirm the theory, whereas in reference 2 it was concluded that the LANL data favored the "classical simple theory". It is interesting, therefore, to reexamine the data presented in NUREG/CR-6224 in order to ascertain reasons for this apparent discrepancy.

The approximate correlation for water flowing through NUKON at 120F is given as

$$\Delta H/\Delta L_0 = 3.7U + 4.1\,U^2 \qquad \text{(B-30b)}$$

This was derived for an average bed compression of

$$1/R = (1 - \varepsilon_m)/(1 - \varepsilon_0) = 2 \qquad \text{(B-28)}$$

Since the viscous term in the head loss is proportional to $(I/R)^{0.5}$ and the inertia term is independent of compression, at low values of solids volume fraction, $(1 - \varepsilon_m)$, the actual correlation is represented by

$$\Delta H/\Delta L_0 = 2.62\ (1/R)^{0.5}\ U + 4.1\ U^2 \qquad (14)$$

According to the NUREG the bed compression is given by (B-24) or (B-25) which are equivalent to

$$(1/R)\ = 1.3\ (\Delta H/\Delta L_0)^{0.38} \qquad (15)$$

When (14) and (15) are solved for the head loss gradient versus velocity, the result is a unique curve. It is shown in Figures B-20 and E-20, which are the same.

The dashed curve in the Figures is the approximation based on an average 1/R=2, leading to (B-30b). For instance, at U = 0.15 the approximate head gradient is 0.647 and at U=1.5 it is 14.8. Using the more complete correlation, reflected in (14) and (15), it is found that 1/R = 1 at U =0.15 and 3.9 at U=1.5, The corresponding head loss gradients are 0.48 and 17, agreeing with both the figures and Table E-5.

Now, the classical theory, based on principles of mechanics, predicts that (15) is wrong. The compression should depend on the head loss and not on the head loss gradient. If we assume that (14) is a good fit at some average thickness, such as 1inch, then the true form should be more like

$$1/R\ = 1.3\ (\Delta H)^{0.38} \qquad (16)$$

or, using the correlation developed in reference 2,

$$1/R = 1.49\ \ (\Delta H)^{0.29} \qquad (17)$$

If (16) or (17) is used in (14) the head loss gradient does depend on the original mat thickness. Therefore if such a dependence is apparent in Figure E-20 and Table E-3, it should be possible to compare the two theories.

Looking at the figure and the table of data it appears that the head loss gradient is indeed larger than predicted for the thicker beds and smaller than predicted for the thinner beds. It is 18 for the 2inch bed, 16.5 (average value) for the 1inch bed, 12.4 for the 0.5inch bed and 12 for the 0.125inch bed. Also, extrapolation of the data for the 4inch bed to U = 1.5ft/s gives about 19, all in units of ft of water per inch of bed thickness. These may appear to be small deviations, but they are in the expected direction. Since only the viscous term is influenced by the form of the compression equation, we have to subtract the constant inertia term from these head loss gradients to determine what R needs to be in (14) in order to fit the data. This gives more leverage to the differences in overall head loss in their influence on the compression.

Table 2 lists these data points and the resulting computation of what R needs to be in (14) to fit them. It also lists the predictions from (16) and (17) of what the compression would be at the measured head loss.

ΔL_0	ΔH	$\Delta H/\Delta L_0$	R_{fit}	$R_{(16)}$	$R_{(17)}$
0.125	1.5	12	1.94	0.66	0.6
0.5	6.2	12.4	1.5	0.385	0.39
1	16.5	16.5	0.29	0.27	0.3
2	36	18	0.20	0.20	0.24
4	76	19	0.16	0.15	0.19

Table 2 Values of bed compression ratio predicted from NUREG/CR-6224 head loss data, compared with predictions from equations (16) and (17).

Had one accepted the NUREG/CR-6224 equation (15) the predicted value of R would have been a constant value of 0.26 for all of the bed thicknesses.

This evidence is rather weak, because of the inaccuracies and lack of repeatability (e.g. between P01 and P02) in these tests. However, the indication is that (15) is incorrect. Moreover, the predicted influence of the "correct" theory underestimates the necessary correction to R at low bed thicknesses. Therefore the evidence not only supports the "correct" theory but would suggest that the correction be even larger than is expected. If one only accepts the results for the 1, 2 and 4inch beds in Table 2, the evidence appears to support either (16) or (17) rather than (15).

This indirect method of predicting bed compression from the head loss measurements was necessary because, unlike in the LANL tests, no direct measurements of bed thickness were made.

It may appear that the difference in head loss gradient from the thinnest to the thickest bed is rather small, varying from 12 to 19ft/in. However, the term that varies is the viscous term. The inertia term is constant at 9.2. Therefore the variation in the viscous term is from 2.8 to 9.8, a factor of 3.5.

Since compression affects only the viscous term, it is most important to estimate compression correctly when the viscous term dominates the head loss. The ratio of the terms, from (14), for NUKON and water at 120F is $(2.62/4.1)(1/R)^{0.5}/U$. The ratio is also proportional to the specific surface area, S_v, and the square root of the reference solids fraction $(1-\varepsilon_{0m})$. For the LANL series 6 tests, with S_{vp}=450,000/ft, the value of S_v is 291,000/ft compared with the value of 171.000/ft for NUKON alone. The reference solids fraction increases form 0.014 for fiberglass to 0.0246 for the fiberglass-CalSil mixture. Then the ratio of the viscous to the inertia term is $(2.62/4.1)(291/171)(0.0246/0.014)^{1/2}(1/R)^{0.5}/U$. Using (17) as well as (30) from reference (2), which is $\Delta H = 35(UL_0)^{1.32}$ and includes the effect of bed compressibility, the ratio of the viscous to the inertia terms is then $2.94\ U^{-0.81}L_0^{0.19}$. For example, with L_0=1.1inch it is 11 at U=0.2ft/s and 6.3 at U=0.4ft/s, which is above the maximum velocity (0.393ft/s) tested by LANL in Test E.

As more particulate matter, such as CalSil, is added, the specific surface area and the reference solids fraction both increase and the viscous term becomes more dominant. Therefore it becomes more important to compute the correct compressibility of the bed, which influences this term.

Relaxation effects

Relaxation effects are a prominent feature of the LANL Series 6 tests. They appear to be the major source of uncertainty in predictions.

Relaxation is also apparent in the data reported in NUREG/CR-6224. It even occurs to some extent, over a period of about 2 or 3 minutes, for the NUKON alone tests in Figure E-19. In the 500% sludge tests, shown in Figure E-21, relaxation appears to occur over about 1000 seconds after the flow rate is incremented. At the highest flow rate (negative) relaxation leads to a drop in head loss. In the 5000% sludge tests relaxation is both positive and negative. It is particularly prominent when the velocity is increased from 1ft/s to 1.5ft/s. One would expect the head loss to increase by at least 50%. However, it first increases from 28ft to 40ft and then falls back to 30ft over about 1000seconds. In Figure E-24 this appears to be associated with a prediction that is about a factor of three above the data. The authors ascribe this to changes in the structure of the bed in the form of holes punched through it.

This negative relaxation is also associated with hysteresis. The head loss for decreasing flow rates is significantly less than for increasing flow rates in both Figures E-21 and E-22. At the lowest flow rates the head loss is reduced by an order of magnitude in Figure E-22. In the LANL report it was the other way around.

Both effects appear to be caused by changes in the bed structure. Though the effects are different in the NUREG and the LANL reports, they both indicate the importance of changes in the bed structure, which are not captured by a theory that treats the bed as a single "node" with average properties characteristic of a uniform bed.

Sludge limit

(B-27b) was introduced to impose a limit to bed compression. This appears to be invoked mostly for the thinnest beds and is due to the overestimate of compression because of the inappropriate appearance of L_0 in (B-27a).

Figure E-24 shows the effect of the suggested limit. The trend of increasing slope in head loss versus sludge-to-fiber mass ratio is stopped at some point. There is a kink in the curve and the head loss then increases at a more modest, essentially linear, rate.

The data points for the 1inch bed show no change in trend at the predicted kink. Those for the 0.5inch bed have only one point with an apparent fall off in head loss. The points for the 0.25inch bed show considerable drop in head loss, but this is probably associated with the 'punching of holes' mentioned above and is not a symptom of reaching a sludge limit to the bed density.

There must eventually be some limit to the compression of the bed, even when there are no particles in it. There just seems to be no clear evidence to support the actual criterion suggested in this NUREG.

What matters?

Review of the original NUREG/CR-6224 document did not change the conclusions reached earlier (pages 14 and 15) from my review of LANL's use of the NUREG/CR-6224 correlation scheme. The only qualitative difference discovered was that changes in bed structure could decrease the head loss by an order of magnitude rather than increasing it to a similar extent.

One may ask whether the conclusions matter for the problem of screen blockage following a LOCA.

There are two issues:

1. Should methods be endorsed which are based on false physics, as appears to be the case in the NUREG models for bed compression and the slurry limit?

2. Do the errors or uncertainties in the methods have sufficient impact to influence safety-related decisions?

My answer to (1) is an unequivocal "no". Technical credibility lost by accepting false methods fundamentally damages the agency in the eyes of its staff, industrial users, and the broader well-informed technical community. Errors in basic technical approach should always be acknowledged and corrected.

The answer to (2) depends on the effects of errors on the predicted screen head loss and the NPSH of the pumps. A head loss of 15ft may be tolerable whereas one of 30feet may entirely compromise the ability to recirculate water. If the head loss is predicted to be 1foot, it may not matter if in reality it is 5feet.

The order of magnitude of the influences of several effects may be estimated:

A. The power, "m", to which the particle fraction, $(1-\varepsilon_m)$ is raised in the head loss gradient is 1.5 in the NUREG (for fibers), 2 in Bird, Stewart and Lightfoot for particles, and 1.83 in reference 2, deduced approximately from the LANL head loss data. The effect on head loss in the viscous regime is $(1-\varepsilon_m)$ raised to the power m-1, because of the compensating effects of bed compression. Typically, $(1-\varepsilon_m)$ may vary by a factor of four, so the influence on head loss ranges from 2, through 3.16, to 4 depending on the value of m. The influence is greatest when the viscous term dominates, as it does for high particle/fiber ratios and low velocities. The maximum influence of the uncertainty in "m" is about a factor of two on head loss.

B. Compressibility of the fiber mat was the cause of the changes in $(1-\varepsilon_m)$ discussed above. The NUREG approach is inappropriate because of the inclusion of the factor L_0 (same as ΔL), in the denominator of (B-27a)). If the correlation were correct for a 1inch bed, then errors for a 4inch or 0.25inch bed would be a factor of $(.25)^{0.38}$ or 0.6 either way. When this is raised to the power m-1, the effect is less. This estimate does not account for the feedback to the compression due to the increased head loss. If we accept (31) in reference 2, the effect of length is a factor of $L_0^{1.32}$ rather than L_0. Therefore, the error for a 0.25inch bed is a factor of $0.25^{0.32}$, or 0.64. An engineering estimate of the combined influence of these two effects again leads to a factor of the order of 2.

C. The change in bed structure, termed "relaxation" in reference 2, noted in both the NUREG and the LANL report was observed to change the head loss by roughly an order of magnitude. There is no way yet to predict these effects and no way to estimate realistic limits to their influence. Therefore they clearly exert by far the largest effect on predictions and are prime candidates for comprehensive research. This should include the effects of the way in which the materials are prepared and how this relates to LOCA conditions.

References

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2. "Flow through a Compressible Porous Mat: Analysis of the data presented in Series 6 Tests reported by LANL in LA-UR-04-1227", Graham Wallis, 9/03/2004