EXPERT REBUTTAL REPORT
U.S v. BP Exploration & Production, Inc., et al.

Oil Release from the MC252 Macondo Well

Prepared on Behalf of the United States

Prepared by:

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EXECUTIVE SUMMARY

In my original report, I described the use of measured pressures and flow rates along with established correlations relating these in order to calculate the cumulative oil discharge from the Macondo well. I calculated a total discharge of 5.0 million stock-tank barrels (mmstb). I also calculated that the flow rate just following the blowout was approximately 63,000 stock-tank barrels per day (stbd), which decayed to approximately 55,000 stbd just prior to installation of the capping stack. Further, I validated my model through comparison with pressures measured during shut-in, comparison with measured flow rates of collected oil, and through prediction of the observed relationship between reservoir and BOP pressures over the 86 days of flow.

Several defense experts responded to my report. Here I address their criticisms through several topical areas. The areas are:

BOP Pressure Gauge Offsets and Accuracy: The analysis by Dr. Trusler largely validates my opinions regarding the BOP gauge and dispels the BP notion of an offset in the gauge of -966 psi. I have made additional calculations using his proposed corrections and find the value of the total discharge increases significantly. I also provide an alternative correction to BOP pressures for the period July 13 to 22 based on my conclusion here that gauge voltages provided to Dr. Trusler were already corrected to add 966 psi.

[redacted]

Use of Fixed Discharge Coefficients to Describe Two-Phase Flow: Here I provide additional evidence that use of a constant discharge coefficient properly and accurately relates actual flow rates to the square-root of the frictional pressure drop, even for parallel paths. I also show that I properly accounted for variations in gravitational pressure drops, either directly in my calculations or through my analysis of uncertainties.

Variation in Productivity Index and BOP Discharge Coefficient: Through additional discussion and analysis of BOP pressures, I demonstrate that neither the productivity index, the wellbore resistance, nor the BOP resistance could have varied by itself between May 8 and shut-in. I also show that it is not credible that several of these varied together in concert.

Flow Rates and Discharge Before May 8: I show that a rapid increase in the PI is consistent with the results of calculations by others and is consistent with the nature of erosion. I also provide additional evidence that erosion of the CSR, BSR and riser had little impact on flow rates, based in part on calculations by a defense expert. Through additional analyses, I also demonstrate that delayed failure and drop of the pipe below the BOP increases the cumulative discharge.

[redacted]

Use of Non-Commercial Software: As stated in my original report, I chose the methodology I did because geometries up to and through the BOP and riser are largely unknown. These are required inputs to commercial fluid dynamics models, so such models cannot produce accurate results. I confirm here that my empirical approach of characterizing the well through measured pressures and flow rates is accurate and that I properly validated this model.

Having read and considered the various defense expert reports, I do not feel that any of their criticisms are sufficiently valid so as to alter my estimates of flow rates or the cumulative discharge. I therefore confirm that my original analysis correctly describes flow from the well and that my best estimate of the total discharge remains valid.

BOP PRESSURE GAUGE OFFSETS AND ACCURACY

Background

Several defense experts addressed the measured BOP pressures used in my report. Some criticized my treatment of the data, while others validated it. After examining their analyses, I can confirm that my treatment of these data was appropriate and that the results presented in my report remain valid.

Dr. Johnson states flatly that "The offset that Dr. Griffiths calculated for PT-B is incorrect,"[1] and goes on to make a number of criticisms based on analyses that were originally done by BP during the top-kill period.[2] These analyses led BP to conclude that the PT-B gauge exhibited an offset of -966 psi, and Dr. Johnson asserts that this is valid. The flaw here, as discussed in my original report, is that the PT-C and PT-K pressures used in this analysis were inappropriately corrected according to a calculated pressure at the bottom of a mud column such that the resulting offset in the PT-B gauge is too large by about 100 psi.

[redacted]

In contrast to such general statements, Dr. Trusler undertook a careful examination focused entirely on this issue of gauge accuracy.[4] His analyses support my conclusion that the BOP pressures are generally reliable, and his proposed corrections are entirely consistent with mine for the period prior to July 9. He does however make a few statements that I believe are overly strong. I also feel that there is a better alternative explanation for behavior of the BOP transducer for the period from July 13 to 22.

Reply

In the event that Dr. Trusler is entirely correct in his analysis, I have recalculated the cumulative discharge based on the various corrections to the BOP pressures presented in his report. The result is a total discharge of 5.4 million stock-tank barrels (mmstb), an increase from my best-estimate of 5.0 mmstb.

BOP Pressures Prior to July 11

I find that the corrections proposed by Dr. Trusler seem reasonable and credible for the period prior to July 11. As he points out, these corrections are perhaps not "best" represented by a constant offset but instead vary somewhat with pressure. From a practical perspective, however, a constant offset is sufficiently accurate for my analysis, as demonstrated here through examination of Dr. Trusler's results. I therefore feel that some of his statements are too strong.

Dr. Trusler states that "fixed offsets are incompatible with the data" and that "it is not possible to reconcile the data fully by means of a simple offset model." While this may be true in the most literal sense, I disagree that fixed offsets are "incompatible" with the data. This is illustrated in Fig. 1 below. Here I calculated corrected pressures using Dr. Trusler's equations and used these corrected values to calculate his pressure-dependent offset relative to reported values for the periods of May 8 to 18 and May 19 to July 11.

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[1] Expert report of A. E. Johnson. Page 31, heading 4.6.
[2] BP-HZN-2179MDL02208359.
[redacted]
[4] Expert report of J. P. Martin Trusler.

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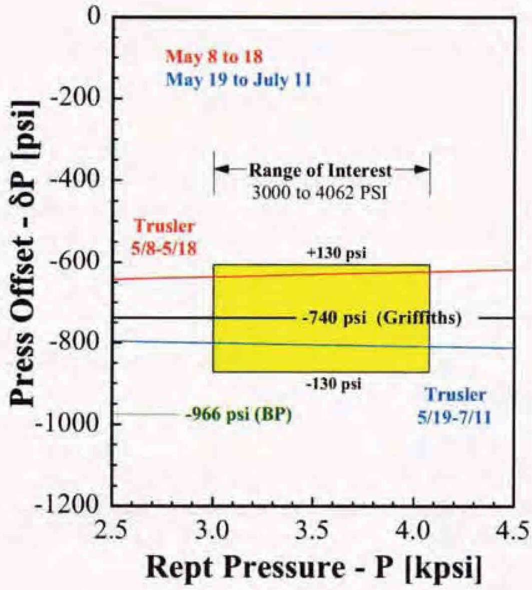
We see in Fig. 1 that Dr. Trusler's corrections correspond to one pressure-dependent offset for each of the two periods (red and blue lines). These are annotated in the plot. While these differ somewhat, they are each very nearly constant over the range of pressures of interest. This range is illustrated by the width of the yellow box, spanning the region from about 3000 psi to just over 4000 psi in reported values. This is the full range of pressures over the 86 days while the test rams were closed, up to installation of the capping stack. Over this range, Dr. Trusler's offsets for the period before May 19 (red line) span the range from -623 to -637 psi, a variation of just over 2.2%. These offsets are therefore almost constant over the range of interest. His offsets starting May 19 (blue line) range from just -800 to -810 psi, a variation of just 1.2%. So these offsets are also reasonably constant over the conditions of interest.

In contrast, I used a constant offset of -740 psi for both periods, which is very close to the midpoint of the two periods addressed by Dr. Trusler. My uncertainty in this value, as stated in my report, is +/-130 psi (height of yellow box). All of Dr. Trusler's offsets and corresponding corrections therefore fall within my range of uncertainty over the range of pressures of interest. And, the net impact of these differences is nil. Using Dr. Trusler's two corrections for the period before July 11 along with my best-estimate conditions thereafter yields a cumulative discharge of 5.0 mmstb. This is identical to my best-estimate value based on a constant correction of 740 psi.

Figure 1. Pressure-dependent BOP gauge offsets as calculated using the corrections of Dr. Trusler.

The range of pressures of interest is indicated by the width of the yellow box. The height of this box reflects the +/-130 psi uncertainty in the constant offset of -740 psi used in my original calculations.

Although the offsets of Dr. Trusler are not strictly speaking constant, they each vary by at most 2.2% over the range of pressures of interest.



The analysis by Dr. Trusler shows that the offset in the BOP gauge at the time of the top kill was approximately -800 psi based on reported BOP pressures of 2000 to 3000 psi while the test rams were open. This is consistent with the values of -810 to -866 psi I determined for that period and documented in my report (Table 1, Page 38). It is also consistent with my estimated offset of -740 psi for the period before July 11, differing by less that 60 psi from my value. As such, neither Dr. Trusler's analysis nor my own is consistent with the offset of -966 psi proposed by BP staff during the response and reiterated by Dr. Johnson.

Based on these observations, I conclude that all criticisms of Dr. Johnson and others relying on the BP offset of -966 psi are unfounded. I further believe that my constant offset of -740 psi with uncertainties of

+/-130 psi is fully justified in light of Dr. Truslers analysis. Finally, I believe that this uncertainty is appropriate given the calibration of the PT-B transducer.

BOP Pressures for July 13 to 22

I do not agree with the corrections to PT-B proposed by Dr. Trusler for the period July 13 to 22. This is not because I found related errors in his analysis, but rather because the data he was provided do not reflect the true output of the transducer. I instead believe that the "raw" voltages he was provided were already corrected by +0.1932 V, corresponding to a correction in BOP pressures of +966 psi. This is illustrated in Fig. 3 of Appendix A on page 23. My belief is substantiated by two facts: (1) the first appearance in the network diagram for July 15 stating that a correction of +966 was applied; and (2) the offset in voltage between July 8 and July 13 coincides almost exactly with the corresponding correction of 0.1932 V.[5] Based on this, I have developed an alternative correction for the period July 13 to July 22. This leads to an estimated effective resistance of 255.8 Ω, which is close to the nominal expected value of 250 Ω. In contrast, the correction of Dr. Trusler requires an effective resistance of 278.5 Ω, far from the expected value, and which he freely acknowledges is supported by no corroborating information. Details of this alternative correction are provided in Appendix A.

Using Dr. Trusler's corrections for the period before July 11 and my alternative correction beginning July 13, my calculated cumulative discharge is 5.1 mmstb. This is very close to my best estimate value of 5.0 mmstb and well within the range of uncertainty stated in my original report.

Capping-Stack Pressures (PT-3K-2)

Finally, Dr. Trusler examined corrections for the capping-stack transducer, PT-3K-2, and pressures from this gauge were also used in my analysis via the parameter estimation. For all pressures encountered during shut-in, the maximum correction he proposed is less than 35 psi. Such small uncertainties in the capping-stack pressure have no discernable influence on any results presented in my original report.

[redacted]

[5] From BP-HZN:2179MDL01514072. Exhibit 8690, page 20. The 966 psi correction is included in the only conversion presented for PT-B and the associated 301 address.

[redacted]

USE OF FIXED DISCHARGE COEFFICIENTS TO DESCRIBE TWO-PHASE FLOW

Background

A few defense experts assert that my results are somehow in error because I use fixed discharge coefficients to describe multi-phase flow in the wellbore, BOP, and various portions of the capping stack. One variant of this argument is that I segregate gravitational and frictional pressure drops, and that this is somehow inappropriate, especially for two flow paths in parallel. Another variant is that flow rates might not be proportional to the square-root of the pressure difference. In a related issue, one expert states that I assume a constant gravitational pressure drop or elevation head. Finally, one expert states that my discharge coefficients fail to account for the presence of gas in the two-phase mixture.

Reply

Gravitational Pressure Drop

I do not assume that the gravitational pressure drop (elevation head) is constant over the 86 days. Per Fig. 5 of my original report, the initial elevation head I use is 3120 psi, the difference between 11,850 and 8730 psi. The final head is 3050 psi, the differences between 10,310 and 7620 psi. My best-estimate calculations therefore account for variation in elevation head over the 86 days preceding shut-in. Further, I specifically address uncertainties in this variation through its influence on fluid density and its overall impact on the calculated cumulative discharge. This is shown in Table 1 next to the heading "Wellbore Density" and is discussed in Appendix G of my original report. The uncertainties associated with this are small, just -1.3% and +1.1%.

All numerical methods used in fluid dynamics for both single and multi-phase flows separate gravitational and frictional pressure drops, and only the frictional pressure drop is used to calculate the fluid speeds and associated flow rates. This is a fundamental tenet of fluid dynamics. To argue that this is in error or inappropriate is misleading. The only substantive issue here is how variations in the

[6] Exhibit 9905. Page 2.

[7] Exhibit 9453. Page 3, last line; Exhibit 9491. Page 2 (calculation by Adam Ballard).

gravitational pressure differences (same as elevation head) affect fluid properties and how these in turn might affect flow rates. As discussed above, the effect of such variation was in fact included in my calculations over the 86 days of flow. This variation indeed was not included in my calculations during shut-in, but it was most certainly included in my analysis of uncertainties. This appears in Table 1 of my report next to the heading "Head Variation During Shut In" and was discussed in Appendix G. The uncertainties associated with this are again small, -0.7% to +0.0%.

Constant Discharge Coefficients

It is also a fundamental tenet of fluid dynamics that fluid speeds are proportional to the square-root of the frictional pressure drop for turbulent flows.[8] An exception to this exists for a perfectly smooth pipe, but I do not believe that is relevant here, and this exception increases my calculated cumulative discharge in any case.[9] Because flow rates are proportional to fluid speeds, flow rates must also be proportional to the square-root of the frictional pressure difference. Here the only substantive issues are how densities and two-phase factors might vary as mean pressures vary and how such variations might indirectly affect fluid speeds and flow rates. I addressed this issue extensively in Appendix C of my original report. Moreover, the CFD calculations by Dr. Bushnell (Fig. 2 below) and the PROSPER modeling by Tony Liao of BP (Fig. 7 of my original report) both show a square-root relationship between frictional pressure drop and flow rate. As such, I do not believe that any criticism of my use of this square-root relationship is well founded.

In my original report, I provided in Appendix C a detailed technical basis for my use of constant discharge coefficients for the BOP and various segments of paths through the capping stack when present. That technical basis was established through calculations showing that discharge coefficients remained reasonably constant over a very wide range of pressure differences and flow rates for a wide range of published two-phase factors. In my opinion, the defense experts have not provided any substantive argument against that analysis, and I remain convinced that it is valid.

The comparison I made with Dr. Liao's calculations and presented in Fig. 7 of my original report is likewise valid. Again, flow rates in *all* numerical simulations are based on fluid speeds computed using the frictional pressure drop. In Fig. 7 of my report, I compared flow rates in the wellbore calculated by Tony Liao of BP to flow rates obtained using a constant discharge coefficient. Tony Liao produced a number of such results that I might have used. Some of his calculations did not include the pressure drop in the reservoir,[10] while others involved less reservoir depletion.[11] The case I chose was in fact selected because it would exhibit the most pronounced effects of two-phase flow. All of his other calculations involved higher bottom-hole pressures and so flows that more closely resemble single-phase behavior. I specifically chose the case I did because it would present the most challenging test of my use of a constant discharge coefficient. The comparison I presented in Fig. 7 illustrates the relationship between flow rate and frictional pressure drop. I believe this is appropriate and in fact is the only meaningful relationship to discuss in the context of fluid dynamics. I agree that gravitational pressure drops (elevation heads) do influence flow rates indirectly through their influence on fluid densities, and I have addressed this adequately through my uncertainty analysis. As such, I stand by my belief that flow rates can be related to frictional pressure differences through a constant discharge coefficient, and that the

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8 This is the same as saying that the pressure drop is proportional to the square of the fluid speed.

9 For the special case of a perfectly smooth pipe, fluid speeds can vary in proportion to the pressure difference to a power of 0.57 rather than 0.50. If 0.57 were used, my calculated cumulative discharge would increase very slightly. It increases also for a power of 1.0, but by less than 3%.

10 BP-HZN-2179MDL04869885.xls and BP-HZN-2179MDL04896196.xls.

11 BP-HZN-2179MDL04920969.xls, Exhibit 11145 at 5 (second case).

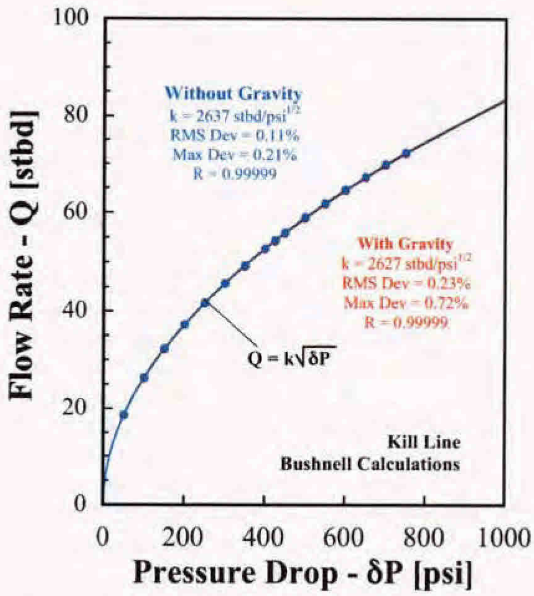
resulting calculated flow rates accurately reflect true flow rates over a range of conditions when discharge coefficients are determined from measured pressures and flow rates.

I have since checked flow rates through the kill line based on a constant discharge coefficient against those obtained using a full two-phase model. Dr. Bushnell calculated flow rates through the capping stack kill line for various pressure differences using a well-established ANSYS computational tool. He also employed a highly detailed description of the geometry, a sophisticated model of the turbulence, and a complete equation of state for the fluid based on the analyses of Dr. Zick.[12] This is illustrated in Fig. 2.

Figure 2. Flow rates through the capping stack kill line calculated by Dr. Bushnell (symbols) using ANSYS CFX and a fit to his results via a fixed discharge coefficient (curves). The pressure drop is the total pressure difference between the inlet and outlet.

Dr. Bushnell presented results with and without the inclusion of gravitational effects. These are indicated by red and blue symbols and curves, respectively.

Fits to both of his results are exceptionally good, exhibiting a maximum deviation of just 0.72%. The discharge coefficients obtained from the two fits differ by less than 0.4%.



Dr. Bushnell considered two cases, one in which the effects of gravity were included in his calculations and one in which these effects were not. Both of these are shown in Fig. 2. In both cases, he reported in his spreadsheet the total mass flow rate of both phases, not the flow rate of just the oil. In Fig. 2, I have converted these mass flow rates to reflect the flow rates of stock-tank oil using conversion provided in his expert report.[13] The flow rates he reported span a very wide range, from roughly 20,000 to 70,000 stbd, depending on the pressure difference.

Also shown in Fig. 2 is the fit to each of these results from Dr. Bushnell using a constant discharge coefficient relating flow rate to the square-root of the pressure drop. Despite the complexities and detail in his analysis, a constant discharge coefficient reproduces his results extremely well. The RMS deviation between the two fits and his two calculations are less than 0.3%, and the maximum deviation for either case is 0.72%. The discharge coefficients for these two cases are 2627 and 2637 stbd/psi$^{1/2}$. These differ by just 0.39%. They also differ by only 3.4% from my best-estimate value of 2551 stbd/psi$^{1/2}$ for the discharge coefficient of the kill line.

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12 K factor Kill Line.xls.

13 Expert report of Nathan Bushnell. March 22, 2013. The conversion I used is 451.0 stbd per kg/s total mass flow rate based on the values presented in his footnotes on page 52 and a value of 0.15899 m^3/bbl for conversion from cubic meters to barrels.

Parallel Flow Paths

One final concern raised about my analysis was whether a single fixed discharge coefficient adequately describes flow rates when there exist two flow paths in parallel as might exist with the drill pipe and annulus below the BOP. In this case, the pressures at beginning and end must be the same for both paths, hence the total pressure drop must also be the same for both. Because the average pressures are also the same along both paths, and the temperatures must be similar if the two paths are co-located, the average of the densities along both paths must be comparable provided the composition of the fluid entering each of the two paths is similar. As a result, the gravitational pressure drop along both paths must be comparable, so the frictional pressure drop along both paths must be comparable since this is the difference between the total and gravitational drops. In such a condition, the flow rate through the combined path is properly described by the sum of the discharge coefficients for each path alone. This sum provides the effective discharge coefficient for the combined path. That single value can then be used in series with any other discharge coefficient that appears along an overall path that includes the two paths in parallel. This effective discharge coefficient for two parallel paths represents an approximation to the relationship between flow rate and pressure difference, just like any other discharge coefficient that I use, and so is subject to the same sort of uncertainties resulting from variations in density or elevation head. But as shown elsewhere in this section, these uncertainties are quite small.

The preceding discussion is based on a presumption that the composition of the fluid entering each of the two paths is the same. This would be the case for bubble and slug flows, but might not be for flows exhibiting lateral segregation of the gas and liquid phases. If, for example, there was annular flow in the wellbore upstream of the start of the two paths, then it would be possible that gases predominantly entered one of the paths while liquid predominantly entered the other. I do not believe that this ever occurred in the Macondo well but will consider it here just for the sake of completeness. In such a case, the majority of the stock-tank oil could flow along the path carrying liquid while the majority of the gas could flow along the other path. Because my discharge coefficients describe the flow of stock-tank oil only, and this flow of oil would occur primarily along just one of the two paths, the effective discharge coefficient for the two paths in parallel would then be, in this case, the same as that of the single path carrying liquid. Here the path carrying gas is superfluous as it would not carry significant stock-tank oil. As such, a fixed discharge coefficient will also accurately describe flow through two parallel paths in which there is significant segregation of the gas and liquid phases between the two paths.

Results presented by Dr. Johnson purport to illustrate that a fixed discharge coefficient cannot accurately represent flow in parallel paths in fact do not support this at all.[14] Rather, the behaviors he describes result purely from artifacts of changing flow regimes in one or both of the paths. These changes, however, are highly suspect as it is clear his solutions suffer from many issues. For example, these solutions exhibit flow *down* the wellbore annulus in some cases, increasing flow rates with decreasing pressure gradients, flow rates through the much larger annulus that are smaller than those through the drill pipe, and extremely poor convergence of the solutions in some cases.[15] As such, I do not believe that his results discredit my methodology in any way.

Presence of Gas in Mixture

The magnitudes of my discharge coefficients are all tied directly to measured flow rates of stock-tank oil as determined by BP. They relate the flow rate of just this portion of the mixture to the pressure difference. The widely-used productivity index (PI) does exactly the same thing. That is, the PI relates

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14 Expert report of A. E. Johnson. Page 19, section 4.3.

15 Johnson Drill Pipe High-OLGAS~1.xls, Drill Pipe High-OLGAS~2.xls, Drillpipe Low-OLGAS~1.xls, Drill Pipe Low-OLGAS~2.xls, No Drill Pipe-OLGAS~1.xls, No Drill Pipe-OLGAS~2.xls, Johnson_Model_Output.xls. (Exports from Johnson's Appendix C drill-pipe models)

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the flow rate of stock-tank oil directly to the pressure difference. The presence of the other constituents, whether in the gas or liquid phase, is not neglected but is instead accounted for in the PI. My discharge coefficients do exactly the same thing.

Finally, stock-tank oil is an identifiable collection of molecules, regardless of how "stock-tank" is defined or measured, so a volume of oil at stock-tank conditions is in fact exactly equivalent to a mass of stock-tank oil.

VARIATION IN PRODUCTIVITY INDEX AND BOP DISCHARGE COEFFICIENT

Background

Multiple defense experts have commented on potential variation in the productivity index (PI) and the BOP restriction over the 86 days of flow. Most seem to acknowledge that the values I use at the time of shut-in are reasonably valid, but question use of these values at earlier times. In essence, they state that I ignored important variations in the state of the reservoir and BOP over the period of flow.

These comments take a great variety of forms and carry many related implications. Several experts noted possible changes in the state of the BOP based on its examination upon retrieval and either imply or state that I did not properly account for erosion of the BOP rams. One states that I did not account for closing of the various rams before May 8. Several also argued that the PI might vary significantly over all or some part of the 86 days and that I did not account for this in my calculations. Finally, at least one defense expert claims that my argument indicating that the PI remained relatively constant for the period of May 8 to July 15 is circular and therefore flawed.

Reply

My original report dealt with the periods before and after May 8 in two sections. This is because there are measured BOP pressures after May 8 but not before. Here I will deal only with the period May 8 onward. The period before May 8 is discussed in the following section.

An important preliminary note in this discussion is that my best estimate of the cumulative discharge is based on flow rates computed using the reservoir and BOP pressures and just the PI and wellbore discharge coefficient. This methodology already accounts for any potential variations in the state of the BOP through their impact on BOP pressures. No defense expert has questioned this. As such, any argument that my results are flawed because I did not account for changes in the state of the BOP between May 8 and July 15 is simply wrong. I did use a constant discharge coefficient for the BOP in my two alternate calculations but, as demonstrated here, this is well justified.

In my original report, comparison of my calculated best-estimate discharge and that of two alternate calculations led to my conclusion that the reservoir PI and discharge coefficients for the wellbore and BOP did not change significantly over the period from May 8 to shut-in. As illustrated in Fig. 3 below, my best-estimate calculation of the discharge uses the pressure difference between the reservoir and BOP and just the productivity index and wellbore discharge coefficient. This calculation automatically accounts for any changes in the BOP resistance, but not for changes in the reservoir PI or wellbore. My first alternate calculation uses the reservoir and ambient pressures along with the PI and both the wellbore and BOP discharge coefficients. This does not account for any potential changes in PI, wellbore, or BOP. The second alternate calculation uses the difference between the BOP and ambient sea pressures and just the discharge coefficient for the BOP. This accounts for any potential variation in the PI and wellbore.

but not in the BOP. Despite clear differences among them, these three distinct methods yield a calculated cumulative discharge that ranges from just 5.0 to 5.1 million stock-tank barrels, an incremental variation of only of +/-1%.

Now consider a scenario in which the PI and/or discharge coefficients for the wellbore and/or BOP might have varied. If any of these variations were significant, then the true discharge would differ from my calculated values in at least two of my three methods. If, for example, only the PI varied, then the discharge for my best estimate and first alternate methods would differ from the true value, but the first alternate using only the BOP discharge coefficient would not. Similarly, if only the BOP discharge coefficient varied, then both of my alternative calculations would differ from the true value, but the best-estimate value would not. And, if both the PI and BOP varied, then all three of my cases would yield a cumulative discharge that differs from the true value.

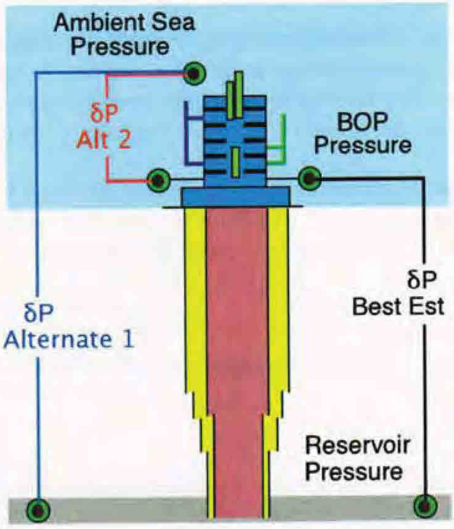
Figure 3 (Figure 6 of original report)

Schematic showing pressure differences (δP) used in my best estimate and alternative calculations.

Best estimate calculations are based on the difference between the reservoir pressure and pressures measured at the BOP using PT-B. This is illustrated by the black line on the right.

The first alternative calculation uses the difference between the decaying reservoir pressure and the fixed ambient pressure of ~2200 psi at the seafloor, as indicated by the blue line at the left.

Second alternative calculation uses the difference between the measured BOP pressures and ambient seafloor pressure. This is indicated by the red line on the left.



For each of the three methods, and for any such scenarios, the difference between the true discharge and my calculated value may be viewed as a discrepancy, and there is one discrepancy for each of the three methods. That is, there is one discrepancy between the calculated and true values for the best-estimate method and one for each of the two alternate methods.

A critical point here is that there is only one value of the true discharge. There is also effectively just one value of the calculated discharge because the best-estimate and two alternate methods are the same to within +/-1%. As such, the three discrepancies for the three methods are all nearly identical. This, however, places stringent constraints on what can and cannot have happened over the course of the 86 days. The reason for this is that one of these discrepancies properly accounts for any changes that might have occurred in the BOP but not elsewhere in the system, one accounts for changes in the PI or wellbore but not in the BOP, and one does not properly account for any such changes that might have occurred.

From the differing physical changes that the three methods account for, we can conclude immediately that the PI alone did not change significantly over this period, that the wellbore alone did not change significantly, and the BOP alone did not change significantly. If any of these had occurred, the three discrepancies could not be equal. Nevertheless, all three discrepancies are in fact the same.

I believe that there are just two possible explanations for this observation. The first is that indeed nothing varied significantly in the reservoir, wellbore, or BOP over the period from May 8 to July 15. As such, my calculated discharge is equal to the true discharge, the discrepancies are all zero, and so the discrepancies are all the same. This is the conclusion expressed in my report.

The second explanation is that somehow the PI, BOP and perhaps the wellbore all changed together in such a manner that the three discrepancies just happen to be the same. That is, whatever change in PI and/or wellbore that occurred had to precisely match a change in the BOP such that all three methods yield the same cumulative discharge within +/-1%. This requires either a truly fantastic coincidence or some collusion between the PI, wellbore and BOP that cannot exist given that these are independent physical entities. As such, I did not consider this to be a credible explanation at the time I wrote my report. Based on the additional analyses discussed below, I am further convinced that this alternate explanation is not credible because such fortuitous behavior is required on a more-or-less continuous basis over the period May 8 to shut-in in mid July.

The requirement that this behavior had to occur on a continuous basis follows from additional analysis of the BOP pressure history and the ratio of (1) the pressure drop between the BOP and ambient sea to (2) the drop between the reservoir and BOP. Details of this are discussed in Appendix C below. Based on observations presented there, I believe that the ratio of the overall resistance of the BOP relative to that of reservoir and wellbore remained unchanged throughout the period from May 8 to shut-in on July 15. In this case, any change in the PI or wellbore affecting flow rates would have to be very closely matched by corresponding changes in the BOP that occurred on a continuous basis over the entire period. Again, I do no believe that this is credible given that these are independent systems. Rather, I believe that this ratio remains nearly constant because the state of the reservoir, wellbore, and BOP all remained essentially unchanged in their influence on flow rates throughout the entire period from May 8 to July 15.

Finally, my conclusions for this period include any change in the state of the pipe extending below the BOP. If this pipe eroded and dropped from the BOP during this time, this would appear as a sudden, large, and permanent change in the ratio shown in Fig. 4 of Appendix C. No such permanent change occurs. I therefore also conclude that the pipe did not drop at any time after May 8.

FLOW RATES AND DISCHARGE BEFORE MAY 8

Background

Several defense experts have argued that my estimates of flow rates before May 8 are in error because I did not properly account for erosion in the BOP, did not taken into account closure of the various rams intended to seal the well, I concluded that the pipe below the BOP dropped into the well prior to April 29, or because I did not properly account for proposed gradual evolution in the productivity index. Dr. Nesic in particular argues that the blind-shear rams (BSR) offered significant resistance to flow early following their closure but that this resistance all but vanished by May 27. He additionally asserts that that the flow rate doubled between April 22 and May 27.

Reply

In contrast to statements by several defense experts, I did in fact consider closure of the various rams prior to May 8. Indeed my entire analysis of this period is based on a timeline tied to these events, as was presented in Appendix I of my original report. I also considered the possible impact of erosion on flow rates during this early period.

All of the comments offered by the defense experts center in some way or another on the issue of erosion or other evolution in the state of the reservoir or bottom of the wellbore, the BOP and riser, and the pipe below the BOP. As such, I will respond by addressing these distinct subsystems individually. Several experts have offered alternative time-lines for the evolution of these, which I will also address in the context of these subsystems.

Erosion in the Reservoir, Cement Plug, Reamer Shoe, Float Collar, etc.

In my original report, I concluded that the resistance to flow in the bottom of the well was falling rapidly just before the explosion on April 20, and I used this conclusion in my calculation of the flow rates prior to May 8. This conclusion was based on calculations made by Mr. Emilsen.[16]

The calculations by Mr. Emilsen employed Olga-Well-Kill to simulate flow into and from the well. In these calculations, he varied the height of the net-pay zone to match measured pipe pressures and arrival of gas at the surface from 16:00 to 21:50 on April 20. In this manner, he produced several scenarios, of which his Case 7 most closely reproduced the observations. His variation of the net-pay zone is equivalent to variation in the productivity index in his calculations, but in fact this variation serves only as a surrogate for any restriction that might have existed and varied at the bottom of the well. Dr. Emilsen acknowledged this in his deposition,[17] and I acknowledged it in my original expert report. As such, the work by Mr. Emilsen cannot lead to any conclusions regarding the productivity index (PI) per se. It does, however, show that resistance to flow in the bottom of the well was falling rapidly.

In my calculations of flow rates before May 8, I used a PI that increased to 43.8 stbd/psi over about 9 hours following the blowout based on extrapolation of the PI corresponding to the increase in net-pay height from Case 7 of Mr. Emilsen's report. I did this because he provided the only calculations tied to actual data. And, I used this PI in exactly the same manner as did Mr. Emilsen, as a surrogate for whatever might actually have been happening at the bottom of the well during this early period.

My extrapolation of Mr. Emilsen's values is based on two main considerations. First, his best match to the data showed that whatever restriction to flow that existed in the bottom of the well was vanishing quickly such that the surrogate PI increased by 25% in just 30 minutes. At this continued rate, I concluded that it would reach my best-estimate value of 43.8 stbd/psi in about 9 hours. Second, the pace of this increase is consistent with the pace at which other similar events were unfolding once the blowout was underway. We know with some certainty, for example, that the 5-inch pipe extending above the upper annular of the BOP was severely eroded, perforated, and failed completely within 36 hours from the start of the blowout. This did not occur in weeks or months, it occurred in a matter of hours. Because flow rates are the same throughout the system, I therefore believe that erosion in the reservoir, cement plug, reamer shoe, and/or the float collar would have occurred on a comparable time scale, hours or a few days -- not weeks, and this view is consistent with Mr. Emilsen's calculations.

The defense report of Dr. Johnson states that Mr. Emilsen's results show that this change was just a single step and that the two values of 13 and 16.5 feet for the net-pay height match all of the data over which each is applied.[18] I believe that this is a specious argument because Mr. Emilsen had no apparent

[16] From "Deepwater Horizon Accident Investigation Report," September 8, 2010. Appendix W, Case 7, Page 54. To match data and observations, the pay zone was increased from 13 and 16.5 feet, corresponding to effective productivity indices of 7.4 and 9.4 stbd/psi based on the nominal value of 49 stbd/psi and maximum pay zone of 86 feet used in that report. At this rate, the productivity index would reach 43.8 stbd/psi in 8.6 hours.

[17] Deposition of Morten H. Emilsen, December 8, 2011. See page 70 for example.

[18] Expert report of A. E. Johnson. Page 11.

capacity to vary the net-pay height in a continuous fashion. Mr. Emilsen simply observed that flow rates would have to increase to match the observed pressures, and he implemented this in his model in a step-wise fashion. I therefore believe that my extrapolation is valid. There was *something* at the bottom of the well that was causing the resistance in this region to fall rapidly, and there is no reason to believe that this rapid decay in the resistance would suddenly stop at 21:50 on April 20.

Dr. Johnson also presented two possible scenarios for the time-history of the PI (or other restriction at the bottom of the well) that might have occurred over the 86 days.[19] Both of these are hypothetical, and no physical basis is offered for either. In both cases, the PI varies from 10 to 43.8 stbd/psi over the 86 days, but differ in their time histories. In one case, the PI remains at 10 stbd/psi for 30 days, then increases linearly to the final value. In the other, it increases linearly over the entire 86 days. I do not believe that either of these is credible. As discussed elsewhere in my rebuttal, I believe there is sound evidence that the PI did not vary beyond May 8, just 17 days following the blowout. Moreover, both of the scenarios require that the rapid evolution of the PI determined by Mr. Emilsen suddenly stops or slows dramatically by April 21. This is also not credible in my opinion. Finally, I believe that the character of erosion when there are multiple resistances in series is such that it happens quickly when apertures are small and fluid velocities are high and that the rate of erosion drops dramatically as apertures are enlarged and fluid speeds fall. This character is not exhibited in either of the scenarios presented by Dr. Johnson.

Finally, Dr. Momber commented on my conclusion that the PI was increasing rapidly and showed a number of calculations suggesting that this would not be consistent with erosion of cement.[20] Dr. Momber misquotes my original report multiple times, however, to give the impression that I based this conclusion solely on erosion of the cement plug. In fact, I repeatedly state in that report that I do not know the nature of the restriction in the bottom of the well or why it was evolving rapidly. I only suggest, in a footnote, that the "likely mechanism" was erosion and never indicate that this was erosion of the cement plug. My only statements specifically about erosion of the cement were that if it happened, then it happened quickly. As discussed previously, I also believe that the results of Mr. Emilsen could equally describe erosion of the reamer shoe, the float valve, the skin surrounding the wellbore, or any other restriction that might have existed in the bottom of the well.

The analysis of Dr. Momber is also based entirely on the properties of fully-cured, fully-hardened cement. [redacted] As such, the cement would not either fragment or erode as described by Dr. Momber. It would simply be washed from the area by the flow of oil from the reservoir leaving some other restriction to limit flow rates. From this perspective, all of the results of Dr. Momber are irrelevant to what actually occurred following the blowout and so in no way contradict my conclusions.

In light of these discussions, I remain convinced that the PI increased rapidly beginning on April 20, reached the value I estimated within several hours or a very few days, and remained very nearly constant from that point until shut-in on July 15.

Erosion in the BOP

As noted in my original report, my best-estimate flow rates after May 8 are based on the reservoir and BOP pressures and just the PI and wellbore discharge coefficient. Through this methodology, any erosion in the BOP occurring after May 8 is already properly accounted for in my best estimate of the cumulative

[19] Expert report of A. E. Johnson. Page 12.

[20] Expert Report of Andreas Momber. [redacted]

discharge. My estimates of flow rates and discharge before May 8, however, are potentially influenced by such erosion.

This topic involves the state of the variable-bore and test rams, the casing-shear ram, the blind-shear rams, and the riser. As such it also involves a lengthy discussion, which I present in Appendix D. Here I will discuss just highlights of that discussion.

In my original report, I stated that erosion of the blind-shear rams (BSR), erosion of the casing-shear rams (CSR), and erosion in the riser kink did not significantly affect flow rates prior to May 8. I each case, I provided a basis for why I believed this. These arguments were all presented in Appendix I of my original report, starting on page 46. Dr. Nesic criticized these statements and provided a number of calculations intended to demonstrate that they are not valid.[22]

The analysis presented by Dr. Nesic purports to show rigorous calculations of erosion in the BSR, CSR, and riser and the impact of this erosion on flow rates. He concludes that the collective influence of erosion in the BOP led to a roughly doubling of the flow rate between April 22 and May 27 and that this flow rate increased almost linearly between these dates.

The calculations of Dr. Nesic suffer from three very serious problems and are presented in a manner that is misleading. First, he freely admits he does not know flow rates and instead uses bounding values of 5,000 and 65,000 stbd. He then claims that his results, when scaled, are insensitive to the flow rate. Based on his own model, however, this range of flow rates leads to a range of erosion rates that differ by a factor of 4,700. This is roughly the difference between 26 minutes and 86 days, and this difference cannot be scaled out. The relative pressures drops for a given geometry are in fact invariant to flow rate, as shown in his Fig. 33, but the erosion rates yielding the geometry between April 22 and May 27 certainly is not. The erosion he describes could have occurred in 26 minutes or 86 days based on his two bounding flow rates. In light of this, his "calibration" calculations are also meaningless: in those he would know the flow rate; here he does not. With such huge uncertainties in erosion rates, Dr. Nesic cannot make any valid statement about the extent of erosion or the timescale over which it occurred. Nevertheless, he claims that he "quantified the effect of erosion on flow rate and provided a timeframe over which it happened."

The second major problem with Dr. Nesic's analysis is that he assumes that all of the resistance to flow from the well resides in the BOP. Moreover, he assumes that this resistance does not include the resistance of the VBRs and test rams. These assumptions greatly exaggerate the impact of erosion on flow rates because he is eroding the only structures that control his flow rates. In reality, flow rates are determined mostly by resistance in the reservoir and wellbore. Even in the BOP, the CSR, BSR, upper annular, and the riser kink provide only about half of the resistance; the remaining half resides in the VBRs and test rams. This is discussed in Appendix D. A similar assessment was made by BP during the response. Based on pressures measured on May 8, they concluded that complete removal of the entire BOP along with the riser would increase flow rates by only 15 to 30% because of other resistances in the flow path.[23] Dr. Nesic's calculated "relative" flow rates are therefore meaningless.

The final problem in the analysis of Dr. Nesic is that the actual geometry of the BSR is not known because spacing between the two rams is not known. This spacing was estimated by DNV based on post-retrieval analysis in which recovered pipe was positioned between the rams.[24] That provides only an

[22] Expert report of Srdjan Nesic.

[23] Exhibit 9513.

[24] "Forensic Examination of the Deepwater Horizon Blowout Preventer" Report No. EP030842, page 164, Det Norske Veritas, March 20, 2011.

estimate of the *smallest* possible spacing between the rams and offers no indication whatsoever of their positions at any time of interest. Even small uncertainties in this spacing lead to large uncertainties in flow rates. Still, Dr. Nesic states that values he reports are "…"hard" numbers because the exact flow geometries are known…"[25]

Even given these problems, and despite his statements to the contrary, the results of Dr. Nesic do not contradict my statements regarding the CSR. His results per Fig. 33 show the relative pressure drop across the CSR remains almost unchanged from April 29 to May 27, a period of about four weeks. By his own estimate, the pressure drop through the CSR over this period fell by just 15%. Over the period from April 29 to May 8, this would correspond to a change of less than 5%, and this would affect flow rates from the well by much less than 1%. This is entirely consistent with my statement concerning the CSR.

Similarly, his results do not contradict my statement concerning the riser kink. Again per Fig. 33 of his report, the pressure drop through the kinked riser was not large compared to any of the other contributions to the total pressure drop through the BOP at any time, and his pressure drop through the riser remains about the same from April 22 to May 27. This is entirely consistent with my statements regarding the state of the riser expressed in my report and used in my calculations of flow rates. Any parsing of how much erosion occurred in the riser or when this occurred is therefore irrelevant to the results expressed in my original report. The riser simply did not at any time present a significant resistance to flow from the well, with or without leaks in the vicinity of the kink.

Finally, Dr. Nesic makes a number of strong statements regarding erosion in the BSR and its impact on flow rates that in fact do contradict statements in my original report. The magnitude of this impact, his doubling of the flow rate, is however without any basis because he does not actually know the true spacing of the BSR rams. It is also much too large an effect because of his assumption that all resistance resides in the BOP. Finally, he cannot possibly know the rate of change of this flow rate between April 22 and May 27 because of the huge uncertainties in erosion rates.

The basis for my statement that I do not believe erosion in the BSR significantly affected flow rates prior to May 8 is the very low pressure drop, just 52 psi, that was measured through the BSR on May 25.[26] At this time, the pressure drop across the CSR was about 600 psi, that across the middle VBR was about 400 psi, and that across the test rams was about 500 psi.[27] I do not believe that erosion of the BSR could have led to this condition. With the multiple resistances in series that are all eroding, any erosion that affected flow rates would tend to make all of the pressure drops the same, and this is clearly not the case for the BSR. To reach this condition on May 25, I believe that the pressure drop through the BSR had to be small from roughly the time it was closed on April 22. I therefore concluded in my original report that the BSR never offered any significant resistance to flow and so any erosion of the BSR also would not affect flow rates. I remain convinced of this.

Pipe Drop Into the Well

Several defense experts have questioned my opinion that the drill-pipe extending below the BOP was severely eroded and either perforated or dropped into the well as of April 29, three days following closure of the test rams where the pipe was eventually failed. This opinion is based on the fact that identical pipe failed at the upper annular in less than 36 hours, as compared to three days, and that the geometry for flow

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[25] Expert report of Srdjan Nesic. Page 36.

[26] BP-HZN-2179MDL02208359.

[27] BP-HZN-2179MDL02208359. The pressure drop across the test rams is listed as 725 psi based on a correction to the BOP gauge of +966 psi. Using my correction of -740 psi, the drop across the rams is 499 psi.

through the leaking annular was quite similar to that for flow through the leaking test rams. In another section of this report, I also provide evidence that the pipe did not drop any time after May 8.

I do believe that that pipe dropped before April 29 and use this opinion in my estimate of the flow rate for the period of April 29 to May 8. This early drop does not, however, lead to high estimates of the cumulative discharge. On the contrary, my estimate of the total discharge would increase if the pipe dropped later on.

When attached to be BOP, the entire length of the pipe resides within the 9-inch portion of the casing and so offers a relatively large annulus for flow between the pipe and casing. When dropped into the well, at least some portion of the pipe must reside within the 7-inch casing, significantly reducing the size of the annulus. As such, pipe drop would increase the resistance of the wellbore, reducing the wellbore discharge coefficient. Since my estimate of the wellbore discharge coefficient is based on conditions at shut-in, when the pipe had already dropped, my estimated value would be too low if the pipe were still attached to the BOP. As such, my estimate of the discharge would also be too low if the pipe remained attached to the BOP over an extended period.

To illustrate this, I have calculated the discharge for various pipe-drop times of 20, 40, and 60 days from the blowout. These yield discharges 5.1, 5.3, and 5.5 mmstb in contrast to my best-estimate value of 5.0 mmstb.[28] From my perspective, the entire issue of when the pipe dropped is therefore academic as I would not defend these higher values of the discharge because I do not believe that they are consistent with what actually occurred in the well.

[redacted]

[28] These values are based on my best-estimate values of the PI as reported in Appendix D of my original report. The wellbore discharge coefficient before pipe drop is taken as 1410 stbd/psi$^{1/2}$ per Tony Liao's calculations (BP-HZN-2179MDL04920969.xls) and my best-estimate value thereafter.

[redacted]

[redacted]

USE OF NON-COMMERCIAL SOFTWARE

Background

Several defense experts questioned my use of specialized software developed just for the Macondo well and suggest that it would be more appropriate to use general-purpose commercial software to model the flow.

Reply

I chose the methodology and model employed in my study because I felt that it would provide the most accurate description of flow rates, both at shut-in and over the 86 days, and that it would therefore provide the most accurate estimate of the cumulative discharge. I have developed similar software dozens of times over my career at Sandia, addressing a broad range of such specialized problems. I also hold two software copyrights and have licensed both of these models commercially.

The reason I did not use commercial software is that the true physical dimensions and geometry of the BOP, float collar, and drill pipe in the well are not sufficiently well characterized to enable accurate calculations of flow rates using the methodologies employed in commercial software. Such dimensions are required inputs for all commercial software, and if these input dimensions are not accurate then the calculated flow rates are not either. The productivity index is also not known accurately from measured permeabilities because the geometry of the flow path between the reservoir and wellbore is not well characterized. My methodology does not require these dimensions or the PI because flow rates through the reservoir, wellbore, and BOP are all characterized empirically using pressures and flow rates measured on the actual system.

Confidence in the results of any numerical model, commercial or otherwise, must be based on validation of the model through comparison with actual data. My methodology and the resulting software I developed are built on such data from the ground up. Beyond this, however, I validated my model by demonstrating that it accurately reproduces all of the BOP and capping stack pressures measured during shut-in, that it accurately reproduces measured flow rates of collected oil under multiple conditions, and that it accurately predicted the observed relationship between pressure decay rates in the BOP and reservoir over the 86 days of flow. In contrast, I cannot find a single example in the defense reports of Dr. Johnson, Dr. Nesic, and [redacted] in which they validate their models against *anything* actually measured on the Macondo well. They instead resort simply to statements that they use "industry standard" models. That may be true, but it is not a sufficient basis for confidence in their results.

Finally, I do recognize the value and importance of commercial general-purpose fluid dynamic software. When geometries and dimensions are well defined, the fluid is well characterized, and the results are properly validated, these computational tools are invaluable in system design and evaluation across many industries. Here, however the geometries and dimensions within and below the BOP are not known accurately, so these commercial tools cannot produce accurate results. In contrast, my methodology and the resulting software I developed represents a specialized model that describes the Macondo well exactly as it existed. It is not a general-purpose tool and would not be useful in that capacity. I do believe, however, that it provides the most accurate description possible for flow in the Macondo reservoir, wellbore, BOP and capping stack.

Statement Regarding Opinions

The opinions expressed in this report are my own and are based on the data and facts available to me at the time of writing. Should additional relevant or pertinent information become available, I reserve the right to supplement the discussion and findings in this report. The information required by the Federal Rules of Civil Procedure was provided in my original report and has not changed. A list of considered materials is provided in Appendix F.

APPENDIX A: ALTERNATIVE CORRECTION OF BOP PRESSURES IN THE PERIOD JULY 13 TO 22

As stated in my report, I believe it is likely that a correction of +966 psi was added during this period to the conversion from voltage to pressure at the system level. The basis for this belief discussed in my report is: (1) the sudden large increase in the reported BOP pressure at this time; and (2) the very first appearance of this correction in the network status diagram on July 15.[31] Based on the analysis presented here, I believe that this correction was implemented via the addition of 0.1932 V to the true output of the PT-B transducer prior to the time the "raw" voltages were recorded.

Given this state of things, the true voltage output V' from the transducer and resistor can be expressed as

$$V' = V - 0.1932 = \left[\frac{(P-966)}{1250} + 4\right]\frac{R_0}{1000}$$

where, V is the reported voltage, P is the reported pressure, and $R_0 = 250\ \Omega$ is the reference resistance. The factor of 1000 in this expression yields the voltage in Volts. From this, the true current i' can be calculated using an effective overall resistance, R_{eff}.

$$i' = 1000\ \frac{V'}{R_{eff}} \tag{2}$$

Here the factor of 1000 yields the current in mA. From this true current and the DNV calibration curve per Dr. Trusler, the true pressure P' is given by

$$P' = 1234.8\ (i' - 4) + 673.3 \tag{3}$$

These three equations describe the correction of the reported pressure, P, to the true pressure, P', once the value of the effective resistance is known. To obtain this value, I simply matched the reported BOP pressure of 7224 psi per PT-B to the capping stack pressure of 6605 psi per PT-3K-2 as of 14:22 on July 15. The result is R_{eff} =255.8 Ω. This value differs considerably from the value of 278.485 Ω obtained by Dr. Trusler, but it is much closer to the expected value of 250 Ω. This, I believe, adds credibility to my alternate correction.

Despite the large difference in effective resistance, the two methods yield very similar conversion between the reported and true BOP pressures. This is illustrated in Fig. 1. Here the true pressures from the two methods are plotted as a function of the reported value. Dr. Trusler used in his analysis the extrapolation of earlier pressures to July 14 using the long-term pressure trend. This yields a true pressure of about 3810 psi at the reported pressure for that time of 4112 psi. My correction yields a lower value of about 3590 psi. This value, however, is consistent with extrapolation of the earlier data using the trend only from day 60 on. This difference at low pressures therefore does not provide a basis for deciding which method is correct.

[31] From BP-HZN-2179MDL01514072. Exhibit 8690, page 20. The 966 psi correction is included in the only conversion presented for PT-B and the associated 301 address.

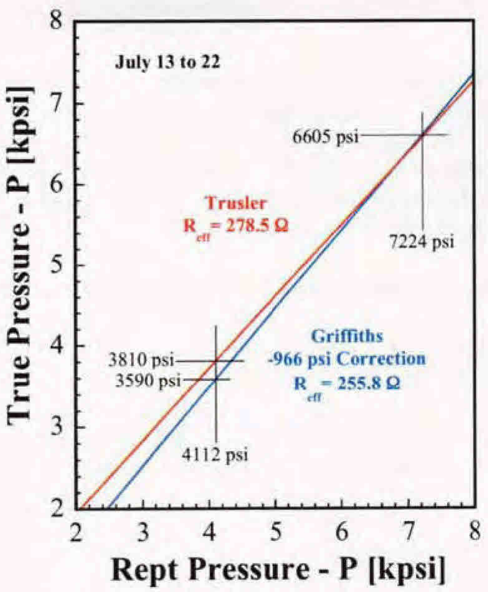


Figure 1. Comparison of Trusler and Griffiths corrections BOP pressures in the period July 13 to 22.

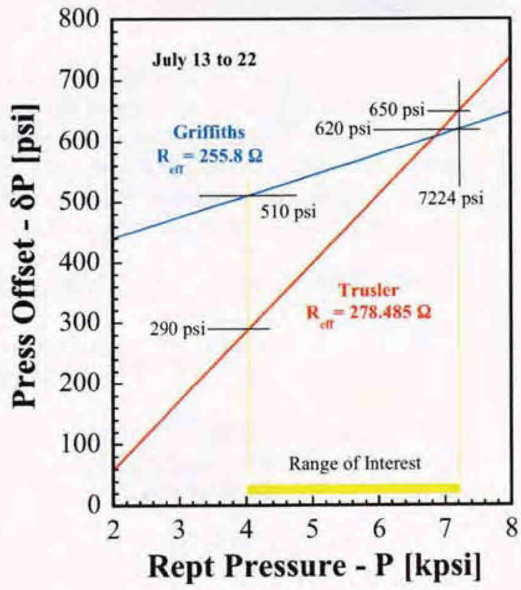


Figure 2. Pressure-dependent offsets for Trusler and Griffiths corrections of BOP pressures for July 13 to 22.

While the corrected pressures resulting from the two methods agree fairly well, the apparent offsets and impact on transducer sensitivity differ very significantly. This is illustrated in Fig. 2. Here the offsets are calculated by subtracting the pressures calculated by each of the two methods from the pressures reported by BP. We see that the offset per Dr. Trusler varies quite a lot over the range of interest, roughly 4000 to 7200 psi. At these extremes his apparent offset varies from roughly 290 to 650 psi, with a mean over the range of 470 psi. The error in sensitivity for his correction is 11.3%. In contrast, my correction yields a range from just 510 to 620 psi, and has a mean of 565 psi. My constant offset of 620 psi is thus in error by at most 110 psi, which has negligible impact on my results, and differs from the mean value by just 55 psi. Here the error in transducer sensitivity is just 3.5%. Again, I believe this adds credibility to my method because the sensitivity is close to the expected value.

Finally, I consider the reported voltages for the period between July 1 and July 17.[32] These are shown in Fig. 3. Prior to July 1, the reported voltages exhibited a downward trend with continuous oscillations on the order of +/-0.04 V. This behavior continues from July 1 through July 8 as shown. Beginning July 9, the voltages clearly become erratic, leading to replacement of the battery on or just before July 13. I neglect this data in my analysis, as did Dr. Trusler. These neglected values are indicated in red. Also neglected in my analysis here is the data after July 14 at 16:32. This corresponds to first closure of the capping-stack ram, the start of various manipulations on the choke and kill valve, and ultimately shut-in of the well. The variation during this period is thus due to true variations in the pressure.

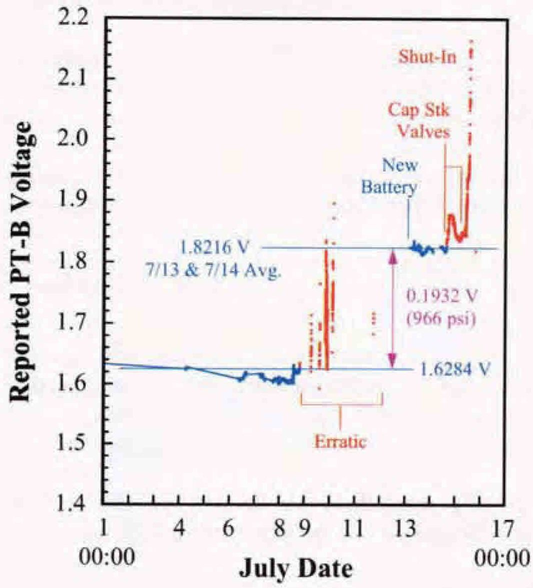
32 The voltages I show in Fig. 3 are taken from the file "PT-B Final Pressures.csv" of the modeling runs by Dr. Trusler.

Figure 3. Reported "raw" voltages for PT-B in the period July 1 to 17.

Values in blue are considered in this analysis. Values in red are not.

Voltages became erratic on July 9, leading to replacement of the battery. Dr. Trusler and I both neglect this data for this reason. Voltages starting at 16:30 on July 14 are influenced by closing and opening the ram and valves on the capping stack and so are likewise neglected in my analysis.

This plot is equivalent to Fig. 6 of Dr. Trusler's report.



The average voltage for the period July 13 through 14 at 16:30 is 1.8216 V, as indicated in the figure. Correcting this value by -0.1932 V, corresponding to 966 psi, yields 1.6284 V. This is also shown in the plot. Now we see that this aligns almost exactly with the voltages of July 1 to 8. This is not a coincidence. The pressures on July 8 and July 13 must be almost identical, so this is simply clear evidence that 0.1932 V was added to the true voltages before they were recorded. The reported values are thus not "raw" voltages but are instead already corrected to add 966 psi to the reported pressures.

Confidential per BP

TREX-011486R.0023

[redacted]

TREX-011486R.0024

[redacted]

Appendix C: Analysis of BOP Pressure History and Implications for Variations in PI

The specific criticisms offered by the defense experts involve greatly differing technical opinions and the use of multiple models, assumptions, and scenarios. In such cases, I believe that the best course to satisfactory resolution is to examine the data and let it speak for itself with as little modeling and the fewest assumptions and scenarios as possible.

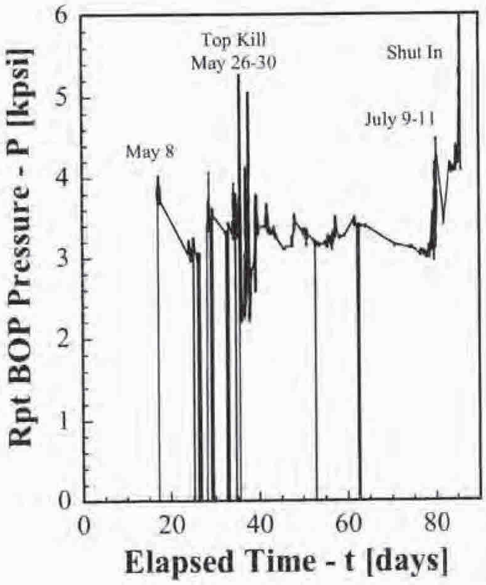


Figure 2. Raw BOP pressures as reported by BP. All available data for this period is shown. Vertical lines extending to zero pressure represent dropouts in the data, brief periods typically a point or two for which the recorded voltage is zero.

Spikes in the pressure during the top-kill period from May 26 to May 30 correspond to periods of mud pumping.

Pressures for July 9 to July 11 are untrustworthy and led to replacement of the gauge battery on July 12.

The rise in pressure beginning after July 11 corresponds to shut-in of the well.

Measured BOP pressures are shown in Fig. 1. These are uncorrected pressures just as reported by BP.[34] As discussed elsewhere in this report, the pressures starting July 9 are unreliable, leading to replacement of the battery on July 12. BP expert Dr. Trusler and I agree on this. Pressures starting July 14 reflect shut-in of the well and so are of no interest here. For the moment, I will discuss only changes in BOP pressures, so any corrections or offsets do not yet need to be considered.

The raw pressures show numerous dropouts and a number of spikes during the top-kill period that obscure overall trends in the data. To remedy this, I have calculated daily average pressures using the values of Fig. 1. The purpose of this averaging is simply to eliminate noise and smooth brief spikes in the data such that trends are more easily seen. This is especially helpful for the period of the top-kill activities, during which the raw pressures exhibit such extremes due to pumping mud that it is pretty much impossible to discern anything. To ensure validity of this process, I did not include dropouts in the averages.

Figure 2 shows these daily averages for the period May 8 through July 8, spanning 62 of the 86 days. Not every day had reported values, so there are a few small gaps. Again, these are uncorrected pressures subject to offsets, so changes in these pressures are meaningful even if the absolute values are not. Note that these averages very accurately reflect the data of Fig. 1.

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[34] Measured BOP pressures are from the BP files: "MC252_DataDump_071810.xls", SNL087-001206; "05_14 - 23 May.xls", SNL088-072912; BP-HZN-2179MDL06336851; BP-HZN-2179MDL06089077; and "PT_B Offset 2 15 Jun thru 14 Jul.xls", SNL022-007753.

Figure 2. Daily averages of BOP pressures from May 8 through July 8.

The mean drop in pressure relative to the trend during the top-kill period is 680 psi, which corresponds well with estimates of the pressure drop across the closed test rams as of May 25.[35]

The blue line is a least-squares fit the pressures shown, excluding the top-kill period. The dashed red lines show variations about this fit of +/- 200 psi.

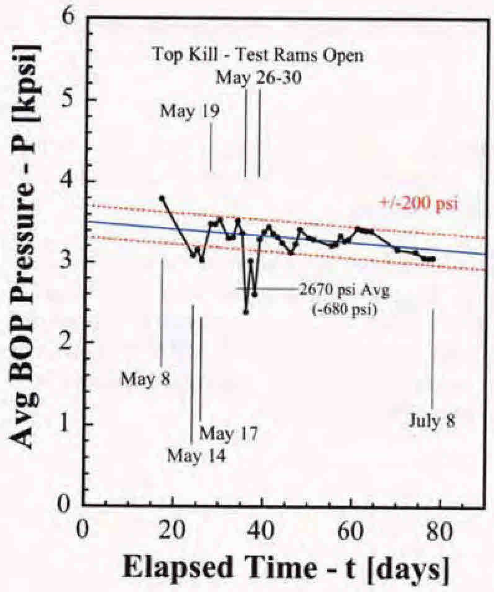

Figure 2 exhibits several significant features: (1) the drop in pressure following May 8 and subsequent rise on May 19; (2) the temporary drop in pressure during top-kill activities; (3) a general decline in the pressure associated with decay of the reservoir pressure; and (4) mild oscillations having a magnitude of at most about 200 psi. It is also noteworthy that the pressure of May 8 and May 14 to 17 fall just outside the range of these oscillations, while the temporary drop during top-kill falls well below. Most of these features will be touched on shortly.

Absent assumptions, models, and scenarios, the resistance to flow along any path is represented by the pressure drop between two points. That is, a higher resistance requires a larger pressure drop to drive a given flow rate. The overall effective resistance between the reservoir and BOP is thus represented by the pressure difference between these two locations. And, the resistance to flow between the BOP and ambient sea is represented by the difference in pressures between those two locations. The ratio of the overall effective resistance in the BOP to that in the reservoir and wellbore is thus represented by the ratio of the pressure drop between the BOP and ambient to that between the reservoir and BOP.

This ratio is shown in Figure 3. Here the ratio is calculated using the pressures of Fig. 2 above, the reservoir pressure history per Fig. 5 of my original report, and an ambient sea pressure of 2198 psi. Here the BOP pressures are corrected using both my constant value of 740 psi and the corrections of Dr. Trusler for each of his two periods covered by this data.

Like the pressures of Fig. 2, the calculated ratio of pressure drops shows several interesting features: (1) an abrupt drop from May 8 to May 14 followed by an increase on May 19; (2) a large drop and then recovery during the top-kill period; and (3) some periodic oscillations. Unlike the pressures of Fig. 3,

[35] Per BP-HZN-2179MDL02208359 the pressure drop across the test rams is 725 psi. This however is based on the flawed +966 psi correction to the BOP gauges. My correction of 740 psi would place the value at about 500 psi. Per MC252_DataDump_071810.xls, SNL087-001206, the rise in pressure when the test rams were closed at 04:08 on May 30 is 440 psi.

however, there is no obvious trend. The horizontal line through the values is the average over the period, excluding top-kill. Here this is provided just a visual reference. I will discuss each of these features shortly, but will begin with the top-kill period to illustrate an important point.

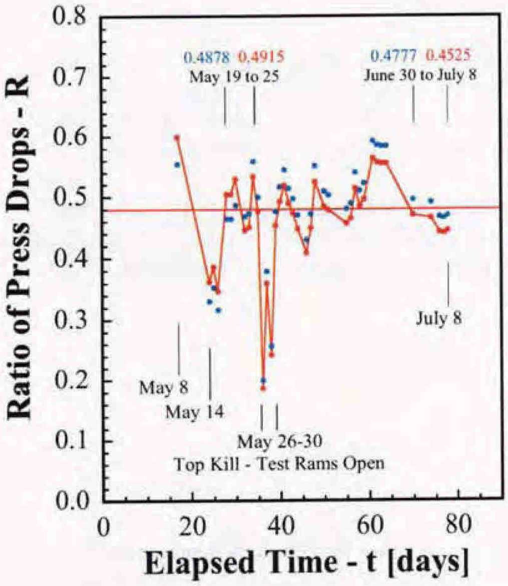

Figure 3. Ratio of the pressure drop between the BOP and ambient sea pressures to that between the reservoir and BOP.

Values in red employ a constant correction of 740 psi for BOP pressures, as used in my other analyses. Values in blue employ BOP pressures corrected per the expert report of Dr. Trusler. The two corrections show only minor differences in the ratio of pressure drops.

The ratio of the two pressure drops falls suddenly during top-kill on May 26 but then recovers to its original value on May 30. The reason for this drop and subsequent recovery is that the BOP test rams were opened on May 26 and closed early on May 30, and the ratio reflects this. When the test rams are opened, the effective resistance of the BOP is reduced, the effective resistance of the reservoir and wellbore are unchanged, and so the ratio of pressure drops falls. When the rams are closed, the resistance of the BOP relative to the reservoir and wellbore returns to its original value, and so does the ratio of pressure drops. The important point here is that measured BOP pressures recognized that the rams were opened and adjust accordingly to reflect the lower resistance of the BOP relative to that in reservoir and wellbore. Any other change that occurred in the system between May 8 and July 8 would show a similar spike either up or down depending on what had happened.

Now consider the two periods just before the top-kill ending May 25 and the period ending July 8. The average of the ratio for the period May 19 to 25 is either 0.4879 or 0.4915 depending on the Trusler corrections or my constant correction of 740 psi. The averages for June 30 to July 8 are 0.4777 and 0.4525, respectively. Between these two periods, spanning 51 days, these averages differ by just several percent. The Trusler values in fact differ by only 2%. From this I conclude that there was essentially no change in the effective resistance of the BOP *relative* to that of the reservoir and wellbore over this entire period.

The fact that the resistance of the BOP relative to that of the reservoir and wellbore remains constant does not require that anything remained constant over this period. There are in fact two possible explanations for this behavior. The first is the that the resistance of the BOP and that of the reservoir and wellbore did in fact remain constant over this time and, as a result, the ratio of the two resistances remained constant. This is the simple obvious explanation. The other is a bit more involved.

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The second explanation is that BOP resistance and the effective resistance of the reservoir and wellbore *both* varied is just such a manner that their ratio remains unchanged. For example, the PI might have increased over this time such that the effective resistance of the reservoir and wellbore fell by half. But this would have to be accompanied by a reduction of resistance in the BOP by the same factor of two, and this change in the BOP would have to match that in the balance of the well to within a few percent. This seems highly fortuitous. Further, the ratio remains relatively constant over all of the intervening period, save for the oscillations, so whatever changes might have been taking place in the reservoir or wellbore would have to have been matched by changes in the BOP of just the right sort on a continuing basis throughout the period. This seems to me even more unlikely given that the reservoir and BOP are independent systems containing distinctly different materials, having very different fluid speeds, and are separated by roughly three miles. Finally, if the proposed change in the reservoir or wellbore is large, then the change in the BOP required to maintain a constant ratio of the two resistances is also large. Large changes that just offset one another is still less believable.

In my extensive experience with physical systems of all sorts, simple explanations for simple observations are almost always correct; complex explanations for simple observations almost never are. To illustrate: Consider flying in a plane. In a plane you have a groundspeed, your speed relative to the ground, and you have an airspeed, your speed relative to the air. These are not the same and differ by the speed of any head or tail wind. Now if at some time your groundspeed is say 400 knots, and some time a bit later it is 400 knots, and later it is still the same, and so on, then there are two possible explanations. The first is the simple explanation that your air speed and the wind speed are constant at these various times, hence your ground speed also remains constant. The second is that your airspeed *and* the wind speed both varied together in just such a manner that the groundspeed remained constant over some extended period at multiple points in time. And, this would have to happen just by chance as the airspeed and wind speed varied. This is explanation that lacks credibility because it requires a continuing correlation between two things that are not related.

Finally, I will discuss briefly the oscillations in Fig. 3 and the period before May 19. Between May 8 and May 14, the ratio of pressure drops falls very significantly, and this indicates that the resistance in the BOP fell relative to that in the reservoir and wellbore. This conceivably resulted from some sudden and real change in either resistance or might conceivably result from expected inaccuracies in the BOP pressures. If real, this sudden fall could represent a drop in BOP resistance or sudden increase in the reservoir and wellbore resistance. The first of these seems most likely. In this case, the drop in BOP resistance might be, for example, the drop in the pipe below the BOP or sudden failure of the seal in one of the rams. The problem with both of these is that they are irreversible, while the ratio of the pressure drops largely recovers on May 19. As such, I can offer no physical reason for this behavior.

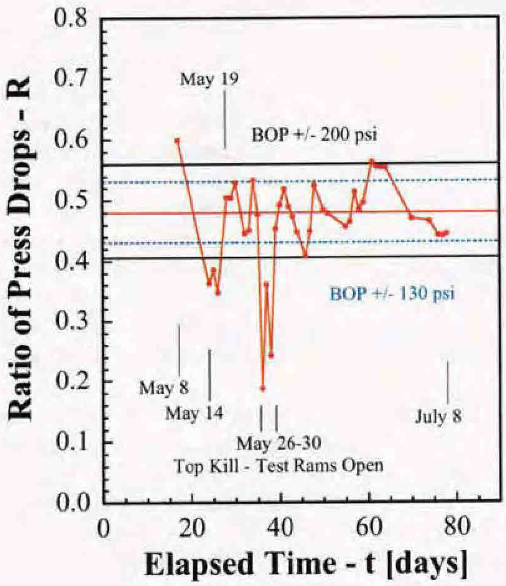
I do find it peculiar that the May 8 value of the ratio is well above the trend line, the May 14 values is well below, and the May 19 value lies very close to later values. And, the average of the May 8 and May 14 values also lies close to the later trend. This suggests that these might result simply from expected and bounded inaccuracies in the BOP pressures.

To explore this possibility, I have calculated the ratio of pressure drops using additional BOP pressure corrections of +/-130 psi and +/-200 psi to represent uncertainties. The first of these is my estimate of the accuracy in my original report; the second has been proposed by others, including Dr. Trusler. The results of this are shown in Fig. 4. Here my values from Fig. 3 are shown along with several lines. These lines represent average values of the resulting ratios for the nominal values and the two corrections of +/-130 psi and +/-200 psi.

Figure 4. Error bands on ratio of pressure drops based on uncertainties in BOP pressures of +/-130 psi and +/-200 psi.

Solid black lines show uncertainties for +/-200 psi. The dashed blue lines are for +/-130 psi.

The temporary drop associated with opening the test rams during top-kill lies outside all bounds. The May 8 and 14 values lie just outside the bands for +/-200 psi.



We see in Fig. 4 that all of the values after May 19 oscillate within the +/-200 psi bands, except for the top-kill period. These reason the top-kill period falls outside these bands is clear, the test rams were opened and this reduced the effective resistance of the BOP. We also see that most, but not all, of these values after May 19 fall within the +/-130 psi bands. While these oscillations may have some physical origin, these observations indicate to me that the variations after May 19 result from inaccuracies in the BOP pressure transducer.

The values of May 8 fall just above the upper +/-200 psi, the values of May 14-17 fall just below, and the value of May 19 fall very close to the middle of the range. Given that oscillations of this sort persisted over the entire period from May 19 to July 8, I believe it is likely that the origin of this oscillation between May 8 and May 19 is the same as that for later times. In this case, the likely explanation is that variations between May 8 and May 19 also had no physical origin and that these variations simply reflect uncertainties in measured BOP pressures.

Based on these observations, I believe that the ratio of the overall resistance of the BOP relative to that of reservoir and wellbore very likely remained unchanged from the perspective of flow for the period from May 8 to shut-in on July 15. In this case, any change in the PI or wellbore affecting flow rates would have to be very closely matched by corresponding changes in the BOP that occurred on a continuous basis over the entire period. Again, I do no believe that this is credible given that these are independent systems. Rather, I believe that this ratio remains nearly constant over the period because the state of the reservoir, wellbore, and BOP all remained essentially unchanged, from the perspective of resistance and flow, over this entire period.

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APPENDIX D: DETAILED DISCUSSION OF EROSION IN THE BOP

Erosion in the Variable-Bore and Test Rams

In my analysis of flow rates before May 8, I assume that the VBRs and test rams all seal perfectly from the time they are first closed until May 8. I make this assumption not because I believe it is true, but because this gave me a lower bound on the flow rates. It is therefore a conservative assumption used only so that I did not overestimate the flow rates from the perspective of not overestimating the cumulative discharge during this period. In contrast, Mr. Shanks, a BP expert, has testified that both the middle and upper VBRs were eroded (and so leaking) within minutes to at most 2 hours after they were closed.[36]

Erosion in the Casing-Shear Ram (CSR)

In my original report, I stated that erosion of the casing-shear ram did not significantly affect low rates between the time it was closed on April 29 and the first measured BOP pressures on May 8. This was based on the fact that the CSR showed little erosion, even after 86 days, except on the bottom face of the upper blade just above the severed pipe. Dr. Nesic suggests that this was not case.

As discussed below, I do not believe the Dr. Nesic's results represent credible analyses of erosion and flow rates, but do wish to point out that his results do not contradict my belief concerning the CSR. His results per Fig. 33, show the relative pressure drop across the CSR remains almost unchanged from April 29 to May 27, a period of about four weeks. By his own estimate, the pressure drop through the CSR over this period varied by just 15%. Over the period from April 29 to May 8, this would correspond to a change of less than 5%. Now the pressure drop through the CSR was at most about 600 psi, so this 5% change corresponds to a change in the pressure drop of about 30 psi, and this produces a change in the flow rate from the well of only about 0.4%. It is therefore very clear that erosion of the CSR had negligible influence on flow rates prior to May 8.

Dr. Nesic additionally claims incorrectly that the CSR "was the largest restriction in the system" as of May 27.[37] At this time, the reservoir pressure less elevation head was roughly 8000 psi and the BOP pressure was roughly 4200 psi. The pressure drop from the reservoir to BOP was thus roughly 3800 psi. In contrast, the pressure drop across the CSR *and* the upper VBR was only 600 psi when the test rams were closed.[38] As such, the resistance to flow across the CSR was at best a small fraction of the total resistance in "the system." Even through the BOP, the CSR did not represent the dominant resistance to flow. At this time, the pressure drop through the middle VBR and test rams was roughly 900 psi, so resistance in the CSR (and upper VBR) was significant but by no means the largest resistance in the BOP.

Erosion in the Riser Kink

In my report, I concluded that erosion within the riser did not significantly affect flow rates from the well over the period from riser collapse to its removal in early June. That conclusion was based on apertures in the kinked section, the character of the several leaks, and the small pressure drop through the riser that was measured before the top-kill attempt. Dr. Nesic makes a number of statements in his report that would appear to contradict this. While I do not have confidence in any of his results, I would like to point

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[36] Direct Examination of Forrest Earl Shanks, II. Shanks Tr 9048-49.pdf. He observes two sets of erosion marks at each of the two VBRs. Per Mr. Shanks, the lower of these in each case had to occur between the time the rams were closed and the time the traveling block fell still later on April 20.

[37] Expert report of Srdjan Nesic. Page 36. This statement also appears on page 5.

[38] BP-HZN-2179MDL02208359.

that the results of Dr. Nesic do not contradict anything I said in my original report that in any way affects my calculation of the cumulative discharge.

Per Fig. 33 of his report, the pressure drop through the kinked riser ("KR" in his plot) was not large compared to any of the other contributions to the total pressure drop through the BOP at any time, and the pressure drop through the riser remains about the same from April 22 to May 27. The first of these statements is consistent with the conclusion in my original report that removal of the marine riser increased flow rates from the well by only about 2.8%. The second statement is consistent with my only conclusion regarding the state of the riser expressed in my report that was used in my estimates of flow rates before May 8. That is, I stated that "I conclude that the state of the riser as of this time (April 22-26) was substantially the same as it was in late May." Any parsing of how much erosion occurred in the riser or when this occurred is therefore irrelevant to the results expressed in my original report. The riser simply did not at any time present a significant resistance to flow from the well, with or without leaks in the vicinity of the kink.

Erosion of the Blind-Shear Rams (BSR)

In my report, I state that I believe that apertures in the partially-closed BSR were sufficiently large that the pressure drop across these rams was never large enough to significantly affect flow rates and, as a result, that any erosion within the BSR would also not affect flow rates. This conclusion is based in part on the minimum expected size of the apertures through the partially-closed rams and in part on the very small pressure drop of through the BSR as of May 25. I do not believe that erosion would have resulted in such a small value without also reducing the pressure drop through other elements of the BOP to some comparable level.

The rate of abrasive erosion depends on a great many factors including properties of the particles, particle loading density in the fluid, properties of the fluid itself, properties of the material being eroded, geometry of the flow path, and especially the fluid speed. Most erosion models show erosion rates that vary with fluid speed to at least the second or third power; some vary with up to the fifth power of the speed. Because of this, erosion rates are *extremely* sensitive to the fluid speed. And, fluid speeds are very sensitive to aperture sizes along a flow path. Now, when any flow path contains multiple restrictions and flow rates are driven by a pressure difference, as is the case here, fluid speeds will always be highest in regions along the path having the smallest apertures and so having the highest resistance to flow. As a result, erosion always tends to equalize multiple restrictions along a flow path. That is, the highest resistances will tend to erode the fastest due to the higher fluid speeds and so will fall toward the values of other resistances along the flow path. Further, as such equalization of the resistances occurs, the pressure drop through each restriction along the path will also tend toward being equal.

The pressure drop through various elements of the BOP were measured on May 25. At that time, the pressure drop across the CSR was about 600 psi, that across the middle VBR was about 400 psi, and that across the test rams was about 500 psi.[39] The resistance to flow for each of these is thus roughly the same, as expected based on the reasons given above. In contrast, the pressure drop through the BSR is just 52 psi, so its resistance to flow is much, much lower. This value of 52 psi is just over 3% of the total pressure drop of about 1500 psi, and I do not believe that erosion of the BSR could have led to this condition. With the multiple resistances in series that are all eroding, any erosion that affected flow rates would tend to make all of the resistances the same, and this is clearly not the case for the BSR. I therefore

[39] BP-HZN-2179MDL02208359. The pressure drop across the test rams is listed as 725 psi based on a correction to the BOP gauge of +966 psi. Using my more appropriate correction of -740 psi, the drop across the rams is 499 psi.

concluded that the BSR never offered any significant resistance to flow and so any erosion of the BSR would also not affect flow rates.

Dr. Nesic disagrees with my opinion and provides numerical simulations suggesting that erosion did in fact increase flow rates through the BOP. In these calculations, Dr. Nesic makes quite a number of assumptions that might be called into question. I will not address these here, however, because I believe there is a more central concern that make his results meaningless owing to huge uncertainties in his erosion rates.

At several points in his report, Dr. Nesic acknowledges that he does not know the actual flow rate and instead uses bounding rates of 5,000 and 65,000 stbd.[40] He explicitly states that "I did not attempt to determine the actual or even likely flow rates..." And, as nearly as I can tell, he never states the flow rate or equivalent fluid speed on which any of his results are based. Dr. Nesic additionally states that he did some kind of sensitivity analyses to examine the impact of this uncertainty on his results, but he does not mention the outcome of this anywhere in his report or its appendices.

As already mentioned, erosion rates depend very strongly on flow rates as these are directly related to fluid speeds. At the least, most erosion rates vary as about the cube of the fluid speed or flow rate. The report of Dr. Nesic indicates that his model exhibits a power of about 3.3, which seems reasonable.[41] Now his bounding values for the flow rate represent uncertainties of a factor of 13 (that is 65,000 divided by 5,000), and this is raised to the power of 3.3 to determine the impact on uncertainties in erosion rates. The result is uncertainty in his erosion rates of more than a factor of 4,700. This is uncertainty of 470,000%. This is the difference between 26 minutes and 86 days. Despite this, Dr. Nesnic asserts that he "quantified the effect of erosion on flow rate and provided a timeframe over which it happened." In my opinion, this is nonsense.

As a result of this huge uncertainty, I do not believe that any of Dr. Nesic's results are meaningful, and his conclusion that the BSR gradually eroded between April 22 and May 27 is entirely without basis. The erosion he calculates could just as easily have occurred in a few days or even several hours. I am especially bothered by the fact that he grossly misrepresents this possibility through his statement that "It is safe to assume that that the trend in Figure 33 is universal for the range of conditions I considered, irrespective of the flow rate."[42] This is a reasonably accurate statement regarding the relative pressure drops alone because these ratios do not depend strongly on flow rates once the geometry is fixed. It is terribly misleading, however, from the perspective of erosion rates given that he cannot know these rates within a factor of several thousand based on his bounding flow rates. The impact of uncertainties in the erosion rates on the time-history of the geometry is simply not considered here. The "trend" he purports to exist in Figure 33 is simply unknowable by the methods he has used given that he does not know the flow rates or accompanying rates of erosion.

Finally, Dr. Nesic makes a statement in his report that the flow rate from the well would almost double between April 22 and May 27 due to erosion within the BOP. His statement is based on an assumption that "the BOP and kinked riser were the main restrictions to flow in this period." This assumption is clearly invalid. As of May 8 the reservoir pressure less elevation head was roughly 8400 psi, the BOP pressure was roughly 4500 psi, and the ambient sea pressure was about 2200 psi. The frictional pressure drop between the reservoir and bottom of the BOP was therefore about 3900 psi while that through the

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40 Expert report of Srdjan Nesic. See page 12. This gives his bounds and his mention of "sensitivity."

41 Expert report of Srdjan Nesic. On page 18 he states that the fluid speed drops by a factor of four and the erosion rate falls by two orders of magnitude. This corresponds to a power of 3.3.

42 Expert report of Srdjan Nesic. Page 35.

BOP and riser was about 2300 psi. As such, the main restriction to flow, by a very large margin, was in the reservoir and wellbore, not in the BOP and riser. His assumption is grossly in error so his conclusion that the flow rate would have doubled in this period is not supported in the manner he claims. Moreover, Dr. Nesic considers resistance to flow only in the CSR, BSR, upper annular, and riser. This represents only about half of the total resistance in the BOP, the remainder residing in the two VBRs and the test rams, so any calculations done using just these resistances would exaggerate the impact on flow rate by roughly a factor of two.

As part of this argument, Dr. Nesic also states that the values he presents in Figs. 33 and 34 for the beginning and end of the interval are "…"hard" numbers because the exact flow geometries are known…"[43] This is a startling claim given that no one knows the exact geometry that existed in the BOP at this time. And while the geometry can be inferred roughly from post-retrieval measurements, even fractions of an inch uncertainty in the space between of the two BSR rams could lead to very large uncertainties in flow rates.

[43] Expert report of Srdjan Nesic. Page 36.

[redacted]

APPENDIX F: LIST OF CONSIDERED MATERIALS

Ex 9139
Ex 9314
Ex 9316
Ex 9905
Ex 9453
Ex 9491
Ex 9513
Ex 11145
Ex 141196
Ex 141197
EX 141198
Ex 141199
Ex 141200
Ex 141203
Ex 141204
Ex 141205
Ex 141206
Ex 141207
Ex 141208
Ex 141209
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Ex 141237
Ex 141238
Ex 141239
Ex 141241
Ex 141242
Ex 141244
BP-HZN-2179MDL01872218
BP-HZN-2179MDL04208336 - BP-HZN-2179MDL04208348
BP-HZN-2179MDL06742964

BP-HZN-2179MDL06743164
BP-HZN-2179MDL06743281
BP-HZN-2179MDL06743285
BP-HZN-2179MDL06743286
BP-HZN-2179MDL06744203
BP-HZN-2179MDL06744881
BP-HZN-2179MDL06745330 - BP-HZN-2179MDL06745698
BP-HZN-2179MDL06745929
BP-HZN-2179MDL07791323 - BP-HZN-2179MDL07791335
BP-HZN-2179MDL07791400 - BP-HZN-2179MDL07791405
BP-HZN-2179MDL07791406 - BP-HZN-2179MDL07791418
BP-HZN-2179MDL07791473 - BP-HZN-2179MDL07791479
BP-HZN-2179MDL07791489
BP-HZN-2179MDL07791499 - BP-HZN-2179MDL07791504
BP-HZN-2179MDL07791513 - BP-HZN-2179MDL07791530
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BP-HZN-2179MDL07791599 - BP-HZN-2179MDL07791604
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BP-HZN-2179MDL07792876 - BP-HZN-2179MDL07792890
BP-HZN-2179MDL07792899 - BP-HZN-2179MDL07792907
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BP-HZN-2179MDL07806037
BP-HZN-2179MDL07806038
BP-HZN-2179MDL07806039
TRN-MDL-07611323 - TRN-MDL-07611560
A model for the investigation of two-phase erosion-corrosion in complex geometries, Anthony Keating, May 1999 (19990500)
A new model for the erosion of metals at normal incidence, G. Sundararajan & P.G. Shewmon, Wear 09/09/82, p. 237 (19820909)
A Study of Erosion Phenomena, Part II, J.G.A. Bitter, Wear 08/24/62, p. 169 (19620824)

Advances in Multiphase Flow CFD Erosion Analysis, Deng-Jr Peng, Afshin Pak, Chinello Luca, Terry Wood, & Andrew Low, OTC 24114, 5/6-9/13 (20130506 - 20130509)
Analytical model for erosion behavior of impacted fly-ash particles on coal-fired boiler components, Sādhanā, Vol. 31, Part 5, October 2006 (20061000)
Avoid equipment failure with erosion prediction tools, D. Schowalter, M. Agrawal & A. Chilka, 12/06 (20061200)
Erosion by a stream of solid particles, J.H. Neilson & A. Gilchrist, Wear 05/28/67, p. 111 (19670528)
Erosion caused by airbone particles, G.P. Tilly, Wear 06/02/69, p. 63, (19690602)
Erosion of oil & gas industry choke valves using computational fluid dynmaics and experiment, Lars Nøkleberg & Terje Søntvedt, Intnl. J. of Heat & Fluid Flow 19 (1998)
Erosion of Surfaces by Solid Particles, Iain Finnie, Wear 1960, p. 87 (19600000)
Erosion-Corrosion mechanisms and maps, M.M. Stack, B.D. Jana & S.M. Abdelrahman
Estimating Erosion in Oil and Gas Pipe Line Due to Sand Presence, Akar Abdulla, 2011 (20110000)
Evaluation of Different Modelling Methods Used for Erosion Prediction, Mysara Eissa Mohyaldin, Noamn Elkhatib & Mokhtar Che Ismail, Paper No. 11005
Deposition of Forrest Earl Shanks II, p. 9048 - 9049
Deposition of Forrest Earl Shanks II, p. 9141 - 9142
Deposition of Forrest Earl Shanks II, p. 9187 - 9193
Deposition of Forrest Earl Shanks II, p. 9201
Failure Mechanism in Impact Erosion of Ductile Materials, Ramesh Padavala (2004)
Flow Rate Model - Griffiths (Trusler Modeling Run)
Griffiths Repro (Strickland Modeling Run)
Hans Vaziri Consideration Materials List, May 1, 2013
K factor Kill Line
K factor Kill Line_5_8 Extended Range
Nigel Richardson Consideration Materials List, May 1, 2013
On the Formation of Surface Ripples During Erosion, I. Finnie & Y.H. Kabil, Wear 1965, p. 60 (19650000)
On the paricle size effect in slurry erosion, Randall S. Lynn, Kien K. Wong & Hector Mcl. Clark, Wear 02/23/91, p. 55 (19910223)
Prediction of Sand Erosion in Process and Pipe Components, Asmund Huser and Oddmund Kvernvold, Det Norske Veritas (1998)
Prediction of Two-Phase Erosion-Corrosion in Bends, Anthony Keating & Srdjan Nesic, Dec. 6-8, 1999 (19991206 - 19991208)

Sand erosion experiments and model development for UNS N06625 cladding and UNS S32750, Technology Insights 2013 (20130000)
Sand erosion of metals and plastics: A brief review, G.P. Tilly, Wear 07/30/69, p. 241 (19690730)
Sand erosion of wear-resistant materials: Erosion in choke valves, K. Haugen, O. Kvernvold, A. Ronold, & R. Sandberg, Wear 1995, p. 179 (19950000)
Solid particle erosion behaviour of ferrous and non-ferrous materials and correlation of erosion data with erosion models, Materials and Design 29 (2008)
The development of a model to predict the erosion of materials by natural containments, W. J. Head & M.E. Harr, Wear 10/28/69, p. 1 (19691028)
The interaction of particle and material behaviour in erosion processes, G.P. Tilly & Wendy Sage, Wear 08/31/70, p. 447 (19700831)
Use of CFD to predict and reduce erosion in an industrial slurry piping system, Gary Brown, 12/13-15/06 (20061213 -20061215)
Examination and comparison of various erosive wear models, MS. Bingley & D.J. O'Flynn, Wear 2005, p. 511 (20050000)
Expert Report of Adrian Johnson PhD, CEng, MIMechE & Consideration Materials - Appendix H, May 1, 2013
Expert Report of Alain Gringarten & Consideration Materials List, May 1, 2013
Expert Report of Andreas Momber & Consisderation Materials List, May 1, 2013
Expert Report of Curtis Hays Whitson, PhD & Consideration Materials List, May 1, 2013
Expert Report of Dr. Carlos Torres-Verdin & Consideration Materials List, May 1, 2013
Expert Report of Dr. John L. Wilson & Consideration Materials List, May 1, 2013
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Expert Report of J.P. Martin Trusler, PhD & Consideration Materials List, May 1, 2013
Expert Report of Kerry A. Pollard & Consideration Materials List, May 1, 2013

Expert Report of Martin J. Blunt & Consideration Materials List, May 1, 2013
Expert Report of Michael Zaldivar, PhD & Consideration Materials List, May 1, 2013
Expert Report of Morten H. Emilsen, October 11, 2011
Expert Report of Richard F. Strickland, PhD, P.E. & Consideration Materials List, May 1, 2013
Expert Report of Robert W. Zimmerman & Consideration Materials List, May 1, 2013
Expert Report of Srdjan Nesic, Ph.D. & Consideration Materials List, May 1, 2013
Expert Report of Tyson Foutz & Consideration Materials List, May 1, 2013
Exports of Johnson Modeling Runs (includes Johnson Drill Pipe High-OLGAS~1.xls, Drill Pipe High-OLGAS~2.xls, Drill Pipe Low-OLGAS~1.xls, Drill Pipe Low-OLGAS~2.xls, No Drill Pipe-OLGAS~1.xls, No Drill Pipe-OLGAS~2.xls, Johnson_Model_Output.xls)