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Exhibit No. ______

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REBUTTAL EXPERT REPORT

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

Flow Rates from the Macondo MC252 Well
Submitted on Behalf of the United States

Prepared by:

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I. Introduction

I am a mechanical engineer specializing in multiphase fluid flow and a Principal Member of the Sandia National Laboratory Technical Staff. In May 2010, I was enlisted to assist in the DOE response to the Macondo well blowout in the Gulf of Mexico. Engineers from three DOE NNSA laboratories, Sandia, Los Alamos National Laboratories (LANL), and Lawrence Livermore National Laboratories (LLNL), supported the United States' response to the blowout. During the response, the DOE NNSA engineers, including myself, conducted estimates of the flow rate of oil from the Macondo well. I performed calculations of the flow rate through the Top Hat 4 device installed by BP. I also estimated the flow rate at the time the Capping Stack was installed and eventually used to shut in the well (July 12-15, 2010) and the cumulative flow from the well over the 86 days of the blowout. These calculations are documented in a Sandia Report (A. C. Ratzel III, DOE-NNSA Flow Analysis Studies Associated with the Oil Release following the Deepwater Horizon Accident, September 2011, SAND 2011-1653) (DOE-NNSA Flow Analysis Report). In my initial expert report in this matter, I refined my prior calculations and submitted a new flow rate calculation for the time of the Top Kill in late May 2010. My credentials, response work, and flow calculations are further detailed in my initial expert report of March 22, 2013.

In brief, and as discussed in greater detail in my initial expert report, for a number of time periods during the blowout, I used conditions with known parameters (*e.g.*, pressure readings, pipe geometries, pump rates, and collection rates) to estimate flow at each time - during the Top Kill (over 60,000 bopd[1]), Top Hat 4 collection (approximately 60,000 bopd), Capping Stack flow (approximately 53,000 bopd). Those calculations support the analysis of other experts who have demonstrated a trend of decreasing flow over time (*e.g.*, Griffiths, Pooladi-Darvish). I also integrate the flow over the entire incident period to obtain a total oil release of approximately 5 million barrels. The assumptions used in generating my calculations of the total release are strongly supported by analysis of BOP pressure data recorded during the blowout (S. K. Griffiths, Environ. Sci. Technol, 46 (10), 5616–5622, 2012). As detailed in my initial report, other studies conducted by BP are also consistent with the flow rates used to obtain this total flow including those of BP engineers and BP contractor ADD Energy. Given that BP engineers calculated a flow rate through the Capping Stack on July 14-15, 2010 of approximately 53,000 bopd, if we combine this with the demonstrated decreasing flow over the course of the blowout, even BP expert Martin Blunt would agree that the cumulative release of oil must be over 4.5 million barrels.

A series of expert reports from the Defendants raised concerns about the calculations I presented in my expert report.[2] While some of those reports point out uncertainties in my calculations, they are uncertainties that I recognized and addressed in my initial expert report. Some reviewers offer alternate interpretations of the same data on which I based my model. Some show a lack of

[1] All references to "barrels of oil" or bopd in this report refer to a stock tank barrel of oil – 42 gallons at 60 F and 14.696 psi

[2] It is worth noting that during, and leading up to, the Well Integrity Test, because of the risk of creating an uncontrolled underground blowout, it was critically important to have accurate data and the best modeling inputs possible. This is not to imply that it was not important to have accurate data throughout the response. The DOE team relied on BP engineers to provide us with those data and inputs. In many of its expert reports, BP now seems to contend that certain data and inputs provided to our team were unreliable or inaccurate.

understanding of my work. I have tried to address each of their concerns within this document. Despite those experts' criticisms, I am confident in my calculations and believe they are reliable.

Below, I start by addressing Defendants' experts' primary critiques of my calculations by topic area: Top Kill, Top Hat 4 collection, and Capping Stack flow. I then address related general critiques regarding my flow models and fluid properties. Next, I explain why BP experts Dr. Johnson, Dr. Momber, and Dr. Nesic fail to demonstrate that flow was increasing over the course of the blowout. I also explain why BP expert Dr. Zaldivar's method cannot quantitatively predict the flow rate through the riser and demonstrate that the flow rate through the kink holes was nearly twice what Dr. Zaldivar calculated (8200 v. 4900 barrels per day). [redacted]

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.

II. Discussion

A. Top Kill Flow Rate Calculations

1. Background

In my initial report, I presented a flow rate calculation based on the pressures and flows measured during the Top Kill event at the end of May 2010. Based on these measurements, I estimate the flow rate for May 28, 2010 to be over 60,000 bopd, which is consistent with the flow rates I estimated for that same time period in the cumulative flow estimate referenced above. I also conclude that the flow rate is certainly greater than 43,000 bopd during this time period.

2. Response to Dr. Adrian Johnson (BP)

In point 6 on page 47 of Dr. Johnson's report, Dr. Johnson does not seem to understand the Top Kill model I presented. He states that the model assumes zero oil flow during the top kill event, and he further states this is incorrect since the pressure achieved is not above the shut-in pressure. Initially, when performing my calculation, I did assume that the oil flow was zero during the Top Kill, but this was simply to obtain a *lower limit* for the flow, and I stated this clearly in my initial report. Dr, Johnson further states that my expert report "does not address the issue further." This is not correct. On page 14 of my initial report, I relax the assumption of no flowing oil, and redo the calculation. The results are presented in Table 2 on page 15 of my report where a higher oil flow rate is obtained. This is not presented as a lower limit but a *best estimate* of over 60,000 bopd based on the BP pressure data obtained during the Top Kill event.

Dr. Johnson presents a number of arguments to attempt to discredit my estimate of the flow rate during the Top Kill event. I disagree with Dr. Johnson's arguments and address each below.

First, Dr. Johnson is correct when he states that I assumed that the Top Kill event had little impact on the K value (resistance) of the BOP. However, I had a good basis to reach this conclusion. I based this conclusion on the fact that the PT-B pressure measurement only significantly changed as a result of the opening and closing of the test ram (and this change seemed reversible). I admit that all of the Top Kill processes likely had some effect on the resistance of the BOP, but that these effects seemed to counter each other and result in a small net change in resistance. Dr. Johnson claims on the bottom of page 45 of his report that the BOP resistance could have changed by two orders of magnitude (factor of 100). This claim is contradicted by the PT-B pressure readings. Even BP engineers concluded that little change in the BOP resistance was caused by the Top Kill (Exhibit 5066, BP-HZN-2179MDL00412974: June 11, 2010 email from Paul Tooms to Kent Wells, et al., Subject: Historical BOP Pressure w/ attachment; Appendix A.6 to my initial expert report). Dr. Johnson discounts this by claiming "That the PT-B reading did not change before and after Top Kill can only be used to show that K x Q (flow rate)^2 did not change. A change in K could have been counteracted by an opposite change in Q^2." This claim simply is not believable. It is not possible for flow rate (Q) to decrease (or increase) if the PT-B reading is constant unless Dr. Johnson proposes a magically timed increase (or decrease) in the resistance between the reservoir and the bottom of the BOP. A constant PT-B pressure, and a constant productivity index ("PI") (PI is a measure of the well's ability to produce hydrocarbons) (at least over the few days of the top kill), results in a constant flow from the reservoir. This constant flow and a constant PT-B pressure imply a constant K (resistance) in the BOP. Dr. Johnson's claim of a potential two orders of magnitude change in K in the BOP is contradicted by the measured data.[3]

Second, Dr. Johnson (point 3 on page 46) claims that I should have used a different time period to perform my Top Kill estimate, and if I had done so I would have obtained a smaller result. This is true, but immaterial. One uses many time periods, and establishes many lower bounds. Then one presents the highest lower bound determined. For example, if one can prove my age is above 10, 20, and 50 years old, one can conclude that I am above 50 years old. One does not accept 10 as the lower bound on my age even though it is a true statement. The time period I used established the highest lower bound.

Third, Dr. Johnson claims that I might have used an incorrect density for the flow through the BOP during the Top Kill (page 44) because I did not account for the actual junk. He is incorrect. The junk (golf balls, rubber pieces, etc.) was injected with little velocity, and thus it flowed up the BOP and either got trapped, or escaped the well. During the time periods that I modeled, the junk was not flowing. Therefore I should not use an altered density due to the junk.

Fourth, Dr. Johnson correctly states that I assumed that all of the mud that flowed into the well immediately exited via the BOP and did not penetrate the well and claims that this is a source of error in my model. Again, he is incorrect. Since the Top Kill did not work, this assumption seems well-founded, as BP engineers concluded after the event (Exhibit 9265) based on the fact that the Top Kill did not work.

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[3] Dr. Johnson uses his Figure 16 to demonstrate that the resistance does indeed change during the Top Kill event. At best, the data presented in this figure implies a modest change in the BOP resistance, but nothing near two orders of magnitude as claimed by Dr. Johnson.

B. Top Hat 4 Flow Rate Calculations

1. Background

In my initial report, I presented a calculation of the flow from the well during the time period around June 15, 2010 when pressure measurements from BP's collection device Top Hat 4 were available. I estimate the flow to be approximately 60,000 bopd. This same calculation is applicable to all time periods from June 3, 2010 when Top Hat 4 was installed until July 11, 2010 when Top Hat 4 was removed in order to install the Capping Stack. This calculation also provides a lower limit on the flow of 43,000 bopd in that time period based on collection rates and assuming zero flow exiting the Top Hat 4 skirt. Of course there was always flow from beneath the Top Hat 4 skirt during that period, so the 43,000 bopd figure provides only a lower bound. Only one opposing expert, Dr. Adrian Johnson (BP), challenged my calculation. I disagree with Dr. Johnson's criticisms and believe my calculations are correct and reliable.

2. Response to Dr. Adrian Johnson (BP)

Dr. Johnson directs considerable criticism at my skirt flow estimates, including correctly pointing out that a small error in the pressure measurement can make a large error in the skirt flow (page 10, item 2 of the Johnson Report). Dr. Johnson is indeed correct that the flow out of the skirt is difficult to estimate from the Top Hat 4 pressure reading. For that reason, the flow out of the skirt is best estimated based on visual observations of the flow during changing collection rates. Since the flow characteristics did not significantly change when the collection flow rate was suddenly changed by 10,000 bopd, the skirt flow must be at least 20,000 bopd. I think my estimate is much better than the estimates provided by Dr. Johnson for this flow.

a. Dr. Johnson's Proposed K Factors are Incorrect

In many places within his review Dr. Johnson questions the value of K (resistance to flow) that I have chosen to model the flow out of the skirt in Top Hat 4. He seems convinced that the correct value is "at least 2, but possibly 3, orders of magnitude greater" than the value of 2 that I used. Dr. Johnson's critique demonstrates a fundamental misunderstanding of K values and flow through the Top Hat 4. I present three independent ways to demonstrate that I am correct.

First, I challenge Dr. Johnson's calculation of 2000 bopd out the skirt using a higher K value that he claims is more appropriate. BP purposefully allowed a high flow out the skirt (the flow could have been reduced by opening the fourth vent on Top Hat 4) to prevent drawing in water when the collection increased from zero to 20,000 bopd or from 20,000 to 25,000 bopd. Had the Top Hat 4 drawn in water, hydrates would have clogged it and prevented further collection of oil (similar to the failure of the Coffer Dam collection device). A flow of 2000 bopd from the Top Hat 4 skirt was known to be insufficient to prevent drawing in water since it was necessary to keep the flow rate exiting the skirt at a level greater than any potential changes in the collection flow rates. This concept was well known to the BP engineers at the time (*e.g.*, BP-HZN-2179MDL04869503). BP's collection data shows that collection rates were fluctuating from 0 to 25,000 bopd between June 3 and July 11.

Second, a K value significantly above unity does not make any physical sense. K can be considered a ratio of the amount of kinetic energy lost to the available kinetic energy. Thus, it is limited to unity for a simple device *when the basis is the maximum kinetic energy of the flow*. However, due to traditional formulations for an estimate of the kinetic energy, K values generally are limited by a value of two instead of unity.

Third, the K value plot proposed by Dr. Johnson is not based on the maximum velocity through the system. If Dr. Johnson's proposed K factor is corrected for the proper velocity, his K factor becomes 2 - the same value I used in my calculations. Using Dr. Johnson's own reference 16 (Miller D. S., Internal Flow Systems ISBN 0-947711-77-5 1990), the text regarding figure 14.3 (from which Dr. Johnson states I should obtain a K value) clearly states that the velocity to use is the velocity in the full pipe (not the peak velocity through the smaller orifice). Since I used the peak velocity through the skirt open area to base my K value (and so does Dr. Johnson in his calculation) I had to alter the K value presented in figure 14.3 by the area ratio squared. Using Dr. Johnson's Figure 3 (which he presents as a reproduction of Figure 14.3 in Miller and a possible source for estimation of K values for my application) a K factor of 50 for an area ratio of 0.2 is obtained. When this is altered so that one can use the increased fluid velocity through the reduced area, one obtains a K of 2. Another example from Dr. Johnson's Figure 3 shows a K factor of 200 for an area ratio of 0.1. Again, altering for the increased velocity through the smaller area, a K of 2 is obtained. I have found it is much cleaner to base the K value on the large fluid velocity within the opening. In this way one does not have to deal with infinite K factors which presented a problem to Dr. Johnson (discussed by Dr. Johnson on page B-10). By understanding the physics of the problem one can extrapolate Dr. Johnson's Figure 3 via the following simple equation:

$$K = 2\left(\frac{A}{a}\right)^2$$

where A is the full pipe area, and a is the smaller orifice area. This demonstrates that my use of a value of 2 is completely consistent with the proper interpretation of Figure 14.3 of the Miller textbook. I could not make sense out of the extrapolation posed by Dr. Johnson on page B-10 (equation B4). It does not match the extrapolation he presented in Figure 3. I suspect there is a typo in the equation. In summary, it is Dr. Johnson who has made an error that is two, possibly three, orders of magnitude in error.

b. My Coupling is Correct

Dr. Johnson claims that the flow from the various exits of the well during the time period of Top Hat 4 (collection ports, vent and skirt) are interrelated, and thus any model of them should be coupled. He is incorrect. Since the pressure within Top Hat 4 drives the flow out the vents and the skirt, I can treat each flow (flow through the vents versus flow from the skirt) independently since I have a *measured pressure upstream and downstream on each path*. It is only when one does not have a measured value of the pressure that one has to worry about coupling of the flows. That is not the case here.

c. The Collection Rates I use are Correct

Dr. Johnson accepts my method for calculating the minimum flow from the well during the operation of Top Hat 4. However, he disagrees with the universality of that estimate (*i.e.*, that the calculation applies to the entire period of June 3 through July 11, 2010). He claims that my use of 25,000 bopd as the collection rate is in error since that only occurred for a limited amount of time near the end of the Top Hat 4 period. This criticism is valid only if one accepts the premise that the flow rate was increasing between June 3 and June 24 (the date when 25K collections begins). I do not and the data do not support that scenario. Since the flow rate was decreasing with time during the Top Hat 4 period (as demonstrated by the PT-B pressure gauge), my estimate of the last day should serve as a conservative lower bound for the entire period. I continue to accept the conclusions provided in Dr. Griffiths' expert report where he provides evidence that the flow is decreasing with time. Thus, I stand by my claim that my estimate for a lower bound in the flow during the period of Top Hat 4 collection (and all periods prior) is still valid. In addition, I believe that my calculation of the flow rate on May 28, 2010 during the Top Kill (>60,000 bopd) is correct. I will further address Dr. Johnson's (and others) opinion that the flow is increasing with time during the flow of the Macondo well below.

C. Capping Stack Flow Rate Calculations

1. Background

[redacted]

After the Capping Stack was installed on the well, accurate geometry and measured pressures allowed estimates of the flow rate through the Capping Stack hardware. When this was added to collected flows from connections to the original BOP, the total flow from the reservoir could be estimated. As described in my initial report and the DOE-NNSA Flow Analysis Report, I estimate the flow of oil from the well on July 14 and 15, 2010 (just prior to shut-in) at 53,000 barrels of oil per day (bopd). It is important to note that this is the flow rate with the Capping Stack installed. The Capping Stack provides additional backpressure reducing flow by approximately 4%. This calculation is consistent with BP estimates of 51,500 bopd[5] and 59,098 bopd[6] conducted during the same time period.

[redacted]

[5] Exhibit 9453 (Appendix A.1 to my March 22, 2013 report).
[6] Exhibit 9491 (Appendix A.2 to my March 22, 2013 report).

[redacted]

[redacted]

[redacted]

[redacted]

D. BP Experts Johnson, Momber, and Nesic are Incorrect when they Contend that the Flow Rate was Increasing over the Course of the Blowout

BP experts Dr. Johnson, Dr. Momber, and Dr. Nesic each claim that the flow was increasing with time due to erosion processes that occurred. It is clear from post-incident inspection of BOP equipment that significant erosion occurred. However, the time period in which that occurred is unclear. If the erosion all occurred within the first two days of the accident, the impact on my estimate of the total flow from the well is zero since I did not start my integration of the flow until day three. To accept Appendix B of Dr. Johnson (where Dr. Johnson proposes an alternate model of the transient well flow in an attempt to demonstrate that the model of Dr. Griffiths is not unique), one has to believe that the resistance through the BOP constantly decreases with time in concert with the PI (productivity of the well) increasing with time. These two changes have to occur at remarkably coordinated rates to allow the PT-B pressure reading to behave as if there is no change in either. Dr. Griffiths, however, does not have to propose this serendipitous event and simply has the PT-B pressure reading change due to reservoir depletion, which generates a much more believable model.

In an attempt to discredit the work of Dr. Griffiths (and by implication mine since I rely in part on the work of Dr. Griffiths), Dr. Johnson incorrectly states that Dr. Griffiths has a variable resistance through the BOP. Dr. Griffiths' integration of the flow is better stated as an estimate from the reservoir up to but not including the BOP. By formulating it in this way, the impact of the BOP resistance (constant or not) is not a concern. However, Dr. Griffiths does include a model including the BOP (with a constant resistance) and shows that this results in matching the PT-B pressure measurements and produces similar flow rates. Dr. Griffiths' work was validated by the work of US expert Dr. Pooladi-Darvish. Due to the lack of PT-B data early in May, it is possible that the erosion processes continued past day two as I assumed. This is a source of uncertainty in my modeling. However, the agreement of the Griffiths' model with the PT-B data, and the lack of any significant sand observed in the oil collections prove to me that erosion did not occur past May 14, 2010.

Dr. Momber proposes that the cement rapidly failed resulting in a 2.17 inch channel through 189 linear feet. He then proposes that further failure of the cement occurred very gradually. Dr. Momber's proposed scenario is not credible. He provides some mechanisms that *might* have caused the rapid formation of the assumed initial 2.17 inch channel in "deteriorated" cement. Dr. Momber claims that "erosional processes cannot explain this short-time event." I completely agree that the short time event was not caused by erosional processes. However, it did not occur in "deteriorated" cement as Dr. Momber assumes. This cement was freshly poured and thus not deteriorated. In fact, the United States' cement expert Benge (TREX 5990), BP's cement expert Calvert (TREX 22791), and BP's Macondo Wells Team Leader Guide (Trial Transcript 8950:17-8951:10) all agree that the cement was not set at the time of the negative pressure test. It is likely that unset or poorly formed cement failed completely and quickly, not in two separate time scales as proposed by Dr. Momber. Dr. Momber tries to present literature values for erosion rates in volume per time to demonstrate that the rapid failure assumed by myself and Dr. Griffiths is not credible. However, these rates are more traditionally presented in other units (length per time), and these rates are for concretes (as opposed to well cements) that are successfully formed. They in no way apply to the cement in the Macondo well.

E. Dr. Zaldivar's Method Cannot Quantitatively Predict the Flow Rate

BP retained expert Dr. Zaldivar offers a unique method to estimate the flow. While it is quite impressive for Dr. Zaldivar to demonstrate chaotic behavior (switching from double peak to single peak oscillation modes) in a numerical simulation, I do not think that the model of this instability can quantitatively predict the flow rate. The method has never been validated to show that it can be used to estimate flow. There are numerous approximations that have been made in the study. I will highlight only two here.

First Dr. Zaldivar *imposes* the oscillatory motion on the riser pipe, when, in fact, the flow regime (the "slug flow") is what *caused* the motion of the riser pipe. Dr. Zaldivar's analysis can be disregarded on this basis alone. Figure 1 (From Zaldivar's report page F5-2 shown below) shows that the imposed motion (oddly labeled predicted and not imposed) does not exactly match the actual motion for May 16, 2010 (the sensitivity of the result to the details of the imposed motion is not investigated). Dr. Zaldivar admits there is not good data for the oscillatory motion of May 13, 2010, so he uses the same functional form for this separate time period. In my opinion, it is not possible that the oscillatory motion is the same on both days since the mode switches from double peak to single peak. In the actual process, the density, or buoyancy, change *causes* the oscillatory motion of the riser, not the reverse as Zaldivar asserts (from page 32: "This led to the investigation and confirmation that the observed slug flow was indeed caused by the riser movement.").

Second, the calculation of fluid temperatures in the riser flow is a very difficult proposition and it is my opinion that Dr. Zaldivar has not adequately addressed temperature variables. The insulation due to the buoyancy foams is difficult to predict. These foams permit cold seawater flow through the various gaps (shown below from Zaldivar's report Figure 2 page F4-2), and this is not accounted for. Flow is actually forced into these gaps as the riser oscillates, and this is completely ignored. For the non-moving sections natural convection might be important. Natural convection in the 40 F water is very difficult to estimate due to the local maximum in the sea water density at this temperature. The heat transfer due to buried (to an unknown depth) riser sections is also difficult to model accurately.

In appendix G of Dr. Zaldivar's report, he tries to address the sensitivity of various parameters and models on the predicted flow rates. He shows that the changes in many global parameters have little impact on the final results he obtains. Some sensitivity studies regarding heat transfer are presented, but these uniformly change the heat transfer over the entire system. It is reasonable to assume that the system might be very sensitive to local changes. It is most interesting to note that he claims that the "effect of the riser end position on the estimated flow rates is insignificant." To illustrate this he shows that a change in the elevation of the riser exit (a point that does not oscillate) by 5 feet changes one of his flow rates by 11%. This seems to be a surprisingly large (not insignificant) effect to me and raises concerns. First this is one of the few local changes he investigates (only the exit elevation is changed). One wonders what other input details are important. Granted, we may know the elevation of the riser exit to within 5 feet, but we sure do not know the oscillatory motion of each moving part of the riser to this accuracy. We have data for the motion of the highest point of the riser for May 16, and construct an estimate for the other points. Dr. Zaldivar admits that we do not have any reliable data for the motion on May 13 of any point of the riser. We also do not know the details of the riser path when it is buried. Could small spatial oscillations in the buried depth be important? It is difficult to know what input details are important on such a new technique that is not validated for estimating a flow rate.

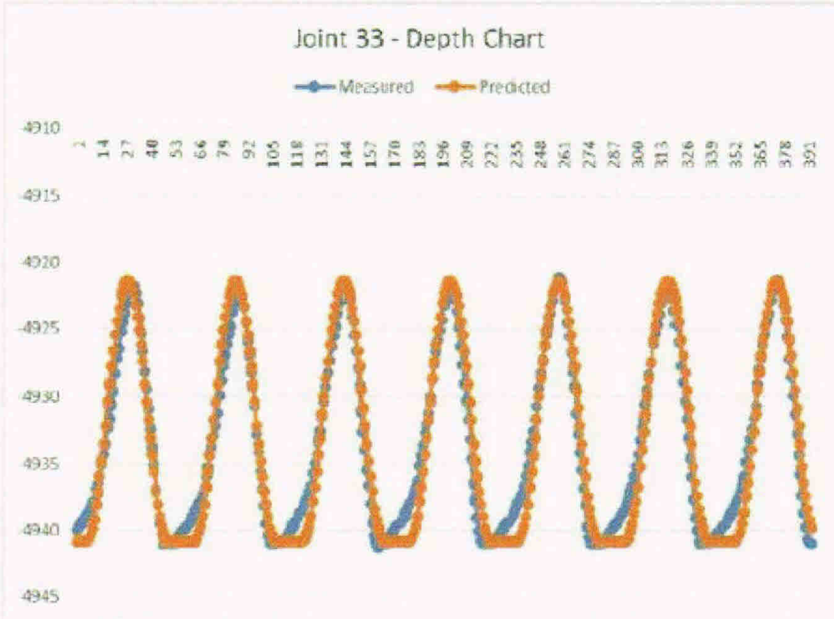

Figure 1 – A representative sample of riser motion data from XLX-36

Figure 2 –A buoyancy module-covered riser joint similar to those used on Deepwater Horizon[3]

Dr. Zaldivar concludes that the flow rate from the well was between 25,000 and 36,000 bopd in the period between May 13 and May 20, 2010. This is significantly below what I calculate for the same time period. Within the study, Dr. Zaldivar estimates the flow of oil out of the kink leaks. He uses a pressure of 600 psi (page F-6) and a flow area of 0.727 square inches (page F2-5). It seems that Dr. Zaldivar uses a K value of 4 (from his table 5 on page G-10) for the flow through the kink leaks, which I propose is too large. I cannot determine how he converts to standard barrels due to the lack of details he provides in his report. However, he does state that the fluid temperature is 80 C (page F-6). Dr. Zaldivar comes up with a flow estimate of 4900 bopd through the kink leaks. When I estimate the flow using the parameters of Dr. Zaldivar and my corrections (using my estimate of a fluid density at 80 C [416 kg/m^3], and a K of 2), I obtain a flow rate of 8,200 bopd. My calculation is consistent with the calculation of BP flow assurance engineer Tim Lockett (Ex. 10650). This leads me to question the other results presented by Dr. Zaldivar. If I simply correct his 36,000 estimate by the ratio of 8200/4900, I get a flow rate of 60,000 bopd for this time period, which closely matches my estimate of the flow during this time period.

III. Information Required the Federal Rules of Civil Procedure

1. This report contains my opinions, conclusions, and reasons therefore.

2. A general statement of my qualifications is contained in the Background section on page 1 of my initial expert report. A more detailed statement of my qualifications and a list of publications is included in Appendix B to that report.

3. I have received no compensation for my expert work in this case aside from my regular salary from Sandia National Laboratories.

4. I have not previously testified as an expert witness.

The facts and data I considered in forming my opinions are listed in Appendix B to this report and Appendix C to my initial report of March 22, 2013. I also reviewed and considered a substantial amount of data during my work responding to the Macondo blowout, including data provided by BP.

APPENDIX A – Additional Specific Responses to Defendants' Experts

A. Additional Specific Responses to Adrian Johnson (BP)

1. My Models and U.S. Expert Dr. Griffiths' Models are Accurate and Reliable

Dr. Johnson's report suggests that the models Dr. Griffiths and I formulated are simplistic because they assume that the flow is proportional to the square of the flow rate. Johnson suggests that with more complex models the flow is not strictly proportional to the flow rate squared, and proposes that he has better equations for the modeling of multiphase flow. While it is true that the pressure drop is not strictly proportional to the square of the flow, this is not an indication that the work of the government experts is sloppy or unreliable as implied by Dr. Johnson. If one reviews the DOE-NNSA Flow Analysis Report (page 41) one will find some discussion about the functional form of the pressure drop with flow rate; two limits are presented: (1) for laminar flow, the pressure drop varies linearly (power of 1) with flow rate, and for turbulent flow the pressure drop varies with the second power of the flow rate (although this is an approximation since the power can be slightly lower than two for non-fully-developed turbulent flow). These two values (1 and 2) present well defined limits for the exponent. Dr. Johnson is correct when he states that the correct exponent is often not exactly 2. Dr. Johnson does not propose a different value, but simply states that my model and Dr. Griffiths' model are not as accurate as the models he proposes.

Dr. Johnson proposes two potential geometries where the pressure drop flow rate relationship exhibits a power very slightly less than two. There are a few points I would like to raise to be more complete here.

First, Dr. Johnson suggests two alternate geometries where he determines an exponent that is less than 2. While the two locations of the drill pipe that Johnson proposes both seem logical, they are not the only possible geometries. It was well accepted in Houston by BP and DOE engineers during the event that a likely position of the drill pipe end was hung up on the cross-over between the 9 5/8 and 7 inch production casing. However, this is in conflict with the top of the drill pipe measurement that Johnson quotes. If we accept the top of the drill pipe measurement, the length of the drill pipe is uncertain due to slippage that may have occurred during the accident that may have pushed the drill pipe higher in the well (and resulted in the bottom of the drill pipe resting at the cross-over). Thus, Johnson's pressure drop relation may be in error.

Second, the model that Dr. Johnson proposes uses an *approximate* geometry (I assume that Johnson has assumed concentric pipes), and includes *approximate* multiphase flow representations. Again Dr. Johnson's pressure drop relation may be in error.

Third, and most important, it is shown in the DOE-NNSA Flow Analysis Report that use of the limiting power of 2 in my model results in a *lower* estimate of the integral of the flow. Any lower value of the exponent will increase the flow estimate. Thus, by using the limiting value of 2, I *underestimate* the total flow. I assume that this same trend will be the same in the Dr. Griffiths' model. The choice of the value of 2 was a conscious conservative choice yielding a lower integral of the flow.

2. Dr. Johnson's Report Does not Include his Fluid Temperature

Dr. Johnson contends that the DOE-NNSA Flow Analysis Report uses an incorrect temperature for the flowing oil. The proper temperature to use is not clear due to the very limited temperature data that is available. BP never collected a reliable measurement. However, the impact of the temperature assumption is noted in my expert report. The government team of experts has tried to be transparent with their models and calculations to make sure that it is clear what is assumed. Dr. Johnson's report in particular fails to provide temperature results even though he constructed a model to calculate the fluid temperature at the well head as a function of time. It would be more complete if Dr. Johnson would provide temperatures so one can better understand his model results.

3. The Equation of State (EOS) Dr. Johnson Uses for his Calculations Conflicts with Measured Data

Dr. Johnson claims that he has used a better equation of state to obtain properties of the Macondo oil. His equation of state results in lower flowing densities and a higher standard liquid density than I used. Both of these changes results in a decrease in the estimated flows when expressed in standard barrels. Dr. Johnson further states "I calculated the volume factor by flashing the fluid from pressure and temperature conditions at the top kill period to stock tank (*i.e.*, standard) conditions." It is unclear what Dr. Johnson means by "flashing from the top kill conditions to the stock tank conditions." I would have expected him perform a single stage flash from the reservoir condition. Dr. Johnson's equation of state calculates a standard liquid density (density of the liquid oil at "stock tank" conditions) of 851.6 kg/m^3 instead of the 838 kg/m^3 that I used. As stated above, this results in a lower flow in terms of standard (or stock tank) barrels. However, Dr. Johnson in constructing his equation of state ignores the fact that this density has been measured by BP and submitted to the Court in support of BP's motion regarding the captured oil (Carmichael Declaration in Support of BP's Motion for Partial Summary Judgment against the U.S. (Rec. doc. 8213-10)) as 36.2 API (or 843.8 kg/m^3) which is in closer agreement to my density.

[redacted]

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APPENDIX B – CONSIDERATION MATERIALS

Adrian Johnson Expert Report, May 1, 2013
Alain Gringarten Expert Report, May 1, 2013
Andreas Momber Expert Report, May 1, 2013
Curtis Whitson Expert Report, May 1, 2013
Kerry Pollard Expert Report, May 1, 2013
Martin Blunt Expert Report, May 1, 2013
Martin Trusler Expert Report, May 1, 2013
Michael Zaldivar Expert Report, May 1, 2013
Sankaran Sundaresan Expert Report, May 1, 2013
Simon Lo Expert Report, May 1, 2013
Srdjan Nesic Expert Report, May 1, 2013
Strickland, Richard - Expert Report, May 1, 2013
Expert Report of Nathan Bushnell, PhD., March 22, 2013
Expert Report of Stewart K. Griffiths, March 22, 2013
Expert Report of Mohan Kelkar and Rajagopal Raghavan, March 22, 2013
Expert Report of Mehran Pooladi-Darvish, March 22, 2013
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BP-HZN-2179MDL07279439
BP-HZN-2179MDL07279442
Erosion in Disturbed Liquid/Particle Pipe Flow: Effects of Flow Geometry and Particle Surface Roughness, J. Postlethwaite and S. Nesic, Corrosion Engineering, Vol. 49, No. 10 (1993)
Density Tables.xls
Zaldivar Modeling, Cd=0.61
Zaldivar Modeling kink - cd=0.5
Zaldivar Modeling kink - cd-0.61
Zaldivar Modeling Kink Best Estimate
Zaldivar Modeling kink cd=0.84
Zaldivar Modeling kink cd=1.0
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312555 2013-04-29 18:40 Blunt's Models/MatLab

codes/convertingToBHP/TemperatureData.csv
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