From: mcnutt@usgs.gov
Sent: Friday, September 03, 2010 2:23:02 PM
To: rcamilli@whoi.edu
CC: abowen@whoi.edu; mark_sogge@usgs.gov
Subject: RE: Acoustic flow estimate

Rich -

Thanks for the additional information on the kink.

And sorry - I didn't mean to imply that the ADCP was measuring gas. Indeed the suggestion was that the return was from the interface of the liquid/gas - i.e. bubbles. As so if the bubbles rise faster than the surrounding fluid, then there could be a bias towards higher velocities. I believe what you are saying is that:
(1) you can distinguish returns from scatterers within the oil and returns from the oil-gas interface, and saw those two moving at the same velocity;
(2) you conducted your measurements within the region of jet-driven flow, not buoyancy-driven flow, so the bubbles were not free to rise at a faster rate on account of their greater buoyancy in any case.

I also note that it seems to me from looking at the figures and numbers in your paper and that from the Plume team you both have peak velocities of about 0.2 cm/s. Furthermore, the discrepancy is not in the kink flow (at least not by factors of 2 or more), and that is where the Plume team also modeled jet flow. What I see as the difference is that you actually have a volumetric image of the velocity and the size of the plume at the end of the riser that gives you more that one point velocity to use and a more realistic picture of the plume size. The "size" of the plume at the point where you measure the velocities of 0.2 cm/s is significantly larger than the size of the plume where the Plume team measured the same velocities, because they just used the diameter of the riser. The plume does spread out after it exits (as a jet would) and you were still measuring high velocities downstream in the cone of the jet.

I'll be working on the document this weekend, so as soon as you can get any authoritative-sounding material to me, the better. However, I suspect that by the time we get figures, appendices, etc. we will be finalizing this for quite some time.

Thanks so much for all of your help in this.

Best,

Marcia

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From: R. Camilli <rcamilli@whoi.edu> [mailto:R. Camilli <rcamilli@whoi.edu>]
Sent: Friday, September 03, 2010 12:25 PM
To: Marcia K McNutt <mcnutt@usgs.gov>
Cc: abowen@whoi.edu; mark_sogge@usgs.gov
Subject: Re: Acoustic flow estimate

HI Marcia,

Just saw the email thread -have been buttoning things up in advance of the hurricane.

I would strongly disagree with the idea that the ADCP was measuring gas because gas would have lacked sufficient acoustic scatterers to generate returns at or above the required S:N (this is why ADCPs don't work in air). Even if we could pickup returns from within the gas, the speed of sound in air is about 4 times lower, so this would bias our estimated velocities downward by an equivalent degree.

We have clear evidence that the ADCP was measuring scatterers within the oil, and acoustic impedance transitions at oil-gas surface interfaces. The ping coherence indicate that the acoustic returns from these particles and fluids were moving at equivalent rates. Furthermore, the focal points of the ADCP measurements were within the turbulent jet region and velocity in this region was dominated by the kinetic energy of the jet's fluids (not buoyancy driven effects of the bubbles).

I'll discuss your question with the other team members and put together a quick writeup for you. How soon would you like it.

Regarding the flow change after the riser shearing, I would agree with the estimate that it only resulted in a minor flow increase. The rate could only increase substantially if the restriction at the riser kink was substantially greater than all of the preceding restrictions combined (i.e., restrictions within the BOP and further down within the well). Given the fact that there were at least two parallel sections of drill string stuck in the riser kink (both acting as conductors while also keeping the internal diameter of the riser open), it suggests that the flow restriction caused by the riser kink was modest. I don't know if you have already seen the report from LT Kusek that details the sheared riser section, but it has some very good measurements of the riser tears at the kink and photos showing the riser cross section with the captured sections of drill string (I've attached a copy) .

Best regards,
Rich

On 9/3/2010 10:58 AM, Marcia K McNutt wrote:
Indeed there was a pressure gauge that was recording at the time. The "infamous" pressure gauge at the base of the BOP that we will bury with Tom Hunter because it has been the bane of his existence. That pressure gauge recorded only a modest change post cutting of the riser, that led everyone to assume that there was only about a 100 psi (if I am remembering correctly) change in pressure from removing the riser. I think that translated into about a 2000 BPD change in flow - within the uncertainty probably. But then some more experiments were done when valves were opened to the sea and it was suspected that this pressure gauge was totally flaky. So people were a little reluctant to hang too much on it. But I think I recall Tom saying that there was another pressure gauge that they believed that was also in play. I will try to dredge up that email.

Marcia

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From: Andy Bowen <abowen@whoi.edu> [mailto:Andy Bowen <abowen@whoi.edu>]
Sent: Friday, September 03, 2010 10:52 AM
To: Marcia K McNutt <mcnutt@usgs.gov>
Cc: rcamilli@whoi.edu; Mark K Sogge <mark_sogge@usgs.gov>
Subject: Re: Acoustic flow estimate

Hi Marcia,

Interesting thread here. Rich is obviously the best person to comment on your question but it occurred to me reading this that pressure data from the BOP might be helpful for a pre/post riser (your question 1 below)? I seem to recall that there were pressure measurements being taken during this time but perhaps the actual location of the measurement is not helpful in determining the effect the riser had on flow?

my 2¢

best regards,

andy

On Sep 3, 2010, at 9:53 AM, Marcia K McNutt wrote:

Dear Rich:

Thanks so much for the write up of the results from the WHOI experiment on the Macondo plume. I read it with great interest last night.

I am almost done with the summary report that reconciles the efforts of all of the groups, but there is one major issue that I need to work out. As you know, there was actually excellent agreement on the post-riser-cut flow that it is in the 50,000+ BPD range. So no issues there.

But where I am hearing some disagreement is in the interpretation of flow rate BEFORE the riser was cut. My own personal opinion (based on weight of evidence) is that:
(1) the riser was a minor restriction to flow;
(2) the WHOI result shows that flow was large while the riser was on (nearly 60,000 BPD);
(3) most of the models from the reservoir and the nodal teams also point to high flow rates early in the incident, exceeding 50,000 BPD;
(4) the models of Paul Hsieh, combined with the DOE flow when the capping stack was closed, would predict a flow during the riser period of ~59,000 to 63,000 BPD.

However, the Plume team does not like this interpretation, because you recall that their estimates during the period when the riser was on were significantly lower. They prefer the following explanation:
(1) the riser was a major restriction to flow;
(2) the WHOI ADCP is measuring the speed of bubbles, not the fluid velocity, so overestimates the flow field;
(3) at least some of the models from the teams predicted lower flow rates;
(4) with significant resistance at the riser, the early flow rate would be less in the Hsieh/DOE model.

I think the weight of the modeling contradicts (3), and it is difficult at this point in time to say much about (1). It does seem that that flow was through the pipe and the annulus at the kink, not really through the full riser, so it is hard to argue in my opinion that the riser itself was a restriction.

Do you have any comment on (2)?

Thanks.

Marcia

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