DO WE UNDERSTAND THE LUMINOSITY IMBALANCE ?

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Abstract

This paper consists of a critical review of the many different hypothesis proposed to explain why some experiments are ”better served” than others. Evidence accumulated along the year shall be used to discard or partially confirm some of the hypothesis. Yet unexplored possibilities will also be discussed, in view of the necessity of better dealing with this problem in the 1999 run.

1 FACTS

Besides a few minor positive details, 1998 will be undoubtedly remembered as the Year Of The Great Imbalance; but was the situation really as bad as that ? At the end of the run, and not without some insistence by us, the experiments provided off-line integrated luminosity data fill by fill. If we sum up all these data, we obtain a total of, respectively, 195.5, 192.7, 207.6 and 185.6 pb^{-1}. These data, however, do not represent the luminosity imbalance very well. In fact, lost fills in which an experiment could not take data for one or another reason (detector failure, solenoid off, etc.) appeared as zero integrated luminosity in the data provided to us; and also fills partially lost contribute to this apparently high imbalance. Delphi, for instance, reported 13 lost fills. Therefore we have applied an algorithm to find a way of compensating for these ”lost fills”, considering how much the other experiments had reported for the same fill, and what was the imbalance factor for the experiments which had ”lost the fill” during that period of the year. Using this algorithm, our best estimates of ”what the experiment would have received if they were able to get it” give, respectively, 199.4, 193.5, 207.6, 192.2 pb^{-1}. If we divide these estimates by the average of the 4 experiments, we get, respectively, 1.006, 0.976, 1.048, 0.970 . So the ”best served” experiment over the whole year was OPAL (5% above the average), and the worst was DELPHI (3% below). We consider this final results to be within acceptable tolerances.

On the other hand, it is worth investigating two long periods in which things were more out of control. The first of these periods lasted approximatively from fill 4610 to fill 4881, and penalized DELPHI. The second period lasted from about fill 5100 to fill 5308, penalizing ALEPH. Figure 1 summarizes very well the evolution of the imbalance along the year. On the top part we find four lines corresponding to the four experiments, oscillating around a value of 1 which would represent the ideal balanced situation. In the bottom part of the picture we see a line proportional to the instantaneous luminosity recorded for a total beam intensity of 4 mA. All these lines have been smoothed to make the picture more comprehensible, but the istantaneous luminosity one should only be used to examine the trend, as the data points are not as precise as the experimental imbalance lines. In all what follows, we will use luminosity data adjusted using the above mentioned algorithm.

1.1 The Delphi Sad Period

For a long period, DELPHI was delivered much less luminosity than the other 3 experiments.

The reason for this imbalance was a posteriori found to be a bad local dispersion at the DELPHI interaction point. In fact, after many unsuccessful tries, the cure came almost by chance, when a global dispersion trim was observed to equalize the 4 luminosities. This trim was in fact tried to maximize the overall luminosity (using as on line feedback the vertical beam size measured by the BEXE detector), but luckily it rearranged things in a way that fixed the imbalance. This was confirmed by applying the same trim to another reference orbit used at that time. A further confirmation came around fill #5050, when the conditions prior to the fix were reestablished for a test, and the same imbalance noticed. As dispersion importance for luminosity and its correction are treated in great detail in the excellent work of P. Raimondi [1], I will only mention that until this year we had only looked at the average global orbit dispersion, neglecting local effects and independent dispersion of the individual beams. The lesson from this year is that these effects can play an important role, now that everything start to be pushed to their limits.

Table 1: DELPHI luminosity was scandalously low for a long period.

FILLS 4550-4610 (before the problem started)				
	L3	ALEPH	OPAL	DELPHI
LUMINOSITY	5.8	5.5	6.2	5.9
/AVERAGE	0.997	0.942	1.055	1.005
FILLS 4610-4873 (in the bad period)				
LUMINOSITY	42.1	41.4	44.5	37.9
/AVERAGE	1.015	0.998	1.073	**0.913**
FILLS 4874-5419 (rest of the run)				
LUMINOSITY	151.9	146.9	157.3	148.8
/AVERAGE	1.004	0.972	1.040	0.984

Apart from the obvious question about the origin of this imbalance, it is interesting to understand why it took so long time to acknowledge and cure this problem. First, we can observe that the rising of the imbalance was masked by several factors (see Fig. 1):

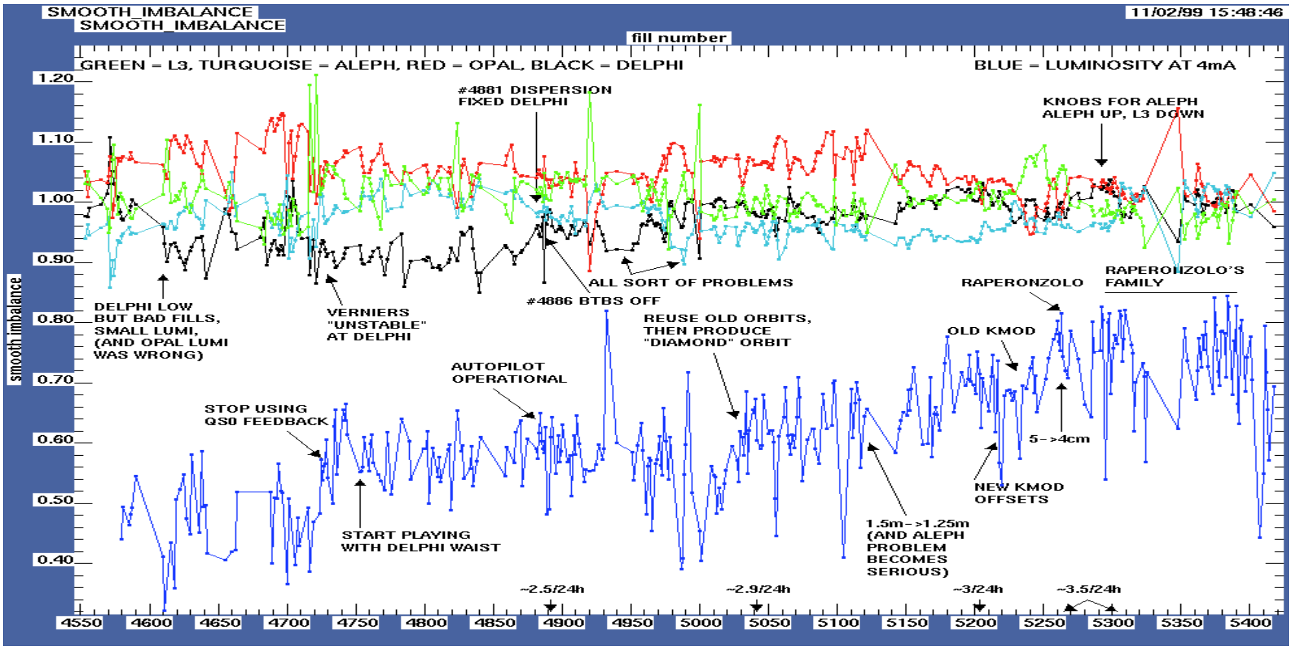

Figure 1: The 1998 Run

- A sequence of bad fills with low luminosity everywhere.
- Problems in correcting the QS0 drift in IP8 using the QS0 Feedback.
- OPAL's luminosity counter had a wrong gain, and reported luminosities much lower than what they really had (so that we initially did not notice DELPHI was much lower than the others).
- Apparent unstability of the Vernier settings in IP8 (between fills #4700 and #4880 **46** Verniers scans in IP8 were performed, against only 24 scans in the other IPs.

Once the imbalance was declared, a chilly wind of panic started blowing, and esoteric hypothesis were tried.

- Like, for instance, playing with the DELPHI waist
- Or blaming the Bunch Train Bumps in the odd points (they were, in fact, not guilty !).

The battle against the imbalance was made more difficult by the inadequacy of the means to quickly measure the luminosity by IP. In practice, only by **waiting for 1 hour without touching the machine** one could assess the value of a trim in term of imbalance.
Another negative point is that, as soon as the problem became politically serious, the Operators concentrated their efforts on the imbalance, rather than on the overall performance, which did not improve until the problem was fixed (in Fig. 1, look at the flat trend of the luminosity between fills #4750 and #4880).

1.2 *The Aleph Episode*

Later during the run, ALEPH was penalized, although to a lesser extent than DELPHI had been before.
The rising of the Aleph imbalance was again masked by several factors:

Table 2: Later in the run, also ALEPH was left behind.

	L3	ALEPH	OPAL	DELPHI
FILLS 5130-5230 (ALEPH is low)				
LUMINOSITY	27.3	25.6	28.1	26.9
/AVERAGE	1.013	**0.947**	1.041	0.999
FILLS 5230-5308 (looking for solution, but also 5cm->4cm)				
LUMINOSITY	29.2	27.9	28.9	28.8
/AVERAGE	**1.016**	**0.973**	1.005	1.002
FILLS 5308-5380 (β^* knob cured ALEPH, but damaged L3)				
LUMINOSITY	16.6	17.4	17.2	16.9
/AVERAGE	**0.975**	**1.019**	1.011	0.994

- In the period from fill #4970 to fill #5000 the overall performance was bad (cryogenic problems, database,etc..)
- The reference orbits were not particularly good.
- Around fill #5030 we managed to improve a lot the overall performance, by re-elaborating "Diamond" orbits starting from old ones. Aleph was left lower than the others, but not significantly.

A modification in the optics triggered an imbalance increase.

- DELPHI and L3 mainly profited from the horizontal β^* squeeze from 1.5 to 1.25 meters, leaving ALEPH well behind.

Panic was instilled again, and some experiment contactpersons started buzzing around the Operators like horseflies around sweating horses. This time, however, a different approach was adopted. The Coordinators tried to devise ways for fixing the problem, leaving the Operators to focus on the overall performance. This approach paid more. In fact, before an effective fix was developed, the machine optimization process continued, a vertical β^* squeeze from 5 to 4 cm was put into operation, and a new family of reference orbits with record luminosities was developed. Meanwhile several attempts to fix the Aleph problem were tried and quickly discarded, because either they did not fix the problem, or they decreased the overall performance.
An effective solution was later implemented, around fill #5308.

- The solution consisted in slightly (∼10%) squeezing more the horizontal and vertical β^*_S in Aleph.
- The overall performance was not significantly modified.
- But : in an unexpected way, this solution clearly lowered the luminosity in L3. We will came back to this point later.

2 POSSIBLE SOURCES OF IMBALANCE

Without any pretension of exhaustivity, we will mention a few possible phenomena which could explain the difference in luminosity between the LEP experiments. Some of these phenomena will be treated more in detail. It goes without saying that we consider our control on the Vernier settings good enough to be discarded as source of luminosity imbalance.

2.1 Local Dispersion

Important, as one could see from the Delphi Sad Period. Treated in great detail in [1].

2.2 Local Coupling

The x-y coupling naturally present in the machine is very well compensated at a global level, as shown by the Q-meter ”closest tune approach” measurements. However, in principle, it is not possible to exclude the presence of bounded regions within which a larger amount of coupling develops. If such a region exists and includes an interaction point, it may significantly affect the luminosity there. At the moment we do not have a way of measuring this ”local coupling”, and so we will not enter in more details, but we find this effect worth mentioning.

2.3 Vertical Crossing Angles

We have a vertical crossing angle at the IP when the two beams follow different orbits in the surrounding region. The way a crossing angle affect the luminosity is by changing the effective beam height seen by the colliding beams. In fact [2], while ideally the luminosity is inversely proportional to the beam height h, in the presence of a crossing angle we have to replace the height with an ”effective height”, given by the formula $h_{eff} = \sqrt{h^2 + l^2\delta^2}$. Here l is the bunch length and δ the crossing angle (defined as half the angle between the two orbits).
To put numbers in the formula, if we assume a vertical beam size h of 3.5 μm , and a bunch length l of 12 mm, we find the following :
As one can see, a small crossing angle (up to 50 microrads) has very little effect on the luminosity, but the difference of 50 microrads between 50 and 100 will generate an imbalance of about 4% . The problem in controlling this effect is that measuring the crossing angles is not so easy. The measurement is always based on measuring the difference between the orbits of the two beams. We could just use the information given by the two BPMs closest to the IPs, and the error will depend on the precision of these BPMs. Alternatively we could try to fit the position at many BPMs around the IP; in this case we would be less sensitive to errors from individual BPMs, but our fit should consider the difference in the real optics functions for the two beams, the values for the Vernier settings etc. At the moment, the least we can say is that we need to refine our way on measuring and controlling crossing angles. The other consideration is that in the past we have considered as acceptable angles of the order of 100 microrads. As we have shown, this should not be the case, especially if the beam vertical size is so small.

Table 3: Loss in luminosity due to a vertical crossing angle. Bunch height = 3.5 microns, bunch length = 12 mm .

crossing angle (μrad)	h_{eff}	L/L_{ideal}	loss
0	3.5	1	0
30	3.52	.994	∼0
60	3.57	.98	2%
80	3.63	.964	3.5%
100	3.7	.946	5.5%
120	3.78	.925	7.5%

2.4 Different effective β^*s at the IPs

We came now to the most interesting part of this presentation. We will show that the combined effect of betatron phase advance errors and beam-beam kick may alter the real values of the β functions at the IPs (also called β^*) in a different way. Moreover, this difference is undetectable when the beams are separated (for instance during our standard β^* measurements), because the beam-beam kicks play a fundamental role in the creation of these differences.

Let us start with the β^*. Neglecting coupling and vertical dispersion, the beam sizes at the IPs are proportional to the square root of the β functions there. The β^* values are, in principle, defined by the construction of the optics. However, due to machine imperfections and to the energy sawtooth, our model of LEP does not correspond exactly to the reality. By observing the effect on the betatron tune of a known trim in the quadrupole closest to the IP, we can measure the real β^* values. This measurements are performed whenever required, in order to detect, and possibly compensate, small deviations of the real β^* values from the **nominal** ones (we are talking of a typical 10-20% effect).**Unfortunately it only makes sense to measure the β^*_S when the beams are separated.** In fact, the beam-beam force seen by the two colliding beams invalidates this kind of measurement. This is very unconvenient, because it is possible to simulate situations in which perfectly balanced β^*_S with separated beams become very unbalanced as soon as the beams are put into collision.

To this purpose, let us consider a phase bump created by increasing the strength of some quadrupoles by a given error, and decreasing the strength of the corresponding quadrupoles one quarter of LEP away by the same strength. In Fig. 2 the effect of such an error on the phase advance with separated beams is shown. One can notice that, apart from the offset due to the bump, almost no beating is created. On the other hand, as soon as one introduces the beam-beam (Fig. 3) the bump is not closed anymore, a lot of beating is generated, and every IP is more or less affected by the error. Using the program MAD, we compute the real β^* values at the different IPs for different bunch intensities, and we plot the results in Fig. 4 (horizontal) and Fig. 5 (vertical). We also compute the β^* values without phase error (dashed line). From the plots, one can see that the β^* values decrease with the beam-beam force, and that this reduction is not uniform if a phase error is also applied. Figure 6 shows the ”normalized beam area” obtained by putting together the previous plots. This should be inversely proportional to the luminosity, and we can see that, in this case, IP8 is clearly favoured. We can also see that the overall performance decreases because of the error, because the other three IPs lose much more than IP8 gains. Finally, if we divide each IP’s normalized beam area by the nominal one (Fig. 7), we find that the imbalance should decrease with the bunch intensity. Even if our way of measuring luminosities at a given current is not very accurate, we have not observed a clear trend in this direction. But we have also observed, all along the year, that the bunch size decreases when the bunch current goes down. If we introduce this factor in our simulation (dashed lines), we see that, in the range of beam currents we used in physics, the imbalance dependency on the current becomes negligible, and possibly below our measurement precision threshold.

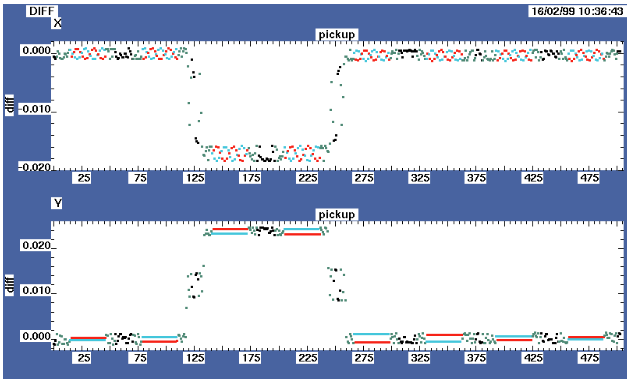

Figure 2: Phase Bump between arcs 3 and 5 induced by an error similar to the one used in our simulations. Without the beam-beam kicks, the bump is well closed and only a small beating is generated.

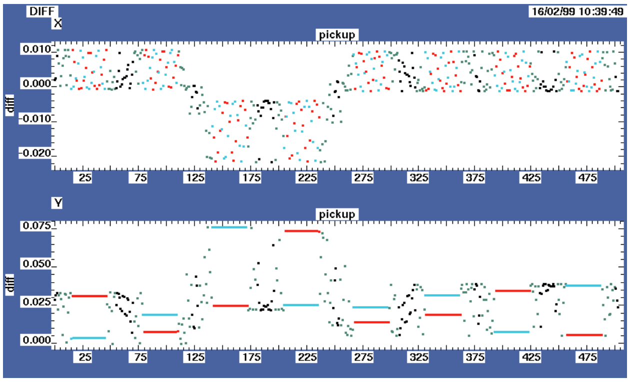

Figure 3: When beam-beam is introduced, the closure is lost. Beating is created everywhere.

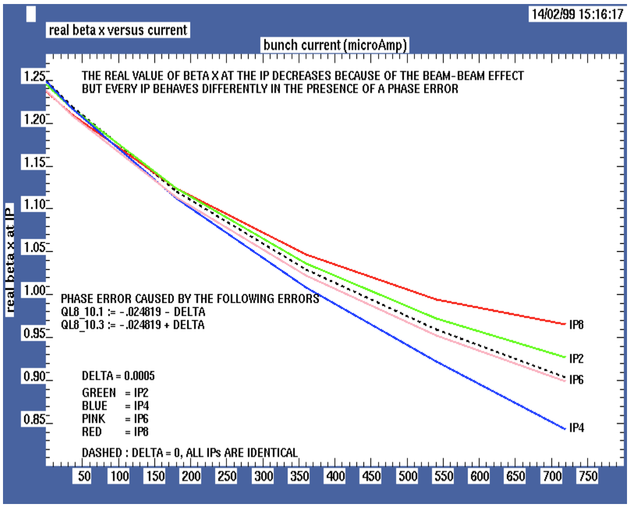

Figure 4: The horizontal β^*_S as functions of the bunch intensity. In the presence of beam-beam forces and of the phase bump between arcs 1 and 3, the β^* values at each IP become different.

2.5 β^* *Knobs and Aleph vs. L3 imbalance*

A natural extension to the concept of β^* compensation described above are the β^* trim knobs, very popular with OP. Their ambition is to improve (or balance) the luminosity

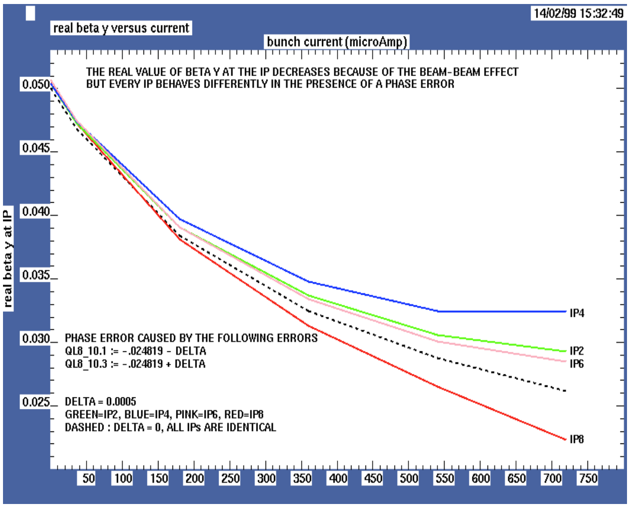

Figure 5: The vertical β_S^* as functions of the bunch intensity.

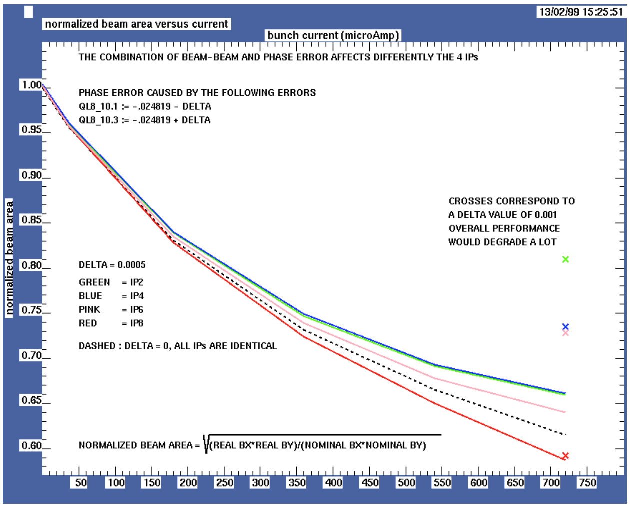

Figure 6: The normalized beam area at the different IPs.

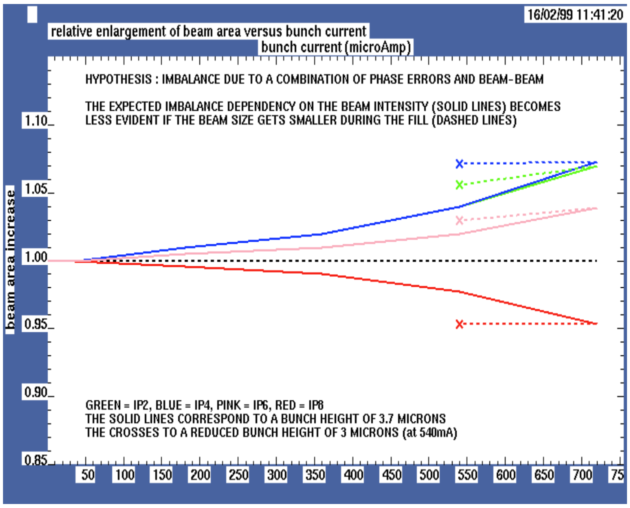

Figure 7: Relative reduction of beam area at the different IPs.

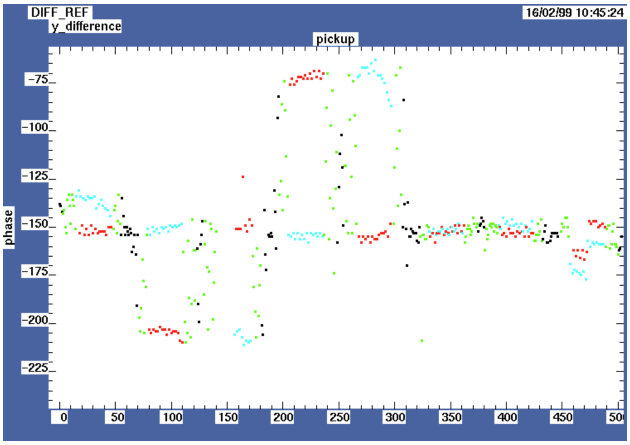

Figure 8: Vertical phase advance modification induced by the IP4 β^* trim, applied to help Aleph. (On the y-axes, 10 = 1 degree)

here and there by trimming this or that QS0 or QS1 set of quadrupoles. Ultimately they try to give the β_S^* a supplement of squeeze, to go beyond the specification of the optics. Unfortunately this is not achieved for free : all considerations that are taken into account while building a complete optics are here neglected. The result can be very bad especially if the optics itself does not have a lot of margin to play with, and if the knobs pretend to modify the β_S^* by a large amount. The situation can rapidly become unstable.

A further consideration is needed : these knobs, by acting on some quadrupoles, modify the phase advance around the machine. This, combined with the beam-beam, may generate an imbalance to an extent greater than the one expected. A confirmation of this fact can be found comparing the luminosity data for fills 5230-5308 with the fills 5308-5380, where one of such knobs was made active, to help the Aleph luminosity. The idea behind this knob was to further squeeze by a small amount ($\sim$10%) both vertical and horizontal β^*s in Aleph. This should have had no effect on the other experiments, but, as already mentioned, while the Aleph luminosity increased by 4%, the L3 luminosity unexpectedly decreased by the same amount. Figure 8 shows the phase advance modification due to this knob (and to a weaker one applied to IP8). It is possible to notice that the big phase beating generated by the knob in IP4 propagates to IP2 and IP6. For some reason, this turned out to be bad for L3.

Another example is given by the effect of the knob BXBYIP8, which should increase the luminosity at IP8 without affecting the other IPs. We introduced it in MAD and we run it with and without beam-beam forces. The results are summarized in the following table. As one can see, without considering the beam-beam forces one unit of this knob would give an additional 6% of luminosity to DELPHI, without perturbing the other experiments. If we introduce the beam-beam forces, however, DELPHI would gain 16%, but ALEPH would lose almost 3.5 %, and the others 1%. With two units of knobs the imbalance would be even bigger.

Table 4: Expected gains in luminosity (L with trim/L without trim) for the knob BXBYIP8.

Conditions	IP2	IP4	IP6	IP8
Beam-beam off				
trim = 1 unit	∼1.00	∼1.00	∼1.00	1.064
trim = 2 unit	∼1.00	∼1.00	∼1.00	1.158
720 μA/bunch				
trim = 1 unit	0.99	0.966	0.99	1.158
trim = 2 unit	0.94	0.84	0.94	1.34

3 CONCLUSIONS AND RECOMMENDATIONS

There exist many different possible sources of imbalance (dispersion, crossing angles, effective β^*, local coupling, etc.). Each source cannot be controlled individually to a perfect extent, but in a non pathological situation no single source can explain a 20% imbalance. On the other hand, if some of these sources combine to penalize an experiment, the result can quickly become untolerable.
Therefore we need

- Accurate ways of estimating the effect of the different sources, and possibly to correct them.
- Observation and recording of all imbalance related phenomena in the Log Book, as well as recording of whatever effective trims were made.
- Quick reaction time and good communication between shift teams.
- More accurate luminosity data from the experiments, and more sophistication in their analysis.

However, we should never neglect the final goal, that is to increase the overall performance of the machine. This goal is not at all in conflict with the achievement of a small level of imbalance, because in a perfect machine there is no imbalance.

4 ACKNOWLEDGEMENTS

Andre Verdier first introduced the idea that phase errors combined with beam-beam could be responsible for the imbalance. Werner Herr studied this effect and was able to simulate cases in which phase errors within the tolerances always admitted would be responsible for huge luminosity imbalances. The example shown earlier is derived from his simulations.

5 REFERENCES

[1] P. Raimondi, ”Dispersion Free IPs”, these proceedings.

[2] M. Sands. The Physics of Electron Storage Rings : An Introduction