

PROSPECTS FOR FUTURE FIXED-TARGET B-PHYSICS AT FERMILAB

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I. INTRODUCTORY REMARKS

Much of the contents of this introductory talk will have been supplemented by contributions elsewhere in these proceedings. Therefore this written version will be abbreviated as much as possible.

The purpose of this talk is not to review what existing fixed-target initiatives are doing or might do in upgraded mode. This restriction is not meant to imply that there is little value for b-physics in existing resources, now and in their future incarnations. However, it may be that to fully exploit the potential for b-physics in fixed-target mode, one needs to go well beyond that level. Therefore, a "bottoms-up" look at the question is appropriate, no matter how difficult the realization of some new facility may be in practice. And this workshop provides a timely opportunity to do so.

For me, the basic issues include the following:

A necessary condition for a major new investment in a b-physics facility utilizing hadron collisions, be it in fixed-target or collider mode, is that the number of produced $b\bar{b}$ quarks per experiment be large compared to the e^+e^- competition.

This seems to me to provide in-principle physics opportunities unavailable to the in-principle and probably in-practice highly efficient future exploitation of b-quark physics in e^+e^- data samples.

A second issue very important to this workshop is whether a big fixed-target investment is competitive with a collider investment of comparable cost.

There are arguments on both sides. Personally, I am open-minded on this point. But I think it is very important that the necessary homework be done.

II. RATES AND IMPLICATIONS FOR TRIGGERING AND EVENT SELECTION

A (very good) $e^+e^- \rightarrow b\bar{b}$ program may be projected² to yield $\sim 10^6$ - 10^7 $b\bar{b}$ /experiment. For a Tevatron fixed-target experiment, we assume 3×10^{-7} $b\bar{b}$ produced per interacting primary hadron and 3×10^6 live seconds per experiment. With a 10 MHz instantaneous rate as an upper bound for a "typical" open-geometry spectrometer, this yields 10^7 $b\bar{b}$ produced per experiment. If one writes 100 events per second to tape, this implies a rejection power of 10^5 . This should be contrasted with the situation in collider mode. Taking 3×10^{-4} $b\bar{b}$ produced per interaction⁴ and a "typical" $\int L dt$ of 3 pb^{-1} per experiment gives 6×10^7 $b\bar{b}$ produced, and a 50 kHz instantaneous rate.

These numbers taken by themselves imply an obvious in-principle advantage in event rate, signal/noise and $b\bar{b}$ yield for the collider option. However, this advantage is mitigated to considerable extent by the many constraints on spectrometer architecture not present in fixed-target mode. One must not jump to conclusions. In both cases, there is competitiveness, as defined above, with e^+e^- , although the fixed-target initiative appears marginal unless very high rate and acceptance can really be attained.

There is a third, distinct option. Perhaps there exist specialized fixed-target b-physics experiments which can run at rates much greater than 10 MHz. Indeed experiment E605 has already observed 10^4 b quarks running at 10 GHz interaction rate. What was seen was $\bar{b} \rightarrow \mu^+ \mu^-$; detectors looked not directly at the target but only through a shield of several feet of lead. Can one do something similar (without the lead) for specialized decays of b-hadrons, such as

$$B_{d,s} \rightarrow \pi^+ \pi^-, K^+ K^-, \rho^-, K^\pm \pi^\mp$$

$$\text{or } \Lambda_b \rightarrow p \pi^-, p K^- ?$$

And is there enough physics output to justify the considerable input of effort? The answers are not clear, but it is important to ask the questions. And it is gratifying that the questions are being asked. In these proceedings will be found⁵ a description of an incipient proposal (P-789) to use the E-605 apparatus (plus a silicon microstrip front end) to search for 2-body nonleptonic decay modes. An even more speculative approach using remote-imaging of the vertex region (via alternating gradient spectrometer elements focussing secondaries onto downstream silicon detectors) has also been studied.⁶ And other ideas on high rates have also been entertained at a series of Fermilab meetings on super high-rate b-physics possibilities and problems.⁷ Most of this work is incorporated in one way or another in these proceedings.

However speculative all this is, one should not forget that in every (good) spill from the Tevatron, several million $b\bar{b}$ pairs are produced. Regrettably these are buried in beam dumps. But the challenge is there.

III. A BIG NEW SPECTROMETER?

For a long time there has been occasional idle talk about a big new fixed-target spectrometer for heavy flavor physics. But finally something has been done. It is ironic that it has been done not for the Tevatron but for the SSC, in its series of summer workshops. In the Berkeley SSC Workshop last summer, a spectrometer design for SSC b-physics was produced which, without any modification whatsoever, serves as an excellent "reference-design" for a Tevatron fixed-target spectrometer. The angular acceptance is 5.7_{-2}^{+2} mrad $< \theta < 350$ mrad. It is designed to operate at a luminosity of $10^{32} \text{ cm}^{-2} \text{ sec}^{-1}$, i.e. a rate of ~ 10 MHz, with RF structure similar to what exists at Fermilab. The device is 80m in length, has two magnetic stages (each magnet aperture is 2m in diameter), 3 RICH counters, 3 TRD's, 2 large calorimeters, and a large amount of silicon and conventional tracking. Event selection is via a high- p_T electron with non-vanishing impact parameter (as seen in the silicon system), together with a D^* associated with the semileptonic decay, reconstructed on-line.

Whether or not this detector is optimal for the SSC and/or the Tevatron, it is evidence that acceptance and rate problems for the Tevatron fixed-target and SSC experiments are quite similar. The signal/noise ratio, alas, is vastly different. If it is possible to accomplish anything of significance at the Tevatron with such a device, it should be easy to really clean up at the SSC.

This serendipitous similitude of Tevatron and SSC b-physics detectors suggests a political strategy as well. Perhaps this spectrometer could be developed as an SSC initiative using SSC detector R&D funds, commissioned in a Tevatron external beam during SSC construction, and then moved into the SSC at the appropriate time. This would have the advantage of physics productivity during the SSC construction/detector R&D phase. This advantage could be extremely important were there any significant delay in SSC commissioning; even in lean funding years support of such a detector out of the SSC budget might not be too difficult.

IV. CONCLUDING REMARKS

Some of the questions I believe this workshop and other studies to follow must address include the following:

- 1) To what extent can existing fixed-target experiments/facilities (and or smooth upgrades thereto) address B-physics competitively with e^+e^- facilities (including future extensions in that program)?
- 2) If existing experiments/facilities are competitive, which ones offer what physics? What are the strengths and weaknesses?

- 3) Should/must one consider a brand new fixed-target spectrometer from scratch? If so, what about the technical specifications, cost and support demands, time scale, and sociology - are there enough physicists interested in it?
- 4) Is the super high-rate option viable? Is the physics scope broad enough? And is it technically feasible?
- 5) In any fixed-target option, what are the crux technical issues?

And last but not least

- 6) How should one rank Tevatron fixed-target opportunities relative to Tevatron collider opportunities (superior, inferior, complementary?) in terms of physics yield, technical ease/difficulty, practicalities (e.g. real estate, time scale, sociology . . .), and usefulness as a stepping-stone to the SSC?

The promise of Tevatron b-physics in all three modes - collider, conventional fixed-target, and high-rate fixed-target - seems to me to be very high. However the difficulties are great. Not everything can be tried. It will be important to make the choices wisely.

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