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COMMENTS ON THE REALIZATION OF A TAU CHARM FACTORY*

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Abstract

Three issues relevant to the success of a tau charm factory are discussed: producing a complete catalog of D meson decays, understanding backgrounds in studies of rare decays, and a τcF detector as a fourth-generation collider detector.

1 INTRODUCTION

Near the close of the Marbella τcF Workshop, I was asked to participate in a panel discussion focussing on the issues that most need to be addressed in order to realize a τcF somewhere in the world. Partially in response to unfolding discussion, I offered the following three comments.

2 TOWARDS A COMPLETE CATALOG OF D DECAYS

To realize a τcF in the world, the importance of its physics program must be justified to the world HEP community. This is a particular challenge in the face of the very successful programs in charm and tau physics at B Factories, LEP/SLC, and in fixed target experiments. What makes the physics of τcF unique and compelling?

Much has been said here concerning the unique probes of light hadron dynamics afforded by a very high statistics sample of J/ψ decays. Much more has been said concerning the unique kinematics of tau decays near production threshold; and coupled with the great appeal of using tau decays for fundamental tests of the Electroweak theory, this is a strong argument for a τcF in itself. The success of charm physics programs elsewhere make the appeal of D physics studies less compelling.

However, one should not forget that D mesons are produced with unique kinematics, as well, at the ψ''. This becomes important in the context of a "dream" that many of the experimentalists in the field share: a complete catalog of D decays.

Presently, 2/3 to 3/4 of all D^0, D^+, D_s decay modes are either measured or inferred. This is not as satisfying as in tau physics where we fret over 3% discrepancies. Of course, tau physics is theoretically "clean", and we expect no mysteries. That the branching fractions don't sum to one is merely an experimental nuisance, which we fully expect to be settled in time.

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In D decays, the theoretical description is much more complex and interesting. There are many models of exclusive decay, incorporating Electroweak, QCD, and rescattering effects in a variety of ways. As yet there is no quantitative understanding of the total lifetime, or even of the lifetime ratio $\tau(D^+)/\tau(D^0)$.

Even if the situation were more advanced theoretically, it would still be true that experimentally, we want to understand D meson decays at least at the same level as we do tau decays, if only as a challenge to the theorists. That means we need to catalog *all* D decay branching fractions above 10^{-3}, and verify experimentally that they sum to one. This will truly test the two-body dominance assumption underlying our qualitative understanding of $\tau(D^+)/\tau(D^0)$, and provide valuable data for the more ambitious model builders.

Why is it easier to catalog tau decays than D decays? Of course, there are fewer, and simpler decay modes of the tau. But another important reason is that we begin with a simple initial state: $\tau^+\tau^-$ production with no accompanying particles. We can use tagging methods to obtain an unbiased inclusive sample of taus. We can do this at *all* beam energies, including B Factories and LEP/SLC.

The decay $\psi'' \to D\overline{D}$ affords us a similarly simple production kinematics. In fact, it is simpler still: there is no missing ν in a D tag, so the recoiling $\overline{D}$ 4-vector is known completely. This is a very powerful tool for a systematic cataloging of D decays, which already began to be exploited at Mark III and BES.

Best of all (for our purposes here), these production kinematics are *unique* to a τcF. At a B Factory, many dominant and rare decay modes will be studied, one at a time, fairly well. But it will be very difficult to fully catalog all modes in an unbiased way; thus, it will never be truly satisfying to the experimentalist with a "dream".

It goes without saying that in order to make full use of the unique kinematics of ψ'' decay at a τcF, the detector must have a truly 4π, full acceptance for all particles recoiling against a tag. This will not be the last time I will mention the importance of a truly superb detector in achieving the goals of the τcF physics program.

3 RARE DECAYS

Here I refer to *all* of the rare decays accessible at a τcF: D meson decays involving FCNCs, penguins, annihilation, mixing and CP violation, tau decays violating lepton flavor conservation, *etc.*.

The high luminosity of a τcF provides us with the opportunity to explore this physics with greater precision than ever before. One should not forget, however, that B Factories will also have high luminosity and high rates for these decays.

Our challenge is to fully exploit the potential of Factories for exploring branching fractions on the order of 10^{-7} by *reducing backgrounds* below that level.

At present, the "toy" Monte Carlo studies performed at τcF and B Factory work-

shops have suggested that there are "no" physics backgrounds which will ultimately limit our ability to see the rare decays. But experience tells us that we always run up against *detector* backgrounds eventually, and often sooner than we had anticipated.

Backgrounds from fake electrons, mistracked or mismeasured particles, hadronic secondaries, cracks and holes in the acceptance, *etc.*, are notoriously hard to anticipate, control, or model. The most nefarious backgrounds come from detector response *tails*, not the more easily modelled Gaussian response of the detector. These backgrounds thus cannot be modelled with a toy Monte Carlo. To anticipate them (before the data arrives, at which time they quickly become obvious) we need *fully detailed* high-statistics Monte Carlo studies, with all the non-Gaussian tails put in as reliably as possible.

This of course requires lots of care, time and energy. Are we willing to put in the work? I maintain that without it, all claims that we have *proven* our ability to achieve the ambitious goals of a τcF will be met with skepticism. Much of the skepticism will of course remain even in the face of detailed Monte Carlo studies (after all, it is still just a Monte Carlo!), but at least we will have done the best we can short of building the τcF with proposal funding.

4 TOWARDS A FOURTH GENERATION DETECTOR

Is our τcF detector truly capable of these ambitious goals? Remember, this is a fourth generation experiment. In SLAC-ese, we can call it the Mark IV.

- Mark I – *discovery* – 8 spark-chamber "planes", LGW, $\cdots$
- Mark II – *exploration* – 20 prop-chamber "planes", Liquid Argon outside the magnet, crude μ-walls; (the Crystal Ball had calorimetry but no magnet; DELCO had Čerenkov ID but no calorimetry), $\cdots$
- Mark III – *detailed study* – 50 drift chamber planes, finely-segmented but low-res calorimeter inside the magnet, crude μ-walls, $\cdots$
- τcF Mark IV – *detailed study, profound exploration, maybe even discovery!* – precision low-mass DC, precision crystal calorimeter, finely-segmented HCAL, precision particle-ID, $\cdots$

And let's not forget the collider, offering a factor of 10 times the production rate or more! Can the τcF detector achieve a factor of 10 improved systematics?

This challenge can only be truly met by building the detector and doing analysis. But we won't have the opportunity to build it until we rise to that challenge by *proving* it can be done *before* we build it. The only way I know of to attack this problem with limited funding is with detailed Monte Carlo studies.

When the τcF meant Spain, it was easy to put the responsibility on the heads of the ambitious, energetic, forward-thinking young Spaniards. Now, however, we all have

the responsibility to contribute, by dedicating our intellect and workstations to running and improving on the Sevilla Monte Carlo.

5 ACKNOWLEDGEMENTS

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