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A Matter of Styles* in the Accelerator World

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Abstract

The word "styles" used here should be understood as explained in the footnote, although they may not be always "excellent". This article is based entirely on my personal experiences in the past forty-odd years, mostly at Fermilab, and as such, a fair amount of prejudices is unavoidable. A matter of styles is discussed in three categories: 1) type of accelerators, 2) generational differences, and 3) different laboratories. The intention of this article is not to label one style as superior to or even preferable over others. Rather, the difference of various styles should be understood clearly in order to appreciate merits and defects of each. A story on the linear optical parameters (Courant-Snyder parameters or Twiss parameters), which are familiar not only to accelerator physicists but to many experimenters as well, is given in Appendix

I. Introduction

The word "styles" is used in the title instead of other words such as "approach", "method" or even "philosophy" to indicate that the intention here is not to discuss superiority or inferiority of one style in comparison with others but to appreciate the differences among them clearly so that one can choose one's own style with conviction. I do not agree with an assertion by some people that what is important is not the style but the contents. We all agree that, in any accelerator work, we must obey Newton's law of

* **style** 6a. "a quality that gives distinctive excellence to something and that consists esp. in the appropriateness and choiceness of the elements combined and the individualization imparted by the method of combining." (*Webster's Third International Dictionary*).

motions and Maxwell's equations for electromagnetic fields. What makes each work, each accelerator and even each laboratory distinct is the style that characterizes them. For an old accelerator man like me, it is sad to see a uniform gray style gradually dominating many laboratories and most of the works in accelerator physics. At the same time, a convergence of approaches and methods in different types of accelerators is perhaps something inevitable and long overdue.

II. Type of accelerators – linacs/cyclotrons/synchrotrons ...FFAG

I started working on accelerators early in 1962 at Yale University where there was an effort going on to build a meson factory* using an 800 MeV proton linac. By 1965, it became painfully obvious to us that Los Alamos is a better place for such a facility in terms of manpower and political influence. Although I continued working on linac as part of the collaboration with Los Alamos, I began to spend more time for the modification project of synchrocyclotron at Nevis, Columbia University. Up to that time, issues such as injection and extraction were totally unknown to me. The use of cylindrical coordinate system in the beam dynamics was also a new experience. For various reasons, which are not relevant to the issue of styles, the modification project ended up as a disaster and I moved to the newly authorized project at the National Accelerator Laboratory, now Fermilab (or FNAL). The 200MeV linac was still under construction and my initial assignment was in a familiar territory. As the construction proceeded, my assignment followed the beam; beam transport from linac to booster, booster, transport line from booster to main ring, main ring, extraction, and the external beam lines. Meanwhile, Bob Wilson's ambitious effort to build an accelerator using superconducting magnets started and I was gradually drawn into it through a committee affectionately called "Underground** Parameter Committee (UPC)". The name of this superconducting ring has been changed from the original "energy saver" to "energy doubler", and finally to the current "Tevatron".

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* I am not sure whether the term "factory" was first used at Yale in connection with this project.

** The word "underground" was used for two meanings, one obvious and the other to suggest that the effort was clandestine at that time.

The intention of talking so much on my past experience is to share the feeling I have with you, the feeling that I have been very fortunate in having opportunities to work on different types of accelerators and that I am not totally unqualified to comment on the stylistic difference of various accelerators.[*]

In 1984, there was a conference at Steamboat Springs, Colorado, *Conference on the Intersections between Particle and Nuclear Physics.* Fred Mills asked me to give a talk, mostly tutorial in nature, on the use of sextupoles in synchrotrons. He felt that many nuclear physicists were seriously considering synchrotrons as their main tool of research as the energy range of interest in nuclear physics was extended beyond ~ GeV. This was somewhat ironic but also appropriate; until I joined Fermilab and started working on its main ring, I didn't know what sextupole is or what its role is in accelerators. I did not encounter the concept of "chromaticity" in my work on linacs or cyclotrons. I accepted Fred's invitation since it provided me with an opportunity to express my unhappiness with synchrotron crowd, or synonymously, with accelerator physicists in particle physics, at least some of them. Let me make a lengthy quote here from the talk I gave since I still feel that there should be a better understanding of what is relevant and what is not in dealing with different situations even when an accelerator happens to be called cyclotron or synchrotron.[**]

"The basic equations describing the particle motion in an external electromagnetic field are of course identical for any type of accelerators but the actual procedures used to design a machine and the emphasis on specific aspects of beam dynamics are not always shared by the cyclotron builders and the synchrotron builders. One can probably argue that the beam dynamics for cyclotrons is more "involved" than the one for synchrotrons. Certainly there exist situations in which synchrotron designers (at least some of them) may look incredibly naïve to cyclotron builders. One example of this may be seen in the calculation of tunes for relatively small synchrotrons (such as rings for the synchrotron light source or for the storage and cooling of antiprotons) where the bend angle of each dipole is of the order of a few degrees instead of a few milliradians and the momentum acceptance is measured in percents instead of 10^{-4}. The standard computer programs for the synchrotron design such as SYNCH or PATRICIA may or may not give the right answer depending on how the field behaves near the edge of magnets. In the same way,

[*] It should be obvious from this story that I have no experience in electron accelerators.

[**] *AIP Conference Proceedings* No. 123, p. 415 (1984).

there are a few tricks of the trade in the design of synchrotrons that must be kept in mind by cyclotron builders ..."

It is unfortunate that many young accelerator physicists regard Hill's equation as given in "Courant-Snyder" an exact description of the particle motion in any accelerators. To be sure, there is an exact treatment in its Appendix but I suspect not many beginners pay much attention to that part. This situation is not remedied much even in relatively new textbooks. For example, there is not much discussion in the main text on the applicability of Hill's equation in otherwise excellent *The Principles of Circular Accelerators and Storage Rings* by Bryant and Johnsen.* Here again, one must go to appendix. How many of us in the high energy accelerator world are aware of the wonderful works by, for example, Gordon or Hagedoorn in cyclotrons? It is indeed a shame that we still have separate international conferences on linacs, cyclotrons and high energy accelerators. The establishment of National Particle Accelerator Conference was meant to remedy this defect but, as we all know, the meetings are conducted more like conventions rather than conferences.

Some people might argue that what I have said here is nothing but a question of approximations we use under various circumstances. The reason I insist using the word "style" is this: when a certain approximation is associated with a particular type of accelerator so closely and for so long, the mere mention of names such as "cyclotrons" or "synchrotron" is used as a license to use that approximation in an indiscriminate manner. Because of this tendency, the recent revival of an accelerator commonly called FFAG is a welcome opportunity for a remedy of this unfortunate practice *as long as we do not attach any preconceived idea to the name FFAG.* It is nothing but a cyclotron at certain energy ranges but behaves more like a synchrotron at other energies. As such, the proper study of FFAG could play a significant role not only in the training of young people but also in the rehabilitation of older generation (myself included).

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* Incidentally, does a finite closed orbit exist in the presence of dipole errors when the tune is an integer? If you are a blind believer in Hill's equation, the answer would probably be "No". This book is of no help in answering the question even if one studied Appendix C. One must go to the end of Appendix A of Courant-Snyder to find the proper explanation.

III. Generational differences

More than anything else, the extensive use of computers and the heavy reliance on what comes out of computers separate the present generation from the (rapidly vanishing) older generation. This is so much so that young people might regard experiences in the past as mostly irrelevant. It is also not undeniable that, when a member of the old generation criticizes this tendency, he (or she) does so with a tinge of jealousy mixed with admiration. Not so long ago, I still remember, tracking of particle motions over one million revolutions was regarded as something unreal. Now it is almost routine; if it is not done, others would say it is because of laziness.

As more and more can become "computable", the modeling of an existing machine to understand its properties and of machines to be built to predict their expected performance has become an essential ingredient of any accelerator project. One recent example is the main injector (MI) at Fermilab for which an enormous amount of magnet data has been used to simulate the particle motion. The commissioning was uneventful (and lacking in any excitement) as nothing unexpected happened. Nowadays, when an issue with no easy answer is raised, one often hears a cry of "Why don't you simulate?". This should be contrasted with the attitude of old generations, "We would never know until we build the machine and run it." Such familiar items as the golden closed orbit or the best operating point may soon become relics of the past symbolizing our inability to predict the property of a future machine by simulations.

I really wish I could use computers as effortlessly as so many members of the young generation are doing. I cannot deny, therefore, that there is an element of sour grapes in what I say below for the current heavy reliance on simulations in general. This is a risk I cannot avoid taking if I am to express my strong sentiment.

There are two types of computer codes that we use in accelerator calculations. One is large-scale, all-inclusive and often very complicated codes such as MAD and

TOSCA and many others for particle tracking, cavity and magnet designs and space charge calculations, to name a few. The other is "home-made" codes, a relatively small in scale and often for a very specific purpose. Most of the former are so extensively used and so well established that one is inclined to use them without paying much attention to their limitations and applicability. A typical example is MAD created at CERN a long time ago. As it has gone through many revisions, one might think that it has by now eliminated all shortcomings that may have existed at its early stage. This is not true. For one thing, it does not handle a combined function magnet properly (unless one artificially divides the magnet into many pieces). More seriously, the symplectic condition is not preserved in its tracking mode so that the beam emittance may increase or shrink without any physical cause for such a change. If one reads its manual very carefully, this is obvious. But I know several instances in which this caution has been ignored and confusing and erroneous results have been obtained. When using a large-scale, complicated code, I should like to see users paying more attention to its limitations and applicability. Otherwise, even an elementary quantity such as the fractional part of tunes may come out wrong as it actually happened at CERN for its anti-proton ring.

Speaking of symplectic restrictions, it is important to understand when one can violate some physical principles and when such a violation is fatal. If the tracking is done for a few hundred turns, a standard integration such as Runge-Kutta may be more accurate than a kick code (such as TEAPOT) in spite of the fact that, unlike the latter, the former does not usually respect the symplectic condition. One sometimes hear a snide comment, "But your treatment violates Maxwell's equations!" but never about violating Newton's equations, this in spite of the fact that Hill's equation does just that. One may violate Maxwell's equations, or anything else for that matter, if the situation justifies such an approximation, and we do this all the time.* This is of course different from a case in which the violation is crucial in obtaining the claim one makes.

It is a trivial thing to say that, for a simulation to be meaningful, the input data such as the measured magnetic field must be accurate. Aside from the troublesome

* Stochastic cooling does NOT violate Liouville's theorem. See Bryant and Johnsen (*ibid.*), Appendix A.

difference in the convention that is used to express high order multipoles in magnets (for example, b_3 is normal octupole at Fermilab but normal sextupole at CERN), there is almost always a confusion regarding the coordinate system used in magnetic field measurements and the one used in tracking. "Upstream end" and "downstream end" of a magnet is another potential source of confusion. A number of technical notes have been written at CERN and at Fermilab (and most likely at other places) explaining the transformation of b_n (normal component) and a_n (skew component) from one convention to another. Nevertheless, I have seen many cases in which such a possibility of errors never entered the mind of (young) people performing a massive simulation. One might object that I include this as a matter of styles. It is sloppiness, I agree. But again it becomes a style when this is a standard procedure rather than an exception.

In old days, it was a major task to simulate particle motions in order to find the so-called dynamic aperture of a machine in the presence of variety of errors and misalignments. As a consequence, results from such a simulation were "precious". We had to squeeze out as much useful information as possible given a limited number of cases. Now the situation is quite different. Simulations for many dozens of random cases are routinely accumulated and presented in gory details. Does this mean we are learning more? Very often, we hear a statement such as " Case #n gives a dynamic aperture of X mm_mr while case #m gives Y mm_mr. " with Y twice as large as X. Or "The dynamic aperture is at least X mm_mr for 67% of random samples we studied." as if to imply that if we built one hundred machines, 67 of them would have that dynamic aperture, which of course is absurd. There will be one and only one machine and we should know what it would be instead of a statistical prediction. Shouldn't we try to answer the question "Why?" instead of simply saying this is a lucky case or an unlucky case? If one cannot tell why such a variation from case to case exists, one should be honest enough to admit that simulations alone are not enough to find the necessary information. To be fair, I should not categorize this as an example of generational difference in styles since not all members of the older generation are blameless in this respect.

IV. Difference among laboratories

I must admit, at the outset, that I am one of those who have been brainwashed by late Bob Wilson, the first director of Fermilab. There may still be a few dozens of people at Fermilab who belong to this group but the distinct character of the laboratory is gradually fading. I don't know what is happening to other laboratories but I suspect the same tendency of getting transformed to a "characterless" entity must be unavoidable. With ever-increasing exchange of personnel and all-too-frequent meetings in the accelerator world, this is quite understandable and may even be a welcome phenomenon. What I thought as the difference in styles among laboratories may well be nothing but a product of nostalgia. For this reason and also to make my task easier, I will simply list a number of anecdotes I have witnessed, mostly at Fermilab but at other laboratories also. These are merely anecdotes and one should not draw any conclusion from them as to which style is better or to be preferred over others.

(1) In the early days of Fermilab, we were painfully aware of a large momentum spread of the beam injected into Booster. The obvious solution to us was to install a debuncher and this need was presented to Bob Wilson. (Some people may wonder why debuncher was not there to begin with. The only answer I can think of is that it did not come free.) Wilson immediately pulled out a piece of blank paper and scribbled boldly, **"We will not build a debuncher!"** That settled the matter decisively but the need to reduce the beam momentum spread did not disappear. We ended up adjusting the phase between the last two linac tanks so that the beam going into the last tank was closer to the unstable fixed point in phase space than to the origin. The beam motion was quite nonlinear and we managed to reduce the beam momentum sufficiently to satisfy the immediate requirement. It is not surprising though that, later, we had to build and install a debuncher after all.

(2) Soon after the commissioning of CERN SPS, there was a talk at Fermilab by someone from CERN describing how easy it was to circulate the beam, this in contrast with many weeks of painful struggles for the commissioning of Fermilab Main Ring. When told that only a few steering magnets out of 200 or so were needed to circulate the beam in SPS, our initial reaction was: "What a waste!" (undoubtedly mixed with envy). Later, we learned that the measured tune was wrong by one unit since they didn't "count the mountains".

(3) Because of save-money-by-all-means policy of Wilson, the main ring magnets were defect in many respects. Of 774 dipoles, I suspect not more than 50% managed to survive to the end of main ring life. The field was a·mess, especially at injection, with not only sextupole contents but octupole and decapole as well. The existence of decapole was obvious from the behavior of chromaticity. Instead of installing correction decapoles in addition to the existing correction sextupoles, someone came up with an idea of inserting G10 between the upper and the lower half of sextupoles, thereby creating decapole-like field for free. This worked very well and the momentum acceptance of the main ring was markedly improved. The same man later tried his hand in a similar inventive manner at SLAC, which ended up as a disaster.

(4) When BNL was in trouble because of ISABELLE magnets, some of us from Fermilab were asked to participate in the discussion of alternative choices there. At the meeting, the chief mechanical engineer of the BNL accelerator department declared that superconducting magnets for any accelerators should be built "like a tank" and that the Fermilab doubler magnets will sooner or later "crumble to the floor in pieces". That was more than two decades ago. Meanwhile, after the retirement of this man, BNL has built and operated successfully the superconducting ring RHIC. I am not sure whether RHIC magnets were built "like a tank".

(5) Accelerator theory was not really regarded at Fermilab as something to pay attention to, at least in its early days. There were 200 or so chromaticity correcting sextupoles in the main ring. Inevitably, one or two of them had to be removed either

because they did not work or something more important had to be installed in that location. Instead of telling technicians to remove others at the same time so that periodicity of at least two would be maintained, we used to say, "Oh, it's just one or two out of 200. What difference could that make? Go ahead and remove them. But be sure to check with us if more have to be removed." For technicians, though, if two out of 200 is OK, three or four couldn't be so different. After all, what is this abstract concept of periodicity playing any role in the real world? Consequently, more and more sextupoles were removed and a very sizable third-integer stopband was created without any of us knowing. The operation of the machine became impossible with the beam so sensitive to the chromaticity adjustment. One day, a man from CERN had an idea that maybe this is due to the 61st harmonic component (the tune was near 20.33) created by missing sextupoles. (So he claimed later although others said they had the same idea.) We went down to tunnel and reconnected some sextupoles so that at least the periodicity of two would be recovered. With this done, the beam intensity was immediately doubled. The news reached CERN (undoubtedly through our man from CERN) and CERN director general praised it as the finest example of the importance of accelerator theory. Although this story appeared in Fermilab News, none of us got any credit and there was no discernible change toward accelerator theory at Fermilab.

(6) It may sound unbelievable now but back in 1970's, nobody was sure of building hundreds of superconducting magnets usable for an accelerator. At Fermilab, we established a set of criteria (somewhat arbitrarily, I must admit) for each multipole component (b_n, a_n) that must all be satisfied for a magnet to be accepted. Every week on Monday, we had a meeting to accept or reject a number of magnets that were measured during the previous week. (Some people called it "Market Day".) Naturally, magnet builders wanted to see all magnets accepted, and members from the accelerator theory group wanted the field quality of the ring to be maintained at the agreed-upon level. It was an interesting give-and-take discussion, not infrequently heated and even emotional. Helen Edwards was in charge of the technical part of the project and she was naturally anxious to finish the construction in time. When we rejected a magnet for one or two components exceeding the allowed values, she would say in desperation, "OK, you may

reject this magnet if you can prove that, with this in the ring, the machine would never work!" Again, just like the chromaticity correcting sextupoles, it was one out of 774 magnets. How can anyone prove that the machine would not work? This was such a powerful rhetorical question that it was invoked many times to get questionable magnets accepted. Clearly something had to be done before it was too late. A crude version of what we now call "sorting" was thus born purely out of necessity.

Finally, what was the style of SSC Laboratory? My direct association with SSC was just one month during the summer of 1993, only two or three months before its termination. Even with this limited observation, it was to me a big blob devoid of any characters. This is surprising as there were many accelerator personnel from Fermilab who were in responsible positions. I suppose the organization was simply too diverse to be managed by a few people no matter how much they were dedicated or hard working. At the same time, I cannot help wondering if, for a project of such a magnitude, it was absolutely essential to have a single leader with a streak of "ruthlessness" and "meanness". The director of SSC Lab was a first-class physicist with impeccable credentials, chosen by the high energy physics community. But his style was certainly not ruthless or mean.

Apologies

This article is based on my talk given at the annual FFAG meeting to present the progress of the project at KEK. My host at KEK, Professor Yoshiharu Mori, allowed me to talk on anything with or without direct connection to FFAG, and I took advantage of his generous offer. I must apologize to him and to those who were present at the meeting for deviating too much from the title. My feeble excuse is that, in Japanese society, there is still an indulgence for an old man's manner.

Appendix. Courant-Snyder Parameters or Twiss Parameters?

A rather nasty review of a book appeared in the August issue of Physics Today. The review was by Ron Ruth (SLAC) and the book, *An Introduction to Particle Accelerators*, was by Ted Wilson (CERN), both of them my acquaintances for almost thirty years. Since I have not seen the book, I cannot say whether the review is fair or not, but one paragraph caught my eyes and it somehow motivated me to add this appendix although it has little to do with the issue of "styles". (Or maybe it is a matter of styles Ron Ruth is objecting.) Let me quote the paragraph:

> "Wilson's constant referral to the Courant-Snyder matrix and the Courant-Snyder beta function as the Twiss parameters and Twiss matrix is an incorrect attribution that permeates the field. Some years ago, Frank Cole contacted Richard Twiss, who didn't understand why the parameters were named for him."

I am not sure how Ron knew about Frank Cole contacting Twiss. It all came about one day at the Fermilab cafeteria when several of us started arguing about this issue of Courant-Snyder or Twiss. At the time, Frank was the editor of *Accelerator Physics* and a reviewer of an article submitted to it raised this question. At least that's the way it started if my memory is not failing me. Someone said it must be another example of CERN *vs* US, CERN (and Europeans in general) for Twiss parameters and US favoring Courant-Snyder. This view seemed credible. For example, if we look at the marvelous booklet, <u>A SELECTION OF FORMULAE AND DATA USEFUL FOR THE DESIGN OF A.G. SYNCHROTRONS</u> by Bovet and others at CERN, the familiar parameters β and α are called Twiss parameters (although the extension of this to the term Twiss matrix must be an invention by Ted Wilson). However, there is at least one counter example to this view. The textbook by Bryant and Johnsen (already mentioned in this article) says on p. 56, "The parameters α, β and γ are known as the *Courant and Snyder parameters* and sometimes as the *Twiss parameter*". The footnote says " Although these parameters are often referred to as the Twiss parameters, it appears that they did not in fact originate with him." This statement agrees with the telephone conversation Frank Cole had with Twiss and repeated by Ron Ruth in his review.

After this finding by Frank, we reached a consensus that the term Twiss parameters is still useful when they are regarded as one convenient way to describe an ellipse, especially a transverse beam shape in phase space.* They are therefore meaningful only for describing a beam, either in a ring or in a transport line. Courant-Snyder parameters are, in contrast, the property of a *periodic* lattice and they exist independent of any beam in it. To be sure, for a matched beam, the numerical difference disappears but they should not be considered as one and the same physical quantities. Courant-Snyder parameters are often used to express the transfer matrix from one point in a periodic lattice to another but this is just a convenience when one wants to follow the motion of a single particle in it. This was of course a private understanding we reached at that time, and I don't think it has ever acquired an official status anywhere, even at Fermilab. Nevertheless, the use of terminology "Twiss *matrix*" is akin to a crime to me.

The principle of strong focusing was patented by Christofilos in 1950 but it was independently published in *Physical Review*, **88** (p.1190) two years later by Courant, Livingston and Snyder. As we all know, the mathematical treatment of strong focusing by Courant and Snyder appeared in *Annals of Physics*, **3** (pp.1-48) in 1958, six years after the Physical Review article. Immediately following the paper by Courant, Livingston and Snyder is a paper by John Blewett (p.1197) suggesting a use of a succession of electric or magnetic quadrupole lenses as the focusing system of linear accelerators. Two years after the publication of Blewett's paper, there appeared in the *Reviews of Scientific Instruments*, **26** (p. 220) an article by Lloyd Smith and Bob Gluckstern, "Focusing in Linear Ion Accelerators". In it, they discuss the stability conditions using matrix formalism and the mathematical treatment is remarkably similar to that of Courant and Snyder, which was to appear four years later in 1958. It is not clear whether Courant and Snyder were aware of this work by Smith and Gluckstern; it is not cited in references. The crucial shortcoming (to me) of Smith-Gluckstern is that it considered the transfer matrix at the center of quadrupoles only. The phase advance μ and the amplitude

* Does anyone use them for a beam shape in longitudinal phase space?

function β are there* but not the parameter α since it is naturally zero there. Much later, I asked Bob Gluckstern why they didn't generalize the transfer matrix to locations other than the quadrupole centers. He said it never occurred to them that such a generalization is needed to discuss the acceptance of a focusing system in linacs. Considering the mathematical prowess of Gluckstern, I am sure he could have developed formalism very much like the one by Courant and Snyder. Then, we would have been blessed with Smith-Gluckstern –Courant-Snyder parameters (together with Twiss parameters)..

* The symbol they used was γ, not β.