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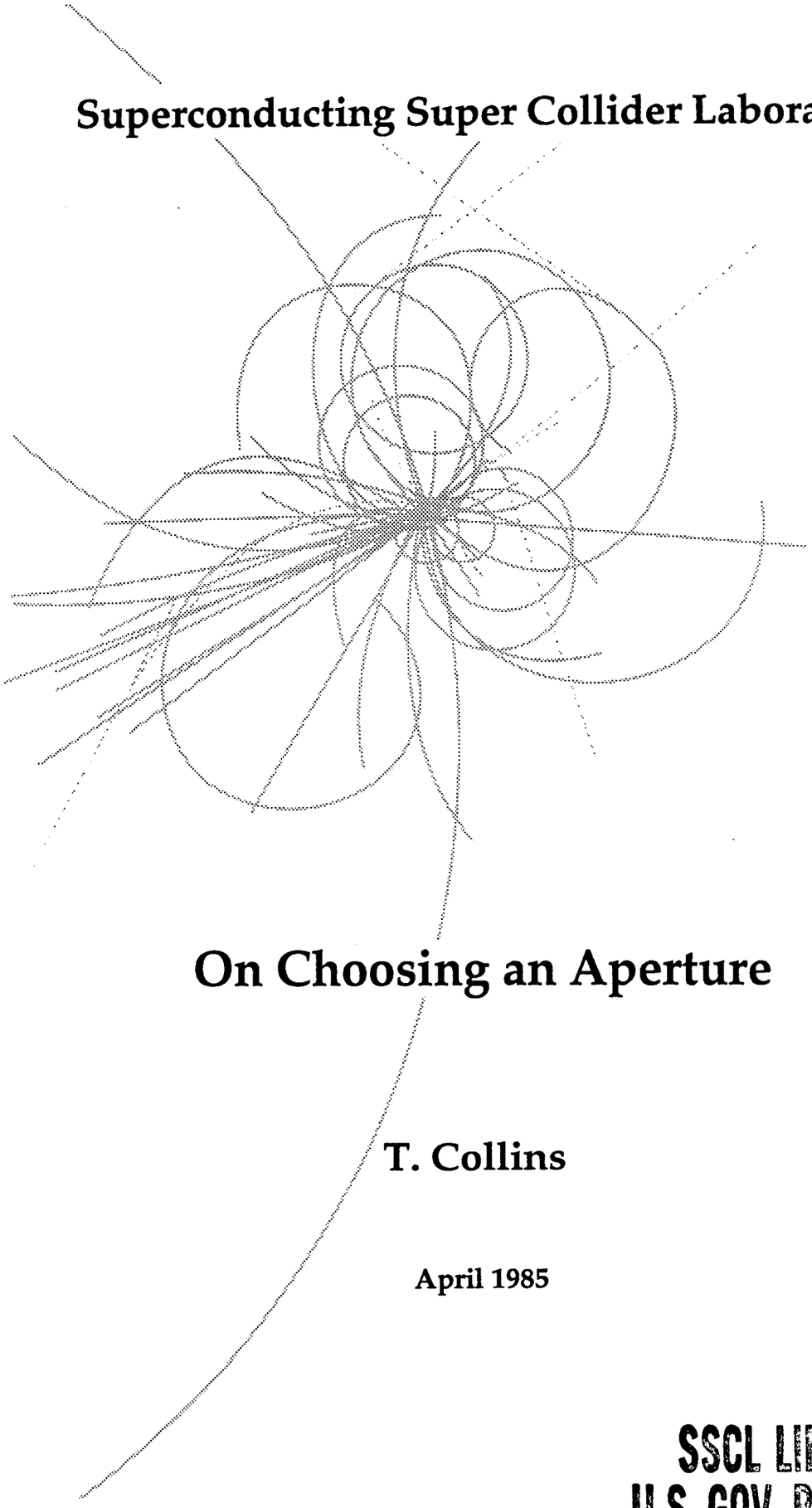

On Choosing an Aperture

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ON CHOOSING AN APERTURE

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The most difficult part of choosing an aperture is choosing the right method of choosing, after that the choice simply requires work. We must find just which important property of a good ring, *of the type we are designing*, makes the severest demand on field quality and hence specifies the largest aperture. For electron rings, beam loss at many sigma makes the greatest demand because of the total dominance of synchrotron radiation. Proton rings and particularly superconducting proton rings are not similar to electron rings. The Fermilab experience with the Doubler and with the Main Ring shows very clearly that "workability" is the dominant virtue. An aperture decision based solely on beam loss from a modest beam size will not produce a good superconducting proton ring; a decision based on workability will.

All rings work but some work very well. The prime example is the ISR. Other rings like the Main Ring work impressively on occasion, but in the case of the Main Ring (which is the only ring I am free to criticize) there have been far too many weeks when the quality of operation has been erratic. In fact living with the Main Ring is like living with a neurotic - you never know if next week will be good or bad, and if bad you can only guess at the cause. One can never understand because it is all fundamentally illogical.

Workability is the property that the Main Ring lacks. The Doubler on the other hand has excellent workability, in spite of the more complex operation necessary for superconducting rings - for example correction elements must ramp to full field and generally will require different relative settings for injection, acceleration, and flat-top. Our problem is to express this quality concept in a numeric form.

Workability will never be a popular citerion in "competitive proposalmanship", an indoor sport sponsored by DOE. It can never be a "hard" criterion because it does involve crystal-balling and judgment. It will have loop-holes through which a competitive designer could drive a truckload of magnets. Nevertheless, from a users viewpoint, it is an essential concept in engineering a good proton ring.

Rings that could work are not always workable, and competence of the operating staff is not the cause, as clearly shown by the Main Ring and the Doubler. The difference is designed into the magnets.

What we can learn from the Doubler design

In 1974 Don Edwards and I undertook to design a Doubler. We felt that the trend in the magnet design, impelled by cost saving, was moving towards an unworkable ring, a new danger created by the new technology.

We developed the "squeezed error" theory which leads to $a_k, b_k \sim 1/r^{k+1/2}$, and, after much cogitaion, concluded that:

a) There are "white" rings which are little affected by non-linearity, and "black" rings that are unworkable; the magnet quality range from white to black is small - a factor of four - and is strongly affected by coil radius.

b) A Doubler ring using a 3" magnet with a full set of ramped, programmable corrections would be medium-gray.

Our recommendations, based on our best judgment, were completly accepted. The Doubler magnet is not the smallest or cheapest that might have been built, furthermore it was constructed with great care to avoid any significant quality degradation from defects in assembly.

The Magnet Test Facilty became very skilled at spotting unlikely repetitions of patterns in the measurements and the Factory was equally skilled at finding specific causes, many of which could never have been anticipated. Largely beacuse of this devotion to quality, the final multipole errors were less than Don and I had anticipated and the ring moved from medium gray to white.

These color predicitions were suprisingly accurate for non-linear behaviour but we did not understand, and would not understand until we had an operating Doubler, the significance of the predictably large normal and skew quadrupole errors (also present in all SSC designs).

Early in construction the coil support system had to be revised because it did not properly control rotation on repeated warm-up and cool-down. In the new make-shift system large external bolts squeezed the cryostat. Proper centering of the coil in the warm iron had to be left to the magnet measurers and they adjusted for zero quadrupoles, balancing a small off-centering against the "natural" quadrupole errors. As a result the anticipated quadrupole errors were reduced by an order of magnitude, and we now know that every bit of this reduction was needed. A true example of serendipity.

From its first operation we knew that the Doubler would be a winner. The ring is insensitive to much of the usual tuning that we have come to expect, for example it works just as well at any reasonable betatron tune. Adjustment is an algebraic process - one measures some beam property, applies design theory to calculate a set of corrections, makes changes as a set and observes the predicted result. "Fine-tuning" by a skilled operator to restore peak operation is not necessary.

In the ensuing months we all became increasingly aware that algebraic operation of a superconducting ring is an absolutely essential part of workablity. The slow cycle time which is necessary in cryogenic systems makes "tuning" in the usual operator sense totally impractical. The ever present possibility of magnet quenches makes it impossible.

I find it difficult to impart the full impact of the change to superconductivity. It is so easy to say "minutes" instead of "seconds" without any feeling for what it means, but suppose that my talk would last 30 hours instead of 30 minutes. In 30 hours either you or I would quench so we would never finish. Clearly a different method of communication should be used!

Algebraic operation requires correct numerical behaviour throughout the system. The part that is of concern in aperture selection is implied by "using design theory, we calculate a set of corrections" - the beam must behave quantatively according to theory in all aspects - and I call this property *computability*. This is the property that Don and I did not properly anticipate. It follows that if a beam is not made computable *as a matter of design*, then subsequent recovery is virtualy impossible.

Criteria for Computabilty

To create a simple, practical, numeric measure of computability I must assume that the magnets have a "normal" pattern of uncorrelated random multipoles. I start (as I usually do) with a definition of amplitudes at a reference β_0, the design maximum in normal arc cells:

$$x = a_0\ (\beta_x/\beta_0)^{1/2}\cos\varphi_x$$
$$y = b_0\ (\beta_y/\beta_0)^{1/2}\cos\varphi_y$$

where a_0 and b_0 are the amplitudes which are independent constants of the motion, *but only in linear betatron theory.* Thus

$$\left[\frac{\beta_0}{\beta_x}\right](x^2 + (\beta_x x' + \alpha_x x)^2) = \frac{\epsilon}{\beta_0} = a_0{}^2 \qquad \pi\epsilon = \text{emittance}$$

is an ellipse in x phase-space, which we usually plot as a circle using

$$u = (\beta_0/\beta_x)^{1/2}\,x, \quad u' = (\beta_x\beta_0)^{1/2}\,x' + \alpha u, \quad \text{and } a_0{}^2 = u^2 + u'^2$$

Rings are not linear and the cirles are more or less smeared in both amplitude and phase, and our *first criterion* will place a limit on the amount of smearing. I find that the amplitude smearing alone provides a good measure of computability.

We start a particle with a_0 and b_0. To avoid difficulties when one value is zero let me define a "combined amplitude" $c = (a_0{}^2 + b_0{}^2)^{1/2}$. After each turn we calculate the apparent amplitudes a and b from displacements and angles using the *design beta functions* and the expressions above. Let

$$\delta a = (a - a_0)/c \qquad \delta b = (b - b_0)/c$$

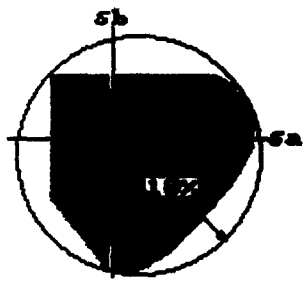

Fig.1 a-b plot

and plot $\delta a, \delta b$ as shown in figure 1. The criterion is the radius of the "circle of confusion", which I set at 10%. As shown the center of the circle is not generally at 0,0. The origin is defined by the particular choice for starting values and could be anywhere within the blob. At the 10% level the displacement of the circle is not significant. My choice for c is 7mm (at $\beta_0 \sim 340$M) and this criterion should be met by at least three sets of a_0, b_0: 7,0 5,5 and 0,7mm.

Figure 2, in case you are not familiar with it, shows the strong bias of gaussian beams towards large equal amplitudes, and explains one of the amplitude choices. It also shows that zero amplitude for either a_0 or b_0 has zero probability; however intentionally-induced large coherent oscillations are our primary diagnostic tool, and that explains the other choices.

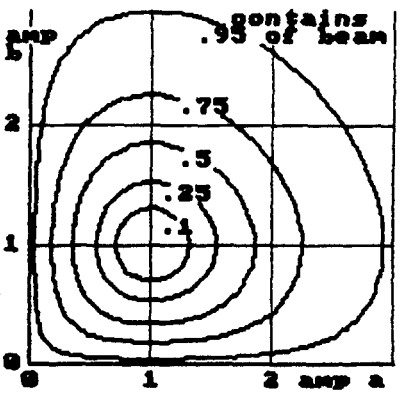

Fig.2 a,b dist'n

The *second criterion* is applied to the same amplitudes and requires that the shift in tune, ν_x or ν_y, from small amplitude values be less than .005

The *third and fourth criteria* are intended to guard against unusual error distributions and should give little difficulty if *one* and *two* are met. Beam loss should not occur at amplitudes less than 1.4 times c above, *ie* 10mm. At $\Delta P/P = +.001$ and $-.001$, c may be reduced to 70% (5mm) for all criteria above.

Finally, all criteria shall be met for at least 90% of possible arrays of the random errors. To be more precise, not more than 10% of the arrays shall fail one or more criteria.

Operational Rules

Tuning ν_x and ν_y to avoid small higher-order resonances is essential, and the a-b plot provides considerable guidance. Of course only one tuning per array is allowed, no retuning for changes in amplitude or momentum.

Skew quad tuning to remove resonant coupling at $\nu_x = \nu_y$ is also necessary. Simply adjusting the mean value of the random skew quadrupole moments so that $\sum \beta_x \beta_y q = 0$ is an acceptable alternate.

Quad error effects must be corrected. In the Doubler this was not necessary because of those crazy external bolts which were adjusted during cold field measuremnts, but this design will not be repeated. One is not permitted to cure the quad errors with a promise and a wave of the hand as we did during the Reference Design Study. If shuffling is to be the cure then a shuffling algorithm must be concientiously applied, however it is clear that we cannot wait for a good algorithm before we decide that present designs are inadequate, so I suggest the following temporary expedient. The best measurements on *cold* magnets do not give exact results, the error for a

particular magnet multipole is about 10% of σ for that multipole, thus effects from at least 10% of σ will remain after the best correction scheme. *For the time being assume 15% of* σ for quad errors is not correctable.

The above does not apply to the other source of skew quad - rotation of the regular focussing quads because the actual axes of the field differ from the external fiducial marks used in levelling. Until someone can prove otherwise I recommend a random σ of 1/2 mrad. (and even this requires *cold* measurement of all quads).

Persistent current effects should be included, particularly the systematic sextupole (with chromaticity corrected) which will be the major contributor to the amplitude dependent tune-shift.

Ignore orbit distortion. Computable beams permit easy, precision steering, provided a full set of adequate steering dipoles is installed.

Ignore all errors in the very strong triplet quads at each end of the low-beta sections (except chromatic effects). There are only three dozen of these beasts and I am persuaded that each will receive an outrageous amount of attention.

There is a problem with a-b plots, they vary cosiderably in shape as one looks at different places around a ring. Fortunately the radius of confusion does not vary nearly as much. One could ask for the largest circle in the particular array but because we are going to look at many arrays for the same error probability I think that would be much too slow. I suggest that one might look at the beam shape at each low-beta point, six places in all, and I would think that we could drop the largest and accumulate statistics on the second largest. The choice of the low-beta points has a special advantage in that the design functions with proper chromatic correction do not change significantly with momentum.

Comments on the values

There is phase smearing in addition to amplitude smearing, and the 10% limit hardly provides for precision measurements. Remember that this refers to multi-turn precision, a single turn will generally be better. I chose 10% and relatively large amplitudes because at this limit quadrupole errors produce about one-half of the effect (not amplitude dependent) and the non-linear multipoles generate the rest. Smaller values would not balance these effects.

The tune-shift limit of .005 at c=7mm. will probably be the most difficult of the criteria at injection fields, but it is about the maximum permissable for computabilty. It may actually be too large for beam-beam considerations. I considered three amplitude levels in establishing this value, and in each case found about the same limit. Remember that we expect an amplitude-squared dependence at small tune-shifts.

As explained above it is imperative in tracking to avoid small non-destructive resonances when establishing computability. It is just as imperative to do so in actual measurement. Many measurements can be made at smaller amplitudes but it is the intention that some measuremnt should be possible at combined incoherent and coherent amplitudes with c=7mm, and a tune-shift greater than .005 would make dodging minor resonances difficult.

At the "loss-limit" of 10mm the tune-shift translates to at least .01 and I have found that this usually by itself implies loss.

At small amplitudes measurement of tune will become fuzzy at the .0001 level. Precise measuremnt of tune changes is the major tool for verifying β, and will be essential in adjusting the chromatic correction of the low-beta sections. Beta is easiest measured by makig a small, known change to a quad and measuring the resulting tune change, then $\beta = 4\pi\,\Delta\nu/\Delta p$. The quadrupole change also generates a wobble in β (at frequency 2ν) with amplitude $\Delta\beta/\beta \sim 10\Delta\nu$, and unless the tune change is limited to small values (<.005) the different sampling of non-linear elements from the change in beta will generate a cumulative tune-shift which spoils the measurement.

Much, but not all, of the smearing can be rather quickly calculated using *Distortion Functions* and this has allowed me to make some perliminary investigation of the distribution of the radius of confusion for random error arrays in simple cell lattices. The distribution is just as one might expect, for example the median occurs at about 70% of the 90%-tile value. The remaining 10% forms a long tail.

With very expensive devices one really cannot say that we have a 90% chance that it will work well! Murphy and his omnipresent Law cannot be ignored and we must have several shillelaghs close at hand to threaten him [*shillelagh: an irish cudgel, from a town of that name famous for its oak and blackthorn*]. In our case a *shillelagh* is a planned proceedure, perhaps somewhat slow, by which we can recover from an unlikely bad distribution, *which we do not use in the original estimate of that distribution.*

I examined skew quadrupole distortion distributions at a tune of about .4 and .4. In each case I retuned (using tuning cells) to .45 and .55 which is the closest resonance and by and by adjusting a special set of skew quad correctors removed the resonance. This is a vector adjustment requiring sine-like and cosine-like corrections. It is much easier to do this on a computer than on a ring because I could simply zero the driving term. Then I returned to the original tune and looked for improvement. For mid-sized distortion the effect was generally small, in some cases the distortion was actually increased, but *the high-valued tail was completely eliminated.* (1000 arrays, 100 high-valued). This is an example of a big shillelagh.

We will need at least four good proceedures, for skew and normal quad and sextupole. If they all work as well as my simplified calculation we can safely design to a lower level, say 80%. This won't have much effect on the choice of the dipole design but it cuts the number of computations in half.

Some final comments

As you will quickly find, present SSC magnet designs will have difficulty with these criteria. I expect complaints that c=7mm is much larger than needed because "the beam is so small". Actually, at injection the Doubler beam size is about twice the SSC size, and of course both are small.

The citeria above were designed and then checked against the Doubler at c=14mm. One finds about 9% for the radius of confusion. The Doubler needs this amplitude for extraction which must be adjusted algebraically. Extraction not beam size is the special design problem that sets the Doubler aperture.

Scaling of lattice designs with energy has the property that we can directly compare amplitudes at β_0. To obtain the same c in the SSC would require magnets with the same absolute quality, that is measured in the same units (cm) for magnets *of the same length.*

Setting c=7mm allows the use of poorer quality magnets in the SSC. For many types of measurement and adjustment we are really working in two dimensions and the area of this 1/2" diameter circle of reasonable computability is one quarter of that in the Doubler. This choice is justified by the absence of the extreme requirements of extraction, but within the smaller area the SSC requires a higher quality of beam.

This area does permit measurement with coherent amplitudes considerably larger than the incoherent size. (We used to call these pencil beams but now I suppose they are needle beams.) Measurements using large amplitude, multi-turn needle beams can be sophisticated - one can produce most of the diagrams that we usually use to display tracking results - and will make possible algebraic adjustment of the complex special sections of the SSC.

It is importatnt to speculate on the most demanding requirement on computability. In my present opinion, it is the prompt analysis of defects. By defects I simply mean any change in the system which should not have occured. Anything from shorts in coils to computer bugs. One advantage of a computable beam is that occurrence of a defect is usually obvious.

The rate of generation of defects does not increase in direct proportion to the ring size - that would be 32 times the Doubler rate - but it certainly does increase. The large ring size does mean that we must develop techniques for pin-pointing the location of the defect from the control room, and because single defects do not create local beam effects, this will require the fullest exploitation of needle-beam measurements.

I emphasize prompt effective action because if several defects accumulate analysis becomes much more difficult. There is the danger that our rings will turn into a Red Queen and the operations staff like, Alice in the company of her Red Queen, will be running as hard as they can just to stay in the same place. The Main Ring is a Red Queen, we don't need another.

Conclusion

Computability criteria are an essential part of the design of a good superconducting proton ring. I recommend using c = 7mm. at this stage of the SSC design. I cannot "prove" this choice, it is simply my best judgment.