MI-0017

MAGNETS

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The ratio of the coil area to the gap (squared) is the best measure of the design problem:

$$A = f g^2 \quad \text{where } g \text{ is the gap.}$$

In the window frame example (and *B2*) $f = 7$ but in the next diagram $f = 5$. This shape looks very familiar and indeed with some attention to detail one can get a high quality field with $f = 5$ *or less*. What one cannot have is a high field in a small gap.

Consider two magnets of this $f = 5$ shape:

4" gap, 80 sq.in. coil window, 46"x30" outside;
2" gap, 20 sq.in. coil window, 23"x15" outside.

At the same field and at the same *relative* position in the gap these magnets will have the same field error (in gauss), and this is true at all fields including remanent and saturation. The current density, measured over the whole cross-section including water holes and insulation, is 1900 amps/sq.in. for the *4"* magnet at 18kG. Less than 2000 allows continuous operation without overheating. For the *2"* magnet the value is 3800 amps/sq.in., which is much too high for even ramped operation!

The same field error at the same relative position does not mean the same field quality. Sextupole, for example, is normally measured in absolute units of $/cm^2$ or $/in^2$, and not in relative units $/g^2$. The sextupole in the *2"* case will be four times greater than the *4"* case, decapole will be 16x. This is not compensated by using a beam amplitude proportional to the gap. If the sextupole dominates beam behavior then we would need 1/4 amplitude instead of 1/2. There is no simple relation but small magnets must use a substantially smaller *fraction* of the gap.

To prevent overheating the *B2* has $f = 7$ and the *B1* has $f = 9\,1/3$ for a current density of 2700 amps/sq.in. at 18 kG. This allows ramped operation at 400 GeV and continuous operation at about 300 GeV (13 kG). At $f = 7$ it is no longer easy to build a high quality magnet, but the designers did a fairly good job with the *B2.* Putting turns in the gap avoids some of the saturation problems. At $f = 9$ it is very difficult to build a good field, as was amply demonstrated.

As you can see from the last equation below the diagrams, the main ring power does not really depend on the magnet gap so the complaint that Bob should have designed for much lower power is not justified. The mistake was in trying to *save* a little power and using the *B1's*!

Let me define one unit of peak power as that used at 150 GeV by a main ring with only *B2's*. The use of *B1's* reduces the power to .88 units and 6.9 units at 400 GeV.

The present design of the main injector proposes 17kG magnets because of the space needed for its complex of injections and extractions. This is *2½ x* the 6.75 kG field in the main ring at 150 GeV. By applying the last equation one can see that a power level of 1 unit would require $f = 17.5$ which is impractical. A level of 2 requires $f = 8.8$ which is almost as bad as the *B1*. A power level of 2.5 is needed to get down to $f = 7$ like the *B2*, which was not easy to build nor of the highest quality. It does not automatically follow that *new* magnets have better field quality than *old* magnets and use low power.

ALTERNATIVES to the MAIN INJECTOR

It is possible that construction funds will not be available for the main injector. Here are a few alternatives which use the present tunnel. In addition to the poor *B1* quality there are several conditions that one would like to improve and the improvement varies considerably in these alternatives. The magnets at present have a peak field of 6.75 kG which is too low to provide a stable remanent field. Increasing the peak field would also increase injection field and improve the quality, however it would also increase the power. If large amounts of empty space appear in lattice then we can probably devise schemes to improve the design flaws in the Main Ring. First priority is the terrible off-momentum orbit (it affects transition) followed by the mess that is thee *D0* overpass. There is a problem with main-ring operation and the *D0* detector not addressed by any of these alternatives.

Replace B1's by B2's. Build 378 clones of *B2* dipoles and install them in the *B1* positions. This can be done a dozen at a time without a long shutdown. The new dipoles must be clones because there are 18 missing *B1's* (medium and long straights) and poor *tracking* makes orbit distortion. The peak power increases from 0.88 to 1 but this should be within the capabilities of the remnants of the original power and cooling systems. There are no supplementary improvements. [If we clone *B2's* does this mean that 2/3 of the new ones will fail? I think not. The problem was *stress* from temperature cycling, not water leaks.]

Discard B1's. Use 384 B2's. One will also need 6 half length magnets for the straight sections where 3 *B1's* are replaced by *1½ B2's*. There is much empty space and the fields a doubled, which is good. The sagitta doubles, from *½"* to *1"*, which may be not so good. The power is two units, which no longer seems bad, and the ravaged power and cooling systems will need reconstruction. There are a number of minor difficulties to solve. One would like top drop the two *B2's* into the center positions in the cell to take advantage of reduced β and dispersion, and to leave space around the quads. The problem is that the dipoles do not just "drop in". The center of the two dipoles will be 2" closer to the wall than center of the four dipoles, and the other ends will be 1" closer. The angle between the dipoles is now 16 mrad. but 8 mrad. is built into the overlapping arms. The dipoles do not need to be centered, they need only be symmetric about the original center; in fact every cell could be different. This option does require a long shutdown.

A New Dipole plus Two B2's / Cell. Special magnets are needed at both medium and long straights. I believe this means the new dipole must track the *B2* but it is not a clone. The beam is improved less than the last alternate. The field increases, but not so much. There is empty space, but not so much. The sagitta increases, but not so much. The power increases, but not so much. Most things a part way between alternate 1 and 2, but it does combine the major work from each - a large dipole construction program followed by a full long shutdown.

Scrap B1's, B2's. Two (or 3) New Dipoles / Cell. And of course the new ones should be high quality, low power, and cheap! Seriously, this may be the best alternate, but not low power. In the first alternate we actually build 18kG. dipoles and use them at 6.75kG., just to get tracking! This is more than enough dipole for 150 GeV. Suppose for example we use 3 dipoles/cell, 15 ft. long (16' slot), at 12kG. The outside steel frame can run at 18kG, so we can use a *2½"* gap without excessive overall size. Injection field is 708 gauss and the quality is excellent. The sagitta is 0.6". There is about 35' of empty space per cell. The power level should be about 2. This is not an optimized design, just a good guess at what could be done.

Install corrections in the present ring. This is actually a new topic.

MULTIPOLES, ERRORS, and CORRECTIONS

It is important but not always easy to *think two dimensions* when considering fields or beams. A majority of the protons in a beam have substantial amplitudes in both horizontal and vertical directions, very few are dominated by one direction. The specification of two-dimensional fields leads inevitably to an expansion in terms of multipoles.

When we were designing the Doubler dipoles many years ago, we found that a slight increase in the coil thickness with readjustment of the angles would cause the 14-pole to change from negative to positive. This produced a small hump in B_y far out on the x axis. I claimed that a small hump was good - it extended the field - and this is the adopted design. If you imagine the scallop shell shape of a high multipole then it is hard to believe that I was taken seriously, but thicker coils increase the field strength. [I checked recently by tracking - the horizontal aperture is slightly wider, all else is slightly reduced.]

Tracking in two dimensions requires the use of field components that satisfy Maxwell's equations precisely, otherwise we can observe a slow growth (or shrinkage) of amplitudes which is not real. Except for beam-beam interaction this requires expressing the fields as a series of multipoles, each of which is a solution of Laplace's equation. In one dimension the multipoles series reduces to a simple power series, which can fit any field shape, so this restriction is not apparent even though the implied two dimensional field is nonsense.

Traditionally field quality is given by the flatness of B_y along the x axis, usually ignoring possible B_x. In principle this is sufficient information to determine off-axis fields but, as we now know, the precision is not adequate and off-axis measurement such as direct multipole measurement from rotating coils is necessary.

In order to "track" using flatness data, one must fit a power series and then use the full multipole polynomials to obtain off-axis fields. It is not uncommon to find that the lower terms, for example the sextupole (x^2), depends strongly on the range of points and the number of terms in the fit. The sextupole creates a *thirds resonance* and surely the *resonance width* does not depend on how I do the arithmetic!

This "problem" is often invoked to support the claim that a multipole expansion is not appropriate for small gap iron magnets, but that is wrong. The real problem is that having been forced to use multipoles we try to simplify things by ascribing physical meaning to each individual multipole term.

A resonance width is measured by starting below the resonance with an amplitude a and tuning towards the resonance until the tune is "pulled" and "locked", then repeating by tuning down from above. The tuning difference between the locking points is the *tuning width* for amplitude a. For a thirds resonance from a sextupole term (in one dimension) the width is proportional to

$(1/4)\, a B_3$, where B_3 depends on sextupole distribution.

There are many more contributions to the width, thus the *10-pole, 14-pole,* and *18-pole* add

$$(5/16)\, a^3 B_5, \quad (21/64)\, a^5 B_7, \quad (84/256)\, a^7 B_9,$$

and the width is given by a power series in a. These additional terms are not negligible. A full evaluation of the widths for various fits resolves the problem for significant amplitudes.

Except for the very simplest parameters - tunes, coupling and chromaticity - we *must* evaluate a series of terms when using multipoles. The question is how many, and here we may be misled by the Doubler dipoles. The *random* errors in those magnets come from errors in wire size which affect wire placement. There are many errors and the result is random multipoles without any significant correlation - if the sextupole is large the 10-pole is unaffected. This insures that any series will converge quickly because the terms add quadratically. The doubler magnets may be unique in this respect because there is no close-in iron.

The random errors in copper-and-steel magnets come from the steel. Remanent fields depend on the maximum field in the steel, and hence on the lamination packing density, as well as the chemistry, internal stress (work hardening) and applied stress. The shape of the field can be complex but the shape changes little from magnet to magnet so the multipoles are strongly correlated. Correlated skew multipoles arise from differences in upper and lower half cores. Convergence can be slow and many multipole terms are required in each calculation. Tracking must use correlated multipoles, unfortunately correlation coefficients are notoriously inaccurate. Superconducting magnets with close-in saturated steel will have the same problems.

Systematic errors are also the same from magnet to magnet and effects can be very large. A suitable symmetry can suppress much of the effect of lower multipoles but a small orbit distortion removes the suppression for higher multipole terms because of their high amplitude dependence. The effect of systematic errors relative to random increases with the accelerator size. They are about equal for the Doubler.

BEAM BEHAVIOR

The main ring beam behavior has always been difficult to measure and understand. Everything is fuzzy, no measurement is clean. During the time from injection to acceleration, which is as much as 1 second, beam is continuously lost. This loss is increased somewhat close to resonances but the resonance effect is very broad and shallow. The momentum aperture is very restricted by a strong non-linear change of betatron tunes. This behavior has resisted analysis.

A few years ago I requested that when time permitted the magnet measurement group should study main-ring magnets using modern rotating coil techniques. This proved to be a hard task. A full set of multipoles was measured at three displaced positions, which should be consistent and this placed a strain on the precision. The results for the *B1* in particular require strong systematic higher multipoles. I was sensitive to this pattern because I had found just these systematic multipoles to be of the greatest importance for large rings. A quick tracking study of a simplified main ring using a PC with color was most revealing.

Two dimensional tracking involves 4-dimensional phase space. I usually calculate amplitudes and phases after each turn from the positions and angles, and plot the amplitudes on what I call an *a,b plot.* It is most useful to carry two particles forward turn-by-turn and to plot them in contrasting colors. The main ring tracking has three regions. The regions have rough boundaries in *a,b* but I presume they are surfaces in 4-dimensional space.

At very small amplitudes, and with the skew quad term removed, each amplitude is constant and the *a,b* plot is a point. With skew quad the plot is an arc about the origin with a slow trading of *b* for *a* and back. At larger amplitudes, with or without the skew quad, the plot is a "fat" arc and the slow motion from end to end is erratic. Presumably the motion is confined to a somewhat distorted surface in 4-space. I call this *laminar* motion, in analogy to fluid flow, and the next region will be *turbulent*.

Above some bounding surface the motion is chaotic. This is not to say that there is no pattern, the coupling pattern persists, but after some time the particle is clearly tracing a different arc. If one starts two particles with very close initial values and plots in contrasting color then the divergence, which defines chaos, is quickly apparent. After a time particles move past some large amplitude surface where one can say with certainty that they are not coming back and will soon be lost. This surface is what I mean by ***magnetic aperture***.

In the main-ring I believe that all turbulent particles will sooner or later cross the magnetic aperture. Typically it takes a few thousand turns but tens of thousands are not uncommon. For the first time we have been reproducing main-ring behavior with tracking, the key being the inclusion of high order systematic multipoles. A ring with only *B2*'s has a much larger laminar region.

CORRECTING MAGNETS

The initial operation of the main ring was confounded with a terrible magnet alignment process. The orbit displacements from quad position error were large and dominated by a 20th harmonic. In addition the six-fold ring structure with straight sections generates a 60th harmonic sextupole from the systematic remanent field in the dipoles. A displacement in a sextupole generates a quadrupole, a large 20th harmonic displacement in a 60th harmonic sextupole makes a 40th harmonic quadrupole which means a huge stop-band anywhere near 20. The main ring had no stable orbit which is indeed very confusing.

Maschke found a cure from a simple tracking program and installed air-core sextupoles on wooden forms around the vacuum chamber in every mini-straight. The effect was miraculous. Naturally the idea that 10-pole correctors would do wonderful things was soon heard, but they were not really practical. I am sure that this past experience will help generate the idea that we ***must*** try a set of 10-poles before considering *difficult and costly* modifications.

By moving quadrupoles we managed to mitigate the distortion from the egregious misalignment at the straight sections. I then demonstrated that turning off and on the sextupole correctors had only a small effect on the beam (low current - no head-tail effect yet) as support for my explanation of the problem. The source of our initial difficulty was not the sextupole but the misalignment. There was then little support for pursuing a 10-pole miracle, and there is less now. Then and now 10-pole magnets are very weak and therefore large and there are many mini-straights where they cannot be squeezed in. I suspect that an uneven distribution will make the beam behavior worse.

Of more consequence, at least in theory is the suggestion to build *anti-Bl* magnets, devices that add field at the edges to patch up the sagging field and thus remove all multipoles. It remains to prove that a suitable pole shape exists. Again we have the space problem in the mini-straights and in this case I am certain that a full set of corrections is required. The worry is that the device is not adjustable and is not tested until a full set is installed (some years from now).

I don't believe patching has ever been profitable.

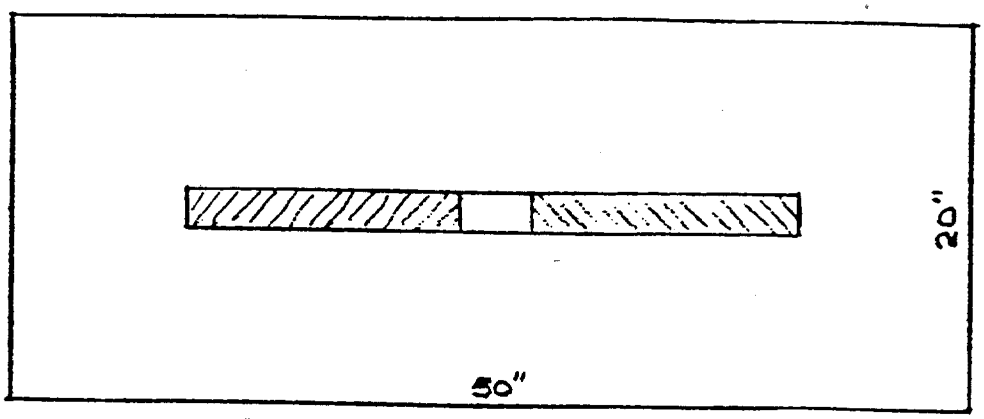
SUPERCONDUCTING DIPOLES

I have a few remarks on superconducting dipoles. They are not new, I have shouted them for some years.

Scaling follows the same rules as above with one interesting addition: small dipoles have lower fields. Consider a half sized dipole. To conserve quality it is simple scaled down exactly from its well designed big brother. For the same field the current will be one half, the coil area is one quarter so the current density is doubled (sound familiar?). The product Bxj is approximately constant so B (and j) is reduced to .707 of big brother's field. The field is proportional to $r^{1/2}$. This rule was verified by the test magnets which were enlarged versions of the doubler dipole produced by Japan, Germany and Russia, and also by the final Isabelle dipole. Somehow our 4 TeV (?) proposal just reverses this rule without building anything.

Magnet designers have become the worst offenders at treating multipoles one at a time. All systematic multipoles and particularly the series of higher ones are important. The only perfect field is the overlapping circles design. The use of wedges on the inner coils (where they are effective) to adjust low multipoles clearly enlarges the important higher multipoles and is a bad design. The 3-shell offset dipole with simple keystoned coils is very much better in overall quality. Note that its ribbon cable is narrower and easier to wind.

By now you know that I don't like cold, close-in saturated iron because of the poor field quality. The idea of using a superconducting shield (Nb-Sn films) is appealing.



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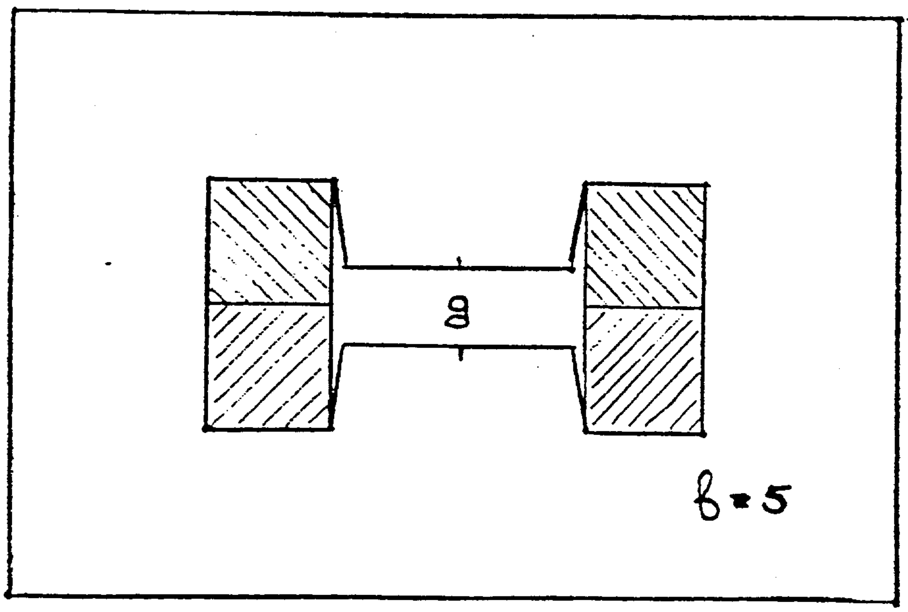

coil window area = $\beta \cdot g^2$

current density = $2.12\ B/\beta g$ (1.05 amp fac)

power / magnet $\propto B^2 \ell / \beta$

power / ring $\propto EB/\beta$