High Field Magnets for pp Colliders

S. Gourlay
Head, Superconducting Magnet Program

In Europe (European Strategy Group) . . .

d) To stay at the forefront of particle physics, Europe needs to be in a position to propose an ambitious post-LHC accelerator project at CERN by the time of the next Strategy update, when physics results from the LHC running at 14 TeV will be available. *CERN should undertake design studies for accelerator projects in a global context, with emphasis on proton-proton and electron-positron high-energy frontier machines. These design studies should be coupled to a vigorous accelerator R&D programme, including high-field magnets and high-gradient accelerating structures, in collaboration with national institutes, laboratories and universities worldwide.*

“. . . . deliver a conceptual design report (CDR) together with a cost review by 2018, in time for the next update of the European Strategy for Particle Physics.”

The super-exploitation of the CERN complex: Injectors, LEP/LHC tunnel, infrastructures

1980 1985 1990 1995 2000 2005 2010 2015 2020 2025 2030 2035

LEP: Construct. | Physics | Upgr

LHC: Design, R&D | Proto | Construct. | Physics

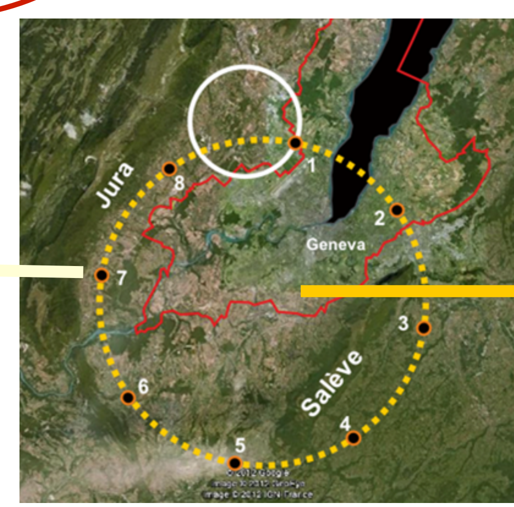
HL-LHC: Design, R&D | Construct. | Physics

HE-LHC: Design, R&D | Proto | Construct. | Physics

Roy Aleksan
CERN

Feb. 22, 2013

Either using existing LEP/LHC tunnel to reach 26-32 TeV collisions



Or build (or reuse) an 80km tunnel to reach 80-100 TeV collisions

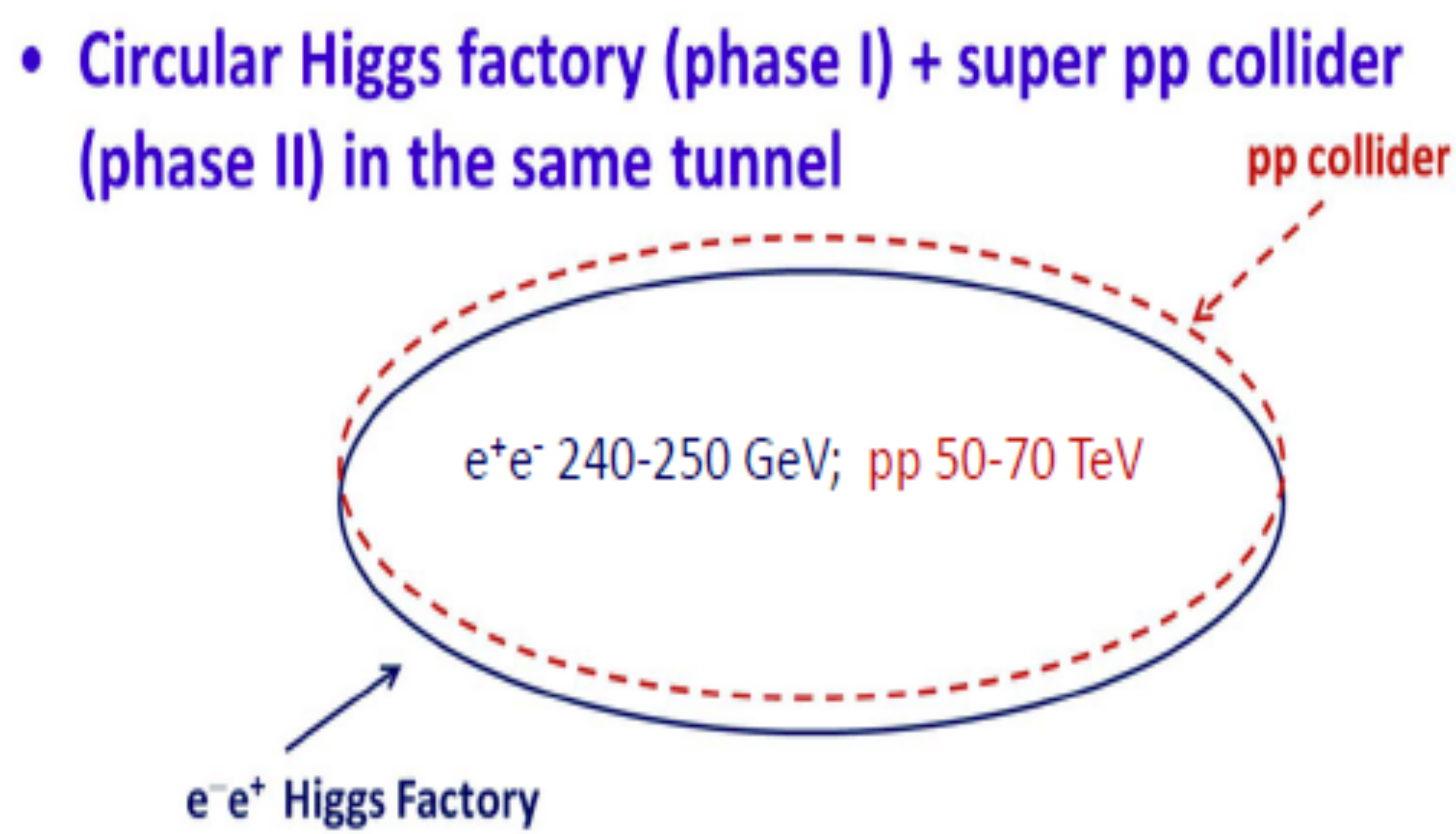
⇨ more detailed study of such a tunnel needed

In both cases, SC challenge to develop 16-20 Tesla magnets!
Magnets for HL_LHC is an indispensible first step

. . . . in China (SppC) . .

For about 8 years, we have been talking about "What can be done after BEPCII in China"

Thanks to the discovery of the low mass Higgs boson, and stimulated by ideas of Circular Higgs Factories in the world, CEPC+SppC configuration was proposed in Sep. 2012



Yifang Wang
Feb. 13, 2014, Geneve

A 50-70 km tunnel is very affordable in China NOW

. . . . and in the US (P5)

pp colliders are one of P5's long term priorities

"The future of particle physics depends critically on transformational accelerator R&D to enable new capabilities and to advance existing technologies at lower cost. "

"The program is driven by the physics goals, but future physics opportunities will be determined by what is made possible."

"Going much further, however, requires changing the capability-cost curve of accelerators , which can only happen with an aggressive, sustained, and imaginative R&D program."

"Primary goal, ... build the future-generation accelerators at dramatically lower cost. For, example, the primary enabling technology for pp colliders is high-field accelerator magnets, . . ."

"Strengthen national laboratory-university R&D partnerships, leveraging their diverse expertise and facilities."

A very significant challenge has been made

Cost will be a dominant issue along with performance, as usual, but these are tied more closely than you might initially think

Models point to an optimal field between 8 and 12 T.

Using currently available technology this is probably correct, but there are other constraints.

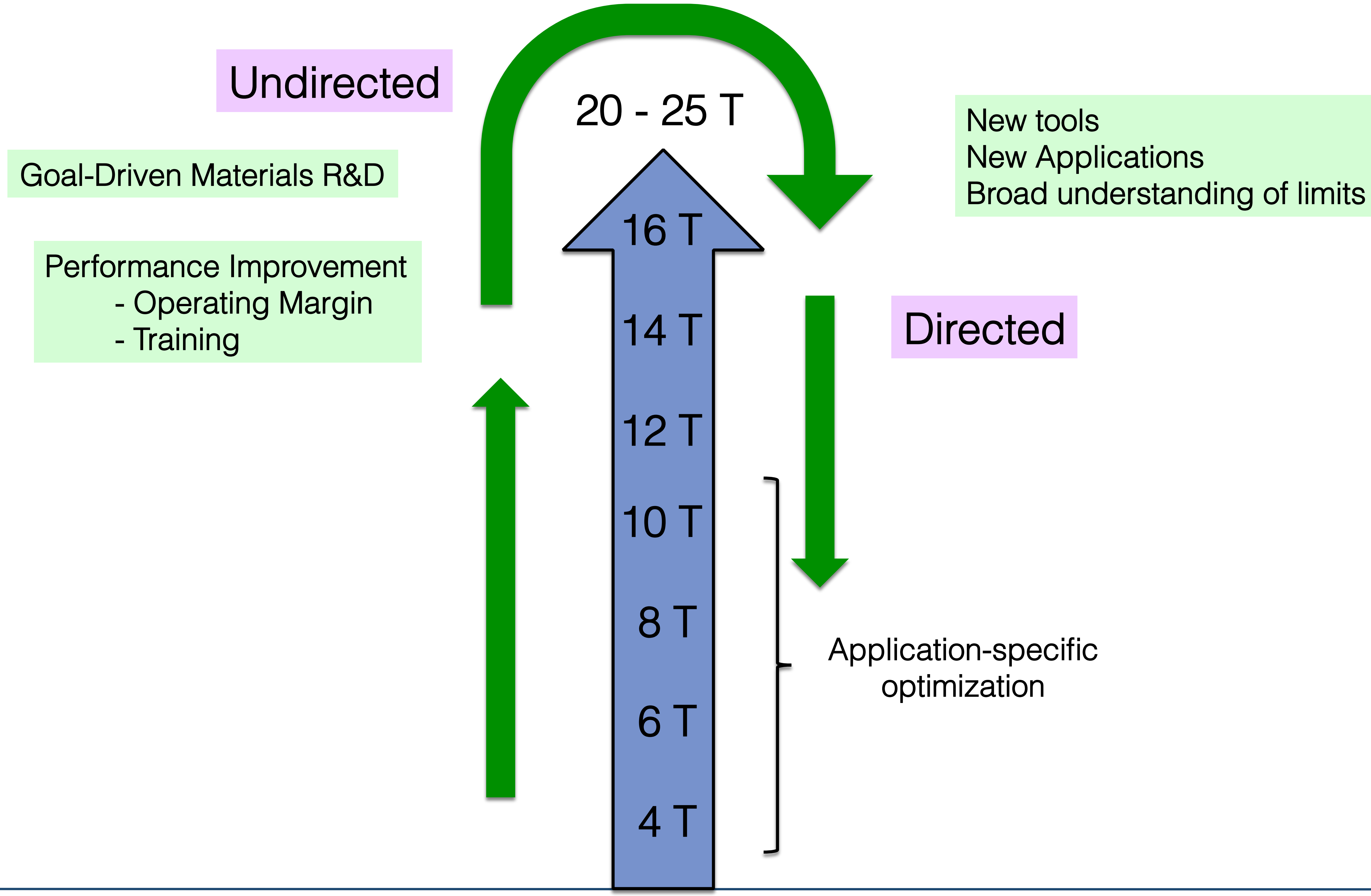
In addition, in order to make a future pp collider affordable, we still need to significantly reduce cost and improve performance.

In the end, it is the cost of the machine, not just the magnets. In order to be able to reduce the overall cost we need flexibility in magnet design choices – bore size, field, geometry.

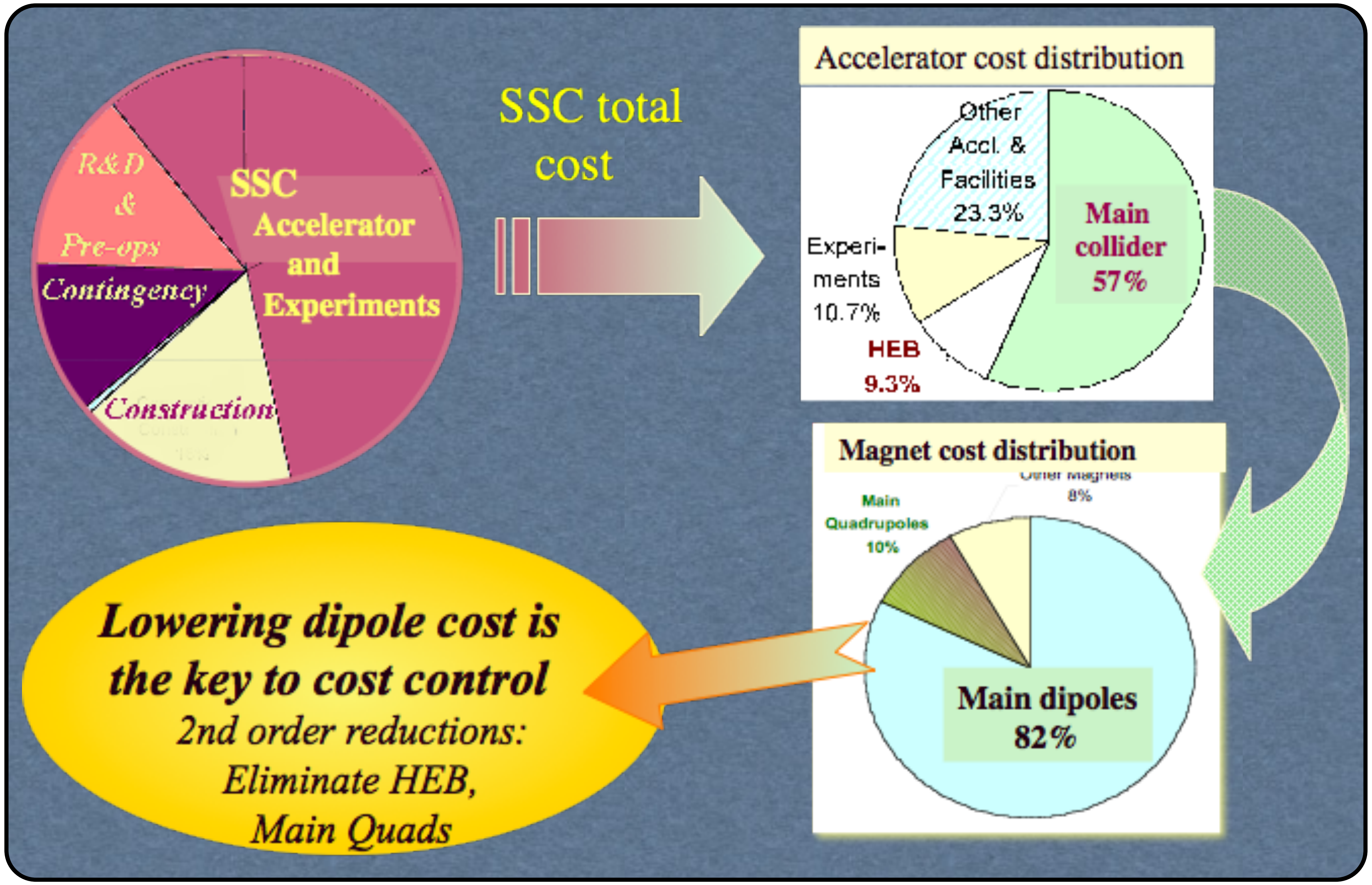
My assertion

Pushing the limits of a technology is the best way to maximize the available parameter space and develop fundamental understanding

Exploring Extremes Generates New Technology



Magnets drive accelerator cost



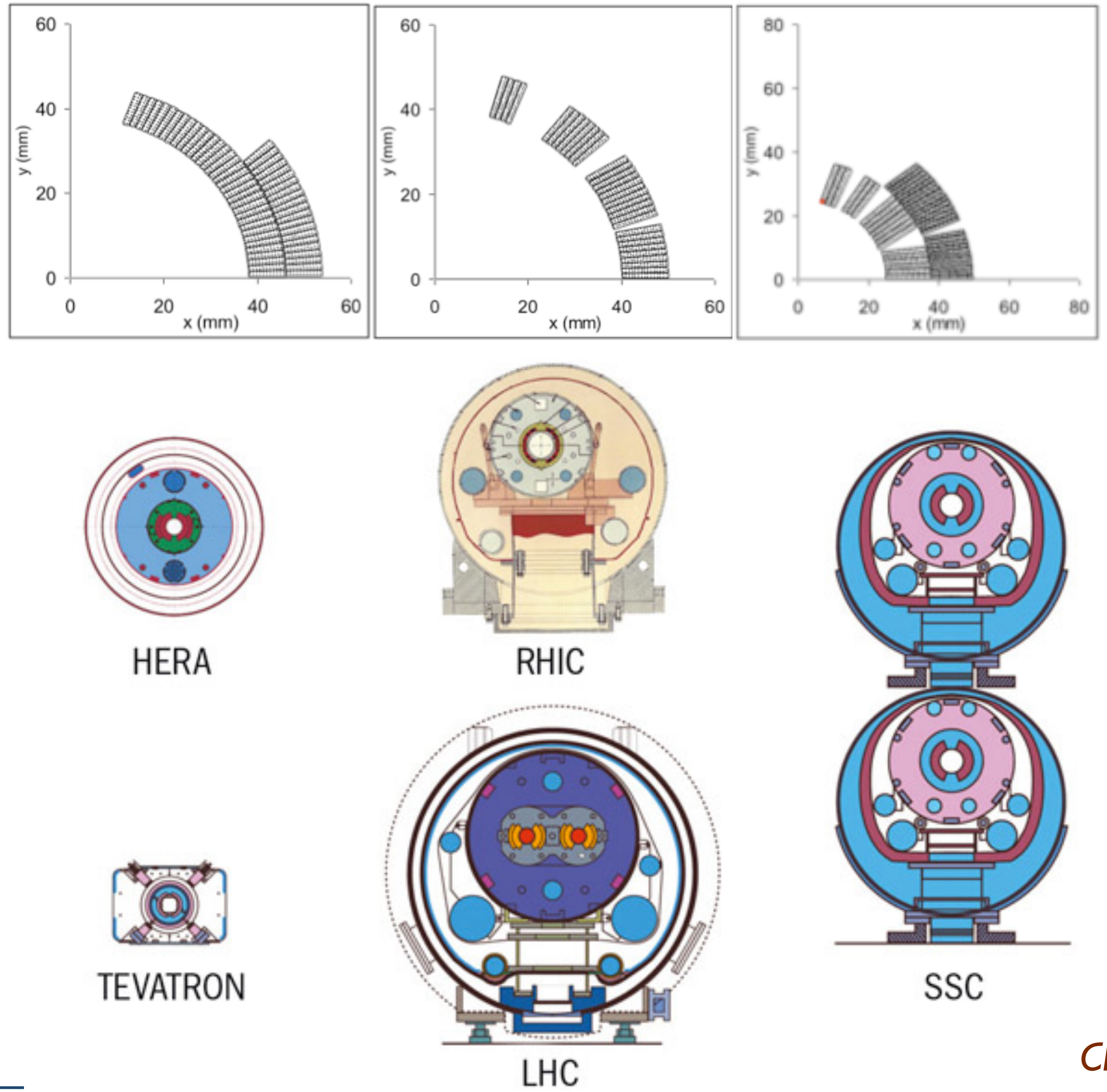
Barletta

CERN cost estimates*:
$\$_{magnets}/\$_{tot}$

LHC: 57%
HE-LHC:
- 70% (26TeV; Nb_3Sn)
- 77% (33TeV; HTS)

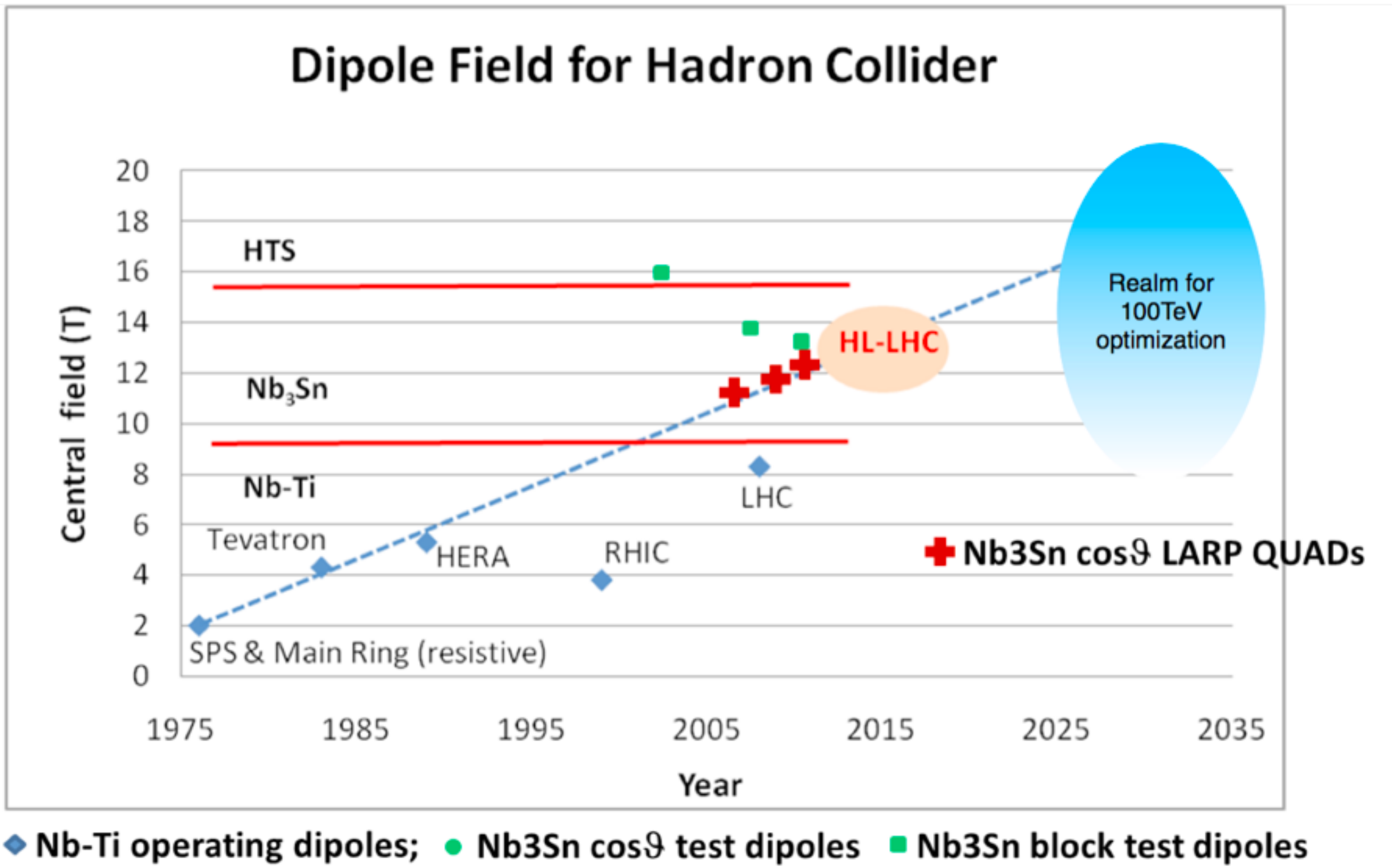
*L. Rossi, "TOE" talk

~ 3 Decades of magnet technology evolution



CERN Courier, Oct. 2011

Some progress towards higher field accelerator magnets



S. Prestemon, LBNL

Still Room for Improvement: The State of State-of-the-Art

About 27 km of NbTi magnets running at 1.9K and (hopefully) 7 - 8T

More than half a century after discovery, Nb_3Sn is ready for major implementation in an operating accelerator. HL-LHC

Some significant improvements in HTS conductors, but much left to do.

High field accelerator magnet development has reached 14 – 15T. Getting close to the Nb_3Sn limit.

Training is still a problem

A relevant historical note:

The program that developed the technology for a critical upgrade of the LHC was started at LBNL more than 20 years before the LHC turned on and while the SSC was still the flagship project of US HEP

Conductors for Accelerator Magnets

Conductor ultimately determines magnet performance

- You can't do any better than the virgin conductor
- But . . . you can do worse!

With few exceptions all accelerator magnets use Rutherford-style cables

- Multi-strand – reduce strand length, fewer turns (lower inductance)
- High current density
- Precise dimensions – controlled conductor placement (field quality)
- Current redistribution – stability
- Twisting to reduce interstrand coupling currents (field quality)

Let's start with the materials . . .

Materials for Accelerator Magnets

Application/performance ⟷ material properties and engineering

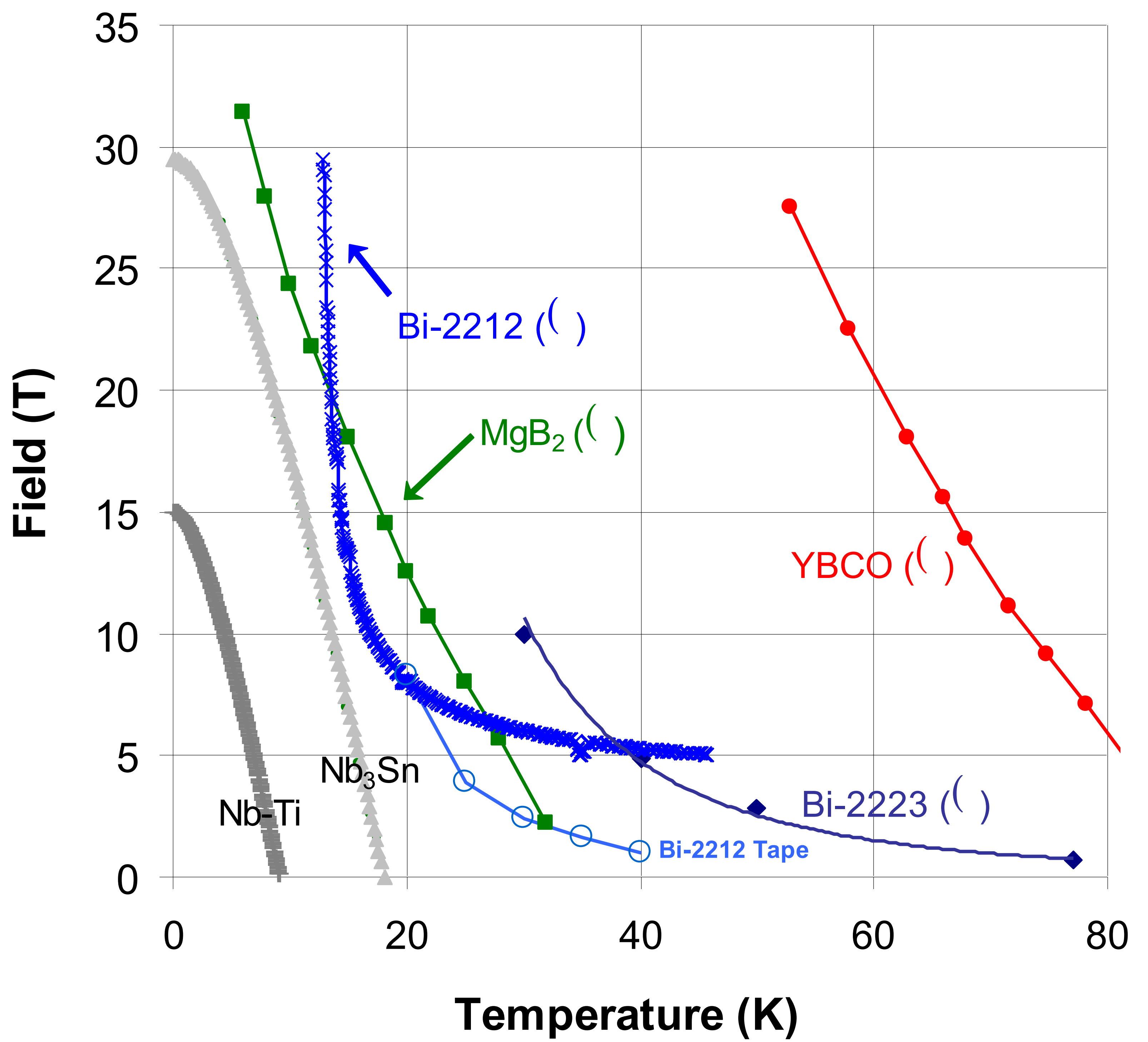
- NbTi
  - — B_{c2} (0K) ~ 14 T
  - — T_c (0K) ~ 9.5 K
    - Max practical field at 4.2 K is 7 T (9 T @ 1.8 K)
    - Excellent mechanical properties
- Nb_3Sn
  - — B_{c2} (4.2 K) ~ 23 – 24 T
  - — T_c (0T) ~ 18 K
    - Max practical field 17 – 18 T?
    - Brittle and strain sensitive
- Nb_3Al
  - — High J_c in magnetic field < 15 T
  - — Mechanical toughness
  - — Actively pursued in Japan
    - — National Institute for Materials Science (NIMS)
    - Rapid-quench process requires later addition of stabilizer

Materials for Accelerator Magnets

Application/performance ⟷ material properties and engineering

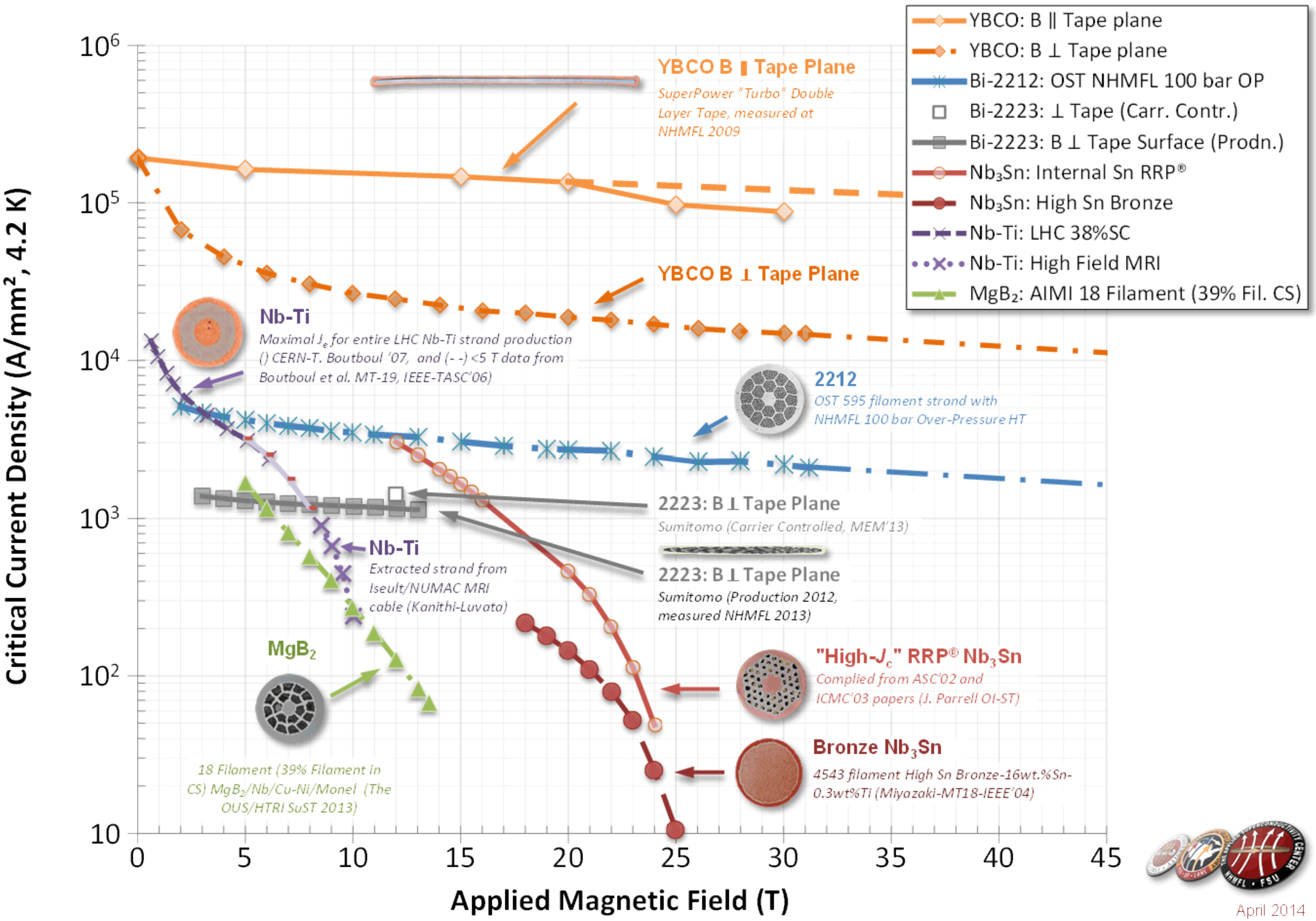
- Bi-2212
  - Round strands in long lengths
  - Requires 900 ^{0}C heat treatment
  - Oxygen Atmosphere at 100 bar
  - Strain sensitive
- Bi-2223
  - Tapes in long lengths
  - Applications for high temperature
- YBCO
  - High critical current but length is a problem
  - Tapes (not wires!)
  - Lousy engineering current density
  - Really Expensive
- MgB_2 (not so HT HTS)
  - Better at T < 25K
  - Anisotropic
  - Low J_c (so far)
  - Stabilization

Field vs Temperature



Courtesy D. Larbalestier, Applied Superconductivity Center at the National High Magnetic Field Laboratory, FSU

Critical Current Density (J_c) vs Field

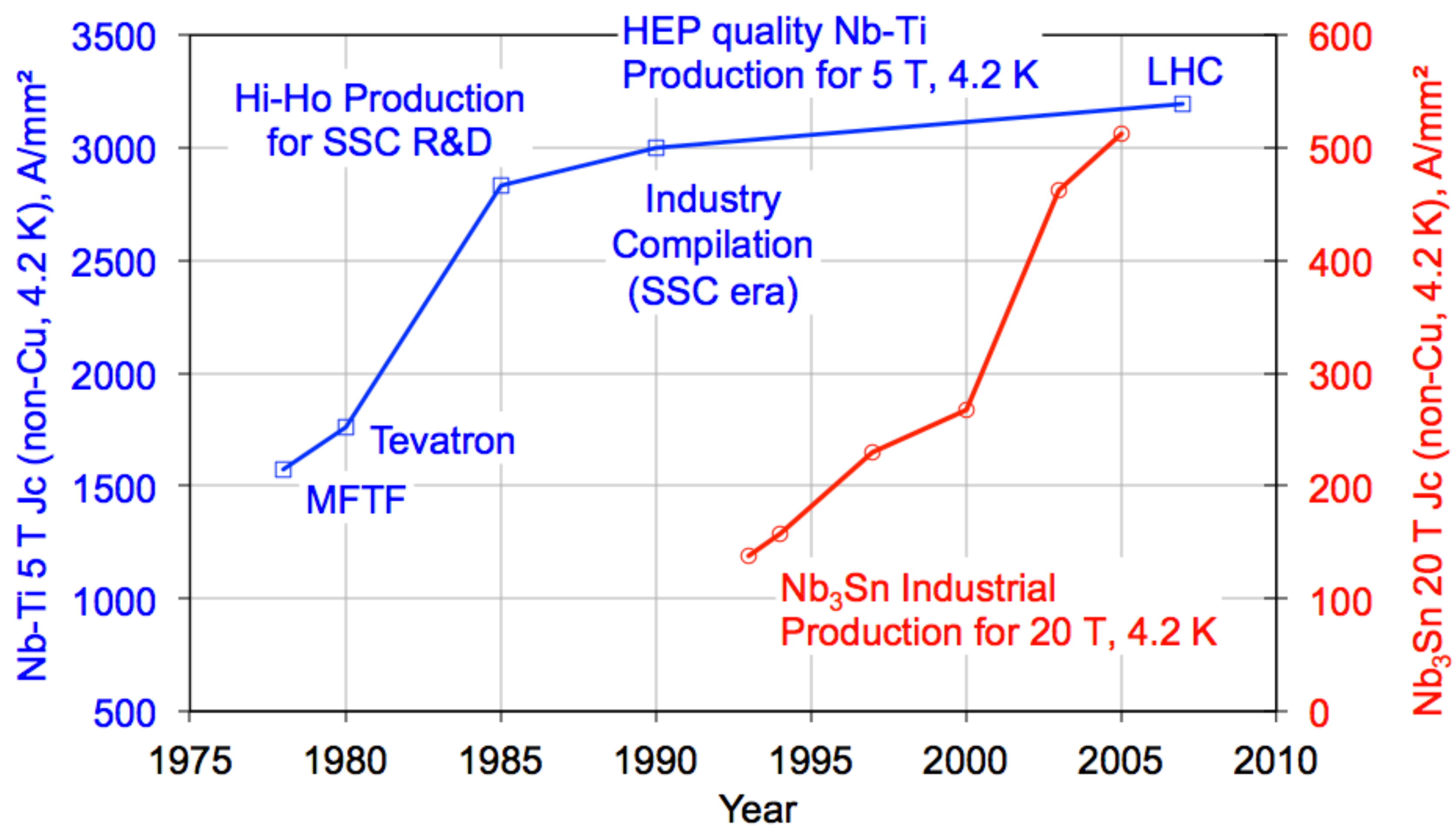


Courtesy of P.J. Lee, Applied Superconductivity Center at the National High Magnetic Field Laboratory, FSU

Where are we with Nb_3Sn?

Nb_3Sn performance has greatly improved (doubled in ten years)

Can we expect more?



An historical view on the improvment of Nb-Ti and Nb_3Sn performance [L. Bottura, ASC 2012]

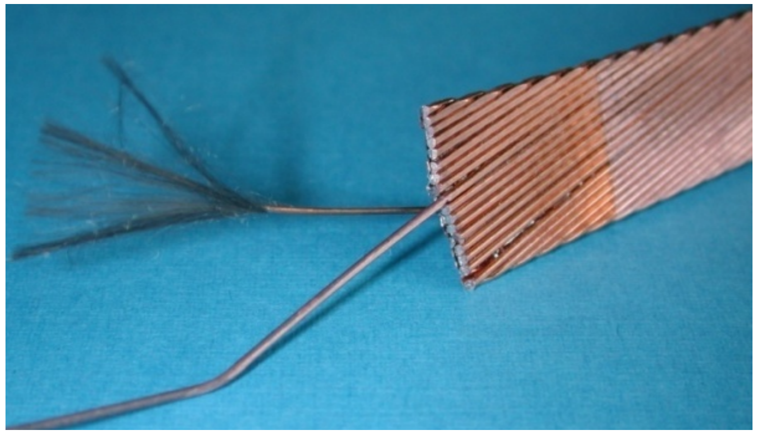
Rutherford Cables

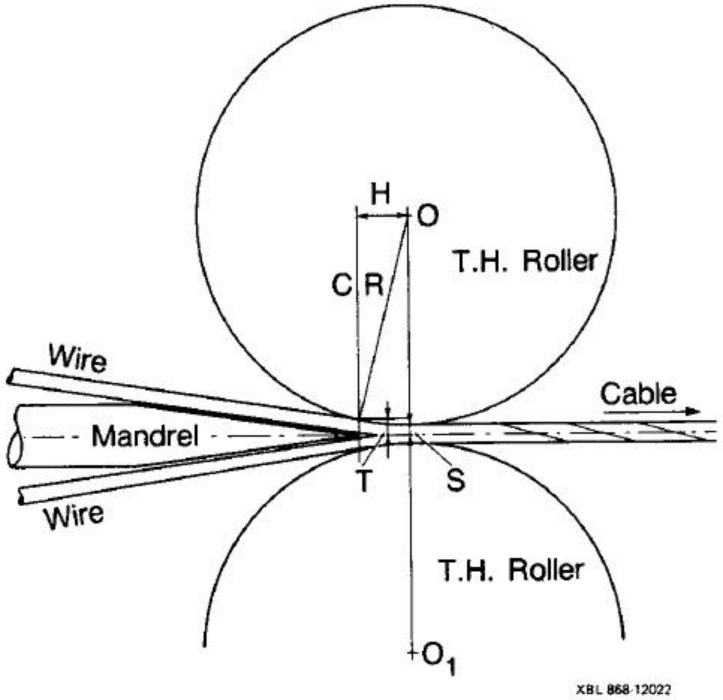
Cable cross-section is rectangular or trapezoidal

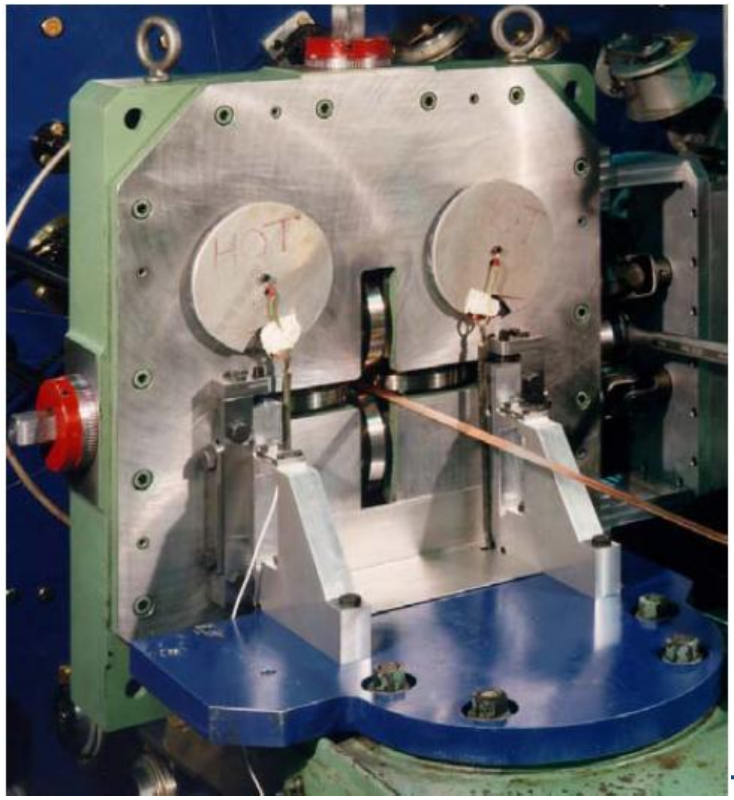
Packing Fraction (PF) ranges from 85% - 92%

- Too much compaction – damage to filaments
- Too little compaction – mechanically unstable

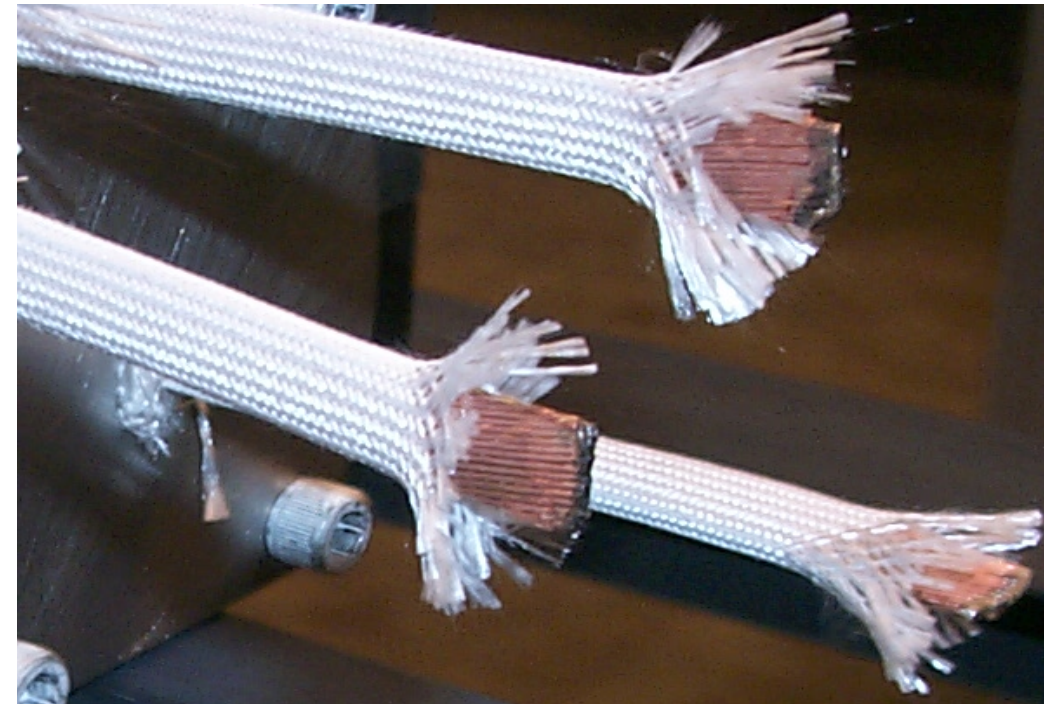
$$PF_{cable} = \frac{N_{wire}\pi d_{wire}^{2}}{4 w_{cable} t_{cable} \cos\psi_{cable}}$$







Engineering Current Density, J_e



Start with J_c of Superconductor

- NbTi ~ 3,000 A/mm^2 @ 5T and 4.2K
- Nb_3Sn ~ 3,000 A/mm^2 @ 12T and 4.2K

Add copper/non-Superconductor

- Typically ~50%

Cable compaction ~88%

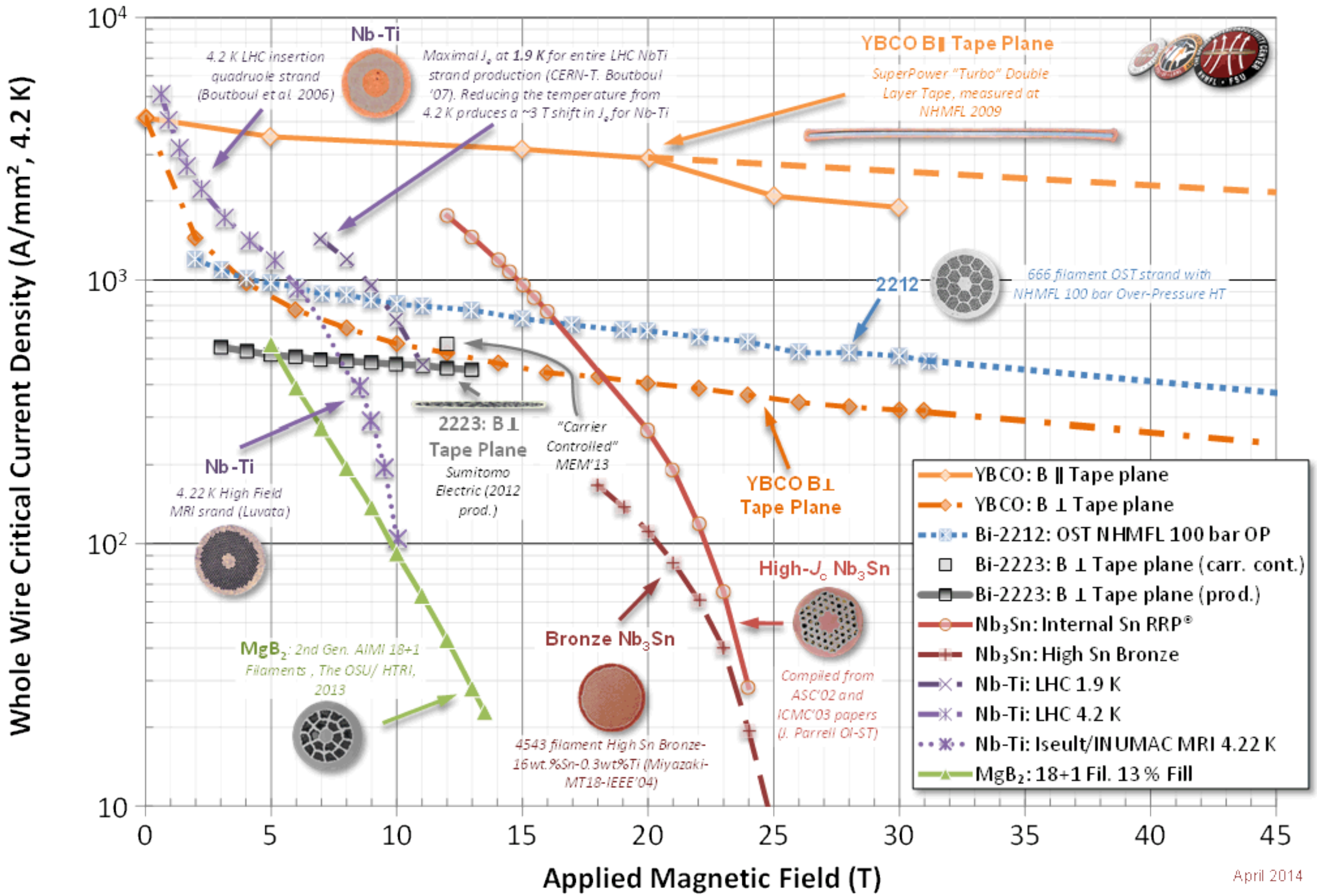
Insulation – order of 100 microns (X2) compared to ~2 mm cable thickness

Filling factor (κ) = $(N_{wire} A_{sc})/A_{ins\_cable}$

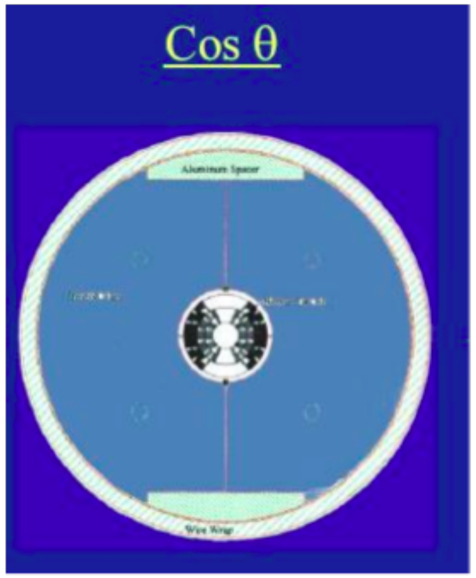
Engineering current density defined as $J_e = \kappa J_c$

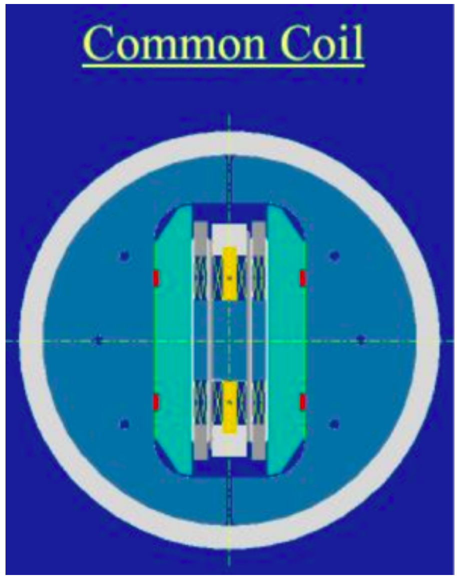
- Typically on the order of 1,000 A/mm^2

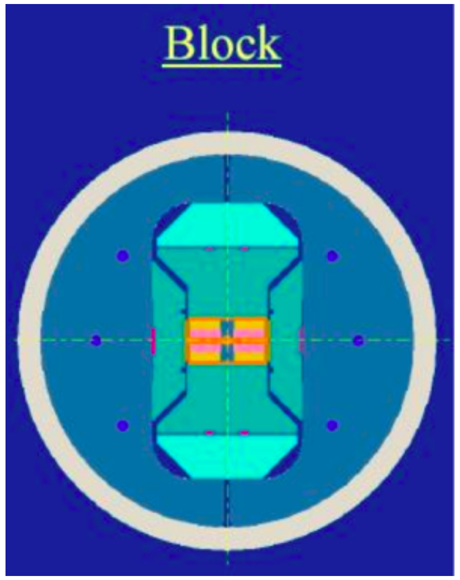
J_e Chart – Peter Lee, Applied Superconductivity Center FSU and NHMFL



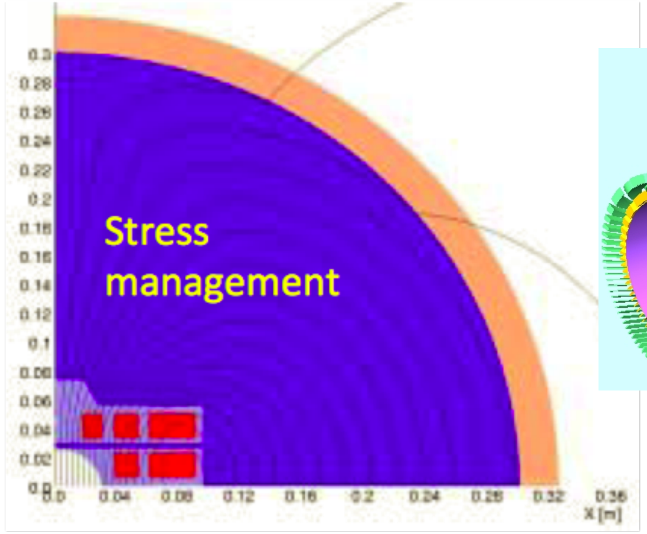
Starting point for magnet technology



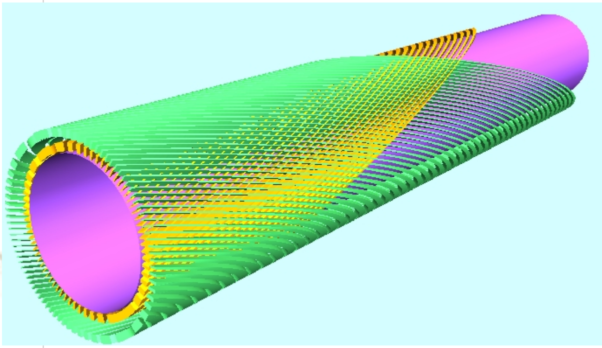




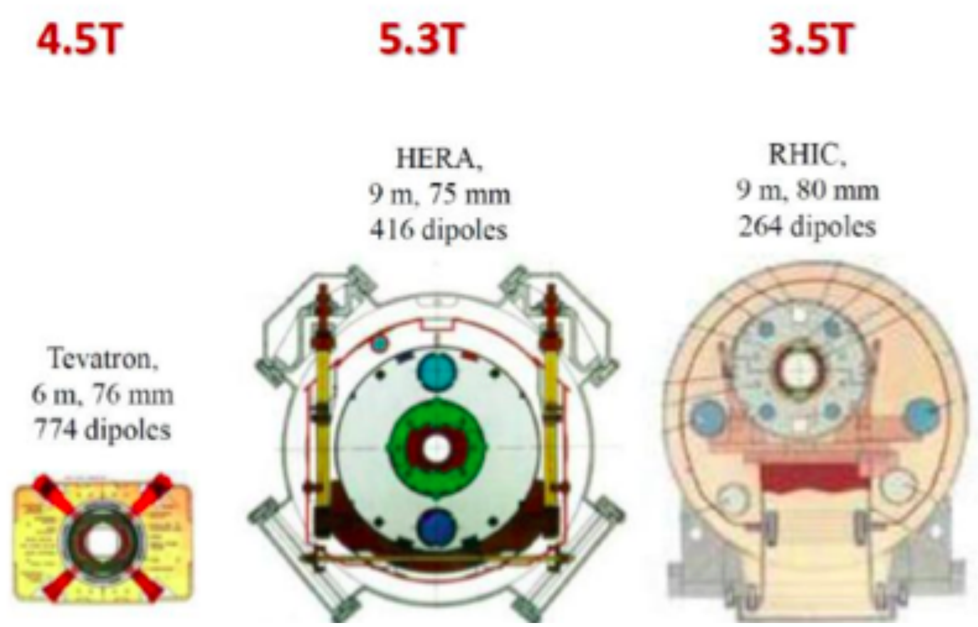
LBNL



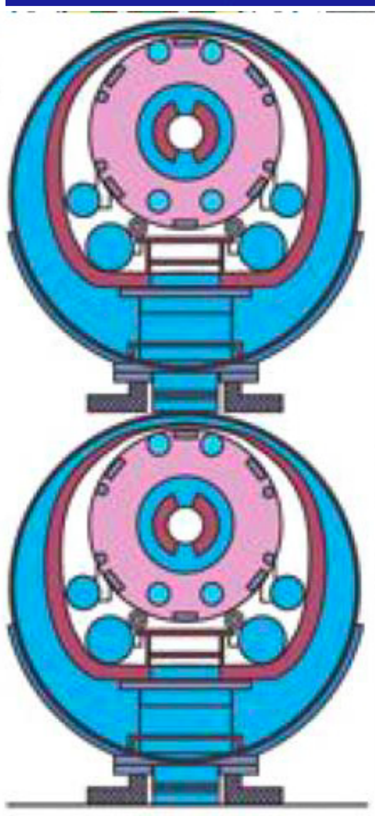
TAMU



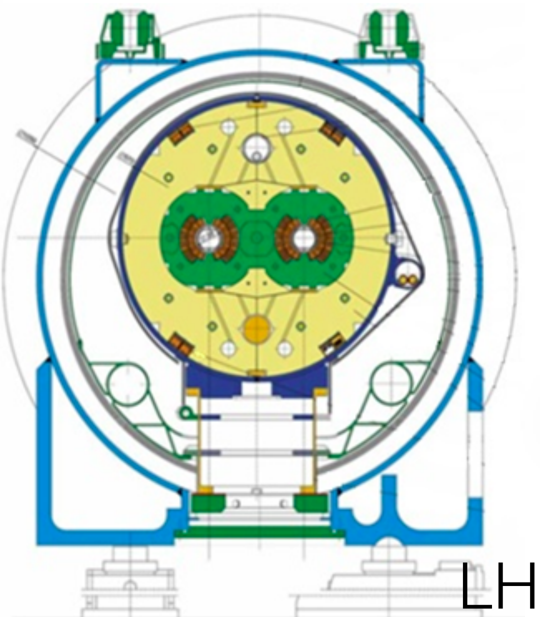
CCT



Shiltsev/Zlobin, FNAL)

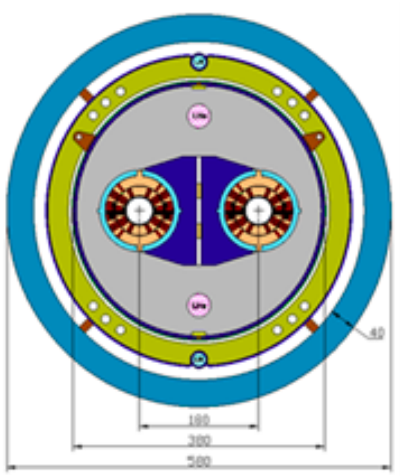


SSC, 50mm
6.6T, 4.3K



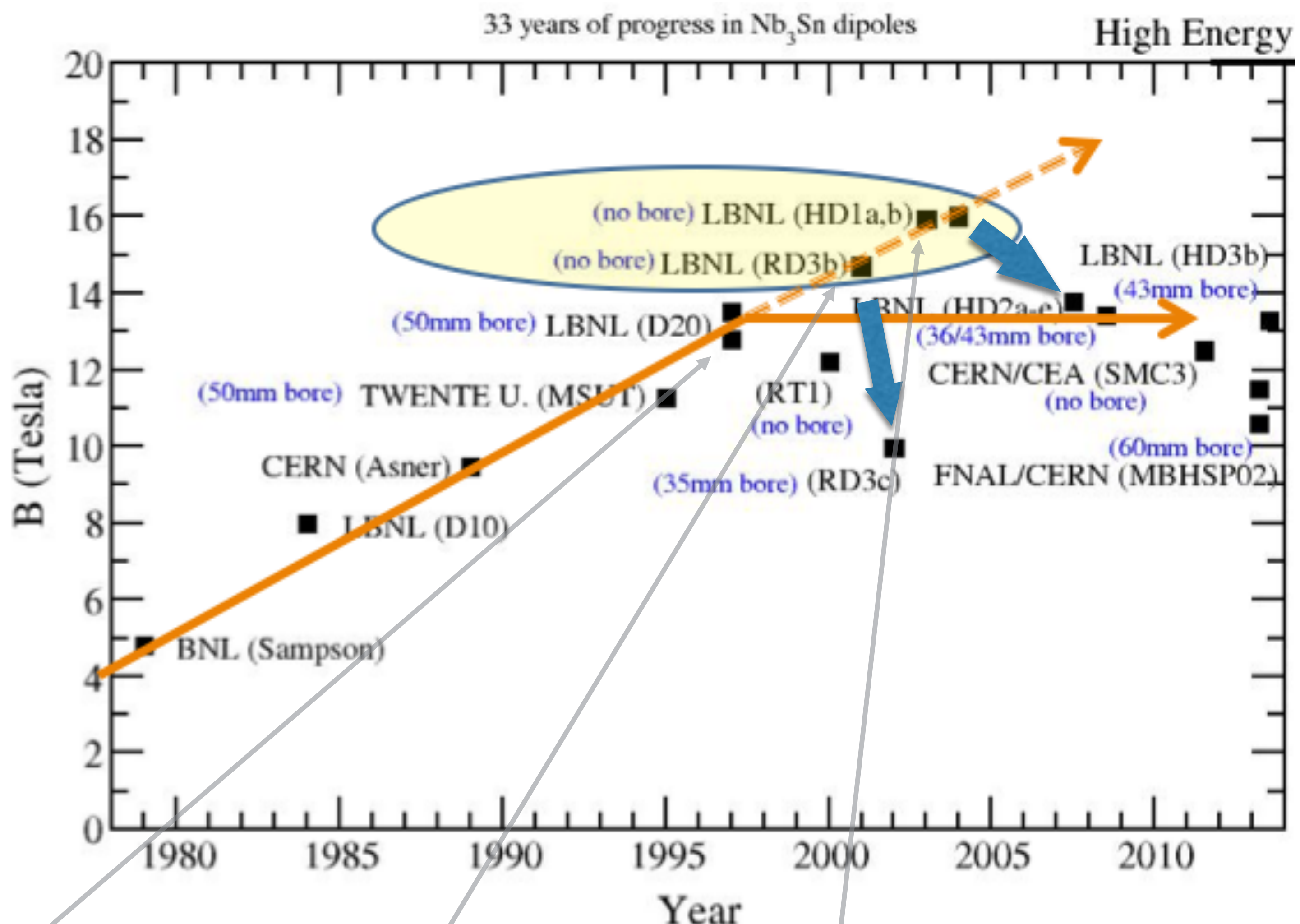
LHC, 56mm
8.3T, 1.9K

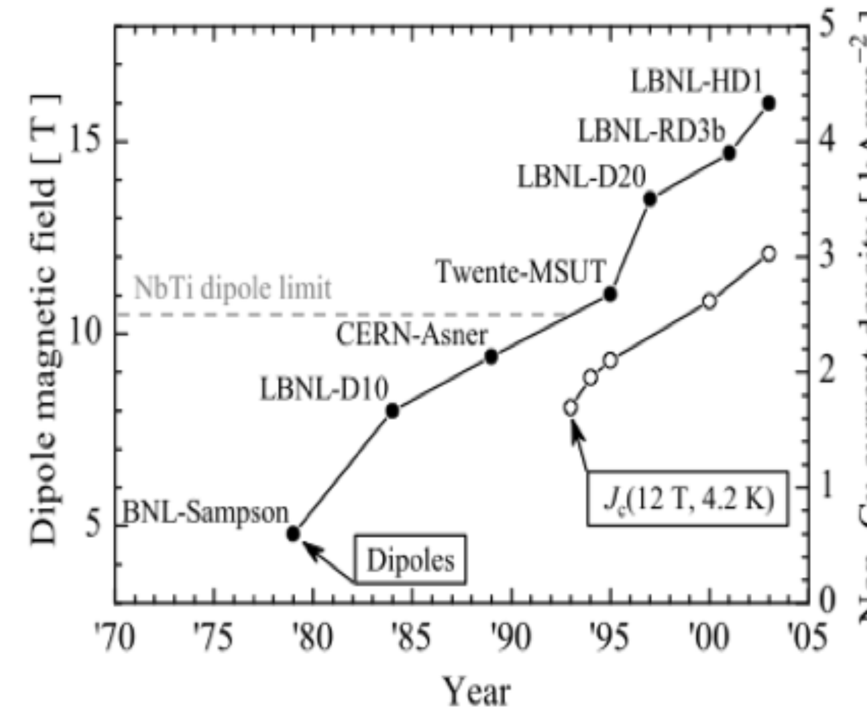
LHC, 60mm
11T,1.9K
FNAL/CERN



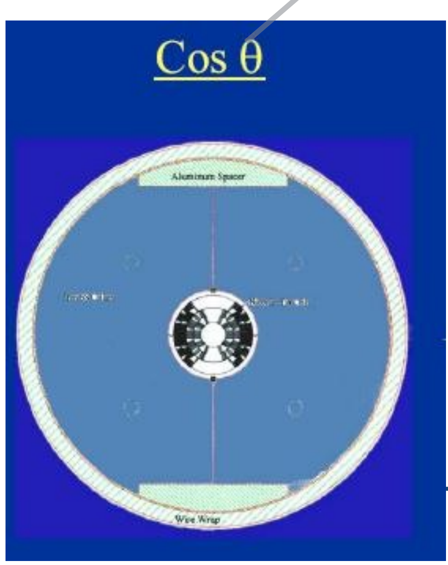
VLHC, 43mm
10T, 4.5K

Dipole magnet records in 3 configurations (LBNL)



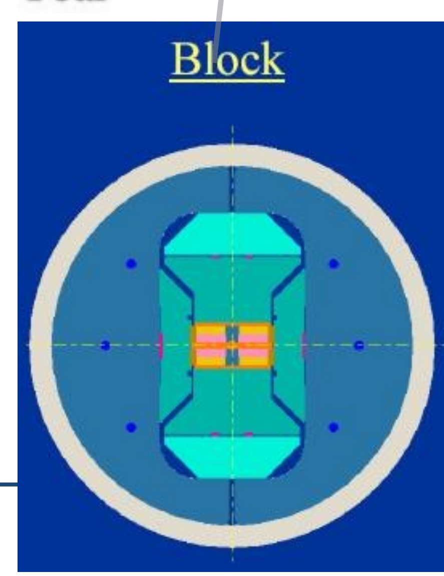


Field performance correlated to Conductor performance



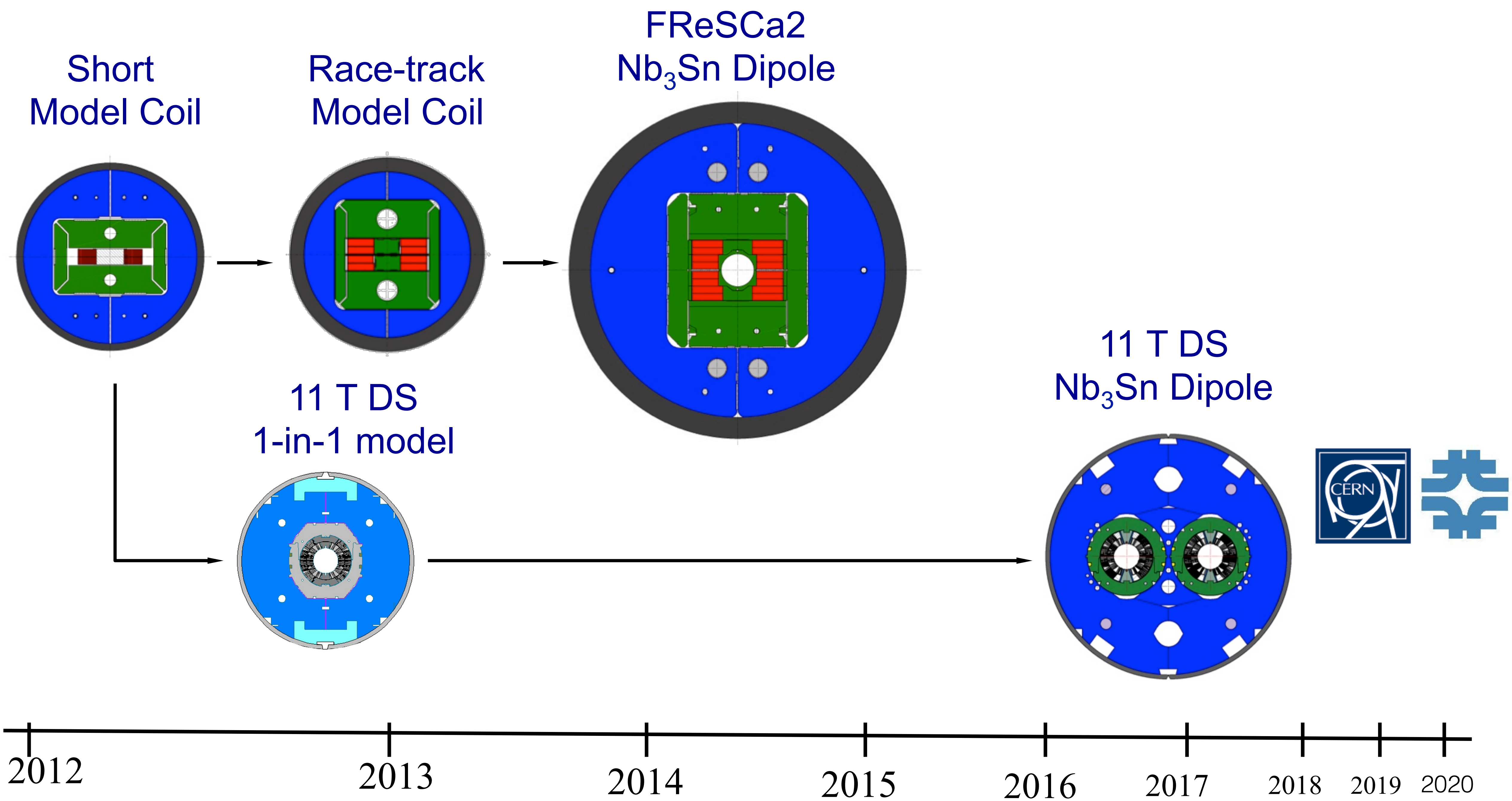
D20

RD



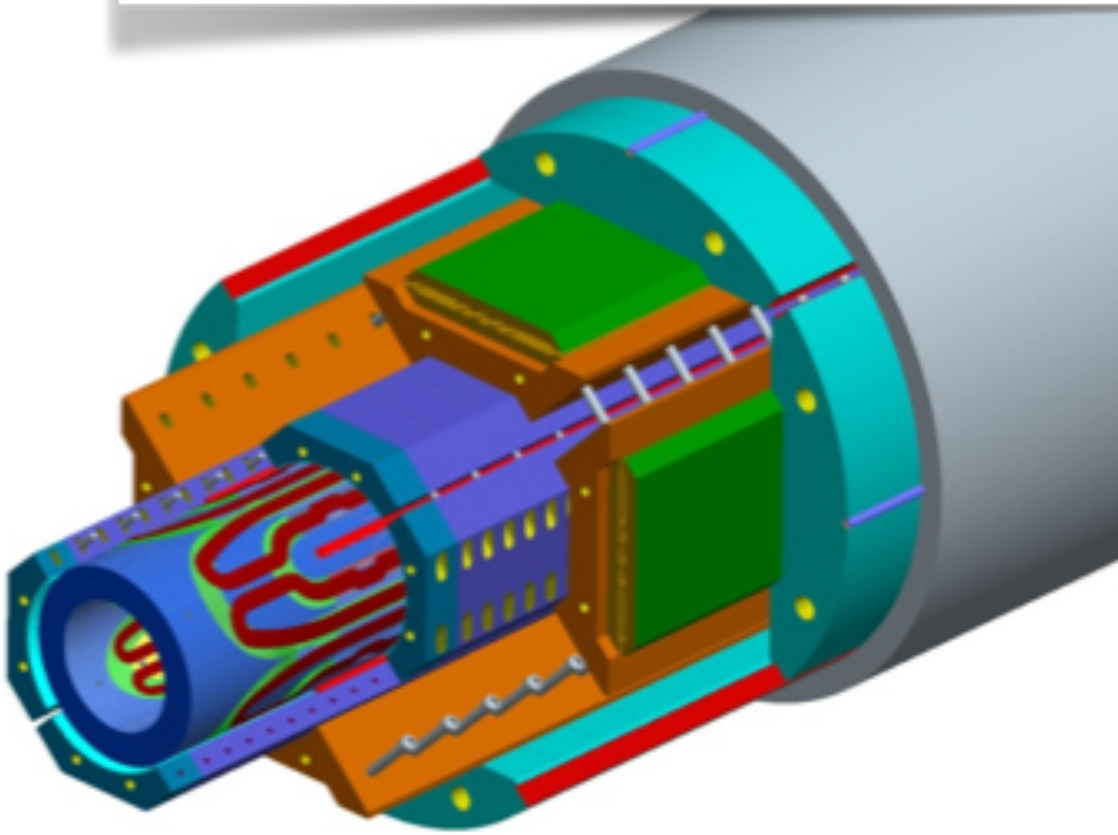
HD

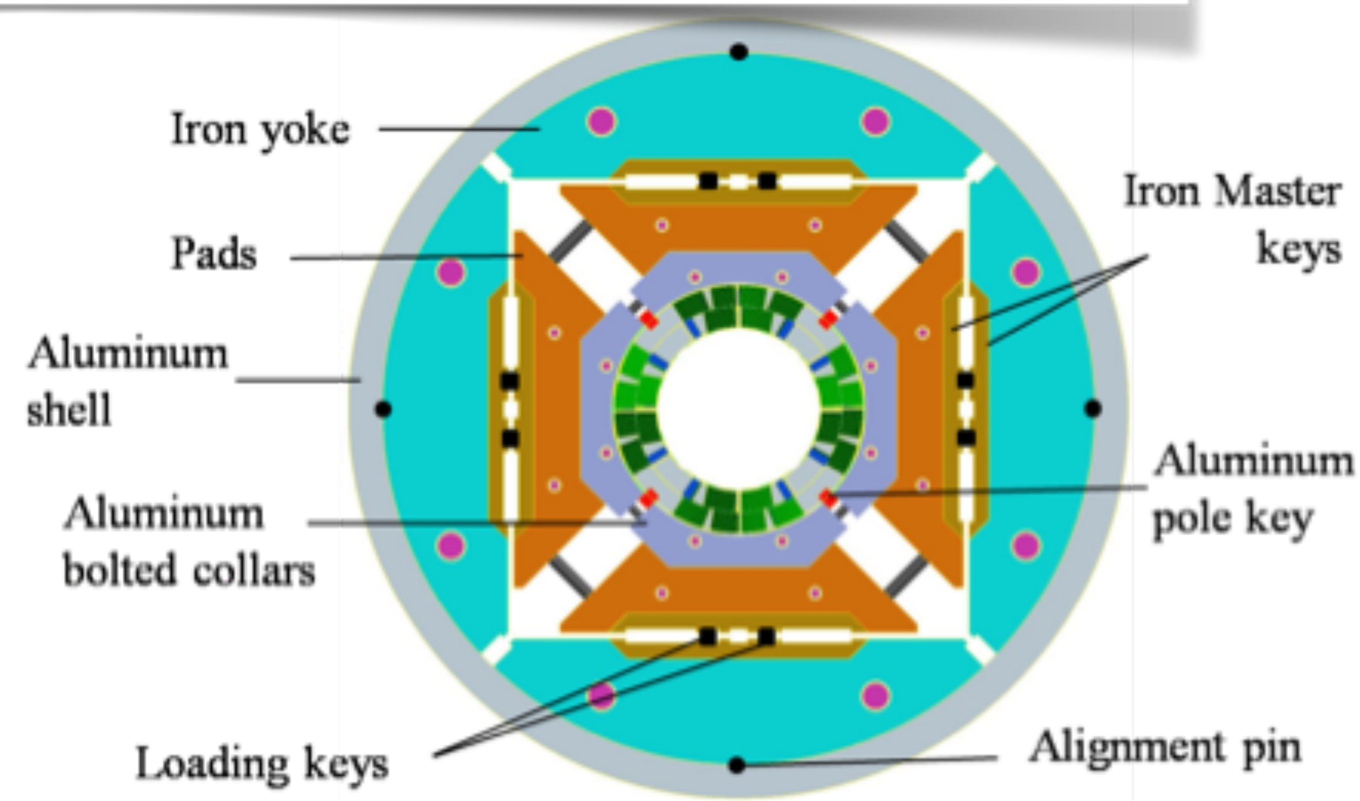
High Field Magnet R&D (CERN)

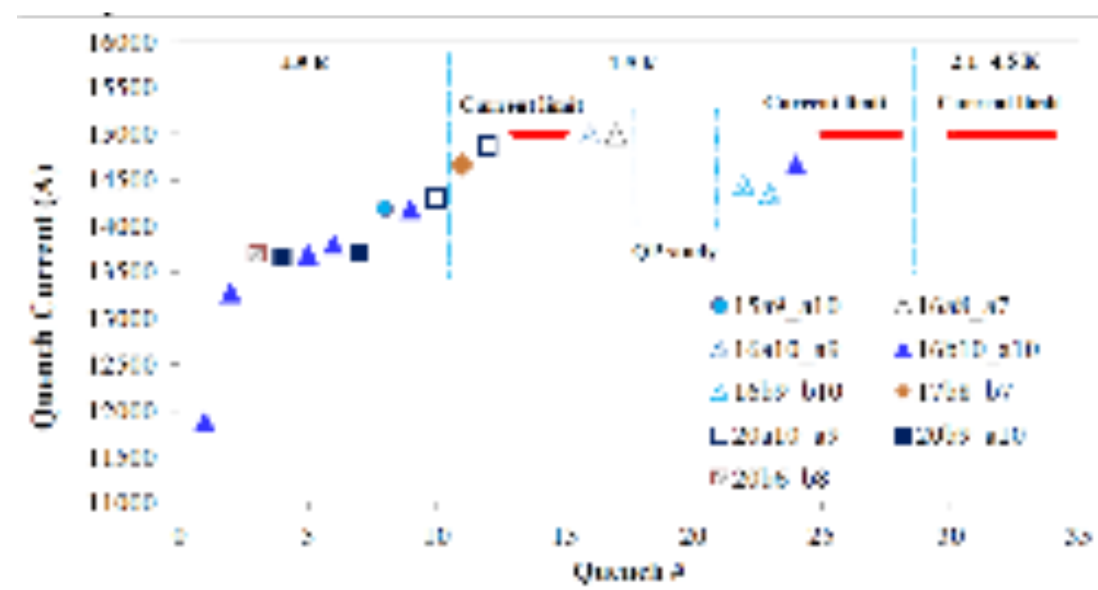


Nb_3Sn technology is being readied by LARP: HQ ➠QXF ➠ Hi-Lumi upgrade

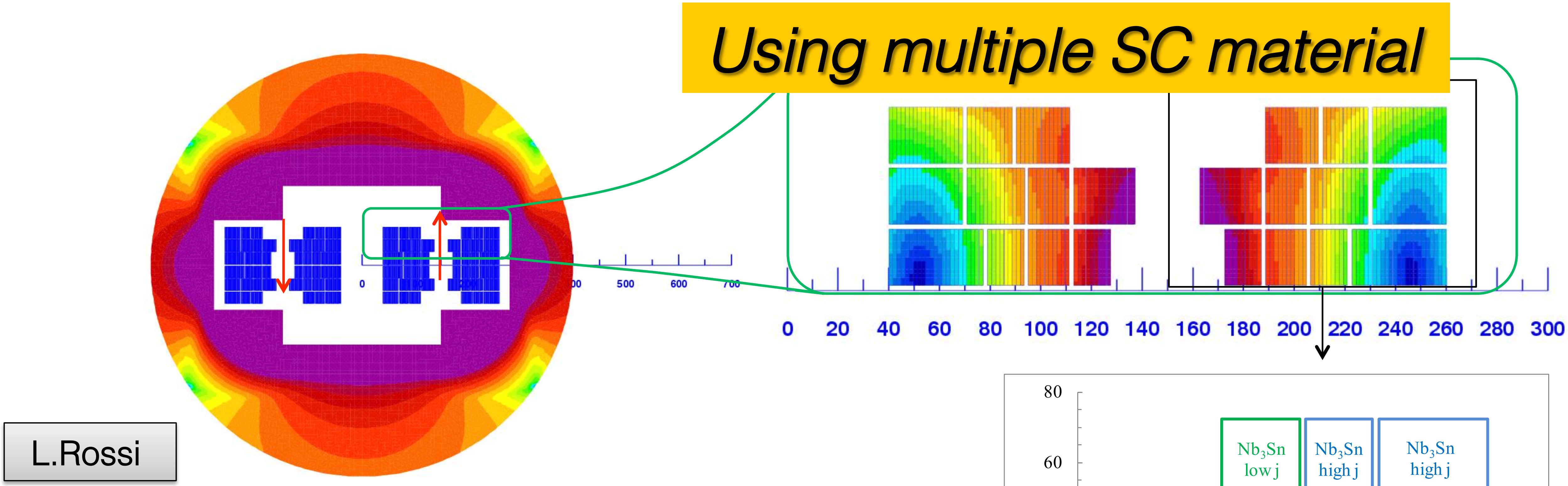
Design, fabrication, and test results from LARP: FNAL, LBNL, BNL

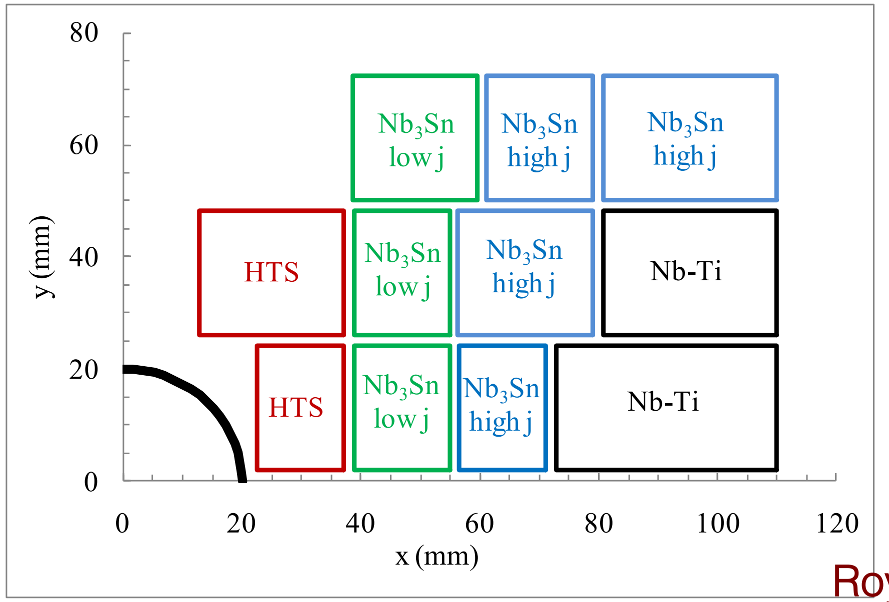






First consistent conceptual high field design from CERN





Material	N. turns	Coil fraction	Peak field	$J_{overall}$ (A/mm^2)
Nb-Ti	41	27%	8	380
Nb3Sn (high Jc)	55	37%	13	380
Nb3Sn (Low Jc)	30	20%	15	190
HTS	24	16%	20.5	380

20 T field!

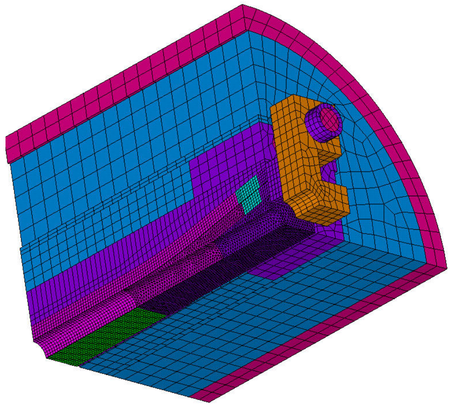
Roy Aleksan
CERN
Feb. 22, 2013

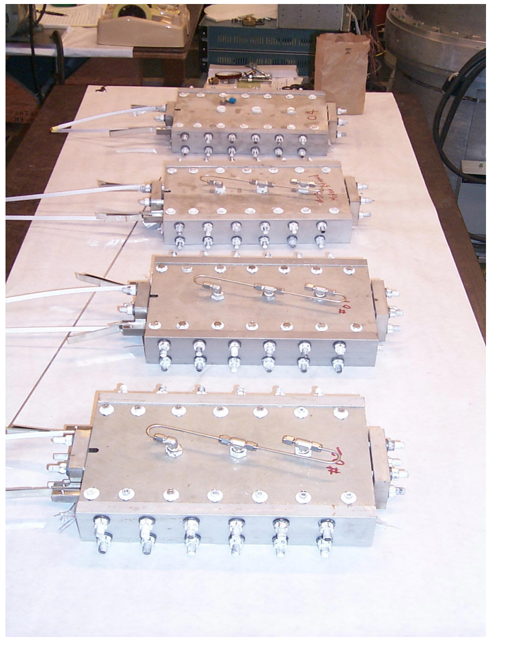
Magnet design: 40 mm bore (depends on injection energy: > 1 Tev)
Approximately 2.5 times more SC than LHC: 3000 tonnes! (~4000 long magnets)
Multiple powering in the same magnet for FQ (and more sectioning for energy)
Only a first attempt: cosϑ and other shapes needs to be also investigated

The community has extensive experience to draw on

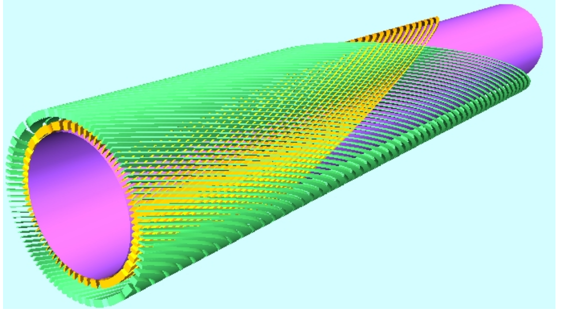
A variety of geometries

- Cos-Theta – D20, 11T and more recently, LARP
- Common Coil
- Block
- Sub-scale racetracks
- Some Canted-Cos-Theta





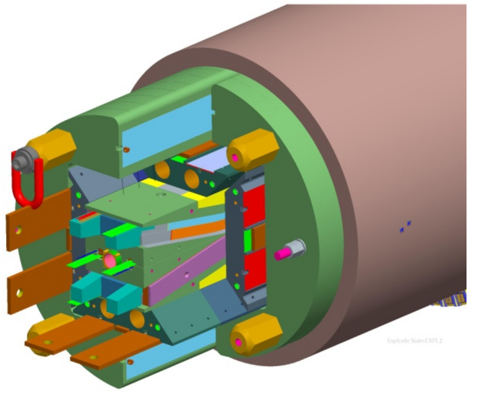
Analysis tools



Unique Instrumentation and Diagnostics

Infrastructure

- Fabrication
- Testing



We have the tools and experience required for success, but need to increase the effort (a lot)

Timeframe for meaningful impact

An high energy pp collider looks to be a long way off, but available technology options need to be understood during the early design stage. There is a need to get beyond what we eventually will use.

It took the LHC about 20 years to make evolutionary improvements to a technology that is now more than 40 years old.

We have time but not that much time. And we need to substantially raise the level of expectation for magnet performance.

So, what is the strategy?

Achieving Aggressive Goals Requires a Paradigm Shift (1)

Old Paradigm: Need ~ 20% operating margin

So, for 16T operating field we would need a 20T magnet
This exceeds the limit for Nb_3Sn and requires HTS

Significantly higher cost than NbTi. The last 2 – 3T is expensive!

New Paradigm: Increase fraction of operating field. Could potentially save billions for a collider.

Note on conductor cost. (highest quality material available)

NbTi ~ \$300/kg
Nb_3Sn ~ \$2000/kg
Bi-2212 ~ \$20,000/kg

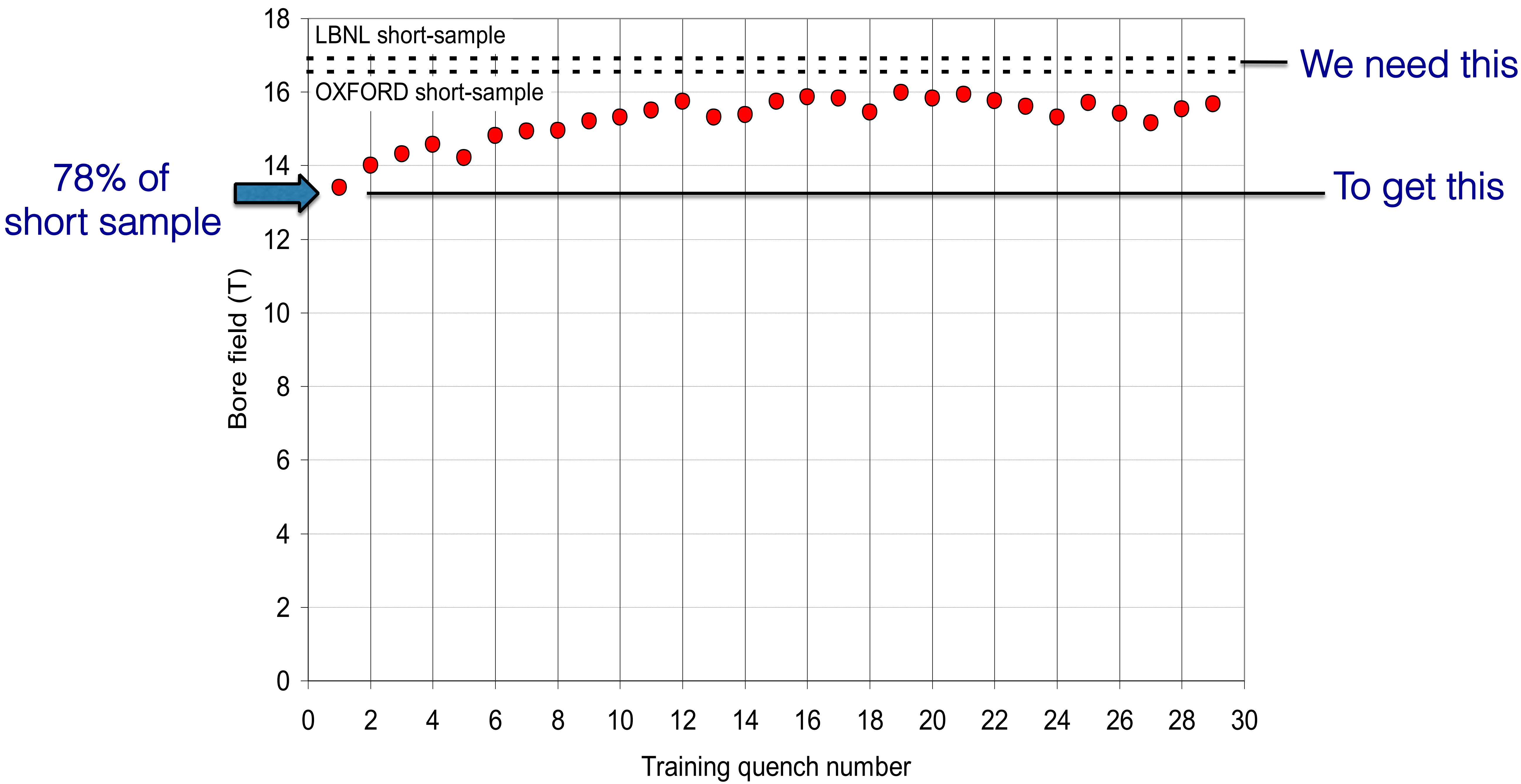
So, an additional 2T using HTS for an insert would cost 1.5X as much as for the first 16T using Nb_3Sn

Upshot: HTS is not a candidate for ring magnets

Old Paradigm: Some training and possible retraining are undesirable but expected and accepted
Conflicts with increase in fraction of operating field

New Paradigm: Understand and minimize or eliminate training (not trivial).
Linked to relaxing operating margin requirement.

HD-1 exhibited “acceptable” training performance



Achieving Aggressive Goals Requires a Paradigm Shift (2)

Old Paradigm: Need grading to minimize conductor

Still true. Even more so with expensive conductor and more of it needed for higher fields. However, grading increases stress and Nb_3Sn is stress limited (~ 200 MPa)

New Paradigm: Need a design that keeps coil stresses within limits. Grading is particularly effective for multi-layer coils required for high fields

Old Paradigm: Large bore is desirable but expensive

Still true but not as much. For high fields, bore size has relatively small impact on conductor quantity. Coil width is large compared to bore size. But, larger bores lead to higher stress.

New Paradigm: Don't obsess over this parameter. Eye on stress, but other issues may dominate.

Old Paradigm: Test and measure field of all magnets

Wasn't intended for LHC but ultimately that was the case

New Paradigm: Magnets have to be as simple and reproducible as possible.

Summarizing the key elements of the new paradigm

1) Decrease operating margin
2) Minimize or eliminate training
3) Fully utilize grading
4) Flexible choice of bore diameter
5) Manufacturability (reliability and reproducibility)

Take baseline technologies to higher level of performance

- The HD magnets are on the asymptote for Nb_3Sn so it will be difficult

Combine with a strong component of high-risk, potentially high payoff disruptive technology development that can leapfrog the status quo

A parallel program of supportive R&D

- Advanced materials R&D
- Explore other applications of the new technology that stress current capabilities

Important ancillary issues

◆ A high field (relatively) small radius machine will need to cope with severe synchrotron radiation heat loads

From L. Tavian talk, FCC Design Study Kick-off meeting

- 28.4 W/m per beam for FCC-hh 100 km, i.e. a total load of 4.8 MW
- 44.3 W/m per beam for FCC-hh 83 km, i.e. a total load of 5.8 MW
- If this load is falling directly on the magnet cold masses working at 1.9 K or 4.5 K (not yet defined), the corresponding total electrical power to refrigerators is

–> 4.3 or 1.1 GW for FCC-hh 100 km
–> 5.2 or 1.3 GW for FCC-hh 83 km

Mitigation requires working closely with vac and accelerator physics

Any collider magnet design must allow for mitigation of this problem

◆ Operating temperature is another
4.5 K has less complex cryo system
Better for absorbing heat load
And perhaps more stable magnet operation

One way forward is to try different ideas

Or retry old ones . . . .

A new application for an old idea

Paper by D.I. Meyer and R. Flasck in 1970

(D.I. Meyer, and R. Flasck “A new configuration for a dipole magnet for use in high energy physics application”, Nucl. Instr.and Methods 80, pp. 339-341, 1970.)

A NEW CONFIGURATION FOR A DIPOLE MAGNET FOR USE IN HIGH ENERGY PHYSICS APPLICATIONS*

D. I. MEYER and R. FLASCK

Physics Department, University of Michigan, Ann Arbor, Michigan 48104, U.S.A.

Received 16 December 1969

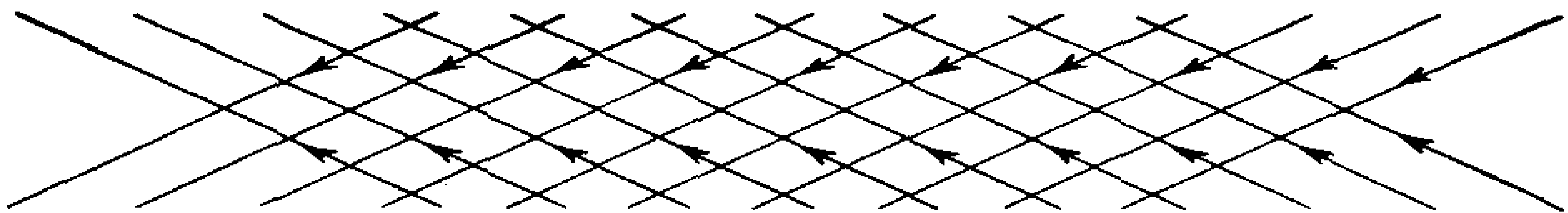


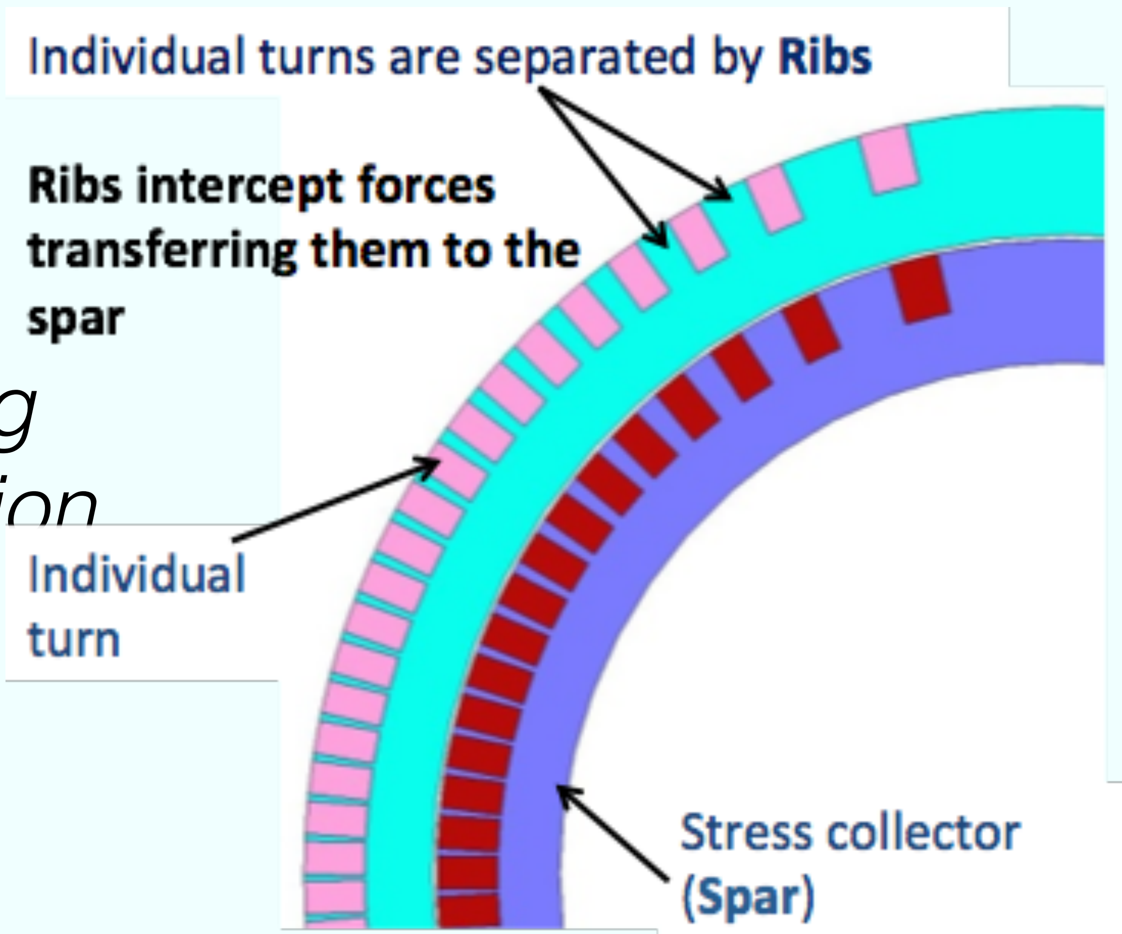
Fig. 2. Two superimposed coils with opposite skew.

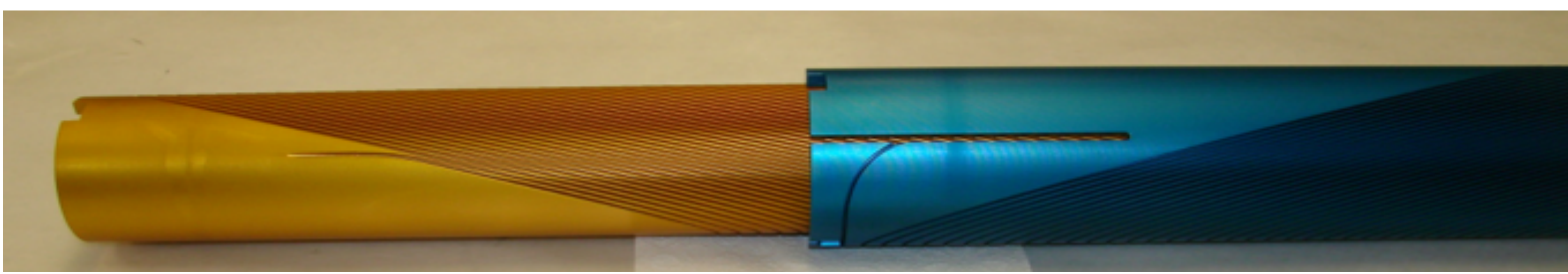
Renewed interest during the past decade

A New Paradigm, building on established foundations
The Canted Cosine Theta Magnet (CCT)

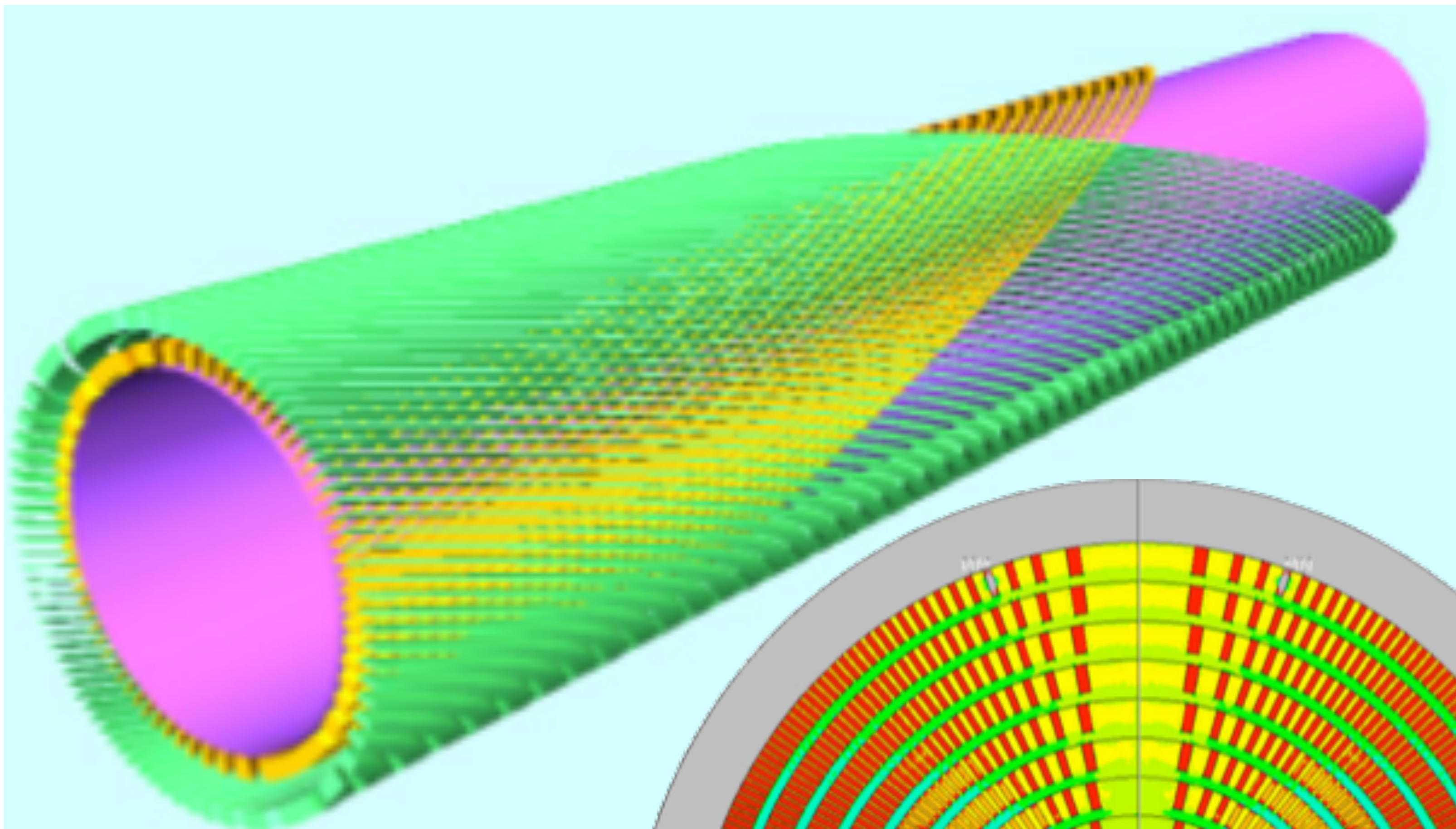
Key characteristics we want:

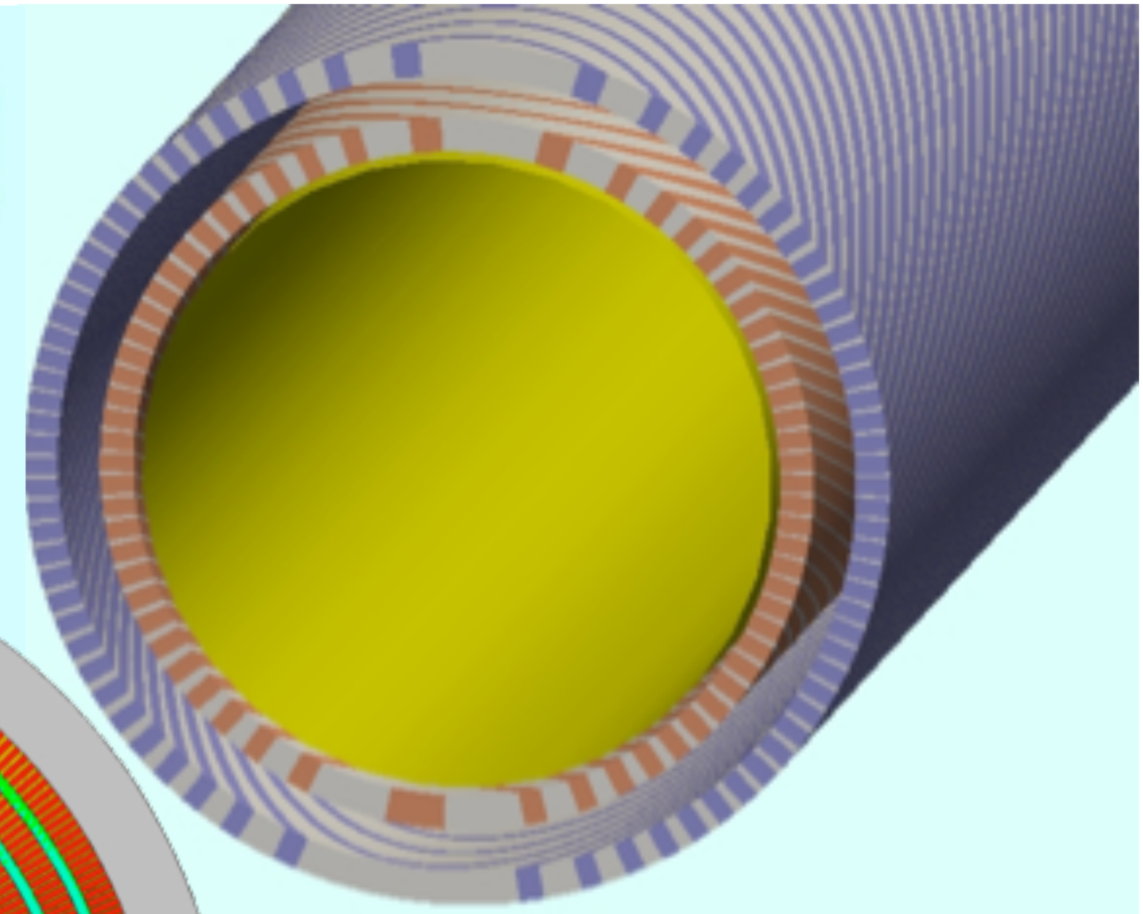
Remove the stress barrier
Incorporate grading for efficiency
Reasonable bore diameter for shielding
Scalable design allowing industrialization

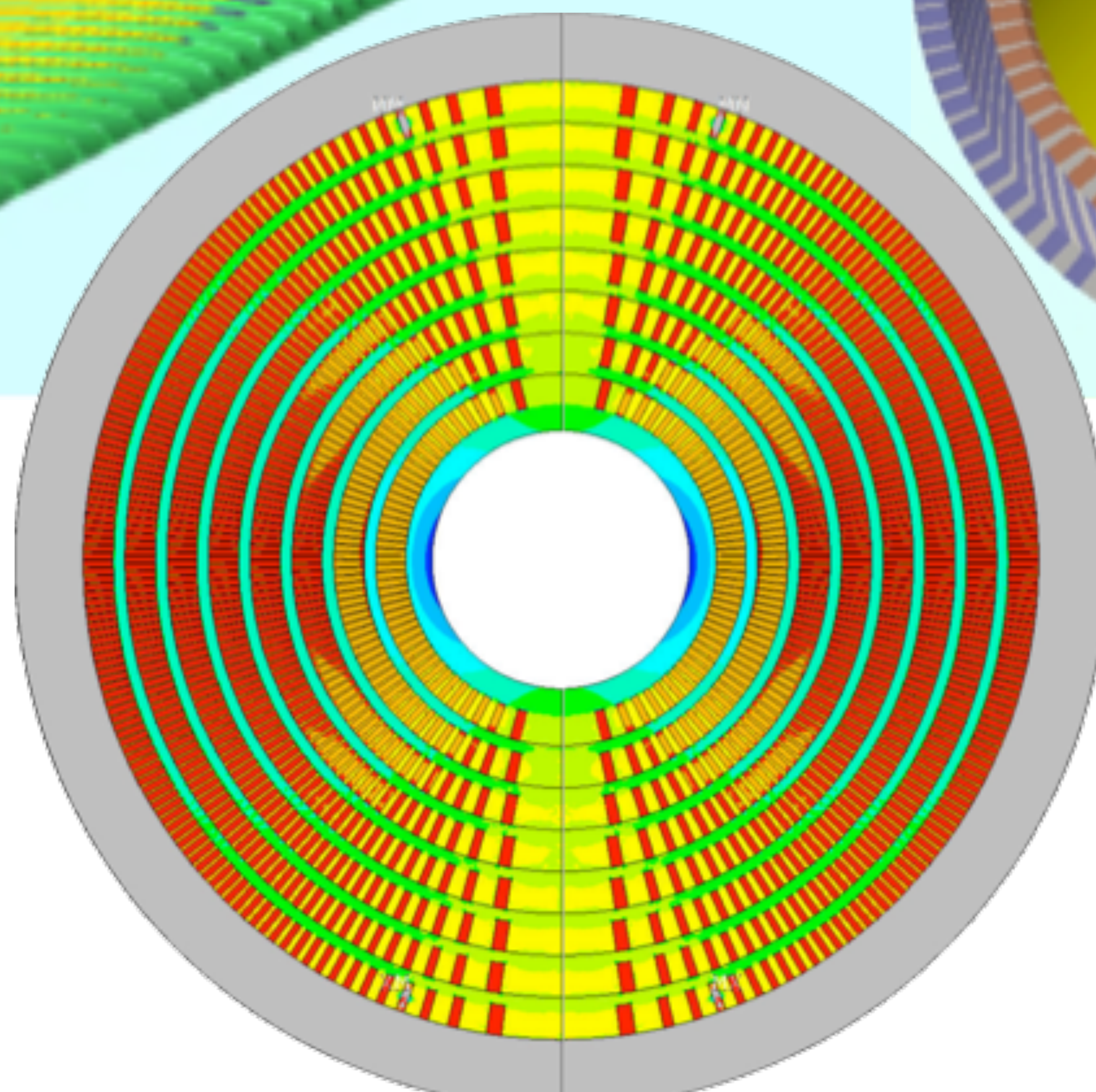




A scalable magnet concept…
building on experience with Cos(θ) and block designs







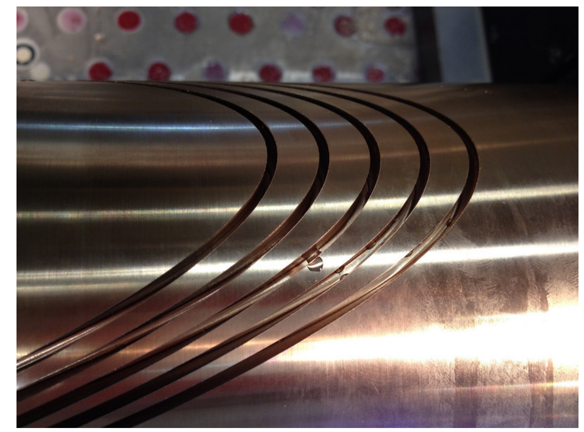
Cos(θ) distribution:
⇒*excellent field quality*
⇒$B_{cond} \sim B_0$

Stress interception
⇒*avoid force build-up*
⇒*reduce conductor stress*

CCT has potential to meet required characteristics

Stress is captured by rib, transferred to mandrel

- ➡ *No accumulation of stress on the mid plane*
- ➡ *No stress issue with larger bore*



Every layer can use different cable size

- ➡ *Allows near optimal grading for conductor efficiency*
  - ✓ *Significant saving in Nb_3Sn over Cos(θ) designs*

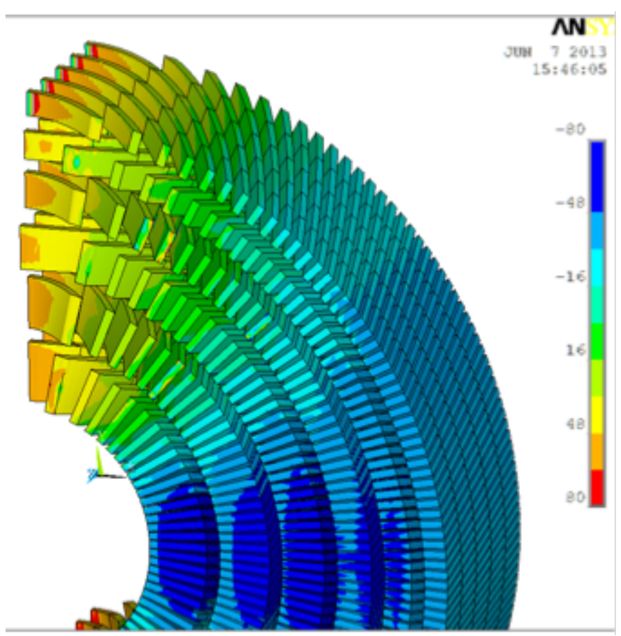
Conductor mass scales with bore radius only

Excellent field quality ("for free")

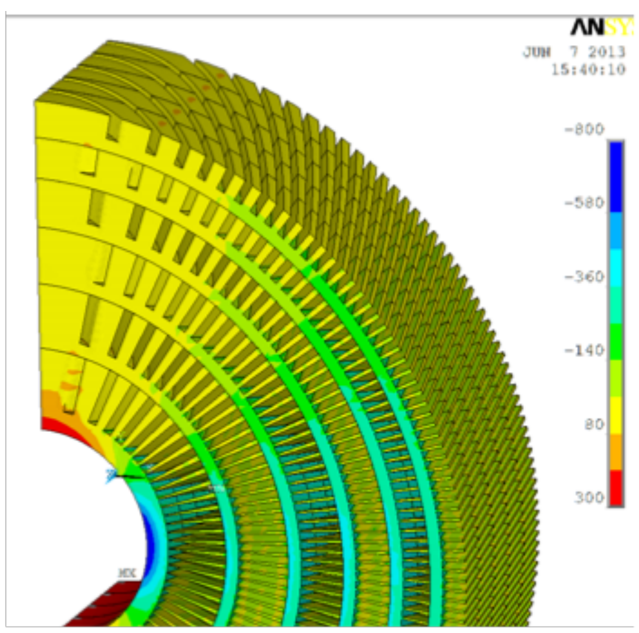
Fabrication:

- ➡ *Minimal external structure*
- ➡ *No spacers, end parts, etc.*
- ➡ *Simple winding ➪ Industrialization*

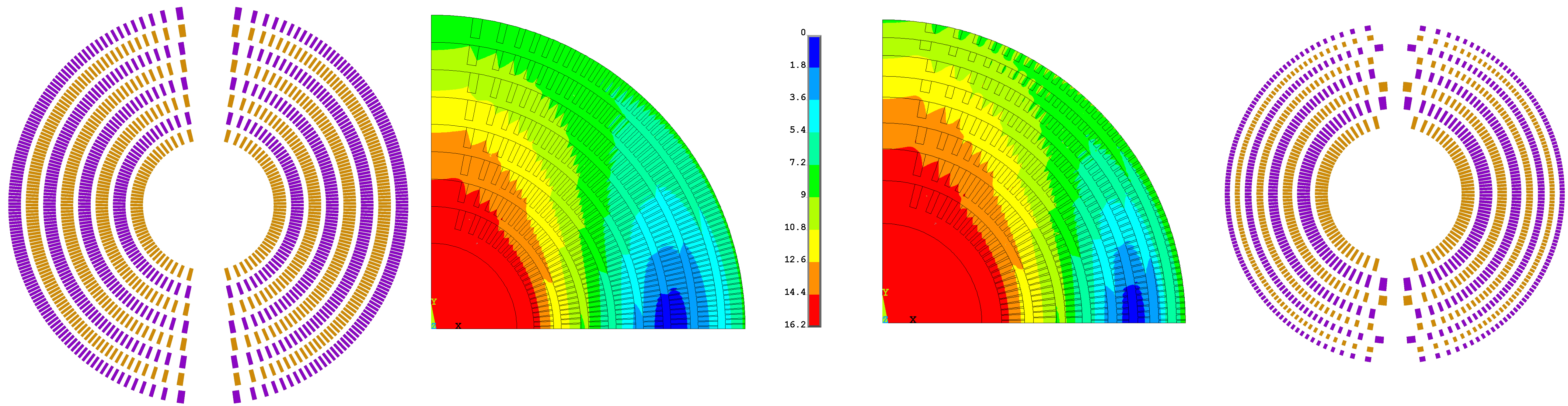
Coil stresses



Structure stresses

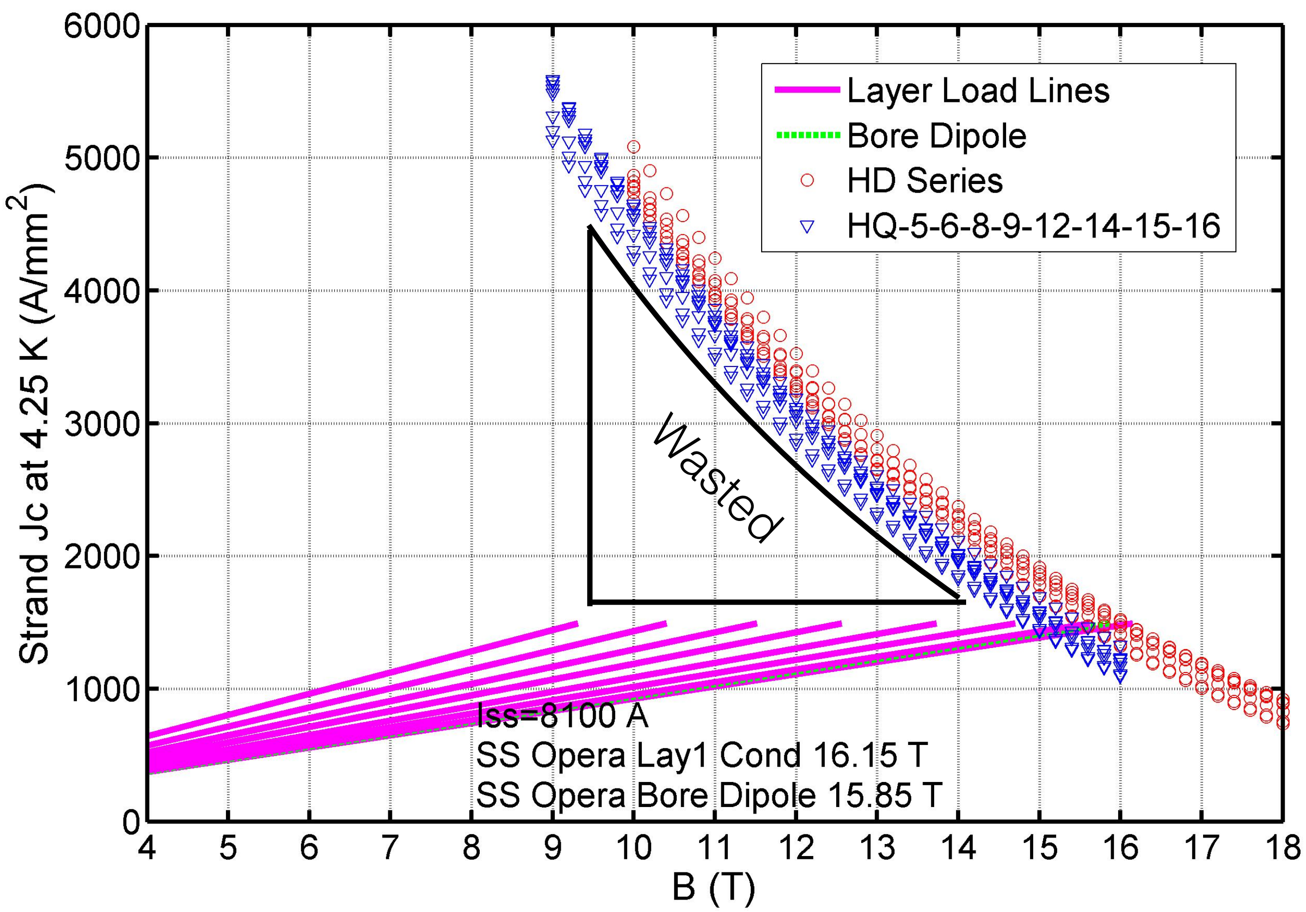


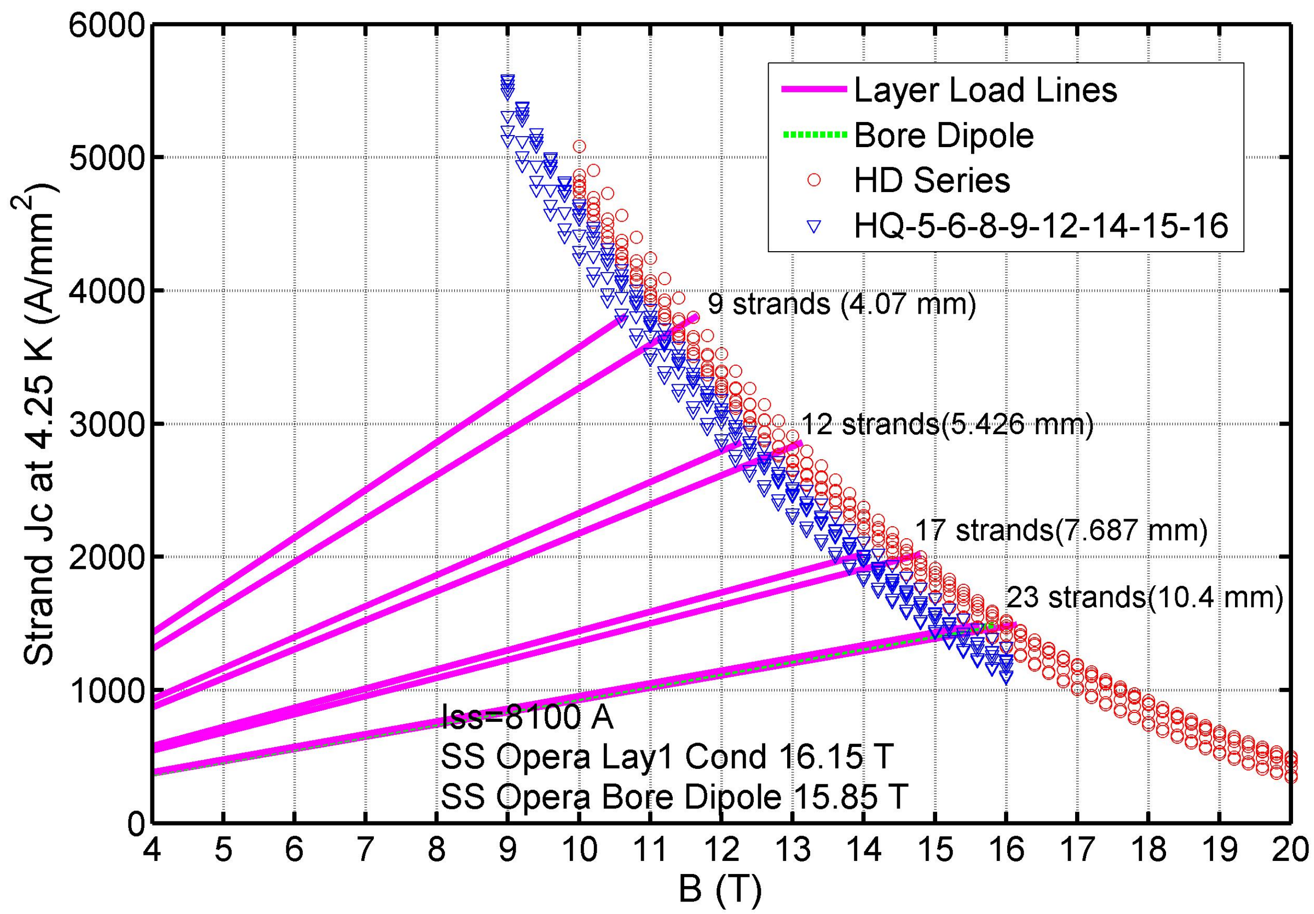
Minimize conductor by “grading”



Un-graded

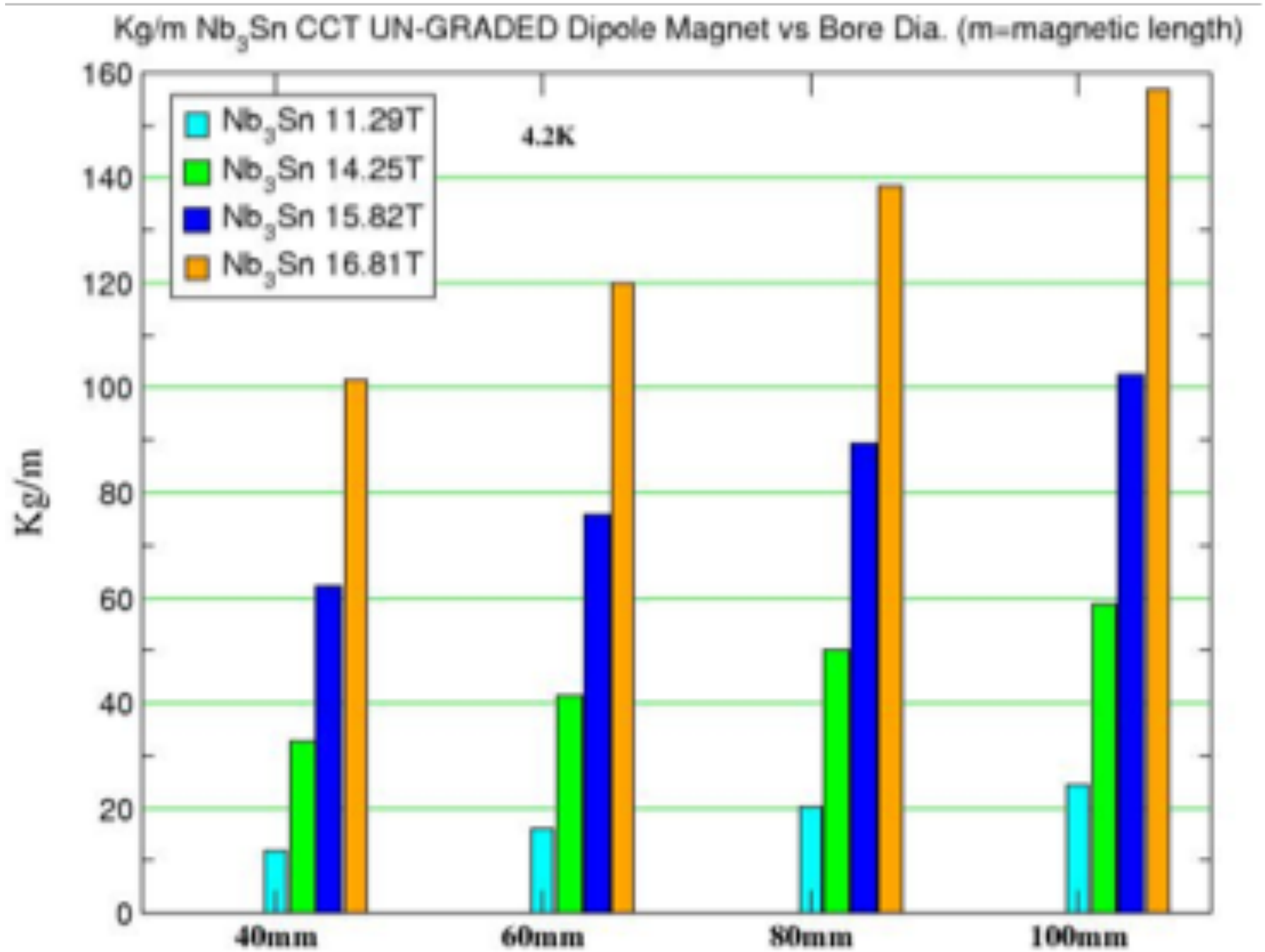
Graded



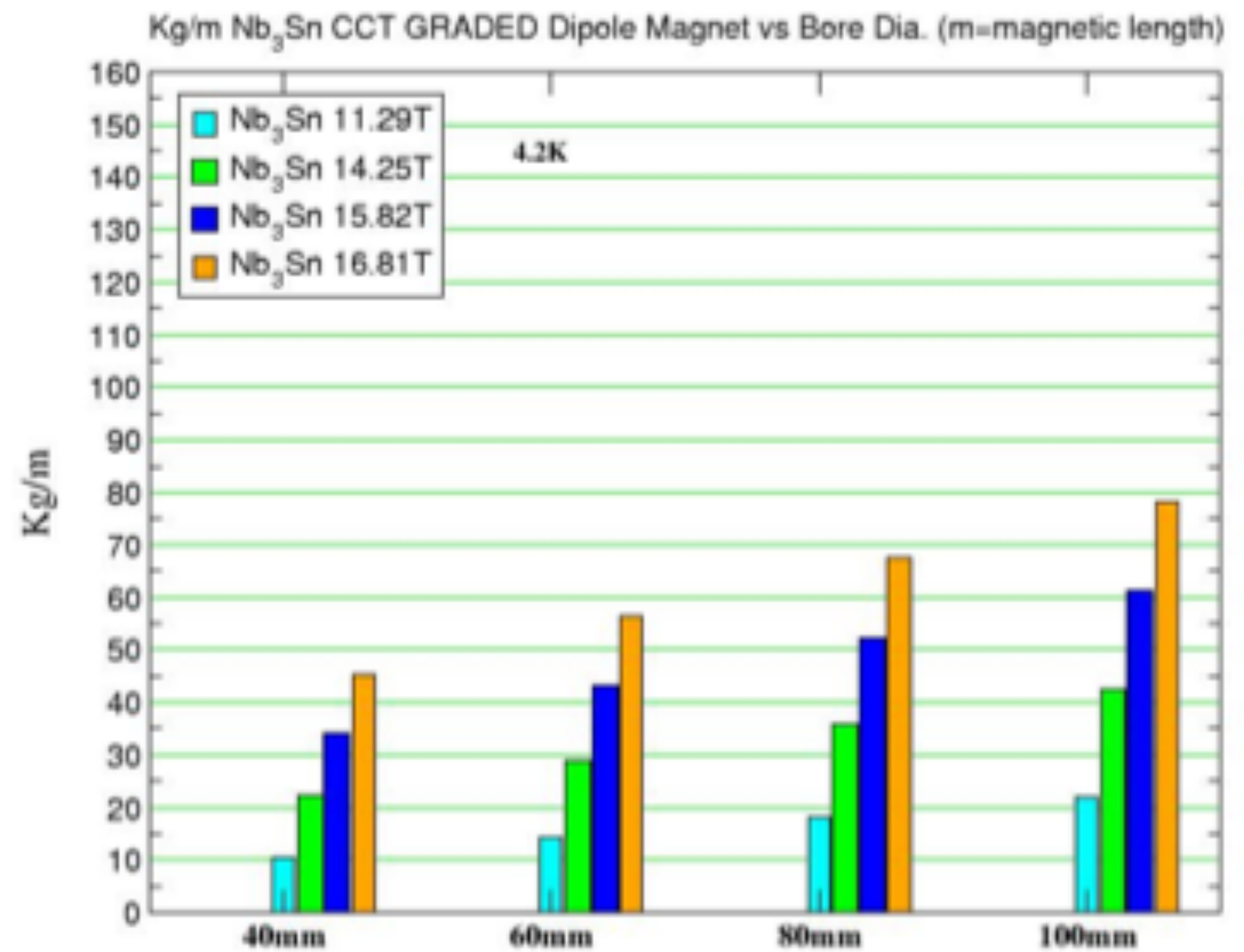


Grading the conductor is critical for large production

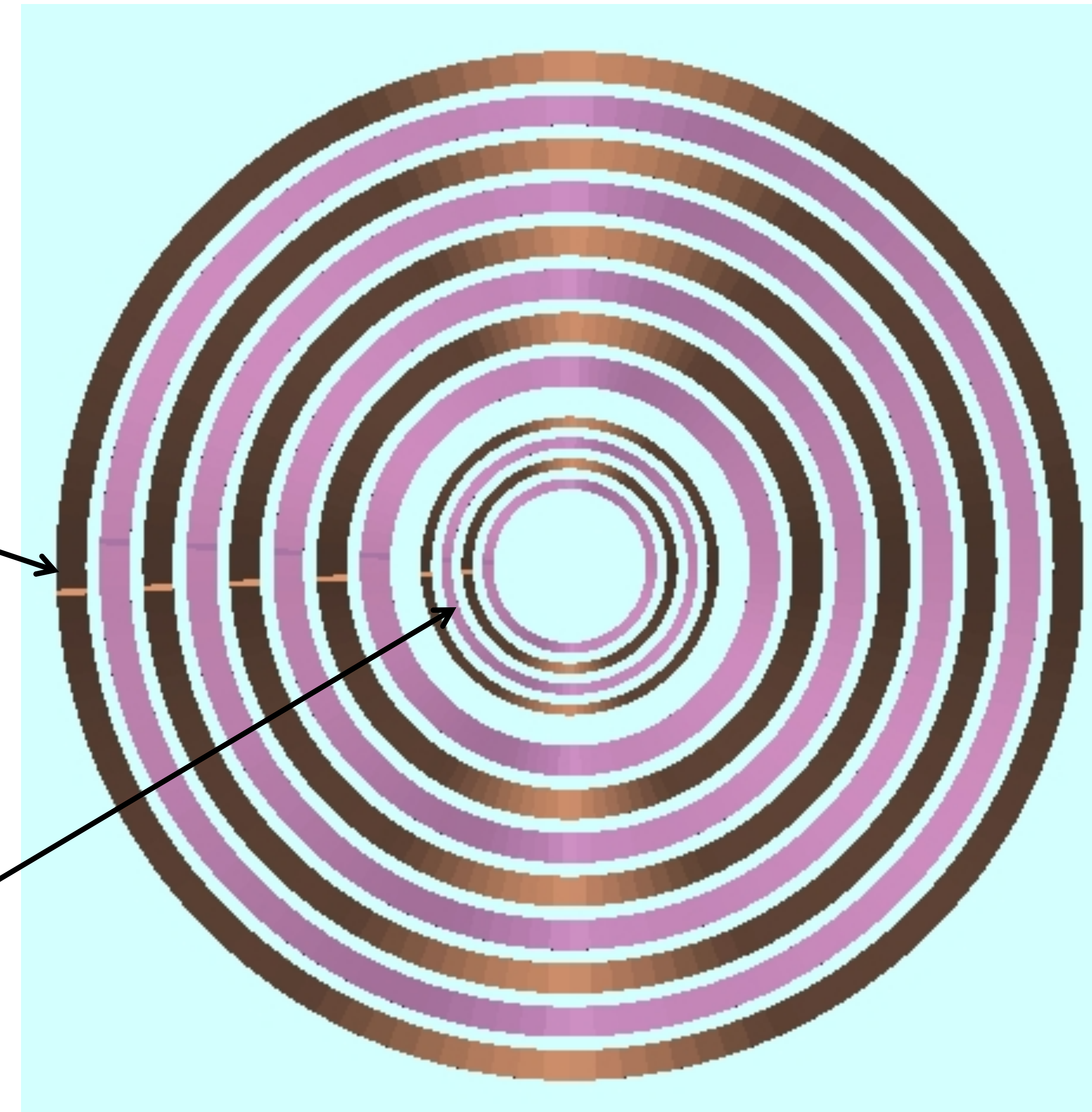
Un-graded CCT

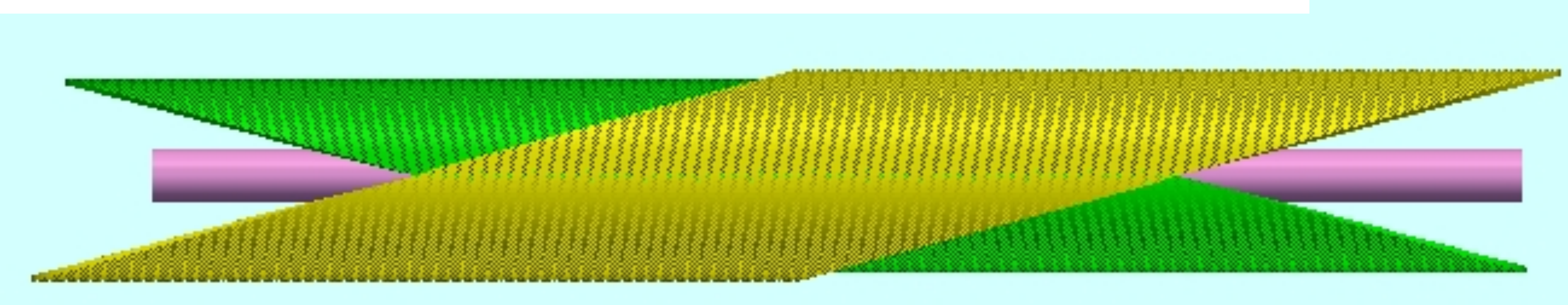


Graded CCT



High Field CCT Magnet Cross-Section





The CCT has the potential to meet the goals of the new paradigm but needs to be demonstrated

Simplicity – better performance?

- Robust, reproducible, manufacturable
- Minimal external structure (little or no prestress?)
- Mandrel (ribs + spar) replaces pole, collars, end parts, spacers
- No body-end transition
- Modest tooling requirements

Intrinsic Stress Reduction

- No accumulation of stress on the midplane
- Allows grading (near optimal conductor efficiency)
- Allows larger bores (conductor scales with bore radius only)

Excellent geometric field quality

Combines the best of our former program

- Subscale characteristics – simple and relatively inexpensive
- High field – scalable to highest fields and use of inserts
- A natural platform to apply the tools we have developed over the last 2 decades

CCT is LBNL's highest priority

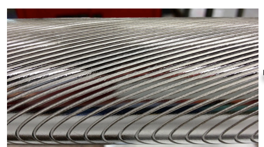
Phase I R&D Program Tree – Creating the New Paradigm

NbTi CCT

2.5T NbTi

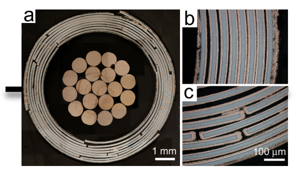
5 T NbTi - - - - - - - - 10 T NbTi

2212 and YBCO inserts



Insert Test

6X1 - - - CORC



Self Field Test

Self Field Test

Rutherford

HTS Racetrack

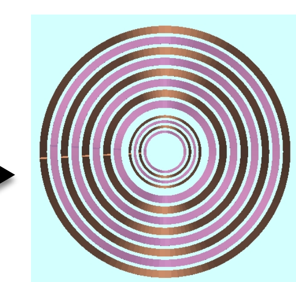
Insert Test

CCT Insert

5-Turn Nb_3Sn CCT

Nb_3Sn CCT

10 T Nb_3Sn — 13 T Nb_3Sn —14.6 T Nb_3Sn—16 T Nb_3Sn + HTS 19 T →



DOE created an accelerator R&D Subpanel to align accelerator R&D with P5

Report is due in April 2015

The US magnet programs (BNL, FNAL, LBNL, and NHMFL) submitted a joint “white paper” outlining a coordinated US magnet R&D program to

1. Develop accelerator magnets at the limit of Nb3Sn capabilities.
2. Investigate accelerator magnet designs with Low Temperature Superconductor (LTS) and High Temperature Superconductor (HTS) coils for fields beyond the capability of Nb3Sn.
3. Drive high-field conductor development, including Nb_3Sn and HTS materials for high-field accelerator magnets.
4. Address fundamental aspects of magnet design, technology and performance that could lead to substantial reductions of magnet cost.

The Program will focus on the development and test of a small-aperture high-field (~16 T) Nb_3Sn dipole demonstrator for the Future Circular Collider (FCC) studies, development of high-current (~10 kA) HTS cables and small insert coils using these cables, and accelerator magnet design studies to identify the possibilities of magnet cost reduction. Phase 2 (FY18-FY20) envisions a long-term transition to accelerator quality magnets relevant for 100 TeV scale pp collider, with integrated management of stress, grading of conductors, and cost savings resulting from design, technology and performance optimization.

Philosophy: Raise the bar on expectations and implement an aggressive approach

- Leverage through collaboration

- Shoot for the moon – high risk, high payoff
    - ✦ Aim for the highest dipole fields (greatest challenge reaps the highest rewards)
    - ✦ New ideas for simplicity
    - ✦ Explore the limitations of materials and structures
- Implement a technically driven program that strives for one test at least every 3 months. i.e. make our mistakes quickly and learn from them

Outcomes are . . .

- New record dipole fields
- A discontinuity in superconducting magnet technology

    A platform that can be used to design and build magnets for a variety of applications with optimal field, coil configuration and bore size
- Significant increase in performance/cost ratio

Roadmap

"Significant cost reduction can only be achieved by introducing new paradigms and aggressively pushing the technology beyond the accepted limits. Modest improvements of the status quo will not be adequate "

Reduce development time via sub-scale studies - Months vs years for new designs

Integrate design and analysis tools - Filament to structure (fundamental understanding)

Diagnostics/instrumentation to for design feedback and fundamental understanding

Aggressive conductor development
- Scalable sub-element structures
- Performance improvement

Demonstrate feasibility of 16 T operating field
- As broad a set of parameters as possible
- Designs need to account for operational challenges, e.g. SynchRad

Cost reduction engineering (engage industry and universities)

HTS (relatively small fraction of program)
- Continued conductor development
- Build HTS accelerator magnets (feasibility at some level)
- Necessary for special applications (separation dipoles, IR magnets?)
- Try to develop market drivers outside HEP to lower cost and maintain R&D

Conclusions

Accelerator quality dipoles with an operating field of 16T are feasible

Making them affordable is a challenge and will take time and require more resources than we have now. It will be a world-wide effort

Very important

Program has to be integrated (AP, cryo, etc) and take into account ancillary problems, e.g. SR heat load

HTS has many issues to understand and overcome

We need to prove feasibility, which could be demonstrated within the next year or two then we can worry about the cost.