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An Overview of Tokamak Alternatives in the US Fusion Program with the Aim of Fostering Concept Innovation

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The US fusion program has operated for just over 50 years, during which time the tokamak has emerged as the most promising vehicle for a burning plasma experiment. However, many other concepts have been built and investigated as alternatives (and possible improvements) to the tokamak, perhaps to make energy from fusion an economic reality sooner. This Paper is an overview of the conventional alternatives to the tokamak and a set of those that are not so conventional with the aim of fostering concept innovation. Usually the devices are grouped into magnetic, inertial, electrostatic, or other categories, with subcategories. Here, the groupings of conventional- and non-conventional-alternatives are used too. The conventional alternatives are those devices that have been adopted as serious alternatives, and for which many references are immediately available (e.g. rfp, mirror, stellarator, spheromak, laser ICF, etc). The non-conventional alternatives comprise approaches that are not being currently investigated or are worth consideration. In this grouping lie ideas like impact fusion, muon catalyzed fusion, and many historical ones (like the Elmo Bumpy Torus). Several examples of the physics of non-conventional alternatives are presented in summary form as examples of skunkworks in the hope that others will take up the challenge of concept innovation.

KEY WORDS: Fusion; history; alternates; tokamak; skunkwork; innovation; innovative confinement concepts.

INTRODUCTION

The talk on which this paper is based was presented at the 2004 Innovative Confinement Concept Workshop [1] and was motivated by a need to interest younger scientists in presenting novel ideas at the ‘Skunkworks’ session. The Skunkworks usually occurs towards the end of the conference, and is an opportunity to present concept innovations. For the last 3 years, only senior scientists were presenting, and it occurred that younger scientists were perhaps neither too familiar with the ideas that had gone before, nor perhaps were they too aware of methods of concept innovation. This paper is therefore written with the aim of bringing many of the previous concepts to light and in the hope that younger scientists will take up the challenge of concept innovation and contribute at ensuing skunkworks.

Previous concepts have been surveyed by a number of authors. Dolan’s 1982 compilation is perhaps the most thorough, giving overview and detail of both magnetic and inertial approaches [2]. A recent book by Braams and Stott covers the history of most of the magnetic configurations in both the US and abroad [3]. Teller compiled descriptions of the main alternatives to the tokamak [4], Lindl reviewed the status of indirect laser IFE [5] in addition to which, there are several textbooks and papers that review the alternatives [6–9]. At the recent 2002 Snowmass meeting (and previous Snowmass meeting in 1999), a large number of alternatives were discussed and talks/papers were presented [10], and the progress in the so-called Innovative

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Confinement Concepts (ICCs) can be followed in the online conference proceedings [11]. The history of the program is described in several prominent works: at about the same time that Dolan compiled his book, Bromberg interviewed most of the important scientists of the day and mined the DoE reports to compile a very thorough history of the program up until 1980 [12]. More recently, Conn [13] and Rowberg [14] have given their perspectives. The Office of Fusion Energy Science maintains an archive of the main documents that define the ICC approach, particularly the reports: *Opportunities in the Fusion Energy Sciences Program* (Table 2 in the Appendix A summarizes many of the machines that are in operation); *Report on Alternative Concepts* (reviews of the achievements made in the conventional tokamak alternatives); and, *Report on a restructured fusion energy sciences program* [15]. Personal perspectives of the history of the US fusion program are provided by Fowler [16] and Heppenheimer [17] amongst others.

It is perhaps interesting to contrast the US approach to that of Europe. In the US, many different configurations have been investigated and continue to be addressed. Such a broad-portfolio approach does not exist in Europe. There, the tokamak, stellarator and rfp comprise the full set of concepts (barring a couple of smaller devices). It is also interesting to note that funding for magnetic fusion in Europe is almost triple that of the US, giving a highly focused and very specialized program. This text focuses on the US program: summaries of alternatives and the history of the fusion program have been given for Europe [18].

The text is structured as follows. Section II gives an introduction to the history of US fusion with reference to major historical events, and a taxonomy of the main concepts is presented in section III. section IV contains a summary of some of the characteristics of non-conventional alternatives; section V outlines the physics basis of a few of these schemes, rewritten as 'skunkworks'. section VI contains a discussion of innovation in fusion science. section VII entails a discussion of some salient points and section VIII contains a brief summary.

HISTORICAL REVIEW OF ALTERNATIVES TO THE TOKAMAK

The history of tokamak alternatives begins in 1969 with the confirmation of keV electron temperatures by Thomson Scattering measurements in the Russian tokamak T-3, made by a British team of scientists [19]. In the years that followed, the tokamak became the main vehicle for pursuing controlled nuclear fusion throughout the world, and in time all concepts became alternatives. The tokamak is now sufficiently developed to consider building a burning plasma experiment to investigate the remaining physics and technology issues on the path to a commercial reactor [20], and while plans for the economic deployment of fusion reactors consider the tokamak as the main vehicle, alternative approaches are included in the hope that they will lead to more attractive reactor cores [21]. The tokamak alternatives are shown in Table 1. These are grouped into categories of mirror, stellarator, frc, spheromak, ST, internal ring, toroidal pinch, inertial (laser and beam). A small number of institutions have played a pivotal role in fusion research: the national labs (LANL, LLNL, ORNL, PPPL, NRL, Sandia), many universities (e.g., UWisc, UWash, UMD, UCLA, MIT, CalTech…), and industry (e.g., GA and GE). The science of alternative approaches is reaching maturity with a broad program, regular meetings and workshops, many institutions teaching fusion science from a large body of text-books, and theoreticians are now armed with capable simulations that can model most of the dominant issues facing the experimentalist.

The historical events, budget and alternative concepts investigated from 1965 to 1983 are shown in summary in Figure 1 (pre 1965 there are many events of great importance, and are treated in many of the references). In the early 1970s an oil crisis occurred and the fusion community was charged with building a new energy source given the best understanding at the time. With energy independence suddenly desirable, funding was made available to design and build both a tokamak (TFTR) and a mirror (MFTF). The fusion budget increased to a high of \$850 M (in 2002 dollars [22]) and remained there for nearly 6 years. It is the view of Conn that *scientific readiness* enabled the fusion budget to be driven up (by an external event out of the control of the fusion scientist). During that time, many other devices were investigated (rfps, multipoles, frcs, spheromaks, dipoles) at substantially lower levels of funding.

An external event occurred again in 1983: oil prices reached their lowest in a decade, and for this reason and for perhaps more political reasons also (see Bromberg), funding for magnetic fusion

Table 1. List of US fusion devices, type date and institution.

Concept Name	Acronym	Location	Type	Operate	Reference
Mirrors					
Table Top		LLNL	Classical mirror	1950s	[4,7]
Toy Top		LLNL	Classical mirror	1950s	[4,7]
Alice		LLNL		1960s	[3]
Baseball		LLNL	Min-B		[2,4]
Multiple Mirrors			Multiple	1977	[2]
Two ‘X’ Two ‘B’	2XIIB	LLNL	Mirror		[2,4]
Tandem Mirror		UW	Tandem		[3,7]
Tandem Mirror Experiment	TMX, TMX-U	LLNL	Tandem	1979	1979 [2,7]
Rotating Mirror			Rotating	1980	[2]
Mirror Fusion Test Facility-B	MFTF-B	LLNL	Tandem Mirror	1983	[2,7]
	TARA	MIT	Tandem	1984	[3]
Phaadrus-B		Uwisc	Tandem	1987	[3]
Mabneto-Bernoulli Experiment		Utexas	Rotating	2003	[11]
Mirror Confinement Experiment	MCX	UMO	Rotating	1999· · · >	[11]
Elmo Bumpy Torus					
Elmo Bumpy Torus	EBT	ORNL	Torodial geometry	Early 1980’s	[2,3]
	EBS	ORNL	Linear geometry		[2]
Field Reversed Configurations					
	ASTRON	LLNL	Electron beam	1973	[2,3]
Relativistic Electron Coil Experiment	RECE	Cornel	Electron beam		[3]
	FRX-B	LANL	Frc	1979	[2]
	TRX, TRX-1,2	STI (Uwash)	Frc	1980–1986	[11]
Large S Experiment	LSX	(Uwash)	Frc	1986–1991	[11]
Large Sourece Modification	FRX-C/LSM	Lanl	Frc		[2]
	HBQM	Uwashington	Frc	~1990	[3]
	CSS	Uwashington	Frc	~1990	[3]
Translation Confinement and Sustainment	TCS	Uwashington	Frc	1999· · · >	[8,11]
	Firex	Cornell	Frc		[11]
	FRX-L	LANL	Frc	2003· · · >	[11]
	PHD	Uwashington	Frc	2004· · · >	[11]
International Ring Devices and Cusps					
Octopole		San Diego	Octupole	1965	[3]
Quadrupole		San Diego	Quadrupole	1968	[3]
Quadrupole		ORNL	Quadrupole	1968	[3]
Spherator	Spherator	PPPL		1968	[3]
	SP-3	PPPL		1969	[3]
	LSP	PPPL		1970–1971	[3]
Superconducting levitron	SCL	LLNL	dipole	1971	[3]
	FM-1	PPPL	Multipole	1971–1976	[3]
Toroidal Magnetic Cusp	TORMAC		Toroidal Magnetic Cusp	1978	[2]
Levitated Octupole		UW	Octupole	1970’s	[3]
Quadrupole		LANL			[3]
SurMAC	SURMAC	UCLA	Dodecapole	1981	
	CTX	Columbia		1999	[8]
Levitated Dipole Experiment	LDX	MIT	Dipole	1999· · · >	[11]
Toroidal Pinches (and rfps)					
Perhapsatron S-4		LANL	teroidal pinch	1958	[3]
Gamma		LLNL	teroidal pinch	1958	[3]
(Scyllac Linear Experiment)	(SLX)	LANL			[3]
Scyllac full torus	SFT	LANL			
	Scyllac	LANL			
	ZT-1	LANL	rfp	1970–1974	[3]
	ZTH	LANL	rfp		
Z-Theta Pinch	ZT-40M	LANL	rfp	1981	[3]
Ohmically Heated Toroidal Experiment	OHTE	GA	rfp	1981–1988	[2,3]
Madison Symmetric Torus	MST	UW	rfp	1985· · · >	[11]

Table 1. *(Continued)*

Concept Name	Acronym	Location	Type	Operate	Reference
Tokamak variants					
Medusa		Uwisc	ST	mid 1990s	
Helicity Injected Torus	HIT and HITII	Uwash	ST	1994···>	[11]
Electric tokamak	ET	UCLA	ET		[11]
National Spherical Torus Experiment	NSTX	PPPL	ST	1999···>	[8,11]
Pegasus		Uwisc	ST	1999···>	[11]
Stellarators					
Early Stellarators A ···> C		Princeton		1950s–1960s	[3]
Hybritron		LLNL		1960s?	
Proto-Cleo		UW	Classical	1970s	[2]
Interchangable Module Stellarator	IMS	UW	Modular	1978	[8,3]
Advanced Toroidal Facility	ATF	ORNL		1980s	[8]
Compact Auburn Torsation	CAT	Auburn	Torsatron	1990s	[8]
Helically Symmetric Experiment	HSX	UW	Stellar	1996···>	[8]
National Compact Stellarator Experiment	NCSX	PPPL	Compact	2006?	[11]
Spheromaks					
Beta-II	Beta-II	LLNL	Coaxial Gun-driven	1980s	[8]
Compact Torus Experiment	CTX	LANL	Coaxial Gun-driven	1980s	[8]
Spheromak 1	S1	PPPL	Inductive	1980s	[8]
Proto-Spheromak	PS	UMO	Conical Theta-Pinch		[8]
Barkeley Compact Torus Experiment	BCTX	UCB	Coaxial Gun-driven	1990s	[8]
Swarthmore Spheromak Experiment	SSX	Swarthmore	Coaxial Gun-driven	1999···>	[8]
Bellan Spheromak	Bellan	Caltech	Planar Gun-Driven	1999···>	[8]
Sustained Spheromak Physics Experiment	SSPX	LLNL	Coaxial Gun-driven	1999···>	[8]
Steady Inductive Helicity Injection expt.	SIHI	Uwashington	Inductive	2003···>	[11]
Z-pinches and linear theta pinches					
Dense plasma focus				1965–1970	[2]
Fast Linear Pinch	FLP	LANL			
Scylla		LANL	Linear theta pinch		[3]
Imploding liners				1980	[2]
Hard-core pinches					[2]
LASL Fast Linear		LANL		1980	[2]
Flow-though Z-pinch	ZAP	Uwash	Z-pinch	1995···>	[11]
Z	Z	SNL		1997	[11]
IEC					
Farnsworth Fusor				1950s	
Hirsch IEC				1967–1968	
(See NY Academy od Science report from 1975 for work in early 1970s)		Uillinois	IEC	1973	
		PennState	IEC	1974	
Various IEC devices		Uwisc	IEC	1990s	[11]
Various devices		Uillinois	IEC	1990s	[11]
Various Devices		Daimler	IEC	1990s···>	[11]
Penning experiment – Ions	PFX-I	LANL	IEC	1994–2002	[11]
Periodically Oscillating Potential Sphere	POPS	LANL	IEC	2000···>	[11]
Laser IFE					
Cyclops		LLNL		1976	[5]
Argus		LLNL		1978	[5]
Shiva		LLNL		1979	[5]
Nike		NRL			[5]
Novette		LBNL		1983	[5]
Nova		LBNL		1985	[5]
Omega		Rochester		1996	[5]
National Ignition Facility	NIF	LLNL		1997	[5]

Table 1. *(Continued)*

Concept Name	Acronym	Location	Type	Operate	Reference
Ion/Electron Accelerators					
Aurora		Harry Diamond		1972	[2]
Proto II		SNL		1977	[2]
Scaled Final Focus Experiment					[8]
Beam Combining Experiment				1996	[8]
Particle Beam Fusion Accelerator	PBFA	SNL		1980	[8]
Single Beam Linac Experiment		LBNL		1980–1986	[8]
Multiple Beam Experiment	MBE-4	LBNL		1985–1991	[8]
High Current Experiment	HCX	LBNL		2002	[8]
Channel Focussing experiments		LBNL		2004?	[8]
STS-500	STS-500	LLNL		2004?	[8]

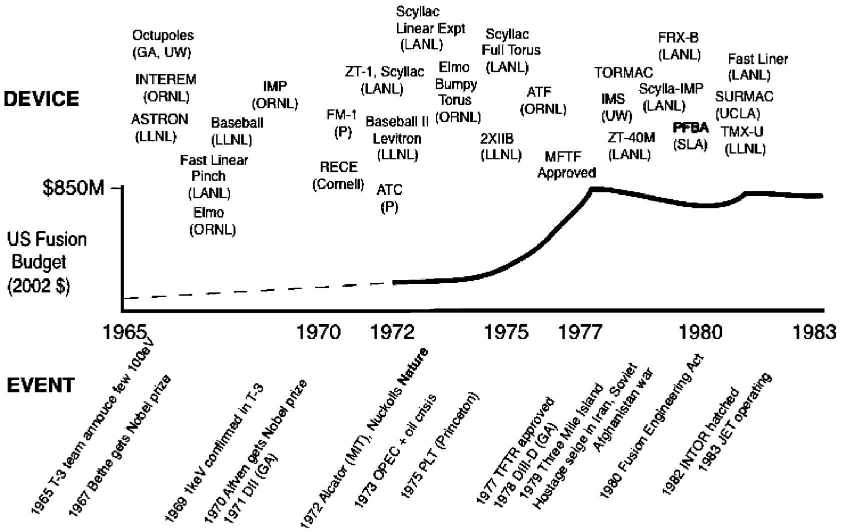

Fig. 1. During 1965 until 1983 there was a steady build-up of funding for fusion and an energy goal.

crashed and continued to fall until 1996, when the fusion energy program underwent a significant restructuring. The period from 1983 until today is shown in Figure 2. The inertial fusion route ran somewhat parallel to the magnetic energy route, although funding for this route has increased with time, and comes from the defense budget [23]. The Nuckolls Nature article [24] is the starting point of the IFE route, and since it's publication in 1972 there have been several large experiments built to look at direct drive and indirect drive with various drivers, culminating in the construction of the National Ignition Facility at LLNL. In 1990, funding for alternative magnetic fusion approaches was omitted from the budget and existing alternatives needed to remarket themselves as tokamak-relevant if they were to obtain support. By 1992, the DoE reversed it's funding decision and went about restructuring. The outcome was a prioritization of science as a principal goal as marked clearly by the OFE changing it's name to the Office of Fusion Energy *Sciences* by 1996. In 1999, a new solicitation for innovative confinement concepts was issued and many proposals were funded and new machines built (and are still being built). Amongst these devices are stellarators, spherical tokamaks, an rfp, spheromaks, frcs, a levitating dipole and a rotating mirror.

A TAXONOMY OF THE ALTERNATIVES

The 100 or so devices that have been built and investigated in the US can be categorized

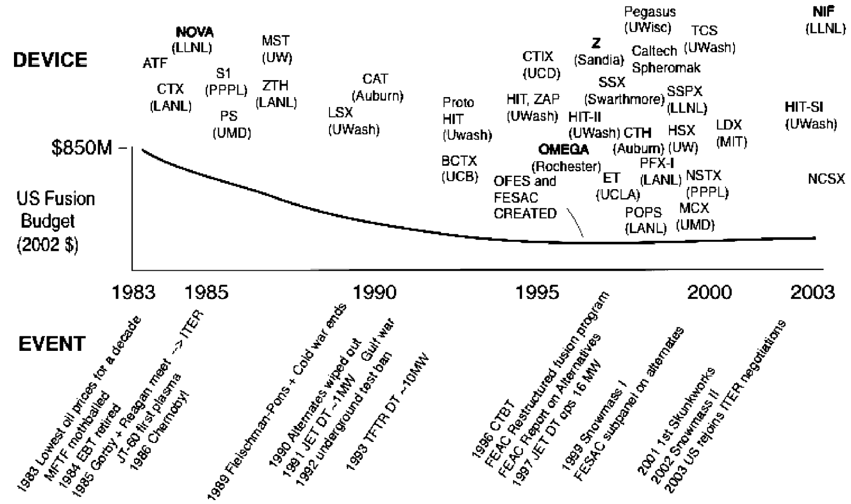

Fig. 2. During 1983 until 2003 there was a collapse in funding and a restructuring of the fusion program to be more science oriented.

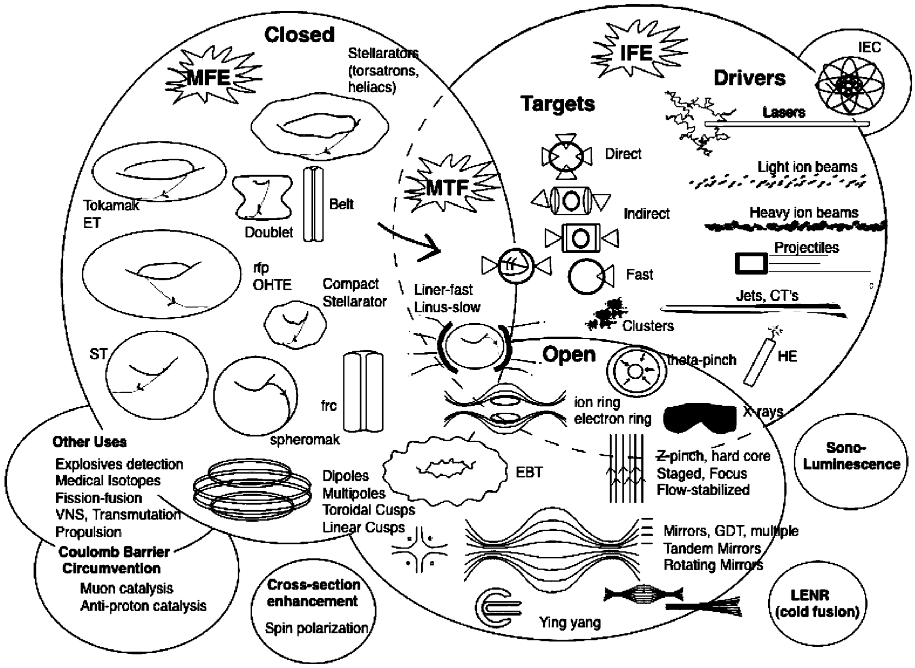

Fig. 3. Conceptual taxonomy of alternative fusion approaches.

according to whether they confine hot plasmas by inertia or with magnetic fields. Figure 3 shows a Venn-diagram representation of the primary fusion configurations (and some secondary ones). There are many ways to treat the information (e.g., Maxwellian versus non-Maxwellian, classical versus non-classical, etc.), but it is important to start *somewhere*. The magnetic concepts are divided into closed and open, and the inertial concepts are divided into drivers and targets. Note that there is in-fact much overlap between these two traditionally parallel and separate development paths: magnetized target fusion lies naturally in the overlap [25], as do X-ray indirect drivers from Z-pinches. Lying outside of both the IFE and MFE sets, but with perhaps some overlap are non-fusion-energy applications like fission–fusion hybrids, space propulsion, etc. (summarized by McCarthy *et al.* [26]).

Also ideas to circumvent the coulomb barrier like muon catalyzed fusion are shown here (as they are taken seriously as possible fusion alternatives elsewhere). Lying outside of these areas still are ideas that are not presently accepted by the scientific mainstream as valid fusion schemes, like cold fusion (which was reviewed by the DoE in 1990 [27] and after 10 years of work by the Navy labs, is about to undergo another DoE review [28]) and sono-luminescence (for which neutron measurements are presently being discussed in the literature [29]). The list of concepts that fall into the ICC category and are presently funded are shown in Table 2.

A SUMMARY OF THE CHARACTERISTICS OF NON-CONVENTIONAL ALTERNATIVES

Those concepts that don't have too many immediately available references (and were unfamiliar to my peers) are defined here as 'non-conventional'; and, those that have been turned over time and again throughout the last 30 years as alternatives to the tokamak are labeled 'conventional'. In the set of conventional alternatives are the rfp, frc, spheromak, ST, stellarator, and so on. Few summaries on non-conventional or 'skunkwork' ideas really exist. The last few years ICC proceedings contain some of the ideas [11], and many of the titles are presented in Appendix A; some of the cross-section enhancement ideas for circumventing the coulomb barrier have been summarized (spin polarization, muon-catalysis, etc [30]); and, many skunkworks have not made it past either the workshop level scientific activity or out of conference proceedings. The non-conventional alternatives, and their reason for inclusion, are shown in Table 3.

Table 2. Innovative Confinement Concepts in operation today

Device	Type	Location	Since	Status
MST	Rfp	U Wisc	1985	PoP
NSTX	ST	PPPL	1999	PoP
SSPX	Spheromak	LLNL	1999	CE
HIT-SI	Spheromak	U Wash.	2004	CE
TCS	Frc	RPPL	1996	CE
CDX-U	ST	PPPL	1993	CE
ET	Tokamak	UCLA	1999	CE
HSX	Stellarator	U Wisc	1999	CE
LDX	Dipole	MIT	1999	CE
INES-E	IEC	LANL	2001	CE
PFX-I	IEC	LANL	1999	CE
FRX-L	FRC	LANL	2003	CE
Pegasus	ST	UWisc	1998	CE
FIREX	Frc	Cornell	2001	CE
MCX	Mirror	UMD	2001	CE
Bernouli exp	Mirror	UTexas	1999	CE
ZAP	Z-pinch	UWash	1998	CE
CTIX	CT injection	Udavis	1999	Basic
CAT	Stellarator	Auburn	1992	CE
MRX	reconnection	PPPL	1995	Basic
SSX	Frc/spheromak	Swarthmore	1996	Basic
BSX	Spheromak	Caltech	1999	Basic

The main characteristics that these non-conventional alternatives possess are that (1) they can be hybrids of winning ideas; (2) they can be overlapping sub-sets (IFE-MFE); or, (3) they are ideas brought in from outside the domain of fusion science and mixed with an existing idea. Note that some of the greatest innovations combined concepts from both the IFE and MFE domains, e.g. without high energy beams (developed for open systems), tokamaks would not produce the fusion yields that they do.

EXAMPLES OF THE PHYSICS OF SOME NON-CONVENTIONAL ALTERNATIVES

This section outlines the physics of some of the less conventional paths to fusion energy, as an inroad to concept innovation. Included in the talk, but not included here for brevity were outlines of fission–fusion (hybrid of energy concepts [31]), impact fusion with jets (hybrid of MFE-IFE concepts [32]), the kinetically stabilized tandem mirror (a twist to an old idea [33]), and sonoluminescence (neutron observations are presently being discussed in the literature). Other concepts that were considered but neither made it into the talk nor will be discussed at length here include: cold fusion; cross-section enhancement; and stellarator-spheromak hybrid [34].

Table 3. Some non-conventional tokamak alternatives and their reason for inclusion in the talk

Concept	Reason for Inclusion
Elmo Bumpy Torus	Hybrid of two winning concepts of the time
K-S Mirror	An innovation of an old idea
Laser-cluster-B fusion	Hybridization of MFE and IFE ideas
Impact Fusion	Unconventional drivers for ICF
μCF	Focus on physics normally outside the domain of hot fusion
Fission–Fusion Hybrid	Hybrid of *energy concepts*
Fusion in a bubble?	Currently controversial subject m playing out in the journals.

The examples are given here in a format that would be considered a valid skunkwork. Traditionally, the fusion skunkworks: (a) offer novel twist(s) on conventional fusion physics and/or fusion technology, and (b) suggest the prospect for either reducing the size/cost/complexity of the fusion core and/or a novel application for fusion energy, and (c) cannot be classified conveniently under the presently established divisions or research programs, and (d) do not obviously violate the laws of physics. Usually the skunkwork presentations are no more that 20 minutes in length, but ought to contain several important aspects: (1) a figure of the concept with labels to all important components; (2) a brief physical description of the operation of the device; (3) a description of the main physics and either analytic expressions showing how the Lawson condition will be met, or by reference to a simple code calculation; (4) some rationale why this concept would be an improvement over existing concepts (less expensive, less complex, etc); and finally (5) a brief discussion of the most significant unknowns. Two or three references to guide the listener would be useful.

Elmo Bumpy Torus (EBT)

One could view the EBT as a machine that tackled mirror end losses by arranging many segments into a torus, and as such was a hybrid of the two leading concepts of the day, and of open and closed systems (mirror and tokamak). Figure 4 shows the cross-section at the midplane and a detail of the magnetic field structure [2]. ECRH experiments in mirrors produced relativistic e-rings at the midplane which dug a magnetic well thereby enhancing stability, and so the EBT used RF heating between the magnetic coils to obtain peak core electron temperatures of around 1 keV [35]. In the early 1980s, a Proof of Principal experiment was proposed but not built. As a magnetic fusion device, the aim of this concept was to obtain sufficiently large plasma beta with a sufficiently long confinement time to satisfy the Lawson condition. However, neither beta nor confinement time were sufficient to consider promoting the concept: magnetic wells were not dug sufficiently deep to reverse the local field gradient and so the system was not sufficiently stable and confinement was correspondingly poor.

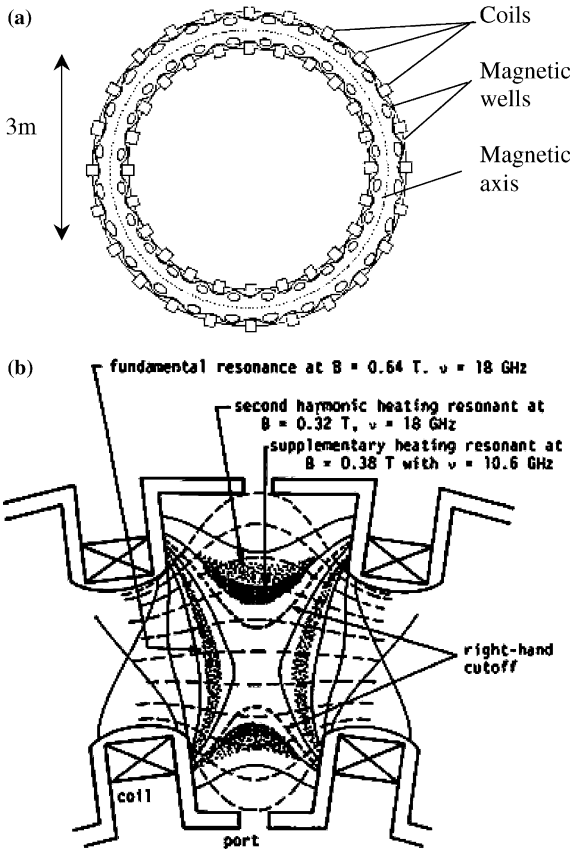

Fig. 4. (a) cross-section through the midplane of the Elmo Bumpy Torus showing magnetic wells between coils; (b) close-up of the field lines in the magnetic wells showing location of RF heating.

Impact Fusion

The aim of 'hypervelocity impact fusion' is to accelerate ~1 g projectile to > 200 km/s into an anvil, to give Q > 1, and was considered in various guises, presented in an annual workshop until the late 1970s [36]. This concept lies in the IFE domain. One example is to make circular accelerator ring with a magnetized superconducting projectile to produce required velocities [37]. Balancing radial forces acting on the projectile one obtains a relation for the major radius of the accelerator, R (Figure 5):

$$R = (\rho_0 v^2/B^2)r.$$

Rings of radius ~2 km would be needed for this case, and become larger if materials are not superconducting. The circumference could be narrowed further by choosing a different target (if for example the target was not solid fuel, but rather a pre-formed frc or spheromak) [38]. As an inertial confinement scheme, impact fusion aims to obtain high densities, but correspondingly shorter confinement times to reach the Lawson parameter.

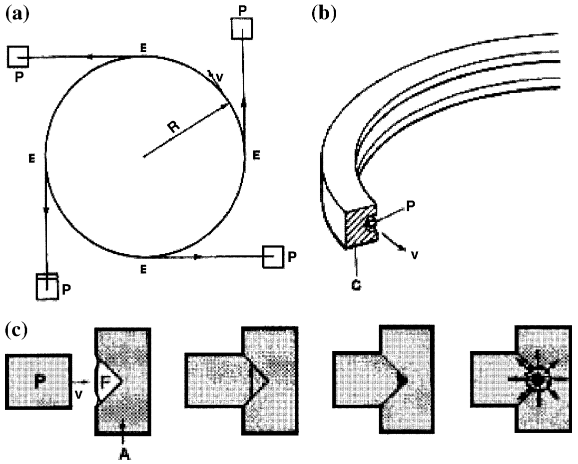

Fig. 5. Illustration of macroparticle impact fusion: (a) circular accelerator ring with target points (P) along exit trajectories (E); (b) cross-section of superconducting channel (C) showing macropartcle (P); and, (c) particle impact on fuel-filled (F) anvil (A).

Fusion from Irradiated Clusters

The aim here is for laser light to be absorbed by clusters of deuterated dust, giving ejection of hot electrons and fast ions. This concept is a novel twist of IFE in that the driver is a laser, and embraces MFE, in that the reactions could be prolonged by use of a confining magnetic field. It is also a non-thermal scheme, and so is related to IEC and some beam fusion schemes. In this concept, ions become accelerated and smash into surrounding clusters with enough energy to produce measurable neutron flux [39]. The neutron yield is proportional to the cluster disassociation time, and so an increase of disassociation time can be obtained by placing target in magnetic mirror field (Figure 6). One suggestion is to make tiny mirrors (size governed by the laser-induced-filament footprint) to trap ionized clusters, and issues related to the stability of the resulting magnetic configuration are discussed [40].

Muon-Catalyzed Fusion, μCF

This method is a departure from 'hot fusion', and as such is an idea that lies outside the domain of 'hot' fusion science. With μCF, 130 dt-fusions occur according to the chain (Figure 7):

$$\mu^- + (d,t) \rightarrow \mathrm{d}t\,\mu \rightarrow 4He + n + \mu^-.$$

The limit on recyling is due to 'μ-sticking' whereby a μ will combine with an alpha-particle and form a helium atom which then leaves the system. However, much attention in present μCF research is paid to unsticking the muon, and it is thought that break-even would require only 300 dt-fusions [41]. Research is being undertaken at several major facilities RIKEN, TRIUMF, etc., and the subject has been recently reviewed by Ishida [42].

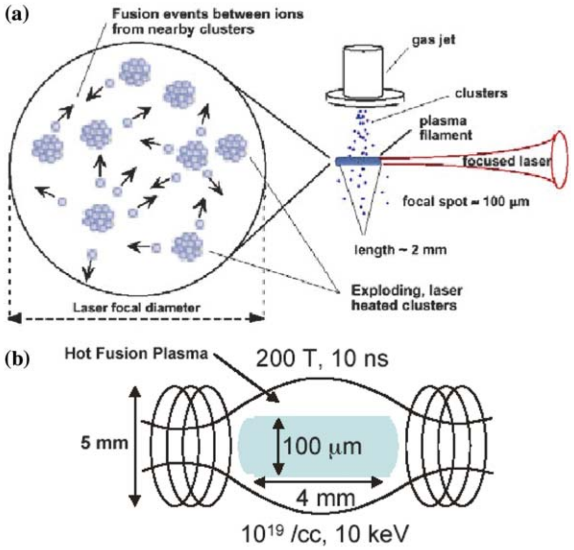

Fig. 6. Fusion from impacting deuterated clusters: (a) small clusters are accelerated under the influence of a laser and impact each other; and (b) a means for reducing the rate at which the particles are lost would be to confine them using a small mirror.

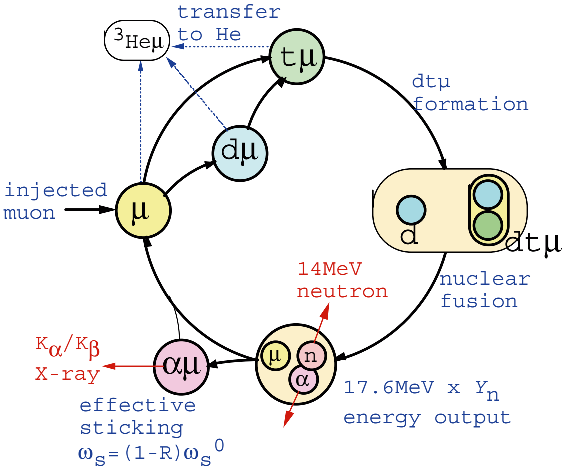

Fig. 7. Life-cycle of the muon in muon-catalyzed fusion (μ-CF).

Role of Innovation in Fusion Science

According to Webster's English Dictionary, innovation is: 1. *The introduction of something new;*

or 2. *A new idea, method or device*. Given the set of all primary configurations, two approaches are available: (1) run through all of the permutations and combinations of concepts to find a new one (e.g. secondary configurations like the stellarator-spheromak); or (2) bring in something new from outside this set. So, which to choose? It is certainly easier to run through the permutations until something new is encountered than to bring in something fundamentally new. It is less risky too: we know the physics of these ideas already and can talk with confidence about the physics issues as they are firmly founded. More risky is to look outside of the set of established ideas to new physics. Brilliant innovations have come about this way: neutral beams, for example, were developed to meet a particular need, which seemed essential at the time (necessity is the mother of invention). Such innovation is considered important, particularly by those who advocate a restructuring of the fusion program [43], perhaps in the pursuit of innovative solutions [44]. However, in actual fact there is a third approach to innovation: most of us are working on some aspect of a greater whole, and so where we choose to innovate is largely up to us, subject to constraints (see below).

DISCUSSION

It is disconcerting to not fully know the past. Without this knowledge we condemn ourselves to the fate of Sisyphus (to repeat it in perpetuity). The history of the US fusion program is taught in cursory forms at all major research institutions and the text-books available cover most of the main devices. However, in an ideal world the fusioneer would have at his fingertips: (1) a complete compendium of all devices and concepts investigated everywhere; and (2) the specific reasons and detailed physics of each device and concept. This sort of information was requested by younger scientists after the ICC talk, and is obviously beyond the scope of the talk and this paper. What may be called for would be a frank summary from the PI of each major device outlining the critical physics issues, how they were being addressed, and perhaps what would be done now to address those issues. To compile a list of concepts might be tall order. Lamentably, it was over twenty years ago that a text-book with such a wide scope was written. A new edition would be timely: in the next 5–10 years most of the brilliant scientists who founded this field will be retiring, and the knowledge that they possess may well be lost for good [45]). A compendium of world-wide fusion activities is performed regularly by Nuclear Fusion (e.g., [46]), although this does not include explanations for the cessation of the devices, and certainly does not outline concepts that have not been built. International meetings may be informative about present activities [47], but it is also critical to obtain the history. It is not enough to think that younger scientists will obtain that information during lunch hours with their senior colleagues: this is an information age! Historical information ought to be accessible through a web browser anywhere for free and as new concepts come to light they ought to be easily added. The next iteration of this text ought to include every concept investigated in the US, and also throughout the world. This would be a large undertaking, but it would be worthwhile. Perhaps as an inroad to concept innovation, younger scientists could be encouraged to make a skunk-work of a previous idea (not necessarily their own), and a prize offered for the most convincing. Efforts along these lines would make the young fusioneer more knowledgeable, for sure.

Fusion science is maturing, with most of the foundational physics written into a number of quality textbooks, and with computing power sufficient for models to be operated in parallel to the experiments. Historically, theory has trailed experiment, but now fusion science is entering a predictive phase: capable codes can now accommodate most experimental results, and are being used to predict experimental operating regimes; and so we are well equipped to address the dominant scientific issues that each device presents, but what are these? It was suggested that the device sets could be put into context of larger physics sets, such as astrophysics, fluid dynamics, etc. A *scientific* taxonomy is worthwhile as it gives a clear idea of which domains of science are necessary for innovation in the domain of fusion science, and so a draft is shown in Figure 8. Central to fusion science are the core issues abstracted from the FESAC Priorities Panel list of 15 important questions [48] (which incidentally, are not too dissimilar from the National Academy of Science questions from a few years ago). Outside those issues lie domains of science that overlap with (or comprise) fusion science (e.g. astrophysics, materials science, nuclear physics, fluid mechanics etc). Once a fusion scheme is in operation we will need

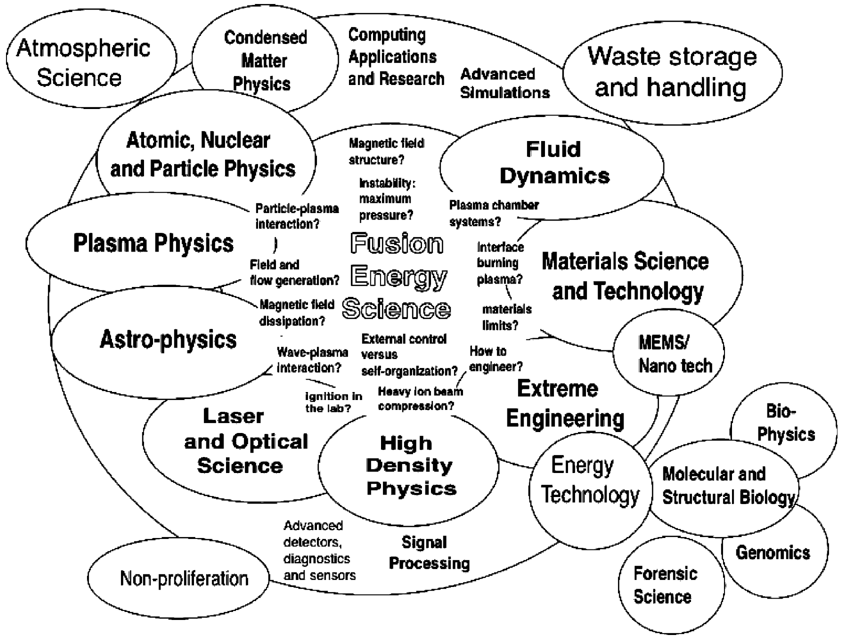

Fig. 8. Scientific taxonomy for nuclear fusion: core questions are from the FESAC Priorities Committee 2004.

to start thinking about proliferation issues and waste-storage and handling. Further, inspiration and methodology can come from fields that traditionally have no overlap with our subject, so forensic science is included as an example. A healthy fusion program ought therefore embrace all of these scientific disciplines, and talking to people involved in those fields can only strengthen our understanding of innovations taking place there.

What will make a skunk work? One example is the toroidal pinch proposed in a patent by Thomson and Blackman in 1946, where details of the device were given, and arguments presented to suggest that fusion might be practical in the lab [49]. This work was obviously premature, but it perhaps captured the essence of a good skunkwork. Many other skunkworks can be found in the appendix and online [11]. At the meeting, it was suggested that to first order, the idea must meet the Lawson condition, and remain stable long enough to get a sufficient gain (either pulsed or steady-state), calculated with an existing code adapted to this end. It was also suggested that departures from this requirement can be obtained if we are to think in terms of circumventing the physics of the Lawson condition (e.g., with barrier penetration schemes). The stability criterion can be circumvented also as it is possible to imagine intrinsically unstable high Q concepts (e.g., the dipole).

Without scientific details of why each and every alternative configuration has not advanced further than it has done, there is a first order effect that will guarantee the concept failure: make it cost more than is affordable. For example, a development path for which spending increases by an order of magnitude every few years will guarantee failure for the concept (at least in today's economic climate). Success perhaps ought to be measured in terms of a Lawson parameter per development dollar $(n\tau T/\text{R\&D\$})$ [50]. There is one more constraint that ought to be applied to our concept if our aim is the *commercial deployment of fusion power stations*, which was discussed by Jim Loman of GE: the fusion scheme must fit both into the R&D horizon and budget of a large energy company. Thankfully, GE's R&D horizons have been extended and budgets increased by their new CEO such that fusion may be of interest to them [51]. This point was well made by Loman in a talk in the same session, where he showed the R&D timelines for wind, solar and innovative fusion schemes on the same figure [52], perhaps as a teaser for a visionary to step forward to tell GE what they should build. It may be too early still for a down-select, but it is worth defining when that moment might actually arrive (afterall, many of the alternatives have already surpassed the confinement properties of T-3, and oil prices have never been higher). Perhaps we can be smarter this time around. Perkins made the point that the fusion reactors must compete economically with fission reactors but that does not necessarily imply that a fusion reactor core must compete with fission. One can consider that the economics is a function of size, cost, complexity, development path, waste disposal, safety, proliferation, and fuel cycle. The overall function for fusion must be seen to be comparable to fission otherwise no utility exec will be interested: a desirable device would therefore be considerably smaller, simpler and less costly to build than any of the present MFE or IFE reactor visions [53]. Wootton and Perkins argue that compactness is advantageous, with ICF perhaps having some advantages over MFE [54]. The attractiveness of simplicity was echoed by Loman in his personal down-select to simply connected devices (mirror, frc, spheromak).

The idea of a regular fusion skunkworks is open to question. Various prominent scientists have all voiced concerns that there is no need for this sort of activity, given that an alternative fusion program already exists. It is true that innovation takes many guises: daily we innovate to find solutions to problems with technology, diagnostics, code-development, etc. This innovation meets a critical need within the existing program to address issues that are already funded, and so *concept* innovation ought only be a recreational activity. However, a skunkwork could act as stimulus for internal R&D funding, or could stimulate other programmatic ends (which have precedents at LLNL). Furthermore, in order to make concept innovations one needs to maintain an expert command of an enormous array of physics, technology and historical issues: a seasoned skunkwork contributor will be a very competent scientist.

SUMMARY

The history of the US fusion program was summarized: an oil-price-driven down-select in the late seventies lead to an up-tick in funds which then declined when oil prices fell; after which the program restructured to address *science*. An overview of all primary tokamak alternatives was given as a taxonomy, demarcating between conventional alternatives (e.g., spheromak, stellarator, frc, rfp, etc.) and non-conventional (e.g., EBT, impact, muon-catalyzed fusion, etc.). There is a rich history of innovation in fusion science: innovation occurs either by hybridization of concepts in the set of all fusion concepts, or by bringing in something new from the broader physics sets that overlap with the fusion physics set. Defining the content of all of these sets will be a substantial undertaking, but a necessary one if we are to imagine a concept that could produce fusion power economically in the near term.

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Appendix A. Skunkwork contributions from the last 3 years

Business Needs Regarding Fusion: Lessons Learned from Alternative Energy J. Loman

Axisymmetric Mirror Stabilized by the Shaped End Wall D. Ryutov

Fusion-Fission-Fusion Fast Ignition Plasma Focus F. Winterberg

Helical Drive for RFPs and Stellarators R. Nebel

Thermoelectric Rotating Torus for Fusion A. Hassam

p-11B Fusion Reactor Concept Using Direct Energy Conversion of Energetic Alpha Particles with Negligible Neutron Production A. Wong

Pulsed Magnetic Mirror Confinement of Deuterium Fusion Plasmas Produced by Laser Irradiation of Deuterated Clusters T. Ditmire

Helium Catalyzed DD Fusion in a Levitated Dipole J. Kesner

Fast Ignition in the Focus of a Double (High V, Low I - Low V, High I) Magnetically Insulated Transmission Line F. Winterberg

Using Finely Layered Ablators for Stabilizing the Ablation Front Instability in an Indirect-Drive IFE Target D. D. Ryutov

The Kinetic Stabilizer Tandem Mirror: Analyses and New Options R. Post

Self Organization of an Inverse Pinch for Magnetized Target Fusion B. Bauer

Strongly Flowing Compact Objects for Magnetic Fusion Confinement L. Steinhauer

Proliferation-Proof Fusion Power J. F. Santarius

RMF Driven FRC Fusion Reactor J. Slough

STEADY-STATE, OBLATE, D -3 He FRC FUSION FOR TERRESTRIAL POWER AND SPACE PROPULSION * M.J. Schaffer

Plasma Magnetic Confinement: The PLASMAK Koloc P.M.

Fusion via stagnation of self-excited counter-streaming toroidal flows in a highly rippled tokamak P. Bellan

The Kinetic Stabilizer/Tandem Mirror: Some Issues and Opportunities R. F. Post

Alfvenically Confined Plasmas A. B. Hassam

Muon Catalyzed Fusion Revisited R. M. Kulsrud

Plasma Heat Engine for High Q Aneutronic Fusion? D. C. Barnes

Impact Fusion Revisited J. H. Hammer

Modular Systems as a Route to Economical Fusion Power R. A. Nebel

Natures Constraints on Fusion: Where Might There Be Some Wiggle Room (Thermonuclear and Non-Thermonuclear)? L. J. Perkins

Carbon Free Energy Self Sufficiency by 2050? Possibly Via the Thorium/233 U Fission Fusion Hybrid; W. Manheimer

Reexamining the possibility of laser based IFE using a $\sim 1\mu$m driver L. Suter

Fast Ignition and Heavy-Ion Indirect Drive B. G. Logan

RMS Repetitive Merging of Spheromaks: A Steady-State Fusion Reactor with Pulsed Heating, Fueling, and Current Drives R.F. Bourque

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