Critical Need for Disruption Prediction, Avoidance, and Mitigation in Tokamaks

S.A. Sabbagh[1], N. Commaux[2], N. Eidietis[3], S.P. Gerhardt[4], B. Granetz[5], J.M. Hanson[1], V. Izzo[6], E. Kolemen[4], R. La Haye[3], J.E. Menard[4], R. Raman[7], M. Walker[3], S. Woodruff[8]
[1]Columbia. U., [2]ORNL, [3]General Atomics, [4]PPPL, [5]MIT, [6]UCSD, [7]U. Washington, [8]Woodruff Scientific, Inc.

Overview:

The near-complete elimination of damaging plasma disruptions in fusion-producing tokamaks, including ITER and FNSF, is the present "grand challenge" in tokamak stability research. Meeting this significant goal will require multiple approaches, use various physics and engineering disciplines, and is best approached by a national research effort aimed toward the major US tokamaks, while leveraging international collaboration aimed toward the unique aspects of international devices. Experiment and theory will need to work in concert to reach this goal. As the plasma evolves, including dynamics caused by transient phenomena or changes in operational state (e.g. confinement transitions, formation of localized internal barriers, dominant alpha heating), stability should be predicted theoretically and measured experimentally. As the plasma evolves toward less stable states, actuators can be used to change plasma characteristics and avoid instability consistent with high fusion power output. When profile control does not avoid instability growth, active mode control systems can be used to maintain safe levels of mode amplitude as the plasma is evolved back toward a stable state. When a disruption is unavoidable, mitigation systems can be triggered to shut down the plasma without causing excessive device damage. A 10 year national research effort is envisioned that would focus on solving the combined challenges of disruption prediction, avoidance, and mitigation (abbreviated as PAM) in tokamaks (both large thermal collapses and current quenches) to support continuous operation of these devices.

Importance to FESAC Strategic Plan:

Tokamak disruption PAM is such a high priority topic in magnetic fusion at present that its importance is usually implied. To summarize for the purposes of the committee, disruption PAM is a high priority in the first element of the DOE Fusion Energy Sciences (FES) strategic plan: "*Advance the fundamental science of magnetically confined plasmas to develop the predictive capability needed for a sustainable the fusion energy source*". Naturally, it therefore strongly addresses the present DOE-FES mission. Disruption PAM is a broad topic, and so it's not surprising to find that it pervades 3 of 5 themes of the Magnetic Fusion Energy Sciences Research Needs Workshop Report (2009). It is also presently one of the few high priority issues for ITER. Finally, it serves the FES Strategic Planning charge in the three Burning Plasma Science Elements: (i) Foundations, (ii) Long-Pulse, and (iii) High Power. These are stated in more detail on slide 9 of Dr. E. Synakowski's presentation of April 9th, 2014 to FESAC entitled "The charge for advice on strategic planning". Regarding "Foundations" - disruption PAM is presently the most important element of today's research on tokamak macroscopic stability. Regarding "Long-Pulse" – disruption PAM is a requirement for long-pulse tokamak operation, since disruptions in tokamak are typically defined by the full and rapid termination of the plasma, during which the plasma current goes to zero. Additionally, disruptions have also been characterized to mean a major, rapid decrease in the plasma stored energy ("minor disruption", or "major beta collapse" are other terms used). These are typically in the range of 50 – 80% decrease in plasma stored energy and are caused by macroscopic magnetohydrodynamic (MHD) instabilities. The combination of high heat loads and electromagnetic forces that occur during disruptions emphasize why disruption PAM research provides critical support for the "High Power" element of the strategic plan – this research directly supports the significant reduced and mitigation of disruptions in ITER. In a large tokamak fusion device such as ITER, loss of stored energy of this magnitude in the timescale of milliseconds can put important elements of such devices at risk.

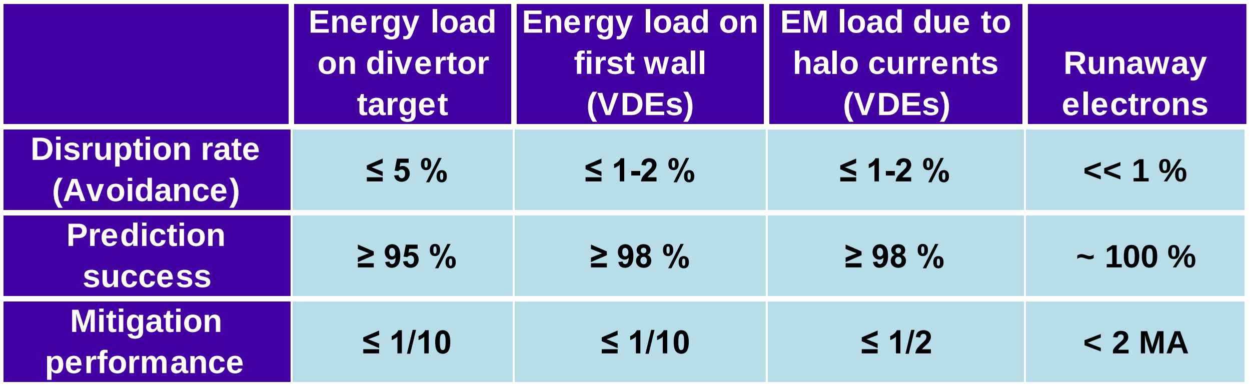
A National Initiative for Disruption Elimination:

This paper proposes a "National Initiative for Disruption Elimination". As advised in the guidance provided by FESAC, we have considered this initiative in the suggested four-parameter space: (1) potential for increased U.S. leadership, (2) time scale, (3) size scale, and (4) spectrum of priorities / science drivers. We first provide a focus for such an initiative, then outline the strategic plan for it - considering each of the four topics above which we hope the committee will find convenient.

a) Focus of a "National Initiative for Disruption Elimination"

This initiative is naturally focused – the elimination of potentially damaging disruptions in a tokamak through a multi-facted approach comprised of disruption prediction, avoidance, and mitigation using a high benefit/cost approach, with quantifiable figures of merit to mark progress toward goals (most broadly stated as disruption probabilities) in present tokamak devices.

ITER personnel have defined requirements for disruption prediction and avoidance success, and disruption mitigation in ITER. Figure 1 briefly summarizes these requirements:



	Energy load on divertor target	Energy load on first wall (VDEs)	EM load due to halo currents (VDEs)	Runaway electrons
Disruption rate (Avoidance)	≤ 5 %	≤ 1-2 %	≤ 1-2 %	<< 1 %
Prediction success	≥ 95 %	≥ 98 %	≥ 98 %	~ 100 %
Mitigation performance	≤ 1/10	≤ 1/10	≤ 1/2	< 2 MA

Figure 1: Summary of disruption PAM requirements for ITER (from M. Lehnen, et al. ITPA-MHD Topical Group Meeting, Culham 2013 © 2013 ITER Organization)

Note that in this table, "VDEs" is an acronym for "vertical displacement events", typically used to characterize plasma instability in the vertical direction. The target values in this table are challenging, but experience in present tokamaks hold promise that they can be achieved. In fact, they can be used as target values for the presently proposed disruption PAM initiative. Disruption statistics from the past operation of tokamaks are mostly not relevant to compare to these goals, since most of today's tokamaks operate with little concern of damage due to disruptions. Therefore, care is generally not taken to avoid disruptions. This fact itself demonstrates the need for a new, focused initiative on disruption PAM, which would include dedicated run time of present U.S. tokamaks to generate and publish such statistics. Some dedicated experiments in DIII-D and NSTX have given statistics on disruption avoidance, including DIII-D operation over a limited operation space showing no disruptions, and NSTX operation at very high stability parameters stating a dramatic decrease in disruptivity through improved control techniques [1,2]. The most attention placed on avoiding disruptions in a large tokamak facility to date comes from the JET device in Culham, UK. Much of this attention was first motivated by the need to prepare for the "ITER-like wall" implementation in JET while still operating with a carbon wall, and is more recently motivated by the actual operation with this wall. JET publications have shown that a low-level of plasma disruptivity in a major tokamak facility is possible. Plasma disruptivity was reduced below 4% in JET operation with a carbon wall [3]. This admirable statistic included all JET operational regimes. This operation also included a disruption avoidance system, but one that has not fully leveraged the understanding of the approach to macroscopic MHD stability boundaries discovered in magnetic fusion research in the past several years. More difficult disruption PAM requirements will need to be reached for a DEMO device based on the tokamak, or for a Fusion Nuclear Science Facility which will have more demanding criteria for continuous operation (measured in weeks). The key question is how can disruption PAM be further improved to bridge from about 4% disruptivity down to 1 to 2% disruptivity and below? The answer is to *exploit more of the available opportunities and actions to avoid and mitigate disruptions*. The presently proposed initiative will take the key critical steps toward defining and demonstrating tokamak operation at low disruptivity in an organized fashion, exploiting more opportunities and combining stabilization techniques, utilizing what we have learned in MHD stability research, control theory, research, and development, and disruption mitigation over many years. One can envision this initiative as the first step to transforming this more general plasma physics stability research into an applied science program aimed at a focused goal. This step is necessary, and it should happen now.

b) The strategic plan for a "National Initiative for Disruption Elimination" from the standpoint of the suggested four-parameter space

We now consider a strategic plan for a "National Initiative for Disruption Elimination" from the standpoint of the suggested four-parameter space of: (1) potential for increased U.S. leadership, (2) time scale, (3) size scale, and (4) spectrum of priorities / science drivers, illustrating how such an initiative can *significantly leverage several advantages* of the present U.S. magnetic fusion research program to yield a very high benefit/cost ratio. Once these more general aspects are considered, the balance of the document will consider part of the spectrum of priorities / science drivers for each of the prediction, avoidance, and mitigation elements.

A "National Initiative for Disruption Elimination" can exploit and highly leverage the present U.S. investment and key strengths in tokamak plasma stability, control, and disruption mitigation research. It is a natural and logical next-step to utilize and expand upon U.S. successes in these areas, with an emphasis in synergizing the elements. This has happened to a limited degree in past research, mostly due to a relative lack of understanding in all three areas. However, several advances in these areas in the past decade strongly call for their coupling in present U.S tokamaks. A relatively modest investment would aim to add focused, incremental support to transform our present US research programs in these areas into a new, focused effort aimed toward near 100% disruption PAM success using quantifiable figures of merit. Strong opportunities to maximize benefit/cost ratio are available, as the initiative would leverage present U.S. facilities with relatively minor, yet important facility upgrades which can elevate the U.S. research effort on disruption PAM to the level of being world-leader in this area. The major U.S. tokamak facilities have demonstrated and enjoyed a high level of university collaboration for years. There is therefore a high level of confidence that university professors, researchers, post-docs, and students will participate as long as university research principal investigators play both a research and management role, and are engaged early in the process so that they can establish programs in the required disciplines. Note that DOE FES has been soliciting ideas for greater university involvement in the larger laboratory facilities, and the disruption PAM initiative is an excellent opportunity to help serve this request. Expansion of the relatively modest funding of international collaborations on tokamak research is another high-leverage opportunity for DOE. Two important examples in the context of the present initiative are leveraging the JET tokamak capabilities (addressing the "High Power" element of the DOE FES Strategic Plan, and the KSTAR high beta superconducting tokamak capabilities (addressing the "Long Pulse" element of the DOE FES Strategic Plan, especially targeting long-pulse tokamak operation above the ideal MHD "no-wall" stability limit). International collaboration is especially logical and advantageous for two key reasons: (i) international collaboration is often stated by DOE FES as an opportunity for the U.S. magnetic fusion program, rating highly as either a strategic or tactical element of DOE FES planning, and (ii) it is significantly underfunded for the potential benefits that are available. This can be appreciated quantitatively by examining the re-solicitation of MFE international research funding that occurred in 2012. The total available funding in that solicitation for tokamak research was $6M, which was reduced to $4M at the time that awards were made due to budget uncertainty at that time. This represents a very small fraction (~ 1% level) of the U.S. magnetic fusion budget devoted to tokamak research, regardless of whether or not U.S. ITER funding is counted in the total.

The elements of the "four-parameter space" are now considered separately:

(1) Potential for increased U.S. leadership: (answer: VERY HIGH)

An honest assessment of the U.S. magnetic fusion program relative to the European program should state that the U.S. has lost world leadership in several areas compared to 2 decades ago. FESAC Committee members can objectively appreciate this statement by simply recounting the answers of the presenters of both the JET and EUROfusion roadmap presentations at the July 2014 meeting made in reply to the following question that the committee asked: "What can the U.S. do to help the JET / EUROfusion programs going forward?" The answer was sobering (paraphrasing): "The U.S. can do some modeling." We can do better than that, but we need to take action and exploit the correct niches that exist in the world magnetic fusion effort. One such nice is a focused program on disruption PAM.

The relatively modest incremental investment proposed for this initiative would position the U.S. as world-leader in disruption PAM. This strong statement is easily understood by briefly examining the strong U.S. assets that this initiative would build upon, and international assets that could be leveraged via collaboration. In summary:

<u>Validated physics understanding for disruption prediction</u>:
- The U.S. is already considered the world-leader in several aspects of the stability (e.g. neoclassical tearing mode (NTM) theory, kink/ballooning/resistive wall mode theory (RWM), with experimental validation) and transport physics (e.g. neoclassical toroidal viscosity (NTV), with experimental validation) related to this element. There are significant efforts world-wide in NTM stabilization physics, so an additional element is needed to bring the U.S. into a clear position of world-leadership. The physics research components exist, and in some cases comprise a program that is applied to experiments. The important, missing niche to be filled is

the explicit focus on disruption prediction *for* avoidance, and implementation of avoidance mechanisms to demonstrate quantifiable success.

Control research and development:
- The U.S. also has strength in instability control – e.g. NTM, RWM – which is branching out into new genres such as control of toroidal Alfvén eigenmodes (TAE), and is becoming stronger in transport-related plasma profile control. Applied plasma profile control is at an early stage in magnetic fusion research throughout the world. This provides a clear opportunity for the U.S. to exploit, but action must be taken quickly. The key niche here for the U.S. is to immediately focus the many new efforts in plasma profile and mode control toward a disruption avoidance goal. These control elements are unlike the disruption prediction physics, because the control elements *require* an application. In addition, the disruption prediction physics elements are *best used* when they directly guide the control research and feedback system implementations from the start. A key goal of a focused disruption avoidance research program should be the implementation of model-based disruption prediction in real-time whenever possible, to be directly used by the control system. The combined integration and implementation of the disruption prediction physics and control elements is the large niche that the U.S. can quickly fill in the world program to become world-leader in this area. If such a focused integration does not happen, the U.S. will give up a leadership role, and will have to follow such a focused effort that will almost certain form abroad within the next 5 years.

Mitigation physics and R&D:
- The U.S. is presently responsible for *building* the disruption mitigation system (DMS) for ITER. The ITER organization is responsible for the *specification* of the DMS. One can argue who has the true leadership role in this effort, but more importantly the final design review of the DMS is scheduled for the year 2017. While DMS construction will clearly continue past 2017, the design of the system should be locked in by then. However, any fusion scientist knowledgeable in this area will agree that the *research* to improve DMS for future tokamaks is not nearly complete. The present research efforts to improve massive gas injection, shattered pellet, and other systems are still highly active, and several new ideas are being proposed that cannot be tested on a timescale that would allow them to be part of the ITER DMS. Therefore, U.S. leadership in this area will only come from a combination of the present U.S. involvement in ITER, *plus* a focused, parallel research effort in U.S. and other world tokamaks, as funding allows. The "National Initiative for Disruption Elimination" suggested here is the perfect venue to attain this leadership position. It's also highly important because such an effort would directly integrate DMS as part of a unified disruption PAM system in the tokamaks that would be part of this initiative.

(2) Time scale: (answer: starting NOW, spanning ~ 10 years)

Nearly all initiatives that are brought before this committee will say that they need to be started right away. This is true of a "National Initiative for Disruption Elimination" as well. The critical need for such an initiative based on the knowledge gaps defined by the DOE ReNeW study, and other studies was stated above. Waiting to address this critical need in an efficient and focused manner simply delays progress.

The aspect that separates the disruption PAM initiative from many other initiatives relates to the figure of merit discussed under Item (1) above – U.S. leadership. Immediate action by the DOE un this regard will allow the exploitation of solving a key gap in magnetic fusion research that will place the U.S. in the lead in the area of disruption PAM.

U.S. leadership on such a fast timescale in the area of disruption PAM is possible for several key reasons:

(i) The key research elements of such a program mostly exist – they simply need to be supplemented and have a well-defined focus. This will also improve motivation, which further serves the purpose of efficiency.

(ii) The key hardware elements of such a program also exist, and again simply need to be supplemented. The major U.S. tokamaks and certain international devices including JET and KSTAR have unique capabilities for disruption PAM and should be exploited in this regard.

(iii) Researchers (both in theoretical and experimental research) in the areas of disruption prediction, avoidance, and mitigation *also* exist, but their efforts are disjoint simply due to the historical evolution of these efforts. Experts in prediction are mainly plasma physicists, while experts in avoidance mainly work in control theory, which to date has had only a small interaction with magnetic fusion, and experts in mitigation have relatively little interaction with either of these other experts. There is a huge niche here for the DOE to bring these elements together and better organize them.

The committee may find it difficult to suggest ways in which the already highly utilized research efforts could find additional time to create such an organization. There are in fact many ways to allow the key research leaders time for this. For example, at the moment DOE requests “Joint Research Targets” (JRT) to form yearly between the three major U.S. tokamak facilities. Each year, the topic of these efforts change, and a considerable amount of manpower is put into these efforts. But because of the change of topic each year, and because these efforts sometimes parallel existing topical science efforts on each device, the progress made is not greater than the separate efforts. The lack of continuity (the efforts end in one year by definition) also tends to strongly decrease the effectiveness of these efforts. So, one quick and easy suggestion is for DOE to stop the mandates, which would allow the manpower put toward such efforts to maintain focus on a multi-year effort such a disruption PAM initiative. Note that eliminating the JRTs is just one time-saving opportunity, and the manpower going into other such organizational efforts could be re-focused into a disruption PAM (and other) initiatives.

(3) Size scale: (answer: LEVERAGES PRESENT U.S. and INTERNATIONAL DEVICES)

An U.S. initiative on disruption PAM again has a major advantage in efficiency and benefit/cost because it can immediately leverage existing U.S. and international tokamak devices. Key elements of the devices is that they are large enough to have sufficiently low plasma collisionality (normalized collisionality parameter $\nu^*_{i,e} < 1$) and elements that support the specific areas of disruption PAM to investigated, such as mitigating high first wall heat loads, or continuous operation at sufficiently high stability parameters (e.g. normalized beta greater than the n = 1 MHD ideal “no-wall” limit). A relatively small subset of toakamaks whouch would include all major U.S. devices, plus JET (for “High Power”) and KSTAR (for “Long-Pulse, High Beta”) as mentioned above would be a sufficient subset.

Finally, all of the these devices already have some hardware upgrade plans (mostly on incremental budgets) that support the disruption PAM effort. However, as we know, the term “on incremental budget” typically translates to mean “will not happen”. The DOE through a disruption PAM initiative should ensure that the relatively modest budget requests for hardware to fully support a disruption PAM initiative actually happen. However, this should not happen in a piecemeal fashion device-to-device, because funding all of the requested upgrades would indeed require a significant increase in budget. Instead, the upgrades should be chosen based on the needs of the disruption PAM initiative, which would involve discussions between leaders of the disruption PAM initiative, the individual facility managers, and DOE.

(4) Spectrum of priorities/science drivers for a “National Initiative for Disruption Elimination”

This section briefly summarizes the science drivers related to a disruption PAM initiative. It is not meant to be exhaustive. Instead, the purpose is to summarize some specific research and further ideas in these disciplines aimed to improve disruption PAM, showing that an ample set of elements is available to form a disruption PAM initiative. A full prioritization of elements in each area is more appropriately handled in a full proposal.

(1) <u>Prediction / detection</u>: Stability research spanning two decades has yielded substantial understanding in instability thresholds for the most dangerous modes causing disruptions (e.g. locked NTMs, RWMs). A new paradigm of understanding, validated by dedicated experiments, has emerged to explain RWM marginal stability in tokamaks through kinetic stabilization effects [4,5,6,7]. Relatively recent understanding and experimental results more favorably extrapolate to future devices. For example, initial RWM models showed that the RWM would become much less stable at lower plasma collisionality. However, more recent work has shown that reduced collisionality can actually yield *greater* stability (Figure 2a) [8]. This illustrates the critical importance of understanding the stabilization physics, as different models can scale completely differently to future devices. Real-time physics-based evaluation of stability criteria can be expanded by greater understanding, as well as exploitation of improving parallel computation technology (e.g. ideal MHD analysis such as DCON). Simplified evaluation of complex models, such as kinetic MHD, will allow greater capability in determining marginal stability conditions for equilibrium profiles (e.g. safety factor, pressure, plasma rotation) through dedicated experiments and model validation. Further developments range from non-linear MHD codes with synthetic diagnostics to large data-driven statistical predictions. Advanced real-time detection using physics models of global instability response (e.g. from resistive wall modes) has been built into state-space control models and needs continued development [2]. Database studies have been conducted to determine the detectability of disruptions based on multiple-input criteria (such as the low frequency $n = 1$ RWM amplitude, neutron emission compared to computations from a rapidly-evaluated slowing-down model, ohmic current drive power compared to simple current drive expectations, and plasma vertical motion) [9]. When the disruption warning is declared for an aggregate point total of 5 points, the percentage of disruptions detected with at least 10 ms warning is very high (99.1%), but the rate of false positives is also high (14.2%). Increasing the threshold on the aggregate point total to 10 results in a disruption detection warning percentage of 96.3%, but significantly reduces the false positive percentage to 2.8%. It should be emphasized that these very positive results were achieved from a database analysis, and now needs to be used to create such statistics in the major U.S. tokamaks, and as part of our international collaborations. It is also very important to realize that this system does not yet fully exploit key disruption detection measurements and models, so the disruptivities given here might

be further improved by combining further inputs. MHD spectroscopy of applied 3D tracer fields to measure global mode stability has been implemented in real-time (Figure 3) [10], but to date has been barely used due to the lack of a focused application to disruption PAM. It now must be proved effective in practical use through experimentation and iterative refinement. Non-magnetic mode detection diagnostics are desirable and should be applied routinely. Developments in these areas will further improve input to tokamak disruption warning system algorithms.

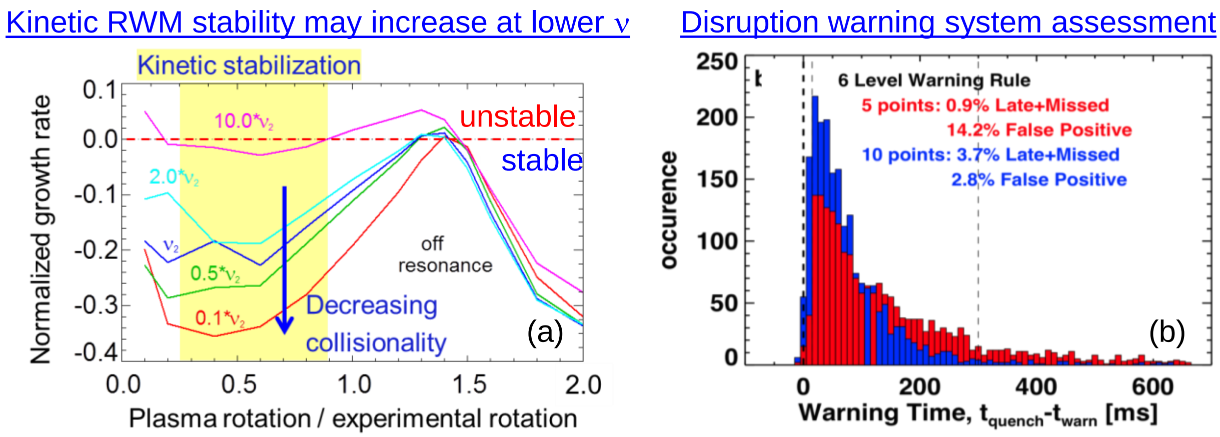


Figure 2: (a) Kinetic RWM stability analysis showing that the RWM can be stabilized at low plasma collisionality, (b) database analysis of a disruption warning system based on multiple predictor variables shows disruption prediction with high success rate and low false positive count.

(2) Avoidance: Active mode control techniques can be used once mode onset is detected and transport-timescale avoidance techniques are too slow. Physics-based real-time algorithms, sensors, and actuators have shown significant successes for RWM and NTM control. These systems must be generalized to further improve performance and be proven to work over long-pulse. Evolution toward non-magnetic elements in these systems is also important. Advanced mode control algorithms are successfully being implemented but need significant further development and demonstration of utility over a wider operational space of the tokamak. This is presently an active area of research for both NTMs and RWMs, but the step to focused, quantified disruption PAM still needs to be generally made. NTM control by electron cyclotron current drive [11] has recently reached new levels of general application, including stabilization of a wider spectrum of mode helicity and mode control in high beta plasmas exceeding the $n = 1$ ideal “no-wall” beta limit [12,13]. Such advanced control is shown in Figure 4a for an NTM with poloidal/toroidal mode numbers of 3/2.

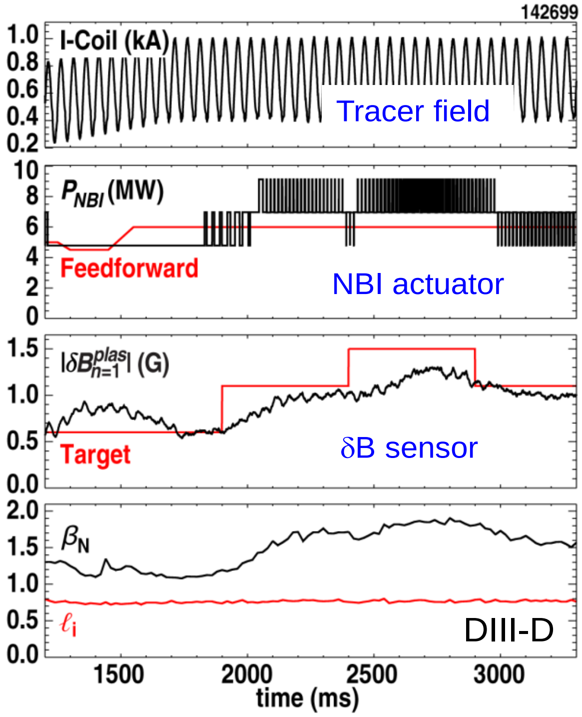


Figure 3: Real-time low frequency MHD spectroscopy used as a disruption predictor in a closed-loop feedback system to control injected power for disruption avoidance.

A physics model-based, RWM state-space control system with real-time modeled sensor output has been demonstrated to sustain long pulse, high β_N discharges with n = 1 fields applied that normally disrupt the plasma (Figure 4b) [2]. This controller was used for RWM stabilization in long-pulse plasmas (limited by coil heating constraints) reaching $\beta_N = 6.4$, and near maximum $\beta_N / l_i = 13.4$, which is twice the value of the ideal n = 1 "no-wall" stability limit and at the highest stability performance parameters of the device (l_i is the plasma internal inductance).

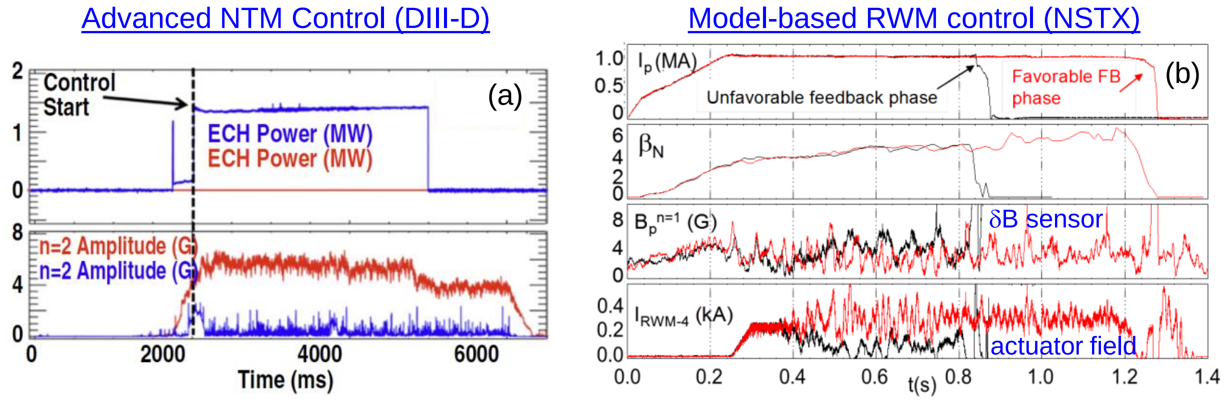


Figure 4: (a) NTM control of a 3/2 mode by application of electron cyclotron current drive, and (b) advanced model-based control of the RWM at high normalized beta.

While active model control tools are reaching a stage of maturity to significantly decrease disruptivity, control of equilibrium profiles remains a generally untapped, major opportunity to avoid unstable conditions, adding an entirely *new layer of protection* against disruptions. Neutral beam injection (NBI), 3D fields, and core fueling techniques are some examples of actuators. Advanced plasma profile control techniques are only now starting to be implemented and proven in tokamaks, but the idea is not new - 25 years ago profile control was being used in simulations to demonstrate access to advanced tokamak operational regimes [14]. Two examples in the large U.S. tokamaks include feedback control of the plasma rotation profile, to be demonstrated for the first time using NTV in closed-loop (Figure 5a), and magnetic profile control to provide control of the highly important safety factor, *q*, profile (Figure 5b) [15].

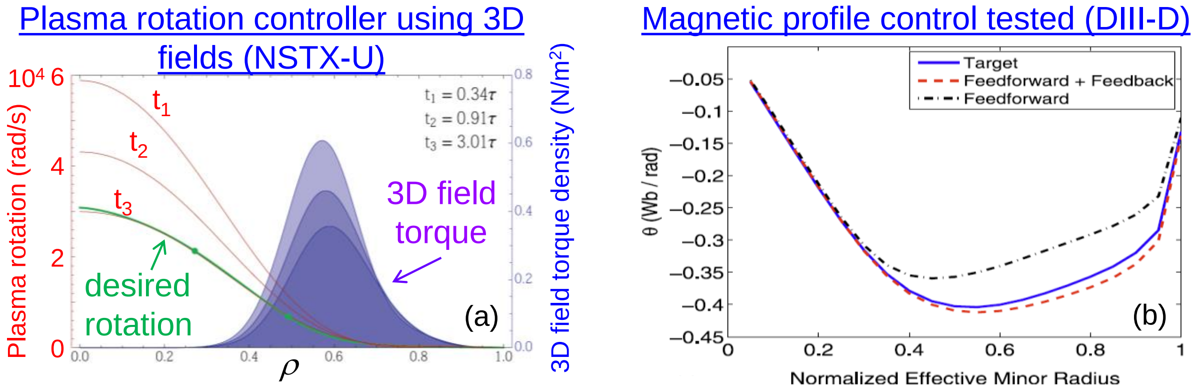


Figure 5: Advanced plasma profile control techniques are starting to be implemented in tokamaks: (a) closed-loop feedback control of the plasma rotation profile by NTV, and (b) magnetic profile control, to provide control of the highly important safety factor profile.

A significant concern for future tokamaks is retaining the general stabilizing effect of plasma rotation in conditions where the usual actuator to provide the required momentum input – neutral beam injection – will not be sufficiently effective, or not be available at all to drive plasma rotation. However, other momentum injection techniques are available, but have not been properly researched yet mainly due to the availability on NBI. One example technique – compact torus injection – can provide significant momentum input (2 mg deuterium compact tori injected at 20 Hz provide the same momentum input as 69 MW of NBI at 500 keV), as well as required core fueling in future tokamaks. This technique needs to be proven effective now in tokamaks to provide these needed capabilities for future tokamaks yielding significant fusion power. Further detail can be found in the FESAC white paper by R. Raman, et al. [16].

Another untapped opportunities for disruption avoidance include the large range of physics models and MHD spectroscopy used in real-time for detection can also provide real-time guidance on stability gradients in operational space, that can be used to steer away from instability by providing input to plasma profile control actuators. These opportunities have been overlooked to date largely because the key actuators have not been available, but this can now change. If predictors indicate that instability is unavoidable, a controlled shut down should be initiated. Additionally, theoretical plasma simulators can be developed to test these algorithms to make faster progress. Off-normal event response algorithms need to intelligently prioritize multiple actuators, and this integration step needs to be demonstrated in a disruption PAM initiative.

(3) <u>Mitigation</u>: In the small percentage of shots where disruption cannot be avoided, a fast discharge termination method is needed to minimize damaging effects including large heat loads from thermal quenches, asymmetric halo currents, and runaway electrons. A leading candidate actuator for disruption mitigation is massive gas injection (MGI), but not all issues with its use have been resolved. For example, gas penetration is not fast enough to mitigate the fastest disruptions, and accelerated penetration typically requires MHD activity, which makes control of the process less certain. Additionally, the optimal radiation pattern is symmetric, however significant toroidal asymmetry of the radiation, which can cause first wall melting, is observed in present experiments (Figure 6). Therefore, further research using MGI is required, including additional aspects and understanding of gas penetration efficiency (e.g. dependence on poloidal location, including the X-point region) and spatial distribution of heat and radiation loads, with related theoretical modeling. Outstanding issues remain (e.g. for ITER) regarding suppression or control of runaway electron generation, and vessel forces associated with halo current asymmetry/rotation.

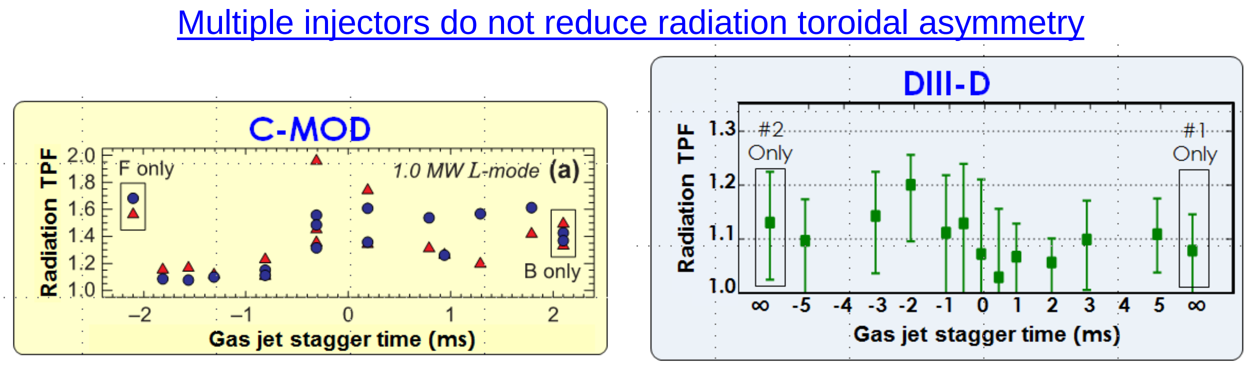


Figure 6: Radiation toroidal peaking factor for different combinations of multiple gas injectors placed at different toroidal positions in the Alcator C-Mod and DIII-D tokamaks.

Alternative actuators are being proposed/developed to deliver more material, more quickly than what has been presently delivered by MGI. Shattered pellet injection (SPI) (Figure 7) [17,18] should be further developed if MGI proves inadequate. New ideas include fast impurity injection by electromagnetic means (Figure 7) [19], and sacrificial limiters, perhaps using low-Z liquid metals. All techniques should allow rapid recovery of high plasma performance after use. Control of the decaying plasma and runaway electron population needs further development to produce a controlled shutdown. Halo current diagnosis in present devices needs to be significantly improved to measure and understand the dynamics, toroidal asymmetries, and related forces.

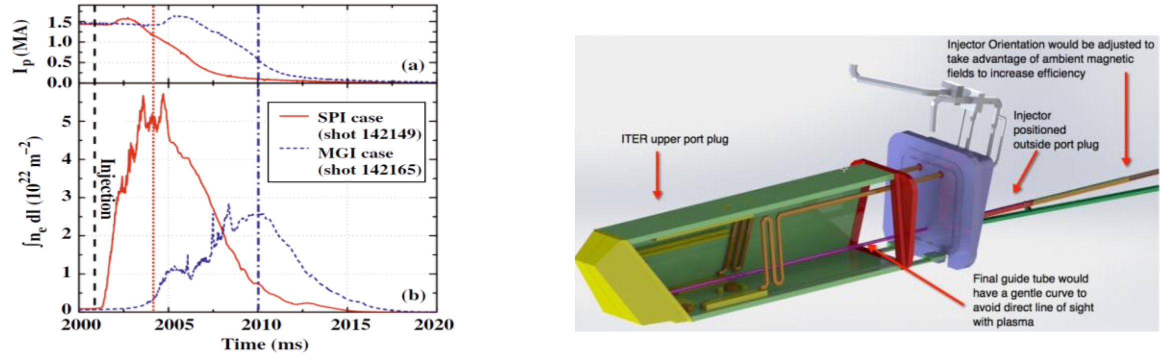

Shattered Pellet Injector results (DIII-D)   Electromagnetic Particle Injector in ITER (schematic)

Figure 7: Alternative actuators for disruption mitigation (at different stages of development): (left) shattered pellet injection, and (right) electromagnetic particle injector (proposed).

Estimated cost of a "National Initiative for Disruption Elimination"

A full and proper estimate of the cost of a disruption PAM initiative that would fill the key gaps as defined by the ReNeW report, and similar reports, and that would reach quantifiable low levels of disruptivity through a focused combination of efforts requires a greater effort than that devoted to a white paper. However, a rough estimate can be made.

As mentioned earlier, the disruption PAM initiative can take advantage of huge cost savings based on use and/or redirection of existing facilities, experimental run time, and manpower.

These elements, which could be considered "zero cost", are:
a) use of existing tokamaks
b) some redirection of experimental run time (NOTE: much of this run time is already assigned to stability, transport, and control efforts – it would just be re-focused on disruption PAM)
c) redirection of manpower (NOTE: This is easier than it sounds, because stability and control experts, and related diagnosticians, would most likely gladly re-focus attention to a new initiative on disruption PAM – most of the existing work already strives toward such an effort, but in a piecemeal fashion)

Elements of the disruption PAM initiative that would incur additional costs are:
a) Increase in manpower working on disruption PAM
b) Increase in U.S. DOE funding aimed toward research on international facilities
c) Procurement of upgrades to U.S. tokamaks to serve the disruption PAM role

The co-authors of this document have not generated a formal budget that would support all of the science drivers defined above. Still, it is reasonable to consider costs based on (i) hardware estimates of U.S. tokamak device upgrades defined through facility 5 year plans and similar documents, (ii) a manpower increase that would step up the U.S. effort to support research enabling “world leadership” status, and (iii) a significant percentage increase in international funding to enable U.S. “world leadership” status in disruption PAM. This exercise was performed assuming a 50% increase in the (rough) estimate of FTEs going into disruption PAM efforts at major facilities and their collaborators, a 50% increase in the present international funding of such efforts on tokamaks, and selected hardware upgrades of the U.S. tokamaks. This exercise yields an estimated increased cost of between $5M - $7.5M/year to fund a national disruption PAM initiative. This includes an estimated $3M/year cost of major facility hardware upgrades assuming a 10 year time frame for the initiative.

---

[1] E.J. Strait,” Establishing the basis for sustained tokamak fusion through stability control and disruption avoidance” (FESAC Strategic Planning Meeting Gaithersburg, MD July 8-10, 2014)

[2] S. A. Sabbagh, et al., Nucl. Fusion **53** (2013) 104007

[3] P.C. de Vries, et al., Nucl. Fusion **51** (2011) 053018

[4] B. Hu, and R. Betti, Phys. Rev. Lett. **93** (2004) 105002

[5] S. A. Sabbagh, et al., Nucl. Fusion **50** (2010) 025020

[6] J.W. Berkery, et al., Phys. Rev. Lett. **104** (2010) 035003

[7] H. Reimerdes et al., Phys. Rev. Lett. **106** (2011) 215002

[8] J.W. Berkery, et al., PRL **106** (2011) 075004

[9] S.P. Gerhardt, et al., Nucl.Fusion **53** (2013) 043020

[10] J.M. Hanson, et al., Nucl. Fusion **52** (2012) 013003

[11] C.C. Petty, R.J. La Haye, T.C. Luce, D.A. Humphreys, et al., Nucl. Fusion **44** (2004) 243

[12] M. Okabayashi, I.N Bogatu, M.S. Chance, et al., Nucl. Fusion 49 (2009) 1

[13] E. Kolemen, et al., Nucl. Fusion **54** (2014) 073020

[14] S.A. Sabbagh, et al., Nucl. Fusion **29** (1989) 423

[15] J.E. Barton, et al., Nucl. Fusion **52** (2012) 123018

[16] R. Raman, et al., “Simplifying the ST and AT Concepts” (FESAC Strategic Planning Meeting Gaithersburg, MD July 8-10, 2014)

[17] N. Commaux, et al., Nucl Fusion **50** (2010) 112001

[18] N. Commaux, et al., Nucl Fusion **51** (2011) 103001

[19] R. Raman, et al., “Fast Time-Response Electromagnetic Disruption Mitigation System” (European Physical Society Conference, Berlin, Germany June 23-27, 2014, paper P5.015)