Science Board Sub-Group - Dark Matter Strategy
Final Report

Executive summary

The Science Board Dark Matter Sub-Group (SG) proposes a coordinated strategy for supporting direct dark matter search (DDMS) experiments that could potentially position the UK for leadership in this area. The SG recommends that the funding model for this area should evolve to match more closely that used in other parts of the STFC programme.

In the short term (no later than end 2012) the SG has recommended provision of limited bridging support for detector R&D preparing for the next generation of tonne-scale experiments. In the medium term (late 2012 to early 2014) the SG recommends awarding design-phase support at the currently envisaged funding level, based on a re-focusing of the existing PPRP proposal to better match STFC strategic goals and position UK groups for scientific and technical leadership. In the long term (early 2014 onwards) the SG recommends capital-phase support for construction over a three-year period of no more than two tonne-scale experiments based on different nuclear targets. Should more than ~£6M be available major participation in and significant leadership of at least one, possibly two, major world-leading experiments is feasible. With between ~£3M and ~£6M significant leadership in no more than one such experiment is feasible. With a budget of less than ~£3M significant leadership as a result of strategic investment is unlikely to be possible. In this case the SG recommends investment at a lower level in no more than two experiments using different targets, with support focused on areas of unique UK expertise with the potential for significant visibility and key contributions.

Background

1. The SG was constituted to develop a coordinated strategy for supporting DDMS experiments that could potentially position the UK for leadership in this area. The SG consists of Dan Tovey (Sheffield, Chair), Val Gibson (Cambridge), Andrew Jaffe (Imperial), Christian Spiering (DESY, international expert).
2. Weakly Interacting Massive Particles (WIMPs), dark matter candidates detectable by DDMS experiments, are the leading contenders to explain myriad astronomical and cosmological observations. Astrophysical observations of motions within galaxies and of galaxies themselves cannot be explained by Einstein gravity sourced by luminous matter. Cosmological observations coupled with the theory of Big-Bang nucleosynthesis imply that any such dark component must be non-baryonic. Particle theory applied to the early Universe further shows that particles with a weak-interaction cross section would leave appropriate relic densities to provide the needed contribution.
3. However, none of these observations and theoretical ideas are iron-clad. Within minimal extensions to the standard model, the axion is a possible DM candidate that would not be detected by DDMS experiments. There are also scenarios in which a baryonic dark matter candidate remains viable. Finally, it is possible that Einstein gravity fails in the so-called low-acceleration regime and is replaced by a theory like Bekenstein's TeVeS, a covariant realization of the Modified Newtonian Dynamics idea. WIMP dark matter nevertheless remains the most likely scenario and will be severely tested by the coming generation of experiments.

4. In DDMS experiments signal events consist of low energy nuclear recoils arising from elastic scattering of dark matter particles. The sensitivity of these experiments is usually quoted in terms of the spin-independent nucleon scattering cross-section. Many theoretical models (for instance supersymmetry) predict cross-sections ≥ 10^{-10} – 10^{-11} pb. The choice of target material and read-out technology is driven by a number of considerations:

   - The nuclear scattering cross-section is expected to scale with A^2 for nuclear mass A due to coherence effects, rendering the use of heavy target nuclei beneficial.
   - A steeply falling spectrum imposes a requirement of extremely low nuclear recoil energy thresholds (<10 keV), in contrast to many other classes of particle physics / astrophysics experiment.
   - Significant background arises from electron recoils generated by Compton scattering of naturally occurring gamma radiation. This can be reduced by using target materials of high radiopurity and by surrounding experiments with heavy shielding. In order to remove residual background it is crucial that target materials possess intrinsic nuclear/electron recoil discrimination properties.

   A lower-level yet potentially irreducible background arises from elastic scattering of neutrons in the detector environment — a copious source of such neutrons being spallation by cosmic rays. For this reason all experiments must be operated at underground sites. In addition hydrogenous shielding can be used to moderate and absorb background neutrons, while large target volumes enabling identification of multiple scattering events can provide a level of active rejection.

5. The current generation of world-leading experiments possess target masses in the 10's to 100's of kg range. The next generation of experiments, anticipated to commence construction in or soon after 2014, will possess tonne-scale target masses and give sensitivity to spin-independent scattering cross-sections ~ 10^{-10} – 10^{-11} pb, covering much of the parameter space predicted by theoretical models. These experiments will represent an important milestone for the field. Above this level of sensitivity it is likely that potentially irreducible nuclear recoil backgrounds from elastic neutron interactions and elastic solar neutrino scattering (itself of great interest for neutrino and solar physics) will become increasingly important and difficult to control.

6. Should a potential dark matter signal be observed in a single-target experiment it will be vital to seek signals in experiments using alternative targets in order to verify the expected A^2 dependence of the nuclear cross-section. Only by using more than one target material can neutron and neutrino backgrounds be definitively eliminated as the source of the potential signal in non-directional detectors. For this reason a global focus on more than one target material / technology is key.

7. LHC results have already significantly constrained the strongly-interacting sectors of theoretically-attractive new physics scenarios such as supersymmetry (SUSY). Further significant improvements in sensitivity are anticipated as the collision energy and luminosity (data-taking rate) increase over the next years. Although these results can indirectly constrain particle models of dark matter in specific theoretical frameworks, direct model-independent constraints are relatively weak. In this respect the results of DDMS experiments probing directly the properties of the dark matter candidate and LHC experiments seeking the heavier and/or strongly coupled states are complementary. A signal observed in

either class of experiment provides exceptionally strong motivation for further studies of the nature of the new physics signal using both approaches.

8. Directional detectors utilising gaseous targets can provide the only irrefutable evidence for a galactic origin for any potential dark matter signal. The low density of such targets provides limited mass compared with noble liquid and cryogenic experiments and consequently they are likely not best-suited to initial observation of a signal. Nevertheless, should a potential signal be observed using alternative targets, construction of large-scale directional experiments will be of the highest priority for both particle physics and astrophysics (dark matter halo studies).

Current state of the field

9. The UK was one of the early leaders in this field, commencing with NaI targets at the Boulby Mine in the early 90's (UK Dark Matter Collaboration) and progressing to liquid Xenon targets at Boulby (ZEPLIN-I/II/III), cryogenic scintillators at Gran Sasso (CRESST) and directional tracking detectors at Boulby (DRIFT).

10. Currently UK physicists participate in the following experiments: Darkside (liquid Ar – R&D); DEAP/CLEAN (liquid Ar – R&D); DRIFT (CS2 / CF4 gaseous tracking – running + R&D); EDELWEISS (cryogenic Ge - running) leading to EURECA (cryogenic Ge – R&D); LUX350 (liquid Xe – commissioning) leading to LUX-ZEPLIN (liquid Xe – R&D). These experiments are, or will be, located at the following underground facilities: Boulby Mine, UK (DRIFT); SNOLAB, Canada (DEAP/CLEAN); Frejus, France (EDELWEISS, EURECA); Homestake SURF, USA (LUX/LUX-ZEPLIN); Gran Sasso, Italy (Darkside).

11. It is notable that the UK DDMS community has grown significantly in the past five years, with 13 academics now employed at 8 institutes[1], including 5 new academic appointments and 3 new participating institutes. It is also important to note that with only one exception UK academics in this area now work within particle physics groups applying for Consolidated Grants via the PPGP. In the past a significant proportion of the field worked in astrophysics.

12. Members of EDELWEISS/EURECA, LUX/LUX-ZEPLIN and DEAP/CLEAN signed the recent community bid to PPRP[2]. The bid requested financial support for exploitation and R&D for EDELWEISS/EURECA and LUX/LUX-ZEPLIN.

13. Internationally, the race towards tonne-scale experiments will pass an important milestone in late 2013 / early 2014. In the US DoE and NSF have jointly issued a call for 'Generation 2' (tonne-scale) experiment proposals with a deadline of October 2013. Submissions are expected from LUX-ZEPLIN and DEAP/CLEAN in addition to other projects. In the EU the TDR for EURECA is expected to be submitted to funding agencies in late 2013.

14. The capital-phase cost of these experiments will be around £20-30M. Optimum UK capital-phase contributions (equipment and staff) are anticipated by the proponents, on the basis of authorship fraction, to be of order £6M, to be spent over 3-4 years.

15. Significant world-leading expertise has been gained by the UK from development and operation of experiments over the past twenty years. In particular the UK possesses unique capabilities in the following areas:

[1] Edinburgh, Imperial, Oxford, RAL, Royal Holloway, Sheffield, Sussex, UCL
[2] "UK Involvement in Direct Dark Matter Searches", STFC Reference: ST/J003875/1

- DAQ systems
- Background modelling and shielding design
- Cryostat engineering, especially electron-beam welding
- Low background measurements at Boulby Mine
- Cryogenic cabling
- High voltage distribution
- Analysis software development
- Directional detectors

This expertise has been recognised in recent years by the admission of UK participants into major international collaborations without significant capital or common-fund contributions.

Responses to community consultation

16. The SG oversaw a community consultation[3] on the matter of DDMS experiments, assisted by the STFC office. We received 28 individual and group responses (including from many of the proponents). We particularly note the strong response of the theory community. SB has access to the full results, and we summarise here the responses.

17. The overall response was overwhelmingly positive, with the vast majority of respondents calling continued UK participation “very important”, and strongly supported continued UK involvement in the field, noting the community’s history of technical and scientific innovation. Most respondents would like to see wide participation in a number of experiments, citing the scaling with target mass and the need for complementary technologies for a robust detection. However, they acknowledged that in a climate of limited funding, leadership in a smaller number (perhaps only one) would be preferable to small roles in multiple experiments.

Findings and general recommendations

18. It is clear to the SG from material provided by the office and from discussions with international leaders in the field that the UK undoubtedly possesses unique expertise and has the potential to secure significant leadership within the relatively large international collaborations currently forming. The SG believes that the optimum route to securing such leadership lies with taking on responsibility for key aspects of the design and construction of experiments at an early stage, maximally exploiting existing expertise. The SG understands that the timescale for such work is strongly constrained by the requirements of funding agencies in both the US and elsewhere in the EU. It is therefore critical that funding for such work should begin to flow with minimum further delay. Without this the UK will be unable to contribute effectively and build leadership for the future.

19. Given the current state of the field the strategy should enable three distinct phases of project funding:

    - Short-term: limited bridging support for R&D activities covering the period during which STFC formulates its strategy and works with the community to form a mutually acceptable proposal for subsequent work. For the future health of the field this period should be as short as possible. Following submission of a bridging request by the SG in mid-May (see Annex B) we

[3] http://www.stfc.ac.uk/About+STFC/39043.aspx

understand that STFC awarded a total of £110k to the proponents of the PPRP proposal covering the period to 31st Dec 2012.

- Medium-term: Design-phase support for development and design work contributing to the next generation of world-leading tonne-scale experiments and building UK capability and leadership for the long-term.
- Long-term: Capital-phase support for construction of the next generation of world-leading tonne-scale experiment(s).

20. Given the international context it would seem appropriate to consider a future capital-phase project starting in early-mid 2014, contingent on the timescales for funding decisions in the US and elsewhere in the EU. Medium-term design-phase funding will therefore likely be required only until this point. Given the short timescale, should SB decide that this field is worthy of further support it is vital that design-phase project funds (including dedicated effort) be made available as soon as possible thereafter. We believe that the case for such support could be made with primarily just a change in emphasis of the existing PPRP proposal without complete resubmission. We hope that this would expedite the process of considering such a proposal and hence enable the period of bridging to be minimized and hence additional new money funds for dedicated effort to be allocated quickly. This would enable UK engagement and influence to be maintained and avoid a critical loss of momentum. Following discussions with the Office we anticipate conservatively a maximum bridging period of 8 months to end Dec 2012 (see above), however it should be emphasized that it is extremely desirable indeed that 'medium-term' support be allocated sooner than this date, should this be feasible.
21. The SG believes that this field should move to a funding model similar to that applied to other STFC projects. The SG recommends that Academic FEC for all experiments in this field be considered for support on group Consolidated Grants, primarily considered by the PPGP, in competition without prejudice with requests for Academic FEC for other experiments. The SG believes it to be important for the future health of this field in the UK that a strong base of Academics be supported in this area and hence recommend that such considerations should not be limited just to those Academics who requested FEC on the PPRP proposal.
22. The SG also recommends that funds for exploitation of DDMS experiments, including dedicated RA effort, should in future be awarded via Consolidated Grants, with project funds reserved for R&D, design and construction.
23. The SG recommends that RAL PPD continue to support this area in cases where staff possess unique expertise of value to the UK DDMS community and consistent with the agreed SB strategy.
24. The UK has significant international leadership in the area of directional detectors. Although such experiments do not currently compete in terms of spin-independent scattering cross-section sensitivity, they provide the only means of definitively identifying a galactic dark matter signal. For this reason the SG recommends that STFC considers without prejudice requests for focused R&D support in this area via the PRD line.
25. Although not directly related to the question of support for DDMS experiments, the SG notes that the UK possesses a particularly strong community of theorists working on dark matter models and phenomenology, including implications of galactic dark matter halo models for direct detection. The work provides invaluable support for the worldwide field and should not be over-looked in the relevant theory grants rounds. The SG recommends that more direct

collaboration with the theory community in the planning and data-analysis phases of the experiments should be encouraged.

Short-term (bridging) and medium-term (design-phase) recommendations (to 2014)

26. Short and medium-term funding should be targeted at development work preparing for long-term capital-phase funding request(s) to be made in late 2013 / early 2014.

27. In the context of the above general strategy the SG examined the proposal submitted to the PPRP by members of the EDELWEISS/EURECA, LUX/LZ and DEAP/CLEAN collaborations. It was clear to the SG that this proposal demonstrates a strong science case for UK participation in the field and largely makes a strong case for funding. The SG therefore believes that a case for support for the medium-term design phase of future experiments could be based upon this proposal with only relatively few modifications. The SG's opinion differs from that of the proponents and from the recommendations of the PPRP primarily on the question of priorities for support. The SG views the highest priority to be the design, development and construction of future experiments, which should provide a strong foundation for future UK leadership in the field (see above). The SG therefore recommends that the requested project funds, including project staff effort, be refocused towards this goal.

28. The technology areas outlined in the PPRP proposal in which the UK has demonstrated internationally leading R&D and design expertise are:

    - Cryogenic cabling for EURECA
    - DAQ systems for EURECA and LUX-ZEPLIN
    - High voltage distribution for LUX-ZEPLIN
    - Background modelling and suppression for EURECA and LUX-ZEPLIN
    - Cryostat engineering for LUX-ZEPLIN

    Significant UK contributions to any/all of these areas would be of great benefit to the experiments internationally.

29. The SG recommended support for travel and equipment related to the above areas in its short-term bridging request (see Annex B).

30. The SG recommends that STFC and the community develop urgently a concrete plan for provision of staff effort, travel and equipment in the medium-term. This should be focused on the above priority topics and be considered as a light-touch refocusing, rather than complete resubmission, of the existing PPRP proposal. Under the assumption that the current budget envelope (understood to be ~£230k/year) remains available the principle required change will be a shift of emphasis of project posts from exploitation to development.

31. The SG notes that through recent academic hires the UK has obtained a significant role in the DEAP/CLEAN experiment. Although members of DEAP/CLEAN signed the PPRP submission, a request for funding was not made. The SG recommends that STFC investigate with the community whether medium-term support can be awarded in such a way as to provide benefit also to projects other than LUX-ZEPLIN and EURECA. Provision of such support should not be considered to be a requirement – rather if aspects of the agreed work-programme are likely to be of significant benefit to more than one experiment then a coherent approach would be desirable.

32. The SG acknowledges that some participation in analysis of data from current generation experiments is desirable. We recommend that requests for such

support (Academic FEC and dedicated RA effort) be considered by the PPGP. The SG understands that requests for such support have indeed been made on the relevant CGs, appropriate to the usual split between programmatic work and science exploitation.

Long-term recommendations (capital-phase, 2014 onwards)

33. The long-term capital-phase strategy will necessarily depend upon the level of funding available, which in turn is presumably dependent upon the outcome of the Programmatic Review. In a scenario in which only very limited funding is available it is unlikely that a strategic approach can guarantee leadership via investment in specific work-packages. In such a scenario the most profitable route to visibility is likely to be via supporting unique expertise held by members of the UK community, possibly developed during the design phase. Beyond a critical minimum funding level work-package leadership on the basis of UK responsibility for major deliverables becomes possible. Following discussions with members of the community the SG estimates this level to be around £3M (staff and equipment), to be spent over approximately 3 years. With very significant capital-phase funding (at least ~£6M) such leadership may be possible in more than one experiment.
34. Given these considerations the SG recommends the following capital-phase funding strategy under three budget scenarios:
    - High: total UK capital-phase investment at least ~£6M over three years. Major investment possible in at least one, possibly two, world leading experiments. UK investment gives potential for strong visibility and key contributions, with the potential for significant leadership at the highest levels.
    - Medium: total UK capital-phase investment between ~£3M and ~£6M over three years. Major investment possible in at most one world-leading experiment. UK investment gives potential for strong visibility and key contributions, with the potential for significant leadership at the highest levels, from work-package leadership (low end) to co-PI (high end). Lower-level investment in a second world-leading experiment using an alternative target material should be considered, provided it has the potential to give substantial additional science benefit on the basis of unique UK expertise.
    - Low: total UK capital-phase investment less than ~£3M over three years. Level of UK funding insufficient to guarantee leadership at the work-package level or above. High visibility and key contributions possible, but must rely more on unique UK expertise rather than major strategic investment. The SG recommends that funds be allocated on the basis of clear evidence of potential for such contributions to no more than two world-leading experiments using different targets.
35. The SG recommends that the STFC call for capital-phase funding should not specify the number of bids to be submitted. However, it should be made clear to the community that further consolidation of activities would be viewed as a strength of any proposal.
36. Given the similarities between the technologies used in liquid Xenon and liquid Argon detectors the SG recommends that in all funding scenarios support should be allocated to at most one noble liquid target. The noble liquid community should be encouraged to consolidate behind one such proposal when feasible.

37. Decisions for long-term UK capital-phase investment will inevitably be affected by the outcome of funding rounds in the US (DoE/NSF G2 down-select) and in the EU (EURECA bids). It is unlikely that the UK could fund a programme of the required scale without significant support from international partners.

38. UK capital–phase support should ideally be awarded in phase with these decisions. Given the lead-time for PPRP and SB to consider bids it may be appropriate for the call for proposals to be issued in mid 2013, with a deadline in Autumn 2013 and a final decision taken in early 2014 once the outcome of US and EU funding rounds is known.

39. The SG recommends that STFC consider means of fostering community cohesion following any technology down-select as a result of the long-term capital-phase funding decision. A key strength of the UK community is its unique expertise in areas of general importance for DDMS experiments, including shielding design, cryostat engineering, DAQ electronics, data analysis software development, background modelling and data interpretation. It is important that loss of such expertise following any down-select be minimised. One possible option might be to allow applicants to request resources on competing bids without prejudicing the outcome of the review process. There would be no guarantee that requests submitted on competing bids would be supported in such cases – the PPRP and SB would of course need to be convinced that a strong science case exists for the requested funds.

Annex A: Sub-group work programme

The Sub-Group met on six occasions. The initial physical meeting on 30th March in Oxford consisted of a closed introductory session with Matt Griffin (SB Deputy-Chair) and Tony Medland, followed by a fact-finding discussion with the proponents of the PPRP proposal.

Sub-group discussions focused initially on understanding the national and international context for the review, developing urgent recommendations relating to the treatment of this field by STFC consolidated grants panels, and identifying components of the PPRP proposal suitable for limited short-term bridging support.

Following submission of an interim report and bridging proposal in mid-May the focus moved onto development of the medium-to-long term strategy. A community consultation exercise inviting email responses to an agreed set of questions provided input to this process. Focused telephone discussions were also held by DRT with key members of the community to gather relevant information.

Annex B: Interim report and bridging request (submitted 18th May)

(1) Executive summary

The Science Board Dark Matter sub-group has focused its initial work on the question of short-term support for the field following the rejection by SB of the recent PPRP proposal. It is our view that without urgent short-term support UK participation in this field will be significantly harmed in the medium/long-term. We find that on this timescale (and also in the medium term) funding should be concentrated on development of the next generation of experiments. We request £110k bridging to provide minimal 'keep alive' funding for R&D up to the point that further funds can be released in accordance with an agreed SB-approved STFC strategy. We also make recommendations for the treatment of Academic FEC, exploitation RA effort and RAL PPD effort, which should be considered urgently given the advanced state of the current PPGP review.

(2) Initial findings

It is clear from the material provided to us by the office and discussions with international leaders in the field that the UK possesses unique expertise and has the potential to secure significant leadership within the relatively large international collaborations currently forming. We believe that the best route to securing such leadership lies with taking on responsibility for key aspects of the design and construction of experiments at an early stage, maximally exploiting existing expertise. We understand that the timescale for such work is strongly constrained by the requirements of funding agencies in both the US ("Generation 2" experiment selection by DoE/NSF commencing in October 2013) and elsewhere in the EU (EURECA TDR scheduled for completion in late 2013). It is therefore critical that funding for such work should begin to flow with minimum further delay. Without this the UK will be unable to contribute effectively and build leadership for the future.

Although we have yet to consider medium/long-term strategy in detail (this will be discussed in our main report to SB in July), it seems clear already that the strategy should enable three distinct phases of project funding:

- Short-term: limited bridging support for R&D activities. This should cover the period during which STFC formulates its strategy and works with the community to form a mutually acceptable proposal for subsequent work. For the future health of the field this period should be as short as possible.
- Medium-term: Design-phase support for development and design work contributing to the next generation of experiments and building UK capability and leadership for the long-term.
- Long-term: Capital-phase support for construction of the next generation of world-leading future experiment(s).

Given the international context it would seem appropriate to consider a future capital-phase project starting at some point in 2014, contingent on the timescales for funding decisions in the US and elsewhere in the EU. Medium-term development funding will therefore likely be required only until this point. Given the short timescale, should SB decide that this field is worthy of further support it is vital that design-phase project funds (including dedicated effort) be made available as soon as possible thereafter. We believe that the case for such support could be made with primarily just a change in emphasis of the existing PPRP proposal without complete resubmission. We hope that this would expedite the process of considering such a proposal and hence enable the period of bridging to be minimized and hence additional new money funds for dedicated effort to be allocated quickly. We anticipate conservatively a maximum bridging period of 8 months to end Dec 2012 (starting retrospectively on 1st May), however it should be emphasized that <u>it is extremely desirable indeed that 'medium-</u>

term' support be allocated sooner than this date, should this be feasible.

(3) Bridging request

In the context of this nascent strategy we have examined the proposal submitted to the PPRP by members of the EDELWEISS/EURECA, LUX/LZ and DEAP/CLEAN collaborations. It is clear to us that this proposal demonstrates a strong science case for UK participation in the field and largely makes a strong case for funding. We therefore believe that a case for support for the medium-term design phase of future experiments could be based upon this proposal with only relatively few modifications. Our opinion differs from that of the proponents and from the recommendations of the PPRP primarily on the question of priorities for support. We view the highest priority to be the design, development and construction of future experiments, which should provide a strong foundation for future UK leadership in the field (see above).

Given the above considerations we believe short-term bridging funds should be provided from within the envelope of the existing proposal, focusing on aspects related to R&D and design. The technology areas outlined in the PPRP proposal in which the UK has demonstrated internationally leading R&D and design expertise are:

- Cryogenic cabling for EURECA
- DAQ systems for EURECA and LUX-ZEPLIN
- High voltage distribution for LUX-ZEPLIN
- Background modeling and suppression for EURECA and LUX-ZEPLIN
- Cryostat engineering for LUX-ZEPLIN

Significant UK contributions to any/all of these areas would be of great benefit to the experiments internationally. For this reason we find the arguments for equipment, technical support and associated travel specified in the proposal to be strong. Where Common Fund (CF) is requested for EDELWEISS participation we understand that a significant component will be spent in the UK and will provide the equipment required for R&D for the next phase (EURECA). We therefore find the case for a sub-set of the CF request also to be strong.

We acknowledge that some participation in analysis of data from current-generation experiments is crucial (see below) and so we also recommend support for a limited amount of related travel.

We believe that support for the above areas at a similar or somewhat reduced level relative to that requested in the proposal is critical in the short term for continued UK engagement in the field. We therefore urge STFC to consider urgently the provision of £110k bridging support to the proponents covering the period from now until 31st December 2012. The composition of this proposed figure is specified in Table 1.

(4) Additional interim recommendations

The proposal submitted to the PPRP contained in addition to the above items significant requests for new money for Academic FEC, exploitation RA effort, and RAL PPD effort. The sub-group believes that this field should move to a model for the provision of such support closer to that applied to other STFC projects. We recommend that Academic FEC for all experiments in this field be considered for support on group Consolidated Grants in competition with requests for Academic FEC for other experiments. We believe it to be important for the future health of this field in the UK that a strong base of Academics be supported in this area and hence believe that such considerations should not be limited just to those Academics who requested FEC on the PPRP proposal. We recommend that dedicated exploitation RA effort in this general area should also be requested via Consolidated Grants. We urge the relevant grants panels (particularly the PPGP in its current deliberations) to consider requests under these headings without prejudice, taking into account Science Board's view of the importance of this field. We urge RAL PPD to continue to support this area through allocation of permanent staff effort.