**Appendix 3: Goldschmidt Meeting Workshop**

Sacramento Convention Center, **June 7, 2014**

9:00 AM: Welcome and introduction to the goals of the Workshop (Mark Harrison)

Geochronologists are a diverse group and the blossoming of our discipline has brought forth a variety of methodologies that present different challenges. Some of you utilize mass spectrometers as one of your primary research tools, and others have never touched one. Some of you are frustrated about the low accuracy with which we know decay constants while others would not be impacted by an order of magnitude improvement in their knowledge. Costs to establish geochronological facilities range over a factor of a hundred. So as we talk about the future of geochronology, let’s keep a big tent perspective on the well-being of the entire community. I sent you excerpts from NRC’s 2012 New Research Opportunities in Earth Sciences report – which was the precipitating event for this consultation – and I hope you found a moment to peruse its recommendations. Geochronology was singled out for special attention, which is great news – we’re being talked about. But not all the things being said are entirely complimentary and it’s important for us to take stock and define a path forward that works well for us and those outside our field that require our products.

9:10-10:30: Improved knowledge of decay constants (Speakers: Ian Hutcheon, Patrick Boehnke, Paul Renne; Moderated by Blair Schoene)

Rationale: The existing convention for geochronologic decay constants has remained unmodified for nearly four decades. Concerns have been increasingly raised that these canonical values are inconsistent with more recent measurements, while enhanced precision and demand for inter-calibrated decay schemes at a fine scale has illuminated the need to revisit and refine these values. The geochronological community has traditionally not taken an active role in acquiring fundamental knowledge of decay constants, but the significantly improved precision and accuracy available today presents us with an opportunity to encourage NSF support of a new generation of measurements. Patrick Boehnke briefly reviewed the current status of decay constant measurements, Ian Hutcheon reported on opportunities to enhance determination of a geochronologically important decay constant, and Paul Renne provided an advance peek into the decay constant workshop held the next day. This was followed by a discussion of opportunities and the priority of such enterprises relative to other community needs.

1. “To lambda or not to lambda” presentation by Blair Schoene. Blair reviews the present status of decay constant measurements.
   1. Should we seek new decay constant studies and NSF funding for them?
      1. Rick Carlson: The relative importance of better determination of decay constants is system dependent. Some are okay, some are a big problem.
      2. Matt Heizler: need funding to do it, should be high priority; who other than NSF would fund it?
      3. Greg Balco: “Do we need new decay constants, or do we use Steiger and Jaeger?” was a strange question. Whatever values are used, we must indicate the values used. The concept of a central authority for decay constants is strange. While using a certain set of values (i.e. Steiger and Jager) was necessary once, today it is important to use the best values and indicate which one you are using in a particular paper.
      4. Rick Carlson: Rb-Sr values from S&J are clearly wrong and the lunar community has never accepted them; U-Pb and Rb-Sr would give us different values and only one can be right. Some attention to using the same values as a community is of importance.
      5. Matt Heizler: What is the difference between this being done in a centralized way and in a single PI lab? We can only have so many people working on things like decay constants. (It was noted that this would be discussed further after the talks.)
2. Paul Renne's presentation on the decay constant workshop the following day
   1. The call for new decay constants has evolved and there is a need to use the same set of values throughout the physical science community: our joint IUGS/IUPAC task group felt that 238U should be the lynch-pin because it seems to be the most precise.
   2. Key issues:
      1. Metrologic traceability should be an important goal
      2. Uncertainty analysis: no measurements should fall outside consideration (a new philosophy in nuclear geochemistry)
      3. Decay Workshop goals: History and mission of the task group, specific actinide systems and what the way forward is
   3. Questions/comments:
      1. Mark Schmitz: which agencies would fund decay constants work?
      2. P. Renne: funding from IUGS &IUPAC but at a low level (i.e., travel costs for meetings, etc.). In the future: it’s not clear what interest there would be from other agencies. NIST is a possibility. NPL in the UK seems interested in an advisory sense but not participatory or supporting. (Has this been actively pursued?).
3. Patrick Boehnke's presentation. Patrick showed that a meta-analysis of decay constants reveals a range of problems ranging from a clear inter-lab inability to reproduce some decays constants (e.g., 176Lu) to the fact that only one reliable study of 238U has been published (i.e., Jaffey et al., 1971) thus precluding a meta-analysis of its inter-lab variability. The present status warrants not only numerous new measurements of decay constants, but argues for a competitive landscape in which to evaluate these new data (e.g., the Particle Physics Data Group) rather than one arising from a centralized authority (e.g., UNESCO committees).
   1. Questions/comments:
      1. Matt Heizler: This many-small-studies approach is reminiscent of EarthTime and this was informative in demonstrating a great degree of scatter between labs. Would many smaller scale studies of decay constants be a good approach or would it be better to put a lot of thought into analytical approaches at the beginning? Should we talk about similar protocols upfront to get more consistent results or is this going to bias us (not necessarily rendering an accurate result) or let PIs do smaller studies with no overarching uniform standards for the measurements.
      2. Patrick’s response: Various methods have different uncertainties inherently; different approaches should give similar answers but different uncertainties (e.g. there are many ways of counting e.g. Lu-176, unlike K-40 (which could be standardized); Depends on what measured, betas vs. gammas: they have different uncertainties, different labs have different machines that also have different errors and uncertainties.
      3. Paul Renne: Distinguishing between outliers and dispersion is inherently subjective. Patrick replies: We used only the description of the analytical method not the result gotten by the study to include/exclude various studies. There were not enough studies to distinguish outliers.
      4. Mark Harrison: We’ve now seen many diverse approaches being proffered to assess lambda, e.g. top-down IUGS-IUPAC approach and, for example, the Particle Physics Data Group (with periodic meta-analysis), to get a community discussion going.
      5. Greg Balco: We should have a data discovery approach where people can determine what decay constants to use based on what they think is best and then report what they use.
4. Ian Hutcheon's U-decay presentation. Ian described how his nuclear chemistry group at Lawrence Livermore National Laboratory might bring mothballed counting equipment out of storage and undertake new counting measurements on the roughly gram quantities of 99.99% pure 238U and 238U in their possession.
   1. Jaffey et al. (1971) did a good job but underestimated their uncertainties; it is possible to re-do measurements, just need funding
      1. Matt Heizler: Are these experiments difficult to redo?
      2. IH: No, we just need to get the 4π beta detector back up and running
      3. Noah McLean: do you assume Th is in secular rates equilibrium with U?
      4. IH: We can measure beta emission rates directly. We must just make sure the starting material is pure (i.e., zero age)
      5. Mark Harrison: What improvements to Jaffey et al. can we expect?
      6. IH: Not really any drastic ones but they botched the uncertainties. They only counted alpha decays whereas we will count both alpha and beta decays; unlikely to do much better with 235U than 238U despite the higher decay rate.
      7. Paul Renne: If funding could be found, would there be willingness of the national labs to do this?
      8. IH: Yes, experiment is non-destructive and Livermore could be interested in doing it. (Paul Renne: Perhaps we could characterize it as a national security issue?)
      9. Kevin McKeegan: How do you assess experimental uncertainty?
      10. IH: Best to do experiments at multiple facilities. The problem is the purity of the starting materials. Most labs cannot accept the starting material and we cannot share it outside the USA; 235U could be done at Livermore, 238U less of a problem and could probably be done at other places
      11. Brad Singer: how about other isotopes?
      12. IH: They are not part of the weapons program so we don’t have the right starting materials.
      13. Paul Renne: there are some experiments in process to some extent, come to my workshop tomorrow.
      14. Noah McLean: where did Jaffey go wrong?
      15. IH: That’s not entirely clear. The error bars are well below what we can do today.
      16. Anthony Koppers: How do you deal with uncertainties? What if you get a different answer than Jaffey et al.?
      17. IH: ideally we would have multiple labs perform experiments
      18. Matt Heizler: Are ingrowth experiments intractable for long-lived isotopes?
      19. Patrick Boehnke replies: For Rb-87, the best work is Rottenberg for ingrowth experiments. Sm-147 is probably doable. Lu-176 maybe. But Rb (in the Rottenberg experiment) had been sitting for >30 years.
      20. Ryan Ickert: Are there other countries that have these detectors that we could collaborate with?
      21. I.H.: I don't know but the main issue is obtaining the pure 235U starting material.
      22. Patrick: Kossert does this in Germany with scintillation counters; this is happening mostly in medical science –but still themain issue is getting starting materials.
      23. Greg Balco: Is there any evidence that the Jaffey et al. values are wrong?
      24. I.H.: no, can't fault experiments at all just the uncertainty issue
      25. Mark Harrison: On Lu-176, picking a winner is premature. Let 1000 flowers bloom, etc. It is scary that U-238 is so shaky and we should realize that we can’t proceed with the old number with confidence; the potential for high precision U-Pb dating makes 235U and 238U attractive targets; maybe more than the USA can handle alone?
      26. Mark Schmitz: When would IUPAC change their values?
      27. Paul Renne: (basically never)
      28. I.H.: would have to pry their fingers off them or new experiment- we should ask them if this should be the most important experiment
      29. Mark Schmitt: most of us want good intercalibration between methods and don’t necessarily care about absolute values but 238U is the lynchpin.
      30. Matt Kohn: what precision can we get to?
      31. I.H.: 0.1%
      32. M.K.: other systems?
      33. I.H.: some we can do better than a permil.
      34. Kip Hodges: One experiment won't improve things that much, we need many and NSF shouldn’t get the impression that we need just one. We should look for international collaborators. Dan Condon: For the price that is estimated by Ian ($350k), the British Geologic Survey would instantly pitch in $$$.
      35. Rick Carlson: For Lu-176 there are dramatic improvements using geologic comparisons? Should we base other decay constant calibrations on U?
      36. Patrick Boehnke: Lutetium is an example of how much disagreement there can be even with multiple measurements.
      37. Matt Heizler: : The Ar-Ar community needs to get their house in order first. There are multiple priorities here. U-Pb and Ar-Ar intercomparison is not limited by Jaffey’s decay constant but by reproducibility between labs.
      38. Sonia Esperanca (NSF): What would improvements in decay constants get us in terms of science? That’s what will make it a competitive investment. What is the gain in the research field. Give us (the NSF) direction; you are the experts. Give us cost/benefit analysis. Primarily we fund cutting-edge research. Make the case for this being cutting-edge. (perhaps link this to ideas for paleobiology, etc.)
      39. Mark Harrison: geochronologists need to advise NSF how we might move forward, how it will lead to scientific advances and in terms of research: NSF surely wants to invest in cutting edge stuff
      40. Matt Kohn: NROES report includes examples of U-Pb dating studies where high precision rather than high accuracy is most important; are absolute ages critical to most studies?
      41. Marc Caffee: Are people satisfied with half-lives and decay-constants of cosmogenicsystems?
      42. Greg Balco: Actual scientific limits are imposed by poorly known Al-26 half life; knowing this better would in fact be useful (if maybe not a huge priority).
      43. Randy Irmis: Intercalibration is absolutely essential and absolute timescale is important to many.
      44. Matt Kohn: need to keep in mind there are other methods to date other than isotopes, e.g. orbital cyclicity are getting precise ages- should we calibrate to that?

11:00-12:30: Support of single-PI labs vs. centralized facilities (Panel: Marc Caffee, George Gehrels, Dan Condon; Moderated by Brad Singer)

Rationale: Presently, NSF-EARs Instrumentation and Facilities program supports a set of national geochronology facilities (http://www.nsf.gov/geo/ear/if/facil.jsp) that have arisen on an essentially ad hoc basis (i.e., without a specific RFP) while the recent NRC report, *New Research Opportunities in the Earth Sciences* (NROES), raised the possibility of creating one or more national geochronology centers at a level beyond the capacity of existing EAR programs. A panel discussion with representatives of the NSF/IF geochronology facilities and a UK geochron organization addressed the strengths and weaknesses of the current system and explored possible consequences of creating national centers for geochronology on a larger scale. There followed a discussion of the challenge to elucidate what balance between national facilities and single PI laboratories (or networks of such laboratories) could best drive innovation while fulfilling community needs.

Panel Discussion: National Facilities

* 1. Marc Caffee describes PrimeLab which gets$0.6 M/yr from NSF and the balance comes from other grants and user fees; with NSF if you request funds for AMS analysis then you can run at PRIME Lab for half the going rate.
     1. Matt Heizler: Perhaps could you talk about the human consequences personally of running such a user facility? Mark Caffee: If I didn’t immensely enjoy it I wouldn’t do it; wish we had more resources.
     2. Matt Kohn: Where are the bottlenecks? What are the limitations?
     3. M. Caffee: chemicals, sample prep; we only have two chemists in PrimeLab and limited fume hoods. The machine has been operating 85% of the time this year so the machine itself is not the limiting factor
     4. M. Kohn: What fraction of funding goes to human resources?
     5. M. Caffee: user fees pays for the consumables (e.g. chemicals) and people. 85% of days this year we’ve taken data; the rest was maintenance either scheduled or not.
     6. Kip Hodges: AMS running all the time but not always doing cosmogenic nuclides- running 24/7 ≠ no backlog M Caffee: 75% of AMS is running geoscience the other 25% is biomedical research (e.g., mammoth projects with Ca)
     7. Danny Stockli: How do we engage users? Do we just send them data?
     8. M. Caffee: there are users who don't need scientific collaboration and just need the facility, there are also users who contact us and need tight collaboration. Both are valid and both are necessary
     9. Danny Stockli: What is the user selection process?
     10. M. Caffee: 60% are NSF users; all users are reviewed but if they have government funding they are not reviewed. People without funding can go through a program PrimeLab has that helps if the project is good/interesting.
  2. George Gehrels describes LaserChron. Full cost is $8/analysis, $4 for NSF funded research. We don’t charge for standards. We run a pilot data set at no cost and provide student travel cost support.
     1. Matt Heizler: Who dictates the funding cycle?
     2. Gehrels: we asked NSF and they advised to stay on the 3 year funding cycle
     3. Aaron Zimmerman: What is your resistance to “mail order geochronology” or industry work? (Gehrels mentioned this in his talk)
     4. Gehrels: We think it is a great learning experience and training opportunity to have users come do it themselves
     5. Matt Kohn: What are the bottlenecks?
     6. Gehrels: So far have been successful in getting all NSF users onto the facility. Demand is however increasing; turnaround time a few months from first contact. The bottleneck is not having users send in their samples early enough for prep and imaging.
     7. Brian Jicha: Are there international users for LaserChron Center or PRIME Lab?
     8. Gehrels: very few requests.
     9. M. Caffee: NSF has not discouraged international users and we have many.
     10. (?), USGS: Does LaserChron center run other minerals, e.g., apatite, titanite, etc.? George Gehrels: Monazite yes, but we avoid phases with high common Pb.
  3. Kevin McKeegan describes UCLA SIMS.
     1. Matt Kohn: What are the bottlenecks?
     2. K. McKeegan: These are slow analyses (a few per hour) and we have a balancing act between acquiring data and instrument/application development; there is little time for messing around and training.
     3. Benefit of user facility:
     4. K McKeegan: The facility is good for our students as it involves them in a variety of things (that they wouldn't without users)
     5. Danny Stockli: seems instrumentation isn’t funded by NSF- what is the upshot of being a multi-user NSF facility
     6. K. McKeegan: That’s been true for UCLA up to now and the problem is the cost of instrumentation and maintenance is going up.
     7. Anthony Koppers: Seems the distinction between national and single-PI lab is arbitrary: the difference is whether there is directed NSF funding
     8. M. Harrison: That’s true and the present status is somewhat unfair because NSF funding is such that you would likely have to kill an existing facility to fund a new idea or lab; so young people are at a disadvantage; we need these new ideas so we need more funding.
     9. K. McKeegan: A key facility issue is the problem of hiring skilled people long term - university support for these roles has evaporated and there is no job security on individual project grants; however, facilities do provide a little more security
  4. Dan Condon describes Britain’s NERC Isotope Geosciences Laboratory. NIGL is a national users facility that supports geochronology undertaken by NERC-funded and British Geological Survey scientists.
     1. Brad Singer: If centralized facilities are the only way towards more (and more stable) funding from the NSF, innovation etc., then are we in favor of this model and how might we implement this?
     2. Jen Wade (NSF): What dictates which projects/proposals are successful and how are they selected?
     3. D. Condon: 40% of proposals are successful and judged on intellectual merit –a committee decides which projects get supported as the facility is fully funded
     4. K. McKeegan: this sounds like it operates like astronomy in USA (e.g. telescope time and usage)
     5. Sonia Esperanca: NSF is starting collaboration with NERC within the next year so it should be relatively easier to collaborate with them. How often is NIGL reviewed and funded? Dan Condon: every 5 years; there are some other issues currently
     6. Matt Heizler: How competitive in the UK is it to be the main user facility?
     7. D. Condon: There’s an open call for funding; other facilities can get funding as well
     8. Dave Shuster: Shifting money into centralized facilities will cause single-PI labs to suffer. Brad Singer: What is the risk?
     9. Pamela Kimpton: NERC taking single-PI funding isn't accurate because of the way money is dealt with and divided; The UK is structured to avoid that trade-off as there are two funding streams in the UK: National Capability (BGS, etc.) and another. NIGL comes from the National Capability and so takes no money away from single-PI facilities.
     10. Dan Condon: That funding structure is changing slightly now
     11. Susan Zimmerman to McKeegan: What is your interaction with NSF? And the broader community?
     12. K. McKeegan: we interact with NSF every funding cycle (3-4 years for UCLA) and there is an advisory committee to facilitate our interactions with the broader community (e.g. soft rock, geobiology) but it does not have authority over the lab.
     13. Matt Kohn to D. Condon: Do you write annual reports?
     14. In UK: yes
     15. NSF facility PIs: We write annual NSF reports
     16. Danny Stockli: What are ramifications of central (vs single-PI) facilities for student training? With centralized facilities, will we train enough new experts on using these instruments?
     17. D. Condon: NIGL does both mail-in and user training (students and post-docs often come); no one size fits all.
     18. Rick Carlson: Perhaps the onus should not only be on NSF. Probably there aren’t a lot of universities that would support having a PI only overseeing a user facility, but this might be more likely to work at a national facility or national lab.
     19. Kyle Samperton: What are the priorities of lab leaders in single-PI vs. user facilities: different techniques or approaches? What are the possibilities of benefits of a more centralized lab like NIGL for difficult work such as high-precision U-Pb
     20. Matt Kohn: Within the current funding model the “uniqueness” criterion is very important, e.g. an AMS facility (more unique) versus an electron probe because there are so many.
     21. Patrick Boehnke: To current facility operators: How often do you refer potential users to another facility that would be more cost-effective or useful?
     22. M. Harrison: We do all the time for time and cost-effectiveness and/or better fit labs for a specific project (e.g., directing folks to LaserChron)
     23. Mark Caffee: We refer a lot of radiocarbon users elsewhere.

1:30-2:30: How can funding agencies best encourage geochronologic innovation? (Panel: Anne Trinquier, Sonia Esperanca, Kevin McKeegan; Moderated by Mark Harrison)

Mark Harrison’s preamble: My sense is that new geochronologic methodologies sometimes arise spontaneously and other times are motivated by specific applications. In cases where novel hardware is required for advancement, our ca. 60 year history shows that there has been no single path to commissioning new generation instrumentation. The post-war development of modern geochronology was essentially a by-product of a nuclear physics culture focused on deterring nation-state aggression. Development of the first digitally-controlled, thermal ionization mass spectrometer resulted from a national effort in planetary R&D, ultimately also fed by the cold war. The development of the high resolution, high sensitivity ion microprobe was carried out by a block-grant supported research institution in a nation 1/15th our size with the first such instrument in this country funded through a private gift. The race to perfect multi-collection TIMS from the mid-80s to mid-90s was largely driven by intense competition among multiple manufacturers. Inductively-coupled plasma mass spectrometry arose from an adaptation of instrumentation developed in a different discipline. Although the very diversity of these origins might at first glance seem a strength, the landscape that produced these revolutions is starkly different today. The cold war and space race are long gone. Block-grant funded research organizations are virtually extinct. Previously available sources of private philanthropy appear unresponsive to geochronology. The mass spectrometry industry has been consolidated into essentially two competitors. All these changes, coupled with a decline in the local capability to build scientific instruments, contribute to limiting hardware innovation.

This changing landscape is nested within a broader national trend of level federal support of R&D following 30 years of rapid growth. This is perhaps most clearly seen in the biomedical research community which experienced a period of explosive growth through the millennium change, but is now barely coping under level funding (Alberts et al., 2014, PNAS 111, 5773). This enlarged community would require near geometric funding growth just to retain the same standard of care that was enjoyed 30 years ago when total federal outlays for non-defense R&D were half as much in constant dollars as today. Under level funding, the fabric of biomedical research culture is starting to fray – an unintended consequence of what was once seen as an enviable growth spurt. To a lesser but still significant degree, growth of the geosciences over this period has put tremendous stress on the operation of now relatively numerous single-PI labs; too few dollars for what could be historically argued are too many facilities. Sustaining these operations reduces the amount of support for costly, high risk, but potentially game-changing innovation.

Innovation on a smaller scale remains viable but it is not always clear whether the core NSF-EAR programs are optimally organized for supporting the development or refinement of geochronological methodologies. The increasing emphasis on surficial processes and environmental change have brought a raft of new applications to geochronologists, but the technological legacy of the discipline retains firm roots in the hard rock sciences. Thus while new or refined methods to determine, for example, climate change chronologies can be swiftly adopted by scientists across a broad range of fields, responsibility for their development has not typically been shared among the benefactor communities. In effect, the geosciences sensu latu may be enjoying the opportunity to buy their geochronology by the glass while the EAR has been left responsible for tending the vineyard.

Given the above constraints, it is at least arguable that 1) looking to the past offers little direction forward, 2) adapting the size of the community to sustainable resources should be on the table, and 3) that stewardship of geochronology should be expanded across all geosciences disciplines that utilize its products, including the responsibility to support innovative, potentially high risk R&D.

1. Panel Discussion
   1. Sonia Esperanca (NSF)
      1. Sonia's response to Mark's question” “NSF's mandate explicitly encourages transformative science recognizing the higher risks investigators incur in such ventures, yet there is widespread perception across most scientific communities that we don't yet have the balance between risky and safe quite right. Assuming the perception is true, does this reflect a risk-averse federal culture, a hyper-critical reviewer pool easily able to sabotage proposals that challenge the current paradigm, a mix of both, or something else entirely?”
         1. I personally think we fund transformative research; although it’s hard to know what is transformative. Anything NSF does identify as potential transformative will get funded.
         2. There is a balance between risk and reward (risk of failure of project)
         3. NSF doesn't see a lot of 'transformative' research
      2. Suzanne Baldwin: Why did the Frontiers of Earth System Dynamics (FESD) program have its money taken away?
      3. Sonia: I don’t know. Funding and pressures from infrastructure support –it’s easier to maintain infrastructure and stop research than vice versa. I think NSF is doing an adequate job of funding geoscience research and innovation. Jen Wade: Reviewing community is very good at tearing apart potentially transformative work. Steve Harlan: reviewers have a problem making case for transformative research; they’re not good at emphasizing strengths in proposals
      4. Pete Reiners: Is there something the community could do to articulate transformative research for NSF? Perhaps a synergistic program with engineering, a new program with fundamental funding, write a white paper, etc.? (outside of being better reviewers).
      5. Sonia: Where do you think your community could lead? We’ve asked you to do this in a sort of visionary document; part of making the case that in the long run this sort of investment will pay off.
      6. Greg Balco: Can we make the case that innovation funds lead to commercially viable innovation? Especially on hardware innovation, that makes things commercially viable?
      7. Sonia: give NSF something they can wrap around and let’s see - the mood is changing toward making partners with the industry, other organizations, other agencies, etc. Be broader minded to how we make our case.
   2. Kevin McKeegan's answer to Mark Harrison’s question: “25 years ago, you helped specify the design of the first high resolution, high sensitivity ion microscope and went on to build the world's only million volt ion microscope. You now have a pending grant to develop an improved instrument but it's based on the same ion optics and mass spectrometer chassis you used a quarter of a century ago. Has innovation stalled, are there transcendental approaches out there we're just too timid to investigate, or has the relevant technology simply been perfected?”

My response focuses on mass spectrometry hardware in terms of innovation. Why are American manufacturers not really in the game for earth sciences? Just not enough of us, or enough demand? A good example of innovation is CHILI, although geochronology applications are limited. Areas for innovation: high precision, high selectivity (molecular-specific, spatial resolution, etc.); progress on these fronts but room for improvement. Another issue is long time scale for development, e.g. MegaSIMS took 17 years to be developed between the proposed NASA mission and the key paper being published. There are various kinds of innovation that would be applicable across many sciences (e.g. better detectors): NSF could fund if it had specific proposals. Maybe cross-agency fund for innovation of this type is needed. There remains room for innovations in precision and spatial scale and their tradeoffs; we might have to involve collaborations with instrument manufacturers

* + 1. Pete Reiners: we want to ask ourselves what innovations in applications would drive science and generate intellectual and/or financial propulsion
    2. Rick Carlson: We rely on commercial instrumentation…if want to do X then we buy Y machine instead of building one. Two groups to consider: a) we are serving a large user community that wants results versus b) people who want to push the edges of techniques for one particular application (e.g. MegaSIMS). Like when Thermo achieved higher precision this opened the door to investigations of extinct radionuclides, etc. There are fields limited by current techniques but we’ve given up in university labs those staff with the backgrounds to build unique instruments to further particular goals.
    3. Matt Heizler: What my lab did was convince the company they could make money making us a product (high-gain amplifier), because we didn’t think we could write an NSF proposal for this. But it took a lot of convincing.
  1. Question to Anne Trinquier (ThermoFisher): “Consolidation of mass spectrometer manufactures and what seems to be a low rate of start-ups potentially limits the diversity of instrument choices available to the geochronological community. In that context, is the size and competitive nature of the present commercial market what drives manufacturer innovation or does it arise more from the individual ingenuity of industry engineers and customers?
     1. Anne: Yes and yes. Thermo is a company driven by profit but their aim is to be a science support company –to push science and innovation (even before there is any competition); Thermo has a number of collaborators; many Thermo colleagues are from academia and have research backgrounds and collaborate with customers
     2. Rick Carlson: how to serve a community need for geochron ( with ± traditional machines for traditional data) and innovate – in order to innovate, money had to be spent for something that will take time: how do we achieve these balances?
     3. Kevin McKeegan: community should advertise our successes more- people at mass spec companies don't know what we do (e.g. measure isotopes to ppm level)
  2. Brad Singer: Mark asked me to comment on EarthCube workshop in Madison last October. Some aspects
  3. Matt Heizler: When or if did NSF get out of the business of general education? A lot of students are trained at the masters level to do geochronology but few go on to become geochronologists and instead do other things. I hope education of students is still an NSF-based research goal?

1. Sonia: Some communities want us to pay for every development and some prove the new development first then come for funding. Some can get away with this better than others because they can make the case in a way the peer community will buy. This is a tough kind of proposal to write. Does the group have a track record of achievement? Etc. Perseverance, salesmanship, community trust are important.

* + 1. Anthony Koppers: how to get out of the dilemma of needing time and money to prove something but NSF wanting to see it proven first
    2. Sonia: some people get away with making the case for success (through past vision of what can be done next): it is really about perseverance and sales (being a salesperson of your idea/work)
    3. Danny Stockli: What is the role of IF in developing /innovating?
    4. Sonia: There is a disconnect between facilities & instrumentations. What they fund seems scattershot. A lot of money for facilities gets eaten up. What’s the future role for IF. We might need new way to do business at NSF.

2:30-3:15: Developing synergies between disciplines (Panel: Suzanne Baldwin, Kevin Furlong, Becky Flowers; Moderated by David Shuster)

Rationale: In the past decade there have been notable and significant advances made in which multiple geochronologic techniques have been brought to bear on problems that elude solution by any one method. Closer collaboration between practitioners of the various geochronologic approaches enhance the likelihood of breakthrough science. In a similar fashion, the next generation of numerical modeling codes that relate geochronologic data to physical mechanisms await collaborations between two groups – geochronologists and geodynamicists – who have not traditionally communicated well. Kevin Furlong offered a geophysicist’s perspective on what geochronological data was being demanded by tectonicists and a panel discussion led by Suzanne Baldwin and David Shuster explored ways of enhancing contacts between these communities, including alerting funding sponsors to encourage key interdisciplinary interactions.

Specifically, 1) innovative research often occurs at disciplinary interfaces, yet the appropriate NSF program(s) and mechanisms to adequately support geochronology for such research is not always clear, and 2) high-risk research involving innovative applications of geochronology in conjunction with other data types and/or numerical modeling may be viewed beyond the scope of a given core program. Interdisciplinary projects can involve one or more funding modes to support geochronology conducted in single-PI labs, including: 1) subcontract for “service”, subaward to the geochronology PI, 3) collaborative proposal submitted to a core NSF program, or 4) a collaborative proposal in a “special” programs/solicitations (e.g., Integrated Earth Systems). Each of these modes has advantages (e.g., efficiency) and disadvantages (e.g., double overhead, superficial involvement of geochronology PI). The question is, do the existing modes of funding geochronology at NSF adequately promote/facilitate cross-disciplinary research? These topics are addressed in the following slides:



1. Discussion following presentation:
   1. Main Question: Do existing modes of NSF funding encourage cross-disciplinary collaboration?
      1. Suzanne Baldwin: we might want to make a list of our top 20 hits to promote our science to the NSF GEO program managers
      2. Kevin Furlong: How do outsiders see geochronology? They need "more" data.
      3. Mark Harrison: pointed out that NSF is providing great educational opportunities, for example the SIMS workshop. Mark Caffee: We’ve had no formal program for the past few years. However our door is open for students to come learn. George Gehrels: We hold a short course at GSA every year; generally 40-50 people/year, from students to PIs. Short course modules available on LaserChron webpage.
      4. Kip Hodges: National facilities yield opportunities for outreach to students; the cost of data generation is so high that it is a detriment to cross-disciplinary research (e.g. hard for a PI in another field who can’t support their own stuff plus get extra $$ for the geochron work).
      5. David Schuster: Who foots the bill for geochron? All core programs need to step it up for geochron; geochem/petrology carries a lot of this burden.
      6. Pete Reiners: IF spends 80% on facilities; what proportion of that is geochronology facilities? (answer: not much; most of it is IRIS, etc.)
      7. Danny Stockli: Our community wants to stay as integrated as possible; we need to be careful about having a pool of money just for contract geochronology.
      8. Patrick Boehnke: Instead of focusing innovation on more precise or accurate measurements, perhaps we should focus on making measurements cheaper and more efficient? e.g. many users need a large quantity of data but don’t necessarily need it to be very precise.
      9. Mark Caffee: The fear among PIs is that if they write too many analyses into proposal then they aren’t funded. Therefore the proposals funded don’t yield high quality studies because they don’t collect enough data. NSF needs to not penalize people for including enough analyses.
      10. Brad Singer: it is a good idea to get single PI labs together and do it through student education and funding- like Becky Flowers EarthScope model.
      11. Blair Schoene (referring to upscaling EarthScope model to NSF-wide): We need to make sure geochronology is integrated into proposals from the beginning, not something to be added on to already-funded science proposals as an afterthought. The bottom line is that geochronology is crucial for a lot of science and it should be treated as such.
      12. Rebecca Flowers: And making it so that asking for a larger number of analyses doesn’t lead to a proposal being penalized.
      13. Rick Carlson: We could do something analogous to IRIS by tying together labs and have them supported by facility that would do geochronology on/for other projects (e.g. tectonics) when needed.
      14. Steve Harlan: Geochronology money was set aside from Earthscope with restrictions on what/where it was spent. Money distribution was difficult because it was part of the facility itself. The funding should have been in the “science” part of the project not “facilities,” as this complicated things.
      15. Jen Wade: Money tied to specific lab facilities will not always map well to a particular project.

3:15-3:30: Afternoon break

3:30-4:45: Topical breakout sessions

Topical breakouts permit specialist discussions of the development of improved standards and the role that geochronology has played in transformative geologic research among specific or closely allied methodologies. The former is key to improving the accuracy and intercalibration of geochronologic methods and the latter takes advantage of a gathering of key representatives of the nation’s geochronologic community to fully document the relationship between the provision of absolute time and transformative earth science research. The goal is for each breakout session to develop their own contribution to this narrative and to identify scientific breakthroughs close at hand and the resources that could realize those discoveries. The ultimate goal of this exercise is to make a compelling case to funding agencies that geochronology has been, and will continue to be, a uniquely capable tool for fundamental discovery.

Group Break Out Summaries

1. K-Ar Group (led by Brad Singer/Suzanne Baldwin)
   1. PI’s priority is to work on their own funded proposals
   2. Vision to fully utilize existing facilities: our favored model is a distributed network over a centralized facility (e.g., a National Geochronology Virtual Institute)
   3. We recognize the need to collaborate with other communities to extract the full potential of all methodologies
   4. We are training the next generation of the “ energy” workforce (e.g., in analytical techniques, determining thermal histories, etc.)
   5. Interested in quantifying rates of processes, and in collaboration with other communities ( e.g., geodynamicists) we are on the verge of understanding how mantle dynamics, lithospheric and surface processes are linked ( i.e., a 4-D understanding of Earth processes)
   6. Distributed network is favored over one centralized facility. Vision is to have fully utilized facilities that allows innovation.
   7. Proposal to develop a national geochronology institute for integrative partnerships that NSF puts money into and then labs apply to be a part of
      1. At the NSF GEO-level: sustainable grants on competitive basis, 5 year duration grants (e.g., could be modeled after NASA’s Astrobiology Institute)
      2. McKeegan: expand to others, not just NSF including NASA, DoE (argue it's training so investment of in the new generation instruments is everyone’s responsibility)
2. U-Th, U-Pb, U series (led by George Gehrels and Blair Schoene)
   1. Motivations/drivers for investing NSF money into high precision U dating
      1. Mass extinctions
      2. Climate change, biotic change: Use high-precision to link to human time scales
      3. Higher precision needed for intercalibration efforts
   2. Other goals
      1. 4D Earth models
      2. Holocene records especially on human time scale (beyond 14C record), climate change
      3. Volcanic hazards, supervolcanoes
      4. Basic processes: elemental and isotopic partition
      5. New isotopes for spiking
      6. More labs for U-series
      7. Good idea to write up examples of how things become transformative- specific examples for each other geo-discipline (form the Ven diagram we started whole day with)
3. U+Th/He & Fission Track (led by David Shuster)
   1. General agreement labs basically at capacity (turning people away)
      1. Not clear about what to do about this though
   2. Technique fairly labor intensive
      1. Technical support staff: important to keep track of how they are paid for esp. with NSF funding: Need to look for ways to improve funding for technical staff support
   3. Need to be champions of our work- promote work and how important it is (esp for other geo disciplines)
4. Cosmogenic, 14C and OSL (led by Marc Caffee)
   1. Consensus that bottleneck in field is sample preparation
      1. Possible solutions
         1. The bottleneck is sample prep (establish more chem labs, support commercial labs, increase through-put in labs (not a typical practice in academia)
         2. Those that have sample prep facilities could open their labs up
         3. Facilities better communication about best practices
      2. The quality of sample preparation is a cause of concern; poor chemical prep leads to poor results
      3. Better communications between all labs and facilities disseminating information about best practices
   2. Need more reference materials and have them available and in the literature
   3. CRONUS Group Project is a good example of what can be done when a group of people come together to propose idea to NSF
      1. Supported by a number of NSF programs (tectonics, geomorphology, etc)