**Appendix 2. Results from on-line questionnaire**

Following is a compilation of results from a questionnaire that was made available (on-line) to 243 geochronologists in the USA. The fields represented include using 14C, 40Ar/39Ar, cosmogenic isotope, fission track, Lu-Hf, Re-Os, optically stimulated luminescence, Sm-Nd, (U-Th)/He, U-Pb, and U-series chronometers. The list of geochronologists is included following the survey responses.

**Topic #1: Decay constants**

QUESTION 1: Do you think that determining new decay constants for use in geochronology should be a high priority for geochronolgists? If Yes, please rate priority (5 Highest):

Rate:

(1): 1

(2): 5

(3): 3

(4): 6

(5): 4

NO: 1

QUESTION 2: Do you think that determining new decay constants for use in geochronology should be a high priority for NSF funding?

Rate:

(1): 1

(2): 6

(3): 2

(4): 7

(5): 2

NO: 2

If you answered "Yes" to any of previous questions, jump to Question 4.

QUESTION 3: Do you think new official values should be a recommended and used, or should the recommended values of Steiger and Jager (1977) continue to be used?

NEW VALUES: 4

STEIGER AND JAGER (1977): 0

QUESTION 4: Do you think this work should be focused on intercalibration between different decay schemes or counting/ingrowth experiments?

INTERCALIBRATION: 13

COUNTING/INGROWTH: 17

(many people chose both on this question)

QUESTION 5: Do you think this work would be best accomplished by single-PI-driven research proposals through existing programs or via an NSF supported initiative?

PI DRIVEN: 12

CENTRALIZED: 8

QUESTION 6: On a scale of 1-5 where 1 is of no importance and 5 is absolutely vital, rate the importance of the following statement: How important to you is it for different scientific communities (i.e., geo- and cosmo-chronologists, chemists and physicists) to use a common set of decay constants?

(1): 1

(2): 1

(3): 3

(4): 5

(5): 10

QUESTION 7: Do you think NSF should pursue international cost sharing on these efforts?

Rate:

(1): 0

(2): 0

(3): 4

(4): 6

(5): 7

NO: 3

**Topic #2: Support of single-PI labs vs. centralized facilities**

8. Keeping in mind the set of 5 current national multi-user geochronology facilities (PRIME LAB, Arizona Laserchron lab, UCLA SIMS lab, NOSAMS 14C lab, and Woods Hole SIMS lab, what is the appropriate mix of NSF support for single-PI labs versus national multi-user facilities for any of the chronometers listed below?

• U-Pb TIMS

• U-Pb TIMS/LA-ICP-MS/SIMS

• 40Ar/39Ar

• U-series disequilibrium

• U-Th/He (Thermochronology in general)

• 14C (radiocarbon)

• Cosmogenic Nuclides (10Be, 26Al, 36Cl, 14C, 21Ne, 3He, others)

• Other clocks (Re-Os, Sm-Nd, Lu-Hf, etc.)

* AMS techniques clearly require multi-user support due to the immense cost of these facilities. Some of the other multi-user labs, such as the Arizona Laserchron Lab, have also clearly been very successful. However, single-PI support should clearly be the priority overall, as our science will fail to advance without the sort of innovation that can only happen in this environment. The emphasis should be on moving science forward to make new discoveries and to discover the techniques that will revolutionize the field tomorrow, not on stamp collecting with the techniques of yesterday and today.
* I could say a lot about this subject, but I will try to be brief. I am not sure the current 5 multi-user facilities cover the many topics shown in this question. Also, there are 2 SIMS labs but most SIMS work is done at other labs not funded in this way - many independently, or by NASA-NSF joint funding. Similarly for AMS, 14C measurements are widely done by a broad range of single PI and/or independently-funded labs such as Irvine, Arizona, Georgia, Naval Research Lab, Livermore, etc, etc. in the US. Overseas, there is a broad spectrum of AMS labs of all kinds. Hence, the multi-user laboratory seems to be less justifiable in some fields. In others, they are presumably still useful. However, the broad range of topics listed here isn't going to be addressed by a few facilities - some new model is needed. What that model is, I am not sure.
* It is varies from system to system. Some chronometers require very expensive instrumentation for analyses, but the sample preparation may not be so costly/time extensive or the same protocol for sample preparation can be used for a wide range of different sample materials. In that case it makes perfect sense to have a national facility. Other system may not require as expensive instrumentation for analyses, but require very time extensive sample preparation that can be very different for different sample material, in that case smaller focused single PI-labs are probably more cost-effective than a national facility.
* Cosmogenic Nuclides (10Be, 26Al, 36Cl, 14C, 21Ne, 3He, others) U-Th/He (Thermochronology in general)
* More national, multi-user facilities would be valuable for 40Ar/39Ar, U-Pb TIMS/LA-ICP-MS/SIMS, and (especially) (U-Th)/He; Prime lab is great for actual CRN counting, but more multiuser labs for sample prep would be beneficial. Depending on the laboratory interest, 3He CRN could be combined with (U-Th)/He
* The issue with national facilities is that it implies NSF support for the accuracy of results from a single (or perhaps multiple) facilities--when those results may not agree with those from other labs. For some systems this is not a problem, but others (e.g. Ar/Ar) it is.
* There is room for both. Single-PI labs have the better record of pushing technique forward, and technique advances are why geochronology is in so much demand. Single-PI labs usually don't have sufficient staff to process samples outside the range of science topics pursued by the PI. "Service" geochronology thus is better done by a lab supported to do just that. In part, it depends on the cost of the instrumentation involved as it likely is not within NSF's capability to establish a large number of, for example, AMS labs.
* National multi-user facilities should be restricted to labs that require many personnel (including electrical engineers) to maintain, and for which a limited number (1-3) exist in the US (e.g, SIMS, PRIME). All others should be supported as single PI labs.
* NSF should support only those facilities that are rare and/or whose work is unusually expensive (usually these correlate), in the context of demand. Prime lab, the 2 SIMS labs, and Nosams are great examples where I think NSF support is needed. At one time Arizona's lab was sufficiently unique that it also warranted direct NSF support. But there are now so many LA-ICP-MS labs doing this kind of work that I think it should be dropped from the list. This is not a slap at their work, which has served the community in an exemplary fashion. But NSF can't support every lab. Similarly, I think there are either sufficient labs for the other chronologic systems, in consideration of demand, that NSF doesn't need to add new multi-user facilities to the list.
* Cosmos, 40Ar/39Ar, U-Pb LA-ICP-MS, SIMS
* The optimal mix depends very much on the tool and the degree to which process routinization is possible as well as the economy of scale. For U/Pb via SIMS and LA-ICPMS, and cosmogenic nuclides via AMS, multi-user facilities are logical and have been quite successful. For systems requiring more iterative and often sample-specific analytical protocols, such as Ar/Ar and U/Pb TIMS, it is important to maintain single-PI (or limited multi-PI)facilities, as these tend to be those in which innovation occurs.
* As an example my lab houses 3 mass specs and all the supporting hardware to provide data as a facility. My lab lacks the manpower to utilize each instrument at the 24-7 level, but support as a facility could add the people necessary to yield the highest amount of data per instrument to better serve the needs of the community. I believe at least one ultrahigh precision facility is necessary in either or both the 40/39 or U/Pb TIMS areas.
* I think a mix that is proportional to external visitor usage is appropriate. I do not think prioritizing NSF funding for one analytical method vs another, instrument types, or single-PI vs multi-user facilities should be the criteria used for funding support. I think emphasis should be placed on innovative research projects and method development.

QUESTION 9: Would networks of single-PI labs be an effective means of providing geochronologic data to the user community?

YES: 16

NO: 4

QUESTION 10: What would be the benefit of having centralized national facilities that provide expertise in more than one chronometer?

* NSF should pursue this goal by supporting collaborative research within the existing scientific infrastructure, not by attempting to manufacture a centralized system that is driven by throughput rather than innovation.
* It depends critically on the particular series of chronometers. I can imagine some chronometers would benefit, but that it would limit others.
* Standardize the sample preparation processes and measurement Quality control
* Would certainly enable more effective multichronometry studies
* The application of multiple chronometers in one facility allows for more interaction between geochron PIs from different chronometers on a problem, which is arguably better than separate labs providing data to the user, who may not have the expertise to interpret data sets from multiple chronometers.
* Intercalibration of different methods; development and standardization of best practices; training new practitioners (or updating methods of existing practitioners); critical mass to resolve new questions through the combination of multiple chronometers.
* Academic evaluation mechanisms being what they are, running a service laboratory that produces random ages embedded deep in some paper where chronology plays a minor role is unlikely to get the lab manager the kind of recognition that will be much appreciated in an academic geoscience department.
* Perhaps users can be more efficiently trained and enough personnel would be available to assist with sample preparation, equipment maintenance, data collection, data reduction, data interpretation and preparation of manuscripts.
* In principle having multiple capabilities at a central facility provides opportunities to combine chronologic systems in a more comprehensive fashion. In reality, it's a pain to travel elsewhere to collect even one type of data, let alone multiple types. It doesn't make a difference to me if I have to travel one place for one type of analysis and another place for a different type. This seems like a low priority. Also, it would be rare for a central facility to host the best analysis across the board. I would rather travel to, 3 different labs in the country that are the best in their respective techniques, than use a central facility that likely isn't the best in some areas.
* While working on a project that uses multiple phases as chronometers, I suspect gathering data from the same lab creates a bit more continuity.
* It would be one-stop shopping, convenient for the gollygeologist customer, but likely to generate a lot of uninteresting regional formulaic observations and waste scarce resources that could be better spent on more interesting problems.
* Probable benefit to logistical efficiency, i.e., samples less likely to get lost in the shuffle.
* This would be the best educational opportunity for students that are using multiple chronmeters. Being surrounded by experts during lab visits is valuable for all.
* As a graduate student working in a highly specialized lab, I can think of a major drawback, namely that if I were instead to have to use a centralized facility that I would miss out on a lot of analytical training that comes from working in house versus being a visiting user.
* Benefits would be increased efficiency and more interdisciplinary research projects. Ability to tackle research questions with appropriate method.

QUESTION 11. From the geochronology producer community perspective, what, if any, are the "bottlenecks" in providing data to the wider user community? (for example, is it one of the following: inadequate sample preparation facilities, inadequate support for staff, the cost is too high, expertise is unavailable for the particular problem, or other issues).

* inadequate sample preparation facilities, inadequate support for staff, the cost is too high
* In principle, useful data should be made available to the general public in a timely manner. This is only possible if NSF provides support in several areas that are currently ignored. We desperately need more technical support staff at universities, as active researchers are already over-committed and do not have time to maintain these databases. NSF's own funding process emphasizes publication records in the same existing journals that do not foster widespread dissemination of data. The current scientific journal model is clearly broken, and NSF should commit both funds and reform of the proposal review process to the goal of support open source publication of both data and the scientific papers that result from NSF grants.
* The bottlenecks are: sample preparation, standardization, intercomparability of laboratories, staff costs (High, and mostly paid by non-NSF funds), universities increasingly applying additional costs above overhead and other similar issues.
* Maintaining a steady support for staff in order to keep 'corporate memory' and avoid boom & bust cycles. Costs can be prohibitive but can be alleviated to some extent by sponsoring graduate students if turn around time is not critical.
* inadequate sample preparation facilities inadequate support for staff the cost is too high expertise is unavailable for the particular problem
* Mostly inadequate sample preparation facilities and inadequate support staff
* Limited person power. Modern instruments are capable of incredible throughput, but particularly for geochronologic techniques that require mineral separation and especially dissolution and chemical separation prior to mass spec analysis, a lot of people have to be involved. Current NSF grants simply do not allow the maintenance of a 10-12 person staff in a single-PI lab.
* inadequate staff support, and the cost of providing data to the wider community is too high .
* Definitely not sample prep facilities. There's always issues of staff support, which in turns creates higher costs (staff salaries are expensive and that cost is passed on to users). For some types of analysis that are fairly routine (e.g. DZ, but components of many other systems), expertise isn't a big issue. But for other types of analysis, where chronology needs to link into other scientific aspects (e.g. petrology), figuring out even how to approach a problem requires huge time investment, and ultimately slows research.
* Cost discourages higher risk investigations
* Lack of compelling, interesting projects proposed by the wider user community.
* Sample preparation is often a bottleneck. Inadequate funding for technical support for maintenance and repair creates excessive down-time. High-level data analysis and interpretation is often an issue as PI's are often overcommitted and unable to devote adequate time to large volumes of data.
* Most for my lab and likely all its inadequate support staff to handle multiple projects in a timely fashion. In my lab, 2 soft money technicians and two PI"s couldn't keep up with one mass spectrometer, let alone the 3 we now have.
* high cost, long waits for instrument time, lack of well-characterized and widely distributed age and trace element standards

QUESTION 12: Would establishment of commercial geochronology laboratories help meet the needs of users?

Rate:

(1): 5

(2): 3

(3): 2

(4): 3

(5): 0

NO: 10

**Topic #3: Developing synergies between disciplines**

QUESTION 13: Do the existing modes of funding geochronology at NSF adequately promote/facilitate cross-disciplinary research?

* No, NSF should establish permanent structures for basic research into problems in geochronology. This would include establishing something like EARTHTIME as a permanent program with a wider emphasis, and placing more importance on innovation.
* I would say the current model is pretty much broken. The whole thing needs to be rethought
* Probably not, but if would mean a further reduction in the funding for research that is not considered cross-disciplinary to enhance it, then I'm not sure that it is in the best interest of the community to further its focus.
* Not necessarily. In many cases, researchers in other fields do not fully appreciate the complexity encountered in most geochronology efforts. Often we receive inadequate samples and the researchers may have inadequate funds to do the job right. Then, of course, there is always the problem of researchers who aren't themselves geochronologists facing required budget cuts and (quite naturally) choosing to cut the geochronology budget rather than the budget to do their own work (e.g., field work, grad student support, etc.)
* Depends whether you consider geochronology as a tool or research discipline in its own right. A geologist who needs 10 zircon dates to understand the stratigraphy of the area they are mapping likely doesn't want or need to know the details of the technique used. On the other hand, someone who specializes in geochronologic techniques likely would not be interested in all the different types of regional applications to which the technique can be applied.
* Not really- For example, geochronologists should be able to obtain funding through Earthscope, but the funding has largely gone to the geophysics community. And the lack of funding for technical support seriously hampers any efforts towards promoting/facilitating cross disciplinary research.
* Yes. The work I do is highly cross-disciplinary, and I have never had difficulties getting chronologic components of my research funded.
* Yes, for the most part. However it is not always easy for geochronology PI"s to formulate the questions of importance to the disciplines.
* the onus is on the proposal PI's and co-PI's in my opinion

QUESTION 14: If you direct a single-PI geochronology laboratory, what incentives exist for your participation in cross-disciplinary research? What disincentives do you have, if any?

* The incentives are a. participating in a large multi-institution team brings funding that might not otherwise be obtainable, b. cooperating with a larger group is beneficial and c. some common goals might be achieved. Disadvantages are (from my own experience in a large multi-pi group) that a few labs do most of the work.
* Scientific curiosity is probably the greatest driver for cross-discipline research. The added time required familiarize oneself with another discipline and the issues of obtaining funding can be disincentives.
* I try to keep costs for external users as reasonable as possible, but it is impossible to charge what it really costs to do the work if you accept (as I like to do) visiting students to work in the lab. It costs considerable research staff salary to help these folks, so I find myself limited in the number of collaborative studies I can sustain. Another major problem is that most such users do not have sample preparation or mineral separation facilities at their institutions and want to do that work in our facilities or ask our staff and students to do it. That can be a major burden because few external users can pay additional appropriate charges for mineral separation and sample preparation. Commercial firms try to pick up the slack, but are sometimes slow to get the work done.
* Best practices/cleanest chemistry; improvement of own methods and ability to interpret data; incorporation of other types of chronometers to address questions that might be intractable with only one.
* I do a lot of cross-disciplinary research because that's where I think the exciting science topics are. Although geochronologic techniques are central to my research, I pursue and develop them in order to solve multi-disciplinary science problems that are the main driver of my research effort. The incentives are that NSF and NASA apparently like the cross-disciplinary research efforts I do, so I get funded.
* There are no incentives to participate in cross-disciplinary research. Disencentives- It is impossible to establish recharge centers for the types of analyses performed. It is not cost effective to engage in contract work for academic users because of the high cost of salary support for laboratory personnel.
* Well, the only reason anyone goes into science is to learn new stuff, and cross-disciplinary research does that most effectively. The only disincentive is that you can overcommit to exploratory projects, but that's easy to control within a lab.
* 1. doing good, interesting science 2. crafting a compelling proposal to get funded
* Incentives- ability to participate in interesting research. Disincentives- lack of time, technical support.
* Main incentive is that it is intellectually gratifying to see where geochronology can support other research areas. No disincentives come to mind.

QUESTION 15: Given that geochronology is often a central requirement in a broad range of studies, should each of the NSF core programs have the provision to support geochronologic method development?

YES: 16

NO: 5

* There should be a dedicated program to support this goal, otherwise it will continue to receive inadequate attention from funding bodies. The application of geochronology is vital to the geosciences--no credible geology study can exist without understanding the chronology of the events involved--but innovation in geochronology is underfunded and increasingly under pressure as funding bodies and universities emphasize quantity over quality in analyses and publications.
* I think this is a good idea - however, I think "core programs" tend to have a constituency, and their core mission is to support their core constituency. The current model isn't good at allowing cross-disciplinary study.
* Sustaining grants to laboratories that have a history of innovation.
* Alternatively there could be a separate method development program to address these needs.
* This potentially seems like an additional opportunity for cost sharing across programmatic boundaries
* NSF core programs are split between topic (e.g., tectonics) and technique (geophysics, geochemistry) focus. Most geochronologic method developments arise from geochemistry, and that's where they should compete for funding.
* I do spend a lot of time developing methods but only because each new project requires tailoring analysis to the problem that needs to be solved. So in that sense, I feel like it's my job as a scientist to do this. I'm not sure how to justify methods development as a scientific goal, except possibly in the context of instrumentation. And there's already support for that.
* Geochronology should live and die by its ability to attract funding and support through the merits of scientific problems proposed by individual PIs. It shouldn't need special provisions or subsidies. But, I do think that NSF programs should support the best science that comes to them--if the reviewers and panel like it enough it should be funded. Certain areas should be neither disqualified nor targeted for special
* Method development that is highly specific to certain NSF programs should be supported through those programs.
* I&F supports equipment but generally not analysis to support method development. Therefore single PI"s are generally left to boondoggle costly and time intensive measurements.

QUESTION 16: Within your area(s) of geochronologic expertise, do you feel the needs of the broader earth science community in interdisciplinary studies are being met?

YES: 13

NO: 8

* NSF should specifically fund methods development projects at a much higher level. These studies are vital to advancing the capabilities of the field, which is a much more important long term goal than measuring a large number of dates using stagnating techniques. Unfortunately, lack of staff support, short-term research funding, and emphasis on high publication rates all retard this advance.
* There have been various attempts made a interdisciplinary studies with mixed success. I think this would be an important goal in future NSF facilities, science and technology centers and similar structures.
* The capacity in terms of lab space, expertise, instrument time is there in most of the labs that I have knowledge of. Funding and time to make use of the capacity is often the limiting factor.
* It's very hit and miss.
* Yes, but the weird geochronologic techniques I pursue are not of wide interest to the "broader earth science community".
* I feel that studies worthy of attention from geochronology labs are getting the attention they deserve. Just because someone can't get Ar dates for their pet regional karaoke geology project in Bumblekastan doesn't mean there is a problem.
* Most of my colleagues in applied fields would say no. However, I maintain that the really exciting, first-order, "transformative" projects have little problem getting service from the geochronology producers. It is mainly the more parochial projects, or those of less obvious import and/or promise, that are underserved.
* Really yes and no. For what we can do in the time we have we are meeting the needs, however the needs far out pace the current infrastructure.

QUESTION 17: Do the existing funding opportunities for interdisciplinary research promote adequate geochronologic training of non-specialist (i.e., applications-oriented) researchers and do they promote adequate training of graduate students who specialize in geochronology?

YES: 7

NO: 12

* Most working geochronologists lack adequate training in both the nuclear chemistry that underlies the techniques and the mass spectrometry that is vital to their application.
* At our lab, we have always encouraged graduate and undergraduate researchers to learn our lab techniques and processes. This is done on a voluntary basis, and frankly there is little funding to support such efforts. An REU program or GREU program that really addressed these issues might be a good idea.
* Most of our external users come to us with little understanding of the methods and concepts.
* Not sure I understand the question. Some "non-specialist" users of geochronology don't really need to understand the technique beyond knowing what the age means, and how to choose a sample that will give an interpretable result. My impression is that graduate students specializing in geochronology get adequate training in the technique, although more and more instrument automation is making it difficult to train students in the basic physics/electronics of an instrument and what to do when it breaks.
* It really depends on the level of funding available. If students are funded on research assistantships through grants they can be successfully trained. If students are supported on teaching assistantships, they have insufficient time to specialize and to be trained.
* I don't think that we should confuse applications-oriented and interdisciplinary research. Many applications are anything but interdisciplinary.
* These are two different questions. 1) Adequate training of non-specialists: More could be done here by having workshops in applied venues- those I've participated in have been very successful and well-attended by "consumers". One caveat- we need to avoid overselling our capabilities, as this builds unrealistic expectations, i.e. of accuracy.

2) Training of graduate students specializing in geochronology: This is not well-funded, and should be better supported although there are limited career options for pure geochronologists and care must be taken to avoid producing them beyond the employment capacity of academia, government and industry. My feeling is that the pipeline is not yet full.

* For the most part no. We encourage visitors, but time is always too short to fully engage them. Here is where well-staffed national facilities could contribute better than single PI labs.

**Topic #4: Supporting geochronologic innovation**

QUESTION 18: What do you perceive to be the most significant barriers to innovation in geochronology and how might they be overcome?

* I do not see significant barriers to innovation in geochronology
* The current NSF funding model is a disaster for innovation in geochronology. Funding for methodological research is woefully inadequate, and when it does come, it involves timescales too short for genuine exploration, and onerous reporting requirements that elevate rapid dissemination of mediocre science over the careful, deliberative processes that have historically led to important breakthroughs. This model needs to change.
* In the US, the main problem is the cutbacks in funding at all levels of government, and the lack of any political will to fund basic research. This is also true in some other countries. Countries bucking this trend such as China, Korea and Australia are moving ahead aggressively.
* In 'lean' times getting funding for innovative projects can be very difficult. Not sure how to overcome this, if everyone is in 'survival' mode it is difficult to sell the idea that a new or different method is worth developing as it would be one more competitor for limited resources.
* Lack of statistical model to interpret dating results High cost long processing and measurement time
* Limited funds for the acquisition of multiple mass spectrometry systems so that researchers can devote adequate time to method development while still getting data from standard methods.
* The lack of understanding in other geological fields for the importance of method development (e.g. standards, decay constants). Many see potential advances on this front as incremental, and thus do not understand their importance for the highest accuracy and precision geochronology.
* Too little funding. When geochronologists built their own mass specs, they had technical staff that included electronics engineers and machinists as well as lab technicians. As we shifted to buying instruments, we became more reliant on instrument manufacturers to dictate to our community what an instrument was capable of - and luckily they have pretty far reaching vision for the most part - but on this path, we have given up much of the expertise to truly drive instrument development. For example, who do I go to if I want to develop a cavity ion source so that I can get better than 1 ppm data on tens of micrograms of sample run at many nano amps? NASA said no.
* The limited number of mass spectrometer manufacturers is a significant barrier. Barriers can be overcome with sufficient funding made available for R&D, by training the next generation of geochronologists, and by ensuring that sufficient time is made available for PIs to engage in innovative experimental design and construction.
* Probably the biggest hurdle is people support. Our technical staff are so completely committed that there's little bandwidth to innovate. BUT, there has to be a dedication to innovation to make any progress. Just because a lab is busy doesn't mean that providing additional tech support will lead to innovation. Many busy labs develop an analytical protocol and never deviate from it.
* I guess I don't think there are barriers to innovation in geochronology, besides the tendency of many Earth scientists to not care about as much as they care about using "proven" techniques to hammer away at regional tectonics problems. I think that innovation worthy of support generally gets support.
* Funding
* For the most part innovation is done on the side and sometimes is driven by a specific scientific question. We don't have the time or the luxury to sit back and just think about and develop new innovative measurements or applications for them.
* Personal bias.

**Topic #5: The role of geochronology in transformative geologic research**

QUESTION 19. Because knowledge of time lies at the heart of geology, every significant advance in geochronology has resulted in a major breakthrough in our understanding of Earth. The earliest age measurements over one hundred years ago (Boltwood, 1907) immediately catapulted the discussion of the age of our planet from a few 10’s of millions of years to billions. The ability to date young basalts (McDougall and Tarling, 1964) permitted calibration of the geomagnetic time scale that led directly to the plate tectonic revolution. These are but two examples of the power of geochronology to transform our understanding of Earth. Having an exhaustive enumeration of the ways - big and small - that capturing the absolute time dimension has revolutionized our science would permit us to make the most compelling case for possible for enhanced support of geochronology to government agencies. In the space below, please provide examples that come to mind in which your dating method has changed the scientific perspective of another field.

* U-series and U-Pb dating of hydrogenic fracture-coating minerals at Yucca Mountain indicated very slow and constant long-term average rates of deposition thus implying hydrologic stability of the site.
* Thermochronology has revolutionized our understanding of the dynamic and complicated nature of tectonics and erosion.
* I can give 2 examples: 1. Radiocarbon dating has transformed archaeology and related fields by providing a time scale where only conjecture and intercomparison of structures, pottery and other subjective tools were used earlier. 2. In the last 20 years, the field of geomorphology has similarly been transformed. Cosmogenic nuclides have given a time-scale to a field that was much more subjective in the past.
* Patterson's 'Age of the Earth', 1955 is the first that comes to mind.
* Reveal the spatial and temporal patterns of climate changes. Landscape evolution
* Low-temperature thermochronology and cosmogenic radionuclide dating have revolutionized our understanding of erosional processes and timescales in active orogens. The integration of thermochronology and petrology (both igneous and metamorphic) have revolutionized our understanding of the timescales of petrologic processes. Geochronology and thermochronology of a variety of target materials have enabled a better understanding of the timing and frequency of bolide impact events in the inner Solar System, something that has implications for the early history of Earth and the Moon as well as the tempo of biological evolution.
* The 40Ar/39Ar method has had transformative effects on the fields of paleoanthropology and archaeology, especially in East Africa, which had added a great deal to our understanding of human origins.
* When I was a graduate student (ok, a really long time ago), good ages for early solar system events came with precisions of 20-50 Ma. Almost everything interesting (except Moon formation) happened within the first 5 Ma of Solar System formation. We know that now because short-lived, and U-Pb, geochronologic techniques were developed to the point of producing sub Ma age precision. These dating revelations have driven a variety of astrophysical studies aimed at understanding planet formation. Improved geochronologic techniques have allowed us to find a rock record older than 4 Ga that has provided a completely unexpected answer to the surface environment of the early Earth with huge implications for the origin of life. Improved age precision on flood basalt eruptions took them from being long events associated with continent breakup to almost instantaneous events needing a novel geodynamic driver, and with huge consequences for extinctions. I could go on and on...
* A few ( of many) fields that have been impacted: Paleoclimate studies- e.g., dating ice cores, Hominid evolution, Early Earth's evolution, dating impact events and meteorites Archeology and paleoanthropology Radiometric and exposure age dating of the Martian surface dating landscape evolution
* 1) Chronologic microanalysis. The identification of Hadean zircons in early Earth (e.g. Bill Compston's work) and, at the other end of the spectrum, young monazites in the Himalaya (Mark Harrison's work) have totally changed our perspectives on Earth processes. 2) Links between mineral chemistry and chronology (petrochronology). This has been a game changer in petrology and tectonics, elucidating processes and discriminating models with unprecedented detail. 3) LA-ICP-MS. The ability to rapidly obtain chronologic and chemical data has provided new perspectives on all applications of chronology to sed-strat, igneous-metamorphic petrology, and tectonics.
* I certainly agree with the first part of the minimanifesto. But I'm not sure mandating "enhanced support of geochronology to government agencies" will help anything. In fact I suspect it will lead to oligotrophic plutocracy favoring a few large labs with lots of money, but those labs will be obligated to do a lot of boring stuff. Where is the point in that? This is not the question you asked though. I feel that the method I work on [(U-Th)/He] may have, on a good day, increased our collective perspective on using geochronology creatively, to address a wider range of problems using a versatile technique, to address questions that sometimes we weren't previously aware were even questions.
* 1) Elucidating the time scale of human evolution.

2) Clarifying the nature and causes of mass extinctions.

3) Establishing the timescales of volcanic processes at regional and local (i.e., volcanic hazards) scales.

4) Clarifying the history of variations in Earth climate.

5) Clarifying the history of Earth's magnetic field variations.

6) Clarifying the association between ore deposits and other geologic processes.

* Current ultrahigh precision measurements are providing rate information with unprecedented resolution for evolutionary processes related to mass extinctions.

I will add more as time permits ...

**Comments**

* I think the problems in delays in NSF proposal funding, after positive review - which have become chronic - need to be discussed
* How can the community best pool their resources to ensure the long term viability and sustainability of geochronologic facilities in the US? How should software and hardware upgrades best be funded?
* We need readily distributable chronologic reference materials for microanalysis. That should be handled through a central facility, e.g. as NIST and the Smithsonian currently do.
* Thanks for providing the opportunity for the community to discuss interesting questions.
* There are many calibration and intercalibration issues beyond decay constants that should be addressed.

Should NSF support routine round-robin interlaboratory comparisons? Should NSF support production and distribution of standards? Should these be "certified" by some higher authority like NIST? Should NSF require standardized data reporting for NSF-supported work? Should NSF fund periodic workshops for specialists to discuss these issues?

* This is partly my own personal quest, however intercalibration of laboratories is a major problem. If we remain unable to compare data between labs we will continue to fill the literature with data that cannot be used relative to the stated precision.

Researchers who received this questionnaire:

|  |  |  |  |
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| Elizabeth | Catlos | UT Austin | U-Pb |
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| Andreas | Moeller | Univ. Kansas | U-Pb |
| Brian | Monteleone | WHOI | U-Pb |
| Paul | Mueller | Univ. Florida | U-Pb |
| Sam | Mukasa | Univ. New Hampshire | U-Pb |
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| Emily | Peterman | Bowdoin | U-Pb |
| Wayne | Premo | USGS | U-Pb |
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| Troy | Rasbury | SUNY Stony Brook | U-Pb |
| Matthew | Rioux | MIT | U-Pb |
| Jason | Saleeby | Caltech | U-Pb |
| Kyle | Samperton | Princeton | U-Pb |
| Scott | Samson | Syracuse Univ. | U-Pb |
| Mark | Schmitz | Boise State | U-Pb |
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