TESTIMONY BEFORE THE HOUSE COMMITTEE ON SCIENCE, SPACE, AND TECHNOLOGY

THREATS FROM SPACE: A REVIEW OF PRIVATE AND INTERNATIONAL EFFORTS TO TRACK AND MITIGATE ASTEROIDS AND METEORS

Michael F. A'Hearn
Professor Emeritus
Dept. of Astronomy, University of Maryland
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Mr. Chairman and members of the Committee, thank you for the opportunity to appear today to discuss the potential threats of near-Earth objects (NEOs) in the context of the NRC report on this topic that was issued in 2010. I was the chairman of the mitigation sub-panel for the NRC report, but today I am not representing the NRC, nor NASA, nor the University of Maryland.

The NRC Study: As mandated by Congress in the Consolidated Appropriations Act, 2008, NASA commissioned the NRC to study Surveys for Near-Earth Objects and Hazard Mitigation Strategies. The Steering Committee was chaired by Dr. Irwin Shapiro of Harvard University and the two sub-panels, one for Surveys and Characterization and one for Mitigation Strategies, were chaired by Dr. Faith Vilas, then Director of the MMT Observatory in Arizona, and by myself, respectively. The committee had a wide variety of expertise, ranging over the entire scope of the impact hazard problem. Several public hearings were held, with testimony from numerous experts, some of whom were advocates of specific projects while others were experts in impact prediction and risk communication, and yet others were policy experts.

The committee concluded that the money being expended at that time on NEO surveys was inadequate to meet the congressional mandate of finding 90% of potential impactors larger than 140 m on any reasonable time scale. The committee did not make a specific recommendation on the forward path, but described forward paths for surveys and discovery as a function of how much money Congress wished to appropriate to "buy insurance" against an impact. The amount of money to be appropriated would directly affect the timeline. The committee also recommended initiating a search for potential impactors in the 50-140-m range. The committee noted that there are basically four approaches to mitigation – evacuation for the smallest impactors, slow push-pull techniques, such as the gravity tractor, for moderately sized impactors with long warning times, and then kinetic impactors and standoff nuclear explosions for successively larger impactors and/or shorter warning times. A research program to better understand these mitigation approaches was recommended. Actual mitigation experiments in space were suggested, provided sufficient funding was provided, and overall programs were described for three different levels of funding.

The committee's report, Defending Planet Earth – Near-Earth-Object Surveys and Hazard Mitigation Strategies, was released in 2010. The remainder of this testimony concerns the details of some of these recommendations, both as recommended by the NRC and including my personal perspectives on the issues.

Impactors <140 meters: At the time of the NRC report, results newly published at that time indicated that previous modeling of impacts, by scaling from nuclear explosions of known yield, were incorrect due to the rapid downward motion of an external impactor compared to a nuclear explosion, for which the source can be considered to be at a fixed altitude. These results, which are still neither refuted nor explicitly confirmed, show that substantial damage can be inflicted by objects that are even smaller than 50 meters in diameter. To be specific, the new calculations suggested that the Tunguska event, which in 1908 flattened every tree over roughly 2000 square km in Siberia, was due to a body in the range of 30-50 meters diameter. Based on our knowledge of the size distribution of NEOs, that corresponds to an event that should occur roughly every century or two. For comparison, the best estimate of the Chelyabinsk meteor in February, which caused one building collapse and lots of broken windows with many people injured, is that it had a diameter of 15-20 meters, much smaller than any of the previous estimates of a hazardous size. The size of the Chelyabinsk meteor is better known than most since the trajectory has yielded a reliable velocity and the recovered samples can be used to infer the density of the body. Such an event should occur every several decades. Thus it is clear that objects much smaller than 140 meters are frequent and are capable of significant damage on Earth, although most of these impacts in the past went unnoticed because they occurred over the ocean or over very sparsely inhabited land areas. Detailed modeling of the effects of small impactors, say from Chelyabinsk-size to 140-m diameter, is a gap that should be filled, although most of the computer codes to tackle this problem accurately are under restricted access.

It is widely understood that small objects are much more abundant than large ones in nearly all the populations of the solar system, and specifically among the NEOs. Very roughly, a 14-m NEO is 1000 times more likely than a 140-m NEO. Thus the "next" significant impactor will most likely be closer in size to Chelyabinsk than to 140 meters. It therefore is important to plan for such an event, even if the hazard to life is small.

A key issue for the small impactors is that they are normally so faint prior to impact that we do not know how to detect them very far in advance. Many of them can only be discovered days to weeks before impact. Fortunately, this limitation coincides with the fact that the region of destruction by such an impactor is sufficiently small that evacuation (aka "duck and cover") is a realistic mitigation to minimize loss of life (but not property damage). You will hear about current efforts related to the ATLAS system from Dr. Yeomans and about the private venture to deploy the Sentinel system from Dr. Lu. All other things being equal, space-based systems offer a major advantage in principle, as long as the orbit is sunward from the Earth, such as at Earth's L1 Lagrange point, because it avoids the need for multiple sites on the ground. However, a cost benefit analysis must be undertaken that includes, limiting magnitude, wavelength range of operation, and the data processing approach. The ATLAS system alone is not sufficient for reliable detection because it consists of only two telescope systems, i.e., telescopes at only two sites, but it is designed to be sufficiently low in cost that other countries could realistically deploy similar systems, thus providing 24-hour coverage of both northern and southern hemispheres. The real issue then will be simply implementing the real-time coordination among the systems.

Programs at Various Funding Levels: The NRC report noted that any program dealing with NEO hazards as policy, as opposed to programs dealing with NEOs as scientific targets, should be considered as a form of insurance. The hazard is different from other terrestrial hazards, however, in that the insurance can be used to prevent damage rather than paying for restoration after damage. The question should be thought of, therefore, as a question of how much insurance the nation should by. The committee then described three different scenarios, depending on how much insurance was being bought, with rather arbitrary levels being chosen for the scenarios.

At a level of $10 million per year, the then operating survey programs could continue, as could a modest research program into issues related to the NEO hazard. This level would not meet the congressionally mandated George E. Brown survey to detect 90% of potential impactors larger than 140 meters in diameter.

I note that current spending in NASA's NEO program has increased to roughly $20 million per year, allowing some new initiatives such as the ATLAS program, operations of the PanSTARRS system (currently only one telescope but soon to be two telescopes), and research grants into mitigation related topics. Spending for the Large Synoptic Survey Telescope is not included in these totals – that telescope, if operated in NEO survey mode only, could meet the 140-meter goal relatively quickly.

At a level of $50 million per year, operation of a telescope such as LSST could be funded for NEO-optimized searches, although this assumes construction funding for astronomical research, *e.g.*, from NSF. Alternatively, an in-flight mitigation mission might be feasible if conducted as a minor part of an international partnership.

At a level of $250 million per year for a decade, the advanced surveys to 140 meters could be completed, either from the ground or from space, and a unilateral mitigation experimental mission would be feasible.

None of the NRC's recommended funding levels addressed the question of impactors smaller than 140 meters. With current technology, late detection appears to be the only feasible approach. Limits for the Sentinel system are not readily available to me, nor are the actual limits of the ATLAS system so I cannot comment on their relative contributions. The NEO program office at JPL has funded an independent study to assess the capability of the ATLAS system.

One also needs to remember that, once the George E. Brown survey to 140 meters is complete (90%), the remaining unidentified impactors include both the smaller impactors and the long-period comets. Although the long-period comets very rarely impact Earth, cumulatively they are likely to lead to as many or more deaths as the much more frequent small events. They have been ignored up to this point because they have been such a small fraction of the total threat, but that situation will change dramatically. One has to decide whether to deal with the small, frequent events or with the rare, large events, or both, analogous to deciding whether to deal with frequent auto accidents or infrequent large airliner or ship accidents or both.

International Cooperation and Collaboration: International collaboration is very important in the entire effort to deal with the impact hazard, from discovery, through impact prediction, to mitigation. Unfortunately, despite considerable discussion at the individual scientist level and considerable discussion at the governmental level up to the United Nations, the U. S. is the only nation with a funded, active and effective survey/discovery program. Canada has just launched (February 2013) and Germany will soon launch a small satellite designed to discover sub-populations of NEOs, but the U.S. is still the predominant nation in funding an active program for tracking NEOs, both through the JPL NEO Program Office and through funding the entire operation of the Minor Planet Center that is nominally sponsored by the International Astronomical Union.

It should be pointed out that the only terrestrial impactor ever predicted in advance was 2008 TC_3, an impactor much smaller (roughly 4 meters) than the Chelyabinsk meteor. This was discovered less than one day before impact, by R. Kowalski at the Catalina survey, based in Arizona. The impact was predicted only because the Catalina survey included a (NASA-funded) telescope in Australia in addition to the telescopes in Arizona, which allowed very rapid follow up data, and it was the combination of data from both telescopes that allowed the rapid prediction of the impact, including a prediction of the time and location of impact, both of which were extremely accurate. Thus an internationally distributed, and closely interactive, network of telescopes is critical for predicting small impactors. Fortunately, 2008 TC_3 was so small that it caused no damage on the Sahara Desert in northern Sudan where it entered Earth's atmosphere, although small pieces were subsequently recovered days later.

The area in which international collaboration is even more important is mitigation, due largely to the fact that incorrectly changing the orbit of a potential impactor could merely move the impact site from one country to another, with obvious international implications. Even the Chelyabinsk meteor was claimed by a fringe politician in Russia to be an American weapons test, but fortunately the Russian Academy of Sciences was in the forefront of public announcements, clearly declaring that this was a natural meteor. Unfortunately, there has been even less international discussion on this topic than on the survey/discovery/prediction topic, although there have been discussions within the UN's Action Team 14 of COPUOS. This is an area in which international collaboration, not just discussion, must be established before action is needed.

Contributions of Basic Research to Detection, Characterization, and Mitigation: There is considerable overlap between basic scientific research on comets and asteroids, *i.e.*, on the bodies that include NEOs, and policy-based work on the issues of hazard prediction and mitigation. However, the focus is very different between the two areas and consequently there are significant activities that are not included in one focus or the other. It is for this reason that NEO hazard activities require a separately identified source of funding, associated with national policy, that is not taken out of the scientific programs.

The research activities related to surveys and discovering bodies are aimed at finding statistically significant samples to enable interpretation, and these were the precursors of the specific hazard surveys, which are aimed at discovering as close to all of the objects as is practical (widely being taken to be 90% of the estimated total population). The research surveys, coupled with the work of dynamical researchers studying the orbits of the bodies, are what led to the recognition of the scale of the hazard and many of the individuals involved in those surveys are also involved in the hazard-driven surveys.

Research activities are also directly related to mitigation, but clearly distinct from actual mitigation planning. One of the key issues in mitigation, and for that matter even in predicting the scale of the damage from an impact, is to understand the physical properties of the impactors. Research programs using remote sensing have shown unambiguously that there is a wide variety of physical characteristics among the NEOs, ranging from likely coherent bodies that are the source of iron meteorites through really porous cometary nuclei that are likely to have been the source of the dinosaur-killer K-T impact 65 million years ago. Remote sensing can study a large number of objects and they are sensitive primarily to surface properties of the objects, to their size, and in some cases to a crude measure of their shape and their density.

Important, detailed characteristics of the NEOs can only be learned from *in situ* studies and PI-led, competitively selected missions, under NASA's Discovery and New Frontiers programs, provide the key mechanism to carry out these studies. Such missions can only be used to study a very few targets for budgetary reasons. A team led by Mike Belton and myself proposed the Deep Impact mission to the Discovery Program many years ago purely as a scientific mission, with only two sentences in the proposal about the possible peripheral benefits for NEO hazard mitigation. What the mission did for hazard mitigation was to demonstrate active targeting to impact on a small body, the nucleus of comet 9P/Tempel 1 (a technique needed for our science but also a technique needed for mitigation) and it also demonstrated the very porous nature of cometary nuclei (probably 10% of NEOs are inactive cometary nuclei). The observations of the ejecta were used both to determine the bulk density (much empty space inside!) and to estimate the momentum transfer efficiency of the impact as relatively low (roughly 2), a critical parameter for altering an NEO's orbit with a kinetic impactor. The mission also showed the challenges of attitude control in the last minute of approach to a cometary nucleus. These results have been presented to various groups directly concerned about mitigation, such as the Defense Threat Reduction Agency. The results of the subsequent flyby of comet Hartley 2 as part of the EPOXI mission showed the diversity among cometary nuclei and the heterogeneity from place to place on a single nucleus, both of which must be taken into account in mitigation.

The OSIRIS-REx mission, scheduled for launch in 2016, is a very different mission to a different type of NEO, the asteroid 1999 RQ_{36}. This mission will return a sample of the asteroid to Earth for detailed analysis, but while at the asteroid it will also produce, for example, a detailed map of the gravity. In addition to the material properties learned from the returned sample, gravitational mapping can be used to understand the internal structure of the asteroid, critical information for understanding how to mitigate by changing the orbit, whether by kinetic impact, or nuclear explosion, or even with a gravity tractor, which depends less on the physical structure but does depend on the bulk density and the shape.

These competitively chosen "research" missions are not sufficient to completely address mitigation, but they provide most of the necessary information on the range of physical properties one might encounter. Unfortunately, the NASA budget for planetary exploration has been such that NASA's Discovery program (competitively selected, PI-led missions with a cost cap of $425M in the latest round), have been devastated compared to even a decade ago. The NRC's recent decadal survey of planetary science recommended that NASA's priorities should be first to maintain a cadre of good researchers, and then to maintain a regular cadence, averaging a new start every two years, for the smallest missions (the Discovery Program), then the New Frontiers program (similar to the Discovery Program but for missions twice as expensive), and finally flagship missions (center directed missions that have lately cost more than $2 billion). Although not every mission in Discovery and New Frontiers is relevant to hazard mitigation (the most recent selection in the Discovery program is a mission to Mars), restoring Discovery to the originally intended cadence of research missions would significantly help with the mitigation effort by ensuring the existence of other missions to comets and asteroids to provide information necessary for mitigation.

Ultimately, however, specific mitigation missions must be considered as discussed above under program levels. They should be funded over and above the research program and they could be either separately funded add-ons to scientific missions or stand-alone missions, or international collaborations, with the international collaboration a high priority. Note that once the range of physical properties is understood, it is still very difficult to determine the physical properties of an actually threatening NEO without sending a mission to it, a possibility with very early discoveries but not with late discoveries.

What Should be Done in the Event of an Identified NEO Threat? After an NEO threat is identified, the initial steps are well defined. NASA is the lead agency for identifying threats and they have a reporting path through the U.S. government that covers all relevant federal agencies and the POTUS. Reporting to other countries is also urgent and should be done through the U.N. in order to reach all governments. In addition, there should be direct communication with countries and international agencies that have relevant capabilities for mitigation. Immediately following the alert, it is crucial to share all available data publicly. This is routine for the positional observations of the NEO and for the resultant orbital computations through the Minor Planet Center and through JPL's NEO Program Office. Beyond this, however, it is crucial to share all available information on the physical characteristics of the NEO from whatever source and on the details of the impact prediction. In the case of 2008 TC_3, which presented no hazard, this information was communicated through the channels normally used worldwide by astronomers and information was made readily available to news media.

The next steps depend critically on the nature of the threat – how big the impact will be, how far in the future it will occur, and where it will occur. An all-out effort to determine the characteristics of the particular impactor is crucial – remote sensing being needed in any case and, if time permits, a mission to characterize the NEO should be initiated in order to optimize the mitigation. Short warning times, however, may preclude an advance characterization mission and in that case the range of expected properties must be used to design a fail-safe mitigation. Action paths are, to my limited knowledge, not yet in place domestically. For a small impactor, a plausible route is through FEMA. For a larger impactor, however, either the military or NASA might be the one to take charge. For truly large impactors, the lead country and agency should be coordinated among those countries that have the capability to execute any mitigation. This decision/action tree should be fleshed out and made publicly available long before any specific threat is identified.