GOALS AND PRIORITIES FOR THE STUDY OF CENTAURS AND TRANS-NEPTUNIAN OBJECTS IN THE NEXT DECADE: A COMMUNITY WHITE PAPER

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Abstract

We discuss the primary science questions involving Centaurs and trans-Neptunian objects (TNOs), and the motivation for their study from the ground and with spacecraft missions in the coming decade. Two areas that must continue to be supported are laboratory studies of ices and organics and telescopic characterization surveys of TNOs and Centaurs. Such work is vital if we are to adequately design and plan space missions, since we have only just started to understand (a) these small bodies' context within Solar System formation and evolution, and (b) their genetic links to other populations of small bodies. The long-term prospects for space missions require investment in technology development to enable long-duration missions in the outer Solar System. We provide a consensus view of which feasible, space-based studies would return important science and broaden our understanding of Centaurs and TNOs.

I. Subdiscipline Overview

The study of Centaurs and trans-Neptunian objects (which we will collectively refer to as TNOs) has progressed rapidly since 2000. Recent years have seen a great expansion in the number of known objects, increasing five-fold from the beginning of the decade to over 1340 currently (late 2009). These small bodies carry dynamical and compositional clues to the history of our Solar System – they are the less-processed (but not pristine) remnants of the icy building blocks that formed the outer planets, and their current orbital distributions are marked with the fingerprints of the planetary formation environment. In general, the study of TNOs provides insight into some of the most fundamental questions of planetary science: How did the Solar System achieve its current dynamical structure? How do small bodies change as they evolve (collisions, radiogenic heating, aqueous alteration, etc.)? How was water and organic material distributed in the early Solar System? Astrophysically, TNOs are the closest, most-observable analogues to the growing inventory of debris disks observed around other stars; around 10 to 20% of main-sequence, solar-type stars surveyed by the Spitzer Space Telescope (e.g. Trilling et al. 2008) have such disks and so could have significant but unseen small-body populations of their own.

Over the last decade, the more immediate goals of TNO science have been to (a) discover and determine orbits for as many of these objects as possible and (b) learn the basic characteristics and ensemble properties of this hitherto unexplored population. The study of TNOs has already provided valuable and (in some cases) unexpected scientific insights about the nature of the group itself. For example: simulations of collisions and dynamical evolution indicate that 99% or more of the original population is no longer present; the census of objects shows that the trans-Neptunian region is populated by at least two different subpopulations. Furthermore, TNO studies have given us a deeper understanding of the connections between this group and the rest of the Solar System. For example: the apparent dynamical structure in the trans-Neptunian region suggests that the giant planets migrated significantly from their formation locations (possibly in a chaotic manner); surveys of surface properties reveal some of the genetic links between TNOs, Centaurs, comets, water-rich asteroids, and Trojans. With all these answers came many more questions, but the next decade of outer Solar System studies promises a revolution in our understanding due to an unprecedented amount of data expected from high-étendue sky surveys, continued studies of individual objects, and the New Horizons mission with its forthcoming flybys of TNOs.

Jupiter's orbital semimajor axis is 5.2 AU. For the purposes of this document, we consider any object with perihelion distance greater than this (that is not a dwarf planet and not a Jovian Trojan) to be under our purview. For the sake of economy, we refer to such objects collectively as TNOs even though a small subset (some Centaurs) is in fact cis-Neptunian. Exploration of Jovian Trojans and icy dwarf planets is discussed by Grundy et al. (2009) and Britt et al. (2009).

II. Top-Level Scientific Questions

Most investigations of TNOs ultimately seek to place these objects in the proper context of our own Solar System. The overarching questions that drive these studies can be broadly grouped into physical, compositional, or dynamical categories.

A. What are the Physical Properties of TNOs?

What is the size distribution of TNOs , and how does the distribution change at the smaller sizes? What physical processes dictate the shape of the size distribution? What are the masses and densities of TNOs? How have major collisions changed the bulk structure and rotation states of these objects? What is the tensile strength, and on what length scale? How many objects are in multiple systems? How do physical properties differ between a parent and its satellites? What are the mechanisms for creating multiple systems?

What is the interior structure of TNOs and how does that structure change with size? How is energy transported through the interior? What are the thermal conductivity, heat capacity, and porosity of these bodies? What are the relative strengths of insolation, endogenic heating, and structural changes (e.g. water-ice crystallization) for determining the interior temperatures? To what depths has significant heating occurred? How and when does cometary activity start in Centaurs (and potentially in all TNOs) and how does it differ from the kind of cometary activity seen in inner Solar System comets?

B. What are the Compositions of TNOs?

What is the bulk composition of TNOs, and how do the materials seen on their surfaces reflect that composition? What are their colors and albedos? What is the relative abundance of volatiles and water ice, and how does the composition vary as a function of other object properties? What are the isotopic abundances, and how do they differ from such abundances in other parts of the Solar System? How are icy and rocky material mixed inside these bodies – and at what length scale? How are different ices mixed?

Why are TNOs in the cold, classical population apparently different in observed quantities from the rest of the TNOs? How did compositional gradients in the solar nebula and protoplanetary disk influence the final properties of the forming TNOs? Are there “snow lines” in the protoplanetary disk for multiple volatile species and does that produce observable effects? Are compositional difference driven by formation rate versus dynamical timescale, so objects forming closer to the Sun formed fast enough to get ^{26}Al, or to have formed in a gassy nebula, while things further out accreted slower and thus had different materials to draw on?

C. What Physical and Chemical Processes affect TNOs, and How?

How much does original composition influence the colors and spectral response we see today, and what are the dominant processes that would have changed these quantities? What do reflectance differences among bodies tell us about their different histories? How important are surface chemistry, cometary activity, impact gardening, and endogenic heating (i.e. cryovolcanism) for objects of various (small) masses? What causes the variation in color between objects in the same dynamical subgroup?

Where are the organics in TNOs? Why do we see organics on some surfaces (e.g. Davies et al. 1993) and not others? Is this variety primordial? What does cosmic-ray exposure do to various original mixtures of ices and organics (e.g. Grundy 2009)? How do collisions change irradiated surfaces? Is there a common origin for dark red-colored material in the Solar System?

What ices exist on the surfaces of TNOs of various sizes? Though CH_4, CO, and N_2 are likely too volatile to still exist on the surface (Schaller and Brown 2007), at what layers below the surface do these volatiles still exist? Are there mechanisms (such as recondensation or collisions) for volatile transport between surface and subsurface? Are more refractory volatiles identifiable on TNO surfaces? Since crystalline ice is now seen in TNOs (e.g. Jewitt and Luu 2004), what processes (e.g. irradiation, collisions) determine the crystalline-to-amorphous water ice ratio? What role did liquid water play (if any) in the compositional development of the TNO interiors?

D. What is the <u>Dynamical Structure</u> of the Trans-Neptunian Region?

What are the relative populations of the resonant objects, the cold and hot classical objects, and the scattered disk objects (including the Centaurs)? How do TNO orbits (in all dynamical classes) evolve on Myr and Gyr time scales? What did planet migration do to the original mass in this region? What are the dynamical pathways for TNOs to become Centaurs and thence inner Solar System Jupiter-family comets, and how efficient are these paths? How often are Centaurs captured by giant planets? How do collisions change the mass and structure of the trans-Neptunian region?

III. Required Research and Research Facilities

A. Assets that Address Top-Level Science Questions.

Laboratory work, numerical modeling, and telescopic work are all required to address some of the top-level science questions mentioned above. Laboratory experiments can create TNO analogues and use them to constrain the chemical and radiative processes affecting outer-Solar System surfaces. Computer simulations of the interiors of TNOs provide insight into the objects' thermal and compositional evolution. The major issue with modeling TNOs however is the lack of constraints on the composition of some of the volatiles expected in these objects. Ammonia hydrates, methanol hydrates, nitrogen ice, methane ice, clathrate hydrates are suspected to be present in these objects, but their relative concentrations are not well constrained even from cosmochemical models. In addition, the potential geophysical role of organics that could be in liquid form in the conditions of pressure and temperature expected in medium-sized TNOs has also been poorly explored. For these different compounds there are few, if any, measurements of thermal, physical, and mechanical properties in the conditions of pressure and temperature relevant to TNOs. Such experiments are relatively costly because they require work at very low temperature, but would very much improve the modeling of TNO evolution.

Many groups have ongoing projects designed to study as many TNOs and Centaurs as possible. However target faintness and limited telescope time mean that the sample size grows slowly. In particular, the smaller TNOs and Centaurs are especially understudied. Nonetheless, surveys of a large number of objects are invaluable in overcoming some of the current low-number statistics that plague phenomenological studies of TNO and Centaur properties. Ground and space- based long term monitoring of bright Centaurs like e.g. 29P/ Schwassmann-Wachmann 1 also provide new clues on some of the physical processes going on in their surfaces (Trigo-Rodríguez et al. 2008). There are simply too few objects with known orbital parameters and even fewer with known colors, albedos, reflectance spectra, rotation spins, shapes, etc. Large databases would help to refine and answer top-level questions by giving us statistical clarity.

B. Assets that Define Cost-Effective Missions.

Related to the point in the previous paragraph is the forthcoming explosion in the number of known TNOs and Centaurs. All-sky surveys such as Pan-STARRS and LSST are expected to yield thousands more outer-Solar System objects, and the facilities will observe them often enough to yield accurate orbits. This improved census of the trans-Neptunian region will not only nullify the poor-statistics problem, but will also provide a cornucopia of interesting and feasible spacecraft targets at – even more importantly – a range of sizes. Active objects experiencing outgassing, photometric outbursts, etc., should be a priority as these processes can identify objects having relatively fresh surfaces.

These surveys will operate at visible wavelengths and perform only imaging. For further characterization, expansion of observations in both the wavelength regime and time regime is necessary. In particular, we note that much of the characterization will require continued access to large ground-based telescopes. Follow-up of the targets with spectroscopy, when feasible, will especially require consistent access to large ground-based facilities. Mid-infrared observations to understand thermophysical properties will certainly require air- or space-based telescopes such as Sofia, Herschel, and/or JWST. But all such characterization observations will allow for more informed choices about (a) mission targets and (b) what experimentation to use at the target.

C. Context for Mission Data and for Maximizing Science Return.

Continued laboratory work is a vital component in planning the acquisition of and interpreting mission data. In particular, obtaining better optical constants for various relevant compounds is an important line of research.

D. Data-Archiving Needs.

Finding an appropriate and scientifically exciting target for a mission requires having a baseline understanding of what is “typical” within the TNO population. We must have broad knowledge of colors, spins, shapes, surface structure, and compositions. To this end, it is important that a publicly-available archived database of TNO properties exist.

IV. Technology Needs

Several technological advances would facilitate *in situ* exploration of TNOs and maximize scientific return. Some ideas are discussed below. In many cases, there may be a need for flight readiness assessment, but this could be done (for example) as technology demonstrations through smaller programs such as SmEx or CubeSat. Note that many of the technology needs described here would not solely apply to Centaur and TNO missions.

A. Propulsion.

Rendezvous missions and multi-body tours will frequently require low-thrust propulsion for success at reasonable mass and cost. Additional investments in this technology are required to ease operational difficulties, with extended and improved mission and trajectory planning systems. Visits to TNOs will likely require nuclear power systems, and this would facilitate having missions to multiple targets.

B. Automation.

For many conceivable missions, onboard/autonomous navigation tools would need to be developed. Such reactive, autonomous command systems will enhance science return, reduce operational costs, and provide for greater mission reliability. For example, for a spacecraft encounter with an object (a) whose size is not *a priori* well-constrained, and/or (b) whose orbit calculation does not have the benefit of a long, historical observing arc, the error ellipse on approach will potentially be large. Thus a great deal of operations time will be needed for navigation imaging. Autonomous on-board systems would do much better and update the encounter sequence in real time based on what appears in the approach imaging.

For many autonomous systems, technological issues would have to be solved, especially if such systems were (e.g.) on a lander. Neither utilizing surface maps for navigation nor using altimetry to determine target relative position (as opposed to simply measuring time to contact) has been done extensively. Contact itself could be a challenge for an autonomous system because of difficult-to-predict torques and possibly complex ascent strategies.

C. Telecommunications.

Depending on the distance, science return may be limited by bandwidth. The strategy that NASA chooses to resolve mission communication bandwidth limits may require some technology development, depending on the solution. Extending the life of the Deep Space Network 70-m antennas may be the simplest but most costly development to support deep space missions. Concepts such as very large arrays of small (5-m) antennas are attractive from a cost and data return standpoint but provide no radiometric navigation capability, necessitating greater investment in interferometric or optical navigation methods in deep cruise. Optical communication links offer tremendous downlink capability, and theoretically offer navigation capability, but this is as yet unproven, and would have to be reliably developed.

D. Instrumentation.

Landers and surface probes must be relatively small and often have restrictive mass and power constraints. Future use of such probes will require improved miniaturization technologies. Instruments that are vital to the science goals of a mission may need to be shrunk.

For some mission targets, fast imagers of wide field-of-view for use in low-light, non-in-situ environments will need to be developed. Lightweight medium-range LIDARs and/or altimeters for close proximity operations will need to be created. Use of structured light systems can provide a means of using visual imagers for direct ranging. Gimbaling of sensors will greatly enhance science return by decoupling to a great extent the image planning process from the engineering needs of spacecraft body pointing, and appropriate low-cost and lightweight software and hardware systems should be developed. For surface operations, dust may be a problem for imagers, so reliable dust prevention or elimination technologies need to be developed.

V. Major Mission Priorities

A. Flagship class missions.

Any outer Solar System flagship mission should be required to fly by serendipitous Centaur or TNO targets that fall within a specific Δv window. This means that any such mission must have an extra mass, power, and cost margin included from the start. When an outer Solar System flagship mission is selected, and a nominal trajectory identified, dedicated surveys should be undertaken to find objects with orbits that may take them within the vicinity of the spacecraft. Active Centaurs like e.g. 29P/Schwassmann-Wachmann 1 would be particularly valuable targets for these types of missions.

In addition, flagship missions to the giant planets should be required to fly by some of the planet's irregular satellites. Since these bodies are likely captured outer Solar System objects, they probably formed from the same pool of planetesimals as the TNOs did. Visits to irregular satellites could be as scientifically valuable as a visit to a TNO or Centaur itself. Buratti et al. (2009) discuss future exploration of these bodies in more detail.

B. New Frontiers class missions.

The known variety of objects among these populations warrants designing missions that will sample this variety and thus provide excellent context for wider-ranging Earth-based studies. We will potentially begin to sample the variety with New Horizons's visit to Pluto in 2015 followed by a flyby of a still to be determined, but presumably small, TNO. Other mission designs would follow on this idea; for example, the "Argo" conceptual mission would fly by Triton (thought to be a captured TNO), and then take advantage of a gravity assist by Neptune to travel to another, scientifically-selected TNO. A very challenging but very useful New Frontiers-class mission would involve spacecraft visits to multiple Centaurs and/or TNOs. In particular, visits to a series of "typical" (i.e. small) TNOs would be invaluable. Such a mission would engender a concerted effort to identify accessible targets that epitomize the diversity of the trans-Neptunian region, although it would also require significant innovations in mission architecture and/or propulsion systems.

A variation on this idea involves a mission that samples objects from multiple Solar System populations. For example the "Shotput" mission concept (Klesh 2009) is a design for a New Frontiers mission that would fly by a main-belt asteroid, then a Trojan (in this case contact binary Hektor), and then a Centaur (in this case 39P/Oterma). The spacecraft would also release impactors that would collide with the Trojan and the Centaur, thereby sampling more-pristine subsurface material.

VI. Discovery Science Goals

Space-based platforms could be used to observe stellar occultations by small TNOs. Such observations would let us measure the size distribution and heliocentric radial distribution of the more distant TNOs and potentially of the Oort Cloud. Such a mission could use mature technology and be similar architecturally to Kepler, but be optimized for detection of small bodies in our own Solar System.

VII. Balancing Priorities

Our knowledge of TNOs requires a consistent ongoing investment in the acquisition of time on large-aperture ground-based facilities. Such investment is necessary if we are to conduct investigations and observations of physical properties that would enable us to identify spacecraft targets that are optimal for addressing specific science issues. Furthermore such work is crucial for providing an adequate context for the interpretation of spacecraft observations of TNOs. Since New Horizons's (post-Pluto) TNO flyby in the late 2010s is very likely to involve a small (~10-50 km) TNO, ground-based studies of a broad size range of TNOs are of particular importance to maximize science return from the New Horizons mission. New mission opportunities, which are likely to be a portion of a mission payload, should have no negative impact on the funding of the above basic research as no mission will obviate the essential need for this research in support of missions past, current and future.

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