Our Ethical Obligation to Planetary Science in the Age of Competitive Space Exploration

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I. Introduction

While the importance of scientific knowledge and understanding is widely acknowledged, their value tends to be assumed rather than championed, which presents difficulties for prioritizing the role and focus of planetary science in an increasingly competitive space exploration environment. We apply lessons from the academic study of ethics to planetary science, concluding that it has value both inherent to itself and as applied to our society. We are therefore ethically obligated to support objectives that enable planetary science and increase scientific return.

There is urgency in the need to understand why planetary science is important: In space, science is no longer the only game in town. The growing capability of the private sector and maturing efforts by national space agencies in human exploration have the potential to disrupt, obstruct, or preclude science return from otherwise relatively pristine locales. Over the short term this includes Mars, water resources at the Moon, and certain near-Earth asteroids.

We advise the scientific community to consider factors outside the normal scope of science while prioritizing near-term planetary investigations, in particular, the potential for non-science spaceflight activities to disrupt planetary science goals, which in turn would disrupt the fundamental value provided to society by planetary research. These factors are particularly salient given the relatively modest pace of NASA's planetary program, which historically averages 8 missions per decadal period. Only a handful of new investigations will be possible to destinations that may be perturbed by other users in the very near future.

II. The Intrinsic and Inherent Value of Planetary Science

In order to understand our obligation to pursue planetary science, we must first establish its value. For this we turn to ethics—the academic discipline devoted to scrutinizing value claims. Recent work on the philosophy and ethics of space exploration (Schwartz 2020) provides fresh insights into the value of space science, both intrinsic and instrumental.

Something is *intrinsically* valuable when it is worth pursuing or protecting for its own sake. In other words, it adds value to the world simply by existing. One reason to believe that planetary science is intrinsically valuable is that it generates scientific knowledge, where this knowledge is worth acquiring whether or not it produces technological advancements. Additionally, we can base the intrinsic value of scientific knowledge on the idea that acquiring scientific knowledge is a part of a life well-lived. The more opportunities to pursue planetary science, the more people who are able to discover, share, and consume this new knowledge, improving the intellectual well-being of society.

Something is *instrumentally* valuable when it serves to achieve some further end or goal. One reason to believe that planetary science is instrumentally valuable is because of the general instrumental value of scientific knowledge. Given the centrality of science to all facets of modern life, we have a rudimentary obligation to *care about learning about* the basic facts of our universe. This is something we must do in order to successfully address other matters that we care deeply about, since scientific research is the ultimate societal “problem solver.”

The relationship between basic research, applied research, and societal progress is typically understood linearly: advances in basic research reveal information that finds its way into applied research, which ultimately comes to benefit society at large (Bush 1945; Munévar 1998). However, this relationship is not always unidirectional. Sometimes “applied” research turns up anomalies that call for revisions to “basic” science. Interesting questions that form the bases of new and worthwhile investigations can emerge at any point in the spectrum from basic to applied research. Planetary science stands out because it is unusually holistic in terms of its utilization of the entire scientific apparatus. Since it is nearly *always* putting both our knowledge and our technological systems in new and extreme environments, it is inherently likely to turn up interesting anomalies, to put existing ideas and theories through rigorous “stress tests”, and to create the conditions for technological and societal innovation (Munévar 1998; Schwartz 2020). For instance, the field of biology, as currently interpreted through a single example of abiogenesis, is perhaps the most profound area for potential innovation should life be discovered elsewhere in our solar system (Dreier 2020b). Similar bounties await from planetary science investigations of all kinds.

Moreover, it is increasingly the case that space and planetary science informs smart policy development and improves democratic governance (Schwartz 2020). Early exploration of Venus and Mars helped warn us about the extreme outcomes of global climate change. We depend on Earth observation for national security, economic development, weather forecasting, and climate studies. Strong basic scientific research acts to “support the democratic culture, broadly conceived, simply by satisfying intellectual curiosity or offering aesthetic pleasure” (Brown and Guston 2009), something planetary exploration provides in excess with its revealed splendor of the solar system.

Finally, planetary science will become increasingly instrumentally valuable as national efforts to send humans to the Moon and Mars mature; and as the private sector becomes increasingly capable in resource extraction and, ultimately, human space settlement. In this context, planetary science investigations will become essential, particularly in setting standards for human health and wellbeing in varied planetary environments.¹

Planetary science stands out in both its intrinsic and inherent value. Its exploratory nature and young age (the field is less than 60 years old), ensure that each new project will generate significant amounts of new knowledge. Given its technologically demanding nature, it requires and generates great skills and advanced processes to acquire this knowledge. It has a near-peerless potential to test and validate planetary-scale theories originally developed with the single data point of Earth (Lapôtre 2020). And it becomes ever more relevant as competing actors pursue non-scientific missions to scientifically interesting destinations.

III. Competing Actors as a Risk to Scientific Return

The foregoing remarks lend substance to the claim that there is an obligation—ethically speaking—to engage in planetary science based on its value. What we have yet to make obvious,

¹ Consider, for example, the discovery of the reactive perchlorate chemistry in the martian soil (Hecht 2009), or the potential health consequences of inhaling lunar and martian dust (Krisanova 2013).

however, is the priority of planetary science (and space science generally) compared to other uses of the space environment (e.g., national security, Earth observation, telecommunications, commercial development). A challenge facing planetary science, unique to other NASA sciences—is the growing capabilities of the private sector, in particular those focused on in-situ resource utilization (ISRU), resource extraction, and human exploration. SpaceX, in particular, is designing a massive rocket from the ground-up to carry humans to Mars expressly for permanent settlement. Permanently shadowed craters that retain water ice at the lunar poles, the most astrobiologically interesting locations, are the target for future commercial exploitation. And though the prominent asteroid mining companies went bankrupt, NASA is investing in private entities with an eye for deep space resource extraction.

There is no legally-binding framework that provides expansive protection or preservation of scientific destinations in space. Thus, at some point in the not very distant future, non-science focused space entities could engage in spaceflight activities that disrupt, obstruct, or preclude future science return. The need to think proactively about protecting planetary science goals should be a familiar one, especially among those aware of the history and discussion surrounding COSPAR’s planetary protection policies (Kminek et al. 2017). What is less often recognized is that the need for protection extends beyond astrobiology and the search for evidence of extraterrestrial life: *The potential for conflicts among spaceflight activities exists for space research generally*. While this point has been raised in a small number workshops and in select space policy publications, an official path forward remains uncharted (Rummel et al. 2012; Elvis, Milligan, and Krolkowski 2016; Krolkowski and Elvis 2019; Matthews and McMahon 2018; Newman and Williamson 2018; Schwartz 2014, 2016, 2019, 2020; Schwartz and Milligan 2017; Ehrenfreund et al. 2013).

Given these emerging conditions, it is not sufficient to assign priority to investigations *solely* in reference to traditional scientific considerations or solely based on the stated priorities of the scientific community. **The value of an investigation must be treated as a function of how tolerant the program is of perturbations from other actors.** Research proposals that can only be accomplished effectively *prior* to the onset of perturbing activities (e.g., local or global disruptions from a sustained human presence or development activities) may need to be assigned a higher priority than they might merit in other circumstances.

Not all destinations face similar disruption or are of similar scientific value. In all likelihood, near-future private space exploration and human exploration will be focused on inner solar system destinations, in particular Mars, the Moon, and a subset of accessible near-Earth asteroids. Therefore, a starting point for the scientific community would be to consider how stated ambitions by private and public entities could impact future scientific work at these destinations. This concern applies to any scientific investigation whose findings could be rendered hopelessly ambiguous through environmental disruptions caused by others (Vondrak 1974, Almár 2002, Cotts and Hummel 2009).

This is an especially troublesome possibility given the paucity of our understanding of these environments. Even if some manner of a “space environmental impact assessment” is

implemented (Kramer 2017), this would only tell us how a space environment has been perturbed. We would still need to speculate as to the long-term *consequences* of such a disruption, which could deny irreplaceable scientific knowledge (which, recall, has its own innate value) to every single generation of humans yet to be born. As many planetary processes are unconstrained, it is not clear how we might go about reliably making such predictions or weighing the loss of yet-to-be discovered knowledge.

We acknowledge that asserting pre-eminent scientific priority to the solar system is both impractical and futile, given that government regulators and private entities have no legal requirement to consult with the scientific community before pursuing their missions. But if one concurs with the claim that planetary science is, by itself and as a practical measure, *valuable*, then we are compelled to act—to the degree possible—on that condition.

IV. The Ethical Obligations to Planetary Science

If an ethical obligation to pursue a spaceflight objective exists, then, all things being equal, some amount of good would come from satisfying this objective (Schwartz 2020). Given that planetary science creates good in a society, and assuming societies are bound to “do good” on behalf of their citizens, we can consider this criterion met and that this obligation exists. Two important conditions must be considered, however, in order to provide any hope of prioritization given the vast number of activities that rise to this level of societal obligation:² that it is possible in the first place to do an activity, and that doing this activity is a justifiable use of energy and resources.

Consider a counterexample. A Martian settlement is clearly a societal obligation in the extreme long-term, if we accept that the continued existence of the human species as a “good” (which we do). But we currently lack the technological means to send humans to Mars to stay for indefinite periods, face no immediate (external) extinction-level crisis, and would bear an unprecedented cost to undertake that effort, even taking into account the more optimistic predictions many have made about future hardware capabilities. Arguably, given our additional conditions, we are not *currently obligated* to pursue off-Earth human settlement, even though it is an important consideration in the far future.

Pursuing planetary science, on the other hand, is obviously possible with current technology or minimal technology development. It also is fantastically affordable, with total planetary science costs (research, mission development, and management) averaging a mere 0.13% of annual discretionary expenditures by the United States government since 1973³ (Dreier 2020a). That fractional expenditure led to unprecedented insights into the formation of our solar system, extended the sciences of geology, atmospheres, dynamics (to name a few) to other worlds, and expanded the concept of habitability beyond a single “zone” of temperature ranges around a star to ocean worlds hidden in the outer reaches of the solar system. Even a modest increase to that

² Obviously, there are many important obligations that exist beyond the scope of space science. For this paper, we take as a given that space exploration is a legitimate societal activity. Not just for the reasons stated, but by the fact that democratic societies have elected to pursue space exploration activities in every year since it became technologically possible to do so.

³ The first year of the post-Apollo era, when NASA became, according to an official White House policy statement, “a normal and regular part of our national life” (White House 1970).

average expenditure would lead to an increase in “good activities” without significantly impacting other worthwhile programs. Planetary science, given its inherent and intrinsic value, its ready implementation, and modest cost to the nation, thus meets the expanded criteria to consider it an ethical obligation worth pursuing now.

Additional considerations relating to future external perturbations now come into play. While ambitious goals of commercial and human exploration to Mars, ISRU, resource extraction, and settlement may appear distant in time (or even impossible), the relatively modest rate of planetary science projects means that time pressure is more immediate than it might initially appear. Since 1973, NASA’s planetary science program launched an average of 8.4 planetary missions per decadal period,⁴ peaking at 13 missions between 2003 – 2012 and bottoming out at a mere 3 from 1983 – 1992 (Dreier 2020a). These missions required, on average, 6.3 years of development work before launch—with high variance in transit times to their scientific destinations. Absent a technological breakthrough or seismic political shift, both averages are unlikely to change dramatically in the near future for government-funded, scientific missions.

Suppose, in something of a “best case” scenario (for planetary science), that SpaceX’s plans for Mars settlement are wildly ambitious, and it falls behind schedule by 30 years. Suppose also that, in this same period no new asteroid mining attempts are made, and NASA-supported lunar ISRU efforts and human missions to the Moon and Mars do not materialize. In this extended window, NASA will likely launch a mere 25 planetary science missions with the option of exploring (largely) pristine environments. This is supposing that, in a period comprising a full third of the entire space-age, *all* ambitions by private and human spaceflight groups would fail—an outcome possible but by no means certain.

Additionally, NASA’s Planetary Science Division maintains (correctly) a programmatic balance in both destination and mission type, meaning that only a fraction of these 25 mission opportunities would be directed to destinations susceptible to perturbations by external actors. Will those handful of missions be enough to address the extensive battery of fundamental science questions that face possible disruption? What scientific investigations should be prioritized given this limited—and irreplaceable—opportunity? This external pressure compels us to consider specific locales of planetary science as a high-priority and immediate obligation. **The committee should consider this potential knowledge loss when prioritizing the scientific goals and missions for the coming decadal period.**

V. Concluding Thoughts

It is important to dwell upon the notion that planetary science is a societal obligation. We are not saying it is the only obligation, or even the most important, but it is a genuine obligation and fulfilling it would carry significant value—both intrinsic and instrumental—to our society. Planetary science provides opportunities to stress-test scientific theories originally developed within the single data point of Earth, provides relevant and useful data for future space exploration

⁴ The first planetary science decadal survey was released in 2002 and covered the years 2003 – 2012. Using this ten-year window, we can extend three “decadal-equivalent” periods back to 1973 for comparative purposes.

and human safety, and supports democratic values through its commitment to gathering novel data and generating aesthetic pleasure. If we are compelled to pursue activities that increase the societal good, then planetary science is a means to do so.

From this perspective, scientific investigations that face potential disruption from other types of exploration or utilization should be considered with new urgency. The community should seriously contemplate that it has only a handful of opportunities in the coming decades and take an expansive view of external pressures when prioritizing near-term scientific priorities. What scientific questions could be disrupted by human exploration of Mars or ISRU at the Moon or other asteroids? What types of missions could address these questions that can be funded, built, and return data in these time scales? The answer may be that these activities constitute a minor detriment to future scientific return. But it is important to consider these nonetheless, as their impacts may grow as capabilities mature in the coming decades.

For the same reasons, finding ways to increase mission cadence becomes even more important, particularly over the next few decadal periods. While the significant high-skilled labor demands of scientific exploration make it difficult to lower costs, there are new opportunities worth pursuing in smallsat science, ride-shares, and fixed-price public-private partnerships that could help to return more science more quickly. This, in turn, creates more value to society and captures baseline data from destinations potentially facing disruption by other actors. Moreover, there is no ignoring the fact that the more funding the program has the more it can do. No number of smallsats can replace a well-designed flagship mission (National Academies 2017). It is no accident that the lowest number of planetary missions, historically, coincided with historically low levels of program funding in the 1980s. Once agreement is reached on its priorities, the scientific community should vigorously engage with its representatives in government to increase the resources provided to this rare and valuable endeavor.

We wish to re-affirm that the concerns raised in this paper should play a greater role in the planetary science decision-making process. Possible strategies for accomplishing this include: devising criteria for classifying the manner and degree to which a solar system body or destination is at risk of perturbation from non-science activities (Almár 2010); devising criteria for classifying a research proposal on a scale which ranges from being worthwhile *only if* the target destination is pristine to being worthwhile *regardless of* the manner or degree of perturbations (Schwartz 2020); devising criteria for classifying research for its potential to constrain planetary processes so as to enable more reliable predictions about the impact of perturbations in the space environment (Schwartz 2019); and finally, utilizing all of this information, as deemed appropriate by the scientific community, in the process of assigning priority to planetary science missions in the decadal process.

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