The Greatest Missions Never Flown: Anticipatory Discourse and the “Projectory” in Technological Communities

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The Greatest Missions Never Flown

Anticipatory Discourse and the “Projectory” in Technological Communities

LISA MESSERI and JANET VERTESI

ABSTRACT: This article introduces the concept of the sociotechnical projectory to explore the importance of future-oriented discourse in technical practice. It examines the case of two flagship NASA missions that, since the 1960s, have been continually proposed and deferred. Despite the missions never being flown, it argues that they produced powerful effects within the planetary science community as assumed “end-points” to which all current technological, scientific, and community efforts are directed. It asserts that attention to the social construction of technological systems requires historical attention to how actors situate themselves with respect to a shared narrative of the future.

Introduction

Historians of technology have recently turned their attention to past representations of the future. Patrick McCray’s book *The Visioneers*, for example, describes “visionary engineers” whose individual technological aspirations took hold among groups of enthusiasts, whether or not the technologies were eventually instantiated.¹ Such an interest in past futures

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¹. Patrick McCray, *The Visioneers*. See also Joe Corn, ed., *Imaging Tomorrow*; Nik Brown, Brian Rappert, and Andrew Webster, eds., *Contested Futures*; Marita Sturken, Douglas Thomas, and Sandra J. Ball-Rokeach, eds., *Technological Visions*. 

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invites historians of technology to examine the unrealized technologies littering the archive as not simply technological failures or fantasies, but as material culture produced to maintain communities in the face of technological uncertainty.

However, future-making happens at different scales, by different kinds of actors, and in different institutional contexts. In this article, we focus on near-term future-making in a state-sponsored scientific and technological context: NASA’s planetary science missions. Rather than examining creative individuals with powerful visions, we turn our attention to the scientific communities who craft and sustain interest in a vision of their shared immediate future: a ten-to-fifteen-year projected future that revolves around the building of a particular instrument or technology. Significant to our analysis is the perpetual delay of purported touchstone missions. These delays, which community members frequently attribute to the shifting fortunes of budgets and congressional approvals, make apparent the social work required for sustaining a vision of the future.

From Imaginaries to Projectories

To flesh out the role of the future in technical work, we draw on a line of scholarship that focuses on the role of shared future-oriented narratives about technoscientific possibilities in such examples as state-formation, legal policy, and scientific work. Anthropologist George Marcus uses the term technoscientific imaginaries to describe how scientists reflexively incorporate national-level concerns or moral imperatives in their scientific work. Even at the lab bench, scientists participate in, comment on, or take a moral position on technoscientific debates and imagined prospects. In their comparison of U.S. and Korean approaches to nuclear power policy, for example, Sheila Jasanoff and Sang-Hyun Kim describe sociotechnical imaginaries as “collectively imagined forms of social life and social order reflected in the design and fulfillment of nation-specific scientific and/or technological projects.” They show how national policy about technological priorities also articulates national imaginaries about shared technoscientific futures, and the country’s (and citizens’) role in shaping and enabling that future. Overall, work in this domain shows how “anticipatory discourse” is entangled in nation-state building, community orientation, and technoscientific work.

Our interest is in how communities formally articulate this anticipatory discourse and use it to stabilize their technical and professional goals

4. For an application of ibid., see Martyn Pickersgill, “Connecting Neuroscience and Law.” Pickersgill takes this concept into the level of legal discourse, demonstrating how state concerns play out in the development of “neurolaw” in both the local and transnational contexts.
when developing ambitious technologies. We therefore ground the concept of the technoscientific imaginary in what we call a “projectory.” Projectory combines the momentum of trajectory with the forecasting of projection to capture historical actors’ own future-casting and the shared near-future goals of their current work. The projectory is a material instantiation of the “imaginary,” made concrete and traceable through circulated documents that codify a community’s orientation toward future technological states. In its barest form, the projectory frequently appears as a timeline, positioning a target technological system as the culmination of several intermediary systems and technological tests. Historically and sociologically, we argue, the projectory serves an important material-discursive role in the production of actors’ cohesive social worlds. Our analysis focuses on the ways in which historical actors describe and animate their work in future-oriented terms.

The cases we chose are notable because the projected technology—in this instance, the development, launch, and operation of a spacecraft—is in perpetual deferral. Although such long-term uncertainty need not accompany a projectory, technologies with precarious fates highlight the stabilizing role of the projectory in sociotechnical communities. Even if the greatest missions fail to fly or technologies are not built, there remain timelines and documents of projected futures around which people attempt to more securely structure both their immediate work and long-term career ambitions. In this broadest sense, technologies have not only a projectory, but also a community responsible for their mutual articulation. We therefore suggest that what holds communities together is the stating and restating of this projectory, more so than the final technology. To understand the historical role of the future, then, historians of technology must examine the relationships among a community, their projectories, and the resulting (if any) technological systems.

The NASA missions that we examine in this article are the Mars Sample Return (MSR) mission and the Terrestrial Planet Finder (TPF). They are “great” because the fulfillment of these missions promises to answer extraordinary science questions: “Was there ever life on Mars?” and “Are we alone in the universe?” Yet, they also present a paradox: although they are seen as touchstone missions and have well-articulated projectories that demonstrate their technological feasibility, the communities advocating for their launch have faced decades of disappointing cancellations and

5. By invoking the term trajectory, we do not wish to place undue emphasis on the actors’ retrospective accountings that construct a linear vision of a technology’s development. Many historians of technology have embraced Donald MacKenzie’s critique of such “natural trajectories,” favoring an understanding of technological change with an eye toward the institutional and social contexts. See MacKenzie, Inventing Accuracy; see also Florence Millerand and Geoffrey Bowker, “Metadata Standards,” and Kristian H. Nielsen, “Technological Trajectories in the Making.”
deferrals. Thus, part of these missions’ very “greatness” is inherent in the fact that they do not disappear.\(^6\) Despite their perpetual postponements, the missions are neither failures due to sociotechnical complexity nor are they inconsequential moments in the history of space exploration.\(^7\) Instead, even as the technology fails to come to fruition, their persisting trajectories shape technological development, career paths, and community membership.

Our purpose in this article is not to retell the stories of robotic Mars exploration and exoplanet astronomy in their entirety, nor is it to explain the intricacies of funding systems. Instead, we will present snapshots of turning points in these two fields from the 1960s up to the present to demonstrate how these communities continue to survive due to the persistent invocation of their trajectories.\(^8\) We review the missions in detail, as well as their implications for technology design, community participation, and funding.

**The Many Returns of Mars Sample Return (MSR)**

The principle behind MSR is simple: send a (robotic, typically) explorer to Mars, pick up some well-chosen samples of Martian rock and dust, geologically characterize the region around the samples, and then return them safely to scientists on Earth without any possibility of contamination. The practicalities and technological challenges behind MSR, however, are enormous. One significant challenge is how exactly to get those samples back to Earth. Such a feat would require establishing launch capabilities on the other planet, which are not only difficult to produce in isolation, but also add mass and volume to the initial spacecraft, requiring a bigger and more expensive rocket. This impedes a spacecraft’s ability to launch from Earth, let alone from Mars.

\(^6\) We do not wish to argue that these two missions are, in some actual way, “the greatest” missions of all time, nor that they are unique among all missions or Big Science projects. However, they serve as touchstone examples of the imagined not-too-distant future, with implications for our understanding of large-scale sociotechnical systems more generally—especially those that have been successively planned though never built.

\(^7\) On technological failure, see Charles Perrow, *Normal Accidents*; Diane Vaughan, *The Challenger Launch Decision*; and Alexander Brown, “Accidents, Engineering, and History at NASA.”

\(^8\) Our study draws on both historical and ethnographic materials to contribute to the history and sociology of science and technology. Both authors have spent several years (2007–12) immersed in the planetary-science, exoplanet, and astrobiology communities conducting sociological and anthropological fieldwork. In these sites, the scientists we spoke to kept returning to these missions, describing them as touchstones or essential aspects of their community’s aims, goals, and structure. Follow-up work on these topics through archival records and oral-history interviews revealed that such talk has been around for a very long time.
Lunar samples, returned by both American and Russian astronauts, proved formative to the development of the planetary science community on Earth during the 1970s. It is therefore unsurprising to find documents from the Apollo era of space exploration producing plans for sample-return missions to and from Mars. Such documents range from proposed statements of purpose to technical reports, each outlining possibilities for an MSR mission in the context of the early U.S. space program and its cold war exigencies. What is surprising, however, is that this mission never happens—yet, these documents do not go away. Tracing the appearance of the phrase “Mars Sample Return” throughout decades of archival materials demonstrates not that the same mission, imagined in the same way, continues to be denied funding; rather, the phrase becomes a recurrent touchstone to which the community continually returns over the next fifty years to refer to a mission that is always projected to be between eleven and thirteen years away from launch.

In 1962, NASA Marshall’s Future Projects Office, under the EMPIRE program, commissioned a series of reports from contractors, ranging from Hughes and Martin Marietta to TRW and a division of the Ford Motor Company, on the topic of manned interplanetary missions. Sample return was already part of this conversation. In these early technical documents, Mars missions, including sample return, were conceptually oriented in Apollo-like series, casting successive mission activities over the coming decade as contributing toward an imagined future project. Authors of these documents assumed that sample collection and return would be human-driven. As one of these reports put it, “[t]he Mars Mission appears to be rapidly taking shape as the direct follow-on from the Apollo project.” Another report, however, stated that “[t]here is much to be done before a Mars landing mission can be accomplished.” These reports struggled with many unknowns about the Martian surface. One concluded that “the ultimate goal of landing men on the other planets should be held as a long-range objective,” but in the meanwhile proposed “a logical first step” of a flyby mission that “if properly integrated into the overall space effort, such a mission could . . . lead directly to development of those areas of technology required for landings.”

9. On the divergent interpretations of these samples, see Ian Mitroff, “Norms and Counter-norms in a Select Group of the Apollo Moon Scientists.”
By the mid-1960s, NASA established a program called Voyager (later renamed Viking, and not to be confused with the later Voyager spacecraft) to take over from the Mariner missions, combining Mariner-style flyby technologies with Apollo-style landing technologies. Again, the program aimed to fly several spacecraft in series, each developing the technology that the latter craft would build on until human occupancy could be supported. Earlier spacecraft would provide reconnaissance in terms of imagery and atmospheric information necessary to safely land, relay information, and relaunch. The missions were projected to fly in sequence in 1973, 1975, 1977, and 1979 (fig. 1), each one developing the technology to be used on future missions into the 1980s (fig. 2). A *National Geographic* article authored by Carl Sagan in 1967 also described Voyager as a “Space age *Santa Maria,*” and estimated a timeline of 1974 for the mission’s “complex scientific systems.”

The fate of Voyager in the budget cuts of Vietnam-era America is well-known. As the project ballooned in size and scope, Voyager proponents struggled to streamline their mission goals and budget. At the same time, Congress debated how best to contain the state violence erupting in response to the civil rights movement and protests against the Vietnam War. When, in August 1967, the Marshall Spaceflight Center issued a call for proposals from its contractors for “Planetary Surface Sample Return Probe Study” for Mars and Venus, members of Congress vocally disapproved.

This moment speaks to the complexities of the budgeting and planning process. Developing scientific missions and large technological instruments can require up to twenty years of investment; yet in the United States, Congress only allocates money for public projects one year at a time. While the actual process of NASA’s budget allocation is complex and has changed over time, certain principles remain constant: each year, the NASA administration comes up with a proposed plan and budget (sometimes through soliciting input from its scientific and technical community members via the national academies), which it then submits to Congress and the Office of Management and Budget for approval. The budget is debated at a line-by-line level in the House and the Senate, and the government eventually allocates an amount that may or may not reflect the original request. Budgetary allocations to NASA therefore must take into account other federal-level commitments, whether wars overseas, defense spending, or social programs, as much as they do the interests of the space-exploration community.

Fig. 1. Voyager mission’s “plan for exploration of Mars” places the Mariner missions in sequence, to be followed by orbiters, landers, rovers, and sample return. (Source: “Summary of the Voyager Program” by NASA Office of Space Science and Applications, pp. 8–9, in Elliott C. Levinthal Viking Lander Imaging Science Team Papers, 1970–1980 [PP04.02], box 14, folder 27, NASA Ames History Office, Moffett Field, California.)
However, as technoscientific communities return again and again to the budget table, facing the yearly possibility of defunding or de-scoping their projects, the projectory is a way of mitigating uncertainty and producing community continuity despite resource instability. For example, the Viking missions of the 1970s adopted much of the original Voyager timeline and included sample return in their prospective final stages of development. At least one envisioning projected a mission timeline hosting four spacecraft on the surface of Mars over the course of a decade. Vikings 1 and 2 (ultimately, the only missions that launched) were only the beginning, bringing stationary experimental apparatus to the planet’s surface to test for biological properties in the Martian soil. The Viking planning team foresaw follow-up missions with roving vehicles in the late 1970s and early ‘80s, the last of which would include some capability for sample return. These missions usually appeared to the far right of any timeline diagram, positioning them as the anchoring missions toward which other missions were merely first steps. But they also became figurative futures, which could easily be downscaled as present technical concerns warranted shifting farther out to the right of the diagram and therefore farther into the future. Although there are references to a Vikings 3 and 4 in the archival material, these quickly disappeared from the record.17

17. Mention of Vikings 3 and 4 is made in the Levinthal Papers; see especially Summary of the Voyager Program.
These early instantiations of MSR featured missions projected in sequence into the future, with the hoped-for sample return repeatedly appearing as the goal eleven to thirteen years out. Throughout the 1970s, MSR remained a consistent goal for the Mars community. This is apparent in the reports of the Space Science Board, a division of the National Academy of Sciences, distinct from NASA yet tasked with setting a scientific agenda for planetary-science priorities. Every ten years, the board releases a decadal survey in which a committee—a group of experts drawn from the relevant scientific community—puts forth a prioritized list of missions for NASA to consider in its budgeting process. Unequivocally, in 1974, the Space Science Board recommended that MSR be “adopted as a long-term goal.” In particular, the board’s Committee on Planetary and Lunar Exploration (COMPLEX) restated this goal in its survey document for the period 1977 to 1987. In this document, COMPLEX asserts that “[t]he study of Mars is an essential basis for our understanding of the evolution of the earth and the inner solar system. To accomplish the scientific objectives, we recommend that intensive study of Mars by spacecraft be achieved within the period 1977–1987.” Summarizing its findings, the committee concluded that

[the advantages of examination in an earth laboratory of properly selected samples from an extraterrestrial body are so great that sample return should be considered one of the basic modes of intensive study of a planetary body. . . . As a result of this examination, COMPLEX recommends the following:

1. Sample return from solar-system bodies should be considered a mission technique within the framework of a continuing program of scientific exploration and not a terminal, long-term goal.

2. Studies should be initiated to develop the special technology for such sample returns.]

Alongside stating the central importance of sample return, COMPLEX placed this mission within the context of longer strings of missions, conceived of in a series and working toward an eventual goal:

In order to carry out an adequate program of exploration of the solar system, there will be a need to return several times to some planets with different spacecraft carrying different experiments, over a period of two decades. In this context, no single mission mode should be considered an end or terminal goal in its own right. Rather we view exploration of a planet as an organic program of investigations


including both in situ studies and studies on returned samples. Efforts must be expended to ensure that maximum advantage is taken of the interactive and mutually supportive natures of the various program components.20

Such an approach is concordant with the Apollo-esque vision that transferred to Mars-exploration missions through Voyager and Viking. When “no single mission mode should be considered . . . [a] terminal goal in its own right,” each mission becomes properly seen as one of a series, thus building a technological trajectory.21

Whether due to Viking’s high costs, NASA’s inability to fund missions conceived in series in the economic recession of the early 1980s, or the recurring of conflict between the United States and the Soviet Union, no missions flew to Mars during the ten-year period of 1977 to 1987 despite such strong endorsements from the planetary-science community. Yet, a variety of actors across NASA’s constitutive institutions continued to push for an MSR during the 1980s and into the ’90s as evidenced by technical- and scientific-study report documents that combine sample acquisition or caching technology with the technology to support a roving vehicle on Mars.22 For example, in 1984, the NASA Advisory Council, a subcommittee of the House Committee on Science and Technology, submitted a report to the House as an addendum to the 1983 budgetary appropriations for NASA in which it outlined the developments needed for future planetary missions. In this document, the council recommended that MSR should be part of both the “core” and “augmented” programs for continued exploration efforts. This subcommittee of scientists and engineers also articulated the “deficient areas of technology support” necessary to support an MSR mission, including the development of systems for “sample acquisition, contamination, sample storage, environmental protection and autonomous operation.” This would require attention to “several other challenging technology areas,” such as “robotics, rover technology, automated rendezvous, sample transfer, artificial intelligence for adaptive guidance and control and hazard avoidance, aeromaneuvering, aerocapture, and possibly in-situ propellant generation.”23 In the same year (1984), a Mission Requirements

20. Ibid.
21. Ibid.
22. Note that while science and technology studies scholars are reluctant to draw distinctions between the scientific and the technical, preferring instead the nomenclature technoscience, these are the actors’ categories distinguishing between the scientific and engineering sides of NASA as a bureaucratic organization. As we use the terms here, they refer to actors’ categories that designate institutional lines of support and argument.
Report described the MSR as an “[i]nterplanetary injection mission with return: the objective is to acquire samples of Martian surface materials and return them to Earth orbit to be retrieved by the shuttle or analyzed on the Space Station.”24 NASA approved a pre-Phase-A study prior to 1984, with its findings reported that year.25 A similar team delivered another report in 1986, subsequent to a call for a study in 1985.26 By 1987, the Solar Exploration Division of NASA convened a Phase-A study team of engineers from the Johnson Space Center (JSC) and Jet Propulsion Laboratory (JPL) to examine the possibilities and technical challenges for mission launch, landing, and return, projected for launching in 1998.27

None of these missions took flight, but such top-down articulations guided the vision for Mars exploration laid out in 2001, when NASA established a Mars Exploration Program Office. The missions that did fly, such as the Mars Exploration Rover and Mars Pathfinder missions, bore vestigial traces of an initial scope that included sample cache and/or return. For example, in a 1998 report describing the future Mars Exploration Rover mission, then projected to launch in 2001, several authors (who had participated on Viking and eventually became Mars Exploration Rover mission team members) describe the mission as having “sample collection” capabilities, such that samples could be “acquired and cached.”28 In 2001, sample-

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27. James R. Rose, “Conceptual Design of the Mars Rover Sample Return System”; see also Roger Bourke, Johnny Kwok, and Alan Friedlander, “Mars Rover Sample Return Mission.” At the First AIAA International Symposium on Space Automation and Robotics in Arlington, Virginia, in November 1988, the representative from the JSC team described the Mars Rover Sample Return (MRSR) mission as “the most technologically sophisticated robotic exploration mission ever undertaken by the United States.” He outlined the many technological innovations that would need to take place before the mission could fly, using language like “this approach requires a launch vehicle and upper-stage capability beyond that which is currently available and will not be available until the turn of the century.” A few weeks later, the JPL side of the team presented some of its findings at the AIAA Aerospace Sciences Meeting in January 1989 in Reno, Nevada. The papers describe four different mission-system configurations designed and studied to support “the requirements for orbit attainment and maintenance, Mars landing, sampling and roving, ascent, rendezvous, docking and sample transfer, and Earth return of the sample. . . . All configurations include a rover and an ascent vehicle on the Mars surface to acquire the samples and bring them to Earth orbit.” The presenters’ titles (mission manager, systems engineering manager, and mission design group supervisor at JPL and a senior analyst from a contracting space-technology company) indicate at least a base level of institutional support at JPL for the Phase-A study, including a contractual agreement with NASA to underwrite the study, a project line and office at JPL, and support for a mission-definition team. For more on JPL’s role in articulating sample-return projects, especially from the 1980s to the early 2000s, see Erik M. Conway, “Dreaming of Mars Sample Return.”
return missions were expected to launch in 2011 (fig. 3); indeed, the Mars Science Laboratory, launched in 2011, was at one time projected to have sample caching capabilities but was later de-scoped.\textsuperscript{29} NASA’s public presentations in 2012 and 2013 feature a “Fly, Orbit, Land, Rove, Return Samples” mission philosophy, including charts that extend this projectory across the solar system; the empty boxes on the right-hand sides of the charts indicate where future missions have yet to be articulated\textsuperscript{30} (fig. 4).

During the fifty-year period, 1962 to 2011, then, the sample-return mission repeats again and again as a discursive trope. Although the mission’s systems, potentialities, and formulations change at each iteration, the continuing use of MSR as a moniker blurs the differences among such formulations. This ongoing deferred gratification never put the scientific community off this goal; instead, it returned to the MSR with renewed fervor. Work on current and near-term missions is therefore articulated as “pre–sample return,” hence paving the way for the eventual necessary technology and setting goals for immediate research and development. It should come as no surprise that the 2011 decadal survey also featured MSR as the top-priority mission for its projected period, 2013–22.\textsuperscript{31}

For further evidence, consider this quote: “Next-generation Mars communications networks will provide communications and navigation services to a wide variety of Mars science vehicles including: spacecraft that are arriving at Mars, spacecraft that are entering and descending in the Mars atmosphere, scientific orbiter spacecraft, spacecraft that return Mars samples to Earth, landers, rovers, aerobots, airplanes, and sensing pods” (see Kul Bhasin et al., “Advanced Communication and Networking Technologies for Mars Exploration”). Further proposals and studies are available during the period from 1989 to 2001, each proposed by various prominent members of the community.

29. At the Seventh International Conference on Mars at Caltech in July 2007, a community member opened the science talk with the statement: “As we know, the MSL will now have a new task, a sample collection.” Further, the community was buzzing about an announcement by then–associate administrator Alan Stern’s proposal that he was “eyeing 2018–2020” as the next opportunity for sample return. This was treated as an official announcement by community members, one of whom explained in their talk, “[w]hat’s important is that Alan Stern and Jim Green have announced a stake in a ground for a Mars Sample Return in 2020.” At this point, the sample collection and sample return technologies were separated into two stages for technology development, each providing a future sequence for mission development to come. By 2009, the sample cache capability had been de-scoped from the mission.


31. The report, written by a committee chaired by the principal investigator of the Mars Exploration Rover Mission, put MSR at the top of the list of priorities, ahead of other missions in development that had already been selected for funding through NASA competitions. The language is similarly strong, recalling previous examples: “The major focus of the next decade will be to initiate a Mars sample-return campaign, beginning with a rover mission to collect and cache samples, followed by missions to retrieve these samples and return them to Earth. It is widely accepted within the Mars science community that analysis of carefully selected samples from sites that have the highest
FIG. 3 Missions in the Mars exploration program, envisioned in sequence in an official slide deck (ca. 2001). Note the sample return at the far right.
(Source: Courtesy of Roger Launius, Division of Space History, National Air and Space Museum, Smithsonian Institution, Washington, D.C.)
FIG. 4 Slide from the 25 October 2012 presentation at the Solar System Exploration @ 50 Symposium, given by NASA’s Planetary Sciences Division director, Jim Green, indicating the trajectory for Mars alongside the other inner planets. (Source: From http://www.nasa.gov/pdf/706594main_JimGreen.pdf [accessed 10 November 2013].)
“TPF is dead. Long live TPF.”

The Terrestrial Planet Finder (TPF) is a space telescope designed to look at our closest stellar neighbors and directly image and analyze Earth-like planets. In this context, an Earth-like planet is one with a rocky surface, an atmosphere, and signs of water and oxygen—in other words, one that has the potential to harbor life. Directly imaging exoplanets is rarely accomplished, and trying to image one that gives off as little light as a habitable planet only increases the difficulty. Engineers have proposed several different models for TPF over the years, ranging from a multiframe constellation system to placing a giant star-shade between the telescope and the star so as to block out the starlight in order to catch a glimpse of the orbiting planet. As with MSR, TPF has come to stand for not one specific technology or mission design, but a goal that can be accomplished in a number of ways. Since its inception in the late 1970s, the community has several times taken TPF, repackaged it, and placed it at the end of currently planned missions. The vocal community in favor of TPF remained visible to themselves, other astronomers, and NASA because of this constant articulation of a projectory leading toward the Finder.

In 1978, a Stanford University engineer, Ron Bracewell, proposed an interferometer as a feasible apparatus for detecting planets around other stars. Decades before such planets were known to exist, Bracewell’s short “Letter to Nature” outlined how a nulling interferometer—a system that uses the property of electromagnetic interference to block out starlight—would adequately disentangle the signal of the star from the accompanying planet.32 His idea gained traction in both the United States and Europe, and in the 1990 decadal survey written by the National Academy of Sciences’ Committee on Astronomy and Astrophysics, a project called the Astrometric Interferometry Mission (what would later be called the Space Interferometry Mission [SIM]) was listed as the fourth of five prioritized, moderate space-based programs.33 The committee suggested this program as a way to find planets around other stars, likely Jupiter-sized planets, by observing the wobbles of stars. The committee described this as a “technology-demonstration mission,” the first step along a projectory, and not the mission that would directly detect an exoplanet using interferometry.

In 1995, five years after this report was published, the field of exoplanet
astronomy changed drastically. Swiss astronomers Michel Mayor and Didier Queloz detected the first exoplanet orbiting a sun-like star in the constellation Pegasus using a detection method called radial velocity. Although this planet was nothing like Earth, it was the moment that astronomers had been waiting for: a chance to put pressure on funding agencies and be the first to detect a planet similar to our own. SIM, which already received a ringing endorsement from the decadal survey, could not alone find an Earth-like planet. Thus, NASA commissioned “A Road Map for the Exploration of Neighboring Planetary Systems” (ExNPS) in which selected astronomers proposed several possible designs for a space interferometer to NASA. The resounding conclusion was that by 2005 (a decade hence), NASA would be well on its way to developing a (yet-unnamed) space interferometer.

In 1996, NASA administrator Dan Goldin officially announced the agency’s commitment to finding an Earth-like planet. Speaking to the members of the American Astronomical Society, Goldin echoed President Kennedy’s moon-shot speech and promised the listening astronomers that NASA would launch a TPF and return an image of an Earth-like planet within a decade. The first principal investigator of TPF, Chas Beichman, recalled Goldin’s enthusiasm for the project: “Dan Goldin decided he wanted a new, beacon-like project for NASA and he thought, in a sense, that the Apollo pictures looking back at the Earth were very iconic for NASA. He said, ‘Now, we’ve done the picture of Earth, let’s go find other Earths.’” Goldin had made the search for exoplanets a priority since he became head of NASA in 1992, and even after he was no longer its administrator, he regarded TPF as the mission that could restore the wonder that America once had in NASA.

At the same time that interferometry was being developed in the United States, French astronomer Alain Léger was also developing a space interferometer targeted at finding an Earth-like planet. This European Space Agency (ESA) mission, called Darwin, was presented through ExNPS, as...
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well as a proposal that, if NASA and ESA both decided to fund a space interferometer, they should consider partnering. Although both sides were convinced that the projects were technologically feasible (though difficult), they also realized that this would be an enormously expensive undertaking.

Consequently, shortly after NASA began funding TPF and ESA began funding Darwin, a “Memorandum of Understanding” was drawn up between the two agencies that a space interferometer would be a joint mission. Thus these two separate missions effectively became the same project. Americans still referred to it as TPF and Europeans as Darwin, and often in documents it was labeled TPF/Darwin. At the time of launching one name would be decided on, but in the late 1990s and early 2000s those who worked on the project reported a fluid and easy partnership that spanned the Atlantic. TPF/Darwin was a feasible project, but one for which a series of precursor missions were necessary. Figure 5 illustrates well how the project was portrayed as a culminating mission at the end of a projectory.

When interviewing scientists involved with TPF and asking them to narrate their experience with the project, each recalled a time in the early 2000s when things got “weird.” Up until this point, it seemed that there was a uniform vision of what TPF should be and how it could be accomplished, and the exoplanet community, NASA, and ESA were proceeding in lockstep. As Geoff Marcy, who was a member of the TPF Science Working Group, recalled: “If we could transform ourselves back twelve years [to 1999], people were nodding their heads, it was in the textbooks. Sure, we’re going to have TPF, it’s going to be an interferometer, and it’s going to separate the planet light from the star glare and we’ll take snapshots of the planet and we’ll get spectra. That was pretty well established.”

Regardless of this perceived harmony, a few people began asking if there was not an easier way to accomplish the project. The interferometer consisted of four to five satellites flying in formation. In 2000, astronomer Wes Traub (who would officially join the TPF team in 2005) attended a workshop at the Space Telescope Science Institute (STScI) in Baltimore to figure out whether TPF could be done with a “normal”—that is, single—telescope. STScI was the steward of the Hubble Space Telescope, which was held as the standard to which other space telescopes should aspire. At this workshop, astronomer David Spergel suggested the idea of a “coronograph.” A coronograph uses a mask placed at the telescope’s focal plane to reduce light diffraction, which would minimize light distortion caused by a bright star. Following this meeting, participants offered a few ideas on how a coronograph would work for planet-hunting.

The true “weirdness” came when NASA decided to support the develop-

38. Geoff Marcy, personal interview with the author (Messeri), 24 January 2012.
opment of both the coronograph and the interferometer, launching one within a few years of the other. The exoplanet astronomers and broader astrophysics community greeted this decision with mixed reactions. While this was clearly a boon for TPF, even those involved in the process bristled over NASA superseding the traditional process for selecting space missions. In 2004, as NASA was deciding whether or not to fund both designs, Anne Kinney, director of the agency’s Astronomy and Physics Division, requested a special report from the National Academy of Sciences regarding TPF. Kinney asked that the same committee responsible for the decadal report, which had listed TPF as third among astronomy and astrophysics priorities, provide an updated assessment of the mission’s scientific objectives. Before this committee convened, Kinney sent a letter of addendum informing the National Academy’s committee that both designs of TPF would receive NASA funding. In the special report filed by the National Academy, the authors, although positive about TPF’s potential, reprimanded NASA for circumventing the standard paths to funding: “The panel is very concerned about breaking with a process for developing a strategy that has served astronomy and astrophysics very well—the broadly debated, carefully balanced, and widely endorsed portfolio that the 2000 decadal survey presented.”40 The decadal surveys are the one resource that scientists repeatedly reference as having sway at NASA, and to threaten the process is to threaten the stability they feel they have in the unstable funding ecology.

With two equally promising architectures, scientists and engineers started using a new language to describe the interferometer and coronograph separately: TPF-I and TPF-C. With NASA’s financial commitment to launch both TPF-I and TPF-C, the exoplanet community now had three high-profile missions in the pipeline: SIM, TPF-I, and TPF-C. Beichman continued as the principal investigator of TPF-I, and Traub joined Beichman at JPL and became the principal investigator of TPF-C in 2005. NASA gave TPF-C top billing, with a launch date in 2014, and slated TPF-I (still being developed with ESA’s Darwin) to launch sometime before 2020.41

40. National Research Council, Review of Science Requirements for the Terrestrial Planet Finder. This stands in contrast to the Mars case, where continued recommendation in both the decadal survey and congressional documents was not enough to guarantee a sample-return mission—for example, during the 1977–87 period. Here, NASA is taking the prerogative to give preference to a lower-priority mission, indicating that even weighty documents like the decadal survey can be used by different groups for different rhetorical purposes.

41. Note that TPF-I, although longer in development, was not slated to fly first. There are a variety of opinions as to why this decision was made. One astronomer, while retrospectively trying to figure this out, suggested a conspiracy theory intended to ensure that it was a NASA project (and not a NASA/ESA project) that spotted the first Earth-like planet. A scientist more closely affiliated with TPF dismissed this theory and suggested instead that TPF-C was closer in kind to Hubble and therefore perhaps more
The doubling down on TPF was a mixed blessing. Although TPF was still one program, the division between TPF-I and TPF-C caused a reorganization of the team. The extent to which there was friction between those working on “I” and those working on “C” is a topic of debate within the exoplanet community even today. Beichman recalls an immediate splintering: “There were proponents of each of these activities [TPF-C and TPF-I] saying I want to do this, and my idea is better than yours, and pretty soon the community fragmented.” Traub, on the other hand, dismissed the narrative of infighting; he certainly agrees that people advocated for their project, but he did not recall rhetorical strategies in which TPF-I was made to look worse or unfit for flight.

In FY2005, NASA delayed the launch dates of SIM and TPF due to the budget overrun with the James Webb Space Telescope (JWST) and the consequent need to mount a final Hubble repair mission. In addition, the broader astrophysics community was increasingly unhappy that a disproportionate amount of the budget was going to the relatively small exoplanet community. Then, devastatingly, in February 2006, less than a year after Traub moved to JPL as the principal investigator of TPF-C, the FY2007 budget stated that TPF had been “deferred indefinitely.” But did this mean it was canceled? Confusingly, no. Instead, TPF was downgraded to a “technology-development” program. The funding was drastically reduced, but a modest amount was set aside to continue developing the technology for an interferometer, coronograph, and a new third option, an “occulter” (TPF-O). In 2007, ESA did not select Darwin for flight during the 2015–25 window, no doubt because without TPF-I, Darwin was infeasible (and vice versa).

TPF and SIM limped along until the National Academy’s Committee on Astronomy and Astrophysics released its 2010 decadal survey. Titled New Worlds, New Horizons in Astronomy and Astrophysics, the survey continued to suggest that exoplanet astronomy is fundamentally important to the broader community. Indeed, the second of three scientific objectives for the coming decade was to seek nearby habitable planets—a recapitulation of TPF’s mission. Even the motivation for planet-finding hearkened back to Goldin’s vision from 1996:

On Christmas Eve, 1968, Apollo 8 astronaut William Anders took an iconic photograph of the rising Earth from his vantage point orbiting the Moon. It highlighted, to more people than ever before, that we humans share a common home that is both small and fragile. It also

of a known quantity. Additionally, because it was one craft as opposed to multiple ones, the engineering, although still challenging, was more feasible.

42. Beichman interview.
43. Wes Traub, personal interview with the author (Messeri), 27 January 2012.
brought into focus the question, What does Earth look like from much farther away? Remarkable discoveries over the past 15 years have led us to the point that we can ask and hope to answer the question, Can we find another planet like Earth orbiting a nearby star? To find such a planet would complete the revolution, started by Copernicus nearly 500 years ago, that displaced Earth as the center of the universe.45

This project was no longer called TPF, but instead the New Worlds Technology Development Program. For the new program to succeed, the decadal survey recommended continuing to pursue different options for the telescope, with an eye toward recommending one design in time for the next decadal review, a directive eerily similar to the one issued a decade earlier. In reading the report, one might not even be aware of the previous two decades of work that surrounded TPF. In a footnote and brief mention in the body of the report, the decadal survey announced that the community would no longer recommend SIM, as other scientific projects were more compelling. TPF was mentioned only once, in a passing reference pertaining to its recommendation in the previous decadal survey.

Community members cite the combination of this report, the very real budget pressures brought on by JWST, and the global financial crisis as reasons for NASA officially canceling both TPF and SIM in 2011. Traub does not hesitate to connect the cancellation of these programs with the decadal report: “The current decadal survey, in a footnote on one of its pages, in small print said that they didn’t think [SIM] was very interesting and so it got canceled immediately by NASA.”46 Without SIM as a testing ground for space interferometry, TPF-I had little hope of future resuscitation. Acknowledging that TPF was officially canceled even though the decadal survey still called for finding terrestrial planets, Beichman intoned, “TPF is dead. Long live TPF.”47

Shaping Technologies, Shaping Communities

These technologies must be seen now as they were seen at the time: as projected through a series of precursor missions into the near future. They therefore serve an important role in shaping and developing the present technology that will eventually enable the group to achieve its technological goal. Because the missions are conceived of in serial, MSR and TPF anchor present work as moving toward a perceived end-goal in a multi-phase sequence. Actors frequently call work on current and near-term mis-

46. Traub interview.
47. Beichman interview.
ions a “technology demonstration” that paves the way for the eventual necessary technology, setting parameters for immediate research and development. Such future missions suggest which technologies need to be developed in order to attain their eventual launch, and in doing so, they establish funding and research and development priorities on the ground. Even if MSR and TPF never launch, they will have been significant forces in shaping the missions that did so.

Further, MSR and TPF are themselves not the ends of projected futures. Early proponents of MSR posited it as the next step along the road toward manned missions because it would enable the development of technologies for such remote launches. MSR itself is therefore part of a larger projectory that aims toward human exploration of Mars. This was clearly stated in an American Institute of Astronomics and Aeronautics publication in the 1980s on the topic of aeroassistance for the Mars missions: “A predecessor to the manned Mars mission, as well as a great contributor to the study of the solar system, could come in the form of the Mars Rover/Sample Return Mission (MRSR). This mission concept, scheduled for a 1998 launch, will utilize aeroassist to capture into Martian orbit, as well as Earth orbit upon return.” Likewise, TPF is a step toward an extra–solar system mission. Marcy, at a 2010 exoplanet conference, implored President Obama to declare that by the end of the century the country will launch a probe to Alpha Centauri (our closest star) and return pictures of its planets, asteroids, and comets.

These cases exemplify what goes into shaping a technological projectory—a history that is written long in advance of the technology itself. It is the material tracings of a sociotechnical imaginary that determines which legacy technologies need to be developed now in order to integrate into and support future missions. Thus, the imagined future plays a key role in imposing local constraints. This is particularly evident in the primary artifacts that represent the projectory: timelines, such as those seen in figures 1 through 5, that circulate among the community. Ultimately, these shifting timelines suggest that attention to the near future is just as, or perhaps more important than, the lofty philosophical goals on which these technologies rest. TPF might be a stepping-stone to Alpha Centauri, but before scientists can even launch TPF, the timeline indicated in figure 5 grounds present work in the near-future goal of work at the Keck Observatory in Hawai’i and the launch of less ambitious missions.

Not only does a projectory shape objects—it shapes subjects as well. Even when these missions are announced as indefinitely delayed (as TPF was in 2006), they are still imagined as just waiting their turn and scientists continue the demanding work necessary for launch. We often encountered this sentiment in our fieldwork with the communities in question. For

example, at the Seventh International Conference on Mars in 2007, an astrobilologist described sample return as the ultimate answer to the question of life on Mars, positing it as part of the “astrobilology strategy”: “Follow the water” [the Mars strategy in the 1990s] . . . was the easy part. The next step is [to] search for organics. Well here the Viking results aren’t very encouraging. . . . Hooray for returning a sample to Earth for analysis!” These moments provide evidence that the missions deeply affect their communities, and even serve a rallying or focusing function for the community’s effort and organization. As astronomer Sara Seager, an ardent supporter of TPF, proclaimed in 2012: “If we want to find an Earth-like planet, like an Earth twin, a true Earth analog . . . we have to do a terrestrial planet finder. . . . That’s why it’s inevitable. Whether it actually happens in our generation or not, that is still out there.”

The expectation of the “inevitability” of MSR is treated as a matter of fact in the Mars community. In a lecture given in 2006, Charles Elachi, the head of JPL, described future missions as follows:

In 2009 we’ll have MSL [Mars Science Laboratory] launching . . . with a heavy focus towards the chemistry of the surface. . . . we will have the ability to take samples, crushing samples . . . and then in the long term still the objective is to do long-term sample returns from Mars. . . . I think it’s technically feasible . . . the limitation definitely is not the imagination or the science, the limitation really is the funding.

Several scientists who believe in the inevitability of these missions structure their current professional activities around a future mission. In a more optimistic moment, when TPF was still modestly funded though considered by most to be canceled, Seager described a project she was working on with MIT undergraduate students. This project was unlike anything the theoretical astrophysicist had done before because it involved hardware development. When asked in an interview why she was deviating from her expertise, Seager answered: “My real goal is to be PI for TPF and I need to get the hardware experience.” In this way, she framed her current work in terms of a future position that she would like to have on a practically unfunded mission. It is not the technology, but the persistence of the projectory, that gave Seager the belief of a future for TPF.

49. Author’s ethnographic field notes (Vertesi), Seventh International Conference on Mars, 17 July 2007.
50. Sara Seager, interview with the author (Messeri), 19 January 2012.
51. Charles Elachi, “Space and Earth Science Exploration Opportunities in the Next Decade.” The MSL mission was later called Curiosity; it was delayed, not launching until 2011.
52. Sara Seager, interview with the author (Messeri), 7 October 2009.
53. Some graduate students in the Mars community certainly see their careers as leading toward MSR. One expressed that her involvement in the present missions was aimed toward continued involvement so that she could “be there” for sample return.
man, the actual principal investigator of TPF-I, was less optimistic about his role in a future TPF: “Sure, I’d like to have [some involvement]. But it’s going to be some young hot postdoc [who is] most likely getting engaged in that.” He went on to say that he was satisfied with working on the coronograph for the JWST by way of technological development for TPF. Even though he was optimistic about a TPF-like mission happening (he referenced the 2010 decadal survey as proof of its inevitability), he did not see his involvement being anything more than superficial.54

Much work in the social and historical studies of technology has shown that technologies are shaped by, and also shape, their communities.55 Notably, this shaping work takes place even when the technology itself is a distant possibility on the horizon. In the cases of both TPF and MSR, we witness communities in formation, the creation and maintenance of relevant social groups, long before the technology is actually developed.56 The communities surrounding MSR and TPF do not disappear with the loss of funding, but maintain their devotion to the future mission. Through smaller grants, Phase-A studies (that is, agency-funded preliminary work), or independent work on the topic, the communities continue to flesh out the potential for the mission. This has ramifications for the growth of a subcommunity that advocates for the mission at each opportunity for input. Notably, charismatic leaders may also come forward to lead the community toward this desired goal.57 Establishing the projectory can therefore produce powerful subcommunities that are passionate about a particular kind of science and aim to exert some control over their funding priorities.

As much as individuals and communities structure around these missions, they also splinter. Funding some missions and not others leads to deep divisions within communities, visible in both planetary science and astrophysics today. Therefore not everyone in the community believes fervently in these missions. One exoplanet astronomer, hearing about TPF, rolled his eyes and said: “That mission! Those guys still believe it’s gonna happen, even if it takes them fifty years.”58 A planetary scientist expressed outrage at the “Mars Mafia”—the outspoken majority that proclaims the importance of sample return above all other missions, to what this scientist said was the detriment of the rest of the planetary-science community. Such missions, even when they have never launched, can prove foundational for the formation of scientific identities, communities, and boundaries.

54. Beichman interview.
55. Ronald Kline and Trevor Pinch, “Users as Agents of Technological Change.”
58. The quotes in this paragraph are from fieldwork and are left undated to preserve anonymity.
Funding the Projectory

In discussing why these “greatest missions” have not flown, we would be remiss if we did not touch on budgetary factors. After all, our actors frequently refer to the NASA budget as either supporting or restraining their technoscientific endeavors. Also, as these cases demonstrate, there is considerable tension among the decadal reports, NASA, and congressional priorities. Sociotechnical imaginaries are indeed frequently linked to state-funded projects that require considerable financial support. This matter of funding is much more complex and shifting on the ground than it may seem from actors’ accounts. Fiscal allotments cannot simply be understood through looking at congressional statements or traced through official documents. We assert that both actors and analysts in this domain face two interrelated problems: a problem of scale, and a problem of time.

The problem of scale arises from the fact that NASA’s budget allocations come from a variety of bottom-up and top-down activities, and frequently involve clashes over documents that purport to represent either “the will of the community,” such as decadal surveys, or “the will of Congress,” such as in annual budgets. Both require setting priorities, which, in turn, is the result of considerable political activity and haggling within each community in question. For example, one of the reasons TPF was first deferred and later canceled was due to the relations among members of the astrophysics community, some of whom are exoplanet astronomers and others who are not. The original incarnation of TPF—TPF-I—was a big-ticket item and well-supported in the decadal survey, even though the community of exoplanet astronomers was small in respect to the broader community of astronomers. Then, in the early 2000s, NASA committed to not one, but two (three, including SIM) large exoplanet missions. According to our informants, this decision was made outside the normal protocol of a decadal or similar survey and simply “came from on high” (that is, from the Science Mission Directorate at NASA). At the same time, the JWST, another big-ticket mission for the astrophysics community, began running over budget, thus creating a funding squeeze across NASA’s Science Mission Directorate. As a result, non-exoplanet astrophysicists began speaking out against the disproportionate amount of money that a small sector of the community was receiving, detracting from their own piece of the pie.59 Neither the documents nor community-member reports are sufficient to explain why TPF was ultimately defunded; however, in this ac-

59. This suggests another reason that TPF-C might have been slated to launch first. Whereas very little argument could be made that TPF-I was a multipurpose mission, TPF-C could conceivably be used to perform astrophysical research beyond exoplanet characterization. As Beichman tried to explain: “Everyone, when they take off their professional astronomer hat, loves the idea of going and looking for life, looking for planets and all that stuff. . . . When you say you want [TPF] to compete with your mission or the big shot for your X-ray mission, Hubble successor, whatever it happens to be, they say screw that. I’m not getting enough out of this” (Beichman interview).
count, we see the importance of the community-maintained projectory for
the purpose of cohesion in the face of uncertainty.

Top-down money-shuffling amplifies existing strains in the scientific
community over which projects obtain support and which are “stealing
everyone’s lunch.”60 In the case of Mars exploration, the considerable
funds spent on Vikings 1 and 2, combined with the simultaneous expense
of the Vietnam War, canceled any possibility for immediate follow-up
despite evidence that future missions were planned. This pattern repeated
in the midst of spending on the Second Gulf War in the early 2000s. Follow-
ing the extensive cost overruns and slipped launch date for the Mars
Science Laboratory in 2009 (which itself included a sample cache as a base-
line design concept, later de-scoped), NASA’s Science Mission Directo-
rate rearranged funds within the Planetary Science Division with the ex-
pectation that the Mars program would have to repay the loans to the other
programs during the period 2014–16. As a NASA representative ex-
plained, these “significant cuts to the Mars program” were responsible for
“deleting five years of technology assessment that . . . paved the way for
Mars Sample Return.” Due to this requirement for repayment, the Mars
program was forced to “[reduce the] scope of its 2016 mission, an ambi-
tious and very important mission because it was considered a critical test
bed . . . both of those impacts being that the Mars program is delaying the
Mars Sample Return . . . that is where MSL has left us.”61 At the end of
2009, another NASA representative explained to the Mars Exploration
Program Committee that a mission slated for 2020 “might be skipped to
build up the budget for sample return in 2023.”62 The iterative back and
forth between NASA and scientific-community members therefore sug-
gests that distinctions of scale are slippery at best.

Time also produces similar problems. The “greater” these missions are,
the further over the event horizon they are projected. MSR is usually set for
eleven to thirteen years hence, and the projected date of launching moves
correspondingly with the date of discussion. For example, in 1964, it was
projected to launch during 1975–77, while in 2007, the projection was for
2018–20. Such temporal distance produces an impossible commitment
under NASA’s jurisdiction. The agency must return to Congress for budg-
etary approval on an annual basis, and new U.S. presidents issue new
directives every four or eight years. Although each slip in the schedule is
ascribed to budget cuts or current crises, in reality, the mission cannot be
funded with that kind of schedule in any realistic sense.

60. This is an actor’s form of talk, used to refer to whichever mission was absorbing
all the funds. Whether or not this is a true statement, that a disproportionate amount of
money is relegated to one mission at the expense of another, we prefer instead to treat
this as a marker in conversation in which a community member aligns her/himself with
one or another side or subcommunity against another mission.
As money is imposed and constrained both internally and externally, both from within the community as a bottom-up factor and from NASA as a top-down one, perpetual delays cannot be explained by funding decisions alone. However, the moments when our historical and contemporary actors invoke funding constraints, we suggest, are moments that invite analysis. They are the *explanandum* as opposed to the *explanans*. Community members fight to gain enough recognition so as to deserve line items in the budget; as soon as numbers are allocated to this mission, a sub-community becomes both visible and contestable. Discussion about budget is not a question of making a sociotechnical imaginary come true or a question of implementation at a state level; instead, such a discussion is part of the work of making and sustaining a projectory.

**Conclusion: Future Projectories and the History of Technology**

Donald MacKenzie, in his critique of technological determinism, concludes that “[w]hat appears to be a natural trajectory ought instead to be seen, I suggest, as an institutionalized pattern of predominantly incremental technological change involving, centrally, a self-fulfilling prophecy.”63 The cases we offer suggest that even when a prophecy has not yet been fulfilled, it may still be an active component in the social shaping of technological systems.64 The examples of MSR and TPF demonstrate a crucial role for unlaunched missions in shaping both technological developments and scientific communities through producing a shared sociotechnical projectory: a presumed future path along which all is seen to depend and contemporary work justified. Thus, whether or not these prophecies ultimately come to fruition is no straightforward indicator of their power and influence.

The notion of the projectory presents implications for the historical development of sociotechnical systems beyond space science. Whether in Thomas Sprat’s enthusiastic 1667 “history” of the nascent Royal Society of London, in Thomas Edison’s laboratory, or in the “pitches” of the technology start-up firm, anticipatory discourses in a variety of forms accomplish the difficult work of holding technoscientific communities together, accruing and adhering membership, and bargaining for legitimacy even (or perhaps especially) in the face of fiscal or political uncertainty.65 However, the

64. Sociological and psychological studies of millenarian religious sects produce similar findings about the role of disappointment in not undermining or shrinking a community, but rather reinforcing belief and renewed efforts in proselytizing, thereby reinforcing existing social structures. See Leon Festinger, Henry Riecken, and Stanley Schachter, *When Prophecy Fails*; Paul Boyer, *When Time Shall Be No More*; and Ronald Numbers and Jonathan Butler, eds., *The Disappointed*.

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projectory may hold particularly powerful sway in the histories of publicly funded Big Science, wherein communities of practitioners must demonstrate to their funders’ satisfaction (and to the exclusion of other candidates) that their plans are practical and achievable despite their novel or ambitious forms. They must also aim to surpass the temporal rhythms and vagaries of congressional approval and regulatory documentation in order to secure support for their communities and careers that endure beyond the budget cycle. In such cases, the projectory can play a central role in the rhetorical strategy of gaining funders’ allegiance and support alongside grandiose claims about scientific possibilities and technological achievements, while at the same time providing a rallying point in the face of instability.

Ultimately, this article aims to show how technological futures do not belong solely at the level of state policy or individual visionaries, nor do they operate as spectacular visions. They are also powerful social and material forces that act at the level of individuals’ career and research decisions. The projectory, whether written or drawn in planning documents or described in career rationales, makes these future visions concrete and meaningful for actors on the ground; it also makes the role of such tangible near futures visible to historians of technology, challenging us to look beyond the literature on technical “failure” or even the analytical question of “Could it have been otherwise?” As we have described, imagined near futures can exert a powerful influence over the shape and scope of community and technological developments, as well as in the establishing of state-funding priorities, even as they are produced and reproduced through moments of conflict and consensus among different stakeholders. It is in these dynamics that we glimpse how the community leverages anticipatory discourses in action, and witness the implications of past futures in the historical development of sociotechnical systems.

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66. Originally developed to describe the industrialization of workers according to clock time, *temporal rhythms* may include a variety of factors according to which labor is temporarily structured and embodied; see E. P. Thompson, “Time, Work-Discipline, and Industrial Capitalism.” On scientific work, see Steven Jackson, David Ribes, Ayse Buyuktur, and Geoff Bowker, “Collaborative Rhythm.”
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