# *Science Imagines Its Future*

## Lorraine Daston

***2024 Balzan Prize for History of Modern and Contemporary Science***

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**Director:** Professor Lorraine Daston (MPIWG Berlin/University of Chicago, USA)
**Senior Advisors:** Professor Erika Milam (Deputy Director, Princeton University, USA), Professor David Kaiser (Massachusetts Institute of Technology, USA)
**Format:** Two summer schools of five days each, to be held in Berlin in 2027 and 2028

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***Project Description***

Astronomers deliberate about whether they should plan large telescope arrays to study black holes in remote galaxies or rather to divert those resources into a new generation space telescope. Archaeologists debate whether they should excavate a site rich in important but fragile ancient artifacts now or rather wait another generation or two until methods and preservation technologies have hopefully improved. Botanists and zoologists meet to decide whether the latest findings in evolutionary biology and genetics are sufficiently solid and consequential to warrant thoroughgoing changes in the principles by which organisms are classified. Historians and social scientists try to guess the research interests of their successors in amassing gigantic databases. Physicists differ as to whether the future of their discipline lies in ever more powerful particle super-colliders or rather in the infrastructure needed for other specialties, such as solid-state physics. Archivists, librarians, and museum curators everywhere wonder what documents and objects to preserve for future scholars and scientists, given the constraints of money and space.

Such infrastructural decisions – for example, what objects to collect, from ancient pottery shards to ice cores, and how to study them – have left deep and long-lasting imprints on disciplines; decisions in the present will similarly shape the scientific future, favoring some objects of inquiry and excluding others. All disciplines, whether in the humanities, social sciences, or natural sciences, are in intense discussions over how best to organize and store digital data so that it will be as long-lived and flexibly usable as print media (and, in some cases, older media, such as cuneiform clay tablets or silk scrolls) have proved to be. In order for science and scholarship to go on, their practitioners must imagine and plan for the future, sometimes a future that stretches centuries ahead.

This project builds upon earlier work conducted under the auspices of a Working Group, "The Sciences of the Archives", at the Max Planck Institute for the History of Science. In studies of disciplines ranging from astronomy to geology to data science, the resulting Working Group volume, *Science in the Archives* (2017), explored how various sciences (as well as some fields in the humanities, such as classical philology), collected observations, fossils, photographs, inscriptions, data banks, and other sources that stretched the timescale of their research into the deep past. Just as eighteenth and early nineteenth century scholars and scientists reimagined their objects of inquiry on a planetary scale, in terms of the earth's magnetic field or globe-spanning ocean currents and wind patterns, so their successors since the late nineteenth century tried to expand the temporal reach of their disciplines by creating and preserving scientific archives.

These ambitious projects—such as the collections of human and natural artifacts amassed by the world's great museums or the astrophotographic map of the entire sky as seen from the earth circa 1900, known as the Carte du Ciel (begun in 1887)—were the beginning of Big Science and also of international scientific organizations. For the first time, individual researchers across the world had to coordinate their efforts, calibrate their instruments, standardize their procedures, and, crucially, make and honor long-term commitments, sometimes for decades or even centuries. The humanities led the way. The historian of ancient Roman law, Theodor Mommsen, was the one who coined the term Big Science and organized the first such project worthy of the name: the *Corpus Inscriptionum Latinarum* (begun 1853), a collection of all extant Latin inscriptions from the Roman Empire, became the inspiration for many other such grand projects in other fields. Some of these projects, like Mommsen's *Corpus Inscriptionum Latinarum*, were underwritten by academies and governments; others, like the first international cloud atlas (1897), were initiated by scientific societies and staffed by volunteers.

In all cases, the scale and degree of organization, investment, cooperation, and commitment were unprecedented, and always precarious. National hostilities and personal rivalries threatened to tear these international projects apart. Yet somehow many survived revolutions, two world wars, decolonialization, and the shredding and remaking of the geopolitical order in the twentieth and twenty-first centuries.

It was against this background of how scientists and scholars gradually learned to make collective decisions about the preservation of the past that their efforts to plan for the future must be understood. These projects mark the birth of the modern scientific community, but it was a difficult birth, and its survival has remained precarious.

However great the challenge of deciding what items from the past must be preserved for use of a discipline in the present, it is dwarfed by the difficulty of planning for a discipline's future. Scientific archives are subject to the vicissitudes of time, and their creation is a wager on the future. Some collections that seemed essential at the time they were assembled, like the nineteenth-century collections of skulls assembled by anthropologists, have turned out to be worse than useless: the archives of a discredited and racist science of human variation. Other investments, like the fossils gathered by paleontologists and the Latin inscriptions gathered by the classicists, have paid rich dividends, and remain in constant use by researchers in those disciplines.

Despite these risks, a discipline that creates an archive at least knows that it is of use to researchers in the present. Disciplines that plan for the future cannot cling even to that short-lived certainty. Their bets on the future are also gambles on which lines of inquiry, all of which are viable and promising in the present moment, will turn out to yield the most important results in the future. And in how distant a future? How long must a discipline wait in order to assess whether their bet has paid off? Decades? A Generation? Centuries? In long-term projects, for example, observational stations in organismic biology, the question of sunk costs haunts researchers who may have spent a lifetime making painstaking observations that will not bear fruit for at least another generation or two, given the slow pace of evolutionary change. Depending on the discipline and its objects of inquiry, the timescale of planning may surpass anything conceivable by governments, especially democracies with short election cycles – a relevant consideration for scientific investments in the future often financed by public funds.

To plan for the future is to imagine the future – or rather, several alternative futures simultaneously. The Balzan Prize project will explore how disciplines imagine their future under conditions of radical uncertainty. Since the early nineteenth century at latest, it has been clear to scientists and scholars that progress in their disciplines will not always be gradual, linear, and cumulative; it will be punctuated at irregular intervals by surprises that could not have been extrapolated from what came before and which may indeed shake the foundations of the discipline. The vocabulary of "revolutions", "breakthroughs", and "disruptions" tries to capture the abruptness of these moments on non-linear progress, which are at once the most exhilarating and melancholy moments in a discipline's history: whole new worlds open up for inquiry, at the price of leaving much of the old world in ruins. To imagine and invest in a scientific future is therefore inherently paradoxical: if the wager pays off, it will be thanks to scientific progress that could not possibly be foreseen.

These are real-world cases of what decision theorists call "judgment under uncertainty", and they are far more complex and consequential than the examples usually provided, which assume a limited and known number of outcomes with fixed probabilities, often modeled on buying a lottery ticket (also the subject of an earlier MPIWG Working Group, which published its results as *How Reason Almost Lost Its Mind: The Strange Career of Cold War Rationality* [2014]). In the case of disciplinary decisions about how to invest always scarce resources of time, money, and intelligence, neither the possible outcomes nor their probabilities can be known, only guessed at, and in full awareness that such guesses are doomed to be wrong. The astronomers who initiated the Carte Du Ciel could not have foreseen that their photographic plates would be used a century later to detect dark matter; nor could the classical philologists who began the *Corpus Inscriptionum Latinarum* have known how their successors would have used the collections of squeezes of inscriptions to reconstruct the lives of people who left no traces in the official annals of the Roman Empire. These are examples of investments that paid off in ways that outstripped the imagination of their initiators.

Many other such investments in the future did not keep pace with subsequent developments in their disciplines. The reasons for failure are many. A once promising avenue of research turns out to be a dead end (for example, the mid-twentieth-century anthropological initiative to create a cross-cultural collection of dream motifs). New methods and instruments render old ones suddenly obsolete (for example, the shift from optical to radio telescopes in astrophysics). The discipline undergoes a theoretical transformation that orients research in a completely new direction (for example, the impact of the discovery of the structure of DNA on genetics). The funders lose interest (for example, the demise of the super-conducting super collider in U.S. elementary particle physics), or worse, actually sabotage the project, as is the case for social science inquiries on politically sensitive subjects in many countries. After the key organizers of a project step away, other long-term research programs simply lack the personnel needed to sustain them.

Scientists and scholars today face equally consequential decisions about how to plan for their disciplines' futures. No discipline can avoid such decisions, and this goes for the natural sciences, the social sciences, and the humanities. For the last decade, humanists and social scientists have debated whether the first priority should be the accumulation of digital resources or the interpretative research that makes sense of those resources. Analogous debates are going among geneticists about which genomic data to store and how to use it. Climate scientists argue over whether they should plan for a future driven by intricate models that require expensive time budgets on super computers, or whether both human and financial capital would be better spent on a denser network of observational satellites. Infrastructure is always at issue: the currently available material media for data storage consume vast amounts of electricity and have a half-life of about 30 years, and changes in both hardware and software can render older data inaccessible; yet a book printed on good-quality paper in 1500 can still be read without further difficulty today. As these examples show, these are multi-angled debates about methods, instruments, and infrastructure as well as about the Next Big Thing among research topics.

Just because the decision-making processes involved in planning for future science have almost no equivalent in any other form of governance – certainly not in any existing polity, whether democratic or autocratic, and arguably not even in organized religion – they are of broader interest beyond science. In an era in which citizens of all countries are confronted with crises that unfold on a planetary scale over generations, the organization required for scientific and scholarly projects that plan on a superhuman scale of time and space may provide some valuable lessons, both positive and negative.

***Participants***

Ten early career scholars, chosen in an open, international call for applications on the basis of their academic records and the fit of their research project with the summer school topic. Ideally, all would participate in both workshops, to ensure continuity. Advanced graduate students, postdocs, lecturers, and assistant professors or the equivalent would all be eligible to apply, if not more than seven years have elapsed since receiving their doctorate degree.
Several other senior scholars (in addition to the director, deputy director, and an additional advisor) who would act as docents, chosen on the basis of their publications and research interests, with an eye to making sure that all of the early career participants had expert interlocutors in their fields of interest.

Invited guests, mostly scientists, who would be interviewed about their own involvement in consequential decisions about the future of their disciplines. Possibilities include the past and present presidents of the Max Planck Society and other major research organizations, as well as scientists and scholars who have led such major planning initiatives in astronomy, physics, biology, philology, history, and sociology.

***Program***

At least half of the summer school would be devoted to the presentation of research related to the main topic by early career scholars in workshop format, to provide maximum benefit of commentary and discussion. Presentations by the docents and interviews would comprise the remainder of the program.

***Dissemination of Results***

It is probably in the best interests of the early career scholars that they be given full freedom as to where they publish the results of their research (as a journal article, book chapter, monograph, or other forum). Funds would be made available to help with at least some publication costs, such as image permissions.

The summer schools would also request that participants write a brief (800-1200 words) blog post on some aspect of their research aimed at a broader audience, to be posted on the MPIWG and possibly other institutional websites. Podcasts of at least some of the interviews conducted at the summer schools would also be part of this online repository of the project, with a full bibliography (with links, as appropriate) to all resulting publications.