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Building the Scientific Research Infrastructure of Tomorrow

Building the Machine That Turns Questions Into Knowledge

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Introduction

A laboratory can be full of extraordinary equipment and still be unable to answer its own question. A sample reaches the instrument without a reliable record of how it was collected. The person trained to operate the instrument has moved on. A grant pays for a new machine but not for its maintenance. The data are deposited in a format the next team cannot read. Then a result is announced that nobody outside the original group can check. None of these failures is a failure of curiosity. They are failures of infrastructure: of the arrangements that carry a question through people, facilities, measurements, interpretation, challenge, and reuse.

Scientific research is often described through its breakthroughs. The description is not false, but it makes the system hard to build. Breakthroughs are outcomes. Infrastructure is what gives researchers the ability to ask a difficult question twice, let a different group inspect the answer, and keep learning when the original team leaves. A reliable research system needs rooms and instruments, certainly, but also sample custody, methods, technical staff, accessible records, ethical review, long-term stewardship, and a way for a result to become another team's starting point. Its product is not merely a paper or a patent. Its product is a growing capacity to distinguish what we know from what we only hope to know.

That capacity has to survive friction. A research question may take years to mature while funding arrives in short intervals. A shared instrument has more possible users than available time. An experiment generates data quickly but its meaning can remain unsettled. The incentives that reward a dramatic finding may not reward the careful work of checking one. A project can be excellent by its own terms and still leave an unusable archive behind. These are design problems, not reasons to abandon science. They tell us what the infrastructure must protect: continuity across changes of personnel, traceability across changes of format, and legitimate disagreement across changes of interpretation.

There is a simple way to see the entire system. Imagine a question entering a research institution: can a material behave as expected under a particular condition? Someone decides whether that question is worth asking now. A team translates it into a method and secures equipment, supplies, time, and permission. Measurements are made, recorded, corrected, and interpreted. A different team tries to reproduce the important parts. The results are communicated, challenged, and either used or set aside. The process looks like a line when drawn on a slide. In practice it is a network of queues and handoffs. The sample can wait for a machine; the machine can wait for a

repair; the data can wait for a curator; a decision can wait for evidence that has not yet been collected. Building research infrastructure means designing those handoffs rather than treating their failure as bad luck.

The first choice is what to count. If a laboratory measures only publications, it can grow its output while losing the staff who make the instruments run. If a funding agency counts only new facilities, it can build laboratories whose operating budgets disappear before the equipment reaches its useful life. If a data repository counts uploads without testing whether anyone can understand or access them, it can become a warehouse of inaccessible files. Measures are useful, but no single measure tells us whether a question has gained a trustworthy answer. The better unit is a research capability: a supported chain that begins with a question, creates inspectable evidence, and remains usable by someone who was not present at the start.

This book follows that chain. The question portfolio determines what the institution can afford to investigate, and what it must leave open. The instrument commons makes expensive capacity available without making it ownerless. The experimental layer coordinates protocols, samples, and safe work so a measured difference means something. The data layer preserves evidence and its lineage beyond a project's funding window. Verification turns a claim into a proposition that other people can challenge. The people layer keeps specialist skill from disappearing between grants. Funding and governance decide whether those layers live on the same time horizon. Publishing and translation carry findings into wider use without asking the research system to promise more than its evidence supports. The final chapter assembles the layers into an operating discipline for tomorrow.

The boundary of this book is deliberate. It does not claim that every scientific field needs the same apparatus. A field study, a computational model, a clinical investigation, and a materials laboratory put different pressure on space, equipment, data, and ethics. They do, however, share the problem of custody: somebody must know what was done, with what, by whom, under which constraints, and how the next investigator can tell. The details vary by field; the need for a system that can answer those questions does not.

The infrastructure of tomorrow will not be built simply by buying faster tools. Automation can produce measurements faster than institutions can inspect them. Larger data stores can retain more observations without making them easier to interpret. Powerful models can generate candidates for investigation without establishing which candidate is true. Each gain in speed increases the value of the quieter layers: calibrating instruments, preserving provenance, designing checks, and giving people time to follow the evidence when it is

inconvenient. The central ambition is therefore modest but demanding: build a machine that can say not only "we found this," but "here is how you can find out whether we were wrong."

Chapter 1: The Question Portfolio

A research institution cannot investigate everything it finds interesting. It must decide which questions can move now, which should be prepared for later, and which should remain open for another institution to pursue. The portfolio is that allocation of attention. It is not a list of approved topics pinned to a wall. It is the arrangement of people, facilities, money, and time around uncertainty, designed so that learning in one project changes the next decision without forcing every project to justify itself as an immediate product.

The portfolio has two competing obligations. It must protect work whose value cannot yet be demonstrated, because inquiry that merely confirms the present funding priorities will not discover their limits. It must also account for resources: an instrument hour spent on one proposal cannot be spent on another, and a specialist assigned to one program is unavailable to a competing one. A good portfolio makes those constraints visible without treating uncertainty as a defect. The question is not "which project will succeed?" before a project has produced evidence. The question is "what collection of investigations gives this institution several credible ways to learn, while preserving the capacity to change course?"

An ambition is not yet a research question. "Understand the disease," "make the material stronger," and "improve the model" are directions of travel. An answerable question narrows the uncertainty until the team can say what observation would make its present belief less convincing. The translation requires the scientists' craft, but infrastructure can make it easier or harder. Access to past results can reveal a duplicate experiment; a methods advisor can expose a missing comparison; a technician can explain that the measurement precision is not sufficient to discriminate the proposed alternatives. The question desk, whether a formal office or a habit of review, is therefore a capacity for refining inquiry before scarce resources are committed.

This step must not be confused with demanding a promised finding. A project may end by showing that its hypothesis was wrong or that the measurement cannot settle the dispute. Those are valuable outcomes if the method and limits are inspectable. A portfolio that rewards only confirmed hypotheses will select for questions whose answers seem safe, then mistake predictability for progress. The infrastructure should support a clear test and an honest account of its failure, not a guarantee that the test will produce a headline.

Research projects run on different clocks. Some questions can be addressed by reanalyzing existing records; others require seasons of fieldwork, sustained

operation of a facility, or the development of an instrument that does not yet exist. The short clock is useful for course correction. The long clock allows work that would be impossible if each grant cycle had to yield a finished result. A durable portfolio needs both. It can divide a long program into reviewable stages without pretending the entire outcome is known in advance, and it can reserve short-cycle capacity for unexpected observations that deserve a prompt test.

The clock also applies to foundations rather than only projects. Collections need curators after the initial acquisition. Data systems need migrations when formats change. Instruments need calibration and replacement parts between celebrated runs. The portfolio that funds investigations but not their supporting foundations gradually spends a stock of inherited capacity. Its apparent productivity conceals a maintenance debt. The honest portfolio includes the upkeep required to keep its options real, even when upkeep has no new discovery to announce.

A choice among proposals is a decision under incomplete information. The process cannot make the uncertainty disappear, but it can reveal why one project received time and another did not. Criteria might include the question's clarity, the proposed method's ability to distinguish alternatives, access to required capacity, ethical constraints, the possibility of sharing results, and the extent to which the work complements rather than repeats an existing program. None of these can be reduced without loss to a single score. Review should make tradeoffs legible and permit a reasoned exception, with the exception recorded rather than hidden in a ranking formula.

This matters most where the portfolio crosses disciplines. The proposal that joins two fields may fit neither field's standard review vocabulary; the equipment needed to support it may be charged to one department while the benefit appears in another. The machine needs a way to see across those accounting lines. Shared review, transparent facility scheduling, and a budget for work that does not belong neatly to an existing unit allow the institution to test an interface between fields without making the researchers solve every administrative mismatch themselves.

A portfolio is a living control system. At each review it should ask whether evidence has changed the question, whether the method is still capable of answering it, and whether another use of the capacity now offers more learning. Stopping a project is not always an indictment of its team. An early result can show that a line of inquiry is unpromising, or a new observation can make the planned experiment redundant. Continuing because funding has already been spent is not fidelity to science. It is a failure to distinguish a sunk

cost from an open question.

The reverse is also true. A project that has not yet produced a publishable result may have created an essential method, a stable dataset, or a skilled team. Stopping it at the wrong review point can destroy more option value than it saves. The decision should inspect the evidence, the new capability, and the cost of rebuilding later. This is why the review record belongs to the infrastructure: the next committee must be able to see what was learned, not just which line item was closed.

The question portfolio hands the rest of the machine a qualified demand signal. It says which kinds of observations are needed, how soon they matter, what resources they will consume, and what conditions would justify changing direction. An instrument facility can then schedule capacity against actual research needs rather than political seniority. A data steward can plan for the kind of record the work will create. A funding program can budget for the maintenance its questions require. Without that signal, every layer optimizes its own local measure: the facility seeks utilization, the repository seeks deposits, and the program office seeks completed awards. The portfolio's job is to keep all three attached to the same purpose: the production of evidence someone can challenge and reuse.

Imagine a shared research center receives three proposals at the same moment. One team wants to test whether an apparent effect in an existing dataset survives a different analysis. Another wants to build a sensor for a phenomenon it cannot yet measure reliably. A third proposes to collect a new set of samples in a setting its institution has seldom studied. The proposals cannot be ranked honestly by asking which will generate the most papers next year. They make different demands on time, produce different types of uncertainty, and rely on capabilities with different lifetimes. A portfolio decision has to state what it is purchasing in each case.

The reanalysis may need modest machine time but deep methodological attention, and a disappointing result may save an entire community from chasing an artifact. The sensor may need engineering time and repeated calibration before it produces an observation worth publishing. Its failure could still identify the limits of the current technique. The field collection may open an important comparison, yet depend on consent, local partnerships, and a stewardship arrangement that cannot be invented after the samples have been taken. The review should therefore compare the projects at the level of question, evidence path, and dependency, not merely at the level of cost per anticipated article.

Suppose the center can fund only two. A reasoned choice might preserve a

small, time-boxed reanalysis and a staged sensor development effort while helping the field team find a partner capable of handling its custody obligations. Another center with different facilities and relationships might reasonably choose differently. The important property is that each decision can be explained in terms that the teams can challenge: what is being learned, what has to exist first, and what evidence would reopen the rejected proposal. A rejection becomes less arbitrary when it identifies a missing condition the institution can help address. That is a service to the research ecosystem even when the budget cannot support every project.

A decision log might capture the alternatives in plain language: the question; the proposed discriminating observation; the necessary capacity; the ethical and data commitments; the smallest useful stage; the point at which continuation would be reconsidered; and the reason for the current allocation. It should not pretend to calculate a precise probability of a discovery. Predictions presented with false numerical authority can make subjective preferences harder to see. Written assumptions are more useful than a polished but opaque ranking: when the sensor becomes available, or an unexpected dataset appears, the organization can revisit what it actually believed when it made its choice.

The portfolio holds more than active experiments. It also holds promises made to people and to previous work. A collection accepted for long-term custody has ongoing costs even while no new researcher is using it. A sensitive dataset may require a secure access process that cannot be switched off between grants. An instrument serving several teams may need a maintenance reserve before new demand is considered. If the institution treats those obligations as overhead outside the portfolio, new projects will appear cheaper than they are and older capabilities will quietly decay.

A useful ledger distinguishes exploration from obligation without pitting them against each other. Exploration funds tests whose outcome is uncertain and whose future cost can be reviewed. Obligation funds commitments that the institution has already accepted, with explicit retirement routes where a service is no longer justified. A transitional category supports promising methods that are not yet stable enough to become permanent services. The boundaries should be open to revision: an exploratory method can become common infrastructure if demand and evidence warrant it; a once-central service can be retired if its users have a credible alternative. Without these transitions, institutions either freeze today's methods into permanent entitlements or rebuild the same fragile capacity in one short award after another.

The ledger also reveals a moral hazard in expansion. A program may be easy to launch with a grant and hard to sustain when the grant ends. The project proposal should name what remains afterward: staff, access rights, records, maintenance needs, and any promise made to collaborators. It can then design a handoff, funding route, or bounded end date before the service acquires dependent users. The person raising this issue is not opposing ambition. They are asking that ambition include the people who will inherit its consequences.

A portfolio drawn solely from the requests of established teams reflects who already knows how to ask for resources. Early-career researchers, people at smaller institutions, and researchers working across disciplinary boundaries may encounter a more basic barrier: they cannot obtain a short trial of a facility or methods service without demonstrating a track record that the trial itself would help build. The infrastructure can reserve limited exploratory access with clear criteria and mentoring, while still asking whether the proposed observation is interpretable and safe. Fairness is not a promise to fund all requests equally. It is a way of making promising questions legible before institutional seniority has determined their fate.

This choice has operational costs. Protected exploratory slots reduce the time available to existing projects. They need support to make the trial meaningful, not merely an open booking calendar. A newcomer may need help specifying a sample, learning the instrument's limits, or drafting a data plan. The portfolio should budget those supports openly and ask whether the exploratory route is producing answerable questions, not simply counting how many people used a slot. If the route is rarely used, the institution should investigate its discoverability and entry requirements before declaring that there is no demand.

A question can also arrive from outside the institution's usual field. The portfolio needs a way to listen without claiming that every external concern is already a research project. The responsible first step may be to frame the uncertainty with the affected people, examine existing evidence, and identify whether a new study would change a real decision. Asking a community to participate in data collection before agreeing on what the result could do is a poor start to a long research relationship. The portfolio's job is to create a credible path from concern to answerable inquiry, including the possibility that the needed response is not another experiment.

Review points work best when a team specifies what it expects to learn at each stage. For an instrument development program, the first question may be whether a measurement is stable enough to compare repeated trials, not

whether the full scientific theory has been confirmed. For a collection project, the first stage may test whether custody and consent arrangements are workable before scaling recruitment. A failed stage can close one route while leaving the underlying question important. The review should distinguish a dead end in the method from a dead end in the problem.

There is a cost to stopping too quickly. If reviewers demand a publishable result from every stage, work that produces foundational methods will be systematically undervalued. There is also a cost to continuing without a credible path: the program absorbs staff and facilities while making no progress on the uncertainty it was chosen to address. Review can ask three practical questions. What did the stage show, including uncomfortable observations? What has changed in the plan because of that evidence? What resource is required for the next discriminating step, and why is that use preferable to another use now? The questions do not eliminate judgment, but they make a continuation decision accountable.

The portfolio should publish its process and protect confidential details where needed. A public or internal summary of why a class of work is supported can guide future proposals. Individual personnel matters, sensitive records, and unpublished scientific ideas require appropriate boundaries. Transparency is a design task, not a demand that every document be exposed. When done well, it allows researchers to understand the institution's priorities while retaining room to challenge them and propose questions nobody anticipated.

A refusal is an instrument of the system, not merely an ending. A proposal that misses selection still represents learning: a question someone believes worth asking, a method someone has begun to imagine, and a gap between the institution's capacity and its community's needs. The portfolio can capture that information rather than discarding it. A short record of the refusal, its dominant reason, and what would need to change for the proposal to return gives the institution a map of demand it chose not to meet. Over a cycle, those records show whether refusals cluster around a missing capability, a missing methodological service, or a mismatch between the questions researchers ask and the questions the institution rewards.

The refusal record also disciplines the reviewers. If a proposal is rejected for lacking a comparison, the next applicant should be able to learn what kind of comparison would strengthen the work. If it is rejected because the institution has no capacity for a measurement, the record names the gap rather than implying the science was weak. Feedback is a service the portfolio provides to its community, and it costs reviewers little when written at the moment of decision. A form letter that could apply to any proposal teaches nothing and

quietly trains applicants to treat review as a lottery.

Some refusals should be revisited. A proposal rejected for capacity reasons may become feasible after an instrument is commissioned or a partnership formed. A question rejected as premature may become urgent when new evidence arrives. The portfolio can keep a small reviewable list of such cases, not to relitigate settled decisions but to notice when circumstances have genuinely changed. An institution that never revisits a refusal assumes its initial judgment was perfect, which is a claim no review process can honestly make.

The portfolio's view of what is worth investigating is shaped by what reaches it. Proposals arrive from people with time, confidence, and institutional support to write them. Fields with strong funding traditions produce more applications than fields where the method is still forming. Senior researchers may frame questions they already know how to answer, while genuinely novel questions arrive in less polished clothing. A portfolio that simply ranks what arrives optimizes for whoever is best at requesting, which is not the same as whoever has the most important uncertainty to resolve.

The institution can look for the distortions deliberately. It can ask which groups have never submitted, and whether that reflects a lack of interest or a barrier at entry. It can invite methodological problems as a category of proposal, since a group struggling with a measurement may not frame its difficulty as a research question even though solving it would unlock many. It can examine whether its review criteria systematically favor certain designs, and whether the favored designs are actually more informative or merely more conventional. The exercise does not demand quotas; it asks the portfolio to know the shape of its own blind spots.

A portfolio's most consequential decisions are often about questions it declines to pursue. Some questions are premature: the method to answer them does not exist, and building it would consume capacity that other questions need more. Some are already being answered elsewhere, and duplicating them would waste an instrument hour that could test something unexamined. Some should be declined on ethical grounds, not because the knowledge would be worthless but because the way of obtaining it would be wrong. Declining for a reason is different from declining by omission; the portfolio should be able to state which questions it considered and why it declined them.

Declining on ethical grounds deserves particular care, because the refusal itself must be reasoned and not merely reflexive. A question that touches a vulnerable population may be answerable with a redesigned method that protects participants more strongly than the original proposal. A question with

dual-use risks may be investigable within defined boundaries and reporting duties. The portfolio's ethical review should be able to distinguish a question that cannot be asked responsibly from one that needs a different design, and to say which of the three it has decided, in terms the next proposer can act on.

Ethical review is usually described as a gate, but it works better understood as infrastructure: a service whose product is studies that can be defended later. A review body that only approves or blocks sees each study once, at the moment of least information, and its judgment ages as the study's reality diverges from its protocol. A review body built as infrastructure stays connected: it learns what actually went wrong in past approvals, revises its guidance, and treats a protocol amendment not as a nuisance but as the system working. The difference shows up years later, when a question arises about a completed study and the institution can show not just a stamp but a continuing record of care.

The process should also protect the people it reviews. A researcher navigating an approval cycle with ambiguous requirements learns to guess, and guessing produces both over-compliance, which wastes research time on unnecessary protections, and under-compliance, which leaves participants exposed. Clear published criteria, realistic timelines, and a consultation route before formal submission are not conveniences; they are how the process makes honest work easier than corner-cutting. An ethics process that researchers fear or evade has already failed at its one job, because its influence ends at the moment people stop coming to it.

Finally, the process must keep its own records to the standard it demands of studies. Decisions should be recorded with reasons, so a later reviewer can see what was known at approval time and what changed afterward. Participants' undertakings, consent documents, and data promises should be preserved as long as the material they govern. The irony of an ethics process that loses its own records is not merely embarrassing; it makes the institution unable to answer the questions its careful review was supposed to anticipate.

Some research questions outlast the careers of the people who begin them. A longitudinal study, a long-running field site, or an instrument development program may pass through several generations of staff, each of whom inherited commitments they did not make. The portfolio layer owes these questions a form of governance that does not depend on the founding team's continued presence: a written charter stating the question's purpose, the decisions reserved for review, and the conditions under which the program should end. Inherited work otherwise drifts, defended by momentum rather than by evidence, or collapses when its informal keeper retires.

The charter also protects the newcomers. A researcher joining a twenty-year program should be able to read what the program promises, what it has already established, and where its open questions lie, without reconstructing the history from correspondence and lore. That document is itself a research product, and funding it is part of the program's cost. Institutions that treat succession documentation as overhead eventually pay for its absence in the currency of abandoned questions, some of which will have consumed decades of other people's effort.

The portfolio's decisions teach most when their reasons travel. A selection process that writes down the dominant reason for each refusal gives the next round of proposers a curriculum: what the portfolio valued, what it found missing, and what would make a similar question competitive later. The curriculum works only if it is honest, because proposers read encouragement into ambiguity, and a diplomatic refusal that softens the reason teaches applicants to fix the wrong weakness. A refusal that names the actual reason, kindly and specifically, is a gift to the person who receives it.

Some research questions exist because the public will live with the answer. A question about water, air, safety, or health carries a stakeholder who does not submit proposals and does not read journals, and the portfolio's processes need a route by which that stakeholder's interests are represented without being performed. Representation can be practical rather than ceremonial: a committee seat for a practitioner, a consultation with affected communities before the design is fixed, or a standing requirement that consequential questions state whom the answer is for. The route's quality shows in the outcomes, and the test is whether the portfolio can name, for any public-facing question, whose interests the design protected and how.

The portfolio's most useful service to a proposer is occasionally a refusal to answer the question as asked. A proposal that asks whether a technique improves accuracy may be driven by an operational problem the proposer cannot state yet: measurements keep disagreeing, and nobody knows which side to trust. Answering the question as asked would produce a number while leaving the operational problem untouched. A review conversation that asks what decision the answer will inform often uncovers a different, better study, and the portfolio that holds those conversations becomes a research instrument itself.

The practice requires restraint in both directions. The portfolio should not rewrite every proposal to its own taste, because proposers know their fields better than reviewers do, and an odd question may be exactly the right one. But a short structured exchange, what will you do with the answer, what would

you do if it came out the other way, is cheap, and it sorts the questions that are real from the questions that are merely available. The portfolio's deepest function is not the allocation of money; it is the sharpening of what its community knows how to ask, one conversation at a time.

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ABOUT THE AUTHOR

Aiman JoeHanz writes the Building Infrastructure of Tomorrow series, dismantling the systems that carry modern life so the reader can name every component and see how the loads move. In this volume the wrench turns on the machinery of knowledge itself: the portfolios, laboratories, records, and review structures that decide whether a society can trust what it claims to know.