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The Value and Evaluation of Basic Research

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In Brief

The primary goal of scholarly research is to create new knowledge. Because this knowledge can be used in a variety of ways within society, its production is supported with public funds. Core funding for the production of knowledge is thus tied to the expectation that knowledge serves, at least indirectly, a societally relevant purpose. In the case of basic research, this purpose is not apparent at the time the research is conducted, because its goal is to gain insight without immediate practical benefit.

This paper aims to structure the discussion of the value of basic research and its appropriate evaluation in such a way that its value is apparent even when it does not manifest itself in immediate practical benefit. To this end, the paper proposes to distinguish between three levels of research evaluation, each with its own evaluation and justification criteria:

Research project level

Is the project suitable for generating new, reliable knowledge with reference to a specific research program? A project is evaluated based on scholarly quality criteria (methodological validity, transparency regarding theoretical assumptions and empirical foundations, comprehensibility of how results are derived, internal consistency, and positioning within the scholarly literature). The project should close a specific, clearly defined knowledge gap without claiming to have an (immediate) societal impact.

Research program level

To what extent does the underlying research program increase the potential for knowledge to have a societal impact in the broadest sense (understanding, explaining, structuring, or shaping the world)? A program is evaluated based on whether its core assumptions are tenable both within and outside *Wissenschaft** and whether it creates the theoretical and empirical foundations upon which societal impact is built. A decisive criterion is whether the program provides a structure for knowledge within which central questions of societal relevance can be investigated and research projects can be embedded.

Societal impact level

What impact does the use of knowledge from a research program have on society? Here, the societal benefit of using knowledge is evaluated. Examples include medical diagnostic and therapeutic procedures, technological innovations, but also the sheer interest in gaining insight (e.g., into how the universe came into being) or the creation of personal meaning. The value is measured by how strong and how lasting this impact is with regard to societal values or interests.

Based on this threefold distinction, argumentation strategies can be developed that clarify the contribution of basic research:

At the research project level, the focus is on describing the theoretical or empirical gap to be closed. The goal is to show how the project contributes to creating, probing, or extending the foundations of a research program.

At the research program level, the aim is to demonstrate how the structuring of knowledge serves to foster understanding and increase the potential for societal impact. Basic research creates the conditions necessary for new research programs to emerge and for existing ones to be further developed. Without a structured knowledge base, research would remain confined to existing research programs.

* Preliminary note: In the absence of a precise English equivalent, the German term “Wissenschaft” is used throughout this translation to denote the systematic pursuit and production of reliable knowledge across the natural sciences, social sciences, the humanities, and the formal disciplines such as mathematics.

Retrospectively, **at the societal impact level**, historical examples can demonstrate how applications often become possible only decades after the initial basic research was conducted. Such retrospectives highlight the indirect but essential role of basic research for societal impact.

Finally, the paper proposes concrete measures to strengthen basic research:

Incentive systems

Funding modules should comprehensively cover research programs –from basic research through translational research to infrastructure, communication, and knowledge transfer. Particular importance is attached to the establishment of knowledge infrastructures (e.g., databases, repositories, libraries). In calls for proposals, a potential practical benefit should not be a factor; instead, the focus should be on the scholarly quality and originality of the research.

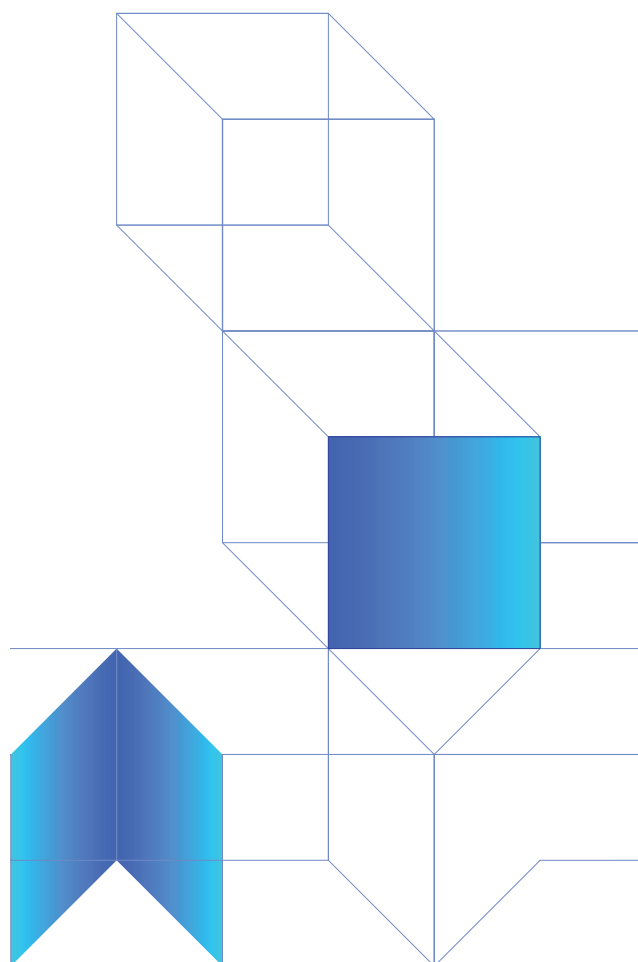
Communication efforts

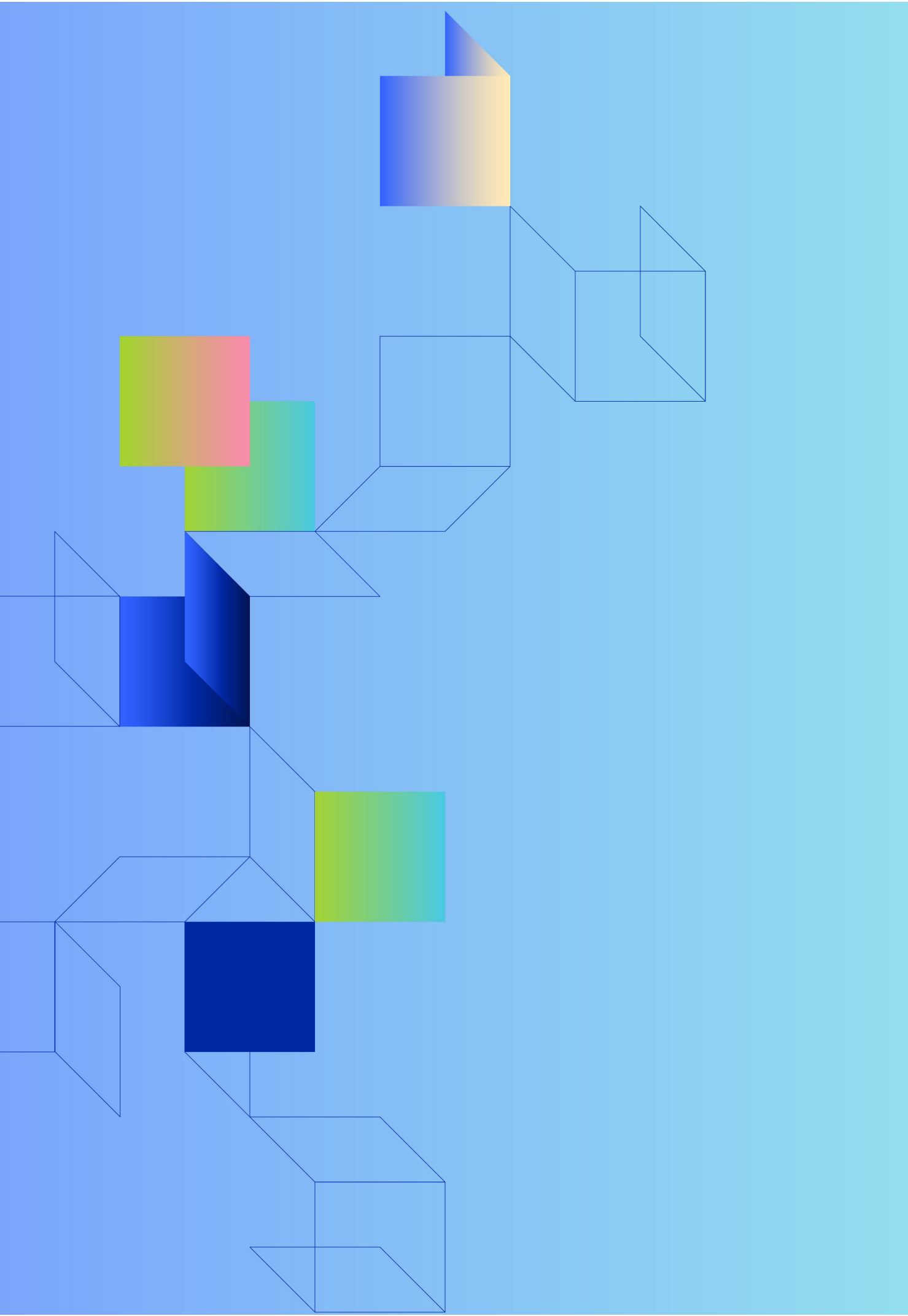
Criteria for evaluating basic research projects must be communicated transparently and in a way that is understandable to the public. At the same time, success stories should be presented in a way that clearly highlights the duration and the original research focus to avoid unrealistic expectations of “quick” results in practice.

Evaluation

Projects should be evaluated prospectively on whether they deliver systematically organized and reusable knowledge. Retrospective evaluations of programs should be based on time horizons spanning several years to several decades, as appropriate to the field of research, to adequately capture potential societal impacts.

Overall, the working paper shows that basic research should not be measured by (short-term) utility in the economic sense, but rather by its ability to create reliable knowledge that can only lead to concrete societal impact in the medium to long term. Although publicly funded research is justified by its societal impact, requiring every research project to deliver immediate practical benefits can, in the long run, undermine the ability of Wissenschaft to deliver such benefits. A clear division of labor between the project, program, and societal impact levels during evaluation, along with tailored funding, communication, and evaluation mechanisms, ensures that basic researchers can continue to conduct their research freely while contributing to long-term societal progress.





Wissenschaft as a System in Society

Wissenschaft occupies a central position in the modern knowledge society. It serves to understand, explain, structure, and shape the world. It owes this position to the societal impact resulting from insights and techniques acquired through Wissenschaft.

Scholarly work provides the foundation for effective medications, stable bridges, and sustainable energy sources. It helps identify societal problems, understand human action, and point out logical fallacies. It offers approaches to understanding fundamental questions of existence, ranging from an individual's personal origins through the positioning of societal events in their historical context to natural-history descriptions of the formation of the Earth and the universe.

Practical Benefit

It is the practical benefit of these various forms of societal impact that provides the justification, in large parts of society, for defending academic freedom and funding scholarly research with public funds. However, this benefit does not materialize immediately or in every case; rather, it is the result of a long process that often begins with research focused on fundamental questions that are, for the most part, relevant only within Wissenschaft.

One example is the current upheaval brought about by statistical models, which are generally grouped under the term “artificial intelligence.” The researchers who first described so-called “neural networks” in the 1940s could hardly have imagined that their attempt to use them to explain how the brain works would one day help computers drive cars, write texts, and produce deceptively realistic images and videos.¹

Research Means Wanting to Know

This example shows that the long-term value of scholarly work does not lie in immediately changing the world. Its value lies in making knowledge available in a structured, comprehensible, and accessible way so that it can be retrieved and used when needed – whether that moment comes in a month, a year, or only after decades. Research means “wanting to know”; the results of scholarly research are statements about phenomena in and of themselves.² This also means that it need not be the task of Wissenschaft to determine, initiate, or actively promote the economic, societal, or political use of knowledge.

This paper discusses why basic research is necessary for Wissenschaft to yield a practical benefit, and how the value of basic research can be justified and communicated without direct reference to its practical benefit. In doing so, it takes up an older debate shaped by figures such as Vannevar Bush and Donald Stokes.³

No Wissenschaft Without Basic Research

In the following, the term “basic research” refers to scholarly work aimed at creating, probing, or extending the foundations upon which entire research programs – all the way through to application – are built. At its core, basic research involves developing internally consistent statements about the world that are open to scrutiny, as well as describing the conditions under which these statements and the resulting knowledge hold. The focus is on the precise description, understanding, and explanation of phenomena, not on the question of what benefit outside Wissenschaft can be derived from the resulting descriptions, insights, and explanations.

Conflicting Expectations

Nevertheless, basic research finds itself caught between two conflicting expectations: On the one hand, looking ahead, it is expected to gain fundamentally new insights into the world. Because this requires the ability to conduct research free from practical constraints, it is accepted that it need not deliver an immediate societal benefit. For this reason, a substantial part of the funding required for such research is provided by the public sector.

On the other hand, the value of basic research is retrospectively measured almost exclusively by the economic, societal, or political benefit its results have yielded. Many have criticized and disparaged the aforementioned research on neural networks over the past decades, precisely because its practical benefit remained limited for a long time.⁴ Now that this benefit has become apparent, the evaluation of the original research has also shifted for the better. Research that was long dismissed as an intellectual dead end or a useless gimmick has now forcefully demonstrated just how much impact it can have.

Distorted Evaluations

However, the retrospective over- or undervaluation of past research projects or programs can severely distort the forward-looking evaluation of basic research. This occurs when the decision on which basic research projects to fund is no longer guided by the insight to be gained but by the hoped-for practical benefit. As a result, scholarly work is measured – both by the public and within the academic community – by what can be done with it in practice. It is not the knowledge created, but the immediately resulting practical benefit that is considered the determining factor in evaluating research performance. This, in turn, increases the pressure on researchers to demonstrate such a benefit through their work – even before the knowledge from which this benefit is to arise has been created.

Sociopolitical Claims on Basic Research

Given the central position of Wissenschaft in society and the associated expenditure of public funds on research, policymakers and the general public have a perfectly legitimate claim to have a say, to a certain extent, in how these funds are used.

This involvement generally occurs at the level of general expectations and hopes placed on the results of scholarly research, for example in the form of strategic-political⁵, macroeconomic⁶, or societal⁷ advantages, demand⁸ by individual actors for labor, means of production, or products, and support in understanding and interpreting the world⁹.

Infringing on Academic Freedom

From the perspective of Wissenschaft, problems arise when the desire for a say in decision-making shifts from the level of application goals and general expectations of Wissenschaft to the disciplinary level and the level of individual projects. This can manifest itself in such a way that basic research projects are measured based on whether they yield a practical benefit, whereby the value of research projects is assessed not by quality criteria from within Wissenschaft but by notions of benefit from outside it. This has occurred in the past, for example, during media controversies over the benefit of basic research projects funded by the Swiss National Science Foundation¹⁰ or during the regulatory evaluation of applications for research approval¹¹.

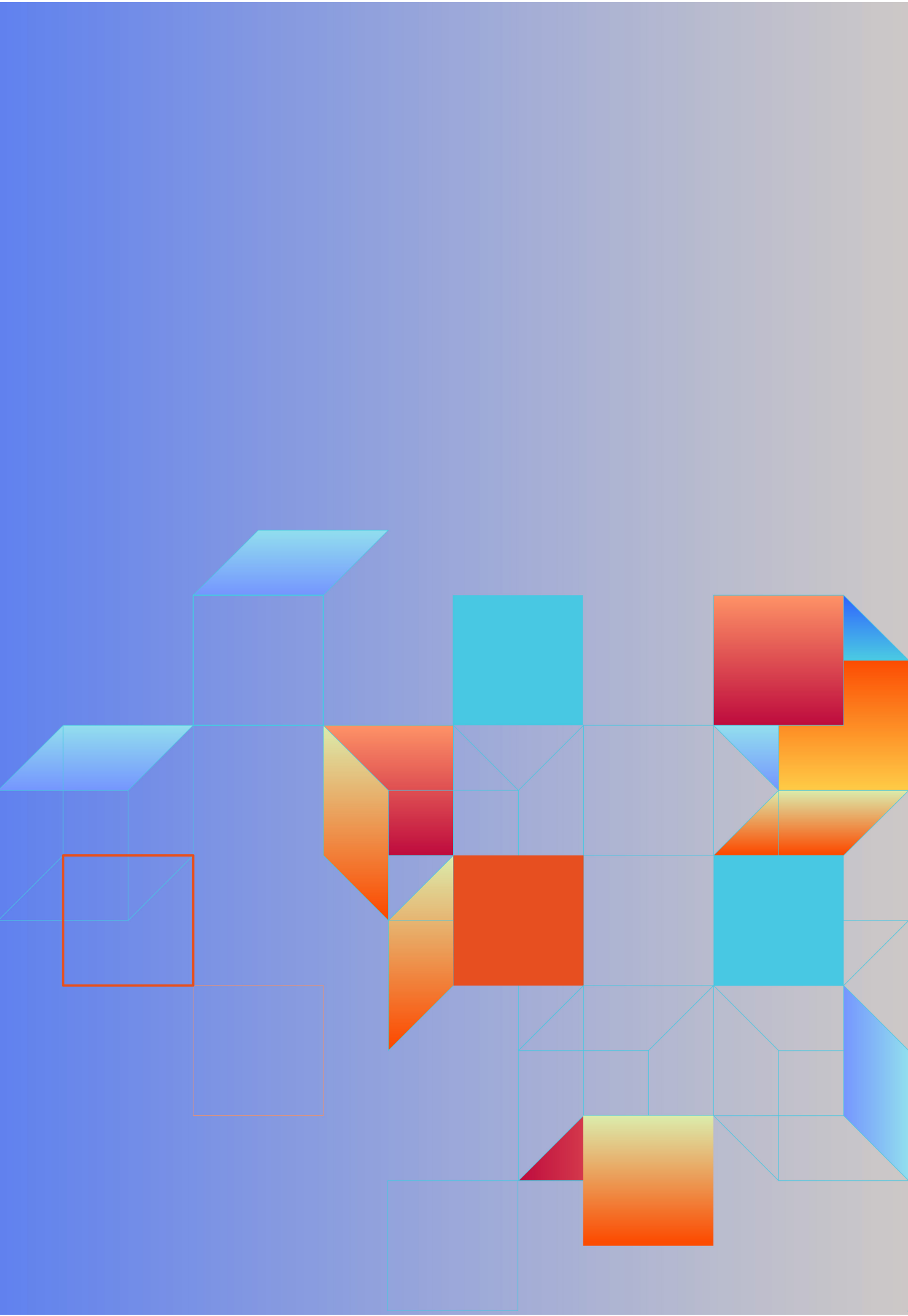
This is particularly problematic in the field of basic research, as it pressures researchers into making promises of success that cannot be fulfilled: They want to – and are expected to – produce fundamentally new knowledge through their work, but are also expected to reliably predict, on top of that, what benefit can be achieved outside Wissenschaft as a result. Such an endeavor is doomed to fail because it imposes standards on basic research that cannot and should not be inherent to it.

Such a development is counterproductive from both a scholarly and a societal perspective. From a scholarly standpoint, orienting research too strongly toward immediate utility carries the risk that methodological quality standards and the acquisition of knowledge will fall by the wayside. From a societal standpoint, Wissenschaft no longer lives up to the expectations that justify its special position in society and the freedoms that come with it if knowledge is created solely to serve as a short-lived intermediate product for achieving a practical benefit.

Perverse Incentives

Here, too, the research on artificial intelligence provides a good illustrative example: Because exaggerated expectations regarding the practical benefit of this research were stoked in the past, these expectations were repeatedly disappointed, leading to so-called “AI winters,”¹² during which available research funding was drastically reduced. Although research on artificial intelligence continued even during these scholarly “ice ages,”¹³ the ups and downs in research funding driven by the promise of a practical benefit led to perverse incentives that hindered research activity in this field.

The approach pursued in this paper is to structure the discussion on the value of basic research in such a way that this value is apparent even when it does not manifest itself in an immediate practical benefit. At the same time, the aim is to call for a division of labor in the evaluation of research that takes into account society’s claim to a say without encroaching on the disciplinary freedom of Wissenschaft. This serves to reconcile two positions that may at first seem contradictory, namely that publicly funded research is justified by its societal impact, and that basic research projects are exempt from having to provide such a justification themselves. This is possible because the demonstration of a benefit is not eliminated but delegated: It is provided at the level of societal impact, used as an orienting framework at the level of the research program, and taken as given at the level of the research project.



Three Levels of Evaluation of Scholarly Research

Society's mandate to Wissenschaft is clear insofar as its goal is to create reliable knowledge. However, which knowledge is considered particularly desirable is subject to an ongoing process of negotiation in which interests from both within and outside Wissenschaft play a role. If interests from outside Wissenschaft gain interpretive authority even within Wissenschaft, there is a risk that scholarly values and standards will fall by the wayside.

What is proposed is a division of labor in the evaluation of scholarly research in which different segments of society can make claims on the system of Wissenschaft as a whole, while trusting that the functioning of this system ensures that these claims are accommodated. Conversely, this entails a responsibility on the part of actors within Wissenschaft to live up to this trust and to ensure, at the disciplinary level, that scholarly quality criteria are observed and that basic research has its place alongside applied research.

A distinction is made between three levels that are intertwined but at which evaluation proceeds according to different criteria.

Societal Impact Level

At the heart of this level is the use of knowledge in practice outside Wissenschaft. This use includes methods, techniques, and products, as well as mere knowledge used outside Wissenschaft. Societal impact is to be understood in a broad sense: It encompasses everything that serves to understand, explain, structure, or shape the world in practice outside Wissenschaft.

The manufacture of effective medications, the construction of stable bridges, or the production of sustainable energy sources are thus just as much a part of societal impact as the identification of societal problems, the establishment of efficient procedures in the event of a disaster, or the understanding of human action.

The creation of personal meaning, placing political events in a historical context, cooking or gardening tips, research-inspired "life hacks," the use of technologies in the entertainment industry, or the description of the origin of the universe are also found at this level. Essentially, this level covers all forms of use of knowledge produced in Wissenschaft for purposes outside Wissenschaft.¹⁴ The specific uses to which scholarly insights are put, and the resulting societal impact and its value, are beyond the direct control of Wissenschaft.

Research Program Level

A research program here refers to the theoretical-methodological and empirical framework within which knowledge is created. The term is borrowed from Imre Lakatos.¹⁵ For Lakatos, a research program is characterized by a "hard core" of fundamental assumptions and a "protective belt" of revisable auxiliary assumptions. The development of a research program is guided by a negative and a positive heuristic. The negative heuristic prescribes that the hard core not be abandoned in the face of conflicting findings, but that the auxiliary assumptions be adjusted first. The positive heuristic, by contrast, comprises indications of the direction in which the research program should be developed further. For Lakatos, the scholarly strength of a research program is shown above all by whether this further development yields new theoretical and empirical content. Building on this, the potential of a research program is understood here as its capacity to open up

further tractable questions and thereby to structure future knowledge production. A research program thus not only orders knowledge that already exists; it also opens up paths of development along which knowledge can arise that becomes available for later questions.

Protecting the hard core of a research program is productive insofar as its presuppositions need not be probed in every piece of research. But it also comes at a price: What is routinely presupposed must be probed separately. This is precisely what basic research accomplishes: It is the engagement within Wissenschaft with those assumptions, concepts, procedures, and empirical findings that are presupposed in a program's applied research practice.¹⁶ Practical application matters for basic research only insofar as the research program within which it operates may itself be oriented toward practical applications.¹⁷

More Knowledge, Greater Potential

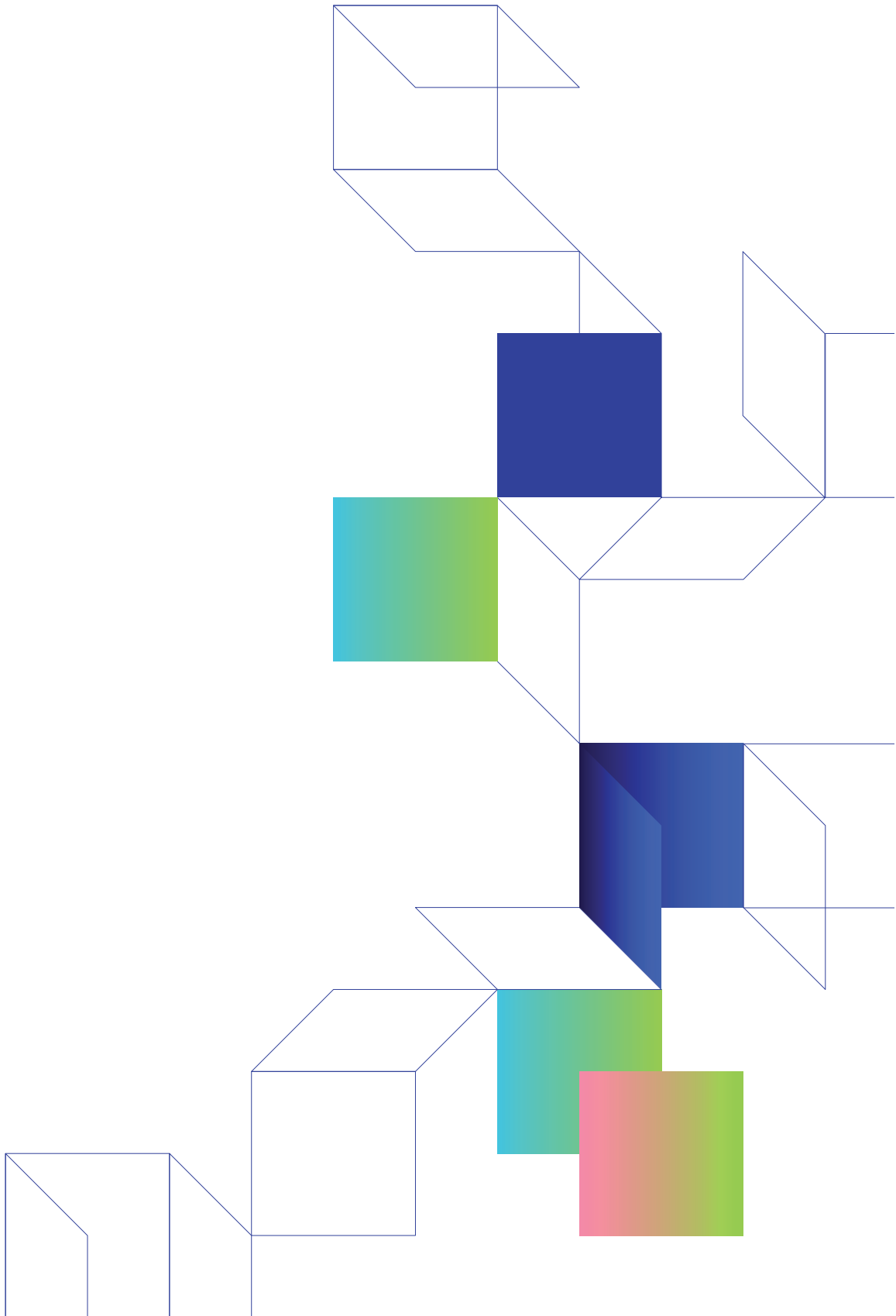
In short, research programs provide a framework for both basic and applied research, one that individual research projects take their bearings from. Whereas applied research works with the presuppositions of the research program, basic research works on them. The knowledge created within a research program does not aim to have an immediate societal impact; it serves to increase the potential for such an impact. The more knowledge is created in a structured, comprehensible, and accessible way, the greater this potential becomes. In this sense, research programs constitute the hinge connecting the research project level to the societal impact level: They help structure the knowledge that individual research projects create, which in turn helps put that knowledge to use in achieving an impact regarded as societally desirable. The value of a research program is therefore measured by how well it helps structure knowledge so as to increase the potential for putting that knowledge to further use.

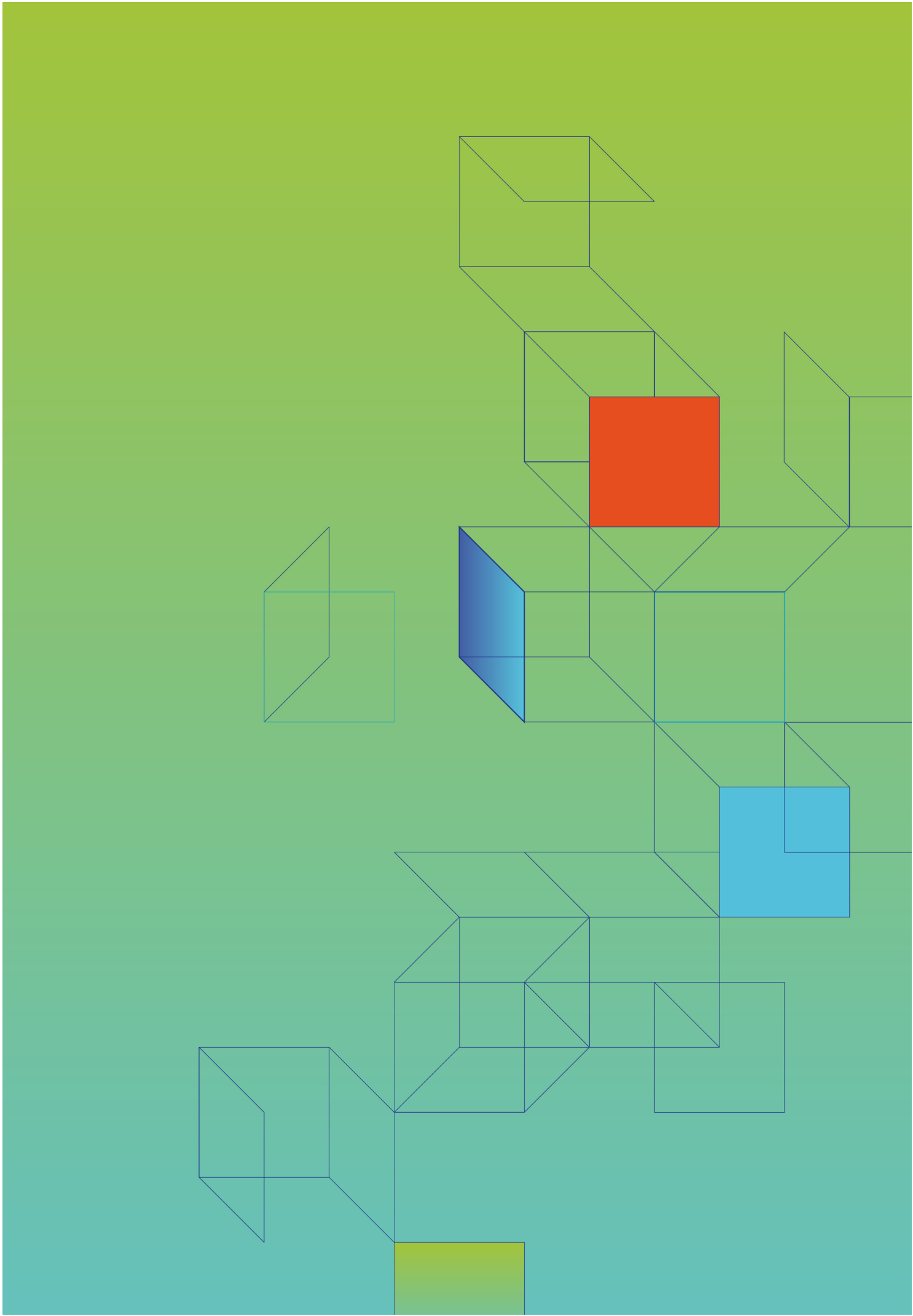
Research Project Level

A single research project focuses on a clearly and narrowly defined research question and aims to create knowledge. It is embedded within a research program and, with regard to the selection of methods, standards, and questions deemed admissible and relevant, it takes the framework established by that research program as a point of reference.

Basic research projects fulfill the following tasks in particular:

- Systematically describing, ordering, and providing access to phenomena, sources, artifacts, or data on which later explanations rest. Examples: creating taxonomies, critical editions of texts, the construction of reference corpora and long-term cohorts.
- Developing new approaches to understanding, explaining, or structuring theoretical problems or empirical phenomena. Examples: the introduction of complex numbers for the treatment of algebraic equations; prion theory as an explanation of transmissible neurodegenerative diseases; the distinction between biological sex and social gender for investigating the biological and societal causes of sex differences; the development of methods in conceptual history for reconstructing the semantic change of political and social concepts.
- Theoretically clarifying the presuppositions, consequences, and limits of scholarly methods and models. Example: clarifying the conditions under which Bayesian and frequentist test procedures lead to opposing conclusions (the Jeffreys-Lindley paradox).
- Empirically testing central theoretical assumptions and their implications. Example: the search for the Higgs boson and its detection nearly fifty years after the theoretical prediction of its existence.





The Value of Basic Research

The evaluation of research and its results is guided by different core questions at each level.

- **Societal impact level:** What benefit does the impact resulting from the use of knowledge have?
- **Research program level:** How does the research program help to structure the production and use of knowledge, thereby increasing the potential for impact both within and outside Wissenschaft?
- **Research project level:** Is the project suited to create new knowledge with reference to a specific research program? New knowledge can arise here either within the relevant research program or in deliberate demarcation from it. However, a reference point in the form of a research program is required.

This results in a distinct justification strategy for each level, following the pattern below:

The object of justification is justified by a core value relative to a frame of reference.

Specifically, this leads to the following three levels of justification, presented here in ascending order from the research project to societal impact.

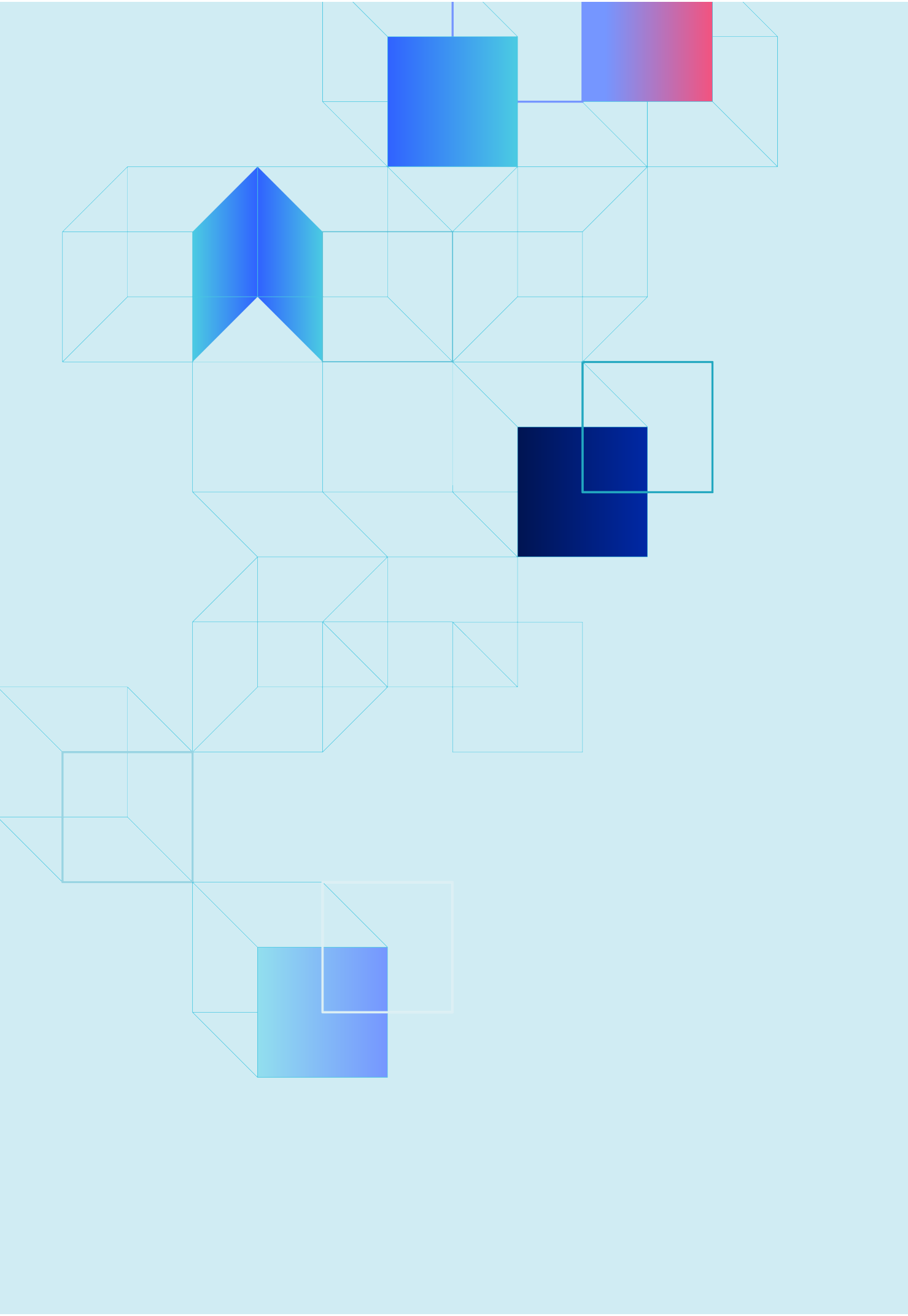
- **Level 1:** The value of a research project is justified by the knowledge it adds to the research program it takes its bearings from.
- **Level 2:** The value of a research program is justified by reference to its potential for societal impact in the broadest sense (understanding, explaining, structuring, or shaping the world).
- **Level 3:** The value of societal impact is justified by the benefit it yields against the background of societal values or interests.

	<u>Object of justification</u>	<u>Core value</u>	<u>Frame of reference</u>
Level 1	<u>Research project</u>	<u>Knowledge</u>	<u>Research program</u>
Level 2	<u>Research program</u>	<u>Potential</u>	<u>Societal impact</u>
Level 3	<u>Societal impact</u>	<u>Benefit</u>	<u>Societal values or interests</u>

	Level 1	Level 2	Level 3
Core question	Is the <u>research project</u> suited to create new <u>knowledge</u> with reference to <u>a specific research program</u>?		
Basis for explaining the value	Compliance with scholarly quality criteria that ensure the reliability of the knowledge. For example: – Methodological validity – Transparency regarding theoretical assumptions and empirical foundations – Comprehensibility of how results are derived – Internal consistency – Positioning within the scholarly literature		
Notes	New knowledge can emerge here either within the relevant research program or in deliberate demarcation from it. However, a reference point in the form of a research program is required. The evaluation of an individual project is conducted in relation to the respective research program. This allows for diversity in the quality criteria applied, which vary by discipline, and permits both practice-oriented research and basic research within the same research program. To explain the value of an individual research project, it is sufficient to show its epistemic value with reference to a research program. The objectives of a research project can also be explained exclusively with reference to the research program; that is, it is sufficient to demonstrate why a project contributes to the increase in knowledge with reference to the respective research program. The research program specifies which objectives are more or less desirable for a research project. Whether the research program itself is valuable should be clarified at Level 2.		

	Level 1	Level 2	Level 3
Core question	How does the structuring of knowledge through the <u>research program</u> serve increase the <u>potential</u> for <u>societal impact</u> in the broadest sense?		
Basis for explaining the value	Potential that a research program promises in terms of understanding, explaining, structuring, or shaping the world. General expectations and hopes placed on the results of a research program. The explanation of a research program's value draws on both scholarly and societal values or interests, with the following two questions at the center: – Does the research program address questions that are of societal and scholarly relevance? – Are the core assumptions of the research program tenable in and outside Wissenschaft?		
Notes	Societal values or interests anticipate neither decisions about practical applications (Level 3) nor answers to questions at the level of the individual project (Level 1).		

	Level 1	Level 2	Level 3
Core question	What <u>benefit</u> does the <u>societal impact</u> yield against the background of <u>societal values or interests</u>?		
Basis for explaining the value	Specific benefit that various research programs have yielded in relation to a particular impact.		
Notes	These impacts can be specific (e.g., ensuring the stability of the power grid, enabling weather forecasts, etc.) or involve very general everyday benefit (understanding fundamental physical processes, understanding interpersonal relationships, developing normative principles based on ethical analyses, etc.). Crucial to explaining the value is the impact resulting from applications that build upon the knowledge of a research program. From this perspective, the value of Wissenschaft itself is purely instrumental, in that it enables the desired impact more reliably than alternative approaches to gaining knowledge do.		



Describing the Value of Basic Research at Each Level

The division above can be made independently of whether the research in question is basic research or applied research. However, because the latter already aims for societal impact at the level of the research project, it is easier to present arguments for its necessity. For basic research in particular, the proposed division of evaluation levels is therefore a crucial tool for enabling a principled evaluation.

Basic Research as Prerequisite

The work of basic research is valuable precisely because it does not deal directly with questions of practical benefit, but rather focuses on probing, extending, restricting, and building the foundations of that on which knowledge is created. This work is central because it creates the prerequisite for what society demands of Wissenschaft: The knowledge it produces should be reliable, and this reliability increases when scholarly insights are based on theoretically consistent arguments that are open to scrutiny and, for empirically oriented disciplines, on predictions about the world that have been repeatedly tested and confirmed. However, because applied research within a research program leaves that program's core untouched, basic research is needed in order to probe, extend, restrict, or in the first place create the framework conditions of a research program. This also contributes to the reliability with which knowledge produced in Wissenschaft is used at the societal impact level: Applications that rest on a critically probed theoretical or empirical foundation will generally exhibit a higher degree of reliability than applications resting on assumptions that have not been probed – even though technical applications have historically often preceded their theoretical understanding.

Evaluation Requires Knowledge

Because what counts as fundamental in Wissenschaft can be recognized as such only in relation to the respective research program, a high degree of disciplinary knowledge and expertise is required to evaluate a research project. Even among disciplines related in subject matter or field (e.g., probability theory and mathematical statistics, neurobiology and psychology), the questions considered fundamental differ.

As a result, the role that basic research plays in a given discipline is difficult for non-specialists to grasp, let alone critically evaluate. This difficulty is compounded when it comes to communicating the value of basic research to audiences in politics, business, or other sectors of society that operate outside the system of Wissenschaft. The present division of levels of evaluation helps to overcome the associated hurdles.

General Justification Strategy

	Level 1	Level 2	Level 3
Core question	Is the <u>research project</u> suited to create new <u>knowledge</u> with reference to <u>a specific research program</u>?		
Explanatory strategies for describing the value of basic research	At the level of the individual project, the explanatory strategy for justifying a basic research project is confined to showing which specific theoretical or empirical gaps are closed with reference to a particular research program: Creating the empirical foundations of a research program Basic research is needed for the systematic description, ordering, and provision of access to phenomena, sources, or data on which later explanations rest. Building the theoretical foundations of a research program Basic research is needed in order to develop new approaches to understanding, explaining, or structuring theoretical problems and empirical phenomena. Theoretical engagement with the scope of validity of a research program Basic research is needed in order to clarify theoretically the presuppositions, consequences, and limits of scholarly methods and models. Empirical engagement with the scope of validity of a research program Basic research is needed in order to convert the central theoretical assumptions of a research program into empirically testable predictions about the world, and to test them. In all four cases, basic research aims to probe, extend, restrict, or build anew the foundation of a research program. Without critical engagement with the hard core of a research program, there is no way to identify possible inconsistencies or to determine the framework conditions of a research program.		

	Level 1	Level 2	Level 3
Core question	How does the structuring of knowledge through the <u>research program</u> serve to increase the <u>potential</u> for <u>societal impact</u> in the broadest sense?		
Explanatory strategies for describing the value of basic research	Explaining how basic research is a central route by which new research programs can arise and existing ones can be examined or discarded. Without basic research, we would remain confined to the same theoretical framework conditions.		

	Level 1	Level 2	Level 3
Core question	What benefit does the societal impact yield against the background of societal values or interests?		
Explanatory strategies for describing the value of basic research	Direct impact: Which insights from basic research are central to making a particular use possible? Examples: – The general and special theories of relativity and satellite navigation (GPS) – Boolean algebra and digital circuit design – Understanding of cell cycle regulation and modern cancer therapies – Quantum mechanics and magnetic resonance imaging – Sampling theory and the production of population statistics – Evolutionary theory and the management of antibiotic resistance Historical impact: Which insights from basic research were central along the way to making a practical application possible? Examples: – Research on the immune response of bacteria and CRISPR-Cas9 gene modification – Research on chemical signal transmission in the frog heart and the development of psychopharmaceuticals – Investigation of heat-loving bacteria in hot springs and the polymerase chain reaction (PCR) Indirect impact: Where did investigating questions of basic research require substantial investment in solving application problems whose solutions can be used elsewhere? Examples: – The search for the Higgs boson and the construction of the Large Hadron Collider – Research on how the universe came into being and the construction of space telescopes and other space technology What direct, historical, and indirect impact have in common is that they retrospectively link already existing practical benefit to insights from basic research.		

Justification Strategy in the Specific Case

	Core question	Explaining the Value of Basic Research
Level 1 Level 2 Level 3	Is the <u>research project</u> suited to create new <u>knowledge</u> with reference to <u>a specific research program</u>?	Describe the specific theoretical or empirical knowledge gap in relation to the research program.
Level 1 Level 2 Level 3	How does the structuring of knowledge through the <u>research program</u> serve to increase the <u>potential</u> for <u>societal impact in the broadest sense</u>?	Describe how the research program helps to structure knowledge in such a way that the potential for impact within or outside Wissenschaft increases.
Level 1 Level 2 Level 3	What <u>benefit</u> does the <u>societal impact</u> yield against the background of <u>societal values or interests</u>?	Describe why the desired impact is beneficial against the background of societal values or interests, and how the research program or related research programs have led to a practical benefit in the past.

Justification Strategy in the Specific Case Example: Neurodegenerative Diseases

	<u>Object of justification</u>	<u>Core value</u>	<u>Frame of reference</u>
Level 1 Level 2 Level 3	<u>Research project</u> Investigation of the role of specific gene variants in the development of neurodegenerative diseases (e.g., Alzheimer's) using genetically modified mouse models	<u>Knowledge</u> Insights into molecular and cellular mechanisms of neurodegeneration that were previously unclear	<u>Research program</u> Neuroscientific and molecular-biological foundations of brain function in both healthy and diseased states
Level 1 Level 2 Level 3	<u>Research program</u> Basic neuroscientific research on the functioning of the healthy and diseased brain (e.g., Alzheimer's, Parkinson's)	<u>Potential</u> Theoretical and empirical basis for the development of targeted therapies, diagnostics, and prevention	<u>Societal impact</u> Application of the knowledge in clinical practice, drug development, and diagnostics
Level 1 Level 2 Level 3	<u>Societal impact</u> Diagnostic and therapeutic applications in clinical practice (e.g., early detection and drug treatment of Alzheimer's disease)	<u>Benefit</u> Improved health, enhanced quality of life for those affected and their families, and reduced societal costs through prevention	<u>Societal values or interests</u> Health, quality of life, cost savings in healthcare, societal solidarity and care

The simplified justification for conducting a research project runs as follows:

- As a society, we consider health and a high quality of life to be crucial. Because neurodegenerative diseases impair both, we are interested in preventing and treating such diseases (societal impact).
- For this to succeed, we need an understanding of how such diseases arise. We gain this understanding by building the theoretical and empirical foundations regarding how the brain functions in both diseased and healthy states (research program).
- To better understand how the human brain functions, we must clarify the influence that gene expression in specific brain cells has on the course of the disease. The use of a mouse model in which a specific gene is knocked out allows us to investigate the function of that gene and thereby understand the effect of that specific gene on brain function in mice (research project).

Justification Strategy in the Specific Case

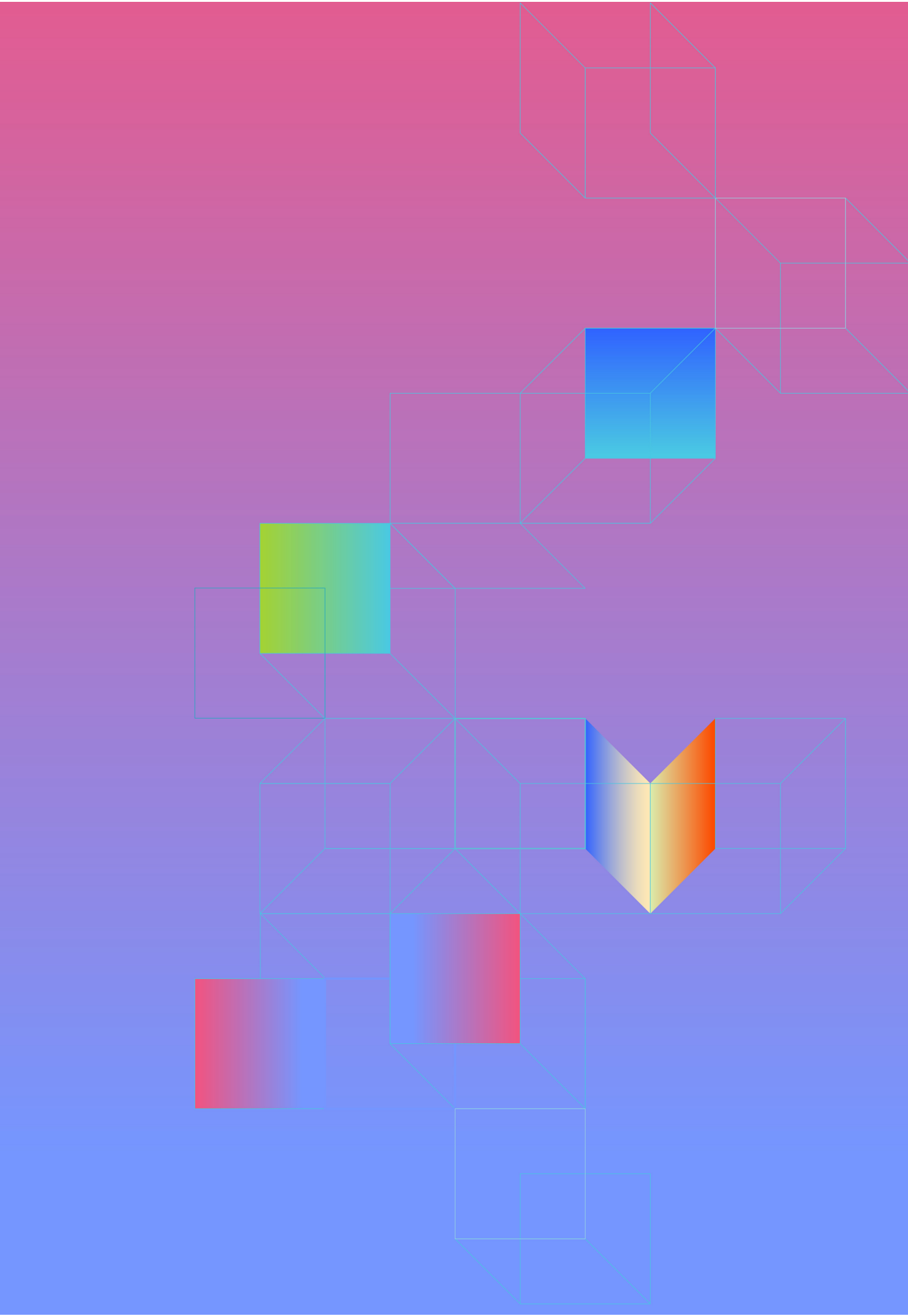
Example: Historical Semantics

The evaluation model should also hold up in cases where the impact at Level 3 arises not from a technical application but from the understanding of a matter, as is the case in the following example.

	<u>Object of justification</u>	<u>Core value</u>	<u>Frame of reference</u>
Level 1 Level 2 Level 3	<u>Research project</u> Critical edition and translation of a corpus hitherto transmitted only in manuscript, together with documentation of its transmission, dating, and textual variation	<u>Knowledge</u> An accessible corpus open to scrutiny; insights into the origin, attribution, and possible readings of the corpus	<u>Research program</u> Historical semantics: reconstruction of the semantic change of central concepts
Level 1 Level 2 Level 3	<u>Research program</u> Historical semantics: reconstruction of the semantic change of central concepts	<u>Potential</u> Theoretical and source-critical basis for putting claims about semantic change on a robust footing and for transferring them to further corpora	<u>Societal impact</u> Application of the knowledge in the interpretation of legal norms, the development of teaching materials, or the informing of public debate
Level 1 Level 2 Level 3	<u>Societal impact</u> Application of the knowledge in the interpretation of legal norms, the development of teaching materials, or the informing of public debate	<u>Benefit</u> Historically informed interpretation of norms; cultural-historical education; capacity for judgment in public debate	<u>Societal values or interests</u> Legal certainty; cultural self-understanding; being informed in democratic debate

The simplified justification based on this runs as follows:

- As a society, we consider it valuable to understand our own linguistic traditions. This understanding feeds, for example, into the interpretation of legal norms, into school instruction, and into sociopolitical debate (societal impact).
- For this to succeed, a robust reconstruction of the semantic change of central concepts of our language is needed. Historical semantics provides the framework within which such change can be described and retraced, and it supplies criteria for when a reconstruction counts as robust (research program).
- The critical edition and translation of a corpus hitherto transmitted only in manuscript closes a twofold knowledge gap in this regard: It makes the body of text accessible, and in reconstructing the path of transmission, the dating, and the textual variation, it generates knowledge in its own right about how the text came into being and which readings it permits (research project).



Strengthening Basic Research

Incentive Systems

Proposal 1

Funding for research programs should cover the following areas:

- Basic research
- Translational research
- Infrastructure work
- Communication and knowledge transfer

Rationale: Research programs serve as the hinge between individual research projects focused on knowledge and societal impact in the broadest sense. For this reason, basic research and translational research need to be closely interlocked with the necessary infrastructure and with communication and transfer work.

Proposal 2

Public research funding should specifically support research infrastructure that makes existing and newly created knowledge available in a systematically organized, findable, accessible, interoperable, and reusable manner.¹⁸

Rationale: The easier it is to make knowledge from basic research accessible in this way, the easier it is to show its added value for research programs and for societal impact.

Proposal 3

Immediate practical benefit should not play a role in the evaluation, approval, and funding of basic research projects. The evaluation of basic research projects should be based on criteria designed to maximize the explanatory power of a project.

Rationale: The essential added value of basic research within Wissenschaft lies in the ability to explore scholarly questions free from immediate practical constraints. An immediate practical benefit – if it occurs at all – arises only incidentally during the course of a basic research project and should therefore not be a criterion for funding.

Communication Efforts

Proposal 1

To explain the value of individual basic research projects, the criteria used to evaluate such projects should be understandable, clearly communicable, and should also actually be communicated. A blanket reference to expert decisions, the complexity of the evaluation process, etc., is not sufficient.

Rationale: In terms of communication, basic research lags behind application-oriented research because its subject matter rarely offers direct points of reference outside Wissenschaft. For this reason, general explanations that are understandable to the public are needed – explanations that can convincingly show why even basic research, which may seem incomprehensible at first glance, has its legitimacy.

Proposal 2

To explain the value of individual basic research projects, the criteria used to evaluate such projects should be understandable, clearly communicable, and should also actually be communicated. A blanket reference to expert decisions, the complexity of the evaluation process, etc., is not sufficient.

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Evaluation

Proposal 1

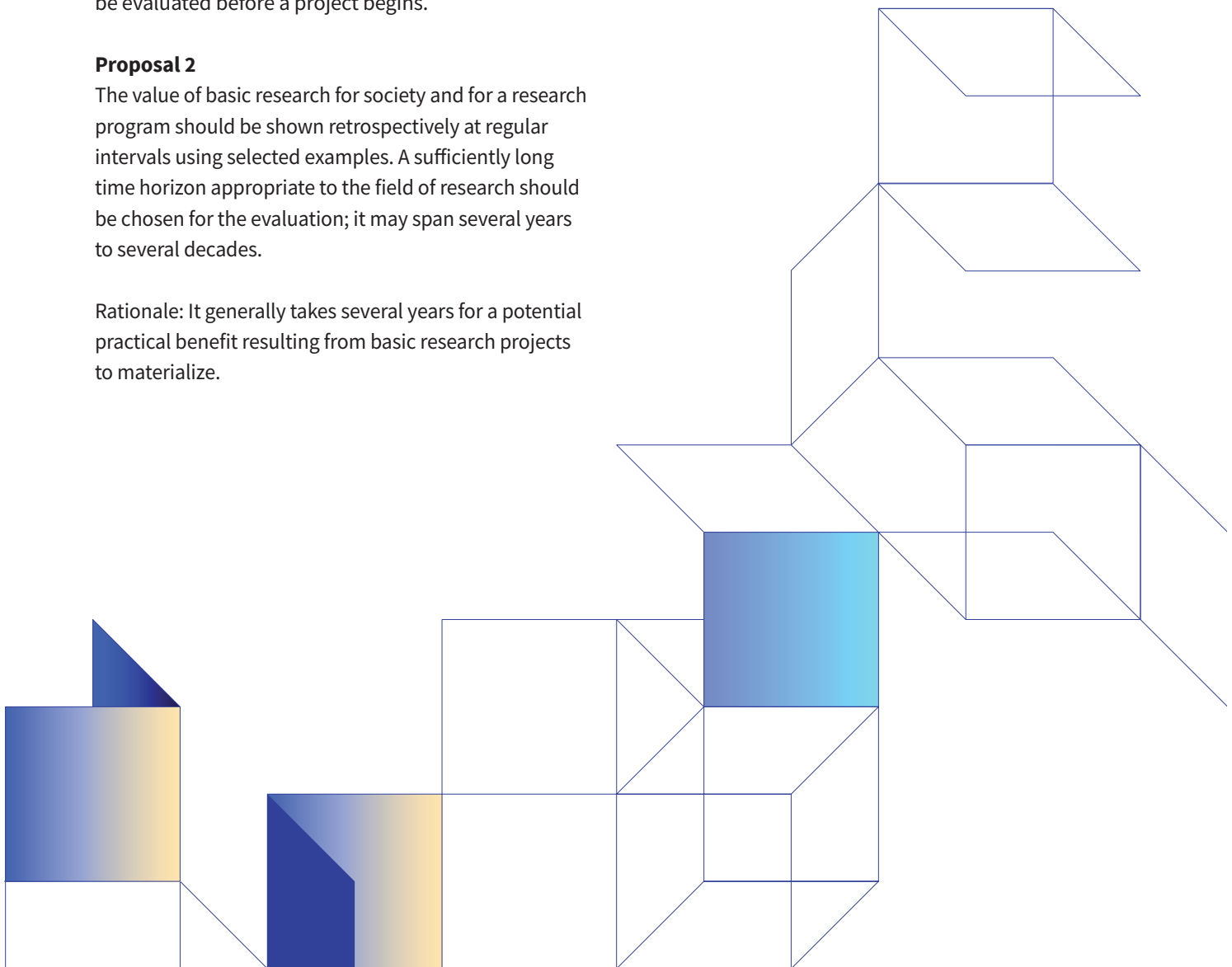
A basic research project should be prospectively evaluated based on whether it delivers systematically organized, findable, accessible, interoperable, and reusable knowledge. The risk associated with a research project should not negatively influence the evaluation, as long as it is ensured that even failed research approaches are documented in such a way that they too meet these criteria.

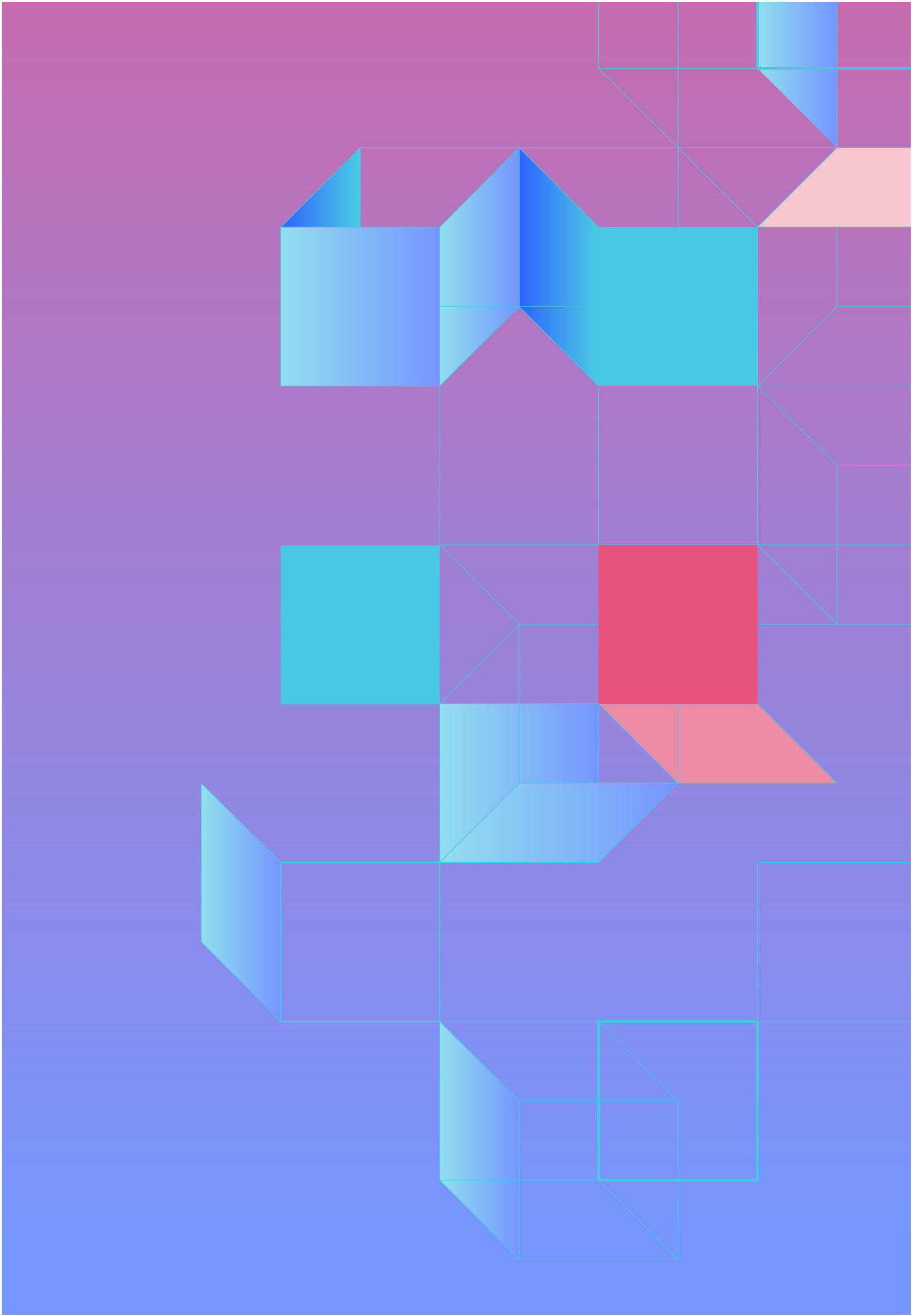
Rationale: The prospective evaluation of basic research projects must be based on criteria that can be evaluated before a project begins.

Proposal 2

The value of basic research for society and for a research program should be shown retrospectively at regular intervals using selected examples. A sufficiently long time horizon appropriate to the field of research should be chosen for the evaluation; it may span several years to several decades.

Rationale: It generally takes several years for a potential practical benefit resulting from basic research projects to materialize.





Conclusion

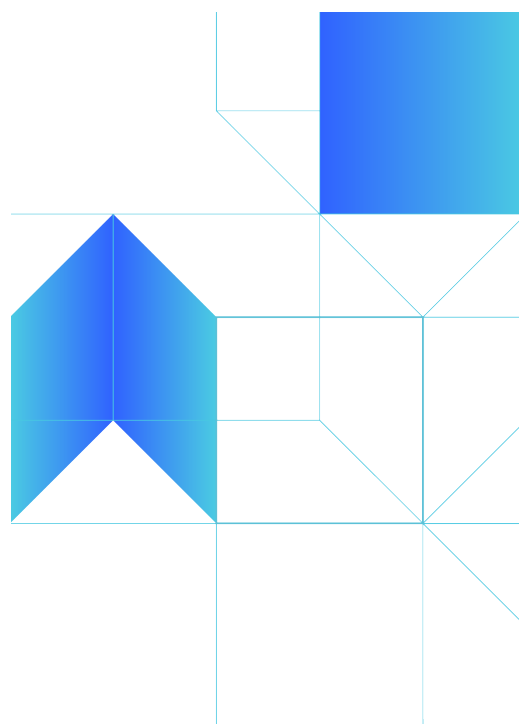
The division proposed in this working paper may seem trivial at first glance, but it helps address the right questions at the right level. Anyone responsible for an individual project in the field of basic research should not be measured by whether the project promises a practical benefit. The focus should be on whether the project promises to yield new knowledge with reference to a specific research program. This primarily involves questions regarding the validation of the methodology, the accessibility of the results, and the comprehensibility of the arguments.

Examples of questions to be asked at the level of the research project include: Has the animal model been validated, and is it suitable for answering the research question? Does the experimental protocol ensure that confounding variables are excluded and that the gain in insight is maximized? Do the experimental procedures reflect the current state of the art? Is the plan for experimental analysis well-defined, comprehensible, and statistically robust? Is it ensured that raw data and analyses will be shared in a manner open to scrutiny upon completion of the experiments?

The precise wording of these questions will differ from research project to research project and from research program to research program, but what they should have in common is that they direct attention to those aspects of research activity that increase the reliability of the insights gained from it. This serves to keep scholarly claims internally consistent and open to scrutiny, and to delineate their scope of validity as precisely as possible. Insights from Wissenschaft should therefore be evaluated above all in terms of adherence to scholarly quality criteria, and not only retrospectively in terms of their practical benefit.

A research project that makes knowledge available in a systematically organized, findable, accessible, interoperable, and reusable manner should be highly valued even if it does not aim for a practical benefit. Moreover, when evaluating basic research projects, the relevance of practical benefit must take a back seat so that the focus can be entirely on the scholarly quality and originality of the project.

Especially if basic research is to yield a practical benefit in the long term, the evaluation of basic research projects should be independent of their immediate practical benefit. What sounds paradoxical is inherent in the special role of basic research: Creating fundamentally new knowledge is what first opens up the possibility of thinking about what might be done with that knowledge in the future.



Footnotes

¹ Macukow, Bohdan (2016): Neural Networks – State of Art, Brief History, Basic Models and Architecture. In: Saeed, Khalid and Homenda, Władysław (eds.): Computer Information Systems and Industrial Management (Lecture Notes in Computer Science, 9842). Cham: Springer. pp. 3–14. https://doi.org/10.1007/978-3-319-45378-1_1.

² A classic defense of research pursued for its own sake is Flexner, Abraham (1939): The Usefulness of Useless Knowledge. Harper's Magazine 179. (<https://harpers.org/archive/1939/10/the-usefulness-of-useless-knowledge/>, last accessed 27 July 2026).

³ The distinction between basic and applied research, in the form in which it became influential in science policy, goes back to Bush, Vannevar (1945): Science, the Endless Frontier: A report to the President on a program for postwar scientific research. United States Office of Scientific Research and Development. Washington, D.C.: U.S. Government Printing Office. Donald Stokes challenged this distinction by arguing that interests in understanding and application often coincide in Wissenschaft (see Stokes, Donald E. (1997)): Pasteur's Quadrant: Basic Science and Technological Innovation. Washington D.C.: Brookings Institution Press). This paper agrees with Stokes to the extent that the tripartite distinction proposed here distinguishes not between types of research but between levels of evaluation: A project may be motivated by an application and still be evaluated, at the level of the research project, exclusively according to epistemic criteria.

⁴ Cf. Macukow (2016).

⁵ See, for example, the call for proposals for the Swiss National Science Foundation's National Centres of Competence in Research (NCCR): "The call is aimed at researchers in Switzerland who want to conduct long-term research projects on topics of strategic importance." (<https://web.archive.org/web/20260416090217/https://www.snf.ch/en/FJBj8XGQ1tjG8J8w/funding/programmes/national-centres-of-competence-in-research>, last accessed 30 August 2026).

⁶ See, for example, the ETH Board's media release on a study conducted by BiGGAR Economics on the macroeconomic value added by the ETH Domain: "The ETH Domain generates 100,000 jobs and 13 billion Swiss francs of value creation." (<https://web.archive.org/web/20250603081617/https://ethrat.ch/en/the-eth-domain-generates-100-000-jobs-and-13-billion-francs-of-value-creation/>, last accessed 30 August 2025).

⁷ See, for example, the description of the objectives of the State Secretariat for Education, Research, and Innovation regarding National Research Programmes (NRPs), the Confederation supports research projects that address societal questions and challenges of national importance. [...] NRPs are designed to mobilise part of the research community around a specific thematic focus. The topics must be socially, economically and politically relevant to Switzerland." (<https://web.archive.org/web/20260809023152/https://www.sbf.admin.ch/en/national-research-programmes>, last accessed 30 August 2026).

⁸ The distinction from economic benefits lies in the rationale: economic benefits are measured using metrics considered economically sound and/or necessary, whereas demand refers to what a specific actor within society demands from Wissenschaft as an outcome.

⁹ See, for example, the arguments in the publication "It's the humanities, stupid!" by the Swiss Academy of Humanities and Social Sciences (https://web.archive.org/web/20230427054039/https://abouthumanities.sagw.ch/dam/jcr:3aeab80f-587c-4251-bab2-eb9290be7b17/abouthumanities_d.pdf, last accessed 2 February, 2025) and in the accompanying publication by Zürcher, Markus (2016): Gegenstand, Relevanz und Praxis der Geisteswissenschaften. Eine philosophisch-anthropologische Begründung. Bern: Swiss Academy of Humanities and Social Sciences (Swiss Academies Communications 11 (5)). (https://web.archive.org/web/20250603084750/https://www.sagw.ch/fileadmin/redaktion_sagw/dokumente/Publikationen/Berichte/Relevanz_GEWI_Zuercher_2016.pdf, last accessed 3 June 2025).

¹⁰ See, for example, Grüniger, Servan Luciano (2024): "We were told: You are not systemically important." (<https://web.archive.org/web/20250117172442/https://reatch.ch/publikationen/man-hat-uns-gesagt-ihr-seid-nicht-systemrelevant>, last accessed 17 January 2025).

¹¹ For example, the Zurich Administrative Court ruled in 2022 that, when assessing an application for animal testing, only those benefits that could be directly achieved through the planned experiments may be taken into account (Judgment VB.2021.00 276, see https://entscheidsuche.ch/view/ZH_VG_001_-VB-2021-00276_2022-11-24, last accessed 14 July 2026).

¹²Floridi, Luciano (2020): AI and Its New Winter: from Myths to Realities. In: *Philosophy & Technology* 33, pp. 1–3. <https://doi.org/10.1007/s13347-020-00396-6>.

¹³Toosi, Amirhosein; Bottino, Andrea G.; Saboury, Babak; Siegel, Eliot; and Rahmim, Arman (2021): A Brief History of AI: How to Prevent Another Winter (A Critical Review). In: *PET Clinics* 16 (4), pp. 449–469. <https://doi.org/10.1016/j.cpet.2021.07.001>.

¹⁴This broad conception calls for a qualification: what is meant is the use of knowledge outside the system of *Wissenschaft*, including those cases in which this use consists in nothing other than understanding itself. The difference between the levels therefore lies not in the knowledge that is evaluated but in who uses the knowledge for what, and by what standards that use is evaluated.

¹⁵Lakatos, Imre (1970): Falsification and the Methodology of Scientific Research Programmes. In: Lakatos, Imre and Musgrave, Alan (eds.): *Criticism and the Growth of Knowledge, Proceedings of the International Colloquium in the Philosophy of Science, London, 1965 (Volume 4)*. Cambridge: Cambridge University Press, pp. 91–196.

¹⁶For Lakatos, the hard core is shielded from refutation by a program's negative heuristic. The text, however, deliberately departs from this view and assigns basic research the role of making the core assumptions of a program themselves the object of inquiry.

¹⁷Cf. the objection by Stokes to the distinction between basic and applied research mentioned in footnote 3: Research motivated by application can also work on fundamental questions.

¹⁸Modeled on the FAIR Principles in the field of research data management; see Wilkinson, Mark D. et al. (2016): *The FAIR Guiding Principles for scientific data management and stewardship*. *Scientific Data* 3(1), 160018. <https://doi.org/10.1038/sdata.2016.18>.

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The author is responsible for the content.

Note on the use of generative AI: The substantive content and ideas of this text, and the unedited first version based on them, were developed independently by the author on the basis of his own work and the interviews with the members of the working group. Substantive feedback from the members of the working group was incorporated in successive rounds of revision. Generative AI was used beforehand solely to support background research and, after the substantive work on the text had been completed, to support copyediting and proofreading. The initial English translation was prepared with the assistance of generative AI and subsequently reviewed and revised by the author. The German original version is authoritative as to content.

