

Chapter 2

Research

2.1 The Concept of Research

If orientation is missing, but is necessary or promising, we carry out research. Research, then, is goal oriented, not a shot in the dark—even though in many cases something that emerged out of the dark proved to be valuable for research. The great and general aim of research is orientation: how to improve, complete, and correct it.

Ordinarily, we associate research with science, institutionalized science, and, most of all, with the natural sciences. Great role models in this regard are genetic engineering and particle physics, both of which excavate the fine structures of the world through persistent labor in the laboratory and tremendous instrumental effort. Perhaps only space travel is similarly impressive as a type of large-scale research that smacks of adventure and the discovery of the true limits of earthly life. However, social research, historical research, linguistic research, case law, or even philosophical reflections seem to be less important offshoots of laboratory science, and their results are seemingly much less consequential.

Just as commonly, research is regarded and described as the expression of a pure quest for knowledge. Our usual understanding of research views it as driven by an interest in things that actually do not concern us at all—hence by the pure desire for knowledge characteristic of theoretical curiosity.¹

Although this certainly used to be a motivating factor—and occasionally still is today—curiosity is, at most, one side of the proverbial coin. Research that is set in motion by curiosity is beyond or above the ends and needs of living human beings. It is not driven by them, but develops separately from needs and practices. Such a concept of research draws a one-sided picture, which continues to be influential in the philosophy of science and even dominates the self-understanding of many scientists. It seems as if research was primarily or exclusively theory construction. The practical side, however, that is, the side of research activities, remains neglected and misunderstood.

¹ Cf. Blumenberg (1973), who tells the story of this “curiositas.”

This, too, is a result of the developments in the 20th-century philosophy and philosophy of science that I already mentioned in Chap. 1. When the theorists who think about knowledge and truth have studied a certain field themselves (most philosophers of science were educated in an exact science like physics or mathematics), but are not actively involved in research activities, it comes as no surprise that they equate the development of science with the development of theory. Thus, Reichenbach's distinction between the context of discovery and the context of justification (a modernization of the old distinction between genesis and validity) was regarded as quite plausible and soon made mandatory—coupled with the demand that one ought to restrict oneself to the context of justification. Needless to say that, in the context of “justification,” people only thought of formal derivations from premises and of the eradication of logical contradictions. The picture that Popper draws of science and its progress in his classic book *The Logic of Scientific Discovery*² is convincing only as far as the restriction to the context of justification (understood as the strict observation of logical steps) remains intact. In retrospect, this limitation is so extreme that it is hard to understand why Popper could receive such tremendous support. After all, in this picture of science, nothing was justified anymore. Instead, hypotheses were set up and upheld as long as they survived attempts at refuting them. Only motives (not justifying arguments) were considered for the genesis of hypotheses. But these remained largely unnoticed, because they belonged to the context of discovery.

When Kuhn threw this picture into disarray with his (by now equally classic) book *The Structure of Scientific Revolutions*,³ part of his achievement was to ignore the restriction to the context of justification and to study the context of discovery. Hence the new picture of scientific progress was more realistic. A theory is not abandoned simply because, when tested, it leads to contradictions; likewise, a different theory is not adopted simply because it passes the tests. Rather, theories are embedded in “paradigms,” which contain many nontheoretical parts, in particular personal experiences, relationships, and preferences. The progress of scientific knowledge is the triumph of one paradigm over another. Whether one paradigm triumphs over another *justifiably* is not a burning question in Kuhn's book, because the new picture of scientific progress is explained with respect to the history of science and not to problems and processes at today's “fronts” of knowledge. In retrospect, it is very easy to say that, for example, phlogiston theory was rightly succeeded by oxygen theory. But for the parties involved in this transition, it is usually a complex question without a compelling answer. Lavoisier, one of the protagonists of this “revolution” (who, tragically and ironically, became the victim of an entirely different revolution, namely the political revolution in France), abandoned the “phlogiston,” but still pursued his own concept of a heat substance (which he called “caloricum”) for decades.

If the question of justification comes up with respect to paradigm shifts, a judgment about the appropriateness of frame structures is needed. I will approach this

² Cf. Popper (1968).

³ Cf. Kuhn (1970).

problem in Chap. 5. For now, I will only say this: Regarding heat as a substance, even as an element, means framing certain phenomena (experienced partly through the senses, partly through apparatuses) in a certain way. Thus, the heat substance is *inside* the warm object, it is able to leave it, go back into it again, etc. But if heat is considered to be the motion of the object's particles, then everything is different. In that case, warming up an object means setting its parts in motion; it no longer means adding a substance. What we would now have to decide is in what sense a theory that frames heat as motion could be better than a theory that frames it as a substance. Kuhn's account of the progress of knowledge, however, does not allow us to judge this according to general theoretical and/or philosophical criteria. It does not even put the question on the agenda as the most central one. Instead, it introduces a perspective that presents the fact of theoretical change as a result of many personal and social circumstances, events, and reactions that have to be described in terms of sociology and social psychology.

As a result, the philosophy of science has for decades been preoccupied with descriptions of various social, mental, and political circumstances—sometimes even peculiar ones—that influence and, to some extent, determine the research process in the laboratory. The philosophy of science was almost perceived as a variation of the description of social and material relations within an alien and strange tribe. The real question as to why research results, theories, hypotheses, interesting effects, or explanations could be right or wrong—and in what sense frame structures might be justifiable—was replaced by the question of why certain people believe something and how they are able to move others to a similar belief. Postmodern social constructivism took over the philosophy of science and, for a time, even succeeded in replacing the concept of scientific or theoretical truth with the concept of being persuaded by just any constructions.⁴

⁴Cf. Latour and Woolgar (1979). According to this account, empirical natural science actually consists in creating any (seemingly arbitrary) constructions of order. But if we ask about the validity claim of this theory, and whether the theory itself is also merely a construction of some order, we receive the following instruction at the end of the book: "[...] we do not claim to have any better access to "reality" [than the scientists, H.W.] and we do not claim to be able to escape from our description of scientific activity. [...] In a fundamental sense our own account is no more than *fiction*" (257, emphasis in the original). Hence, the scientific presentation of neuroendocrinological circumstances in the brain, for example, is described from a sociological and anthropological perspective, which regards itself as a "fiction" and thus insinuates that it is similar to what is described. As far as the latter is acceptable, it is about as relevant as the statement that both the scientist and the philosopher of science use computers. Science as a praxis, however, has felicity structures that have to do with the confirmation and progress of knowledge. Anthropologists of science, who objectify this praxis, can only grasp the outside of the process of forming a conviction. They ignore the difference between a belief of any kind (e.g. the belief formed in a research group after the approval of their grant application) and a true belief. Such a description of science, which does not care about the (implicit) validity claims of the described praxis, is actually no description of science at all. Granted, it needed to be said that scientists act just as smartly, stupidly, and brilliantly as ordinary people. But it is a helpless aberration to seriously present this as a philosophy of science. For socio-anthropological thinking in the philosophy of science, cf. also Knorr-Cetina (1981).

In 1983, Ian Hacking's book *Representing and Intervening*⁵ was published and claimed to be the first thematization of the scientific practice of experimentation. Dingler and the circle of methodical constructivists around Paul Lorenzen,⁶ who had never abandoned the issue, were not mentioned in Hacking's book. But at least the genuine questions about science and knowledge were taken seriously again. Ten years later, Philip Kitcher's book about scientific progress was published,⁷ subtitled "Science without Legend, Objectivity without Illusions." In this book, the rampant socio-anthropological perspective was reined in, but the concept of knowledge was, as it were, personalized. Specific qualities of theories were no longer considered as candidates for the definition of knowledge, but rather the scientists' cognitive or mental states. In the conclusion of Kitcher's book, we read that science has not really progressed as gloriously as legend would have it. But by and large, according to Kitcher, things in science have at least been rational. Now, he writes, the task is to observe and change scientific practice.⁸ Since the end of the 20th century, the so-called new experimentalism has introduced approaches to mainstream philosophy of science that address the practice of science and take it seriously. There is some reason to hope that, in the future, certain things will improve.⁹

Quite obviously, scientific research in its disciplinary guise is an activity that develops standards—systems of interrelated kinds of actions that aim at known or expected felicity structures. So even in science there are regular praxes in the sense explicated here (in Chap. 1). In fact, this is actually quite typical of established scientific disciplines. It makes sense to embed research practice into more far-ranging areas of practice—into general goals, desires, and hardships—when we try to elucidate factual motives for the acceptance or rejection of theories. But these motives tell us nothing about whether the accepted theories are valid or true or whether they are at least better than the ones rejected in any factual respect.¹⁰

⁵Cf. Hacking (1983).

⁶Cf. Dingler (1938), Lorenzen (1961, 1964, 1987), Inhetveen (1983), Janich (1985), Tetens (1987).

⁷Kitcher (1993).

⁸Kitcher (1993), 390/91 ff.

⁹Ulrich Charpa develops a theoretical description of the research process that describes researchers' actions as guided by rules that are typical for the "virtues" of the successful researcher; cf. Charpa (2001). Scientific knowledge is supposed to be understood as "nothing more than the knowledge of researchers" (92). Hence Charpa does not refer to theories, but—in accordance with the perspective advanced by Kitcher et al.—to "mental facts" (93). These, however, are not mere opinions (94). They are "epistemically privileged"—which is because they rest on well-founded decisions to accept a thesis (183 ff.). Charpa calls a decision well founded if accepting T is considered to be the best way to meet a research objective (established in a reputable tradition). *Prima facie*, this is a reasonable and plausible view. It can be reconciled with the notion that T is a thesis for which we have a clear argumentative construction in the sense of the concept of dialogical justification expounded here. It should be noted, however, that traditions, even if they are reputable and have, so far, been successful, may under certain circumstances also turn out to be shackles that need to be cast off.

¹⁰This is why Lakatos wanted to separate these parts of the history of science as "external," which, however, was met with little enthusiasm. Cf. Lakatos (1974).

It is a fact that research has become very expensive in the course of the 20th century, especially in the professions that are considered to be relevant. Hence, it needs financial backers. As a consequence, its goals are determined by other people's interests. Therefore, research partly takes place in the R&D departments of private companies. In general, research provides research results. But even if these are integrated into existing theory, they are not yet knowledge. They only become knowledge through a successful realization in practical life. For this to succeed, it takes more than simply embedding research results into the factual fabric of the dominant interests. What is needed is progress in human self-understanding. Whether the changes following large-scale research in the natural sciences can become meaningful and fruitful will depend, among other things, on the results of research in the humanities and social sciences and in jurisprudence and philosophy. All these disciplines will be necessary for furthering self-understanding or for dealing with the frictions that will most likely develop.

The practice of argumentation deals with new orientations. New orientations are developed by research. That is the reason for why a theory of argument needs a concept of research. Such a concept must not be reduced to a theory-laden conception of what is going on in science labs; it has to be more general.

In order to develop such a concept, I suggest the following view: People want to find their way around. They have a basic need for orientation. First of all, as part of the general necessities of life, this need is embedded in the ordinary activities of coping with life and the world. Under certain circumstances, it may rise above these activities and gain some distance to and independence from them. If something is so unclear, incomprehensible, doubtful, or disputed that subjective assessments and dogmas dwindle, then the need for orientation appears in its purest form. To pursue this need means to do research.

More precisely, research means the following: We deliberately and intensively establish contact with the facts in question. Even though this contact is also guided by what we know about the matter, such knowledge must be used very carefully. I will soon explain why that is the case. In particular, it is important to note that such research-based contact is not merely receptive. Sensuous, emotional, and intuitive forces need to be activated for it. We must commit ourselves to the cause—not just as thinking beings but, more generally, as bodily, vital people. As I said before, in research we mobilize all the knowledge that is available of a particular subject. But at the same time, this knowledge is kept at a distance, because it may be responsible for the gaps and errors in orientation. The facts or phenomena, which manifest themselves in contact with it, are ideally examined without any reservations. Often, we need to develop appropriate procedures for this specific purpose. Depending on the topic, this requires the deployment of means and time and a specific ethos independent of individual interests. In this sense, research is a type of action that aims at “new orientation”—where orientation is to be understood in the sense of the pragmatic concept of theory developed in Chap. 1. Research is ubiquitous human behavior aimed at expanding our possibilities of understanding and acting.

Thus understood, research is multidimensional. We can distinguish between four dimensions. Later, when we are concerned with describing the practice of argumentation, we will meet these four dimensions again in a slightly different shape.

For now, we can distinguish between a theory-forming and a theory-testing, a subjective and an objective dimension. It is part of the felicity conditions of research activities that researchers avoid both being entirely detached from reality and being completely embedded in individual interests. Felicitous research strikes a balance between a commitment to one's own projects and a distance that is concerned with validity and truth. Research is by no means simply trial and error. It is rather typical for serious research that the researcher already knows something, but he needs to use his knowledge as well as keep it at a distance, because it both supports and impedes the process.

Research in this sense does not only exist in the sciences. It constantly occurs in our normal life praxis and in our ways of coping with the world. Where do we find a quiet little pub with friendly service and moderate prices in the city? How do we get our broken down car back home from a deserted country road? Why does the new computer cause such annoying problems? Small children in particular are avid researchers. In fact, their appropriation of the world is exemplary for the concept of research that I aim at here. Not only do they explore the external world and its possibilities, but in doing so they also explore themselves, that is, their own possibilities of doing and causing something, understanding, and communicating. In addition, children and young people strike an almost perfect balance between commitment and distance. Even though they usually know and understand much less than the world, organized by adults, requires them to, they have not resigned themselves (in a frustrated or modest manner) to the fact—a part of the *conditio humana*—that all knowledge and understanding is merely partial. On the other hand, they are not yet fully assured and rigid in what they already know and understand, because their knowledge is backed up by very little life experience. In general, if their acquired theories prove to be a hindrance, they are able to let go of them much more easily than adults.

We live in times of upheaval at the beginning of the 21st century. People's living conditions change at a pace that creates a sense of unreality. The motto of "lifelong learning" contains some truth. In the terminology favored here, according to which learning is not something receptive, but something active—something that activates individual questions, existing epistemic theory, and a creative intuition that, ultimately, may aid in furthering knowledge—it should actually be called "lifelong research." Conscious, awake people have always been known to do research all their lives. If common people of the future developed such a research disposition, they would be far superior to common people of the past and present. It would appear that such a development is even necessary. Because only then can they hope to find solutions to ecological, economic, and political problems that seem hopeless today.

2.2 The Limit of Orientation as the Place of Research: Question, Problem, and *Quaestio*

If orientation is missing, research is carried out. This does not imply that, where there is no research, people are oriented. Adults have usually reached a state of normal orientation: some knowledge mixed with a little more doxa. Taken together,

this is no more than a small boat on the vast ocean of ignorance. Still, this acquired bit of epistemic theory allows for some coarse navigation. This normal orientation enables the average person to get through the day, usually even through years and decades. Even if the questions become big and serious, most of the time we average people have enough wit and thick skin to repel them.

Most of the time—but not always. Sometimes we have reason to pause after all. Technical problems, interpersonal problems, and metaphysical problems can assume proportions in which the orientation deficit becomes painful. As is well known, knowledge can be bought from experts; consequently, we often ask them. But on the one hand, there are no experts for every field, and on the other hand, expert knowledge is also limited and, in particular, shot through with personal opinion—this becomes obvious time and again in questions about large-scale technologies, medicine, economics, etc. Therefore, if there is a pressing concern, we have no choice but to start research on our own, using the means that are available.

In this context, “problem” is one of my terms for the type of situation in which orientation is missing, but necessary—or at least desirable. Without a noticeable lack of orientation, there is no research. We are finite beings with a finite orientation. But few of these limits are perceived as a deficiency. Someone who knows more is not just a bit less limited. He especially knows more about the possible limits of orientation. These limits of orientation become particularly palpable in the case of gaps in understanding for which there is no explanation—sometimes not even a proper description of the circumstances in question—or in the case of contradictions, for example, if what we discover is very different from what we expected to happen.

In order to clarify this explanation, I would like to discuss some situations that are candidates for “problems.” It should be clear that most problems in the daily life of the (rich) industrialized countries are basically interpersonal problems. We have a lot of theory for such problems: ethics, psychology, communication theory, and social psychology. But this theory is both extremely context sensitive and usually not available in problematic situations. If, however, we simply continue to argue without questioning the theoretical basis, we experience episodes like the following one, which is presented as an example for an argument with a system of contrasts in Kienpointner’s *Alltagslogik*¹¹.

A husband and wife (Siegfried and Martha) get into a fight because the woman waters flower pots in the bathroom sink, which makes it difficult for the man to wash his hands.

M: The flowers need it, because otherwise they’ll be ruined. And by the way, a flowering plant costs 11 dollars.

S: Well, then you shouldn’t have bought such an expensive plant in the first place.

M: The flowers were a gift.

S: Ah! First they cost 11 dollars, now they are a gift.

M: If someone gives you a gift, then you can’t let it go to ruin.

¹¹ Kienpointner (1992), 318.

Kienpointner's analysis of the passage is this: Martha's utterance contains, on the one hand, the "conversational implication" that she has bought the flowers and, on the other hand, the explicit statement that they are a gift. Claiming that the flowers "were a gift" and that they "were bought," however, is a "very obvious contradiction."¹²

What is going on here? What is the point of this passage? Is there a deficit in orientation that needs to be resolved? Could the issue be whether it is right for the flowering plants to be in the sink? For that question, however, the difference between having bought something and having received it as a gift—and whether they are contradictory—is completely irrelevant. Is the point that the man wants to demonstrate to the woman that she does not think logically? Or does the woman want to show the man that—logic or not—she has the necessities of practical life and a sensible human behavior in mind? Does mentioning a price really imply ("conversationally") that one has bought the flowers? Is the view that one should not spend 11 dollars on flowers affected by the information that the woman has not spent the money herself? Does the demand not to let flowers that are a gift go to ruin say anything about whether they should now sit in the sink? Perhaps the point is not whether the flowers should be in the sink at all, but rather that two people, who each constantly insist on being right, have found yet another opportunity for their petty games. Do we not need something entirely different than such funny or pathetic cantankerousness? I know that this whole scenario may be regarded as something argumentative. Some people might even like to regard it as "typically argumentative" in order to demonstrate that arguing is a misguided and abstract matter.

But, on the one hand, this is not necessary. It would be enough to say that something is missing here: the question as to what the thesis is and which theory is established in order to proceed with it. If there is not enough theory, we could say that the dispute in question is not decidable argumentatively—it is, in fact, not even something to argue about. We rather have to come to some kind of amicable agreement.

On the other hand, regarding the scenario as argumentative does not make sense. Sometimes we really want or need to do research to counter a deficit in orientation, which means we have to set up theses about the states of affairs in question and discuss them afterwards. But if we devalue our argumentative competence by applying it to foolish bickering, we will not have anything left for those cases where arguments are actually needed.

These remarks arise from the conviction that the practice of argumentation is very important and precious. The rational organization of the world and the justification of human self-confidence depend on it. Hence, not every disagreement or even conflict is an occasion for argumentation, but only situations that are about testing and improving orientations.

¹² Kienpointner (1992), 318.

Let us consider some other examples from our daily life and wonder to what extent they could be opportunities for serious research:

- (1) The light in the living room does not work. The key to the house has disappeared.
- (2) A mathematical task or quiz, a puzzle turns out to be difficult.
- (3) The children want to ice-skate on the frozen pond, but we are not sure that the ice is thick enough.

2.2.1 *Are These Problems Occasions for Research?*

If the light will not switch on, a normally oriented, ordinary person tries to replace the light bulb or check the fuse. If that does not help, usually an electrician who knows the relevant practice of emergency maintenance is called in to take care of the problem. It is clear that, in this case, there is a solution to the problem. The ways to find it are also known. The fault is nothing really new. It might take a while to detect it, but this is merely due to the fact that there are several possibilities and that the inquirer is too inexperienced to find the solution right away. I would like to call this type of occasion a “task,” not a problem. Exercises in schools and universities are of the same type. In these cases, it takes existing competence to find the solution; in turn, the competence is trained by exercising it.

The same applies to the lost key. It might have been misplaced—so we check a few places where it might be. If this does not help, we call a locksmith to deal with the matter. But some people might want more: to find the key at any price, even if all possible spots have already been searched thoroughly. This might make sense if life is sufficiently well structured, so that further ways of locating the key can be found.¹³ If not, everything is up in the air. (In that case, all we are left with are rules of thumb such as “You should search under a lantern, because at least you will have some light.”) A lost key may still be a problem in that case, but only as a source of trouble and no longer as a stimulus for research in the sense outlined here.

The scenarios in (2) are of a different type. In these cases, it is also clear that there are solutions, but currently no standardized approaches to these solutions. I would like to call this type a “puzzle.” What is needed here is not so much a standardized competence, but rather general creative intelligence and imagination. Puzzles are like an equation with two unknowns: the solution and the way to arrive at it. If knowledge can be accumulated to arrive at a solution—as in the case of guessing games that, for example, require you to narrow down results by asking a game master $17+4$ informative questions—solving a puzzle can become an activity similar to research. But the conceptual difference I aim at is this: There is a solution in this case. The solution is already a part of knowledge—but not yet of the guessing person’s knowledge.

¹³ In Gabriel García Márquez’s novel *One Hundred Years of Solitude* (cf. Márquez (1984), 286 ff.), the old blind Ursula “finds” her daughter’s lost wedding ring, because she is completely aware of all her habits—a wonderful episode!

Case (3) is ambiguous. On the one hand, it is clear that there is knowledge about the bearing capacity of the ice depending on its thickness. On the other hand, this knowledge is not available to the family at the lake. The children's curiosity and confidence become a "problem" in the sense of a danger that is difficult to avert. In order to solve this problem, we "impress" upon the children that going on the ice must be "approved" first. Outside the big cities, where such "approval" is a municipal task, questions of this kind are answered by experience. Again, the solution (appropriate ratio of ice thickness and frost duration) is there, in principle; it is just not available in this specific situation. We may then do "small research" (supervised research): We conduct and analyze trial actions in order to test the thesis that the ice can already bear some weight (by throwing stones onto the surface, sending the dog, moving onto the edge of the pond ourselves, paying attention to sounds from the ice...). Each cracking sound refutes the thesis.

Let us move from the small to the big questions:

- (4) It is unclear what it means to "dispose of" the radioactive waste from nuclear power plants for many thousands of years.
- (5) It is unclear how the genetic optimization of living beings may lead to the progress of humanity on Earth.
- (6) It is unclear how German universities can become institutions capable of educating the required democratic-republican elite in the 21st century.

These questions are all problems in the sense envisaged here. We do not yet have a solution. It is not even certain that there is one. Moreover, there are no standardized approaches. It makes sense to consider a solution to (6) as a prerequisite for meaningful research regarding (4) and (5). Anyone who is concerned with the details of such problems will soon find that it is particularly uncertain whether the problem has been described properly in the first place. Complex problems are self-referential in the sense that the process of grasping the problem can be obscured and influenced by the problem itself. In that case, the problem reflects the manner in which it is perceived.¹⁴ This has implications for research, or rather the researchers need to prepare themselves for changes in the deeper layers of their understanding of themselves and the world.

To illustrate this, I will present three larger and more complex historical problems. Since we were born later, we can study the research undertaken in the past and hope to gain at least a few insights for our own research. The main insight I hope to gain is that the role arguments have played in this research will become clear.

- (7) At the end of the 15th century—Constantinople had fallen, and the overland passage to India had been lost—it was doubtful whether the Atlantic Ocean could be navigated by sea and whether India could be reached via the western

¹⁴ Cf. Watzlawick et al. (1974). In his book *Change*, Watzlawick distinguishes between first-order and second-order "solutions." In the case of second-order solutions, the real challenge is how to conceptualize the problem. The book contains very enlightening descriptions. Watzlawick's insinuation, however, that typical mental disorders are of the second-order type—and can be cured quickly and sustainably with ingenious redefinitions of problems—is likely to create illusions.

passage. Research on this issue led to knowledge about the sphericity of the Earth and to the discovery of the New World.

- (8) At the end of the 17th century, researchers of the Western world were preoccupied with the topic of combustion. Their research first led to the introduction of the substance “phlogiston,” then to its elimination, and finally to the transformation of alchemy into modern chemistry.
- (9) At the end of the 18th century, absolute monarchy as a form of government had been recognized as unsuitable and inhuman in France. Its violent abolition during the Revolution created an enormous amount of problems, for example, the specific problems of how to meet the troops of hostile European powers that far outnumbered the revolutionaries, what should happen to the abdicated king, etc., as well as the general problem of how to envision and organize the reality of a rational republic. To this day, we are still preoccupied with that problem.

The great bulk of examples that I will use in this book to illustrate, explain, and justify my proposals for argumentation theory is taken from these three historical episodes of research and problem solving. A fourth field which I will refer to was discussed above under (5): Toward the end of the 20th century, the human DNA was mapped out. Geneticists, initially restrained by a jurisprudence based on moral reasoning, have embarked on a quest for the eradication of hunger and disease by means of genetic engineering. It might sometimes seem as if genetic engineering could provide humanity with the Archimedean point from which it can control its own evolution and create the ideal human being. But life on Earth could also regress into chaos. So when I present, construct, and comment on arguments from this field, I no longer merely exemplify my ideas, but rather test whether a concept of argument that has been sufficiently clarified can contribute to the big debate about the future of humanity.

Back to the list: First of all, it should have become clear by now that not every why question, not every disagreement, nor anything that is somehow difficult induces “research” in the sense intended here. Not only those simple cases that we categorized as bickering, task, and puzzle but also the very large ones—like the meaning of the world and of humanity—are of a different kind. In the latter two cases, there is not even enough epistemic theory to ensure that we understand what the problem is supposed to be.

To summarize, the basic structure of the problem as an impetus for research consists of:

- (a) A definable gap in orientation, which has the form of an open question, a missing explanation, and a persistent contradiction
- (b) A need to fill the gap in orientation that cannot be warded off
- (c) The absence of a clear approach to a solution
- (d) A background of orientations that is available as epistemic theory in order to articulate the problem, outline a solution, and construct mental paths to arrive at it

Perhaps I should state explicitly that this concept of a problem is not quite like our common understanding of that term. It is not necessarily a “problem” if we do not know, master, or understand something or if we are unsure about how to proceed. What probably needs to be added to make it a “problem” is the threat of harm

in case the orientation is not improved. But this is not necessary in order to stimulate research—unless the noticed gap in orientation is already viewed as harmful. I will, therefore, call an occasion for research a “*quaestio*.” Medieval rhetoricians and dialecticians used this word to designate a question in need of discussion. Such *quaestiones* are not necessarily descriptive; they can also be normative. Accordingly, I regard efforts to elucidate and solve problems in the normative domain as “research,” too. By doing so, I have once again accentuated our ordinary use of language. For instance, in this book I regard the question of the proper form of the republic in postrevolutionary France and the question of a sensible use of genetic engineering as research questions. Thus, they have been accentuated differently than if we took mere *agreement* on any solution among the parties involved to be the way to resolve normative questions. Research is not primarily about agreement, but about new orientations. But this is a stipulation with regard to argumentation theory that does not say whether, and in what sense, normative and descriptive arguments are distinct. Both are only committed to the common question of how to rationally continue thinking and acting beyond all previously established practice and theory.

One final point of this topic has yet to be discussed. I said above that problems are multidimensional. I mentioned a subjective and an objective dimension. Problems can be characterized as relationships between orientations, deficits in orientation, and the need for orientation. These three instances may vary from person to person. Hence, with regard to problems, the subjective side always needs to be taken into account. A particular state of affairs is a problem *for someone*: Some things are problems for some people, but not for others. Whether or not a pair of curlews can brood in a meadow near the river Elbe is a problem for the farmer who owns the meadow and for the few environmentalists who are aware of the birds. But it is not a problem for audience members of the local theater’s premiere of *King Lear*. For them, it is a problem who has been cast as Cordelia. This, in turn, does not concern the buskers from St. Petersburg, who hope to make a few Euros in front of the theater by playing the most wonderful music. Some problems affect many people; some—such as ecological problems—affect all of us. And yet this does not mean that everyone who is affected by the problem engages with it and initiates research.

2.3 New Theory That Supports New Actions

Research about a problem means, first of all, approaching the state of affairs in question in the most unbiased way possible. In Chap. 1, I described how a theory that supports a praxis generates an increasingly distinct subject-object relationship, if the formulaic rule stage (“How-theory”) gives way to a proposition stage that elucidates and represents connections (“Why-theory”). Approaching the state of affairs in question means undermining, abandoning, and dissolving this relationship, if necessary. A problem has a certain status within orientations. It is experienced and understood in a certain way. There are different kinds of “adjacent”

epistemic theory which define the problem, act as a “bias,” and need to be put up for discussion in order to reach an appropriate solution. Ideally, all the parts of knowledge and doxa that affect the problem in theory and practice need to be reviewed and, if necessary, abandoned—including the relevant parts of one’s self-understanding.¹⁵ The researcher has to be able to become one with the state of affairs again, if possible.

In the research process, previously available epistemic theory acts as “old” theory. It plays a rather ambiguous role. On the one hand, it acts as a support; on the other hand, it blocks new insights. On the one hand, it is a corroborated requirement for capturing structures; on the other hand, it theorizes the field in question in such a way that the problem occurs in the first place.¹⁶ It is tempting to express the specific difficulty of the researcher trying to get in touch with a state of affairs as follows: He has to penetrate the old way of theorizing to arrive at the thing itself. Yet this characterization contains a misleading image. The old theory seems to be wrapped around the thing like a crust or shell. Should it not simply be discarded, so that the thing can be revealed in all its truth? This view contains a whole lot of Western ontology. The slogan of phenomenology at the beginning of the 20th century was “Back to the things themselves!” The phenomenologists were the last philosophers to clearly express this ontological condition; subsequently, it was criticized by Heidegger—a critique which, though ambiguous and difficult to understand, seems to argue for taking language and praxis into account.¹⁷ As such, it appears to move in the same direction as Wittgenstein’s philosophy of language—though the latter partakes of a different spirit.

If we speak “strictly,” that is, taking the satisfiability of implicit validity claims into account, a separation between theory and thing is impossible anyway. An ontology as a verbal representation of what a thing “really” is, or what it is “in itself,” is always a piece of theory itself. Even though it is more abstract, it still carries a validity claim with it that can only be satisfied by presenting a potential orientation value. Modern (post-Kantian) ontology fulfills much of its justificatory duties by diligently modifying and repeating expressions that have already been put forward over the course of the history of philosophy. Depending on individual

¹⁵ It is quite clear that, in research, humans have to let go of their prejudices. But how this can be done is by no means clear. Great thinkers have devoted themselves to this question. Francis Bacon, for example, who introduced induction as a research method—fully aware that it is a mode of reasoning which easily leads to mistakes—advised his readers to become conscious of their “idols” and to abandon them. For this purpose, he sorted these “idols” (Lat. *idola*, stereotyped thinking) into four groups (Idols of the Tribe, the Cave, the Market Place, the Theater), so that one could at least catch a glimpse of the possible traps that demanded attention. Cf. Bacon (2000), Book I, § 38–68.

¹⁶ Such impediments are usually connected with received ways of framing an issue; cf. also Chap. 5. These are particularly persistent if they also mark boundaries between subjects or disciplines. It was difficult to advance the theory of heat because, among other things, heat was first analyzed in physics, then in the (nascent) field of chemistry, and finally in physics again. In this respect, research is naturally “transdisciplinary;” cf. the instructive examples in Mittelstraß (1989b, 2007).

¹⁷ Cf. Gethmann (2007).

assumptions, this procedure is likely to be met with either respect or disapproval. Anyone who thinks about the meaning of certain propositions in an unbiased, inquiry-based way and tries to find reasons for their validity either fails or is presented with the necessity of serious and lengthy studies—ontology, after all, is a specialized discipline of philosophy. However, the fact is mostly ignored that, from the simplest to the most subtle figures of thought, implicit validity claims need to be satisfied or at least clarified.

In my opinion, we need to understand the validity claims for ontological statements as follows: The author has gained these insights as a result of his reflections; the audience now needs to receive them as an appeal to recognize itself, or rather the preconditions of its own thinking and believing, in the presented formulations (cf. Chap. 9). But some philosophers recognize themselves in the early Wittgenstein, “The world is everything that is the case,”¹⁸ while others prefer the late Heidegger: “The wide expanse of everything that grows and abides along the pathway is what bestows world.”¹⁹ In order to avoid simple relativistic consequences, there seems to be only one thing we can do: to refer to actions and to the standardization of actions in praxes, to form and stabilize concepts on this basis, in short to seriously think pragmatically.

In an action, we are simply and directly involved in the matter. In our practical dealing with situations, events, and other people, we ourselves are a thing in progress, a thing shaping itself. As a result, the aforementioned approach to the states of affairs in question—which is supposed to “strip off the theory”—needs to take place in a praxis. A praxis is a sphere in which the subject-object relationship is, as it were, still soft. In Chap. 1, I developed the concept of praxis in such a way that a praxis consists of actions that are already standardized and schematized. Such a praxis with its felicity structures provides links to epistemic theory. During research, some things could possibly already be changed at this lowest level. An organized praxis and its action schemes need to be dissolved again into actual courses of action and individual actions that are taken into consideration, observed carefully, and possibly altered creatively. In short, we need to test changes that may have little value in terms of instrumental rationality and simply serve the purpose of exploring possibilities.²⁰ The practical extensions and changes thus created can then gradually be supported theoretically. This generates “new” theory. It has a different status than the old, epistemic theory. I call it “thetic” theory.

The center of thetic theory is the “thesis.” A thesis is something similar to a hypothesis. But I would like to make a conceptual distinction concerning the

¹⁸ Wittgenstein (1981) *Tractatus logico-philosophicus*, Sentence 1.

¹⁹ Heidegger (2010), 70.

²⁰ Cf. the “free exchange” that Lueken ((1992), 294 ff.), following Feyerabend, recommends for overcoming incommensurable relations. An incommensurable relation is a relation between heterogeneous theories that has been imaginatively pushed to extremes (cf. Chap. 5). As far as this free exchange is helpful, so is the generated practical contact presented here, which subverts rigid objectifications.

relationship between distance and commitment.²¹ A hypothesis is purely cognitive, while a thesis is a piece of pragmatically construed theory. As such, it guides actions. Someone who “advocates” a thesis does not merely do so verbally, but also acts accordingly, takes risks, etc. To be sure, we work with a hypothesis as well—draw conclusions from it and design experiments. But the main focus is on the distance to the event. In the case of a thesis, it is precisely the other way around: The main point is the conviction that one is right.

This new theory cannot be stabilized with respect to felicity structures in the same way as old theory. The language is tentative and so are the actions. We do not yet know the matter sufficiently. The main task of the new theory is presenting a solution to a problem.

Let us take a look at the examples for the sake of illustration:

Research on combustion in the early 18th century contained experiments where metals were roasted, substances weighed, and the “air” generated in these experiments utilized (by exposing a candle to it, letting a mouse breathe or rather suffocate in it). The experiments were articulated verbally, of course. This language represents the respective state of research at the time and is full of oddities for later readers. For instance, Robert Boyle (1627–1691) “weighed” the “heat substance,” which Ernst Stahl (1660–1734) later christened “phlogiston.” He recorded this weighing—over the fire, 8 oz of tin absorbed about 23 grain of the heat substance in 1¼h—and did not notice anything strange.²²

At times during the French Revolution, no stone was left unturned. Especially during the second part (from August 10, 1792, to the end of the “Reign of Terror” at “9. Thermidor,” on July 27, 1794), the leading figures hardly knew what to do and what they did. Certainly, they needed to rebuild the republic or rather protect it in its early stages. But what was “the republic” under the conditions in France at the end of the 18th century? The revolutionaries resorted to forms of government that were “corroborated.” Some of the protagonists, in particular Robespierre and Saint-Just, based their formulations, figures of thought, and sometimes even clothing on knowledge about the ancient republics of Greece and Rome, which they tried to update in line with the times.

As I said, new theory does not offer anything concrete that characterizes an action as felicitous. It draws its stability from the fact that it crystallizes around a sketch for problem solving. This sketch is articulated in thetic theory. It culminates, as stated, in the “thesis,” which grounds the respective research.

A thetic sketch is an expression of the researcher’s intuition and imagination. Usually, when we hear the word “intuition,” we seem to think of a kind of perception: a sixth sense or a supernatural, holistic understanding of the thing in question. “Imagination,” on the other hand, sounds as if the whole sketch came from the inside only—as if it were merely a human creation in which any objective content would be merely accidental. I think it is almost impossible to decide whether it was

²¹ Cf. Elias (1987).

²² Toulmin and Goodfield call this episode “one of the most tantalizing moments in the development of our ideas about matter.” Cf. Toulmin and Goodfield (1962).

originally a form of perception or mere imagination. In fact, it does not even matter. What does matter is that the researcher abandons established structures of practice, approaches the matter in question, and by doing so gains experiences that can be articulated in a thetic sketch for problem solving. It might be true that the nature of seeking and approaching the matter is already guided by such a sketch or at least by anticipations. But in the thetic sketch, tentative new actions now acquire a structure. These new actions need theoretical support, in order for us to grasp what is important about them—the schematic—and to evaluate its outcomes or consequences.

The research process is therefore not only a theory-building, purely cognitive activity. It is a cycle in which theoretical and practical phases interlock: It questions established theory, contains trial actions and evaluations, creates new theory, compares old and new theory, repeats trial actions and evaluations, improves new theory, further revises old theory, etc.

In this process, many things can change: Praxes can be accentuated differently; objects may disappear, as I mentioned, or may be recognized as reified aspects of misconceived action conditions. New objects can appear, or new aspects may acquire objective solidity within the conditions of action. Old theory may change, be revised, and altered to a greater or lesser extent. People may change by revising their self-understanding, acting differently, adopting different habits, and living different lives.

For certain research purposes, the social sciences distinguish between an “observer’s perspective” and a “participant’s perspective.” The original reason for this distinction was to stress the difference between research in the social sciences and research in the natural sciences. It might be impossible to measure social processes by observation. This requires the participation of the scientists. The participation provides them with experiences while acting in the relevant context. They do not merely collect data. In the natural sciences, the situation is different. It is impossible to participate in the processes of nature. Nevertheless, there are no “pure” observations here, either. There is always a certain degree of the researchers’ involvement through the “constitution” of objects, that is, through theoretical and conceptual guidelines.

For a general concept of research, we need both perspectives. But since the observer’s perspective seems to be more natural to us, we explicitly have to initiate the participant’s perspective. A simultaneous perspective of observer and participant can generally be realized in research through a parallelism of the object level of action processes and the meta-level of reflecting on these processes.²³

This parallelism between participation and observation is mirrored in the attitude of researchers and arguing persons in an antagonistic structure: On the one hand, in the context of a *quaestio*, (old) theory needs to be utilized, but on the other hand, it also needs to be called into question. I would like to describe this as follows: The researcher’s attitude contains both a commitment to the problem—or rather to the

²³ For the theorization of argumentation, and then especially for argumentation analysis, these conditions are essential. Without a participatory perspective, it is hardly possible to grasp, let alone assess appropriately, what is actually happening in an argument. This topic will be discussed in detail in Chaps. 7 and 9.

researcher's attempt to solve the problem, that is, to the thesis—and as a distance to this thesis. After all, the thesis might not be valid; it might just be a figment of the imagination. Ultimately, neither side is preferable. Therefore, commitment and distance need to be brought into a balance.

In any case, the researcher needs to be committed; otherwise, he will not even muster the intellectual and physical energy to generate a thesis, realize it in trial actions, evaluate the realization, reformulate the thesis, etc. If we ask what fueled the great explorations of the past, we find many things: the prospect of fame and fortune (in the case of Colón); service to humanity, coupled with selfishness (welfare committee in the second phase of the Revolution); the pride and joy of discovery (the chemists), etc.

In the case of one's own theses, commitment seems to be a resource that comes naturally. After all, people have desires, dreams, compassion, curiosity, and want to exceed their limits. Without this resource, no research is set in motion. But if there is not enough distance, research does not go very far. It gets off course and leads to dead ends and idiosyncrasies; researchers isolate themselves, end up in harm's way, and even get killed in extreme cases. Distance is needed, so that it can become apparent what we do when we orchestrate new actions. Complete objectivity is impossible. Only in hindsight, when the result has turned into knowledge, can we understand what the researcher has actually done. Beforehand, during the evaluation of the trial actions, these actions are viewed through the lens of the thesis. Consequently, things must have gone really bad for anything to even stand a chance to contradict the thesis. Usually, everything is a confirmation or can be interpreted as a confirmation by expanding or modifying the thesis. For this reason, generating distance is an integral part of research. Sometimes, an old skeptical principle can help: Simply and mechanically assume the counter-thesis. The best way to produce distance, however, is to avoid developing and pursuing thetic theory in a monologic way, solely in contact with the matter in question. Instead, we need to develop it in a dialogue with a critical opponent. A lot will have to be said about this at the end of the chapter.

Good illustrations can be found in our field of historical examples:

Colón, for one, can be diagnosed with a lack of distance. The man risked his life and that of his men to prove his thesis that India lies beyond the Atlantic, at a distance of about 4,500 km. It must have been an overwhelming experience to discover land at the predetermined distance after about 4 weeks, during which the situation on the ships had become precarious. Colón never stopped believing that he had come to India. This view, which he disseminated in speeches, petitions, and letters, was so powerful that, until the recent past, parts of the Caribbean were referred to as the "West Indies," and to this day Native Americans are often called "Indios" or "Indians."

It is trite to call for distance in the turmoil of the French Revolution. The faction of the Gironde, largely consisting of intellectuals, often presented exemplarily well-balanced submissions. Among other things, it argued against the execution of the king. Condorcet, one of its leaders, was rather removed from the turmoil. Although he had been denounced and pursued himself, he was able to clearly and extensively describe the social and human progress that had been achieved through the abolition

of absolute monarchy.²⁴ Saint-Just quite rightly retorted to this: “Those who attach any [*sic*] importance to the just punishment of a king will never found a republic.”²⁵ The king, Saint-Just argued, was not a delinquent citizen for whose sentencing justice could play a role. He was an external enemy of the Republic (he had been proven to have conspired with the foreign powers that marched against France). Such an external enemy had to be eliminated. (I will talk about this argument at great length in the last section of Chap. 5.)

In the development of modern chemistry, the phlogiston theory was a way of framing reality that enabled the execution of many interesting and fruitful experiments. Joseph Priestley discovered “dephlogisticated air” (the later oxygen)—an object with truly exciting new properties—by heating mercury. Priestly believed in the phlogiston theory for all his life—even when its refutation (a big problem was that phlogiston had to have a “negative weight”) was palpable. He did not deem it necessary to establish a critical distance to the phlogiston frame. Thus, Priestley is an example of the kind of scientist in the history of science who has to die for some narrow-minded theories to disappear from the world.

I suggested talking about “research” even in regard to normative questions. In research on normative theories, it is generally more difficult to talk of distance than in research on descriptive theories. Whether the space between Spain and India had been calculated or estimated correctly can be decided in a more distanced manner than the question of whether it is right to execute a king who was still perceived as their ruler by many of his subjects. The simple reason for this may be that there is no answer, that at best the consequences of an execution—some of which can be identified—may be compared with the imagined consequences of a non-execution.

But why is there no answer? Is it because there is no theoretical basis that could provide criteria of correctness? In fact, something like that existed: General norms of natural law and specific legal codes such as the constitution of 1791, which had been signed by the king after his initial refusal and which guaranteed his “inviolability,” were relevant for this question. Equally relevant was knowledge about the consequences of the execution of the English king in the century before (in England, the monarchy had been restituted). And, ultimately, the declaration of human rights that had been drawn up by the revolutionary parliament itself was also relevant.

When we investigate the rightness of a verdict, we need to define precisely what is in question. Depending on this definition, a decision can be called “right.” In the case of the French king, a distinction between moral, legal, and political rightness may provide some clarification. Of course, for the final decision as to whether the execution or any other type of punishment is right, these aspects need to somehow be related to each other, if possible even by integrating them. I will talk about this in Chap. 5.

²⁴Cf. Condorcet (2009).

²⁵Cf. Walzer (1974), Saint-Just 13. November 1792.

2.4 The Thetic Construction

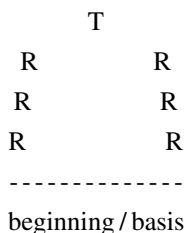
As I have already explained, research is not simply a process of trial and possible error, new trials and new possible errors, until finally an approach proves to be tenable. Rather, research is a very conscious use of theory. It even creates new theory by revising an old theory to a certain degree. In every moment of the research process, old and new theories interlock. Of course, it is absurd to believe that new theory could follow formally (by way of logic, arithmetic, game theory, or probability theory) from old theory. It is impossible to deduce a conceptual sketch that addresses a *quaestio*. Thetic theory is a construct. But this does not mean that, using our creative intuition, we build it up step by step. Such a claim would be both far-fetched and unnecessary. Normally, the sketch is holistic. What crosses our minds—a thought, an idea, a picture—is still whole. But by subsequently reflecting on the sketch, articulating it in language, distinguishing its parts, and justifying or evaluating its tenability, we are able to perceive or reconstruct it as a “thetic construction.”

Such a thetic construction consists of parts that have been taken, as far as possible, from available epistemic theory. In the process, a change in function or status takes place. Epistemic theory is corroborated with regard to its guiding function in a praxis. Thetic theory, however, no longer has this guiding function—or not yet. As a result, parts of the epistemic theory may undergo changes. At the least, they are separated from the context in which they have performed a guiding function until now. Most of the time, they are also reconstructed, reinterpreted, compressed, and expanded in all sorts of ways. Concepts, for instance, are reinterpreted in unusual contexts; previously disregarded implications are developed further and related to information from other fields; new states of affairs are considered in analogy to already known facts. Finally, the thetic construction may also include entirely new parts, new concepts, new combinations of states of affairs, new boundaries, new thought processes, and redesigned action opportunities. Due to the old theory’s change in function, the boundaries between new and old are sometimes blurred. Nevertheless, this is the basic structure of thetic construction:

epistemic theory \Rightarrow thetic theory
constructed further to

At the top of the construction, we find the thesis. It is the answer to the *quaestio*, the uncertainty, the problem that led to the gap in orientation. Conversely, the construction shows how to “ground” the thesis. After all, its “grounds” (or the basis of its justification) are made up of epistemic theory, which is further developed in the thetic construction. Someone who finds the architectural metaphor expressed by the

words “basis” and “grounds” compelling may perhaps also regard the following diagram as helpful:



There is a thesis (T), which articulates a piece of new theory to correct an orientation deficit. And there is a justification, which connects the thesis with the theoretical basis. This justification consists of reasons (R - R - R...), which support the thesis. For the sake of a better visualization, these reasons are represented as pillars—which of course does not mean that there always have to be two pillars. The whole construction bridges, so to speak, the gap in orientation.

I would like to note the following in order to rein in the ramifications of the architectural metaphor: In a concrete construction, the difference between thesis and reason is often not clearly visible. For the purposes of a diagram, however, it makes sense to locate reasons below the thesis. In argumentative conversations, in which such thetic constructions are developed and reviewed, reasons merge almost seamlessly with explanations and clarifications, which accentuate another aspect of the thesis, and even with affirmations that reformulate in a more pointed manner what has already been asserted.

Nevertheless, as a pragmatic difference in status, the difference between thesis and reason is fundamental (once again, the architectural metaphor!). The thesis is “reflected” in the justification—more precisely, in the theoretical basis. Already existing epistemic theory is a kind of mirror in which we want to see the thesis reflected. If it becomes sufficiently clear, we are motivated to work with the thesis.

This thetic construction answers the following question: What certainty and authority entitle us to represent and pursue the thesis? Its real accomplishment consists in presenting the thesis as “theoretically attainable.” The proposed thesis exceeds the limits of orientation. It is supposed to illuminate new parts of reality, in order for us to be able to risk the respective expansions of our current praxis. A thetic construct that is made of parts of available theory (epistemic theory and elements of construction) in a seamless and consistent manner—and that, moreover, leads to the thesis—relieves the thesis of its alien and arbitrary character. It instructs us to regard the thesis as a new, abstract, and stylized outgrowth of already established orientations. The “theoretical attainability” thus demonstrated has the power to both legitimize and motivate. It offers a legitimate reason for accepting the thesis as new orientation and for realizing it in new actions or in extensions or changes of established praxis. And it motivates us to attempt these steps, because it rests on established theory and thus provides the trust needed for research activities—particularly in the case of “heroic research” (cf. below).

2.5 It's a Small World

What now follows is a detailed example that is supposed to illustrate how epistemic and thetic theory interlock in a thetic construction. It is the argument that Cristóbal Colón—long before his rise and fall—presented to the advisory boards of Europe's royal houses, the last of which was the Committee of Fray Talavera at the Spanish royal court.

In popular accounts, this construction is often regarded as an expression of pure, inspired intuition; more sophisticated accounts also view it as indicative of a profit-seeking, objectively untenable adventurism.²⁶ In his imagination, Colón had reduced the size of the Earth substantially. He repeatedly insisted: "It's a small world." Playing down the size of the world in such a way is typical of endeavors that exceed the scale of previous human accomplishments. (We find a similar rhetoric in some contemporary proponents of genetic manipulation.) But if we bear in mind the state of knowledge and proficiency at the end of the 15th century, the following becomes apparent: Colón's construction was indeed possible and his thesis theoretically attainable. His conviction could be considered as a rational representation of the problem, even if it was embedded in the self-image of an egomaniac. Apparently, the Committee of Talavera presented him with arguments that he was unable to diffuse on the spot. (In Sect. 2.7, I will have an opportunity to talk about that dialogue.) What needs to become clear for now is merely that the construction, though not compulsory, was "really possible." Now, let us come to the point.

In this construction, the following five steps can be distinguished:

- (1) Contrary to views that the Earth is a disc—or hump-like figure surrounded by the ocean and divided into two parts (Eurasia and Africa)—India, or the east coast of Asia and the islands situated off its coast, can be reached from Europe by way of a western passage that leads across the "oceanic sea."
- (2) This western passage does not only exist; it can also be traversed with normally equipped ships within a reasonable amount of time.
- (3) A royal house of Europe that reaches countries in East Asia by way of this western passage will tap into vast new sources of income via trade or acquisition. Hence, sending out an expedition trying to find this passage will lead to enormous material advantages for any royal house that undertakes such an endeavor.
- (4) The inhabitants of those countries could be converted to Christianity. Thus, the salvation of those involved, both of the converts and the missionaries, would be neatly advanced.
- (5) He, Cristóbal Colón, the sole person with the required information and the necessary self-confidence, should therefore be entrusted with this expedition.

With this construction, Columbus tried to persuade the royal expert committees, first in Portugal and then in Spain. When they failed to be convinced by it, he sent

²⁶ Some very stark judgments in this vein can be found in Vignaud (1911) and Venzke (1991). In Nunn (1924), some of these views are refuted convincingly.

his brother to England. In the event of another rejection, he would have turned to France. But the Spanish queen ignored the rejection of the expert committee at the last moment and entrusted the man with the execution of the project.

2.5.1 *Assessment of the Entire Construction*

Of course, for an assessment to be adequate, we have to be sufficiently acquainted with the conditions in science, technology, politics, and morality during the Renaissance. On that basis, it is not difficult to assess the legitimacy of the five parts of this construction.

Step (1) contains the spherical shape of the Earth. This was fairly well known at the time—it was not yet knowledge, but part of the epistemic theory of the thinking elites. Nevertheless, it meant something very different than today. After all, the spherical shape does not determine whether the deeper and, in particular, the opposite parts of the world are accessible for humans—and if they are, whether it would be possible for people to return. As I said, these concerns, which were connected with the so-called antipodes argument, will be discussed in the section on dialogue.

Steps (3) and (5) become comprehensible in the context of customs of the time. With the ancient empires as role models, the seafaring nations had established a practice of “discovering” foreign countries—which meant conquering and exploiting them. In this sense, Portugal had, since the 1530s, “discovered” the west coast of Africa and had imported commodities, gold, pearls, ivory, and black slaves in particular. The Portuguese king awarded licenses to all possible kinds of businessmen and adventurers. They allowed these men to privately profit from what they had discovered for Portugal.

Step (4), Christianization was obvious. The Pope, as the supreme power that formally commanded the Western hemisphere, had to ratify such annexations.²⁷ Naturally, then, the justification had to refer to this topos. However, Colón’s accentuated desire for a Christian mission was peculiar.²⁸ In this zeal, he was matched by the “most Catholic of kings,” Hernando and Ysabel of Spain. Nevertheless, the later differences between the discoverer and his employers were caused, among other things, by this very issue.

²⁷ After the “Donation of Constantine,” the Pope owned the entire Western hemisphere. (Constantine had been cured of leprosy by Pope Sylvester. As a reward, he gave him half of the world.) The corresponding document was exposed as a forgery for the first time around the middle of the 15th century. But at the time of the discovery of the New World, it was still in effect, i.e. the Pope decided whether it was right to seize a country in and beyond the Atlantic.

²⁸ Many commentators are fascinated or repulsed by Colón’s Christianity. Cf., for example, Madariaga (1939), Chapters XI and XII, who concluded from the many peculiarities that Colón was a Jew. The Jewish journalist Wiesenthal ((1973), *passim*, especially 109–139) worked on this thesis extensively, only to reject it in the end. For a comprehensive description of the arguments about Colón’s alleged Jewishness, cf. Böhm (1992), where all known arguments in favor are invalidated, which caused Bucher (2006), 251, to regard the refutation as “final.”

The only truly unclear and controversial step was step (2), the geographical argument. The western passage to India was supposed to be traversed with ordinary ships in a reasonable amount of time. But there were several constraints, first of all psychological ones: 30–50 men were squeezed into a small space of about 150 m²; the storms were a threat to their lives and led to extreme situations time and again. Moreover, the amount of time a sailing ship could spend on sea was technically limited by the amount of supplies that could be stored and by the fact that the ship's hulls were damaged by seawater on the outside and by woodworms on the inside. For these reasons, a calculation or at least a somewhat reasonable estimate of the distance between Spain and the east coast of Asia was essential. Of course, there was no knowledge about this—only many, more or less reliable, opinions.

2.5.2 *Assessment of the Geographical Thesis*

According to Colón's geographical thesis, India was located about 3,000 Roman miles (about 4,500 km) west of Spain. It could thus be reached in three to four weeks. I will briefly explain how he arrived at that conclusion. As I said, the aim is to elucidate the nature of the thetic construction and of the "old" theory built into it.

As an educated person of his time, Colón had access, albeit with some effort, to the following documents:

- Ptolemy's cosmographic writings, handed down in the tradition of Arab astronomers
- The Venetian Marco Polo's 13th-century travelogue "*Mirabilia mundi*"
- The representation of the world "*Imago mundi*" (1410), a work by one Pierre d'Ailly, former chancellor of the University of Paris
- Since the beginning of 1492, the new "*Erdapfel*" (the first representation of the Earth in spherical shape) by the Nuremberg-based traveler and cartographer Martin Behaim
- Letters and a map by the Florentine scholar Paolo Toscanelli. These were particularly important. In the research on Columbus, there is disagreement whether Toscanelli's letter to Colón is authentic. Certainly authentic, however, is Toscanelli's letter to the king of Portugal, a copy of which was apparently among Colón's possessions.

In addition to this material by people who could, with some justification, be regarded as competent, information on the matter was also available in a variety of philosophical and literary texts. At the end of the Second Book of *On the Heavens*, Aristotle—then still known as "*the philosopher*"—had explicitly agreed with those who regarded the Earth as (spherical and) small. There had to be a link to India by way of the pillars of Hercules (Gibraltar), he argued, because one could find elephants in both places. And Seneca, in the First Book of *Quaestiones Naturales*, had talked of new continents (*novos orbes*) that the ocean (i.e. the Atlantic) also contained. It is clear that Colón was familiar with this material. As evidenced by a

logbook entry dated September 2, 1492, he also paid close attention to reports and tales by coast dwellers and sailors who claimed to have seen land in the west under peculiar circumstances.

Was all of this epistemic theory? In order to assess the orientation value of this material, we also need to consult the Bible as a general background. For people of the Late Middle Ages, the Bible was the epistemic frame of reference in all matters not answered by evidence. The Bible, however, states (Book of Ezra) that six parts of the Earth are covered by land; only 1/7 is water. Colón often quoted this passage and referred for support to Augustine, who regarded Ezra as a prophet. With this background, he structured the specific information available in such a way that he was, in the end, so convinced of the existence of the countries and islands close to the Asian mainland as if these, in the words of Las Casas, had been located “in his own room.”²⁹

Colón’s way to arrive at his distance specification has by now been well reconstructed.³⁰ It consisted of two steps. First, he determined the distance in degrees of longitude; then, he calculated the size of the terrestrial globe or, rather, the width of a longitude.

The first step, given the sphericity of the Earth, was to formulate any distance as a part of the 360° available for the circumference of the Earth. In order to do so, Colón referred to Pierre d’Ailly, who had referred to Ptolemy and, even before Ptolemy, to Marinus of Tyre. Ptolemy had specified the area of the continental mass at 180°. But this was corrected to 225°, Marinus’ value. Ptolemy had not been familiar with India’s expanse “beyond the Ganges” (India extra Gangem), which was now added to the value. So much for the knowledge of the ancients. Later, Marco Polo’s specification of the distance between India and China justified adding another 28°—which already anticipated the results of the second step. Marco Polo also reported the existence of the big island of Cipango (Japan), located 1,500 miles off the coast of Asia. Colón regarded this as a reason to add another 30°. Together with the 9° of the distance between Spain and the Canary Islands, Colón’s presumed point of departure, this resulted in 292°. For some other reason, which is incomprehensible to me, he added another 8°, so that the distance from the outermost west to the outermost east amounted to 300°—hence the distance to be covered was 60°.

Except for that last detour, everything seems clear. But regardless of assuming 292° or 300°, this was still quite a large value for the size of the land mass.³¹ On Behaim’s globe, for example, it had been 240°. Should Colón not have used this or, ideally, a mean value? Well, I think he was justified in his calculations. He knew from his own nautical experience that some parts of these estimates were uncertain. I will talk about the status of the topographical maps again at some later point. Apparently, Colón’s position was “anchored” securely in the argument from

²⁹ Cf. quotation in the introduction to Columbus’ logbook, Jane (1968).

³⁰ The most accurate by George E. Nunn; cf. Nunn (1924).

³¹ Nunn (1924), 89, provides a list of nine different determinations of the size of the land mass. It shows that the size of Eurasia had constantly increased in the minds of experts since the early Middle Ages. Colón’s specification, however, by far exceeds all the other ones.

authority based on the prophet Ezra's specifications: If only $1/7$ of the Earth is covered with water, then, given the Earth's sphericity, the eastern edge of India cannot be much more than 60° away.

As a second step in determining this distance, Colón calculated the distance between any two longitudes on the surface of the Earth. Of course, this meant specifying the size of the globe. What could Colón know about this? As early as ancient times, the history of cosmography had generated calculations of the Earth's circumference, such as the measurements of Eratosthenes (around -200), Posidonius (about -65), and other topographical surveyors. Some of these measurements deviated by only 2 % from the currently known value.³² But with the decline of Alexandria, this knowledge was lost and became a legend. In the 9th century, some Arabs on behalf of the Caliph Al-Mamun of Baghdad, especially the geographer Al Farghani (Latinized: Alfraganus), determined the width of the longitude at the level of Sinjar as $56 \frac{2}{3}$ miles—a number that remained authoritative from this point in the Middle Ages to the end of the early modern period. Colón worked with this number and even claimed in one of his notebooks to have checked it personally. Hence, it was this value that he used to determine a longitudinal width in Roman or Spanish miles that corresponds to 83.86 km.³³ But since, ultimately, Colón did not intend to cross the ocean at the equator, but at around 30° latitude—the level of the island Ferro (Hierro)—this value was reduced to about 74 km. Thus, the distance to be covered was about 4,500 km.

In this way, Columbus had found a route to India that seemed to be more than just a foolish idea. In addition, there were all sorts of islands along this route, the mythical Antilia, St. Brendan's Island, etc., where one could hopefully interrupt the journey. As is well known, nobody has ever been able to find these islands. In the late 15th century, however, they were by no means merely a chimera or a seaman's yarn. They were marked on maps, and serious endeavors relied on them. In the '70s, Portugal even sent out several expeditions to find and conquer them.³⁴

³² Cf. Peter (1972), 40. Upon closer inspection, however, it turns out that, according to our current state of knowledge, Posidonius cannot be credited with independent measurements at all. The process of Eratosthenes' measurement ("[...] throughout antiquity [...] [the] only geodesy [...] worthy of the name," Miller (1919), 16; Trans. T.P.) is difficult to comprehend today. As a result, two values circulated, namely 250,000 Egyptian stadia (39,375 km) and 252,000 Egyptian stadia (39,690 km). Cf. also Eratosthenes (1969), 99 ff.

³³ The information is incorrect; a longitude is 111.12 kilometers wide. What went wrong? Nunn (1924; 1, 6) still assumed that Al Farghani's measurement of $56 \frac{2}{3}$ miles was wrong. In the meantime, however, it has become clear that the Arabian mile (1.97 km) was longer than the Roman mile (1.48 km), so that Al Farghani was more or less right. Commentators like Venzke, who are aware of this, mock Colón's error as a "gallop through the difficult terrain of a geographical definition of the Earth" (Venzke (1991), 72; Trans. T.P.), without noticing that their accusation is cheap. After all, nobody in the Late Middle Ages knew of this difference. This is a typical error of assessment that arises when an interventional evaluation is made in the immediate aftermath of an internal evaluation, without asking what the participants in the argument could have known (cf. Chap. 7). The really interesting question here is just how the seafarer could have been able, as he claimed, to have verified Alfraganus' specification, even though he interpreted it erroneously (as a Roman mile). Was he no more than a braggart after all? As usual, Nunn (1924; 13–18) has something significantly smarter and more differentiated to say about this, too.

³⁴ Cf. Bucher (2006), 83–87.

I will now turn to an assessment of the validity of Columbus' construction. The estimates and measurements handed down by tradition and then summarized by Pierre d'Ailly were partially epistemic theory. But it is impossible to determine where epistemic theory ended and the imagination that exceeded such theory began. Due to different "systems of measurement" (e.g. counting day trips), there was a huge margin of error with respect to specific distances. Of course, there were expert opinions. But while they all referred to Ptolemy, ultimately they also led to vastly different results. The relevant parts of the thetic construction included an evaluation of the marine maps available at the time, whose status is certainly interesting enough. Colón carried one on his journey,³⁵ which is believed to have been a copy of Toscanelli's map.

Experiences with sailing the coasts of Europe, the Mediterranean, and the North Atlantic reach back into prehistory. The maps in use are visual versions of theories that support the praxis of navigation. What I mean is this: A sailor, who had sailed around the southern tip of Messenia on the Peloponnese and was able, afterwards, to continue in a northwestern direction, "understood" his actions with respect to their geographical possibilities inasmuch as the coastal line recorded on the map showed this course.

The degrees to which the praxis of navigation was developed in Colón's days varied widely. As a result, there was a range of barely confirmed reports about other coasts. Due to the lack of possibilities to confirm them, it was impossible to distinguish a genuine report from a seaman's yarn. On the one hand, the maps—which truly represented the "world view"—contained too little (only two land masses, Eurasia and North Africa). On the other hand, they also contained too much (such as the already mentioned islands in the middle of the "ocean," as well as areas where the sea monsters Gog and Magog were said to dwell, etc.³⁶).

Thus, these maps, in contrast to our present maps, were thetic in several respects. An assessment aimed at isolating the solid elements of knowledge could not be finished in theory alone. Only practical research could lead to further insights.

In addition, the thetic construction by the discoverer of the New World contained experiential knowledge about prevailing wind directions. To the west, there were the trade winds at 25–30° latitude. Equally reliable winds in an eastern direction could be found at 35–40° latitude. This was nautical experiential knowledge which, of course, was secured only for areas near the coasts. Metaphorically speaking, Colón could only be sure of an initial push out onto the ocean. Whether these wind conditions would, in fact, endure all the way to Asia, or rather to the Caribbean, was anybody's guess. Colón's general navigational skills contained further epistemic theory, for example, knowledge about the capacities of sailing ships,

³⁵ Cf. Jane (1968), 11: logbook entry from Sept. 17, 1492; and cf. Jane (1968), 17: logbook entry from Sept. 30, 1492. Cf. also Venzke (1991), 82.

³⁶ Madariaga (1939), 75/76: "Traveller's stories, sacred books, charts and documents, old wives' tales, every form of lore contributed to the discussions. [...] Round a kernel of direct observation there spread a circle of authority, classical and biblical, and beyond it an aura of hearsay, and still further afield a world of imagination."

about food supplies, leadership, and, of course, about navigation. This knowledge, too, depended on the conditions of prior praxis in a way that is difficult to account for. A good navigator was able to stay on course on the high seas without a landmark orientation, just by using a compass, quadrant, and astrolabe (contemporary position indicators for navigating by the stars). But what needed to be done if the information available through these apparatuses became inconsistent was not clear. Apparently, Colón experienced this situation. In the process, he probably discovered what is today called “declination” and decided to rely on the information given by his compass.³⁷

To sum this up, the entire sketch of this trip to India on the western route is a unique thetic construction. The parts of theoretical bases that reconcile the justifications with the best available knowledge are quite apparent.³⁸ To what extent the constructed thetic parts that exceeded this knowledge were valid could not be decided for the time being—at least not in theory. Consequently, the geographical thesis implies the nautical thesis that India can be reached with normal ships on the western route in a reasonable amount of time.

2.6 The Genesis of Thetic Theory: The Research Project

The thetic construction is a theoretical formation in which epistemic theory is expanded into thetic theory in order to bridge a gap in orientation. Hence, the thetic construction creates a piece of theory that enables further practical actions. These actions yield certain results and probably should—especially from a pragmatic point of view—say something about the validity of the construction. It is tempting to think that these results decide whether the thesis is valid or not. In simple, clear everyday situations that is indeed the case. If the dog breaks through the frozen surface of the pond and returns to shake its wet fur, then the thesis that the ice is safe has been refuted: Children you cannot go ice-skating yet.

Apparently, such clear situations were paradigm cases for a naive falsificationism as promoted by the early Popper. Thesis, test, refutation, new thesis, new test... this would be the cycle. But, as a matter of fact, such a cycle does not even properly describe our daily research. Even our everyday theses include epistemic theory and in most cases also real knowledge. This epistemic theory is used to evaluate the test results, incorporate them into the construction, and take them into consideration in the next trial action. Because the dog is lighter than a child, the child would definitely break through the ice. Hence, there will be no further attempt for the time being.

³⁷ Cf. Jane (1968), 11: logbook, entry from Sept. 17, 1492. Some interpreters, however, believe that the whole difficulty is merely an expression of the nautical ignorance of various copyists.

³⁸ “[...] the evidence shows Columbus to have been painstaking in his inquiries and to have utilized the best information available in his time.” Nunn (1924), 30.

Generally speaking, the result of an action is not self-evident. Rather, it needs to be evaluated. In such an “evaluation,” the result is embedded into theory. Consequently, the following difficulty occurs: The theory is a structure of old and new theory, in which not only new theory is tested and possibly changed, but also the old theory needs to remain open to corrections. Thus, in an evaluation, there are always many possibilities to theorize the result of an action. Suppose the result was interpreted as counterevidence. In that case, the theory would have to be withdrawn, altered, broken down—to what degree, and in which way, depends on the case. A complete withdrawal of the thetic theory as a whole, that is, what naive falsificationism recommends, is only one possibility—and an extreme one at that. More realistic is a theory change. In that case, theory can be dismantled, but it can also be expanded.

The latter case, in which a counterevidence is not interpreted as refuting the theory, but as showing its incompleteness, is particularly interesting. Subsequently, the thetic theory is supplemented by including an explanation which classifies, as well as embeds, the inconsistent test results and reconciles them with the theory. In the case of the frozen pond, this could look as follows: The dog broke through the ice at a point where the ice is particularly thin, because a brook feeds into the pond nearby. Maybe we need to try it on the other side.

But is this not simply an excuse? It seems as if the proponents of the thesis want to be right at any cost. After all, why did they not go straight to the other side? In this case, the philosophy of science would describe the theory as being “exhausted.” This is an expression that illustrates the fact that a theory always has a certain mental and theoretical potential. It contains epistemic theory, and this theory is useful for more than just for the *quaestio* which is presently intended to be theorized with its help. Since that is the case, perhaps the thesis—if its potential is fully activated—can explain the counterevidence after all. But that means it would be advisable not to dismiss it prematurely. “Exhausting,” then, means unblocking and tapping into the theoretical potential of a thesis. Yet this “potential” has no clear boundaries. The idea of exhaustion is therefore completely plausible. Its realization, however, is not that easy. What is needed is a criterion to distinguish an “ad hoc explanation” (a lame excuse) from a fruitful explanatory expansion.

These reflections on the “exhaustion” of the thesis give reasons for including a new concept in our conception of research. The thetic construction is not simply the result of a single creative sketch, which we utilize and possibly abandon subsequently. Rather, the construction is something in the making. It only begins with a conceptual sketch. Such a sketch then serves as a theoretical support for trial actions, which approach the matter in question. The sketch can help to evaluate this theoretical apparatus, possibly by changing it. I would like to use the term “research project” for this genetic, cyclically progressing side of a thetic construction. It is an amalgam of Hugo Dingler’s concept of exhaustion and Imre Lakatos’ concept of the research program. The latter was introduced into the philosophy of science to meet the problems of naive falsificationism. But since it was also unable to definitively solve these problems (there is no criterion for determining whether to retain or abandon a program at any given moment), the term was not long-lived in any

significant way.³⁹ I think that it could still have a chance in argumentation theory. But I take the liberty to rename it “research project”: “program” sounds like mere software, while “research project” raises expectations that something will be done practically, too.

“Research project,” then, designates the procedural dimension of a thetic construction. For the “state” such a construction is in at any given moment of its development, I use the expression “position.” Having a position with regard to a *quaestio* does not only mean establishing a thesis. It also means being able to mobilize all kinds of epistemic and thetic theory to advocate it. If the thesis is discussed or realized, this theory is the resource to improve it, if necessary.

In Lakatos’ work, the research program is a dynamic theoretical formation for research in a problem area. It consists of theory (in my words: partly “thetic,” partly “epistemic”), which is designed to develop research activities and to evaluate their results. Crucial for this conception is a pragmatic distinction between the theories developed within the program. Such a distinction must be drawn with respect to the question of how to deal with the theory in the face of inconsistent results—namely whether to revoke or maintain and develop it. Lakatos called the first type of theory, that is, the one that is put up for discussion and possibly broken down, the “protective belt.” The second type, which is retained and secured with additional theory, he called the “hard core.” This core ensures, so to speak, the identity of the program. As long as it is retained, or can be retained, the research program is “alive.”

Its life consists first in generating activities and thetic theory, then in evaluating the results of research activities, whereby everything is put up for debate—except for the “hard core.” From this point of view, research is equivalent to an exhaustion of the theoretical potential of the hard core. As long as this exhaustion enables further productive research, the program “progresses.” The program “degenerates,” on the other hand, if the exhaustion constantly yields results that require the construction of more thetic theory, which, in turn, further determines the practice of research in theory, without ever leading to any confirmation of these determinations in practice—whereby the theory ends up increasingly suspended in midair, so to speak. But this does not mean that it needs to be abandoned. Nothing, especially no meaningful conception of rationality, can force a researcher to abandon a program that degenerates. Some programs degenerated for a long time—years, even centuries—and then progressed again, possibly because a new invention provided a new tool.⁴⁰

To once again address the aforementioned problem—excuse vs. reasonable expansion of the theory—we do not find a real criterion for such a distinction. Thus, whether a project is abandoned, changed, or kept on track in the face of a constant increase in thetic theory that does not lead to successful illuminations of the problem, or that makes it more manageable, is a matter of the personalities of individual researchers—of their ability, as we might say, to strike a good balance between commitment and distance.

³⁹ Holm Tetens went to great lengths to revive the term; cf. Tetens (1994). To my knowledge, however, his attempts were not well received. Wolfgang Detel introduced a simplified variant under the name “Forschungseinheit” (“research unit”); cf. Detel (2007), 129–131.

⁴⁰ Cf. Lakatos (1970), 138 ff.

I would like to turn to an illustration of these concepts now:

Colón discovered land after 33 days, at almost exactly the distance from Spain where he had expected to find India. The journey, that is, the trial action based on his thetic construction, had been faced with enormous difficulties. In the end, the crew had been completely demoralized and on the verge of mutiny.⁴¹ He had mastered everything. The Lord watched over him. And he was confirmed gloriously: The country had been found at precisely the point he had specified. India (or rather Cathay–South China) had been reached. Henceforth, Colón “exhausted” his construction. What he found were not the populous urban cities of Marco Polo’s reports, but villages with naked, benevolent savages. When he heard some similar-sounding words in their language, however, he believed them to speak of the “Great Khan” (the Emperor of China). Even when the signs accumulated on other expeditions that nothing was the way it should have been in India, Cathay, or Cipango (Japan)—and when it turned out that Cuba was not a continent, but an island—he held on to his thesis and produced an incessant stream of corresponding explanations.

Did the project degenerate? This is hard to say. Nunn points out that Balboa (1513) adopted Colón’s point of view; that Waldseemüller and the German cartographers followed him, at least partly; that Cabot believed Colón (1544); and that Gastaldi’s map (1562) still identified the discovered territories as the eastern edge of Asia.⁴² The project was dead (in this respect) once the new continent had been identified and christened America (after the first name of Colón’s friend Amerigo Vespucci).⁴³ But, as already mentioned, the inhabitants of this continent are still called “Indians.”

Now I intend to present a brief example for the progressive type of theory development. The example is by Ludwig Wittgenstein, from his *Remarks on the Foundations of Mathematics*. Holm Tetens has convinced me that it can be interpreted as an example of the exhaustion of a sentence.⁴⁴ Wittgenstein considers the status of arithmetic propositions. Their truth does not depend on experience; on the contrary, they make certain experiences possible in the first place. Wittgenstein argues that we will always adhere to propositions such as $2+2=4$, no matter what any “counterevidence” suggests: “If 2 and 2 apples add up to only 3 apples, that is, if there are 3 apples there after I have put down two and again two, I don’t say: ‘So after all $2+2$ are not always 4’; but: ‘Somehow one must have gone.’”⁴⁵ What we

⁴¹ “Both Fernando Columbus and Las Casas report that, on September 23 and 24, violent riots broke out among the crew. These riots were directed at their leader. They lasted until the eve of the discovery and were accompanied by threats.” Berger (1991), Vol. II, 390 (Trans. T.P.).

⁴² Nunn (1924), 90.

⁴³ This was a process of gradual acceptance. On his world map of 1507, Waldseemüller was the first to have recorded a new continent called “America.” But some years later, on his second map, this continent had been removed again. On this second map, things were represented according to Cristóbal Colón’s reports, i.e. Cuba was the eastern edge of Asia, etc.

⁴⁴ Cf. the nice explanations in Tetens (1994), 32/33.

⁴⁵ Cf. Wittgenstein (1956), 162.

are witnessing here is the birth of a research project. There is a problem, an anomaly. And we transform the arithmetic proposition into the hard core that is now being exhausted: We construct thetic theory in order to explain how the apple could have disappeared.

Arithmetic is a formal theory, that is, it theorizes practices and domains that we have constituted through our own thinking. Is it not self-evident, then, that we will not accept any counterevidence to its propositions? Are formal theories perhaps part of the hard core of any research project? This is a good idea. But in reality, we cannot presuppose that a formal theory is complete and without errors in every respect. Frege's system, which presupposed naive set theory, was complete; still, Russell used it to formulate the famous antinomy that bears his name.

So the fact remains: A thetic construction evolves in the form of a research project that is assessed, changed, and expanded according to the generated new opportunities for acting and their results. For the time being, nothing definitive can be said about the validity of the theoretical and practical results.

This is unsatisfactory: If only history can judge the quality of a research project, and if history is not yet over, at least not while we are still preoccupied with the problem, then the frame of reference for an assessment is simply too large.

2.7 Thetic Theory in Dialogue

I have now outlined the conceptual requirements that allow me to locate the practice of argumentation. Faced with a *quaestio*, we look for orientation; we mobilize the available epistemic theory and further construct it into a thetic sketch; finally, we try to secure its validity by acting with the sketch in mind. In the process, we change and develop it further, if necessary. Yet, we cannot determine in advance whether the sketch is good and correct, whether the orientation it offers is indeed legitimate, and whether it corresponds to the reality of acting—or whether the construction is merely a fiction that leads people to miss, degrade, and destroy themselves and their world. All this can only be determined by approaching the matter in question. Gloomy prospects indeed!

Still, the researcher, or the community of researchers, has a resource that allows him or her, as far as possible, to secure the suitability of the thetic program. This resource is the other human being—the other person who knows different things, provides different experiences, and brings different assessments and intuitions to the table. This other person is able to critically test the thesis and possibly raise objections against it. In other words, the thetic construction needs to be tested in an argumentative “dialogue.” The outcome of such a dialogue is able to provide a criterion for the suitability of the thesis. This is my reason for regarding argumentation as a dialogical event and for dismissing as inadequate all argumentation theories that do not regard a dialogical setting as the standard case of argumentation. I am aware that this is not self-evident. Christoph Lumer expressly disapproves of dialogical argumentation theories by arguing that they theorize “primarily the function

of social coordination,” while at the same time losing “the reference to truth.”⁴⁶ Indeed, involving the Other certainly does not guarantee truth. Moreover, including another subjective element in the matter is problematic because it may become necessary to distance oneself from it again. But to ignore the dialogue and to state that “arguments [are] usually monologues”⁴⁷ is tantamount to throwing out the baby with the bathwater.

It is true that even the dialogical criterion cannot reach as far as the judgment of history. But, as I said, that “judgment” is simply not accessible (on time). For starters, testing the thesis in a dialogue breaks the spell of the seeming alternative between rejecting and further exhausting the research project theoretically and practically—an alternative that can be fatal in the case of large, far-reaching theses.

The role of dialogue in argumentation theory is contested, which may be partly due to an ambiguity in what “dialogue” means. There are at least two different meanings of the term—one is too narrow and the other one too broad. According to the narrow concept, a dialogue is a verbal exchange between pairs of interlocutors, following strict rules,⁴⁸ whereas the wide concept refers to any communicative event involving a mutual acknowledgment. Thus, the idea that argumentation has a generally dialogical character can be rejected on the basis of the narrow concept of dialogue⁴⁹; and it can be affirmed with reference to the wide concept.⁵⁰ Things become even more complex because the notion of a dialogue somehow overlaps with the (narrowly drafted) notion of dialectic: If doubts or disagreements appear, the dialogue is regarded as a dialectical exchange. Therefore, as long as argumentation is basically conceived as a premise–conclusion sequence, “dialectical” does not seem to be a general quality of argumentation. In the view developed here, the term “dialogue” is located in between the narrow and the wide concept, and it includes the aspect of the “dialectical.” Any argumentation striving for the examination of a thesis’ validity needs a control instance and is therefore intrinsically dialogical.

Two roles pertain to this understanding of dialogue: “proponent” and “opponent.”⁵¹ The proponent is the author of the thesis. He or she advocates the thesis by presenting reasons for it. The opponent is the instance of criticism. Criticism does not mean refutation of the thesis, especially not refutation at all costs. Neither does criticism

⁴⁶ Cf. Lumer (1990), 6, 25, 316 ff. Lumers anti-dialogical position is partially explained by the fact that, for him, argumentative validity presupposes scientific truth. In the production of the latter, however, Lumer does not see a place for argumentation.

⁴⁷ Loc. cit.

⁴⁸ Examples of this kind are the dialogues of the Dialogue Logic, of Hamblin, Hintikka, Walton, and others.

⁴⁹ This is the core of the argumentation in Antony Blair’s often cited paper about the limits of the dialogue model of argument; cf. Blair (1998).

⁵⁰ This is the strategy of Christopher Tindale when referring to Mikhail Bakhtin. He quotes him about “the dialogical” as involving “a whole formed by the interaction of several consciousnesses, none of which entirely becomes an object for the other”; cf. Tindale (2004), 98.

⁵¹ In the course of a dialogue about a thesis, the roles may change. If the opponent puts forward an objection which includes an assertion that is subsequently questioned, the proponent takes over the role of the opponent and vice versa.

always have to be cooperative and helpful. All that depends on the individual case. Depending on the kind of thesis, and the way in which it is brought forward by the proponent, excessively rigorous criticism may be as inappropriate as excessively friendly criticism. In this respect, the term “dialogue” should not entail any obligations—except control of the feasibility of the steps of the justification.

Perhaps I should mention that a dialogue does not necessarily require two partners. There is not always a different, sufficiently knowledgeable and cooperative subject who will help to discuss the thesis. In that case, the researcher has to rely on his own critical potential to review the pros and cons of the thesis and assess its roots in the available epistemic theory. As a reflective being, this researcher is capable of dividing himself/herself into two partners by also taking up the role of the opponent. In this context, it might be enlightening to know that the original Greek meaning of “dialogue” was not “interlocution,” but something like “thinking something through.”⁵²

Nevertheless, obviously the required function of critically examining a thetic construction is typically assumed by a real person. Hence, the necessary distance to the thetic sketch—a sketch that is so evident to the author that he is inclined to stick to it and exhaust it more and more—is integrated into the very form of the conversation. In a dialogue, the thetic construction becomes a thesis. For this thesis, the opponent will demand a justification. The justification consists of steps in which the parts of the relevant thetic and epistemic theory are presented and reviewed in the order in which they build on each other. A dialogue, then, begins with a thesis and ends, ideally, with a judgment about its validity, rejection, or its use as the basis for further action.

This, finally, brings us to an elucidation of the most important concept of the practice of argumentation: the “validity” of a thesis. First of all thetic validity is not the same as the truth or correctness of epistemic theory. In the latter case, there are references to functioning practices and to the two criteria of theoretical coherence and practical orientation value, which are even intensified in the case of knowledge. In the case of thetic theory, however, we cannot rely on functioning practices. The actions underlying it are merely tentative; they test the theory and expand or alter the practices. There is simply no theoretical-practical stability yet. This is the reason why there is a problem of assessing research projects—an assessment that could seemingly only take place once this stability has, as it were, been secured conclusively. In short, the reference to the praxis, which is crucial for the validity of theory and knowledge in pragmatic thinking, is still unstable in the process of establishing and improving theses. Therefore, the concept of thetic validity needs to be determined differently.

The concept of thetic validity, I would suggest, has two sides: a subjective-motivational and an objective-criterial side. The objective side is the dialogical

⁵²“Dialogue” is a derivative of “dialegethai,” which meant talking or thinking through (e.g. through a complex, even intractable idea). The prefix “dia” does not mean “two,” but “through.” The protagonist of the ancient tragedy thought about a problem in a dialogue with himself/herself. The choir (as the agent of the forum) commented on these considerations. The second dialogue partner sprung from this interaction.

completion of the already discussed “theoretical attainability.” I call it “objection-free attainability,” or simply “absence of open objections.” The subjective side I will call “credibility” for now. Presently, I would like to briefly summarize both. A more detailed account will follow in Chap. 7.

(A) Objection-Free Attainability

The thesis requires a connection to epistemic theory and, ultimately, to knowledge. If a path can be constructed step by step from established theory to the thesis—according to the ideal of the “methodical series of steps”—then the theory is “attainable.” Each step must be feasible, based on the theoretical material generated by previous steps. As a rule, this connection is not complete. The thetic construction introduces new theoretical elements (see above: differently accentuated concepts, newly applied propositions, connections that have not existed up to this point, new distinctions that have yet to prove their value, differentiations, separations, etc.) The steps all need to be possible (executable), but not all of them, may be necessary. This means that doubts of the kind “why in this way?” are always possible. Such objections are usually empty. Objections, however, which act as a reminder that relevant considerations are not taken into account, or which point out contradictions, are not empty. It is up to the opponent to ensure that the steps are executable, that everything relevant will be taken into account, and that no contradictions remain. If these three requirements are fulfilled, then the thesis is free of objections or “objection-free.”

(B) Credibility

Objection-free attainability of a thesis is a weak requirement—at least in comparison to the strong criteria of validity that, as some philosophers of science claim, are demanded in the sciences. No one is forced to take a thesis that is attainable without any objections as a practical guide. But, apparently, for someone who takes it as such, the thesis is “credible.” “Credibility,” then, is not an objective quality, but a binary relation between a thesis and a researcher, for whom it is useful as an orientation. The action that it supports theoretically is often not trivial; it might even be risky. Calling the thesis credible means that it, or rather its justification, generates the confidence to act on it.

Frequently, confidence generated in a dialogue is rooted in the proponent’s personality.⁵³ Then it has less to do with the merits of the argument. But a thesis can also generate confidence on account of its theoretical qualities: if the justification enables the opponent to have a real insight, that is, close a gap in orientation. It then dawns on him—yes, that’s what it could be, or even: It has to be like that. Such insights usually have an emotional basis. I will come back to this in more detail in Chaps. 3, 7, and 10.

In order to test the suitability of the terminology proposed here, let us have a quick look at our examples again:

⁵³Aristotle discussed these matters under the heading *ethos*; cf. Introduction, section “The Aristotelian Foundation of Argumentation Theory”.

I already mentioned above that Colón encountered people at the committee of the Spanish royal court who were not impressed by his construction and ultimately rejected his request. No protocols or other documents about the conversations survived. But Colón's son Fernando left a strongly idealizing biography of his father behind, in which he casually reported the reasons for the committee's rejection.⁵⁴

In addition to all sorts of doubts (over the centuries, so many learned men were unaware that the eastern edge of Asia was located within reach, and a vagabond Genoese sailor claims to be wiser than all of them), mainly three arguments are said to have played a role:

- (a) The distance is unrealistically small.
- (b) There will be no return from this journey due to a reversal of the conditions of gravity on the other side of the Earth.
- (c) The endeavor is sacrilegious.

(a) The first argument can be assessed with regard to the account given in Sect. 2.5. After all, the construction was not necessarily compelling. Anyone more careful in calculating the size of the land masses could easily arrive at a considerably larger distance. According to the ideas of the already mentioned Nuremberg-based cartographer Martin Behaim, who had participated in many Portuguese expeditions before the coast of Africa, the Eurasian land mass was indeed considerably larger than even Ptolemy had surmised, but it still only amounted to 240°. Thus, the Atlantic would have been twice as wide as in Colón's construction. Consequently, bearing in mind everything that is known about the conditions of the first journey, a crossing would have been impossible. But what would the result of this have been in a dialogue? Why should Behaim have been right? He simply regarded different reports and evidence as important than Colón. Ultimately, this was a case of *doxa* vs. *doxa*. Colón's thesis fulfilled the "objective" criterion of attainability, but it did not become credible to the committee members.

(b) The second argument is the famous "antipodes argument," which had already been brought forward as an objection to the sphericity of the Earth since ancient times: The people on the other side, the argument went, would have to be "antipodes." They would have to walk with their feet upwards or fall down. This, however, could not be the case. Obviously, our low inclination to accept the potency of this argument as an objection to Colón's endeavor is rooted in the fact that the center of the Earth, as the center of gravity, is self-evident to us today. But while this is the case for us today, it was not the case for disputants of the late 15th century.

In a dialogue, the argument would look something like this: The nautical experience that, on the sea, if a ship is far away, you can only see the peaks of masts and sails, speaks against the disc shape of the Earth. It is possible, however, to modify the thesis by integrating this objection: Such experiences are still compatible with a hump shape of the Earth and, in particular, with the idea that there could be a zone in the vaulted area from which there is no going back. The proponent of the disc thesis could advance this accentuated reformulation of the thesis. It would

⁵⁴ Cf. Venzke (1991), 111, 144.

exhaust the potency of the antipodes argument even further. Although no one knows if there is another side of the Earth—regardless of whether it has the shape of a disc, a hump, or a globe—our imagination is confounded by all these shapes. This confusion is an argument against attaching an orientation value to the thesis. Nautical praxis in the 15th century, however, had truly introduced new knowledge; Colón was familiar with this. In 1434, the dreaded Cape Bojador on the west coast of Africa, beyond which the Inferno populated by monsters was said to begin, had been circumnavigated. In 1488, Bartolomeu Dias had crossed the equator for Portugal and made it all the way to the Cape of Good Hope. No extraordinary changes in gravity or difficulties on the way back had been reported. And although the Portuguese were renowned for their secretive policies—for example, they never published their measurements—Dias’ glorious return to Lisbon in December 1488 had been a public event at which Colón himself was present.⁵⁵ Hence, this was epistemic theory that sufficed to refute the antipodes argument.

In short, although Colón did not have access to today’s refutation of the argument, he could regard the antipodes argument as a purely theoretical concern of scholars.

(c) I will not address the third argument now, because it requires other theoretical instruments (subjectivity and the concept of frames, Chaps. 3 and 5).

A thesis is thetically valid if a researcher has successfully made his point in the dialogue: If he or she can demonstrate to competent and approachable opponents that the two requirements of objection-free attainability and credibility are fulfilled. Subsequently, the thesis can be realized in actions. Such actions, however, do not yet constitute a praxis. They have not yet been tried and standardized; they are still research activities. What now determines the quality of the valid thesis is “reality.” If the thesis is good, it opens up new domains of reality. If it is bad, the action may lead to ambiguous results. In the worst case, it can be a downright failure.

Theses can be distinguished according to their scope. Realizing “large theses” means bigger changes of previous praxes than “small theses.” Because theses—even the ones tested in a dialogue—can turn out to be erroneous, realization remains a risk. For that reason, large theses are “broken down” as much as possible. If that is not possible, if money or patience are lacking, even large theses are realized. This, then, is “heroic research.” Its theme is, as it were, “truth or doom.”

Most research from the list of historical examples is “heroic research” in this sense. Colón’s expedition could have failed. (The distance to America is manageable indeed. But there are storms that engulf such small boats. Moreover, there are the psychosocial conditions of a crew unhinged by fear that make any further movement impossible—a crew that had neither the strength nor the supplies needed to turn around.) The rebuilding of French society could have failed. It takes a lot of additional theory and abundant details in this case, anyway, to argue that something either succeeded or failed. In the history of medicine and pharmacy, a number of self-experiments have been reported. Today, we carry out experiments with animals first.

⁵⁵Cf. Berger (1991) Vol. I, 61.

Insofar as harming them possibly means less harm to humans, this could be regarded as a way of “breaking down” the thesis.

The heroic researcher, who risks his own health and even life in pursuing the thesis, has become rare, if not extinct. At the same time, the theses that guide research today, for example, in research about the genetic optimization of human life, have grown immensely. We all are the subject of this research. This is the meaning of the term “risk society.”⁵⁶ A lot of knowledge has been realized in major technologies. At the same time, we do not know much about possibly relevant conditions and consequences of these technologies. In this respect, large-scale technological facilities are not just applications of knowledge, but also, partially, realizations of theses. Overall, we humans are part of the heroic research project “active evolution.” We have to participate in the dialogue about these research activities. We have to keep discussing, until no serious objections to the theses that are realized on a trial basis are left. There has to be enough time and money to do this. Our self-respect as autonomous human beings requires it.

⁵⁶Cf. Beck (1992).



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