ESTUDOS EM DESIGN
DESIGN ARTICLES

"Wicked Problems" and "Tame Problems": deconstructing an aporetic dualism in dialog with Karl Popper and Thomas Kuhn[1]

"Wicked Problems" e "Tame Problems": desconstruindo um dualismo aporético em diálogo com Karl Popper e Thomas Kuhn

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Abstract:
In this essay we carry out a critical and deconstructive analysis of the dualist foundation established by Horst Rittel and Melvin Webber in their 1973 paper "Dilemmas in a general theory of planning". The aim is to escape the aporia launched by the two authors, when they ontologically distinguished scientific and engineering fields from planning and public policy fields, by establishing the concepts of "Wicked Problems" and "Tame Problems". In order to propose an alternative, monist description, we sought theoretical reference in the same reasoning as Rittel and Webber for their dualist thesis: in Karl Popper's epistemological demarcation between science and metaphysics, and in Thomas Kuhn's metaphor of science as puzzle-solving.

Keywords: *wicked problems; tame problems; metaphysics; science; design.*

Resumo:
Neste ensaio procedemos com uma análise crítica e desconstrutiva do fundamento dualista estabelecido por Horst Rittel e Melvin Webber, no artigo "Dilemmas in a general theory of planning", de 1973. O objetivo é escapar da aporia lançada pelos dois autores, quando distinguiram ontologicamente campos científicos e de engenharia, de campos de planejamento e de políticas públicas, com o estabelecimento dos conceitos de "Wicked Problems" e "Tame Problems". Para propor uma descrição alternativa, monista, buscamos referencial teórico na mesma fundamentação de Rittel e Webber de sua tese dualista: na demarcação epistemológica entre ciência e metafísica, de Karl Popper, e na metáfora de ciência como resolução de quebra-cabeças, de Thomas Kuhn.

Palavras chave: *problemas perversos; problemas domesticados; metafísica; ciência; design*.

[1] This study was financed in part by the Coordenação de Aperfeiçoamento de Pessoal de Nível Superior - Brasil (CAPES) - Finance Code 001.

1 Historical context and theoretical framework

When reading *"Dilemmas in a general theory of planning"* by Horst Rittel and Melvin Webber, published in 1973 ("Dilemmas", henceforth), we were surprised by the authors' particular way of explaining their ideas. The text, similar to the essayistic writings of modern thinkers before the establishment of the current scientific article format, lacks a section with bibliographical references, which are not sufficiently supplied by the few footnotes referring to other researchers and works. In addition, the authors' arguments give the impression that the subject is widely known in the academic community and that potential readers are aware of the premises, concepts and empirical data of an ongoing controversy, mainly at home in the United States.

In part, this approach can be justified when we look at the historical context in which the publication took place. According to a review by Kate Crowley and Brian Head (2017), the topic had only been presented in a seminar in 1967 by Rittel (a theorist and professor of "design science" at the University of California). He took it up again in subsequent seminars and courses for students and colleagues in 1969, 1971 and 1972, before coauthoring the final form in 1973 with Webber (a theorist and professor of urban planning, also at the University of California). The definitive article was published in the then newly created journal Policy Sciences, still in its fourth edition.

Regardless of possible omissions of ideas from other scholars, which force us to speculate about their origins (as we will see in relation to the notion of science as puzzle-solving, proposed in 1962 by Thomas Kuhn), the assertiveness with which the subject is dealt with leaves no doubt about the authors' intention to delimit and name new concepts. These characteristics are already expressed both in the title of the paper, which, not by chance, begins with the word dilemmas, and in its abstract:

> The search for scientific bases for confronting problems of social policy is bound to fail, because of the nature of these problems. They are "wicked" problems, whereas science has developed to deal with "tame" problems. Policy problems cannot be definitively described (Rittel; Webber, 1973, p. 155).

The pessimistic tone and aporetic attitude mark the summary and run throughout the paper: right from the start, the authors declare that Candide is dead, in a reference to Voltaire's classic satire of 1759, *"Candide, ou L'Optimisme".* One of the consequences of this skeptical position, and with an argumentative structure to support its aporia, was the interest generated around the issue. The pair of concepts "Wicked Problems" and "Tame Problems", created to think about the diversity of social policy planning issues, as opposed to those of the exact sciences, natural sciences and engineering, led theorists, more attentive to the wicked problems pole, to multiple interpretations, while at the same time making it possible to expand their use in areas such as the environment and even to create derivative concepts such as "Super Wicked" (Levin *et al,* 2012).

If we understand wicked problems as a circumscription within the class of knowledge problems, at once explored and questioned by scholars of philosophy, science, and projective fields such as design, architecture, and urban planning, this delimitation is prolific, as it still remains at the top of academic discussions. According to Crowley and Head (2017), Google Scholar recorded 10,682 citations to the article by the end of 2017.

Interest in the subject increases over time. In a recent publication, Jeffrey Kok Hui Chan and Wei-Ning Xiang (2022, p. 3) state that: "A Google Scholar search by us on January 27, 2022 reveals that 'Dilemmas' has been cited 20,279 times in the English-language publications since 1973; and that there are 107,000 English-language publications that have 'wicked problems' in their titles". Such profusion is partly justified by the theme's relations with the growing awareness of societies regarding pluralism and the dissemination of its ideas and values. These concepts, associated with what Rittel and Webber name (without referring them to a particular author[2]) as "open systems" and "open societal systems", can be seen more and more in crises, enhanced by the almost instantaneous connections of the globalized world and technologically interconnected.

An antinomy generated from two classes of problems defined as having *different natures* and a consequent aporia found in the wicked class, as far as we have investigated, remain after half a century of their demarcation proposed in "Dilemmas", and point to the relevance of the theme gained from its origin in areas of social planning and public policy. This in itself justifies its growing use in design theory. However, considering the analyses of Crowley and Head (2017), it is possible to note that the criticisms of researchers from various fields, and the proposed solutions to the issue, are not consensual in the research communities, regarding the understanding and possibilities of controlling wicked problems.

However, this investigation's objects are not only the wicked problems but also the tame problems, insofar as both are established on the basis of their distinctions and relationships, gravitating around a "dilemma" of origin. This choice is due to the understanding that approaches fail when they leave tame problems out of their analysis, associated with the natural sciences, with *determined* or *determinable causes* and therefore under control, or "tame", as their very name indicates. Such reliance on a fragmented and partial solution, in which one part of the issue, the tame one, is identified as solved, actually hinders progress in understanding the various dimensions of its correlate, the wicked one, focused on the *indeterminate* issues of planning and project.

In order to reflect on a solution that overcomes the contradictions, we propose going back to the point where the conceptual split occurs, where the elements, although dichotomous or *bifurcated*, are imbricated in their origin and are not understood as distinct, of a dual nature and, consequently, antinomic. The guiding principle of this reflection is that if the issue is defended in a dualistic way by Rittel and Webber, this does not mean that the way out of the aporia is to explain the dichotomy in a dualistic way. A monist approach (which does not simply accept one part of the phenomena as isolated from the whole and analytically resolved separately) is complex, but as comprehensive as far as the subject requires, and has the advantage of mediating between its parts.

[2] Popper works with concepts that could be similar to those of Rittel and Webber: "The terms 'open society' and 'closed society' were first used, to my knowledge, by Henri Bergson, in *Two Sources of Morality and Religion* (Engl. cd.,1935). In spite of a considerable difference (due to a fundamentally different approach to nearly every problem of philosophy) between Bergson's way of using these terms and mine, there is a certain similarity also, which I wish to acknowledge" (Popper, 1945, p. 178). However, in "Dilemmas" there is no source of origin for the concepts of "open systems" and "open societal systems".

To support our argument, we need to break down terms and expressions that have a considerable number of meanings, some of which are antagonistic. Thus, we established four caveats associated with terminology, seeking to suppress ambiguities, or at least mitigate them when specifically used in this essay.

(1) There are various definitions of *metaphysics* used throughout the history of ideas and in different schools of thought and thinkers. One of the possibilities for understanding metaphysics is to consider it as an impossible discipline, while aiming to know a transcendent reality, because its propositions do not belong to logic and mathematics or to the experimental sciences. This is, for example, the position of empiricism (including positivism and, in contemporary times, neopositivism or logical positivism), with which we disagree. Another, to which we adhere, is to understand it in the manner of Edwin A. Burtt (1925), Ernst Cassirer (1956, 1957), Werner Heisenberg (1958), Karl Popper (1962, 1972, 1976, 2005) and Thomas Kuhn (1970), as an open field that brings together concepts and principles forming a non-empirical basis as fundamental as sensible experience, for the constitution of the sciences and other areas of knowledge, among which we include projective disciplines and design.

(2) Bearing in mind the magnitude of the knowledge accumulated over the last few centuries, and which goes deep into the most varied fields of knowledge, we believe that an appreciable part of the problems and phenomena that have been *described and predicted by science,* based on their causes and effects, or that arise within advanced research, be they *wicked* or *tame*, are complex. So we don't think that wicked problems —as defined by Rittel and Webber— can be understood as synonyms for *Complex Problems*, in order to differentiate them from *Tame Problems*. As the authors of "Dilemmas" state: "The kinds of problems that planners deal with —societal problems— are inherently different from the problems that scientists and perhaps some classes of engineers deal with. Planning problems are inherently wicked" (Rittel; Webber, 1973, p. 160). Let us pay attention to the repeated use of the adverb "inherently", reinforcing that the distinction proposed by the authors for wicked and tame is not *quantitative* or of degree, but *qualitative*. Even if Rittel and Webber didn't make it explicit, a qualitative difference discriminates ontologically distinct classes of problems.Thus, if we strictly follow this delimitation, the gradation between what is simple and what is complex is not adequate to differentiate the two classes. In other words, complex cannot be understood as synonymous with wicked.

(3) With regard to the term "tame", it is important to consider that contemporary philosophers of science, including Popper and Kuhn, disconnect scientific knowledge (knowledge understood as tame in "Dilemmas") from an essence of truth. We follow Popper (1962) in disagreeing with the essentialist doctrine, which seeks ultimate explanations in the sciences and an absolute and immutable truth. We see scientific knowledge as well-founded and rigorous, but subject to change and replacement through the emergence and development of new theories capable of *describing* their objects of study better and more widely.

(4) We distinguish dichotomy from dualism, understanding the former as a division into pairs, of a heuristic nature, in other words, as a non-essential working hypothesis, and the

latter in a strong, ontological sense, of independence and antagonism between two singular units with distinct natures. Thus, unlike the irreducibility between the elements of dualism, two dichotomous properties or aspects can be understood within a single foundation, or monist conception.

2 Rittel and Webber: deliberate dualism

What defines a wicked problem? The title of section 3 of "Dilemmas" points to a concept that associates this type of problem with projective areas: "Planning Problems are Wicked Problems". Based on this identification, the authors try to corroborate it by contrasting characteristics of scientific practice with those of the planning fields:

> As distinguished from problems in the natural sciences, which are definable and separable and may have solutions that are findable, the problems of governmental planning —and especially those of social or policy planning— are ill-defined; and they rely upon elusive political judgment for resolution. (Not "solution." Social problems are never solved. At best they are only re-solved — over and over again.) (Rittel; Webber, 1973, p. 160).

Given these statements, we can ask ourselves: *unlike a scientific problem, especially in the "natural sciences", what would define a problem as wicked would be its vagueness, or in other words, its indeterminacy?* If this is the case, it would be equivalent to what philosopher Arthur Danto (1981, p. vii) calls an "elusive definition" when trying to define an indefinable field like art. We are facing a kind of paradox that permeates fields other than projective ones. But what matters is whether the science proposed by Rittel and Webber escapes this type of paradox or whether it is not completely immune to such indeterminacies.

A second element can be extracted from the previous quote: in order to preserve the full *determination* and *objectivity* of the sciences, the distinction proposed by the authors is one of nature and not of degree (according to caveat 2, section 1 of this essay). The consequence of this dualistic understanding (caveat 4, section 1) is that the classes are established as irreducible to each other, in other words, ontologically distinct. Such a conception is comparable to a Cartesian-type metaphysics, and its matrix of Platonic origin.

The comparison with René Descartes' dualistic ontology may seem anachronistic, but the consequences that interest us are not, given that its logical structure preserves a timeless meaning. It is a commonplace that this thinker has received numerous criticisms (even from his inner circle of rationalist followers, since the 17th century), when in his *"Meditations metaphysiques"* (1982) he describes reality as consisting of *two completely distinct substances:* thought (*res cogitans*), whose seat is the human mind, and extension (*res extensa*), which in its essence constitutes everything material and corporeal in the world.

If we draw a parallel with the difficulties faced by Descartes when he tried unsuccessfully to explain how these two completely different substances interact, Rittel and Webber could also suffer criticism of this kind if they sought such interaction between their two classes of problems. However, the authors do not fall into this trap of Cartesian modernity and feel safe when they propose a dual conception of knowledge, as they defend the unfeasibility of one of the delimited areas, while at the same time guaranteeing, by their own definition, the impossibility of transit and interaction between them. This is confirmed in the *intentional aporia* of the previous quote, when they categorically state that: "Social problems are never solved". As far as we know, the attempts to untie the Gordian knot proposed by Rittel and Webber, accepting a dualism in the origin of the

word “Dilemmas”, is equivalent to trying to mix water and oil, and the failure of this *re-union* in the Cartesian way should serve as a warning.

Therefore, if the dualistic definition of tame problems and wicked problems invariably leads to an aporia in relation to what can be known and solved within the areas of planning, and we do not admit this dead end, an alternative path is to return to what we believe to be the source of the problem, in its origin and ontological conception, and seek an understanding other than the dualistic one.

Among the theorists who seek to escape from “Dilemmas”, Bayard Catron (1981) and Richard Coyne (2005) point to a possible way out, which we understand as monistic. Catron states that: “Regardless of what R and W or others may contend, most scientific problems are ‘wicked’” (Catron, 1981, p. 14). Coyne, for his part, discusses philosophical theories that can offer answers to the aporia of wicked problems. Among the lines of thought analyzed by this author is John Dewey’s pragmatism. This formulation interests us, not because it is born of pragmatism, but because it questions the dualistic structure at its origin. According to Coyne:

> Within this pragmatic orientation it is possible to proceed with even bolder steps than Rittel and Webber were prepared to advance in their tentative 1973 article. In fact all problems have the character of wicked problems, even maths problems or simple puzzles. This response effectively dissolves the distinction between tame and wicked problems (Coyne, 2005, p. 8).

We agree with Coyne that understanding all kinds of problems as wicked “dissolves the [dualistic] distinction” by obviously suppressing one of the classes. The first positive consequence is the elimination of the antinomy by abolishing its dualistic source in origin. Only the dichotomy between exact and natural sciences on the one hand, and social, human, planning and design fields on the other, remains. This dichotomy is justified by the specific methods and objects of each area and is only perceived as a problem by scientism.

In order to move forward with this approach, an underlying issue linked to the expository form in “Dilemmas” needs to be made clear:

> The problems that scientists and engineers have usually focused upon are mostly “tame” or “benign” ones. As an example, consider a problem of mathematics, such as solving an equation; or the task of an organic chemist in analyzing the structure of some unknown compound; or that of the chessplayer attempting to accomplish checkmate in five moves. For each the mission is clear. It is clear, in turn, whether or not the problems have been solved.
>
> Wicked problems, in contrast, have neither of these clarifying traits; and they include nearly all public policy issues—whether the question concerns the location of a freeway, the adjustment of a tax rate, the modification of school curricula, or the confrontation of crime (Rittel; Webber, 1973, p. 160).

This issue is revealed in the description and examples of problems presented in the previous quote, as they deal with both classes at the same time. And the authors follow this same *modus operandi* when, afterwards, they expose ten distinctive properties of wicked problems in subsections: in most cases, they delineate the attributes of tame problems, and only then those of wicked ones, interspersing them with examples from physics, chemistry, logic, mathematics and even games such as puzzles and chess (associated with the first class of problems), and social policy and urban planning (gathered around the second class). Figure 1 summarizes the comparative structure of the main aspects in both problem genres:

Figure 1: Distinctive properties between tame and wicked problems.

PROPERTIES		
	Tame Problems	**Wicked Problems**
1	Formulable.	Not formulable.
2	Stopping rules with defined criteria.	There are no criteria and no end to causal chains.
3	Criteria for proving correct and false.	There are no true or false answers.
4	Tests under control and consequences immediately determined.	Tests generate various undetermined consequences over a long period of time.
5	The problem-solver can try various runs without penalty.	Whenever actions are effectively irreversible and whenever the half-lives of the aconsequences are long, every trial counts.
6	Finite set of rules.	Ill-defined problems and hence ill-definable solutions.
7	Families and classes of problems with characteristics that allow for common rules.	Each problem is essentially unique and does not allow the transfer of rules from the sciences.
8	—	Causal explanations are symptoms that lead to other causes.
9	A way to refute a hypothesis.	Various ways of refuting a hypothesis or saving it.
10	Scientists can make mistakes.	Planners can't make mistakes.

Source: Extracted by the authors of Rittel and Webber (1973).

Although the titles of the ten topics contain only wicked problems, which can distract us from the umbilical relationship between the two classes, the text, by the very dynamic of clarifying the properties of wicked problems, involves the attributes of both. This dynamic becomes clear when we contrast the two classes with some epistemological concepts from Kuhn's and Popper's theories.

3 Kuhn: "normal science", anomalies and "extraordinary science"

From the panorama established so far, another question emerges: *on what epistemological basis are Rittel and Webber's arguments based, when they formulate these ten properties?* To find an answer, we need to return to "Dilemmas", attentive to the hermeneutic sieve. One *clue*, disseminated throughout the text, comes from the comparison of science and its tame problems to games of chess, and (specifically in the exposition of the fifth property) designated by the metaphor of solving puzzles.

As we stated in section 1, it is possible that the parallels with these games, despite not containing an indication of authorship, derive from concepts of Kuhnian theory, proposed a decade earlier, in his controversial and widespread work, *"The structure of scientific revolutions"*. Regardless of how much Rittel and Webber are supported in this theory, it is essential to take into account that in order to guarantee coherence and consistency to his general conception of science, Kuhn establishes two interrelated concepts, that of *"normal science"* and that of *"extraordinary science*", united by the notion of *"anomaly"*.

Kuhn dedicates an entire chapter, *"Normal science as puzzle-solving"*, to the delimitation of the regular activity of scientists in periods when a scientific theory is consolidated and is successful in describing and predicting phenomena. But periods of *crisis in normal science*

occur when the paradigm that guides the practice of a scientific field cannot, using its internal rules, describe new phenomena, which, therefore, are identified as "anomalies" within the theory.

Extraordinary science emerges in these moments of paradigm crisis as a response to the partial failure of the current theory, with some members of the scientific community theorizing and speculating, returning to philosophy in search of metaphysical foundations and proposing new paradigms to get rid of the growing number of complementary explanations, known as *ad hoc* hypotheses. Here, it is important to highlight two points:

(1) Kuhn's general conception of science is based on the dichotomy between normal science and extraordinary science. These two complementary concepts are intertwined by the notion of anomaly, and his theory cannot be understood in the absence of one of them.

(2) The activity of extraordinary science, despite occurring during periods of crisis in the prevailing paradigm in normal science, is defined by Kuhn as fully capable of leading to the solution of events still without explanation, as long as it seeks the creation of new paradigms, new theories and rules derived from them, with consequent revolutionary ruptures in relation to the previous theory.

Thus, even if there is no conflict between Kuhn's concept of "normal science" and that of "science" in Rittel and Webber, since both solve puzzles with defined rules, the similarities do not go beyond this point and anomalies cannot be equated within "Dilemmas", because:

(1) It is not possible to understand the most recalcitrant anomalies as equivalent to tame problems because, like wicked problems, they escape the established rules.

(2) Nor is it possible to associate anomalies with wicked problems, since unlike these, they can be solved with a new set of rules, within a new paradigm.

If we accept the basic general structure of the Kuhnian conception, founded on the triad normal science→anomalies→extraordinary science, it becomes implausible to think that the problems of science are always tame. Thus, if Rittel and Webber have in mind "science" understood as "normal science", associating tame problems with puzzle solving makes the argument in "Dilemmas" precarious, as it only addresses part of what Kuhn's theory understands as the essence of science. And even if the authors do not make such an association, their understanding of science as capable of solving only tame problems remains limited, as it does not account for events that are solvable, but are not tame, such as anomalies. Perhaps, for this reason, the concept of normal science and, even less, that of extraordinary science are not made explicit in "Dilemmas".

Complementing our assumption, still in this indicial sphere, the relationship with Kuhnian concepts, or at least with his terminology, is expressed in Catron (1981): "While the rules governing the situation, the constraints and the criteria for acceptable solutions all are open to be decided, this is not different in *kind* from the practice of science during periods of paradigmatic crisis" (Catron, 1981, p. 16). Catron, however, does not follow Kuhn to the point of proposing that solutions to crises can be found from new paradigms and their derived theories. Rather, he suggests that: "Similarly, the search for explanations (WC #9) and for solutions (WC #6) is simultaneously an attempt to formulate (tame) the problem. In the scientific analogue, it is a matter of fitting anomolous ('unique') cases to existing 'theory'" (Catron, 1981, p. 16).

In our understanding, Catron's proposal to *"fit"* anomalies into the existing theory has the advantage of suppressing dualism, by placing the two classes of problems to be solved within a single theory, but it loses strength by not including an essential part of the Kuhnian formulation, that science tends to abandon weakened theories, due to the accumulation of numerous anomalies and *ad hoc* hypotheses, in favor of new theories. Furthermore, when Catron suggests "taming" the wicked problems, he seeks to lead the two classes of problems towards the tame ones, something already present in the title of his article: *"On taming wicked problems"*. Crowley and Head (2017, p. 542) reinforce our view by stating that "[...] he [Catron] was less inclined to dismiss the utility of scientific methods".

Additionally, in defense of our non-dualist perspective, it is important to take into account that Kuhn, not only an epistemologist, but also a theoretical physicist, does not radically separate natural sciences from other areas of knowledge, as occurs with the conception of Rittel and Webber. In the 1962 preface to *"The structure of scientific revolutions"*, Kuhn gives a personal account of his disagreement with those who believe in safer answers to problems coming from the natural sciences than from the social ones:

> The final stage in the development of this essay began with an invitation to spend the year 1958–59 at the Center for Advanced Studies in the Behavioral Sciences. [...] Particularly, I was struck by the number and extent of the overt disagreements between social scientists about the nature of legitimate scientific problems and methods. Both history and acquaintance made me doubt that practitioners of the natural sciences possess firmer or more permanent answers to such questions than their colleagues in social science. Yet, somehow, the practice of astronomy, physics, chemistry, or biology normally fails to evoke the controversies over fundamentals that today often seem endemic among, say, psychologists or sociologists (Kuhn, 1970, p. vii–viii).

And in the postscript included in the 1970 edition, *i.e.* before the publication of "Dilemmas", Kuhn states about the theses of his book:

> [...] they are borrowed from other fields. Historians of literature, of music, of the arts, of political development, and of many other human activities have long described their subjects in the same way. Periodization in terms of revolutionary breaks in style, taste, and institutional structure have been among their standard tools. If I have been original with respect to concepts like these, it has mainly been by applying them to the sciences, fields which had been widely thought to develop in a different way (Kuhn, 1970, p. 208).

Even considering the radical nature of Kuhn's positions (Stegmüller, 1976), these excerpts are surprising for their identification of similarities between the natural sciences and other areas of knowledge, but above all, in the postscript, for their insightful inversion of the scientistic idea that only the natural sciences influence the others.

4 Popper: science and metaphysics

A second indication, which can be explored in greater depth, as it is not just a clue, but has an explicit reference to authorship in "Dilemmas", only appears in the tenth property, when the authors cite Popper's first book, *"The logic of scientific discovery"*, a youthful work, originally published in 1935 in German and only in 1959 in English. As in the case of Kuhn, attention to chronology and the date of first publication is fundamental for the constitution of our interpretation, since two other works by Popper, *"Conjectures and refutations"*, from 1962, and *"Objective knowledge"*, from 1972, also precede the date of publication of "Dilemmas", which is hermeneutically relevant. Between 1935 and 1972, Popperian thought matured and underwent significant changes, which the authors apparently do not take into account, as they are based on

the 1935/1959[3] book. So let's follow each of these signs, trying to reconstruct the possible foundations of Rittel and Webber and see if there is congruence in their ideas.

One of the most acute problems in contemporary epistemology concerns the demarcation between science and philosophy, or between science and metaphysics, as Popper sometimes calls it (according to our caveat 1, section 1). To understand how this thinker reflects on the issue, which we correlate in "Dilemmas" to scientific fields *versus* fields of social planning, we must take into account that Popper always sets out his ideas in relation to empiricism and positivism. Popper came into contact with members of logical positivism through the "Vienna Circle" around 1926-27. At that time, this group still had a significant monopoly on the debate about scientific theories and trying to construct a vision of science necessarily led to a dialog with empiricists. In Popper's case, the dialog is critical.

The task that Popper sets himself, in *"The logic of scientific discovery"*, is to elaborate a concept of empirical science and give a clear demarcation between science and metaphysics: "My criterion of demarcation will accordingly have to be regarded as a *proposal for an agreement or convention*" (Popper, 2005, p. 15). If, on the one hand, this first Popperian demarcation proposal has as one of its objectives not to lead to the overthrow of metaphysics (with which we agree), on the other hand, the adoption of a concept of science as a convention, we will see, is problematic.

The complex and centuries-old *"Problem of Induction"* will not be analyzed here, as it is tangential to the subject of this essay. But since Popper believes that the core of his theory lies in criticizing the inductive logic defended by the empiricists, it is necessary to minimally expose the issue, in order to contextualize Popper's arguments in relation to "Dilemmas". So, roughly speaking, if it was precisely an empiricist, David Hume (1975), who stated, back in the 18th century, that induction, used in science, is not logical, Popper, seeking to preserve scientific rationality, goes further and postulates that science does not proceed inductively, contradicting Hume and influential scientists since Isaac Newton. The forceful consequence of this, accepted by Rittel and Webber, is that scientific theories are not verifiable. In Popper's words:

> Now in my view there is no such thing as induction.*[1] Thus inference to theories, from singular statements which are 'verified by experience' (whatever that may mean), is logically inadmissible. **Theories are, therefore, never empirically verifiable** (Popper, 2005, p. 18, our highlight).
>
> *1 I am not, of course, here considering so-called 'mathematical induction'. What I am denying is that there is such a thing as induction in the so-called 'inductive sciences': that there are either 'inductive procedures' or 'inductive inferences'.

Faced with this conclusion, Popper proposes a unique demarcation criterion that includes, in the realms of science, statements that are not empirically verifiable, but which are falsifiable. This means that we cannot "positively" verify whether a scientific system is valid, but we can validate it through empirical tests, in the "negative" sense. If such a system stands up to testing, it retains its validity. As one of the consequences (and unlike the essentialists), truth in the sciences does not lead to ultimate explanations. Therefore, a system that is not susceptible to refutation cannot be scientific. What is at stake is the master intuition of Popperian thought: only the process of refuting theories is logical and occurs not by induction, but by deduction.

[3] Note 4 of the 1973 paper by Rittel and Webber refers to a 1961 edition of Popper's book *"The logic of scientific Discovery"*, and does not mention his other works.

This does not mean that the non-scientific knowledge of metaphysics, for example, is meaningless, as empiricists and positivists maintain. The criterion that Popper establishes is not one of meaning, but of demarcation, and he insists on establishing such a position (POPPER, 2005, p. 18, note *3).

Rittel and Webber, in the tenth property of wicked problems, closely follow the Popperian argument:

> As Karl Popper argues in *The Logic of Scientific Discovery*,[4] it is a principle of science that solutions to problems are only hypotheses offered for refutation. This habit is based on the insight that there are no proofs to hypotheses, only potential refutations. The more a hypothesis withstands numerous attempts at refutation, the better its "corroboration" is considered to be. Consequently, the scientific community does not blame its members for postulating hypotheses that are later refuted—so long as the author abides by the rules of the game, of course (Rittel; Webber, 1973, p. 166-167).

Even following Popper in relation to the lack of empirical verification of theories, the authors do not admit that uncertainty in planning areas also exists in science. However, by relying on Popper, who does not establish certainty for scientific theories either, the separation of classes proposed as two distinct genres of problems is compromised. Given the Popperian foundation of non-positive verification of theories, we can ask: *would Popper admit that the problems of science are tame and with definitive solutions?* Certainly not, as this would lead to the admission of the verifiability of theories and consequent permanent validations in science, creating an internal contradiction in the Popperian conception.

But, let's remember, there is still the issue that Popper, in his debut book (the only work referenced by Rittel and Webber), defends the demarcation criterion as a convention (Popper, 2005, p. 15) and obviously knows that such a defense weakens his theory argumentatively. We don't find any elements associated with this point in "Dilemmas", but if its authors also endorse this fragile conventionalist position of the young Popper, it becomes more difficult for them to rationally justify the radical and dualistic separation between tame and wicked problems.

For his part, Popper, in his second book, *"Conjectures and refutations"*, seeks to eliminate conventionalism: "Theories are our own inventions, our own ideas; they are not forced upon us, but are our self-made instruments of thought [...]" (Popper, 1962, p. 117). In short, with the progressive refinement of Popper's theory, the concept of the nature of science ceases to be an object of convention, while at the same time preserving Popper's cherished idea that the veracity of facts cannot be guaranteed. In other words, a flawed rule of pure agreement with no basis in reality (conventionalism) is abandoned and another with broader possibilities is assumed, which is also fundamental to our purposes: *"inventions"*.

Let us make a small digression here to better understand the link between science and metaphysics, in Popperian theory, and the separation or dichotomy between these two areas of knowledge that does not imply dualism. We know that Popper opposes and criticizes empiricism on two fronts:

(1) The principle of induction in favor of deduction.

(2) The primacy of observation over speculation and metaphysical reflection.

In order to reinforce his arguments, this philosopher makes use of the position assumed by Albert Einstein (laureate of the Nobel Prize in Physics in 1921), in a letter from 1935. This

physicist's criticisms of logical positivism coincide with those of Popper. In the words of Einstein:

> Altogether I really do not at all like the now fashionable *(modische)* 'positivistic' tendency of clinging to what is observable. I regard it as trivial that one cannot, in the range of atomic magnitudes, make predictions with any desired degree of precision, and I think (like you, by the way) that theory cannot be fabricated out of the results of observation, but that it can only be invented (Popper, 2005, p. 482).

The clarity of Einstein's words leaves no doubt as to his position, as well as his knowledge of Popper's convictions, which were similar to his own. Einstein's statement becomes more incisive when we consider that this scientist's attention was focused on the natural sciences, particularly physics and within this, quantum mechanics and the theory of relativity, which were emerging at that time in the first decades of the 20th century as promising fields, formally and internally consistent, but not unified (and still aren't to this day) and with paradoxes related, for example, to the Principles of Complementarity and Uncertainty. According to physicist Werner Heisenberg (1958, p. 44), "The Copenhagen interpretation of quantum theory starts from a paradox". And this is yet another element that we oppose to Rittel and Webber's position: if, contrary to what the two authors think, "uncertainties" also occur in the natural sciences, which have been impregnated with mathematical quantification since the advent of modernity (Koyré, 1971), *wouldn't there be germs of wicked problems in scientific fields?* Furthermore, more than in fields considered exact, wouldn't the inexactitude inherent in the fields of public policy, planning, and projective fields such as design, be a justification for also "inventing" their theories?

Popper continues: "If this 'criterion of refutability' is accepted, then we see at once that *philosophical* theories, or metaphysical theories, will be *irrefutable* by definition" (Popper, 1962, p. 197). But if philosophical theories are irrefutable, how do we know which ones are true and which ones are false? Is there any way to rationally (critically) value an irrefutable theory? Popper states:

> My solution is this: if a philosophical theory [metaphisical theory] were no more than an isolated assertion about the world, flung at us with an implied 'take it or leave it' and without a hint of any connection with anything else, then it would indeed be beyond discussion. But the same might be said of an empirical theory [scientific theory] also (Popper, 1962, p.198).

And continue:

> [...] every *rational* theory, no matter whether scientific or philosophical, is rational in so far as it tries to *solve certain problems*. A theory is comprehensible and reasonable only in its relation to a given *problem-situation*, and it can be rationally discussed only by discussing this relation (Popper, 1962, p.199).

Again, in the Popperian dichotomy, science and metaphysics preserve a link between them, without the need for dualism.

A last, but not least important quest in Popper, which also clashes with the "Dilemmas" thesis, concerns the concepts of *cause* and *effect*, or what is known in epistemology as the *"Principle of Causality"*. Linked to this controversial principle (associated with science, philosophy and, in particular, metaphysics) are the description and prediction of phenomena, whether known or new. Rittel and Weber, when dealing with "the complex causal networks" (1973, p. 159), are fully aware of the relevance of causality as an efficient resource for describing and predicting natural phenomena, but, according to them, it is incapable of being applied unambiguously to social phenomena:

> [...] the problems that planners must deal with are wicked and incorrigible ones, for they defy efforts to delineate their boundaries and to identify their causes, and thus to expose their problematic nature. The planner who works with open systems is caught up in the ambiguity of their causal webs (Rittel; Webber, 1973, p. 167).

In *"The logic of scientific discovery"* Popper presents his definition of causality not associated, obviously, with the criticized inductive logic, but with the deductive one: "The 'principle of causality' is the assertion that any event whatsoever *can* be causally explained—that it *can* be deductively predicted" (Popper, 2005, p. 39). On the same page, however, contrary to what Rittel and Webber propose, Popper states peremptorily: "I shall, therefore, neither adopt nor reject the 'principle of causality'; I shall be content simply to exclude it, as 'metaphysical', from the sphere of science".

It is not part of the objectives of this essay to analyze whether this Popperian position is consistent. The subject is complex and even in theories such as quantum mechanics there is no consensus on the validity of the principle of causality on atomic scales (Cassirer, 1956). It is important to note that the principle of causality is not understood by Popper as a matter of science, but of metaphysical foundation. So when Rittel and Webber apply this concept, or its elements (determined causes), to justify their compatibility with the natural sciences and incompatibility with planning fields, they are going against what Popper advocates, relying on a principle that is not unanimous even in areas such as quantum mechanics.

Given these elements, it is to be expected that Popper takes into account the limitations of scientific and metaphysical theories, including his own. And if we analyze Popperian theory, we will see that it faces a problem generated within its own solution. On the one hand, it establishes a demarcation criterion with a double function: it imposes refutation tests and discards the principle of induction[4]. On the other hand, his criterion is incapable of evaluating speculations of a non-empirical nature. This is because the epistemology he proposes, including the criterion of falsifiability, is part of the speculations that cannot be refuted empirically, that is, it belongs to metaphysics. Popper justifies himself with the argument of the rationality of a theory, scientific or not, being capable of solving problems.

Even if we don't question the insufficiency of the Popperian theory to constitute a comprehensive criterion, we can ask ourselves: *can his definition of rational, which also seeks to justify his position, contribute in a general sense to thinking about social, projective and planning fields?* Precisely because he doesn't fall into the trap of a dualistic distinction between science and metaphysics, we believe so. Our understanding is corroborated by Popper's studies in the social sciences, in which the philosopher affirms an equivalence between natural and social sciences, in what he considers his main thesis (the sixth) in this area: "The method of the social sciences, like that of the natural sciences, consists in trying out tentative solutions to certain problems: the problems from which our investigations start, and those which turn up during the investigation" (Popper, 1976, p. 89).

[4] It's not just Popper who dismisses induction. Despite all the criticism that Kuhn (1970) imposes on Popperian positions, and vice versa, this epistemologist is in full agreement with Popper about his criticism of the *Principle of Induction*, and his rejection of the priority that empiricism gives to observation, to the detriment of metaphysical foundations, in the constitution of the sciences. As the historian of philosophy Wolfgang Stegmüller (1976) states, if Kuhn is not explicit about this agreement, it is because, for him, the empiricist position has no relevance.

As for Rittel and Webber, they could have benefited from those other developments in Popperian theory announced a decade before the publication of "Dilemmas". Here, we insist on the relevance of the Popper *versus* empiricism cleavage. A clear example of this can be found in the paper by design theorist Richard Buchanan (1992), when he criticizes economist Herbert Simon (1969) and his classic book *"The sciences of the artificial"*, specifically for an empiricist and "neopositivist" position. Advancing in his critique of empiricism, Buchanan draws attention to the influence of this school of thought on Rittel's initial ideas and their relationship with Popper's conceptions:

> The phrase wicked problems was borrowed from philosopher Karl Popper. However, Rittel developed the idea in a different direction. Rittel is another example of someone initially influenced by neo-positivist ideas who, when confronted with the actual processes of practical reasoning in concrete circumstances, sought to develop a new approach related to rhetoric (Buchanan, 1992, p. 16, note 36).

Taking these statements into account, we highlight three points:

(1) The way Buchanan puts it, it might seem that he associates the Popperian thought with neopositivism, a misconception that some interpreters sometimes make due to the fact that Popper deals with themes investigated by contemporary empiricists. However, as we have seen, Popper has been against this school since his first book.

(2) Buchanan does not say where the expression "wicked problems" is found in Popper, and as far as we were able to investigate, we did not find it in Popper's writings. But if the authorship really belongs to Popper, this is yet another reason to associate Rittel and Webber with Popperian issues and question the dualism of "Dilemmas".

(3) Finally, the most important thing: if Rittel and Webber were to put the other Popperian concepts we have explained on the agenda, they could choose at least two paths:

(A) To abandon any and all Popperian concepts, to preserve the radicalism of dualism and, consequently, the two classes of problems, and to continue relegating the fields of planning to the same limbo of a metaphysics advocated by the empiricists.

(B) To assume (in addition to the criterion of refutability) Popper's other concepts and, consequently, to admit that the class of wicked problems is also within the scope of science, to abandon dualism and aporia, incorporating monism in its place, and to be able to maintain the dichotomy between sciences and projective and planning fields.

Insofar as paths A and B are internally coherent, but go in different directions, being *logically and metaphysically irreconcilable with each other*, Rittel and Webber's conception, which brings together part of the two paths, proves to be deficient as in Kuhn's previous case: it is not coherent to maintain only Popper's criterion of refutability, as they do, leaving out the other elements that abolish dualism.

5 The two horns of a bull

At the end of this essay, and with it the deconstructive critique of "Dilemmas", we are betting on path B, monist. We share Rittel and Webber's understanding that projective and planning fields are not science, but not for the same reasons as the two authors. The approach we propose leaves out a myriad of questions raised by Rittel and Webber. The richness of a

prolific piece of writing like “Dilemmas” attracts researchers even 50 years after its publication. An example of this fertility can still be found in the *“Problem Definition”* section, with an unexpected statement by the authors about the impossibility of achieving an idealized planning system: “And yet we all know that such a planning system is unattainable, even as we seek more closely to approximate it. It is even questionable whether such a planning system is desirable” (Rittel; Webber, 1973, p. 159). Of course, as we have seen, an unattainable planning system is at the heart of the matter in “Dilemmas”. *But why, for the authors, would an idealized system be undesirable?* Perhaps because it eliminates the richness of social plurality by accepting a single, absolute truth. If this is the case, to a certain extent, Rittel and Webber, with all the conceptual differences and nuances between their positions and those of Popper and Kuhn, also maintain a certain distance from an essential and immutable truth, holder of ultimate explanations, at least in the context of social fields. For our part, a broader understanding of the limits of science, metaphysics and the most varied forms of knowledge, rather than reducing the importance of all these fields, removes the risk of them being understood and accepted dogmatically.

Curiously, the non-essentialist position on truth defended by Popper and Kuhn finds in Nietzsche a herald of the limits of science, in his *“The birth of tragedy”*. And with a metaphor written a century before that of Rittel and Webber (1973, p. 160), about “leprechaun”, “lion” and “lamb”, this philosopher warns us about the excesses of trust placed in science:

> What I had got hold of at that time was something fearsome and dangerous, a problem with horns, not necessarily a bull, but at any rate a new problem; today I would say that it was the *problem of science* (*Wissenschaft*) itself, science grasped for the first time as something problematic and questionable (Nietzsche, 1999, p. 4–5).

With purposes other than those of the provocateur Nietzsche, but still inspired by his words, it is possible to think that underlying two acute problems, like horns, there is a single bull to be understood in its intentions. Thus, we seek to demonstrate that the dualism that separates the two classes of problems is illusory from the epistemological and ontological points of view, and perhaps this chimera is the main reason that frustrates, as far as we know, attempts to find a reasonable answer to the aporia of “Dilemmas”.

On the path we follow, we do not discover, or “uncover”, a truth hidden, in the Platonic way, contained in a pre-existing reality. On the contrary, to escape this and other ontologically similar aporias, it is necessary to *construct*, or *invent*, a way out, in the manner of projective fields such as *design* and speculative fields such as *metaphysics*. Such fields, due to the inventive dynamics of their processes, point to unusual and fertile directions, by assuming the *entanglement* of their constituent elements and objects, in an inseparable relationship with human beings and nature.

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