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Three problems with Kuhn's concept of "crisis"

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Resumo

O objetivo deste artigo é explorar a noção de "crise" de Thomas Kuhn e indicar algumas de suas dificuldades. Em primeiro lugar, Kuhn define "crise" por meio da noção de "anomalia", distinguindo esses conceitos de duas maneiras diferentes: categórica e quantitativamente. Ambas as alternativas, no entanto, se mostram problemáticas. A definição categórica se baseia em uma distinção entre "descobertas" e "invenções" que, como o próprio Kuhn admite, é bastante artificial. A definição quantitativa, por sua vez, afirma que as crises são um tipo de anomalia mais profundo. Kuhn, entretanto, não oferece nenhum critério que permita definir de maneira objetiva essa "profundidade" das crises. O segundo tipo de problema está relacionado à aplicação do conceito de "crise". Aparentemente, Kuhn atribui crises a indivíduos tanto quanto a comunidades. Por fim, há o problema da função das crises. Em *The Structure of Scientific Revolutions*, elas são apresentadas como uma pré-condição para as revoluções científicas. Em artigos posteriores, no entanto, Kuhn parece vê-los apenas como um antecedente comum das revoluções.

Palavras-chave: Thomas Kuhn, Crise, Anomalia, Revolução Científica, Filosofia da Ciência.

Abstract

The aim of the article is to explore Thomas Kuhn's notion of "scientific crisis" and indicate some difficulties with it. First, Kuhn defines "crisis" through the notion of "anomaly" but distinguishes these concepts in two different ways: categorically and quantitatively. Both of these alternatives face considerable problems. The categorical definition relies on a distinction between "discoveries" and "inventions" that, as Kuhn himself admits, is artificial. The quantitative definition states that crises are a deeper, more profound type of anomaly. Kuhn, however, does not offer any criteria for objectively defining this "severity" of the crises. The second kind of problem is related to the application of the concept of "crisis." Apparently, Kuhn attributes crises to individuals as much as to communities. Lastly, there is the problem of the function of crises. In *The Structure of Scientific Revolutions*, they are presented as a precondition to scientific revolutions. In later articles, however, Kuhn seems to see them only as a common antecedent to revolutions.

Keywords: Thomas Kuhn, Crisis, Anomaly, Scientific Revolution, Philosophy of Science.

1 Introduction

In *The Structure of Scientific Revolutions* (SSR, 1962), Thomas Kuhn advances a general model of how natural sciences develop over time. According to him, scientific disciplines are born with the acquisition of a paradigm — a concrete solution to a problem that guides the resolution of new puzzles.¹ From that point onward, science can only experience two kinds of changes: the cumulative process of normal science, which aims to articulate and make the paradigm more precise, and scientific revolutions, the substitution of one paradigm for another. Acquisition of a paradigm, normal science, crisis, extraordinary science, scientific revolution, and normal science once more—these are the successive (and recurrent) stages by which natural sciences historically develop.²

The purpose of this article is to present some difficulties in defining the concept of "crisis," which seems central to Kuhn's scientific development model.³ I will first discuss the very notion of crisis. Kuhn defines it in SSR through

¹Later, Kuhn changed that view. He began to consider that the different schools were also guided by a kind of paradigm, although not one that "identifies challenging puzzles, supplies clues to their solution, and guarantees that the truly clever practitioner will succeed" (KUHNN, 1970b, p. 178).

²This is a rather schematic description of the model proposed by Kuhn in SSR. Furthermore, his ideas on these themes changed considerably over time. He came to assume that the so-called pre-paradigmatic schools possessed some kind of paradigm (see footnote 1 above) and that what actually characterized the development of science was speciation rather than mutation (KUHNN, 1991). I will discuss his different views on the idea of scientific crisis in section 5.

³How important it actually *is* is the subject of section 5.

the idea of "anomaly." Thus, the first section of the article will be devoted to exploring that.

As discussed in the following section, Kuhn contrasts these concepts in two different ways. In one, crises and anomalies are considered categorically different; in the other, the difference is presented as a matter of degree. Both formulations, however, have considerable problems. A qualitative difference between anomaly and crisis ultimately depends on a real distinction between "discoveries" and "inventions," something that Kuhn himself denies. On the other hand, if we consider these concepts as differing only in degree, there is the problem of finding a threshold to distinguish them.

A second kind of problem, which is the subject of section 4, stems from the dubious nature of crises. On the one hand, they seem to be linked to scientists' individual evaluations and their perception that the theory has failed seriously. On the other hand, Kuhn also presents crises as if they were a diffuse symptom in the scientific community.

The last problem concerns the role assigned to crises for the outbreak of scientific revolutions. In *SSR*, crises are seen as fundamental to the weakening of a dominant paradigm and therefore as contributing to the emergence of an alternative. In Kuhn's later writings, especially (KUHN, 1970b), crises seem to become a dispensable mechanism for the transition between theories. This is the subject of section 5.

The aim of this paper is, therefore, to identify three types of problems related to Kuhn's notion of crisis: ontological (what are scientific crises?), methodological (who experiences crises?), and explanatory (what do crises do?).

2 Anomalies

In *SSR*, Kuhn defines the concept of crisis based on the notion of the anomaly.⁴ But, after all, what are anomalies? For that, it is necessary to discuss first what Kuhn understands by "puzzles".

According to Kuhn, the acceptance of a paradigm by a community—whether in the emergence of an initial tradition of normal science or after a scientific revolution—depends on two essential elements. The first is that the achievements

⁴This does not imply assuming a logical precedence of the latter concept over the former. Both concepts could be defined independently, and the comparison could operate in the opposite direction as well—defining anomalies through crises. However, Kuhn opted to explain anomalies first rather than crises. For practicality, I will mirror the explanatory structure of his arguments. None of the problems pointed out here depend on the reliance of one of these concepts on the other.

of the paradigm be "sufficiently unprecedented to attract an enduring group of adherents" (KUHN, 1962, p. 10). This was, for instance, the case of Newtonian optics, which, in establishing the corpuscular approach to the composition of light, came to settle the first widely accepted theory in the field.

However, to consider the achievements of a paradigm as being superior to that of its competitors does not mean to consider the paradigm as a finished piece of knowledge. According to Kuhn, a good paradigm never solves all the problems it faces. Here we have the second characteristic of the paradigms of normal science pointed out by Kuhn: their achievements must be "sufficiently open-ended to leave all sorts of problems for the redefined group of practitioners to resolve" (KUHN, 1962, p. 11), thereby maintaining a tradition of scientific research.

Almost by definition, then, paradigms leave a great number of problems open, and it is the task of normal science to solve them. These problems, which Kuhn names "puzzles," have four main characteristics: i) they are part of a research tradition; ii) the puzzle is, to a great extent, "the only problem that the community will admit as scientific or encourage its members to undertake" (KUHN, 1962, p. 37); iii) puzzles have a guaranteed solution — failure to solve such problems is considered a failure of the scientist, not of the theory;⁵ and iv) any solution to them must obey "rules that limit both the nature of acceptable solutions and the steps by which they are to be obtained" (KUHN, 1962, p. 38). Kuhn also classifies the theoretical and empirical puzzles that scientists deal with in normal science as being basically of three types: "determination of significant fact, matching of facts with theory, and articulation of the theory" (KUHN, 1962, p. 34).

Thus, scientists are busy trying to solve the unsolved problems or puzzles in their respective fields. But what exactly does it mean to say that a problem is not solved? That can, in fact, have a number of meanings. Kuhn does not offer a typology of unsolved problems, but Laudan (1977) develops this issue more deeply. Firstly, such a problem may be one for which there is absolutely no solution. This was once the case with theories that assumed the existence of an ether and required evidence of the existence of this substance (KUHN, 1962, p. 72ff). Alternatively, such a problem may be one for which existing solutions are not seen as satisfactory or, more gravely, a problem whose current

⁵Having a guaranteed solution means that the scientist believes the problem can be solved according to the current paradigm. In other words, the scientist maintains expectations about its solvability.

solutions conflict with the empirical evidence available. In short, a problem may be said to be unsolved in three different cases: when there is no solution for it, when the solution is considered to be wrong, or when the solution is far from ideal. Scientists apply their energy and time to attempts to find acceptable and adequate solutions to such problems.

It is possible now to understand what anomalies are. An anomaly is a specific type of unsolved problem. What distinguishes it from other unsolved problems is that it involves "the recognition that nature has somehow violated the paradigm-induced expectations that govern normal science" (KUHN, 1962, p. 53). Thus, anomalies are unsolved problems that affect the expectations held by scientists.⁶

What kind of expectations would be violated by anomalies? Kuhn presents not one but at least three different types of expectations. The first relates to "expectations about nature" (KUHN, 1962, 127) — what kinds of behavior one can imagine the natural world will display, as well as what types of objects exist. It is, therefore, a belief about what one expects to find during investigations.

The second type of expectation that Kuhn refers to is the set of "instrumental as well as theoretical expectations" (KUHN, 1962, p. 59) linked to a paradigm. This second modality derives from the first: if the scientist believes that a certain type of behavior will occur in nature, it is natural to imagine that certain types of equipment and approaches will work accordingly.

There is yet a third type of expectation, which is linked to the capacity of the paradigm to deal with the problems it faces. As discussed, normal science never solves all the problems it faces; there are always many questions for which scientists have no adequate solution. The nature of normal science, though, assures them that existing problems will be solved through the current paradigm. In other words, the paradigm leads the scientist to expect that unsolved problems will be solved in the future.

If these are the expectations that underpin scientists' research, it is easy to understand the circumstances in which an anomaly is a violation of expectations. The first such violation, in which the first two types of expectations can be grouped, is a confrontation with situations that the theory did not anticipate: an unforeseen phenomenon or a difficulty in the use of instrumentation for which the "paradigm has not readied the investigator" (KUHN, 1962, p.

⁶One of the difficulties of finding a precise characterization of anomalies in SSR is that Kuhn uses the term not only for problems that generate "violations of expectations" but also for violations of expectations themselves (KUHN, 1962, p. xliii).

57). This is a spectrum ranging from an extreme where the theory says nothing about a particular situation to the case where a theory's predictions are directly contrary to the empirical evidence. The second type of expectation break is that which results from frustration with the problems that the theory proved surprisingly incapable of responding to.⁷

These two modalities of expectation breaks are ultimately related. Situations that conflict with what is expected of nature or of the instruments — the first type of expectation break — force the scientist to pay attention to the source of difficulty. The indirect effect of the theory's failures is then to provoke a mistrust in scientists as to the theory's ability to guide research — that is, a violation of expectation in the second sense. The failure of the paradigm to deal with unexpected situations draws the scientist's attention to the difficulty, undermining confidence in the paradigm's ability to deal with unsolved problems. In a certain sense, it is the second meaning of "anomaly" that is the most fundamental.

3 Crisis and anomaly

Kuhn defines the concept of the crisis through the concept of the anomaly (KUHN, 1962, p. 66-7). But it is not so easy to understand how he does that. Indeed, Kuhn contrasts anomalies and crises in two different ways.

His first characterization is qualitative. He states that crisis is to invention (a novelty in the realm of theories) what anomaly is to discovery (a novelty in the realm of phenomena) (KUHN, 1962, p. 67). However, as Kuhn himself admits, this analogy is fragile; the distinction between discovery and invention is, he writes, "exceedingly artificial" (KUHN, 1962, p. 53; see also KUHN, 1962, p. 7, 33). The very notion of discovery, as Kuhn carefully explains in chapter 6 of *SSR*, involves the recognition not only that something exists but also the simultaneous recognition of the nature of this new element. In this sense, novelties in the realm of phenomena also presuppose processes of conceptual assimilation.⁸ It becomes clear, therefore, how problematic Kuhn's first attempt at defining a crisis is.

Kuhn's second characterization of crises through anomalies is quantitative.

⁷Hoyningen-Huene refers to these two cases as an "anomaly" and "anomalous problem," respectively (HOYNINGEN-HUENE, 1993, p. 224).

⁸"Scientific fact and theory are not categorically separable, except perhaps within a single tradition of normal-scientific practice. That is why the unexpected discovery is not simply factual in its import and why the scientist's world is qualitatively transformed as well as quantitatively enriched by fundamental novelties of either fact or theory" (KUHN, 1962, p. 7).

Their difference would be only of degree, a matter of greater or lesser intensity. Kuhn, for instance, states that crises produce "a similar but more profound awareness" (KUHN, 1962, p. 67) than anomalies. Broader changes of theories would be motivated by events similar to anomalies but of much greater amplitude — that is, crises.

This second definition, distinguishing anomalies and crises by their degree of severity, seems more plausible, since Kuhn points out a series of similarities between both notions. Their importance depends on the size of the violation of expectations, they only make sense when put "against the background provided by the paradigm" (KUHN, 1962, p. 65), they vary according to the belief in the future ability of the theory to deal with unsolved problems, and they are generally events that scientists' are conscious of, among other things.

The problem with this second definition comes when we try to pin down the line that separates anomalies and crises. There is no clear criterion that distinguishes a severe anomaly from a crisis. When is a break of expectations no longer a mere anomaly but instead a crisis of the paradigm? How does one set this threshold? If the establishment of a line is impossible, it is not obvious why it is necessary to introduce two different concepts for one single phenomenon.

One possibility would be to consider that these are relatively vague concepts which would nevertheless have some function, even though it is not possible to precisely distinguish anomalies and crises nor to indicate where the passage from one stage to another is. I will discuss this possibility in the conclusion.

Let us now examine another possibility. Why not consider that what differs a crisis from a simple anomaly is its broad effect in the community, as opposed to the restricted extent of anomalies? A crisis would affect the community, while an anomaly would be restricted to the individual. This leads to a second set of questions about who is experiencing scientific crises.

4 Psychology and sociology

Thus, a second set of problems related to the concept of crisis relates to the agent that experiences the related events. By definition, a crisis is an acute perception that the paradigm is not performing properly. But who, after all, realizes that? Is it the community or the individual who sees an unsolved problem as representing a direct attack on the paradigm?

It is interesting to consider here what Kuhn states about the severity of anoma-

lies and what makes some anomalies more relevant than others. He points out some key factors, linked to the two types of expectation breaks previously discussed.

First, the severity of an anomaly varies according to the degree of radicality with which scientists' expectations about the nature are violated. In other words, the strength of the anomaly depends on the difficulty it brings to the theory — the anomaly, Kuhn asserts, is proportional to the "estimate of the extent to which the phenomenon violated paradigm-induced anticipations" (KUHN, 1962, p. 56). Thus, an anomaly that calls into question explicit and fundamental generalizations of the paradigm is very pressing. Furthermore, the magnitude of an anomaly increases or decreases according to its resistance to the scientists' attempts to resolve it.

All these elements, it should be noted, are fundamentally individual assessments. Different scientists may have conflicting perspectives on the importance they attach to an anomaly — both about the extent to which the problem violates theory and about how difficult it has been to find a solution (KUHN, 1977). These different evaluations may derive from their distinct research activities, from social concerns (the practical importance of the problem), from personal matters (the familiarity of the scientist with the problem and her field of action), institutional matters (the prominence of the scientist in the area), and so forth. To a lesser extent, these divergences represent the same variation that exists between supporters of rival paradigms, who "disagree about the list of problems that any candidate for paradigm must resolve" (KUHN, 1962, p. 147).

If this is true for the anomalies, it must also apply to the crises. After all, a crisis is an extreme violation of expectations, and expectations vary from person to person, depending on their different backgrounds. The debates between proponents of Copernican and Ptolemaic astronomy, described by Kuhn, demonstrate well this aspect of crises. What Copernicus saw as an incorrigible failure of Ptolemaic astronomy, followers of the latter saw as a normal puzzle, a temporary gap in between observation and theory (for other examples, see KUHN, 1962, p. 54–58).

Therefore, a violation of expectations seems to be a violation of expectations *for a specific individual*. The difference in judgment among scientists is actually what explains how multiple paradigms coexist at certain times and how a new paradigm "emerges first in the mind of one or a few individuals" (KUHN, 1962, p. 143; see also KUHN, 1962, p. 90). While some individuals maintain their

belief in the traditional paradigm, others experience crises, feeling the need to develop new approaches.

Nonetheless, the examples provided by Kuhn in *SSR* point simultaneously to a sense of crisis in the community. He says, for example, that there was a crisis in Ptolemaic astronomy before Copernicus, and a crisis in chemistry that preceded Lavoisier (KUHN, 1962, chap. vii).

Thus, we can find two distinct concepts of crisis in *SSR*. On the one hand, crisis is seen as the breakdown of an individual's expectations, a logical or psychological phenomenon. On the other hand, a crisis is also displayed as an eminently sociological concept: a diffuse feeling in the community regarding the inability of the paradigm to solve the most fundamental problems of the field.

This difficulty is typical of the presentation found in *SSR*. Throughout the book, there is a clear tension between individual-level and community-level analyses — an error of which Kuhn concedes he has repeatedly been guilty. He often conflates psychological and sociological concepts, treating "groups as individuals writ large or else individuals as groups writ small" (KUHN, 1993, p. 241; see also KUHN, 1989, p. 86–89).

But how do we reconcile these two levels of analysis? The answer lies in an understanding of the highly homogeneous nature of scientific pedagogy. The relative standardization of professional paths causes a great similarity between scientists' applications of paradigms, as well as their perceptions about theories' merits. Scientists, therefore, tend to have fairly similar views on what the most relevant problems in their fields are, what problems the theories should have been able to solve and were not, and so forth — even if marginal differences may have profound effects for the resolution of controversies (see KUHN, 1962, chap. xii). This is what makes it possible to generalize the effects of the awareness of persistent anomalies to the whole community.

Returning to the question raised in the previous section — whether a crisis might differ from an anomaly by its breadth — I conclude that it does not. Both crises and anomalies are fundamentally individual events that, by virtue of the similarity between scientists' professional and educational trajectories, can somehow be treated as generalizations about the communities. In this sense, at least, there is no difference between anomalies and crises.

5 The function of the crisis

A third problem regarding the notion of crisis pertains to its role within the explanatory model proposed by Kuhn. Crises are a violation in expectations, which arises from the fact that the solution for a major problem shows itself to be more difficult to handle than one might expect. The immediate effect is to shake scientists' confidence in the paradigm. The difficulty of providing a response to a problem that is supposedly solvable causes "pronounced professional insecurity" — insecurity "generated by the persistent failure of the puzzles of normal science to come out as they should" (KUHN, 1962, p. 68).

During a period of normal science, a paradigm is seen as providing the outlines of legitimate problems and solutions. In times of crises, however, the affected scientists no longer trust the paradigm as the basis of scientific research. Instead, there is the "blurring of a paradigm and the consequent loosening of the rules for normal research" (KUHN, 1962, p. 84). The outcome of the crisis is hence

the proliferation of competing articulations, the willingness to try anything, the expression of explicit discontent, the recourse to philosophy and to debate over fundamentals (KUHN, 1962, p. 91).

Kuhn insists that if it were not for the crises, few scientists would ever be persuaded to abandon a successful paradigm for a candidate without any retrospect of success. For this to occur, it is necessary that "nature itself must first undermine professional security by making prior achievements seem problematic" (KUHN, 1962, p. 168).

Hence, in *SSR*, Kuhn argues that crises play a key role in theoretical change. By undermining confidence in the current paradigm, crises allow the emergence of new contenders. This may lead to a new paradigm taking the place of the older one — what Kuhn calls a "scientific revolution." For this reason, he claims that crises are "prerequisite to all acceptable changes of theory" (KUHN, 1962, p. 67). Just as the perception of an anomaly is fundamental to the discovery of new types of phenomena, "the sense of malfunctioning that can lead to a crisis" (KUHN, 1962, p. 93) in the paradigm is a prelude to major changes in theory.

This, at least, is the function of crises in *SSR*. After the publication of the book, in the face of innumerable criticisms, Kuhn made some important remarks on the notion of crisis in a series of texts (KUHN, 1970a; KUHN, 1970b).

Two major aspects were emphasized in those texts.

The first point regards the elements that are responsible for provoking a crisis. Previously, Kuhn had emphasized two factors: problems that the paradigm had difficulty in solving and issues outside science that could increase the relevance and urgency of certain problems. Now, he mentions a third possible cause for crises within a specialty: results obtained in other areas. In some degree, scientific disciplines are always in interaction. Because of that, new instruments or laws "may develop in one specialty and their assimilation create crisis in another" (KUHN, 1962, p. 180).

What is most important for our discussion, however, is the second point addressed by Kuhn. According to him, although crises are a common component preceding changes in theories, they are not strictly necessary for scientific revolutions. Instead, they would be only a frequent element in the transition from one paradigm to another. In his words,

Nothing important to my argument depends, however, on crises' being an absolute prerequisite to revolutions; they only need to be the usual prelude, supplying, that is, a self-correcting mechanism which ensures that the rigidity of normal science will not forever go unchallenged (KUHN, 1970b, p. 180; contrast that to KUHN, 1962, p. 28, 67, 93).

Unfortunately, Kuhn does not discuss the role of crises much further. All that is possible to know is that Kuhn no longer saw crises as an indispensable preamble to scientific revolutions.

6 Conclusion

Throughout this article, I have analyzed three classes of problems that involve Kuhn's notion of crisis. The first concerned the nature of these episodes. Kuhn defines crises through anomalies, but he does that in two distinct ways: qualitatively and quantitatively. In the first case, he says that anomalies precipitate discoveries of new phenomena, as well as that crises trigger wider theoretical changes. The difficulty with this analogy is that the distinction between discoveries and inventions is, as Kuhn himself admits, extremely artificial. The second possibility, which considers anomalies and crises as differing only in degree, is more promising, but lacks any further clarification. At what point, after all, does an anomaly become a crisis?

Another set of questions concerns those who suffer the effects of crises: are they individual scientists or scientific communities? The notion of crisis seems to make more sense when applied to individuals, since assessments of violation of expectations may vary from person to person. However, there is a sense in which crises can be seen as hitting entire communities or at least groups of scientists. Since scientists undergo fairly similar and homogeneous professionalization processes, it is natural that they evaluate theories in a very similar way. For this reason, a violation of expectation for an individual is also often a violation of expectation for the rest of the community.

Finally, I discussed two different conceptions of the role of crises. At first, in *SSR*, crises were seen as being a prerequisite for changes of theory. In his later writings, however, Kuhn softens this demand, saying that they are not a necessary antecedent to revolutions but only a frequent one. However, he does not develop this reformulated view.

In discussing the problems pointed out here, I do not intend to suggest that the concept of "crisis" is of little or no use for Kuhn's enterprise. On the contrary, this notion plays an important role in his explanatory model. The notion of crisis is particularly useful for his historical descriptions, since they allow him to emphasize, albeit in a very general way, the psychological factors in the minds of the scientists that facilitate the acceptance of a new paradigm. However, as an epistemological concept, crisis seems to be rather fragile. I have tried to show some of the problems with it and how they could be overcome.

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