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Article

The structure of scientific controversies: Thomas Kuhn's social epistemology

A estrutura das controvérsias científicas: a epistemologia social de Thomas Kuhn

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ABSTRACT

Changes of theories are major events in science. Two main types of questions may be asked about them: i) how do scientists choose new theories?, and ii) how is consensus formed? Generally, philosophers do not distinguish these two questions. Kuhn, on the contrary, offers very different answers to each of these questions. *Theory-choice*, on the one hand, is explained through the application of epistemic criteria, such as accuracy and consistency; nonetheless, because these values do not prescribe a single choice, *consensus formation*, on the other hand, is explained through a series of socio-epistemic mechanisms, namely: scientific pedagogy, diffusion and production of knowledge within the community (the "wave motion"), and restructuring of the scientific field. These mechanisms are the basis of Kuhn's social epistemology, in that they are not restricted to sociology nor epistemology, encompassing both social interactions and epistemic evaluations of theories.

Keywords: Thomas Kuhn, consensus formation, social epistemology.

RESUMO

Mudanças de teorias são eventos centrais na ciência. Dois tipos principais de perguntas podem ser feitas em relação a eles: i) como os cientistas escolhem novas teorias? e ii) como o consenso é formado? A maior parte dos filósofos não costuma distinguir estas duas questões. Kuhn, entretanto, oferece respostas muito diferentes para cada uma delas. A *escolha de teoria* é explicada, por ele, por meio da aplicação de critérios epistêmicos, como precisão e consistência. O fato de que esses valores não prescrevem uma única escolha, contudo, leva-o a explicar a *formação de consenso* por meio de uma série de mecanismos socioepistêmicos, a saber: a pedagogia científica; a difusão e produção de conhecimento dentro da comunidade (o "movimento das ondas"); e a reestruturação do campo científico. Esses mecanismos são a base da epistemologia social de Kuhn, na medida em que não se restringem nem à sociologia nem à epistemologia, envolvendo tanto interações sociais como avaliações epistêmicas de teorias.

Palavras-chave: Thomas Kuhn, formação de consenso, epistemologia social.

Introduction

The acceptance of a new theory is always a major happening in science. Not surprisingly then, philosophers have long been concerned with explaining such events. At least two main types of questions may be involved in this task. One is a question regarding the individuals that participate in the controversy—"why did scientist S choose theory T?" The second is a problem regarding the community as a whole—"how was scientific consensus around theory T formed?" The former may be referred to as the problem of theory-choice and the latter as the problem of consensus formation.

These two questions are often viewed as the same by philosophers, or at least as being connected in such a way that they end up receiving similar answers. The underpinning reasoning goes as follows: provided that scientists act rationally, then the reasons that explain their choices should also be the reasons that explain why they all agree. Thus, the answer to the question of why scientists prefer one theory to another is simultaneously taken as an answer to the question of why a theory is accepted by the whole community.

Here, I want to explore Kuhn's answers to these two problems. Let me start with the first of them: how does Kuhn respond to the theory-choice problem? According to him, scientists choose theories based on a set of values, such as accuracy, consistency, scope, simplicity, and fruitfulness, which are characteristic of science and conveyed by more experient members of the community through practical contexts. Therefore, Kuhn gives a standard and straightforward answer to the problem of theory-choice: scientists choose theories based on a set of epistemic values (see, for example, 1962, 1970a, 1977a, 1983a).

However, Kuhn also states that, because scientists have different personal and professional experiences, they may end up applying values differently in concrete cases (1977a, 1983a). In other words, the nature of values is such that equally competent researchers in possession of the same body of evidence may achieve distinct conclusions about what is the best theory in a field (for a more extensive discussion on Kuhn's theory of rational choice, see Pirozelli, 2019). I refer to this as Kuhn's thesis of the variability of values.

As a consequence, such variability of values produces a cleavage between individual-level questions and community-level ones. The epistemic merits of a theory T are the main reason of why a scientist S chooses it, but since individuals may disagree on what these merits are, these epistemic values are not sufficient to explain a consensus around T. Hence, the theory-choice problem and the problem of consensus formation do not receive the same answer from Kuhn.

For many philosophers, Kuhn's thesis of the variability of values seemed unacceptable (Scheffler, 1982;

Shapere, 1964; Laudan, 1985; Lakatos, 1970).[1] The reason was simple. If epistemic criteria do not necessarily lead all scientists to the same conclusion, then, it was assumed, values would be irrelevant to explain the outcome of scientific debate, and agreement would have to come from some non-epistemic source. These would be precisely the factors pointed out by Kuhn as responsible for determining differences in the application of values—elements such as age, institutional position and political-ideological preference. Thus, community cohesion would be promoted by circumstances regardless of any relation to the quality of the theories in dispute. Not only would the problem of theory-choice and consensus formation receive a different answer, but also the answer to these problems would not necessarily be related.

Curiously enough, Kuhn (1992) believed that a similar reasoning supported the Strong Programme—a school that, in most aspects, was diametrically opposed to most of the philosophers who were so critical of him. For the Strong Programme, if values did not lead to a single choice by all scientists, it was because such values did not have a real epistemic function, and thus did not lend themselves to determining the outcome of disputes. And if that was true, then we should look somewhere else in order to explain the resolution of scientific debates—to things like interests, financial support, and all sorts of social factors. At best, the appeal to epistemic values would be a psychological resource efficiently manipulated by scientists, or as one of Kuhn's most forceful critics argued, "a mere interplay of rhetorical effects" (Scheffler, 1982, p. 79).

For both sides, the inevitable implication of Kuhn's thesis of the variability of values was the replacement of epistemology by sociology. Epistemic values could perhaps be relevant to explain theory-choice, but they would be mostly useless in explaining consensus formation. Since values do not determine a universal choice, consensus formation would be explained by non-epistemic factors. What differentiated the Strong Programme's view from the position of more traditional philosophers was simply their stance regarding this relativistic conclusion (Kuhn, 1992). Whereas his more traditional critics mourned the abandonment of epistemological considerations, social constructivists praised, for the same reason, the emergence of social studies.

Kuhn always rejected this interpretation of his work. He insisted that he never took social factors to be the driving force of scientific development, stressing the role of epistemic values for the resolution of scientific controversies. However, we may ask: if values may be differently applied by scientists, what is their function in explaining consensus after all? And how is consensus formed?

The rest of this article intends to examine Kuhn's answer to the second problem concerning scientific changes—the problem of consensus formation. As it will become clear, Kuhn explains consensus formation through a series of socio-epistemic mechanisms, namely: scientific pedagogy, the wave motion, and the restructuring of the scientific fields. After discussing those mechanisms in detail, I will explain what makes them not only social, but also socio-epistemic mechanisms.

Dominance

According to a common interpretation of Kuhn's remarks, values would weaken the power of epistemological considerations to produce consensus. If values cannot prescribe a single choice, then they cannot provide the necessary agreement that science depends on.

This reasoning, however, is based on a particular premise in that scientists who employ values differently will *necessarily* achieve incompatible conclusions. Contrary to that, though, Kuhn believes that distinct applications of values may still lead to similar preferences. "Individual scientists," he claims, "embrace a new paradigm for all sorts of reasons" (1962, p. 151). This can be better explained through the notion of "dominance".

[1] Kuhn himself was aware that "this characteristic of the operation of shared values has seemed a major weakness of my position" (1970a, p. 185).

A theory *T* is said *dominant* if it "is superior to *all* its extant rivals by every extant set of standards utilized in that field" (Laudan & Laudan, 1989, p. 225). Put differently, a theory *T* is dominant if, despite the fact that scientists apply values differently, they all agree that *T* is better than its rivals.[2]

The possibility that one theory is dominant might explain why scientists who employ values in different ways do not have to (rationally) disagree on which theory is superior. In case a theory *T* dominates its competitors, individuals may all accept *T* (and we expect rational scientists will), despite differing on how they evaluate theories. In virtue of such dominance, epistemic values may possess a function in giving an answer to the problem of consensus formation.

In fact, dominance not just blocks the relativistic consequences of the variability of values, giving epistemic values a place in explaining consensus. In theory, if a dominant theory exists, consensus formation could be explained *solely* through epistemic considerations. Nonetheless, dominance alone is probably insufficient to generate consensus within a community. First, it seems unlikely that we find actual cases of dominance in science. After all, assuming that scientists evaluate theories in their own manner, it is hard to imagine that the whole community reaches the same judgment. Dominance, in other words, sounds like an unachievable demand, in requiring that all members of the community agree on which theory to choose. More importantly, dominance by itself establishes only the *possibility* of consensus in the face of divergent evaluations, saying nothing about its effective accomplishment. It is still necessary to indicate how the emergence and disappearance of dominant theories actually occur.

Dominance is, thus, a sort of *statics* of consensus, establishing in which cases a rational, epistemic-based consensus is possible. For this reason, it must be complemented by a *dynamics* of consensus, which explains how dominance can arise and be undermined. This dynamics constitute the core of Kuhn's solution to the problem of consensus formation. It is constituted by three socio-epistemic mechanisms that can be found in his model of scientific development: scientific pedagogy, the wave motion, and the restructuring of the scientific field.

Scientific pedagogy

The thesis of the variability of values affirms that values may be differently applied by scientists. We must be careful, though, not to overestimate the underdetermination of these criteria. That values can be applied differently does not mean that "they may be judged arbitrarily" (Kuhn, 1970b, p. 158). In practice, for Kuhn, the application of values is *highly determined*.

As Kuhn likes to emphasize, the shared components of scientific practice (among which epistemic values can be found) are transmitted through "a narrow and rigid education, probably more so than any other except perhaps in orthodox theology" (1962, p. 165). This uncritical socialization process has crucial consequences for many aspects of scientific practice. It is, for instance, what allows the esoteric character of normal science, directing scientists' attention to certain aspects of nature.

Scientific pedagogy also leaves strong marks in the application of values. Particularly, it counterbalances the possible openness of epistemic criteria. This socialization process homogenizes scientists' assessments and reduces the scope of acceptable applications of values. Individuals with a similar background—who went to the same schools, read the same literature, and performed similar experiments—tend to incorporate scientific practice in a relatively similar manner, and consequently, apply values in close ways.

Scientific pedagogy works, therefore, as an important normalizing mechanism, especially in the aftermath of a revolution. On the one hand, it reduces possible sources of divergence, engendering

[2] This can be considered as a social notion of dominance—i.e., the theory is dominant in case all scientists consider it better than the other options available. There can also be an individual notion of dominance, as in D'Agostino (2005). A theory is then said to be dominant in case it is better in every aspect for a particular scientist, i.e., according to all the different values used in evaluating theories.

a relative homogeneity in the community; on the other, it spreads and reinforces the already existing agreement within the community. When it comes to considering science over time, pedagogy is particularly effective, as an intergenerational procedure to bury controversies once and for all.

The wave motion

Rigorously speaking, scientific pedagogy is more of a maintainer of consensus than one of its producers. Surely, by reducing disagreement, it extends and strengthens consensus. But it does not create consensus in case the community is divided—it is only when things are more stable that it acts. The next two mechanisms are more appropriately mechanisms for consensus *formation*. The first of them is the process of progressive adherence to a theory, which D'Agostino (2010) calls the "wave-model." I use the expression "wave motion," instead, in order to emphasize its actual role in the dynamics of consensus building.

The wave motion can be described as follows. Convinced of the superiority of a new theory, a few pioneering scientists decide to adopt it.[3] While the majority of scientists stay faithful to the older approach, the adepts of the new theory start working under this new approach. If they are successful, they will produce arguments and evidence that favor this position. Consequently, other scientists in the community may lean towards the more recent approach.

These other scientists will then develop the new theory along with the first adepts. Again, if the group can produce significant contributions, they may convince more and more people of the quality of the new theory. With more evidence, the preference of yet other scientists may change towards the new theory. Over time, the transfer of adherence spreads through the community. The process stops when all scientists come to accept the new theory. In the end, what was once a neglected alternative becomes a consensus (Kuhn, 1962, ch. 12).

Two aspects stand out in the wave motion. First, this mechanism assumes that the factors that lead to the progressive acceptance of a theory by scientists are *epistemic considerations*. The only element responsible for the change of opinion by scientists is the improvement or worsening of theories according to epistemic values. For Kuhn, the production of evidence that results in changes of evaluation is the main factor involved in the acceptance of a theory. For him, those values are "*the* shared basis for theory choice" (1977a, p. 322).[4]

Second, the wave motion mechanism depends on the differing impact of arguments and evidence on the various theories. Otherwise, comparative assessments would remain stable, and no change would occur in scientists' preference. For a comparative evaluation—and evaluation is always comparative for Kuhn (see, for example, 1962, ch. 12; 1992)—, it would be as if the evidence did not exist (even if it is relevant to other purposes, such as a better empirical adequacy of the theory). This is perhaps clearer from a Bayesian standpoint: if $P(E|T_1)=P(E|T_2)$, then "the occurrence of E can never change the preference rating between the two competing theories" (Salmon, 1990, p. 192).

Another notable aspect of the wave motion mechanism is its temporal dimension, absent from the notion of dominance. "Rather than a single group conversion," Kuhn claims, "what occurs is an increasing shift in the distribution of professional allegiances" (1962, p. 157). This happens because new evidence and arguments for theories are produced by scientists as part of their work. Because of that, consensus and dissent do not remain static. As evidence and arguments are generated, scientists' preferences may change. Without this, assessments would remain the same indefinitely.

The change in evidential content, however, does not explain the *gradual* aspect of the wave mo-

[3] I speak here of a controversy as the dispute between an old and a new theory, since this is usually taken as the paradigmatic case of theory-choice. But the dynamics of consensus formation is supposed to be the same for other situations, e.g. when there are more than two theories involved or when rival theories are born together.

[4] This does not mean that non-epistemic factors never influence scientists' opinions. They do, but mainly indirectly, by influencing the application of values, and not the result in itself. See Pirozelli, 2019.

tion—i.e., the fact that transitions are usually made piecemeal. Instead, the gradual character of the wave motion is explained by the variability in the application of values. In a community where scientists held identical appreciations, scientists would all accept the same theory. Hence, we would have the whole community changing all at once in a single flow of adhesion, or there would be no change at all (Kitcher, 1990, ch. 8).

But while variability in evaluation formulas is a necessary requirement, it is not sufficient to guarantee the temporal spread of change. Strong evidence could, in fact, impact all the defenders of the old theory in such a way that they would be convinced immediately, despite applying values differently. In other words, nothing guarantees that all of a sudden the community will not move to a new theory.

In spite of being a theoretical possibility, Kuhn believes this situation is unlikely. In practice, he thinks, the acceptance of a new theory tends to occur in stages (wherefrom the idea of "wave" comes), with the gradual growth in the number of supporters. Given what we said about scientists' assessments, this dynamic sounds more plausible. With countless values at stake, distinct ways of applying them, and many forms of incommensurability operating, the impact of evidence and arguments on the whole community tends to be limited, and their role more ambiguous.

Evidence is always open to interpretation and, therefore, insufficient to convince all scientists that one theory is superior to the others. What for an individual may seem evidence that considerably favors a theory, for another may seem irrelevant; still, another scientist may consider it as counting against the theory and still others may find that such "evidence" is not even real. In sum, there is no single way to evaluate theories, especially when it comes to scientific debates. Scientists' decisions are often taken "on the basis of significantly more equivocal evidence" (1977a, p. 327)—as if there were "non-equivocal evidence". Thus, in order to understand the change in the acceptance of theories in the community it is not enough to know their original assessments. It is also necessary to know how, for each of them, the arguments and evidence presented are interpreted and weighed.

On the other hand, Kuhn does not discard the possibility that *groups* of scientists may change all together. In fact, this is expected, knowing that the socialization processes scientists go through may be quite similar. As said before, scientific pedagogy tends to produce a certain homogeneity in the way scientists evaluate theories. In addition, the scientists participating in a controversy may be subject to similar conditions, or have certain specific personality traits that make them behave similarly. In the early stages of a controversy, for instance, some individuals may be attracted by aesthetic considerations "in defiance of the evidence provided by problem-solving" (Kuhn, 1962, p. 156).[5]

One last aspect we may consider regarding the wave motion mechanism concerns the time it takes. No simple answer is available in this case, though. Controversies can be resolved almost instantly, or drag on for years, decades or even centuries. It all depends on how different scientists' evaluation formulas are, how fast new evidence is produced, and how much force the pieces of evidence exert on scientists' evaluations.

Restructuring of the scientific field

The wave motion describes one of the fundamental mechanisms for consensus formation—the process of increasing adherence to a theory motivated by the results of the research undertaken by scientists. Some circumstances, nonetheless, may limit this process and hinder the reorganization of the community around a single theory. Among such circumstances, the most important one is the high variability in evaluation formulas: the larger the number of people involved in the dispute and the looser the socialization processes are in the community, the more difficult it is for all of them to accept the theory.

[5] Sarkar (2007, ch. 6) and Wray (2011, ch. 11) advance a typology of the groups accepting a new theory.

Because of that, the wave motion may be unable to create a consensus within the community. Some individuals may remain indefinitely faithful to the older approach, despite evidence produced in favor of the new theory. Their evaluation formulas may be so unfavorable to the new approach that they may never come to accept the new theory, regardless of any findings or evidence produced. Episodes such as Priestley's rejection of the oxygen theory illustrates well how some scientists may remain resolute in their theoretical choices, notwithstanding the adherence of most of their peers to the new theory. "There are always some men who cling to one or another of the older views," Kuhn says (1962, p. 19; see also 1962, p. 158). The wave motion may not engender an absolute consensus after all.

If the wave motion mechanism will be able to create a consensus or not within the community is ultimately an empirical and contingent matter. By definition, all rational scientists set a positive value on evidence. Hence, in the limit, the uninterrupted production of evidence in favor of a theory must lead to its acceptance by all members of the community. In reality, though, scientists evaluate values differently and, for some individuals, no evidence or argument will change their preferences. Moreover, depending on the theoretical beliefs of a scientist, the new data may not even be considered as legitimate evidence. There is no perspective, then, that certain scientists will be convinced of the superiority of the new theory in a reasonable span of time. Given the limitation in time that scientific activity is subject to, an actual barrier to the wave motion may exist.

We must now ask: if a part of the community remains attached to the old theory, despite the adherence of the rest of the group, how could the controversy end definitively? Other than the wave motion, what else could generate consensus within a community in which part of the members is not convinced of the effectiveness of the new theory?

Kuhn's answer is that the final settlement of this controversy may depend on changes in the structure of the community. In case some members resist accepting the new theory, a consensus will require the creation and modification of the boundaries in the research field; more specifically, a change in the population of scientists in the community. Replacing one theory with another often implies a corresponding change in the composition and structure of the community. "The new paradigm," for Kuhn, "implies a new and more rigid definition of the field" (1962, p. 19).

Such a social restructuring has two prototypical forms (Wray, 2011, Part II; Hoyningen-Huene, 1993, p. 154)—the marginalization of resistant members (Kuhn, 1962, p. 158) and the formation of new specialties (Kuhn, 1991, 1992).

Marginalization of resistant members

The first type of community restructuring is the marginalization of resistant members. Divergence among scientists is expected during certain periods of scientific activity, especially when some of the theories at stake have a revolutionary potential. Scientists will dispute over the merits of theories, setting off a wave motion process. The supporters of each group will try to convince their opponents, through arguments and evidence, of the superiority of their position

At some point, however, when almost the whole community moves towards the new approach, scientists may come to consider that the efforts devoted to demonstrating the superiority of the new theory are more than enough, and additional actions for this aim represent a waste of energy. For all purposes, they consider that the controversy is over. From then on, the disagreement expressed by the unconvinced scientists, which seemed legitimate before, may start to be seen as unjustifiable obstinacy—or, at least, as something that would no longer be worth insisting on.

The perception that some scientists go beyond what is reasonable in their attempt to rehabilitate a defeated theory—in the view, of course, of the majority of the community—causes reactions in the relationship between the two groups. Scientists who accept the new theory stop debating with their opponents, and prioritize dialogue and collaboration with individuals who already share the same theoretical preferences.

Deliberately or not, the adepts of new theory begin to build their own institutional networks and communication channels, from which the adepts of the older theory are excluded. The individuals who remain irresolute in defending a position considered outdated are progressively put aside by the rest of the community. Their research acquires a peculiar character, their publications no longer appear in the same journals (if they are ever published at all), their presence in events decreases, their articles are less cited, and exchanges with other scientists become less intense. In the end, these scientists become isolated from the rest of the community.

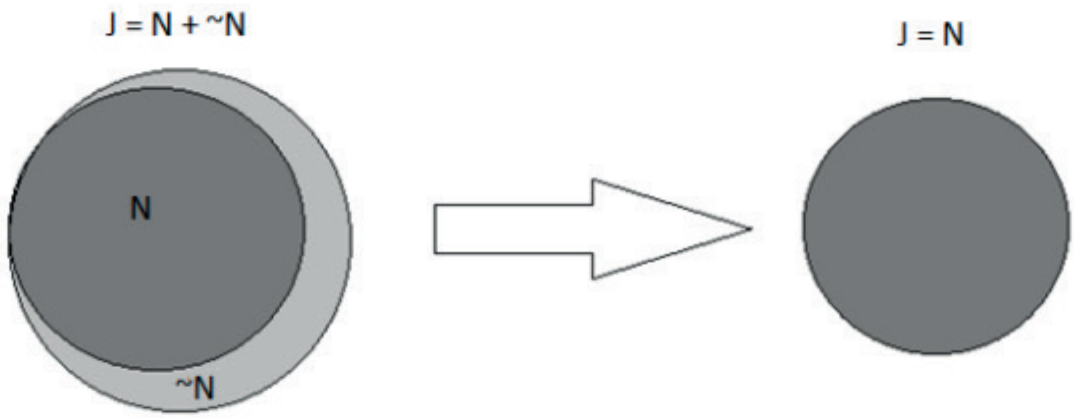
The marginalization of resistant members can be more formally explained. The initial situation is a stage of division inside the community, with some scientists accepting T_1 and others endorsing T_2. A debate then follows, in which case a wave motion process may unfold. At some point, though, the division between those scientists for whom theory T_1 dominates its adversaries, *N*, and those for whom it does not, ¬*N*, stabilizes—with *N* being much larger than ¬*N*. With the marginalization of the resistant members, the old community, formed by both groups,

$$J = N \cup \neg N$$

becomes more restricted. The set ¬*N* is eliminated, and the community becomes simply

$$J = N$$

Scientists who do not accept the theory are—in practice, if not formally—excluded from the community. (Figure 1)



What happens to the adepts of an older theory? Kuhn says that they are "simply read out of the profession, which thereafter ignores their work" (Kuhn, 1962, p. 19).

Kuhn also claims that these marginalized individuals cease to be scientists. This statement is a consequence of Kuhn's sociological definition of science. Science is understood by him as a social practice characterized by a specific set of values (1983a). The same is true for his concepts of scientific community—"the practitioners of a scientific specialty" (1970a, p. 176)—and scientist—the participant in a community characterized by certain practices and values. Ultimately, this whole set of notions is understood by him in a sociological character.

Almost by definition, then, an individual who does not accept the theory endorsed by most of the community and does not properly contribute to the current research—through conversations, texts, and research papers—ceases to be a scientist *de facto*. The social exclusion of an individual from a scientific community is tantamount to the withdrawal of his/her status as a scientist.

Interestingly, though, this sociological definition of scientific practice implies that the early supporters of a theory—that is, those who accept it when most of the community still remains attached to the older paradigm—are not, in this sense, scientists *tout court*, either. Sarkar (2007) had already observed this aspect of the Kuhnian definition of science:

> *Kuhn failed to see a troubling argument by symmetry. If Lavoisier favored the oxygen hypothesis, when his entire profession had not, should he not then be regarded as having ceased to be scientific, too, the late success of the group notwithstanding? (Sarkar, 2007, p. 152, n. 39).*

The only response that can be given to the symmetry pointed out by Sarkar, consistent with Kuhn's sociological position, is to recognize that as much as those who resist a new theory for too long cease to be, in the strict sense, scientists, those who accept it promptly, before everyone else does, are in the same way separated from the scientific community.

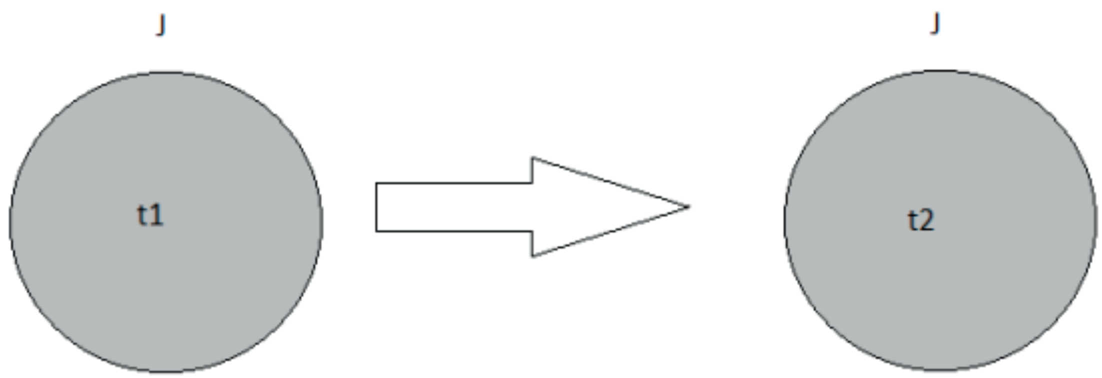
Lavoisier's case, however, demonstrates that such marginalization need not be definitive. The oxygen theory represented an original theoretical approach at the time. So, at least for a while, Lavoisier was relatively isolated from the rest of his community. Over time, though, he was able to produce solutions to problems that seemed intriguing to his fellow chemists, amassing evidence that drew other scientists towards his approach. It was only then that Lavoisier rejoined the group of chemists—having in the meantime reconstructed extensive areas of this discipline (Holmes, 2000).

Disciplinary change

The second prototypical model of communitarian restructuring is disciplinary change. The marginalization of resistant community members is an extremely efficient mechanism of consensus production when the wave motion process exhausts its potential. Its functioning, however, seems to depend on whether the wave motion results in an almost complete agreement—which is, then, completed with the exclusion of the few remaining scientists.

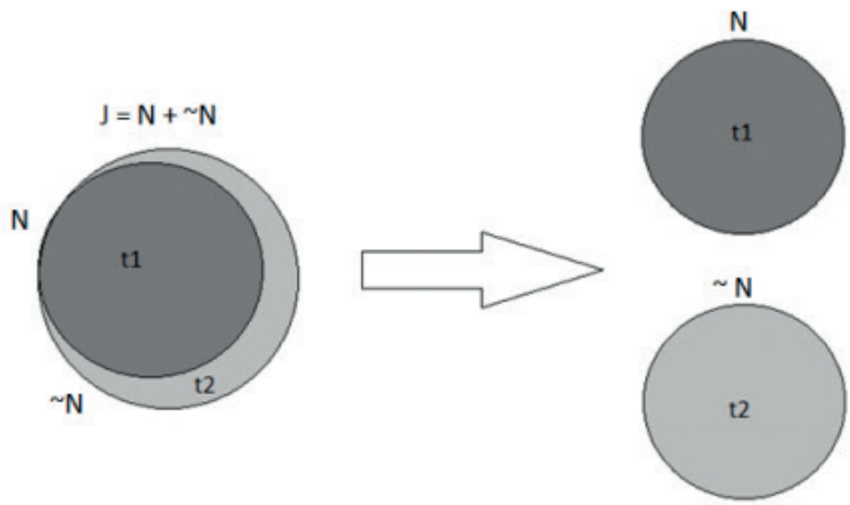
Science, however, would not be very efficient if the resolution of theoretical disputes always demanded the exclusion of substantial portions of the community contrary to a new theory. When the number of adepts of an old theory remains high, another mechanism comes into action—disciplinary change, a split in the group that practices a particular scientific specialty. "The reception of a new paradigm," Kuhn alerts, "often necessitates a redefinition of the corresponding science" (1962, p. 103).

There are three types of disciplinary change resulting from the acceptance of a new theory. The first is when the population remains relatively stable, and the only change occurs at the conceptual level, with one theory succeeding another. This is what Kuhn (1962) calls "scientific revolution." (Figure 2)



In a revolution, a new theory simply replaces an older one, and the community remains substantially the same. This kind of development is linked to an effective wave motion. This does not mean, of course, that all scientists accepted the new theory, as the marginalization of resistant members makes it clear. Nonetheless, if few scientists remain attached to the defeated theory, their exclusion does not seriously damage the community's structure. The result is then, on the one hand, a single community reorganized around a new theory, and on the other hand, an amorphous group of individuals without a strong disciplinary affiliation.

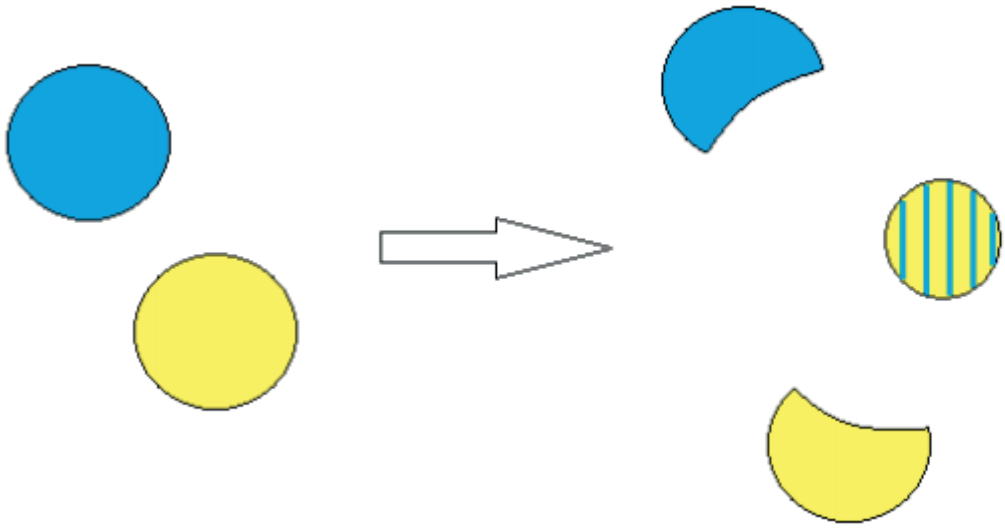
The second pattern of development is that in which a community splits into two. This is what Kuhn calls "speciation" (Figure 3).



In *The Structure of Scientific Revolutions*, as the book's own title makes clear, Kuhn saw the revolutionary substitution of theories as the "usual developmental pattern of mature science" (1962, p. 12). Later, Kuhn started to see in speciation the general mechanism of scientific development. "The biological parallel to revolutionary change," he defended, "is not mutation, as I thought for many years, but speciation" (1991, p. 98). Speciation not only generates new scientific specialties, but also whole new areas of science (Marcum, 2018).

In the case of speciation, the conceptual division within the community causes a fragmentation of the research community itself. There may be two reasons why a community divides. First, the wave motion may not be very effective, being unable to direct all the members of the community towards the same alternative. The only solution, then, is to pursue the different approaches separately. Second, part of the community may decide to explore a new lexicon for conceptual reasons (Wray, 2005). According to Kuhn, new theories are often only successful at dealing with a narrow set of problems (1991, 1993). Some of the scientists, then, may choose to investigate the implications of the new taxonomy further, while others remain under the regime of the older theory. That division is reinforced by communication problems caused by the incommensurability of lexicons. Specific social factors—as the availability of funding—may also provide additional stimulus to the creation of new specialties.

Finally, we can consider a third sort of developmental pattern, which I refer to as "overlapping" (Figure 4). Overlapping is the situation in which "a new specialty has been born at an area of apparent overlapping between two preexisting specialties, as what occurred, for example, in the cases of physical chemistry and molecular biology" (Kuhn, 1991, p. 97).



The structural parallelism between theories and communities is more uncertain in the case of the creation of a new interdisciplinary field. It involves the migration of scientists from at least one of the previous fields, but not necessarily from all the disciplines that gave rise to the new field.

Specialization and overlapping are similar in that they are the result of the creation of a new taxonomy, without completely replacing the older theory. The new disciplinary field investigates a restricted class of problems, with which the older theory was unable to deal with (or, maybe, was not even capable of conceiving it).

This brief typology of types of disciplinary changes does not exhaust the subject at all. On the contrary, a number of problems still remain open: why does the wave motion mechanism have different results in each case? How do institutional factors act in the fragmentation or unification of the scientific community? What is the effect of these different types of dispute resolution on communication among scientists? These are just some of the questions raised by the disciplinary change to which Kuhn offered no answer.

The absence of consensus

We described the three basic mechanisms that lead to the formation of consensus in the community. First, scientific pedagogy, which is responsible for the transmission of values and also for circumscribing the disagreements among scientists. Next, the wave motion mechanism, in which the substitution of one theory for another is thought as a process of progressive regimentation of the adherents of rival theories. Finally, the restructuring of the scientific community, which eliminates the remaining disagreements, putting an end to the controversy. This in turn comprises two sub-mechanisms: on the one hand, the marginalization of resistant members, when only a few members are not convinced of the new theory; and on the other, disciplinary change, when dissent involves a considerable part of the community. These three mechanisms have the respective functions in consensus building of: restricting and reinforcing agreement; decreasing dissensus; and ending the disagreement within the community.

However powerful they are, these mechanisms do not guarantee consensus formation. The wave motion mechanism may have not enough force to convince the whole community, and the restructuring of the scientific community may sometimes not be an option. This can happen, for instance, if scientists think that the controversy should be settled with a more satisfactory solution. The controversy, then, follows for some more time.

In any case, regardless of the consensus building mechanisms just described, consensus may take some time to happen. Kuhn believed, though, that prolonged disagreements were atypical in science. For him, rival approaches in science are "far rarer there than in other fields; they are always in competition; and their competition is usually quickly ended" (1970a, p. 176).

Indeed, for Kuhn, the emergence of normal science may be identified with the usual absence of persistent controversies and the homogeneity of theoretical preferences. While there is no guarantee that scientific controversies will necessarily end, the history of science shows, in Kuhn's view, that consensus formation tends to occur in a relatively quick manner. The mechanisms described by him are, in the end, extremely effective tools for generating agreement among members of scientific communities.

The characteristics of the new consensus

By definition, consensus is the situation in which all scientists in a community hold the same theory. As we have seen, this may happen even when individuals possess incompatible evaluation formulas. What is required for such a consensus is only the dominance of a theory over its competitors for some or most of the individuals, and if necessary, a process of disciplinary change to end the remaining disagreement.

The resulting consensus, it is important to note, is a consensus over choices, not over evaluations. That is, individuals may agree on the superiority of a theory, despite disagreeing as regards how they evaluate theories and the reasons that lead them to their choices (Hoyningen-Huene, 1993, p. 154). This process of consensus-building—independently of the sharing of evaluation formulas—produces what D'Agostino (2005) calls a "shallow agreement"—an agreement regarding results, not a deep sharing of fundamentals.

However, we could ask if a shallow consensus does not, inevitably, provoke an approximation of evaluation formulas. One hypothesis is that the individual differences that produce scientist's evaluation formulas are gradually neutralized over time, generating a similarity in the way scientists evaluate theories. With time, there would be agreement not only on the choices but on the very application of values.

The hypothesis, thus, is that the convergence of choices would engender a convergence in evaluation formulas. We know that the conditional does not hold true for a specific moment: that two scientists prefer the same theory, does not follow that their evaluation is the same. But we may wonder if the approximation would not happen over a longer time horizon. This can be formally expressed as the following: suppose that two scientists, *j* and *w*, prefer the same theory t_1 over t_2. We can express that formally by

$$t_1 >_{w,j} t_2$$

in which > represents the relation of "preference". The hypothesis then states that

$$\lim_{k\to\infty} f_j(t) = \lim_{k\to\infty} f_w(t)$$

in which *k* is time and $f_i(t)$ is the evaluation of theory *t* for scientists *i*.

Kuhn dismisses the validity of this hypothesis. Scientists' choices tend to become similar over time, but there is no reason to believe that their evaluation formulas will become any closer. Although it is possible, Kuhn claims, that "those algorithms themselves also become more alike with time, [...] the ultimate unanimity of theory choice provides no evidence whatsoever that they do so" (1977a, p. 329).

Therefore, the growing agreement regarding theoretical preference does not guarantee any kind of approximation in the way scientists employ epistemic values. For Kuhn, "what converges as the evidence changes over time need only be the values of *p* that individuals compute from their individual algorithms" (1977a, p. 329). Nothing in the growing unanimity of opinions demands that the evaluation formulas become the same.

Actually, for Kuhn, the contrary is probably true: the same individual appreciations that were responsible for producing the dissent continue to be present when the community reaches a consensus. This is what D'Agostino (2005, p. 204) refers to as "residual divergence"—the idiosyncratic factors that explain the division in the community remain present when the scientists reach an agreement (Kuhn, 1977a, p. 329).[6]

Conclusion

Changes of theories are major events in scientific development. Two types of questions may be asked about them: one concerns the reasons that lead scientists to accept a new theory; the other concerns the factors that lead a community to adopt a new approach.

To the first of these problems, Kuhn gives a straightforward answer—scientists choose theories based on epistemic values. He also claims, however, that these values may be differently applied by scientists. For this reason, such criteria cannot explain how consensus is formed in the community. Hence, Kuhn's solution to the individual-level problems does not work as a solution to the second, community-wide problem. Both problems must receive quite different answers.

Throughout this article, I intended to present Kuhn's solution to the problem of consensus formation. For him, three mechanisms contribute to produce agreement within the community—scientific pedagogy, the wave motion, and the restructuring of the scientific field. Together, they limit the potential dispersion to which the variability of values is open to, and help to create a consensus around a new theory. This, I believe, is the essence of Kuhn's *social epistemology*. Consensus, as can be seen, is produced by more than logical or methodological procedures—it essentially involves various sorts of social interactions.

However, claiming that its social dimension is an important part of science (and, specifically, of consensus formation) is broad enough to encompass a large set of approaches—from traditional schools of sociology of science to the most radical versions of social constructivism. So, why asserting that this concern for the social dimension of science makes Kuhn a sort of social epistemologist?

I have no intention here of offering a precise definition of what social epistemology is, and how it differs from other related fields. In fact, it is unlikely that such a definition exists: social epistemology is more probably the reunion of loosely related topics, such as judgment aggregation, testimony, and peer disagreement (Goldman, 2011).

By stressing the social epistemological character of Kuhn's explanation of consensus formation, I neither assume any rigid definition of this field, nor offer a definition of social epistemology that permits to distinguish it perfectly from both sociology and epistemology. Instead, what I intend to do is to stress the sense in which *Kuhn's epistemology is social* and in which *Kuhn's sociology is epistemic*.

The best way to do this, perhaps, is by addressing both terms separately. The "social" part is easier to grasp. By emphasizing the role of social mechanisms in resolving scientific disputes, Kuhn moves away from an individualistic epistemology. The fact that scientists learn theories, values, and all sets of scientific commitments through their peers, and the fact that scientists change their evaluations by interacting with one another, point to the essential communitarian character of science and, especially, of scientific change. More importantly, by showing that the resolution of scientific controversies is linked to

[6] According to Kuhn, then, scientists' evaluations do not tend to become closer to each other. The so-called risk-dispersion argument is the normative counterpart of the thesis of the variability of values. The multiplicity in evaluation formulas is positive for science, since it fosters the production of knowledge by spreading the risks that are inherent to any investigation, better allocating the limited amount of time, energy, and resources that the community possesses. As a matter of fact, a community where consensus led to a conformity in judgments would be epistemically inefficient. See Kuhn, 1970b; 1970a, p. 185-86; 1977a, p. 332. See also D'Agostino, 2005, 2010; Kitcher, 1993, ch. 8.

a restructuring of the scientific field, Kuhn demonstrates that "theory change is a form of social change" (Wray, 2011, p. 10; see also Wray, 2015, sec. 12.5).

More complicated is understanding the sense in which "epistemology" still lives in Kuhn's approach. I will content myself with differentiating it from two main fields, traditional sociology of science and social constructivism. Even these will be treated here in a rather schematic way, since my intention here is not to give a full portrait of them, but just to highlight the peculiarity of Kuhn's project.

Sociology of science aims at accounting for a large set of problems concerning the social structure of science. Among other things, it aims at understanding the historical context in which theories are inserted, the institutional structure of the scientific community, and the factors that restrict or encourage scientific activity. At the same time, the traditional sociology of science explicitly, particularly within the Mertonian school, refuses to deal with the production of knowledge *tout court*. As Merton (1970) claims, "specific discoveries and inventions belong to the internal history of science and are largely independent of factors other than purely scientific" (p. 75).

For Kuhn, however, social patterns *do* explain part of the outcome of scientific activity. Not only do they explain why new theories are *invented*—something with which Merton would agree—, but they also explain, in part, why theories are *accepted*. This is possible because scientists' education and personal experiences are what explain their use of values and, therefore, contribute to explain the choices they make. Thus, for Kuhn, the "social" is central to understanding the "epistemological."

This could lead us to think that Kuhn's proposal is a sort of radical form of social constructivism, aimed at determining the psychological and social factors that would establish scientists' choices in place of epistemic considerations. Indeed, that is how his ideas were often interpreted. If epistemic considerations are not sufficient by themselves to produce agreement, then, it was thought, some sort of external factor was necessary to generate it. Apparently, agreement would be produced through non-epistemic forces that determine scientists' decisions.

As I intended to show, Kuhn's model of consensus formation does not dismiss epistemological considerations. On the contrary: for the most part, scientists choose theories based on epistemic values, even if the acquisition of values is dependent on social factors. Ultimately, it is the epistemic criteria that determine what theories scientists choose.

Surely, epistemic considerations are involved in the wave motion mechanism, through the use of values. But what about scientific pedagogy and the restructuring of the scientific field? In which sense are these mechanisms also *epistemic*?

First, as Wray (2005) explains, disciplinary change, and particularly specialization, may be itself the result of conceptual changes. Pursuing better cognitive tools, scientists may develop taxonomies that account for a narrower set of phenomena. More importantly, both scientific pedagogy and disciplinary change foster the consensus among scientists, producing as consequence a more homogenous and efficient community. By doing that, they permit science to achieve its ultimate epistemic goals of achieving a more comprehensive understanding of the natural world. Thus, scientific pedagogy and disciplinary change are the subject of a social epistemology because they explain the *epistemic* success of science, and not only external or accidental aspects of scientific development.

For Kuhn, these socio-epistemic mechanisms are useful instruments for generating scientific knowledge. According to him, the social structure of scientific research "provides a virtual guarantee that both the list of problems solved by science and the precision of individual problem-solutions will grow and grow" (1962, p. 170). By permitting dissent while at the same time stimulating agreement, scientific communities can create and explore new theories and select the ones they think are most fruitful. Conversely, a community that did not allow disagreement would not be able to explore multiple investigative paths simultaneously.

This, in sum, is why Kuhn's approach may be classified not only as sociology, but as social *epistemology*. For Kuhn, consensus is formed through a set of *socio-epistemic mechanisms*—patterns of interactions among individuals which have a direct impact on how scientists evaluate theories, on how fast evidence is produced and spread, and on how the organization of research fields is established. Together, they enable us to explain the production and growth of scientific knowledge. As Kuhn once said, his work is "deeply sociological, but not in a way that permits that subject to be separated from epistemology" (1977b, p. xx; see also 1983b, p. 28).

Another aspect to be considered has to do with the normative character of Kuhn's social epistemology. The socio-epistemic mechanisms can explain why a scientific community abandons an older theory and accepts a new one, but do they say anything about the outcome of such change—i.e., does the new consensus represent an overall improvement of the epistemic situation of the scientific community?

The answer, I believe, is "no." Kuhn's social epistemology gives an explanation of what leads a community into abandoning an older theory and to adopt a new one, but offers no *justification* of theoretical changes. Given that scientists choose theories mostly for epistemic reasons, we may assume that the newer theory is deemed an improvement by the majority of the scientific community (or to a part of it, since communities can always split and reorganize). Strictly speaking, though, nothing can be said regarding the result of the theory change in itself—that is, apart from specific applications of epistemic values. We can only say that scientists achieved what, from their own standpoint, was seen as a better epistemic position.

This may sound as a serious limitation to Kuhn's project, but I take it as an inevitable byproduct of a larger methodological transformation advanced by him. By claiming that the appreciation of theories is dependent on scientists' idiosyncratic features, and that these may vary from person to person, Kuhn is discarding the idea that we can judge the improvement represented by a new theory regardless of specific interpretations of values. Instead, Kuhn wants us to consider changes of theories as sociological events which alter the distribution of beliefs of scientists and the structure of the community itself. In place of an individual epistemology, Kuhn is offering a social epistemology—one that asks "Why was a theoretical change seen as an improvement to the scientists in a community?", instead of "Was that change of theory an actual improvement?".

Kuhn's explanations on the resolution of scientific controversies do not offer a precise and detailed model of how these events occur. But they are useful in that they provide a general perspective through which consensus is achieved, not through the universal application of a set of rules, but through social-epistemic mechanisms. That is also what links Kuhn's social epistemology with many problems that have recently been considered by other scholars in the field.

For example, the fact that scientists offer arguments to convince their peers, and that this may change the distribution of preferences in the community, does not address the fact that individual scientists may attribute distinct weights to arguments and evidence depending on who provided that information. How do scientists incorporate the opinions and information given by members with different status? What makes someone an authority in a particular field? And what are the social indexes of competent knowledge; how do researchers identify these authorities? The epistemology of testimony provides an essential ingredient to understand the positions and roles of individuals in controversies.

Also, as regards the social interactions which influence scientists' knowledge and choices, we may ask about conflicts that arise from divergence of opinions. What do scientists do when they face disagreement between equally competent researchers? Do they slightly change their opinion, do they embrace that position instead, or do they stay in a position of disbelief? And, also, how *should* they act?

The epistemology of disagreement, then, leads us to questions on normative social epistemology. What kind of community is more appropriate to produce a divergence that is sufficient to generate alternative approaches, and not lose its collaborative practice? How can scientists be trained to be both traditionalists and iconoclasts (Kuhn, 1959)? Those are some of the questions that connect Kuhn's thought to the recent developments in the contemporary field of social epistemology.

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