Kuhn's Notion of Scientific Progress: "Reduction" Between Incommensurable Theories in a Rigid Structuralist Framework

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Abstract. In the last two sections of *Structure*, Thomas Kuhn first develops his famous threefold conception of the incommensurability of scientific paradigms and, subsequently, a conception of scientific progress as growth of empirical strength. The latter conception seems to be at odds with the former in that semantic incommensurability appears to imply the existence of situations where scientific progress in Kuhns sense can no longer exist. In contrast to this seeming inconsistency of Kuhns conception, we will try to show in this study that the semantic incommensurability of scientific terms appears to be fully compatible with scientific progress. Our argumentation is based on an improved version of the formalization of Kuhns conception as developed in the 1970s by Joseph Sneed and Wolfgang Stegmüller: In order to be comparable, incommensurable theories need the specification of relations that refer to the concrete ontologies of these theories and involve certain truth claims. The original structuralist account of reduction fails to provide such relations, because (1) it is too structural and (2) it is too wide. Moreover, the original structuralist account also fails to cover important cases of incommensurable theories in being too restrictive for them. In this paper, we develop an improved notion of "reduction" that allows us to avoid these shortcomings by means of a more flexible device for the formalization of (partially reductive) relations between theories. For that purpose, we use a framework of rigid logic, i. e., logic that is based on a fixed collection of objects.

1. Introduction

According to *Structure,* there are three dimensions of the incommensurability of scientific paradigms: (1) incommensurability of scientific standards or methodological incommensurability, (2) referential or semantic incommensurability, and (3) incommensurability of worldviews or psychological incommensurability.[1] Given this threefold incommensurability of scientific paradigms, it may seem at first glance that there can be no such thing as scientific progress at all. If different scientific paradigms involve different scientific standards and methods, incommensurable references, and incommensurable worldviews, then it

[1] See (Kuhn, 1962, 148-150). Cf. (Hoyningen-Huene and Sankey, 2001; Hoyningen-Huene et al., 2008).

seems as if different paradigms may construe *incomparable* patterns, for which it would be hopeless to find any notion of progress or even continuity. However, interestingly, Kuhn does not draw any conclusion of that kind. In contrast, in the last section of *Structure,* Kuhn develops a rather strong conception of scientific continuity and progress, which he claims to be in full accordance with his equally strong conception of incommensurability. Let us recall this conception of scientific progress with some detail.

According to Kuhn, the complex phenomenon of scientific *revolutions* with their apparent lack of common points completely vanishes when it comes to understanding the sheer progress of the sciences and is replaced by an entirely *evolutionary* perspective. The reason for this phenomenon is that scientific revolutions involve changes at various *nonempirical* levels (cf. the aforementioned three dimensions of incommensurability), whereas scientific progress, in Kuhn's understanding, is exclusively a matter of *the empirical complexity of a theory.* Metaphorically speaking, there are different (possibly incommensurable) scientific vessels, which we always fill with the same empirical liquid, and science develops greater and greater vessels of that kind. At the empirical level, it is perfectly possible, for Kuhn, to talk about theories' closeness to "nature" or "nature itself."[2] However, as the notion of truth is certainly inapplicable to the nonempirical aspects of scientific theories, it does not make sense at all, according to Kuhn, to talk about truth and progress at this abstract level of the sciences. At the abstract (nonempirical) level, there is neither truth nor progress. Insofar, Kuhn's conception indeed defends a *relativism* of a sort. However, at the concrete, empirical level, according to Kuhn, each science grows in an entirely objective and evolutionary way. As the development of the sciences is somewhat chaotic and unpredictable, at the theoretical level, science is certainly nothing that we may be able to understand in terms of teleology. Thus, science is not a goal-directed business at all. On the other hand, science is also a perfectly evolutionary and cumulative enterprise because in the course of its development, it steadily covers increasingly more (and increasingly more complex) parts of "nature." Thus, science is an example for a non-teleological but cumulative process:

> The developmental process described in this essay has been a process of evolution *from* primitive beginnings – a process whose successive stages are characterized by an increasingly detailed and

[2] See (Kuhn, 1962, 168 and 169).

> refined understanding of nature. But nothing that has been or will be said makes it a process of evolution *toward* anything.[3]

In this context, Kuhn also stresses a potentially misleading but highly instructive analogy, namely, the parallel between the turn of the picture of the sciences as propagated in his own book and the turn of the picture of biology as initiated a century ago by Charles Darwin. Kuhn points out that the really challenging feature of the latter account is not its appeal to evolution as such (and the notion of the possible descent of man from apes) but rather the idea that evolution may not be a goal-directed process. Kuhn's own account led to a disenchantment of the sciences in a rather similar way. His "Darwinian turn" led to the rejection of the notion of science as a goal-directed process and therefore to demystification, similar to the one that Darwin had initiated in the field of biology a century ago.

> For many men the abolition of that teleological kind of evolution was the most significant and least palatable of Darwin's suggestions. The *Origin of Species* recognized no goal set either by God or nature. Instead, natural selection, operating in the given environment and with the actual organisms presently at hand, was responsible for the gradual but steady emergence of more elaborate, further articulated, and vastly more specialized organisms. [...]
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> The analogy that relates the evolution of organisms to the evolution of scientific ideas can easily be pushed too far. But with respect to the issues of this closing section it is very nearly perfect. The process described in Section XII as the resolution of revolutions is the selection by conflict within the scientific community of the fittest way to practice future science. The net result of a sequence of such revolutionary selections, separated by periods of normal research, is the wonderfully adapted set of instruments we call modern scientific knowledge. Successive stages in that developmental process are marked by an increase in articulation and specialization. And the entire process may have occurred, as we suppose biological evolution did, without benefit of a set goal, a permanent fixed scientific truth, of which each stage in the development of scientific knowledge is a better exemplar.[4]

This analogy has many fascinating aspects (compare, e.g., Kuhn's "Darwinian turn" with Kant's "Copernican turn" – two milestones in the development of science and philosophy, which both led to a

[3] (Kuhn, 1962, 170f).
[4] (Kuhn, 1962, 172f).

significant demystification and disenchantment of the latter[5]); for our present purposes, however, its most interesting feature is the separation of two different aspects of science that is encapsulated in it. On the one hand, we have the conceptual side of the sciences (the scientific vessels), which leads to incommensurable paradigms and therefore does not involve progress at all. On the other hand, we have the empirical side of the sciences (the liquid we fill into the scientific vessels), which leads to a steadily increasing amount of empirical data that the scientific enterprise is able to grasp (and to generate) and therefore involves a rather obvious and cumulative notion of scientific progress. Thus, a new paradigm will be accepted by the scientific community only if it fulfills the following two requirements:

> First, the new candidate must seem to resolve some outstanding and generally recognized problem that can be met in no other way. Second, the new paradigm must promise to preserve a relatively large part of the concrete problem-solving ability that has accrued to science through its predecessors.[6]

In other words, the new paradigm must be empirically much stronger than the old one in that it should allow us to cover, in particular, the newly discovered empirical data; at the same time, the new paradigm must be able to cover large parts of the old one. This picture immediately leads to a very simple and illustrative way of how to formalize scientific progress: Just take the respective amount of empirical data as covered by a scientific theory and compare these amounts between the different stages of a science.

However, is not the picture of scientific progress that Kuhn draws here totally at odds with his conception of incommensurability? (1) Psychological incommensurability may be seen as a minor problem here insofar as it obviously does not touch the empirical side of the sciences. (2) Incommensurability of standards comes into play here in the context of "Kuhn losses," a problem that we will take into account in the last section, below. In summary, however, (3) semantic incommensurability seems to provide the deepest challenge for Kuhn's notion of scientific progress. Given the possibility of semantic incommensurability, which clearly may involve situations where even the empirical data of different paradigms are formulated by means of incommensurable languages, it certainly seems as if there must exist situations where semantic incommensurability inevitably leads to the sheer impossibility of progress, exactly of the kind that Kuhn points out in the last section of *Structure*.

[5] Cf. Michael Friedman's "neo-Kantian" interpretation of Kuhn in (Friedman, 2001).

[6] (Kuhn, 1962, 169)

In other words, semantic incommensurability seems to destroy exactly the kind of empiricism that Kuhn tries to reanimate in the last section of his book.

At any rate, it seems clear that there is a certain puzzle that evolves here, namely, how to make sense of the seemingly contradictory conceptions of semantic incommensurability and scientific progress as developed in the context of *Structure*. In the rest of this study, we develop a solution to this puzzle, which implies that these two aspects of *Structure* are not contradictory at all. Our solution is based on a formalization of some Kuhnian notions that we develop in the following two sections, on the basis of the respective classical account of Joseph Sneed and Wolfgang Stegmüller.

2. What's wrong with the structuralist account of reduction?

There is hardly any other part of the structuralist framework that has been criticized as often (and sometimes sharply) as the structuralist notion of reduction.[7] In this section we will point out that there are indeed serious problems with this notion. However, addressing these problems, we will immediately propose strategies on how to solve them.

Let us start with a brief review of (a simplified version) of the structuralist notion of reduction. The main point of this notion is that a theory $\mathbf{T}$ is reducible to another theory $\mathbf{T}'$ if there exists a certain structural property that is expressed by means of the existence of a certain relation ρ between the (potential) models of $\mathbf{T}$ and $\mathbf{T}'$. In order to illustrate the essential features of the structuralist account of reduction we use a simplified framework of the following form. A theory $\mathbf{T}$ shall be given in the form of a triple $(\mathbf{M_p}, \mathbf{M}, \mathbf{I})$ of classes of models, in the usual sense of the structuralist formalism of a *theory-element*.[8]

[7] The canonical formulation of the structuralist account of reduction is found in (Balzer et al., 1987, ch. VI.4). See also (Sneed, 1971; Stegmüller, 1976b). A review of early discussions and criticisms of that notion is presented in (Rott, 1987). A volume exclusively devoted to the problem is (Balzer et al., 1984). See also (Hoering, 1984) and (Niebergall, 2000). Last but not least, a certain critique of the structuralist concept of reduction is the main point of criticism in (Kuhn, 1976).

[8] See (Balzer et al., 1987, ch. I and II) for the respective specifications. $\mathbf{M_p}$ represents a class of *potential models*, i. e., the class of all models of many-sorted first- or higher-order logic as restricted to a certain finite vocabulary that fulfills a number of "characterizations" (e. g., a relation symbol may be associated with a certain type, and it may be required that the relation is transitive or the like). $\mathbf{M}$ represents a class of *models*, as obtained from $\mathbf{M_p}$, by the application of a number of laws. $\mathbf{I}$ is a subclass of $\mathbf{M_p}$ – the class of *intended applications*. Thus, the *empirical*

Then, a theory $\mathbf{T}$ is considered *structurally reducible*[9] to a theory $\mathbf{T}'$ iff there exists a relation $\rho \subseteq \mathbf{M_p}' \times \mathbf{M_p}$, such that the following hold:

1. $\mathrm{Rge}(\rho) = \mathbf{M_p}$
2. for all $\mathfrak{S}', \mathfrak{S}$: if $\mathfrak{S}' \in \mathbf{M}'$ and $(\mathfrak{S}', \mathfrak{S}) \in \rho$, then $\mathfrak{S} \in \mathbf{M}$
3. for all $\mathfrak{S} \in \mathbf{I}$ there exists a $\mathfrak{S}' \in \mathbf{I}'$, such that $(\mathfrak{S}', \mathfrak{S}) \in \rho$

Criticism of that conception can be formulated at two levels; it can be identified as being *too wide* and even as being *too restrictive*.

The crucial criticism that applies here is certainly that this definition is much too wide; i. e., we obtain a number of cases where a theory $\mathbf{T}$ appears to be reducible to another theory $\mathbf{T}'$, according to our definition, even though $\mathbf{T}$ obviously has nothing to do with $\mathbf{T}'$. Consider *any* theory $\mathbf{T}'$ with $\mathbf{M_p} = \mathbf{M} = \mathbf{I}$, and define $\rho := \mathbf{M_p}' \times \mathbf{M_p}$. Then, our definition is fulfilled.[10]

In other words, the just-mentioned criticism is crucial indeed, but it is also a sort of truism since we are concerned here not simply with theories *that express purely structural properties* but rather with theories *that express empirical claims* of some sort. This is not to say that structuralists are not aware, in principle, of the fact that empirical claims are structurally incomprehensible. The basic structuralist notion of a theory-element clearly reflects that incomprehensibility in that it is based on the notion of intended applications. However, for dubious reasons, in the case of reduction between theories, this incomprehensibility is no longer respected by the inventors of structuralism, and a purely structural conception has been formulated.

Let us further illustrate this background story by means of some toy examples. Consider an extremely simple empirical theory $\mathbf{T}$, say, a theory, which only claims the truth of a single atomic proposition of

claim of the theory is expressed by the proposition that $\mathbf{I} \subseteq \mathbf{M}$. This conception of a theory-element is a gross simplification of the structuralist formalism because we do not consider *partial potential models*, *constraints* and *links* here, and we also assume that a theory is represented by a single theory-element. However, we only introduce this simplified notion here, in order to illustrate the problems of the structuralist notion of reduction. We certainly do not claim that constraints, links, and partial potential models may be dispensable. Cf. (Moulines, 1984), where the same simplified framework is used (for the same purpose).

[9] This is just a special case of the structuralist notion as defined in (Balzer et al., 1987, p. 277, DVI-5 and DVI-6). In the latter case, a theory is reduced, in our sense, to a specialization of another theory. However, we skip this detail here because it just leads to a notion of reduction, which is even wider.

[10] We do not need complex examples such as they are considered in (Niebergall, 2000, p. 153ff). "Absurd" reductions are obtained here in a rather trivial and straightforward way.

the form $P(c)$ (i.e., we have a vocabulary here that contains just P and c). We may want to express the empirical fact that snow is white, for example. Now, if c refers to snow and P refers to white, then a structure where $P(c)$ is fulfilled becomes a model of our theory. However, there are infinitely many other models of $P(c)$, claiming that grass is green, snow is awful, or Tarski is snowy, etc. Thus, we need a certain restriction of that whole bunch of useless and/or false models to the one and only model $\mathfrak{S}$ that represents the fact we want to express here. This model, to be sure, forms exactly the content of what structuralists call the intended applications $\mathbf{I}$ of a theory. In our toy example, $\mathbf{I}$ contains only one model, namely, the model $\mathfrak{S}$ that points to the fact that snow is white. Now consider another theory $\mathbf{T}'$, where we want to express the same fact but in a different language (say, by means of the predicate Q and the individual constant d). The language may be somewhat literally translatable, e. g., the second language may be German, and we may want to express the fact that Schnee ist weiß. What we may ask now is what kind of criteria a suitable reduction relation between $\mathbf{T}$ and $\mathbf{T}'$ may have to fulfill. The answer is clearly that we are only interested here in the correct mapping of the desired fact (that snow is white) to its actual counterpart in the other theory (namely, that Schnee ist weiß). That is, what we exclusively need here is a function that assigns the intended application of $\mathbf{T}$ to the intended application of $\mathbf{T}'$. It is by no means important here what kind of structure may be preserved or lost in the context of that function. The only thing that matters is that the respective function ρ *has to express* a certain connection between objects that *holds*, i. e., ρ is not just a structural device but something that expresses certain matters of fact and therefore can be 'true' or 'false' (for matters of simplicity we say that ρ 'has to be true'). Thus, we may consider a third theory $\mathbf{T}''$ (e. g., another P-c-theory), which expresses, for example, that grass is green. According to the structuralist formalism, $\mathbf{T}$ would be reducible to $\mathbf{T}''$ in the same way that it is reducible to $\mathbf{T}'$. However, in the latter case, such a reduction would be *plainly false* because the fact that snow is white is clearly not reducible to the fact that grass is green. In a word, the crucial criterion here is obviously that a reduction relation ρ not only has to fulfill certain structural conditions but, in particular, has to express *a certain truth*.[11]

The structuralist account of reduction also suffers from a second kind of weakness. Take the case of phlogiston theory.[12] Here, we have

[11] That reduction involves a truth claim was already pointed out in (Hoering, 1984, p. 43f).

[12] This example is dealt with from a structuralist point of view in (Caamaño, 2009) (see also the remark in the next footnote). The example of phlogiston is also

an initial theory that talks about "substances" such as "air" and "phlogiston", which, according to the theory of modern chemistry, do not exist since the respective phenomena are identified as appearances of oxygen, nitrogen and the like. Nevertheless, we have a number of true predictions in phlogiston theory; therefore, a number of empirical claims that use the terms "air," "phlogiston," etc. are empirically true even in the context of the theory of modern chemistry although these terms do not actually refer according to the latter theory. Let us assume that we have an accurate formalization $\mathbf{T}$ of phlogiston theory and an accurate formalization $\mathbf{T}'$ of modern chemistry (or of that fragment of modern chemistry that is relevant as a replacement of phlogiston theory). Here, again, we may have a mapping ρ between the models of $\mathbf{T}$ and the models of $\mathbf{T}'$ in the sense specified above. But what does it mean to be true for such a relation ρ in the present, much more complex scenario?

The problem we face here is that a reduction of phlogiston theory to modern chemistry obviously cannot be true in an absolute sense, such that everything phlogiston theory had expressed is now expressed in the realm of modern chemistry. The reason for this is simply that there are a number of claims in phlogiston theory that are considered to be plainly false, or at least are no longer covered, by modern chemistry (for any reason). On the other hand, we have all these empirical claims of phlogiston theory that appear to be accurate (as empirical, not as theoretical, claims) and therefore correspond with certain claims of modern chemistry. In other words, a model $\mathfrak{S}$ of phlogiston theory, which appears to have empirical counterparts in a model of modern chemistry, must inevitably disintegrate into a certain part, whose elements can also be expressed in the realm of modern chemistry, and its complement, for whose elements the latter does not hold. Thus, a reduction of phlogiston theory to modern chemistry is unacceptably incomplete as long as it does not provide us with a concrete depiction of these parts of $\mathfrak{S}$ that can be reduced to the new theory and a concrete depiction of the respective counterparts of these parts of $\mathfrak{S}$ in the realm of the new theory. In order to fix this weakness, we have to accompany

one of Kuhn's main examples of a scientific revolution. Cf. (Kuhn, 1962, ch. VI and p. 107) and (Kuhn, 1976, p. 192). Our claims in this paper somewhat overlap with the recent discussion of phlogiston theory in (Ladyman, 2011; Schurz, 2011). The difference between our account and the discussions by Ladyman and Schurz is essentially that we are not concerned with the realism debate here but rather with the problem of incommensurability and with the problem of reduction in the realm of the structuralist framework. However, our conception strongly converges with Ladyman and Schurz insofar as we claim that reduction between theories has to establish a certain correspondence relation between the empirical claims of these theories.

relations between models with relations between the contents of these models (details in the next section). Without such a refined description of the relation between the two theories, a reduction relation may appear to be virtually meaningless because it does not tell us which parts of the models of the old theory can be saved in the new theory and in what sense we may save them.[13]

Note also that the just-suggested solution to the problem of reduction by means of relations between the contents of models overlaps, but is not identical, with the attempts of Carlos Ulises Moulines towards an enrichment of the structuralist reduction device with ontological aspects.[14] We share Moulines's initial motivation:

> When a reduction of T to T' takes place, not only it might be the case that the laws of T are not deducible from the laws of T' and that the relation predicates that appear in T are extensionally and intensionally different from those in T', but it might even be the case that the basic ontologies of T and T' differ radically.[15]

However, we cannot follow Moulines's conclusion, namely, that "[all] we need for reduction, besides scheme (R) [i. e., the initial structuralist account of reducibility], is that there be some ascertainable structural relationship between the base sets of T and those of T'".[16] According to our proposal, this is generally not the case; i. e., we need significantly more than an "ascertainable structural relationship between the base sets of T and those of T'" in that reductions, in our conception, may not only contain relations between first-order objects of the models of theories but also relations between all kinds of objects of theories, including first- and higher-order.

To conclude, an improved version of the structuralist account of reduction has to provide solutions to two main problems, namely:

1. The structuralist account of reduction is *too structural*. It is not sufficient to claim the existence of a certain relation ρ that fulfills certain (more or less weak or strong) structural conditions. Rather, what we actually need is the determination of a relation $\mathbf{R}$ that

[13] The solution to the problem of reduction of phlogiston theory to modern chemistry as provided in (Caamaño, 2009) is simply to restrict reduction to the partial potential models of these two theories (i. e., the non-theoretical objects) and to identify the latter as being identical. However, this implies that the non-theoretical objects of these two theories have to be entirely *theory-independent*. Our conception, on the other hand, will also work in cases where even the non-theoretical concepts of a theory are "theory-infected." Cf. section 3, below.

[14] See (Moulines, 1984). This framework is also used in the aforementioned (Caamaño, 2009).

[15] (Moulines, 1984, 58).

[16] (Moulines, 1984, 58).

establishes connections between the instances of a theory that may be true or false. **R** has to express for the objects of a certain instance of a theory how they actually correspond with the objects of other instances of that theory. Thus, **R** is nothing purely structural but covers a certain claim about how the instances of a theory are *actually* related.

2. The structuralist account of reduction is *too wide*. It is not sufficient to specify a relation **R** (or ρ)that associates models of the old theory with models of the new theory and even to relate models and their first-order domains. Rather, we also have to take into account *the whole content* of these models and to associate these fragments of a model of the old theory that can be saved in the new theory with those fragments of a corresponding model of the new theory that realize these savings.

These two objections are both instances of the general claim that the structuralist account is *too wide*. However, there are also some obvious cases in which the structuralist conception of reduction appears to be *too restrictive*. In principle, there may exist counterexamples to each of the three conditions for a structuralist reduction relation we introduced at the beginning. These conditions appear to be too strong in all these cases where the old theory *was expressively richer* than the new one. The history of science is full of cases of that kind. We have theories that were restricted from the whole universe to the solar system or from infinitely many, infinitely small atoms to finitely many extended elementary particles, etc. It is certainly one of the essential aspects of Kuhn's *Structure* to point to the fact that, after revolutionary changes, theories sometimes have less explanatory power (at least in some respects).[17] Thus, it is hardly surprising that Kuhn criticized the attempt of the structuralist conception of reduction to show that "a later theory resolved all problems solved by its predecessor and more besides"[18] simply because Kuhn had provided empirical evidence that, in cases of scientific revolutions, the latter is generally not the case; i. e., after a revolution, theories generally appear to be significantly *weaker* than their predecessors. As a consequence of this, we conclude that our above conditions 1 to 3 may not hold, under some circumstances (e. g., 'Kuhn losses': see section 4, below).

Note also that, in the present context, it is by no means intended to cover each variety of the notion of incommensurability that was ever considered by Kuhn.[19] Rather, we use this term in a quite re-

[17] See (Kuhn, 1962, p. 107).
[18] (Kuhn, 1976, p. 190)
[19] For an overview of these varieties, see (Sankey, 1993).

stricted way that does not take into account *external factors*, such as sociological and psychological aspects. This becomes possible (and even necessary) because we are concerned with a quite restricted perspective on scientific theories, namely, the question of how we may reconstruct a theory by means of a consideration of its *scientific terms* and *the references* of these terms. For purposes of simplicity, such an account may completely rule out *explanations* of why a certain theory was adopted and why it was later replaced by another theory. It may just provide an "internal" picture of a theory, which, in turn, may be taken as the basis for more comprehensive considerations (that also take care of "external" factors). Given that restriction, the proper notion of incommensurability we need here is the *referential account*, essentially in the way Kuhn had formulated it in (Kuhn, 1976). The latter account identifies two theories as being *incommensurable*, iff they contain terms that do not have corresponding terms in the respective other theory, such that the references of the corresponding terms are congruent, i. e., represented by the same set of objects. In other words, we call theories incommensurable iff their theoretical and/or non-theoretical terms fail to be *directly translatable* into terms of the respective other theory. Kuhn's main objection, in his discussion of the structuralist formalism, was his diagnosis that this formalism is not capable of dealing with theories, being referentially incommensurable in that sense. Thus, the main purpose of this paper is to improve the structuralist formalism in such a way that a proper reconstruction of revolutionary shifts from one theory to a (partly) referentially incommensurable one becomes possible.

3. An informal outline of our improvements to the structuralist account of reduction

An improvement to the structuralist account of reduction that takes into account the above considerations has to fulfill the following conditions:

0. *No general restrictions* to the reduction relation, in the sense of conditions 1 to 3 of the original structuralist account, should be applied.

1. A reduction device has to provide *a substantial claim* about the actual relation between the empirical domains of two theories, and not just a structural existence claim.

2. Besides a relation between the potential models/intended applications of the old and the new theory, a reduction device shall also

contain relations between the contents of the respective corresponding models/applications.

In order to establish these more concrete reduction criteria we shift to the 'rigid' framework as developed in (Damböck, 2009; Damböck, 2012). The main merit of this framework is that it allows us to address the concrete entities a theory is dealing with, and not only (but also!) its structure. The notion of 'intended application' is no longer needed, in this framework, as a fundamental device, because the '(partial) potential models' of a theory as described in the rigid framework are already the intended applications. We obtain this, essentially, by using 'partially interpreted languages', in the sense of (van Fraassen, 1967). In the realm of such a 'language', each 'type' or 'sort' is represented by a concrete and fixed collection of 'possible objects' and therefore the models we obtain by means of axioms, constraints and links are exactly the 'intended applications' of a theory. In this scenario, we also will be able to benefit from the crucial merits of orthodox structuralist conceptions, because the basic sets may be 'large', i. e., they may contain not only the objects of the actual world, but objects of all kinds of possible worlds and applications of a theory. Thus, we may pick out the intended applications by means of some axioms that restrict our ontology or by means of the mentioning of some intended sub-sets of our domains. In the rigid framework, the realm of possibilities is essentially shifted from the realm of classes to the more concrete realm of sets. This may be seen as an inacceptable restriction, in the case of mathematical theories but not in the case of theories *of the empirical sciences*. Since it is only the latter case we talk about here, and since it also has been only the latter case structuralism ever had talked about, the rigid framework seems to be tailored as a tool of improvement for the structuralist framework.

In general, our improved account of reduction will not lead to a full *reduction* of an older theory to a newer one at all (because such a reduction may appear to be impossible). Rather, what we describe here are *relations* between theories that may allow us to reduce *parts* of a theory to another one. The case of a *full reduction* appears to be just a special case here (that will hardly play a role in scenarios of incommensurable theories). In other words, we talk about relations between theories here that appear to be (at best) *partial reductions*. Thus, we henceforth use the term "reduction" here only within quotation marks.

Point (0) merely implies that our whole definition has to be even simpler and even more general than the original proposal of structuralism. We simply cut out requirements 1 to 3 of the original definition and apply requirements of that kind only in the context of special cases of “reduction.”

Point (1), initially, is just a matter of interpretation. The idea of having a relation that expresses connections between the instances of a theory remains unchanged. However, since we do not claim the mere formal property of the existence of a certain relation ρ that fulfills certain structural properties but the existence of certain actual correspondences between the instances of a theory this change in the function of our formalism clearly has to lead to significant changes in the overall formal framework. What we now need is a certain device that allows us to express the actual relation $\mathbf{R}$ between theory-elements as a concrete part of the overall framework, i. e., $\mathbf{R}$ has to be incorporated into the tuple of concrete mathematical objects that characterize our theory-net, -holon, etc. (Since $\mathbf{R}$ expresses a part of the overall net of scientific claims that, like any other part, may be true or false, and therefore may be subject to refinements, revisions, etc.)

Point (2), on the other hand, clearly leads to a formal specification that has to be significantly more complex than the original structuralist proposal. This is exactly the point where we shift to the rigid framework.[20] In this framework a ‘structure’ is given by means of a set-theoretic expression of the following form. Let $D_1, \ldots, D_n$ be the first-order base sets of our framework, and $R_1, \ldots, R_m$ the relation symbols that represent the vocabulary of a theory. Note that the D_i are real *sets* here, in the sense of van Fraassen’s base sets, and not just *types*, in the sense of (Balzer et al., 1987, 6-14).[21] The D_i may contain every possible object that may be addressed in any possible or intended application of a theory. We do not require that the basic sets $\mathfrak{S}(D_i)$, be disjoint. This is not necessary because we are only interested, say, in the cash-value of the ontology of our theory.[22]

[20] The principal layout of the rigid framework is described with much more detail (with the inclusion of constraints, links, etc.) in (Damböck, 2012). In this paper we provide just a sketch of the principal framework, because our main purpose is the problem of reduction, which was not considered in (Damböck, 2012).

[21] As a reviewer of this paper points out, it has to be noted here that this claim is only true for potential models but not with respect to intended applications. Whereas the classical structuralist formalism introduces the objects of a theory at the level of intended applications, we already introduce them at the level of specifications of the D_i (which are mere types in the structuralist formalism and sets in our conception). As a consequence of this formal trick, our formalism becomes more flexible. In particular, in the context of the classical framework it may appear to be extremely complicated (if not impossible) to establish the definition of reduction we develop below.

[22] We do not use our framework for the specification of a semantic for a logic that uses the vocabulary of the theory (but only for the specification of a relation between theories in an informal model theoretic language). However, in order to keep

Then, *a structure* $\mathfrak{S}$ specifies (I) a set $\mathfrak{S}(D_i) \subseteq D_i$ for each i, and (II) a relation $\mathfrak{S}(R_j)$ for each j. Both the $\mathfrak{S}(D_i)$ and the $\mathfrak{S}(R_j)$ may be bound, as usual in structuralism, to certain "characterisations" that establish certain cardinality claims, algebraic requirements, etc. In order to have a concrete expression for *the ontology* of our theory, we define, for each structure $\mathfrak{S}$, the following set $\bar{\mathfrak{S}}$:

$$\bar{\mathfrak{S}} := \bigcup_i \mathfrak{S}(D_i) \cup \bigcup_j (\{R_j\} \times \mathfrak{S}(R_j)) .$$

The set $\bar{\mathfrak{S}}$ contains every element of any basic set and every instance of a relation R_j; the symbol R_j is added as a label that allows us to identify it (i. e., to avoid mixing of all relations of the same type).

On this basis we define a *theory* (or 'theory-element') $\mathbf{T}$ by means of a tuple $(\mathcal{A}, e, \ldots)$ that consists of a set $\mathcal{A}$ of *axioms*, a function e that assigns to each structure $\mathfrak{S}$ the set $e(\mathfrak{S}) \subseteq \bar{\mathfrak{S}}$ of all *empirical* (i. e., observable, non-theoretical) *objects*, and further elements such as constraints, links, etc. that are not discussed here. The *potential models* $\mathbf{M_p}$ of the theory are defined as the set (!) of all structures as based on $D_1, \ldots, D_n, R_1, \ldots, R_m$ that fulfill the characterizations of our framework. The *models* $\mathbf{M}$ of the theory are defined as the set of all potential models that fulfill the axioms of $\mathcal{A}$. The *partial potential models* $\mathbf{M_{pp}}$ are defined as the set of all substructures of potential models $\mathfrak{S}$ that consist of the respective empirical parts $e(\mathfrak{S})$. Similarly, there are also more or less obvious ways how to define the *theoretical content* $\mathbf{Cn_{th}}$ and the *content* $\mathbf{Cn}$ of a theory.[23] The *empirical claim* of a theory, then, is nothing else than the claim that the content $\mathbf{Cn}$ *is identical with the intended applications of our theory.* $\mathbf{Cn}$ is what our theory covers empirically (and if this is not what we want our theory to cover, we have to change it). Therefore, the technical distinction between the notions of 'content' and 'intended applications' (and the respective definition of the 'empirical claim', as the claim that the 'intended applications' belong to the 'content') is no longer needed here. We obtain exactly the same formal and informal result in a slightly different overall setting. The main advantage of this new setting is that all these definitions where rigidity is an essential feature become much simpler (and reduction, of course, is the crucial example for that case).

our construct consistent, we may require here that the domain sets $\mathfrak{S}(D_i)$ and the elements of relations, as specified below, by means of cartesian products of the form $\{R_j\} \times \mathfrak{S}(R_j)$ be disjoint. For purposes of simplicity, we do not consider "auxiliary types" here. Cf. (Balzer et al., 1987, p. 10). Functions are introduced as special cases of relations by means of "characterizations." Cf. (Balzer et al., 1987, p. 14).

[23] For a comprehensive treatment of these crucial definitions see (Damböck, 2012, section 2 and 4, in particular, pp. 710-11).

We now turn to a definition of our improved "reduction" device. If $\mathbf{T}$ and $\mathbf{T}'$ are two theories, then we define a *relation* $\mathbf{R}$ between them as an ordered pair,

$$\mathbf{R} = (\rho, \{\rho_{(\mathfrak{S}',\mathfrak{S})} \mid (\mathfrak{S}', \mathfrak{S}) \in \rho, \})$$

which consists of a relation $\rho \subseteq \mathbf{M_p}' \times \mathbf{M_p}$ and a set of relations $\{\rho_{(\mathfrak{S}',\mathfrak{S})} \mid (\mathfrak{S}', \mathfrak{S}) \in \rho\}$, such that each instance $\rho_{(\mathfrak{S}',\mathfrak{S})}$ is defined as a relation between $\bar{\mathfrak{S}}'$ and $\bar{\mathfrak{S}}$. As mentioned above, we do not want to require too much concerning the algebraic properties of these relations. However, given our rigid interpretation of the ontology as represented by the set $\bar{\mathfrak{S}}$ of a structure, it clearly makes sense to require the following:

1. $\forall \mathfrak{S} : (\mathfrak{S} \in \mathbf{M_p}' \wedge \mathfrak{S} \in \mathbf{M_p}) \rightarrow (\mathfrak{S}, \mathfrak{S}) \in \rho$
2. $\forall \mathfrak{S}, \mathfrak{S}' : (\mathfrak{S}', \mathfrak{S}) \in \rho \rightarrow (\forall p : (p \in \mathfrak{S}' \wedge p \in \mathfrak{S}) \rightarrow (p, p) \in \rho_{(\mathfrak{S}',\mathfrak{S})})$

These two conditions simply imply a certain ontological consistence. If there is a potential model that exists in both theories, then this model is related to itself in the context of ρ (condition 1). Moreover, if two models $\mathfrak{S}$ and $\mathfrak{S}'$ are related and contain identical objects, then these identical objects are also defined as being related in the context of $\rho_{(\mathfrak{S}',\mathfrak{S})}$ (condition 2). In Moulines's terminology, we cover *homogeneous* relations between theories here.[24] Following this terminology we define for each pair $(\mathbf{T}, \mathbf{T}')$ *the homogeneous relation* $\mathbf{R}_h(\mathbf{T}, \mathbf{T}') = (\rho, \{\rho_{(\mathfrak{S}',\mathfrak{S})} \mid (\mathfrak{S}', \mathfrak{S}) \in \rho, \})$ as the (possibly empty) relation that fulfills the above axioms and relates exactly those structures that are identical or contain identical elements and furthermore relates exactly those elements of structures that are identical. We call two theories *empirically identical*, iff the homogeneous relation between them covers exactly all empirical parts of these theories.

On the basis of these definitions, we obtain a number of different scenarios for (partially reductive) relations between theories of increasing complexity.

(A) The simplest scenario, of course, is the scenario where ρ *merely relates identical models to each other*. The extremely important relation of *specialization* between theories is an instance of that scenario (i. e., the case of a completely homogeneous ρ).[25] Here, the only difference between two theories is that one theory may contain models that the other theory does not contain (and vice versa). In the standard case

[24] See (Moulines, 1984, p. 60).
[25] See (Balzer et al., 1987, ch. IV.1). The specialization-relation is intended to cover most cases of normal (non-revolutionary) development of a science.

of specialization, we obtain stronger theories by introducing additional axioms, i. e., more specialized theories simply comprise more axioms and less models than less specialized instances of the same complex of (commensurable) theories.

(B) The second scenario is the case *where we also relate non-identical models in ρ, but merely in such a way that only identical elements of models are related* by means of the respective instances of $\rho_{(\mathfrak{S}',\mathfrak{S})}$ (i. e., the relation is heterogeneous only at the level of models but not at the level of their content). This scenario is obtained in cases where we construe a new theory from an old one by simply rejecting a number of claims of the old theory and/or adding a number of new claims (and leave the rest unchanged).

The standard example for that case is the scenario of two theories being incommensurable at the level of theoretical terms only but sharing the same empirical basis. In our terminology two theories of that kind may be reduced to each other by means of the homogeneous relation that may appear to establish empirical identity between them. In phlogiston theory, for example, the same empirical phenomena are (successfully) covered than in modern chemistry. Therefore, phlogiston theory appears to have the same partial potential models than modern chemistry. Incommensurability is restricted to the field of theoretical entities here. Thus, structuralist reconstructions of phlogiston theory such as (Caamaño, 2009) and the recently unpublished work of José L. Falguera and Xavier de Donato rightly try to establish the continuity between phlogiston theory and modern chemistry by means of having the same partial potential models.

In order to illustrate the way how our formalism allows to reconstruct this scenario, we take a look at the case of phlogiston theory and oxygen theory as dealt with in (Caamaño, 2009).[26] Caamaño's specification of the potential models of phlogiston theory (which starts on p. 336 of her paper) introduces a number of types of sets S, T, C, A, F and a number of relations and functions $g, w, R, \circ$. Similarly, her specification of the potential models of oxygen theory (p. 346f) introduces a number of types of sets S, T, C, O, and a number of relations and functions $g, w, R, \circ$. In the case of the rigid formalism, these types of sets (i.e., S, T, C, A, F, and S, T, C, O, respectively) are introduced by means of some fixed base sets. Let us call them S_b, T_b, C_b, A_b, F_b, and S_b, T_b, C_b, O_b, respectively. The respective sets S, T, C, A, F, and S, T, C, O are defined *as subsets* of the respective base sets, i.e., we have $S \subseteq S_b$, $T \subseteq T_b$, etc. Moreover, we also have to restrict relations

[26] Another example (namely, classical particle mechanics) that illustrates how to translate case studies from the classical into the rigid formalism can be found in (Damböck, 2012, 711f).

and functions, in a similar way, to these relations and functions our theory intends to deal with. These are essentially *the only differences* between *the formal cores* of our and Caamaño's specification, i.e., we essentially may adopt the rest of her formal specifications (with some fairly inessential modifications) from (pp. 336-353). The second important difference between our account and classical structuralism is that *the substantial informal part* of our account is the stipulation of 'base sets', whereas in Caamaño's case the substantial informal part is exclusively to be found in the context of the 'intended applications' (i.e., the notion of 'base set' somewhat replaces the notion of 'intended applications' here). Thus, in our account, we have to make a number of informal stipulations from scratch.[27] In the case of phlogiston theory, for example, we have to stipulate that S_b "is a set of portions of specific chemical substances", that T_b "is a set of temporal instants", that each T is an ordered set that contains two elements of T_b, "one [temporal instant] prior to combustion (t_1) and another subsequent to it (t_2)", and we have to provide similar specifications for the other sets of both theories. Moreover, in our account, we have to stipulate that "the function g determines the combustion or chemical reactions, by assigning to each portion of the combustible substance c and to each portion of air a in t_1 a portion of substance s and a portion of air a' in t_2", and we have to provide similar specifications for the other functions and relations of both phlogiston and oxygen theory. These *informal* stipulations form our theory, together with the *formal* stipulations which are essentially adopted from the classical account (with the only exception that we have to introduce 'base sets', additionally). On this basis, the essential claim of Caamaño's paper, namely, that two theories are *both* incommensurable *and* reducible to each other, can be reformulated in our framework in the following form. We claim (1) that these two theories (i.e., phlogiston theory and oxygen theory) are empirically identical, i.e., based on the same sets of partial potential models, and (2) that they are unrelated in some or all other respects. This is essentially the same connection as established by Caamaño in section 4 of her paper. We may use the same terminology, the same formulas, and the same device of reduction as Caamaño here.

However, the case of empirical identity is just one of a number of possible scenarios. Our framework also allows us to deal with the following more complex cases:

(C) The third scenario is the general case, *where we relate non-identical models and even non-identical elements of models*, i. e., where

[27] The following examples are all adopted from p. 337 of Caamaño's paper.

the relation is heterogeneous even at the level of the content of models. We further distinguish two cases here:

(CI) Our first example for that scenario is the case of two theories *that are incommensurable only insofar as the former fits into the latter just approximately.* The standard example for that case is the shift from Newtonian mechanics to special relativity, where the old theory holds just approximately and only for those cases where velocities are small compared with the velocity of light. Now, if a model of Newtonian physics characterizes the movements of a number of particles, we may pick out these movements that can be identified to be at least approximately correct in the context of the theory of special relativity. Then we have to identify some (one or more) suitable models of the latter theory and associate only these parts of the former model that we identified as approximately true with some (one or more) corresponding elements of the latter model.[28]

(CII) Our second example for the general case of relations between theories is the case of theories *that are incommensurable in a fully-fledged way* because they are based on theoretical and non-theoretical terms that are totally different (i. e., not even similar or approximately identical). In other words, such theories are formulated in entirely different languages, their representatives live in entirely different worlds.[29] Kuhn may have overestimated the importance of such cases or not. What we attempt to show here is that even if such cases of total incommensurability exist we may not have to accept any devastating conclusion for empiricism. More precisely, scenario (CII) is of philosophical importance in at least two respects.

(CIIa) First of all, it seems to provide us with a quite satisfactory solution to the notorious question in what sense a structuralist reconstruction of Kuhn's notion of incommensurability may fill certain "rationality gaps" in Kuhn's account.[30] On the one hand, it is quite obvious that Kuhn's reply to Stegmüller's critique, namely, the claim that the structuralist notion of reduction cannot be used to show that "a later theory resolved all problems solved by its predecessors and

[28] A model that allows us to identify these elements of a model that are approximately true may be chosen, along the lines that were drawn out in (Balzer et al., 1987, ch. VII).

[29] Cf. (Kuhn, 1962, 111).

[30] Stegmüller's attempt to close "rationality gaps" in Kuhn's conception is formulated in (Stegmüller, 1976b) and (Stegmüller, 1976a). Cf., in particular, (Stegmüller, 1976b, p.215): "Two points in Kuhn's conception remain unexplained. First, how it is that the older, dislodged theory had been successful. Second, how we come to speak of progress in the case of scientific upheavals." See also Kuhn's reply in (Kuhn, 1976). For a historical treatment of the program of "Kuhn Sneedified" and the encounter between Stegmüller and Kuhn see (Damböck, pear).

more besides" is accurate.[31] In the case of a theory such as phlogiston theory, which obviously contains a number of claims and solutions that are no longer available in the context of newer theories, there also can be no reduction of these claims and solutions to the context of a newer theory.[32] On the other hand, the improved "reduction" device as introduced above allows us to pick out these parts of the models of our theories that actually correspond. Thus, if we reformulate Stegmüller's objection against Kuhn in such a way that the latter's conception of incommensurability does not allow us to identify *partial correspondences* between incommensurable theories of the just-mentioned kind, then our improved version of structuralism indeed seems to allow us to close this rationality gap.

(CIIb) The second main philosophical merit of our conception is that it allows us to provide a solution to the notorious *problem of "theory-ladenness"*.[33] If our non-theoretical vocabulary is affected by the overall scientific theory, then it firstly becomes unclear in what sense our observations formulated by means of that non-theoretical vocabulary may refer to anything "external" at all or may describe anything "objective" at all. A "reduction" device of the above-introduced form allows us to provide a clear solution to that problem because it allows us to identify the exact counterpart of any observation, as formulated in the context of theory $\mathbf{T}$, in the context of any other theory $\mathbf{T}'$ as long as the latter theory allows us to formulate the respective observation at all.

Assume that $\mathbf{T}$ and $\mathbf{T}'$ are two scientific theories that may use a completely different vocabulary, even at the level of non-theoretical terms (i. e., $\mathbf{T}$ and $\mathbf{T}'$ may be formulated in entirely different languages). Assume further that there is a relation $\mathbf{R}_e$, which identifies exactly all corresponding empirical phenomena of these two theories. Then, we call $\mathbf{T}$ and $\mathbf{T}'$ *empirically equivalent* iff $\mathbf{R}_e$ exactly identifies correspondences between all empirical phenomena as covered either by $\mathbf{T}$ or $\mathbf{T}'$; i. e., $\mathbf{R}_e$ has to cover the whole content of $e(\mathfrak{S})$, for each structure $\mathfrak{S}$ of both theories. (The criteria for which parts of a certain model of a theory may refer to empirical phenomena have to be given here in the context of the respective theory in itself; i. e., the people holding a

[31] Cf. the remarks on p. 10, above.

[32] See (Kuhn, 1976, p. 192). It may be objected here that these claims and solutions that phlogiston theory exclusively provides are not available in the context of modern chemistry simply because they are false. However, it is not the aim of this paper to defend Kuhn's positive claims about the existence of 'Kuhn losses'. Our argumentation is just a conditional one: *if* there are "Kuhn-losses" *then* we have a good solution for that case.

[33] The main immediate sources of the discussion are (Hanson, 1958, p. 19) ("seeing is a 'theory-laden' undertaking") and (Kuhn, 1962). As a starting point to the discussion, see (Bogen, 2009).

certain theory must be fully aware of which kind of claims of that theory refers to "observable facts"[34]). This notion of empirical equivalence is extremely powerful because it is also applicable to theories that are based on totally different sets of empirical data.[35] Thus, our thought experiment shows that even the strongest form of incommensurability, with the inclusion of "Kuhn-losses", may not lead to a situation being inconsistent with empiricism.

This being said, it has to be granted that we deal with the cases of scenario (C) in a rather speculative way here. The question whether there exist any examples in science that cannot be boiled down to the less complex scenarios (A) and (B) must be left open, in the context of this study. Future research will be needed, in order to answer the questions of whether there are 'Kuhn losses', cases of theory-ladenness, and cases of total incommensurability, in the sense of scenario (C). In the present study, scenario (C) is relevant only *as a conditional scenario*. Our argument is that scientific progress, in the sense of Kuhn, might exist, *even if* the strongest possible cases of incommensurability, i.e., cases of scenario (C), appear to exist. If there are no such cases, the better for our claim that Kuhn's account of incommensurability is consistent with his account of scientific progress.

4. Kuhn's conception of scientific progress reformulated

The aim of this last section is only to demonstrate that Kuhn's notion of scientific progress is compatible with his conception of incommensurability, i. e., to work out the main claim of this paper, namely, that the last two sections of (Kuhn, 1962) are coherent. Therefore, the scenario we discuss here is only the case (CII) of fully-fledged incommensurability (with the inclusion of "Kuhn-losses"). Moreover,

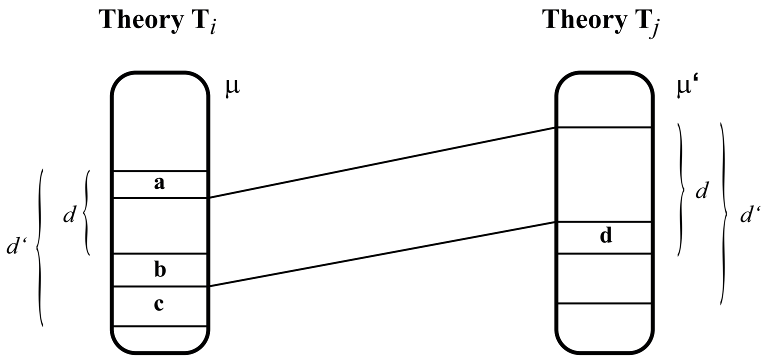
[34] It seems to be neither necessary nor wise to identify the observable parts of a theory by means of the unique identification of a "non-theoretical vocabulary" because there may be certain properties, which sometimes express observable, and at other times non-observable facts. For example, the size of an object becomes un-observable, as soon as the object in question is "small enough." (That is, the identification of a certain term as theoretical or non-theoretical has to be context-sensitive.) For further discussions of the problems of observability and empirical adequacy, see (van Fraassen, 1980, ch. 3).

[35] Empirical data are not neutral but in their selves are formulated in the respective non-theoretical vocabulary of a theory. Of course, we may have some measurements, even in incommensurable theories, where different observational terms are associated with the same numerical values. However, it is quite obvious that this does not necessarily have to be so. On the basis of their entirely different theoretical terms, these two theories may use entirely different units that lead to entirely different numerical values for the same observable phenomenon.

we do not claim here for or against the existence of "Kuhn losses" and strong incommensurability but argue conditionally: even if there is strong incommensurability, even if there are "Kuhn-losses" we may also have scientific progress, in the sense of Kuhn.

Let us assume that the different stages of *a science* may be given by means of a finite sequence $\mathbf{T}_1, \ldots, \mathbf{T}_n$ of theories, in the sense of the previous section (the order of the indices may indicate the temporal order of these theories). Then, we can define a predicate d that ranges over the content of models and identifies all their parts that are corroborated, in the respective context, by means of suitable empirical data. (Note that d must represent substructures of the empirical substructures of theories as picked out by function e. These substructures, in general, will be proper substructures, i. e., the contents of d and e will be non-identical.) Let us further assume that there are relations $\mathbf{R}_{i-j}$ for each i and j between 1 and n, with $i < j$, which may describe the relations between the respective theories $\mathbf{T}_i$ and $\mathbf{T}_j$ in such a way that exactly all d-parts of $\mathbf{T}_i$ and $\mathbf{T}_j$ are associated with their empirical counterparts in the respective other theory (provided that such counterparts exist). The function d', then, may identify all these parts of a model of a theory, which either form d-parts of the actual theory or have counterparts in other theories that form d-parts there. (d', again, picks out (proper) substructures of the e-substructures.)

Consider a theory $\mathbf{T}_i$ and a later theory $\mathbf{T}_j$, and let μ be any $\mathbf{T}_i$-model and μ' its counterpart in $\mathbf{T}_j$ as identified by $\mathbf{R}_{i-j}$. Then, we may obtain the following situation:



Region **a** of model μ represents d-parts of μ, which have no counterparts in μ' and therefore indicates a loss of expressive power in the transition to T_j (i.e., a "Kuhn loss"). Region **b** represents d'-parts that are contributed by theory $\mathbf{T}_j$, while Region **c** represents d'-parts that are contributed by other theories. Finally, Region **d** represents d-parts of μ', which have no counterparts in μ and therefore represent empirical data that are covered only by T_j (i.e., **d** represents an increase in empirical strength in the context of $\mathbf{T}_j$). It must also be noted that Region **b**, e.g., may well represent empirical data that are not measurable at all in the context of theory $\mathbf{T}_i$ (but rather in the context of $\mathbf{T}_j$ only). Nevertheless, given our "reduction" device, theory $\mathbf{T}_i$ appears to be able to represent these data (the latter fact is accessible to the historian of science, who is able to deal with both $\mathbf{T}_i$ and $\mathbf{T}_j$.)

We conclude that the proper measure for scientific progress is given by means of the extended empirical basis d' of the different instances of a theory. As previously mentioned, Kuhn's account implies that in cases of scientific revolutions, we may obtain cases where theories actually become weaker (cf. Region **a** in the above-mentioned diagram). However, the main idea of Kuhn's notion of scientific progress is that, *altogether*, the amount of empirical data as covered by scientific theories always increases steadily and significantly. In other words, scientific progress happens because (and only insofar as) the respective amount of d' increases steadily (and usually exponentially), while we obtain either a relatively small amount of "Kuhn losses" or no "Kuhn-losses" at all.

It is not the purpose of this study to decide whether Kuhn's conception of scientific progress is compatible with or superior to stronger conceptions, such as scientific realism. We merely wanted to demonstrate that Kuhn's conception of scientific progress, as developed in the last section of *Structure*, is consistent with his conception of incommensurability, as developed in the earlier passages of that book. However, this essentially *historical* approach may also be seen as the starting point for the formulation of a *systematic* argument in favor of Kuhn's conception of scientific progress. If it is true that progress in Kuhn's sense is compatible with incommensurability, then it may turn out that stronger conceptions, such as the different varieties of scientific realism, are no longer needed to prove that the development of science is not a miracle. Thus, our argumentation may also be seen as the starting point for the development of a Kuhnian version of antirealism, which appears to rest on the main virtues of scientific realism. However, this is a different story that shall be presented in the context of another report.

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